Lost Wanderers in the Forest of Knowledge: Some Thoughts on the Discovery–Justification Distinction

Don Howard
Department of Philosophy and
Program in History and Philosophy of Science
University of Notre Dame

1. Introduction: What Questions Should We Ask

Neo-positivism is dead. Let that imperfect designation stand for the project that dominated and defined the philosophy of science, especially in its Anglophone form, during the fifty or so years following the end of the Second World War. While its critics were many,[1] its death was slow, and some think still to find a pulse.[2] But die it did in the cul-de-sac into which it was led by its own faulty compass.

The project was this: To provide global, formal explications of central methodological notions—confirmation, explanation, laws—and global, formal answers to global questions about the structure and interpretation of scientific theories, including, most famously, the realism-antirealism debate.[3] That the neo-positivist project would lead to a dead end should have been clear, and was clear to some, from very early on. W.V.O. Quine made the point already in 1951 in "Two Dogmas of Empiricism" (Quine 1951) with his critique of the notion of analyticity, and I have always read Nelson Goodman as having tried to make a similar point in his 1953 lecture, "The New Riddle of

[1] A list of important early critics from within the philosophy of science mainstream would have to include Stephen Toulmin (1953, 1961, 1972), Norwood Russell Hanson (1958), Paul Feyerabend (1975, 1978), and Thomas Kuhn (1962). It can be argued that the effect of later critiques by, for example, social constructivists and feminist philosophers of science, has been felt more strongly outside of the philosophy of science mainstream than within, for better or worse. For more on why that has been the case, see Howard 2003.

[2] Those with the best stethoscopes work in the domain of inductive logic and probability theory. Perhaps the most interesting case, however, is that of Philip Kitcher. His recent work seeks an opening for questions about the way science lives in a social and political context, but his anxiety about the threat of relativism draws him to assumptions about truth and objectivity so strong as to make problematic any systematic integration of value considerations in the doing of science (see, for example, Kitcher 1993, 1998a, 1998b, 2002, and Kitcher and Cartwright 1996).

[3] Here I rely heavily on Arthur Fine's way of characterizing the problem (Fine 1984a, 1984b).

Induction" (Goodman 1955), with the argument that no merely formal explication of the notion of confirmation can mark the distinction between a law-like generalization such as "All emeralds are green" and its nonsensical cousin, "All emeralds are grue." Quine and Goodman also pointed out where the error lay, Quine with his explicit endorsement of epistemological naturalism (Quine 1969) and Goodman with his suggestion that the difference between a projectable predicate like green and an unprojectable predicate like grue was to be explained (not explicated) by means of a concept of entrenchment borrowed from anthropological linguistics. The error lay in the philosopher of science's principled restriction of philosophical attention to the merely formal. Quine and Goodman believed that making sense of empirical science required the tools of empirical science, Quine celebrating such reflexivity (to use the fashionable modern term) as not vicious but virtuous circularity, if, that is, the goal is taken to be not justification, which Hume already taught us was impossible, but, more modestly, the description of how, to borrow a phrase from Quine, sensory input becomes theoretical output.

The project whose ultimate demise was thus foretold by Quine and Goodman was born in the 1930s, chiefly through the work of Rudolf Carnap and Hans Reichenbach. Foremost among its central dogmas, foremost at least for the purpose of legitimating a conception of the philosophy of science as a purely formal enterprise, was the distinction between the *context of discovery* and the *context of justification* and the associated claim that the philosophy of science confines itself to formal questions within the context of justification, for the perspectives on science thus proscribed as inherently non-philosophical—history, sociology, psychology, biology and kindred studies of the contingent—treat of subjects falling within the realm of what Hume dubbed "matters of fact." Logic alone survives as the one "science" in a science of science.

Many questions cannot be asked and answered by a philosophy of science so constrained. Thus, neo-positivism was as if dumbstruck by questions about the role of science in human affairs, whether they be questions about nuclear weapons, environmental problems, or biotechnology.[4] And the relationship between the history of science and the philosophy of science could be theorized in no way other than as an insult to the historian, with the suggestion that history's only role was to provide rationally reconstructed case studies as vindication for the philosopher's normative methodological claims.

But neo-positivism is dead, and so we now ask questions that were once taboo. Are we, however, asking all of the right questions? The point of the present paper is to suggest that, perhaps, we are not. We might think ourselves like Theseus. Having slain the Minotaur, do we now follow our Ariadne's thread back out of the cave? Perhaps. But might we not be more like Hansel and Gretel, having found that the crumbs with which we marked the trail into the woods have all been gobbled up? I worry that we are seriously lost.

To return from the realm of metaphor, my concern is that unless we understand the proscriptive work really intended by the authors of such neo-positivist dogmas as the DJ distinction, we cannot know what questions we should now be asking. It is good that we now explore the way in which experimental practice, the social structures of science, and the psychology of the researcher play a role in the acceptance of scientific theories. As I have argued elsewhere, however, when, in 1938, Reichenbach beatified the DJ distinction in *Experience and Prediction* (Reichenbach 1938), his target was not–or not just–epistemological naturalism. His target was, instead, Otto Neurath,

[4] Philip Kitcher is, of course, the notable exception among mainstream philosophers of science; see Kitcher 1985, 1996. Still more exceptional is Kristin Shrader-Frechette, whose work as a philosopher of science takes her deep into the heart of policy debates; see Shrader-Frechette 1985, 1991, 1993).

Philipp Frank, and the left wing of the Vienna Circle, Neurath especially being then famous for his effort to theorize a positive role for social and political values in theory choice (Howard 2000). The history of that encounter between Reichenbach and Neurath is retold, briefly, in the next section of this paper. For now, I just state the lesson of that history, a lesson itself elaborated later on. The lesson is that, if what was forbidden was the assertion of a positive role for social and political values in theory choice, then the task today is to resume the conversation with Neurath about whether there is and should be such a role for values in the doing of science.

2. The Historical Background: Reichenbach and Neurath on Values and Theory Choice

In 1913, Neurath published a lovely little essay in which one finds adumbrated all of the main ingredients of his mature philosophy of science. As well known in its day as it is unknown today, it carries a curious title: “Die Verirrten des Cartesius und das Auxiliarmotiv. Zur Psychologie des Entschlusses,” nicely translated as “The Lost Wanderers of Descartes and the Auxiliary Motive (On the Psychology of Decision)” (Neurath 1913). The image of the lost wanderer is taken from a passage in the *Discourse on Method*, where Descartes contrasts theory and practice by pointing out that, unlike science, practical action requires our making firmly fixed, if perhaps ungrounded and uncertain decisions. In the realm of praxis we are like people lost in a wood. To find a way out, one must simply decide to keep moving in a given direction, though there be no reason for preferring that direction over others. Neurath believes that Descartes is wrong, that science, like practical action, requires ungrounded decisions. That by which we so decide he calls the “auxiliary movtive” (note—not “auxiliary reasons”). He mocks the foundationalist illusion of fixed rules of method and fixed criteria of theory choice as “pseudo-rationalism.” Why are there no such foundations? The argument proceeds from an essentially Duhemian view of science (Duhem 1906), wherein theories are interconnected wholes, and theory choice is underdetermined by logic and experience:

> Whoever wants to create a world-view or a scientific system must operate with doubtful premises. Each attempt to create world-picture by starting from a *tabula rasa* and making a series of statements which are recognised as definitively true, is necessarily full of trickeries. The phenomena that we encounter are so much interconnected that they cannot be described by a one-dimensional chain of statements. The correctness of each statement is related to that of all the others. It is absolutely impossible to formulate a single statement about the world without making tacit use at the same time of countless others. Also we cannot express any statement without applying all of our preceding concept formation. On the one hand we must state the connection of each statement dealing with the world with all the other statements that deal with it, and on the other hand we must state the connection of each train of thought with all our earlier trains of thought. We can vary the world of concepts present in us, but we cannot discard it. Each attempt to renew it from the bottom up is by its very nature a child of the concepts at hand. (Neurath 1913, 3)

Basically this same view of theory choice is reiterated, with occasional refinement and clarification, in Neurath's writings over the next twenty-five years.

