

Introduction
Logical Empiricism in North America

Alan W. Richardson and Gary L. Hardcastle

MINNESOTA STUDIES IN THE PHILOSOPHY OF SCIENCE

VOLUME XVIII

Logical Empiricism
in North America

GARY L. HARDCASTLE
AND
ALAN W. RICHARDSON, EDITORS

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**LOGICAL
EMPIRICISM
IN NORTH
AMERICA**



*Gary L. Hardcastle and
Alan W. Richardson, Editors*

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PHILOSOPHY OF SCIENCE

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GARY L. HARDCASTLE

AND

ALAN W. RICHARDSON, EDITORS



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Introduction

Logical Empiricism in North America

Since the 1980s, the philosophy of science has taken a historical turn. We do not refer to the attention philosophers of science have paid to rich historical accounts of scientific episodes, a turn often taken to have been motivated by Thomas Kuhn's *Structure of Scientific Revolutions* ([1962] 1996) and to have importantly transformed philosophy of science. We refer, rather, to a more recent but equally significant development, in which philosophers of science have begun to recover the problems, solutions, and motivations of earlier projects in the *philosophy of science*, paying attention especially to how the historical figures engaged in these projects understood them.¹ Crucially, this work aims not to *disconnect* such historical projects from contemporary issues in philosophy of science but to *reconnect* contemporary philosophy of science with its history in a new way. Adapting what is perhaps the most famous sentence in the philosophy of science of the second half of the twentieth century, we can assert that the history of the philosophy of science is coming to be viewed as more than a repository for anecdote or chronology, and can, if we allow it, produce a decisive transformation in the *philosophy of science* we now possess.

This volume, *Logical Empiricism in North America (LENA)*, is a contribution to this historical turn in philosophy of science. It contains essays that take up, in one way or another, the historical, sociological, and philosophical questions surrounding the particular intellectual movement of logical empiricism, both its emigration from Europe to North America in the 1930s and 1940s and its development in North America through the 1940s and 1950s. Although conceived as a companion to an earlier volume in the series Minnesota Studies in the Philosophy of Science, *Origins of Logical Empiricism (OLE, 1996)*, *LENA* can be read independently of *OLE*. In this introduction, we explore, explain, and promote the historical turn in philosophy of science that is represented and reflected in this volume. We also relate the volume's contents to various larger issues in history of philosophy of science, issues that promise to transform philosophy of science itself.

The Historical Turn and Reformation

What has motivated this historical turn in the philosophy of science? Why have philosophers of science begun examining the history of philosophy of science in the way they have? Professional stagnation comes to mind as a possible answer. As Don Howard notes in chapter 2, the philosophy of science is not a leading (or even, perhaps, a growing) field in today's academic world. Within the general learned culture, philosophy of science is not even currently the most widely respected arena of reflection on science. Other branches of science studies—sociology of science, social history of science, and cultural studies of science, for example—are more widely read and debated among those interested in the study of science as a human practice. Perhaps, then, it is from the perspective of such doldrums that some philosophers of science are looking outward for new topics, methods, tools, and skills and looking, therefore, to historical figures in philosophy who concerned themselves with science. Considerable attention *is* being paid by philosophers of science, after all, to acknowledged historical figures (such as Hermann von Helmholtz, Charles S. Peirce, René Descartes, Henri Poincaré, Rudolf Carnap, Karl Popper, Immanuel Kant, and Hans Reichenbach), and lesser-known figures (such as Johann Friedrich Fries, Alois Riehl, and Hermann Cohen) are getting a first look. Perhaps this is all an attempt by philosophers of science to reinvigorate their discipline.

Attention to historical figures in philosophy of science is not novel, of course. For example, a typical training regime in the discipline includes exposure to the canonical issues—confirmation, explanation, the nature of theories, the empirical meaning of theoretical claims, the ontological status of theoretical entities, intertheoretical reduction—and this is often combined with some attention to what historically important philosophers said about those topics. Occasionally (perhaps in Peirce or William Whewell) unanticipated resources might be found for thinking through these issues; more often, one is invited to use an important historical figure (such as Carnap or Karl Pearson) as a whipping boy or dialectical opponent. In either case, however, the student in philosophy comes to the historical texts confident of already knowing the canonical philosophical issues surrounding science.