On one key point, however, Neurath was much more explicit in later years. It is that prominent among the auxiliary motives are social and political values.[5] On Neurath's view, it is a contingent fact, well supported by historical evidence, that we do choose among empirically equivalent theories on the basis of our estimation of the likelihood of their serving our favored social and political ends, this especially in sciences like economics and sociology. Denial of this fact being symptomatic of pseudo-rationalism, it were better that we be honest with ourselves and others about our so choosing. The cause of scientific objectivity and the cause of human freedom (for Neurath those two causes are one) are better served by open, public debate about the values whereby we

[5] The idiom of "values" was not that in which Neurath, himself, expressed the point, explicit talk of "value" carrying the wrong connotations in an early twentieth century Germanophone environment where the *Werturteilsstreit* was a recent memory (Albert and Topitsch 1971, Ciaffa 1998). But it was the idiom employed in the Anglophone literature, especially in North America, whether in Reichenbach's public disagreements with Dewey over values in science or in Frank's later promotion of Neurath's position. For more on the North American setting, see Howard 2003.

choose, for the agents of regressive social and political interests hide their agendas behind the disguise of pseudo-rationalism.

It is surely no accident that, unlike Carnap, Reichenbach, and Moritz Schlick, who cut their philosophical teeth on general relativity, Neurath was an economist and sociologist. Briefly imprisoned after the First World War for his role as President of the Central Economic Office in the short-lived Bavarian Soviet Republic, Neurath went on to become a prominent figure is the Austrian Social Democratic Party, for many years Director of the Social and Economic Museum in Vienna, an institution with deep roots in the worker education movement. An Austro-Marxist, Neurath's far-left politics infused all of his philosophical work. His ambition for the Vienna Circle, the Verein Ernst Mach, the *International Encyclopedia of Unified Science*, and the logical empiricism that was their philosophical doctrine was that they would promote an image of science and scientific philosophy as engines of progressive social change.[6]

Though not Marxists of any stripe, Reichenbach and Carnap were also socialists, Reichenbach having been a prominent leader of the student socialist movement in Berlin at the end of the First World War. But Reichenbach and Carnap disagreed profoundly with Neurath over the role of social and political values in theory choice. It is not widely enough known that their famous dispute over the role of conventions in science, a dispute that we recall, if at all, in the form it took in the protocol sentence debate of the early 1930s, was, in important measure, an argument over this very question (see Neurath 1932, Carnap 1933, Schlick 1934; see also Zhai 1990). Even as a dispute

[6] There is now an abundance of good secondary literature on Neurath. See, for example, Cartwright et al. 1996, Nemeth 1981, Nemeth and Stadler 1996, Reisch 1995, Stadler 1997, and Uebel 1991, 1992.

over the role of conventions, however, the issue is not always clearly enough understood. Here it is in outline.

In an effort to craft an empiricism capable of defending the empirical integrity of general relativity against mainly its neo-Kantian critics, Schlick and Reichenbach had in the 1920s elaborated a view of conventions according to which only analytic coordinating definitions are conventional. They held that once the coordinating definitions are fixed by convention, each remaining synthetic proposition in a scientific theory is thereby endowed with a determinate empirical content of such a kind that the proposition's truth or falsity is unambiguously determinable on the basis of the corresponding experience. Alternative conventions are possible, and simplicity considerations might lead us to prefer one set over another, but the resulting differences in theory are held to be inconsequential, being no different in kind than the difference between English units and metric units. The very fact that two alternative theories are empirically equivalent guarantees that they are just two different ways of saying the same thing, for empirical content is the only content of theory. This is verificationism, the view later attacked by Quine in "Two Dogmas." The view worked against neo-Kantian critics of general relativity by securing the synthetic, empirical status of claims about the metrical structure of spacetime.[7]

But the verificationism of Schlick and Reichenbach does other work as well. For if it works, then it also blocks Neurath's assertion of a role for social and political values in theory choice. If the difference between alternative conventions is no more than the difference between two languages, and if every synthetic proposition has its own, determinate, empirical content, then theory choice is,

[7] Classic statements of this view of the role of convention in science are Reichenbach 1928 and Schlick 1935. For a discussion of the development of this view and the background of debates over the empirical integrity of general relativity, see Howard 1994.

in principle, univocally determined by considerations of logic and evidence, leaving no room for the operation of values of any kind.

Neurath's theoretical and epistemological holism inclined him to a different view of conventions. Such holism being inhospitable to the analytic–synthetic distinction upon which the Schlick–Reichenbach view is based, Neurath tended like Duhem before him and Quine after him to view all of the propositions composing a theory as equally conventional or equally empirical in character. Such holism being incompatible with the verificationist semantics of Schlick and Reichenbach, Neurath tended to view the differences between alternative empirically equivalent theories as being often quite significant, especially from the point of view of the differential capacities of alternative social and economic theories to promote specified social ends.

It was this argument with Neurath that Reichenbach resumed when, in 1938, in *Experience and Prediction*, he premiered the distinction between the context of discovery and the context of justification.[8] Reichenbach wrote *Experience and Prediction* in English during his exile from Nazism in Turkey. He did so for the explicit purpose of introducing himself and his philosophical movement to the North American audience he hoped soon to be addressing directly. Neurath is not named as the target of the DJ distinction, and that he was the target was not evident then, and is not evident now, to Anglophone readers ignorant of the German philosophical literature from which Reichenbach was emerging. But it would have been crystal clear to Reichenbach's former Viennese and Berlin colleagues.

---

[8] The idea behind the DJ distinction was certainly not original with Reichenbach, it having long been central to debates about psychologism. Thus Popper deploys much the same idea in his critique of induction in his *Logik der Forschung* (Popper 1934). For more on the history of late-nineteenth and early-twentieth century debates about psychologism see Coffa 1991, Kusch 1995, and Peckhaus, this volume. What is original with Reichenbach and influential in the further development of logical empiricism is the specific formulation of the distinction and the use to which it is put.

The distinction is introduced in section one of *Experience and Prediction*, "The Three Tasks of Epistemology." These three tasks are, respectively, the descriptive, the critical, and the advisory task. The descriptive task involves only the rational reconstruction of historical episodes for the purpose of bringing to the fore those crucial logical aspects of the episode that reside in the context of justification. The critical task involves the direct analysis of those logical features of the structure and interpretation of theories. It, too, resides wholly in the context of justification and forms the heart of scientific philosophy. The advisory task is most interesting. One might think that the philosopher of science should offer advice about the ends served by the different choices we make when doing science, as Neurath surely would, but Reichenbach recommends a more modest advisory role, in which the advisory task collapses into the critical task. The philosopher of science should advise not about ends, but only about means for the attainment of such ends:

> We may therefore reduce the advisory task of epistemology to its critical task by using the following systematic procedure: we renounce making a proposal but instead construe a list of all possible decisions, each one accompanied by its entailed decisions. So we leave the choice to our reader after showing him all factual connections to which he is bound. It is a kind of logical signpost which we erect; for each path we give its direction together with all connected directions and leave the decision as to his route to the wanderer in the forest of knowledge. And perhaps the wanderer will be more thankful for such a signpost than he would be for suggestive advice directing him into a certain path. Within the frame of the modern philosophy of science there is a movement bearing the name of *conventionalism*; it tries to show that most of the epistemological questions contain no questions of truth-character but are to be settled by arbitrary decisions. This tendency, and above all, in its founder Poincaré, had historical merits, as it led philosophy to stress the volitional elements of the system of knowledge which had been previously neglected. In its further development, however, the tendency has largely trespassed beyond its proper boundaries by highly exaggerating the part occupied by decisions in knowledge. The relations between different decisions were overlooked, and the task of reducing arbitrariness to a minimum by showing the logical interconnections between the arbitrary decisions was forgotten. The concept of entailed decisions, therefore, may be regarded as a dam against extreme conventionalism; it allows us to separate the arbitrary part of the system of knowledge from its substantial content, to distinguish the subjective and the objective part of science. The relations between

> decisions do not depend on our choice but are prescribed by the rules of logic, or by the laws of nature. (Reichenbach 1938, 14-15)

As Reichenbach had earlier explained, the concept of entailed decisions was one of the more important discoveries already made under the heading of epistemology's critical task. The suggestion seems to be that too much attention to the context of discovery leads us to exaggerate the significance of logically and empirically ungrounded choices that lead to a genuine parting of the ways, choices that Reichenbach here terms "volitional bifurcations." Put on the blinders, focus on the context of justification, note the central role of entailed decisions—choices that are implied by prior choices—and the genuinely volitional aspect of science nearly disappears. Narrowing the focus to the context of justification, declaring unphilosophical all questions in the context of discovery, means, then, our declaring unphilosophical all of those questions about the role of social and political values in theory choice that Neurath addressed under the heading of the auxiliary motive, all of those questions about social and political values that, for Neurath, mattered most.[9] Scientific philosophy as Reichenbach characterizes it is but another species of pseudo-rationalism.

Neurath died late in 1945 and so did not survive the war to speak to a new Anglophone audience on behalf of a philosophy of science that claimed a positive role for social and political values in theory choice. Philipp Frank was the one friend of Neurath's view to survive the emigration. Through the 1950s he promoted the Neurath line in many papers and books (see especially Frank 1953, 1957), but he was doing so from the comparatively ineffective position of a part-time teacher of physics undergraduates at Harvard. It was Reichenbach at UCLA and Carnap

[9] Promoting a politically disengaged philosophy of science was more than prudent in those parts of Europe under fascist control in the 1930s. It might have been prudent and was surely expedient in North America in the 1930s, 1940s, and 1950s. See Howard 2003.

at, first, Chicago, and then as the late Reichenbach's successor at UCLA who were training a new generation of Ph.D.s in the philosophy of science and setting the agenda for the discipline in its new North American home. Reichenbach continued to advocate a politically disengaged philosophy of science in works such as *The Rise of Scientific Philosophy* (Reichenbach 1951). Carnap had come around partially to Neurath's view in the protocol sentence debate, but he never wavered in his defense of analyticity, and while his personal political activities, his favorable reception of Kuhn's *Structure of Scientific Revolutions* as co-editor of the *International Encyclopedia of Unified Science*, and his characterization of external questions respecting the choice of a linguistic framework as "pragmatic" (Carnap 1950) might suggest some subtlety in his mature view, his public philosophical example, at least, never even hinted at a sympathy for Neurath's view of the place of social and political values in theory choice.[10]

The fact is that when neo-positivism emerged in the 1950s as the "received view" in the newly institutionalized and rapidly expanding discipline of the philosophy of science, no one was left to speak for Neurath. Interlocutors might have been found among the followers of John Dewey's theory of science, as witness the example of Charles Morris, co-instigator with Neurath and later co-editor with Carnap of the *International Encyclopedia of Unified Science*. But the old socialist Dewey was dead by 1952 and the ranks of his followers were thinning rapidly, Dewey's conception of a theory of science being deliberately driven out of the academy by thinkers as diverse in their sympathies as Sidney Hook and Ernest Nagel. Science might have won the war in defense of

[10] See, however, the not entirely unsympathetic discussion of his disagreement with Neurath on this point in his "Intellectual Autobiography" (Carnap 1963, 22-23). For more on Carnap's reaction to Kuhn's *The Structure of Scientific Revolutions* when it was submitted for publication as part of the *Encyclopedia*, see Reisch 1991.

freedom and democracy, but not even those values were to be accorded a place in the postwar project of the philosophy of science.

3. Resuming the Conversation I: Letting Neurath Speak

If the real target of Reichenbach's introduction of the distinction between the context of discovery and the context of justification was Neurath's philosophy of science, with its assertion of a role for social and political values in theory choice, then questioning the distinction requires, first and foremost, our resuming the conversation with Neurath. In fact, several scholars have recently sought to do just that, most notably Nancy Cartwright (see, especially, Cartwright 1999; see also the authors cited above in note 6). But while much has lately been written about Neurath's politics, a specifically philosophical engagement with his claims about the place of value in science has, thus far, not been a central theme in the conversation. Cartwright, for example, is drawn more to Neurath's celebration of theoretical pluralism. I want us to take up the conversation about values in science.

The potential benefits to the discipline of our renewing the conversation about values are many. Most importantly it might open up the prospect of the discipline's once again finding something helpful to say about the place of science in human affairs, this in a philosophical voice, and not just as citizen participants in the political process who merely happen to be philosophers of science. It might also open up the possibility of a rapprochement between the philosophy of science and those historians and sociologists of science who feel themselves estranged from a purely formal philosophy of science mainstream. It might make possible our welcoming our feminist colleagues into the mainstream. And it might make possible our reintegrating the philosophy of science with philosophy more generally, not now primarily on the epistemological ground where once we met when general methodology defined the philosophy of science mainstream, but in the domains of moral philosophy and, to the extent that contextual values are accessible only historically, the history of philosophy.[11] Moreover, these benefits can accrue without threat of a loss of the philosophical rigor upon which we philosophers of science have always prided ourselves.

If we are to resume the conversation with Neurath about values in science, let us start by giving him the chance to speak first. He did, after all, have a lot to say on this score, much of it quite insightful. I turn then, first, to some central points in the further elaboration of Neurath's socially and politically engaged philosophy of science. A complete account of Neurath's philosophy of science is not to be given here,[12] but rather my own rather selective list of points important for the subsequent conversation.

a. Values and objectivity. Begin with the all-important question about values and objectivity. Is scientific objectivity so obviously threatened by according values a central place? Two points are to be made under this heading. First, Neurath's general epistemological framework is that of Duhemian underdeterminationism. The place of values in science is secured by the fact that, on Neurath's view, logic and experience underdetermine theory choice. But turn that argument around and it implies that values come into play only within what I like to call the domain of under-determination. That is, logic and experience are first allowed to do all of the work they can do. Only then do we ask which of several empirically equivalent theories is most conducive to the achieve-

---

[11] Major chapters of the history of philosophy are, after all, really about the history of science and the history of the philosophy of science. This is especially true, as Gerd Buchahl (1969) argued, in the modern period. Schäfer, this volume, discusses Duhem's arguments for integrating history and philosophy of science.

[12] Those wishing a more complete rehearsal of Neuarth's views should consult first Uebel 1992 and Cartwright et al 1996.

ment of our social and political ends. Though values have an essential, unavoidable role to play, science as thus portrayed is as rigorously empirical and logical, hence, in this sense, as objective as it can possibly be. Value considerations are not intended to trump considerations of logic and experience; they are intended to respect them.

Second, given the fact that social and political values play a role in theory choice, the cause of objectivity is best served by openness and honesty about that role. This is a point often stressed in our day by feminist philosophers of science.[13] But androcentrism is not the only interest served by the pretense of a science that lives above the fray. Neurath thought the interests of capital equally well served by that pretense. Openness about the role of values in science is not always easy to achieve, however, especially for those—typically those in power—whose agendas are thus served by the pose of value neutrality. Think only about the absurd pretense of scientific objectivity among the practitioners of neo-classical economics. Neurath believed that clarity and honesty about the role of values in science is more easily achieved, though of course not necessarily so, by the dispossessed and the downtrodden. Here is how he expresses his version of what we would call standpoint theory:

> Marxism makes it understandable why the bourgeoisie, conditioned by its class position, becomes ever more unscientific in the field of social theory. . . . To many bourgeois it may seem degrading, and an infringement of the dignity which is conceded to science, if one looks at it from the point of view of the class struggle. The proletariat appreciates science properly only as a means of struggle and propaganda in the service of socialist humanity. Many who came from the bourgeoisie are worried whether the proletariat will have some feeling for science; but what does history teach us? It is precisely the proletariat that is the bearer of science without metaphysics. (O. Neurath 1928, 297)

Objective science is, for Neurath, therefore, precisely—if ironically—a value-laden science that is just honest with itself about its value-ladenness.

[13] See, for example, Longino 1990 and 2002.

b. Values all the way down. A politically engaged philosophy of science secures the objectivity of science by restricting the operation of value considerations to the domain of underdetermination. But how large is that domain? A famous and crucial claim of Neurath's in the protocol sentence debate was that the holism and the consequent underdetermination go all the way down to the protocol sentences, the observation sentences wherein theory meets experience. This was a principal premise in Neurath's argument for a physicalist protocol language. Since phenomenalist protocols lack the veridicality required for them to perform their intended foundationalist work, that lack of veridicality a consequence of the unavoidably propositional character of our protocol sentences, better to adopt a physicalist protocol language, the putative referents of which—medium-sized physical objects—have an advantage over the subjective contents of momentary, private experience, the advantage of being public objects. But we pay a price for the public character of physicalist protocols, for like all discourse about physical objects, physicalist protocols are entangled in the web of belief:

> Science is *ambiguous—and is so on each level.* When we have removed the contradictory groups of statements, there still remain several groups of statements with differing protocol statements that are equally applicable; that are without contradictions in themselves but exclude each other. Poincaré, Duhem and others have adequately shown that even if we have agreed on the protocol statements, there is an unlimited number of equally applicable, possible systems of hypotheses. We have extended this tenet of the uncertainty of systems of hypotheses to all statements, including protocol statements that are alterable in principle. (O. Neurath 1934, 105 [translation corrected]; see also O. Neurath 1932, 94-95)

Neurath had the courage of his convictions in thus acknowledging what some might see as a fatal implication of his view. What happens to scientific objectivity if the domain of underdetermination, the domain in which value considerations play a proper role, extends to the whole of science?

More is to be said later about the values-all-the-way-down problem. For now, just a mitigating observation. In principle all physicalist protocols in all scientific fields are under-determined. In practice, however, this phenomenon will be far more pronounced in the social and economic sciences that were Neurath's chief concern. But this is also the realm in which most of us never doubted the theory-ladenness of observation. More so than in physics, social data need an interpretation before they become evidence.[14] From this perspective, what first appeared to be a threatening implication of Neurath's view turns out to be little more than a platitude.

c. Praxis and theory choice. A recurring theme in Neurath's philosophical works, especially when he was writing, as he did regularly, for an audience of his fellow socialists, is that, at least in the social and economic sphere, theory choice is driven by the need for practical action, a need often so compelling and often felt in similar enough ways by many members of relevant communities as to obscure the fact that a genuine array of empirically equivalent theoretical alternatives is always available. "Action" here means both direct political action and the more mundane activities of everyday life, and typically, for Neurath, "action" means not the private actions of atomic individuals but collective social action. I quote a longish passage to illustrate the feel of Neurath's thinking on this point:

> This is how matters stand in every *"layer"* of scientific work, not only in the narrower sphere of systems of hypotheses, as Poincaré and Duhem have pointed out with such intensity. But these initiatives in multiplicity are constricted by life. A whole human lifetime is hardly long enough to immerse oneself in even a single view and to give full thought to its consequences. And how soon one senses the weakening effect of isolation. Thus one deserts the lonely, though perhaps auspicious, notions of an outsider to join in the work in a way of thought that

[14] But even in the physical sciences there can be a significant interpretive moment in the selection of one's protocol sentences. See, for example, Schäfer's discussion (this volume) of Duhem's views on the "moral" element in theory choice associated with the assessment of the reliability of the sources of one's evidence, his example being Kepler on the 8° of arc accuracy in Tycho's data.

> enjoys more support and has therefore better chances of greater scientific achievement. In such ways it happens that not even too many possibilities are treated by several groups at the same time: through adaptation and selection a kind of assimilation of whole generations takes place—not to speak of the cases in which certain trains of thought are anathema, persecuted and suppressed.
>
> This insight that a logically tenable multiplicity is reduced by life has little hope of response because it contradicts the usual view of a connection between achievement and "success." The representatives of a victorious doctrine are too much inclined to believe that their victory could be justified as it were by closer logical investigation. Many see the course of the history of science like that. (O. Neurath 1935, 117)

Neurath adds: "If, in spite of these comments on multiplicity and uncertainty, one sets unswervingly to the work that is seen as a common one, one can do so only because one knows how much the historical situation reduces the manifoldness *via facti*" (O. Neurath 1935, 119). When the action in question is direct political action, there might be more conscious awareness of choices being made:

> The Marxist, as strict scientist, must admit that the course of history allows of various interpretations. But successful collaboration is possible only when those who act fix on one possibility, whether by agreement or propaganda. This choice is itself a matter of action and resolution, but that does not mean that such action has no scientific basis. (O. Neurath 1928, 293)

It is not as though Neurath was the first or last philosopher of science to think about the relation between theory and practice, but what is more commonly intended under that heading are questions addressed via a distinction between pure and applied science, where it is assumed that we first choose our theories on more narrowly epistemic grounds and then act by applying those theories. That is not what Neurath intends. On Neurath's view, practical considerations intrude at the start as well as the finish.

Far more can and should be said about Neurath's philosophy of science. Would that time permitted, for example, our thinking about his promotion—this with almost missionary zeal—of a unity of science not via theoretical unification but via the adoption of a universal physicalist protocol language. Neurath intended this, too, to do political work, for the physicalist protocol language was recommended in part for its being inhospitable to the obscurantist rhetoric favored by apologists for capital and other socially regressive interests. But it was Neurath's employment of the Duhemian view of empirically underdetermined theory choice for the purpose of securing a place for social and political values in science that Reichenbach aimed to block with the DJ distinction. Let us keep the focus there for now, and let us now venture a response to Neurath.

4. A Digression: The Epistemic–Non-epistemic Distinction

Before we can begin our response to Neurath, however, we must do some philosophical housecleaning. One cobweb, in particular, must be removed, namely, the distinction between epistemic and non-epistemic values, a distinction introduced in reaction to Kuhn. Replying to various early critical responses to *The Structure of Scientific Revolutions*, Kuhn sought to defend his claims about incommensurability while denying the relativist implications that some readers found in his story about paradigm conflict. According to Kuhn, all parties to a paradigm dispute typically share a common ground in their commitment to all manner of values, including simplicity, explanatory power, fertility, and empirical accuracy. Where they disagree is in their weighing of these shared values in assessing the merits of competing paradigms (Kuhn 1969, 184-186; 1973). To this perhaps not wholly convincing argument Ernan McMullin responded, not unsympathetically, by recommending a distinction between what he termed epistemic and non-epistemic values. The former, the truth-inducing values, were to be accorded a respectable place in science, the latter not, for a science that chooses according to the former is less open to the charge of relativism (McMullin 1982). A theory's being conducive to one's favored social and political ends would be regarded as a non-epistemic value, and choosing among theories on such grounds invites the charge of relativism.

That empirical adequacy is an epistemic virtue few would doubt. Things get a bit murkier when we turn to explanatory power, fertility, and simplicity. Consider the case of Einstein on simplicity. One would be hard pressed to find a stronger advocate of simplicity as a criterion of theory choice, especially in frontier physics far removed from the realm of direct empirical testability. But near the end of his life, after decades of reflecting on the question, Einstein gave up the attempt to define this elusive virtue, concluding that while scientists tended to agree in their judgments of simplicity, the comparison of theories with respect to simplicity amounted to "a reciprocal weighing of incommensurable qualities" (Einstein 1946, 21, 23; see also Howard 1998). Fertility is no more likely to be captured in a formula, and for all that our intuitions might incline us to expect otherwise, even explanatory power proves hard to characterize in a sufficiently general manner.[15]

Does the frustration we feel in seeking global definitions of simplicity, fecundity, and explanatory power tell us something about those virtues, or does it tell us something about the underlying distinction between epistemic and non-epistemic virtues? I suspect the latter. Who would doubt the prima facie reasonableness of choosing among empirically equivalent theories on the basis of putatively epistemic virtues? But why do we think that in so choosing we choose the theories more likely to be true? It depends partly on how we think about truth. If we think that there is only one truth about nature and that in choosing among empirically equivalent theories we take a step,

[15] And recall from the discussion of Goodman's "New Riddle of Induction" that even the notion of empirical adequacy might prove hard to pin down. For more on recent skepticism about global, unitary characterizations of methodological norms, see Stump and Galison 1996.

fallible though it might be, toward the establishment of that one truth, then the appeal and the cogency of the epistemic–non-epistemic distinction is evident. But at this point I find Quine helpful. This most thoroughgoing Duhemian was famous for arguing that, if theory choice is underdetermined by considerations of logic and evidence, then there can be no global, extra-theoretical talk of truth or the approach to the ultimate truth, for one can put through a Tarski truth definition only theory by theory: "Truth is immanent, and there is no higher. We must speak from within a theory, albeit any of various" (Quine 1981, 21-22; see also Quine 1960, ch. 1, and Quine1969). If Quine is right, then the ground falls out from under the epistemic–non-epistemic distinction, for the distinction requires access to an extratheoretical semantic perspective whose existence Quine denies. The reasonableness of preferring simple to complex theories remains, but so too does the reasonableness of choosing theories that will make the world a better place.

5. Resuming the Conversation II: Doing Neurath the Courtesy of a Reply

Now we are ready to begin replying to Neurath about the place of social and political values in science. Where to begin? I want to begin with what I think is the most serious challenge posed to us by Neurath, namely, the argument that underdetermination and hence a role for value considerations, goes all the way down to the level of the protocol sentences. Note that while Quine has not expressed interest in promoting a role for social and political values in theory choice, he agrees with Neurath that even our observation sentences are entangled in the web of belief (see Quine 1951, 43, and Quine 1960, 42-45).

Why do we find the claim that it is values all the way down so worrisome? It is surely worrisome if one's ambitions for epistemology are of the foundationalist, justificatory sort that

Neurath and Quine disavow. If we can choose from among our protocols only those flattering to our political agendas, then protocols cannot exert upon theory choice the kind of univocal empirical control for which foundationalist empiricists like Schlick hoped. But if the aim is simply to describe how science is done, why be distressed by the claim that there are values all the way down? If that's the way the world is, then that's the way the world is. Wishing it were otherwise won't make it so.

The real worry occasioned by the claim that it is values all the way down is that radical, anything-goes relativism then threatens. Such worries are, however, misplaced, being partly consequent upon a frequent misunderstanding about the underdetermination thesis. Consider the latter in its most extreme, Quinean form: Theory choice in all domains is empirically under-determined, this underdetermination persisting even if one were in possession of the infinity of all possible evidence. It follows, says Quine, that any favored hypothesis or, if Neurath is right, any cherished protocol can be saved as long as one is prepared to make sufficiently radical alterations elsewhere in the web of belief. But from the assertion that *any one* proposition can be, thus, insulated from refutation, it does not follow that *anything* goes, and this precisely because of the theoretical and epistemological holism that is the flip side of underdetermination. The web of belief is a deeply interconnected whole. To save a cherished hypothesis or protocol, one must make changes elsewhere in the web. The freedom of choice is not a freedom simply to deny the manifest evidence of the senses. One has to interpret, one has to tell a coherent story, and one has to tell a story that works.

Yet another reason why many see the threat of radical relativism in the claim that it's values all the way down is that we tend, wrongly, to model scientific decision making as if it took place in a social and historical vacuum. In fact, such choices are made by communities of inquirers whose members typically share, in large measure, a biology, a history, a language, an education, a paradigm, perhaps, and much else besides, the effect of which is to incline them to similar choices. Do we wonder, with Einstein, how it can be that the experts agree in assessments of simplicity even though simplicity proves hard to define? No deep mystery here. A virtual necessary condition on the experts' being recognized as experts is their being similarly socialized into the communities' shared ways of regarding nature. The surprise would be if they disagreed in their assessments of simplicity. And the same holds, though perhaps to a lesser degree, when the values are not aesthetic but social and political. There are no a priori principles guaranteeing the possibility of consensus, just contingent, empirical facts about communities of inquirers making more or less likely the possibility of consensus. Be the ground for its possibility a necessary or a contingent one, consensus is still consensus.[16]

Mention of socialization brings us to another important point well understood by Neurath, which is that theory choice, including the choices that constitute the empirical basis of science, is a collective enterprise. Theory choice does not take place in a vacuum and it is not the work of disembodied, isolated, individual knowers; it is the work of social groups. But if we regard even the experience upon which science rests as a social achievement,[17] yet another perspective avails itself from which to regard the question of radical relativism and the claim of values all the way down. For it shows us that, even as philosophers, we can and should be asking questions about the social structures through which such experience is achieved.

---

[16] Schäfer, this volume, discusses Duhem's view of the manner in which tradition constrains theory choice and, thereby, makes more likely a measure of continuity in the historical development of science.

[17] Dewey, a famous critic of the spectator theory of knowledge, stressed the social nature of experience (Dewey 1929), as do a growing number of contemporary philosophers of science; see, for example, Longino 1990, 2002, Solomon 2001, and Kusch 2002.

An older literature frequently dramatized the threat of radical, anything-goes relativism by the example of the Lysenko affair. In this rightly notorious case, Soviet work in evolution and genetics was set back by over a generation thanks to obvious perversions of scientific practice in the name of a political agenda (see Joravsky 1970). But the problem here was not that Soviet geneticists had a faulty philosophy of science, and the threat was not to be met by the fantasy of a value-neutral science, though that was the standard prescription. The problem was that the social institutions of science were not working as they should. The passing whims of Joe Stalin were no good substitute for peer review.

There once was a time when Robert Merton's work on the social norms of science was deemed a subject matter fit for the philosophy of science journals.[18] But that was a long time ago. Today we might fault Merton's naive philosophical understanding of science, his overly-simple assumptions about truth and objectivity, and his reluctance to adopt a sociological perspective on the content of science, as opposed to its institutional structures, but that should not blind us to the value of his example. For the recollection of the older philosophical literature on Merton's norms should remind us that philosophers *qua* philosophers can and should concern themselves with questions about the social structure of science. Our doing so would pay dividends. I would be delighted, for example, if one could turn to the philosophy of science journals for tough-minded discussions of proprietary research. If philosophers of science were once so exercised over the Lysenko affair, why are they not even more exercised by this new and distressing trend, where research is done under contract to private corporations with the stipulation that the corporation, not the scientist, owns the

[18] The classic sources are Merton 1938 and 1942. Note that the former was published in *Philosophy of Science*. For more on the place of Mertonian sociology of science in pre-1950s North American philosophy of science, see Howard 2003. See also Wang 1999 and Douglas 2004.

results of the research and can block their publication in the standard scientific venues. Why should we trust Merck Pharmaceuticals any more than we trust Joe Stalin?[19]

If philosophers *qua* philosophers can take the naturalistic turn by way of asking questions about the social structures of science, then yet another perspective opens up on the question of objectivity, a perspective famously associated with Helen Longino (1990, 2002). Many worry that viewing science as a social enterprise entails, inevitably, some compromise with objectivity, for it opens the door to interests other than an interest in the truth. Longino argues, with deliberate irony, that, far from subverting the cause of objectivity, the socializing of our view of science promotes objectivity (or at least intersubjectivity) by making public debate about those interests an integral part of science. Neurath's comments on pseudorationalism are directed toward a similar end. Bad science is done when we lie to ourselves about the place of social interest in science. Good science, objective science, is science that is honest about social interest, about the auxiliary motive, for that is the way to reveal those interests for public scrutiny and debate.

In addition to the claim that it's values all the way down, Neurath, as we heard, insisted that the need to act played a major role in resolving underdetermination. If act we must, then we cannot pause like Buridan's ass before empirically equivalent piles of straw. If act we must, then often our choices will have been made for no other reason, if a "reason" it be, than that some choice must be made. And for Neurath that's a good thing. More commonly, however, we choose with a practical aim in view. As mentioned above, this is not the way mainstream philosophers of science have

[19] Kuhn was interested in social structures, but he tended not to be interested in using sociological tools in what one might term a knowledge-critical manner. He taught us to think of a paradigm as the shared commitments of the members of a scientific community, but he did not bother to ask questions about what kinds of community structures were conducive to good science, once, that is, one gets beyond claims about socialization and normal science as a precondition for the doing of science.

usually theorized the relation between theory and practice. What is usual is to distinguish pure science from applied science and then argue that the epistemic merits of the former are unaffected by the latter. Neurath's way of conceiving the relation makes practice an essential part of the epistemological story. What are the consequences of such a reorientation?

One consequence is that this reorientation of the question about theory and practice gives us a more hospitable framework in which to pose questions such as those that interest contemporary philosophers of experiment. Many students react at first with puzzlement to the claim that there is significant cognitive content in experimental practice. How can that be? Experiments are things that I do with my hands. But any good pianist understands what it means to say that there is knowledge in one's fingertips, and anyone who learned to ride a bicycle at age five should understand what it means to say that one has knowledge in the seat of one's pants. The problem with the pure–applied distinction is that it makes doing discontinuous with knowing, or rather "knowing how" discontinuous with "knowing that." On Neurath's view, all knowing is just a form of doing. *Making* a choice between empirically equivalent theories in the course of *acting* is how knowledge is *made*, and this at every level, right down to the *making* of protocol statements.[20]

Let me forestall misunderstanding by noting that such making is not what is intended when the phrase "social construction" is spoken with a Scottish accent. What is made on Neurath's view is knowledge as genuine as knowledge can be, the making being constrained by all manner of factors other than just social interest, factors such as evidence. As that other Neurathian, Quine, would remind us, we can put through a Tarski truth definition, albeit not a single one for all theories.

---

[20] Here is another place where there are interesting and provocative parallels in the work of Dewey; see, again, Dewey 1929. See also Nickles, this volume.

Another consequence of our reorienting our thinking about theory and practice along Neurath's lines is a moral one. The pure–applied distinction makes possible a most annoying moral dodge: As a scientist I am responsible only to the truth and not responsible for the consequences of applying the theories I discover and prove. But if the application is a part of the proof, then responsibility to the truth entails responsibility for the applications. I think that we used to talk about this under the name "pragmatism."

A final consequence that I would mention here of our thus reorienting our thinking about theory and practice is its suggesting that we are overlooking a lot if we think that the only epistemological questions worth asking are questions about the acceptance of theories. If theory choice is so thoroughly practical in character, then a finer vocabulary of epistemic attitudes is needed to do justice to scientific practice. Merely acting on a theory need entail nothing about one's having accepted the theory as true or as possessing a high degree of verisimilitude. As noted above, sometimes I choose just because I have to. Sometimes I choose because my choice is permitted given the relevant constraints. Sometimes I choose because my choice flatters my prejudices. Sometimes I choose because my choice represents the way I wish things were even if I have no clue that they are so. And sometimes I choose because my choice seems the most prudent basis for action after weighing such evidence as might be at hand and the risks of acting on the basis of other choices. We are not helped by being told that attitudes other than grounded acceptance all fall below the divided line, in the realm of illusion and mere opinion.[21]

[21] The need for a more varied vocabulary of epistemic attitudes is a theme in Bas on van Fraassen's *The Empirical Stance* (van Fraassen 2003).

6. Conclusion

It is no accident that, as we have neared the end of this paper, the density of references to Dewey and to feminist philosophy of science has steadily grown. Deweyan pragmatism and feminist philosophy of science are just about the only two philosophical projects of the twentieth century capable of carrying on a respectful conversation with Neurath. It is also no accident that neither occupies a place in the philosophical mainstream. Continuing the conversation with Neurath would require our bringing in those voices far more than was possible here. Important questions would be raised. Thus, an interesting difference between Neurath and Dewey concerned the question of whether science has a role to play in the choice of ends, Dewey saying yes, Neurath no. Save that question for another occasion.

REFERENCES

Albert, Hans and Topitsch, Ernst, eds. (1971). *Werturteilsstreit*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Buchdahl, Gerd (1969). *Metaphysics and the Philosophy of Science; The Classical Origins: Descartes to Kant*. Cambridge, MA: MIT Press.

Carnap, Rudolf (1933). "Ueber Protokollsätze." *Erkenntnis* 3: 215-228.

——— (1950). "Empiricism, Semantics, and Ontology." *Revue internationale de philosophie* 4, nr. 11: 20-40.

——— (1963). "Intellectual Autobiography." In *The Philosophy of Rudolf Carnap*. Paul Arthur Schilpp, ed. The Library of Living Philosophers, vol. 11. La Salle, IL: Open Court, 3-84.

Cartwright, Nancy (1999). *The Dappled World: A Study of the Boundaries of Science.* Cambridge: Cambridge University Press.

Cartwright, Nancy, et al. (1996). *Otto Neurath: Philosophy between Science and Politics.* Ideas in Context, no. 38. Quentin Skinner et al. eds. Cambridge: Cambridge University Press.

Ciaffa, Jay A. (1998). *Max Weber and the Problems of Value-free Social Science: A Critical Examination of the* Werturteilsstreit. Lewisburg, NJ: Bucknell University Press.

Coffa, J. Alberto (1991). *The Semantic Tradition from Kant to Carnap: To the Vienna Station*. Linda Wessels, ed. Cambridge: Cambridge University Press.

Douglas, Heather (2004). "Robert Merton and the Ethos of Science." Paper presented at the Fifth International Conference of HOPOS, the International Society for the History of Philosophy of Science, San Francisco, 24-27 June 2004.

Duhem, Pierre (1906). *La Théorie physique. Son objet et sa structure*. Paris: Chevalier & Rivière.

Einstein, Albert (1946). "Autobiographical Notes." In Schilpp 1949, 1-94. Quotations are taken from the corrected English translation in: *Autobiographical Notes: A Centennial Edition*. Paul Arthur Schilpp, trans. and ed. La Salle, Illinois: Open Court, 1979.

Feyerabend, Paul (1975). *Against Method: Outline of an Anarchistic Theory of Knowledge.* London: NLB.

——— (1978). *Science in a Free Society*. London: NLB.

Fine, Arthur (1984a). "The Natural Ontological Attitude." In Jarret Leplin, ed. *Scientific Realism.* Berkeley: University of California Press, 83-107. Reprinted in Fine 1986, 112-135.

——— (1984b). "And Not Anti-Realism Either." *Noûs* 18: 51-65. Reprinted in Fine 1986, 136-150.

——— (1986). *The Shaky Game: Einstein, Realism, and the Quantum Theory*. Chicago: University of Chicago Press.

Frank, Philipp (1953). "The Variety of Reasons for the Acceptance of Scientific Theories." In Frank ed. 1956, 13-26.

——— (1957). *Philosophy of Science: The Link Between Science and Philosophy*. Englewood Cliffs, NJ: Prentice-Hall.

Goodman, Nelson (1955). "The New Riddle of Induction." In *Fact, Fiction, and Forecast.* Cambridge, MA: Harvard University Press, 59-83. First delivered as a lecture at the University of London, May 26, 1953.

Hanson, Norwood Russell (1958). *Patterns of Discover: An Inquiry into the Conceptual Foundations of Science*. Cambridge: Cambridge University Press.

Howard, Don (1994). "Einstein, Kant, and the Origins of Logical Empiricism." In *Language, Logic, and the Structure of Scientific Theories: The Carnap-Reichenbach Centennial.* Wesley Salmon and Gereon Wolters, eds. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag, 45-105.

——— (1998). "Astride the Divided Line: Platonism, Empiricism, and Einstein's Epistemological Opportunism." In *Idealization in Contemporary Physics*. Niall Shanks, ed. Poznan Studies in the Philosophy of the Sciences and the Humanities, vol. 63. Amsterdam and Atlanta: Rodopi, 143-163.

——— (2000). "Vertrieben und Verirrt: Politics and the Philosophy of Science in Exile." Paper delivered at the Third International Conference on the History of the Philosophy of Science, Vienna, Austria, 6-9 July 2000.

——— (2003). "Two Left Turns Make a Right: On the Curious Political Career of North American Philosophy of Science at Mid-century." In *Logical Empiricism in North America*. Alan Richardson and Gary Hardcastle, eds. Minneapolis: University of Minnesota Press, 25-93.

Joravsky, David (1970). *The Lysenko Affair.* Cambridge, MA: Harvard University Press.

Kitcher, Philip (1985). *Vaulting Ambition: Sociobiology and the Quest for Human Nature.* Cambridge, MA: MIT Press.

——— (1993). *The Advancement of Science: Science without Legend, Objectivity without Illusions*. New York: Oxford University Press.

——— (1996). *The Lives to Come: Tthe Genetic Revolution and Human Possibilities*. New York: Simon & Schuster.

——— (1998a). "Reviving the Sociology of Science." *PSA 98. Proceedings of the 1998 Biennial Meeting of the Philosophy of Science Association. Part II, Symposia Papers*. Don Howard, ed. *Philosophy of Science* S67: S33-S44.

——— (1998b). "A Plea for Science Studies." In Koertge 1998, 32-56.

——— (2002). *Science, Truth, and Democracy*. New York: Oxford University Press.

Kitcher, Philip and Cartwright, Nancy (1996). "Science and Ethics: Reclaiming Some Neglected Questions." *Perspectives on Science: Historical, Philosophical, Social* 4: 145-153.

Koertge, Noretta, ed. (1998). *A House Built on Sand: Exposing Postmodernist Myths about Science.* New York: Oxford University Press.

Kuhn, Thomas S. (1962). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

——— (1969). "Postscript–1969." In *The Structure of Scientific Revolutions*, 2nd ed. Chicago: University of Chicago Press, 174-210.

——— (1973). "Objectivity, Value Judgment, and Theory Choice." Matchette Lecture delivered at Furman University, 30 November 1973. First published in: The Essential Tension: Selected Studies in Scientific Tradition and Change. Chicago: University of Chicago Press, 1977, 320-339.

Kusch, Martin (1995). *Psychologism: A Case Study in the Sociology of Philosophical Knowledge*. London and New York: Routledge.

——— (2002). *Knowledge by Agreement: The Programme of Communitarian Epistemology.* Oxford: Clarendon Press; New York: Oxford University Press.

Longino, Helen (1990). *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry.* Princeton, NJ: Princeton University Press.

——— (2002). *The Fate of Knowledge*. Princeton, NJ: Princeton University Press.

McMullin, Ernan (1982). “Values in Science.” In *PSA 1982*, vol. 2. P. D. Asquith and T. Nickles, eds. East Lansing, Michigan: Philosophy of Science Association, 3-28.

Merton, Robert K. (1938). “Science and the Social Order.” *Philosophy of Science* 5: 321-337.

——— (1942). “Science and Technology in a Democratic Social Order.” *Journal of Legal and Political Sociology* 1: 115-126.

Nemeth, Elizabeth (1981). *Otto Neurath und der Wiener Kreis. Revolutionäre Wissenschaftlichkeit als Anspruch*. Frankfurt and New York: Campus.

Nemeth, Elizabeth and Stadler, Friedrich, eds. (1996). *Encylopedia and Utopia: the Life and Work of Otto Neurath (1882–1945)*. Vienna Circle Institute Yearbook, no. 4. Dordrecht, Boston, and London: Kluwer.

Neurath, Otto (1913). “Die Verirrten des Cartesius und das Auxiliarmotiv. Zur Psychologie des Entschlusses.” *Jahrbuch der Philosophischen Gesellschaft an der Universität Wien*. Leipzig: Johann Ambrosius Barth. Page numbers and quotations from the English translation: “The Lost Wanderers of Descartes and the Auxiliary Motive (On the Psychology of Decision).” In Otto Neurath. *Philosophical Papers, 1913-1946*. Robert S. Cohen and Marie Neurath, ed. and trans. Vienna Circle Collection, vol. 16. Henk L. Mulder, Robert S. Cohen, and Brian McGuinness, eds. Dordrecht, Boston, and Lancaster: D. Reidel, 1983, 1-12.

———(1928). *Lebensgestaltung und Klassenkampf*. Berlin. E. Laub. Page numbers and translations from the excerpt translated as “Personal Life and Class Struggle” in Neurath 1973, 249-298.

——— (1932). “Protokollsätze.” *Erkenntnis* 3: 204-214. Page numbers and translations from the excerpt translated as “Protocol Sentences” in Neurath 1983, 91-99.

——— (1934). “Radikaler Physikalismus und ‚wirkliche Welt’.” *Erkenntnis* 4: 346-362. Page numbers and translations from the excerpt translated as “Radical Physicalism and the ‘Real World’” in Neurath 1983, 100-114.

——— (1935). “Einheit der Wissenschaft als Aufgabe.” *Erkenntnis* 5: 16-22. Page numbers and translations from the excerpt translated as “The Unity of Science as a Task” in Neurath 1983, 115-120.

——— (1973). *Empiricism and Sociology*. Marie Neurath and Robert S. Cohen, eds. Vienna Circle Collection, vol. 1. Dordrecht and Boston: D. Reidel.

——— (1983). *Philosophical Papers 1913-1946*. Robert S. Cohen and Marie Neurath, eds. and trans. Vienna Circle Collection, vol. 16. Dordrecht, Boston, and Lancaster: D. Reidel.

Popper, Karl Raimund. (1934). *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft*. Schriften zur wissenschaftlichen Weltauffassung, Philipp Frank and Moritz Schlick, eds. Vienna: Julius Spring.

Quine, W. V. O. (1951). "Two Dogmas of Empiricism'." *Philosophical Review* 60: 20-43. Reprinted in: *From a Logical Point of View*. Cambridge, MA: Harvard University Press, 1953, 20-46.

——— (1960). *Word & Object*. Cambridge: MIT Press.

——— (1969). "Epistemology Naturalized." In *Ontological Relativity and Other Essays*. New York and London: Columbia University Press, 69-90.

——— (1981). "Things and Their Place in Theories." In *Theories and Things*. Cambridge, Massachusetts and London: Harvard University Press, 1-23.

Reichenbach, Hans (1928). *Philosophie der Raum-Zeit-Lehre*. Berlin: Julius Springer..

——— (1938). *Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge.* Chicago: University of Chicago Press.

——— (1951). *The Rise of Scientific Philosophy*. Berkeley and Los Angeles: University of California Press.

Reisch, George (1991). "Did Kuhn Kill Logical Empiricism?" *Philosophy of Science* 58: 264-277.

——— (1995). "A History of the *International Encyclopedia of Unified Science*." Ph.D. Dissertation. University of Chicago.

Schlick, Moritz (1934). "Ueber das Fundament der Erkenntnis." *Erkenntnis* 4: 79-99.

——— (1935). "Sind die Naturgesetze Konventionen?" In *Actes du Congrès International de Philosophie Scientifique, Paris 1935*. Vol. 4, *Induction et Probabilité*. Actualités Scientifique et Industrielles, no. 391. Paris: Hermann, 1936, 8-17.

Shrader-Frechette, Kristin (1985). *Risk Analysis and Scientific Method: Methodological and Ethical Problems with Evaluating Societal Hazards*. Dordrecht and Boston: D. Reidel.

——— (1991). *Risk and Rationality: Philosophical Foundations for Populist Reforms*. Berkeley: University of California Press.

——— (1993). *Burying Uncertainty: Risk and the Case against Geological Disposal of Nuclear Waste*. Berkeley: University of California Press.

Solomon, Miriam (2001). *Social Empiricism*. Cambridge, MA: MIT Press.

Stadler, Friedrich (1997). *Studien zum Wiener Kreis. Ursprung, Entwicklung und Wirkung des Logischen Empirismus im Kontext*. Frankfurt: Suhrkamp. English translation: *The Vienna Circle: Studies in the Origins, Development, and Influence of Logical Empiricism.* Vienna and New York: Springer-Verlag, 2001.

Stump, David and Galison, Peter, eds. (1996). *The Disunity of Science: Boundaries, Contexts, and Power*. Stanford, CA: Stanford University Press.

Toulmin, Stephen (1953). *The Philosophy of Science: An Introduction*. London: Hutchinson & Co.

——— (1961). *Foresight and Understanding: An Enquiry into the Aims of Science*. Bloomington, IN: Indiana University Press.

——— (1972) *Human Understanding*, vol. 1. Princeton, NJ: Princeton University Press.

Uebel, Thomas E. (1992). *Overcoming Logical Positivism from Within: The Emergence of Neurath's Naturalism in the Vienna Circle's Protocol Sentence Debate.* Amsterdam and Atlanta: Rodopi.

Uebel, Thomas E., ed. (1991). *Rediscovering the Forgotten Vienna Circle: Austrian Studies on Otto Neurath and the Vienna Circle.* Boston Studies in the Philosophy of Science, vol. 133. Dordrecht, Boston, and Lancaster: Kluwer.

van Fraassen, Bas (2002). *The Empirical Stance.* New Haven: Yale University Press.

Wang, Jessica (1999). "Merton's Shadow: Perspectives on Science and Democracy since 1940." *Historical Studies in the Physical and Biological Sciences* 30, 279-306.

Zhai, Zhenming (1990). "The Problem of Protocol Statements and Schlick's Concept of 'Konstatierungen.'" *PSA 1990: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, vol. 1. East Lansing, MI: Philosophy of Science Association, 15-23.