But when the canonical issues seem either misconceived or simply exhausted—when philosophy of science seems intellectually or professionally stagnant—the historical figures are read differently. When philosophers are fundamentally unsure of the philosophical project that ought to be associated with scientific explanation, for example, they are inclined

to read, say, Emile Meyerson's *Identity and Reality* ([1908] 1962) not to find out what his account of scientific explanation was but to help with a rich set of concerns concerning the philosophical project of understanding scientific explanation. They will ask: Why did Meyerson have an account of scientific explanation at all? What resources did he employ in giving one? What relation did that account have to Meyerson's other concerns in philosophy of science? What scientific theories did he use as his explanatory exemplars or marshal as resources for his own work? To take another example, one can read the Marburg neo-Kantians not simply to find out what they thought were the foundations of exact science but to find out what they thought was the philosophical import of the task of giving the foundations of exact science. In these cases, the historical figures no longer simply provide views on the canonical topics or texts to think our way through and beyond. They provide philosophical projects to think *with*. The more stagnant the contemporary philosophical situation, the more interest we would expect in the historical figures, since these now appear as exemplars of fresh philosophical projects that we might *in some way* be able to take up and extend.

As a result of their contextualism and historicism, moreover, such historical approaches do not (and indeed, cannot) devolve into crude "back to X" movements, for any historical X. What such accounts show, indeed, is that we *cannot* go back to Kant, Helmholtz, Carnap, or Popper. Our philosophical and scientific world is not theirs. Nevertheless, the deepest issues in the philosophy of science are sufficiently open that we *can* still learn important lessons from these figures, especially regarding what it is to articulate a new philosophical project concerning science. There is an important difference between going "back to Kant" and going forward by keeping Kant firmly in mind.

No doubt a good deal of the work in this volume looks to historical figures for just these reasons (see, for example, the chapters by Howard, Thomas Uebel, and Alan Richardson). But this is not the only motivation for the historical turn in the philosophy of science. A philosopher confidently ensconced in one or another ongoing living enterprise in the philosophy of science (even one that appears entirely ahistorical) still needs to connect his or her enterprise with philosophical projects of the past, and that requires work in the history of philosophy of science. As Alasdair MacIntyre (1984) and others (see, for example, Wilson 1992) have argued, philosophy in general is deeply historical, even when it expresses itself in a completely antihistorical fashion; there is simply no way to claim that one's interests are philosophical without finding some tradition of philosophy into which they fit. Thus W. V. O. Quine, although famous for erecting

a distinction between philosophers and historians of philosophy, always in his own accounts embeds his philosophical projects in a well-worked-out story of “the empiricist tradition” (see, for example, Quine 1969, 1981, 1995). Even extreme philosophical revolutionaries have to find a way to tie their work to *some* philosophical tradition, on pain of being seen simply as having changed the topic or as having missed the point. A physicist, a counselor, a thief, or a gardener cannot simply *declare* him- or herself a philosopher. It is little wonder that some of the most effective revolutionaries in philosophy attempt less to argue against previous ways of doing philosophy than to “overcome,” “deflate,” or “turn away from” them. Here, at least, traditions of philosophy are not revealed as simply mistaken so much as interestingly and importantly misconceived and thus useful, at least as signs of roads no longer to be taken.

Whether philosophy of science is currently in crisis or not, then, philosophers of science can find ample justification for the historical turn that has in fact emerged in the philosophy of science. And although the scope of philosophy of science extends far beyond logical empiricism, it is no surprise that logical empiricism has been of particular interest to contemporary philosophers of science: It is, after all, not just a major part of the intellectual puzzle of the twentieth century but, for many philosophers of science, the core of our philosophical heritage. And with two decades of serious work in the history of logical empiricism behind us and with an active and well-established center for this work in the Vienna Circle Institute at the University of Vienna, a number of philosophical, historical, and historiographic issues are emerging. In the following section we will describe three such issues that, in one way or another, run through all the pieces in this volume. But first, we will quickly summarize the volume’s eleven chapters.

The volume’s first two chapters, Richardson’s “Logical Empiricism, American Pragmatism, and the Fate of Scientific Philosophy in North America” and Howard’s “Two Left Turns Make a Right: On the Curious Political Career of North American Philosophy of Science at Midcentury,” address the history of logical empiricism in general ways and in terms of general themes. Although Richardson and Howard each argue for specific and provocative theses, their chapters also serve to introduce those new to the historical work surrounding logical empiricism to the set of figures, movements, and research problems currently on the table. Richardson, for example, raises the question of logical empiricism’s relation to North American pragmatism. Simple characterizations of this question invite overly simple solutions: Logical empiricism *replaced* pragmatism, we might be inclined to say, and it did so because it solved a greater range of philosophical problems, because it was truer, or perhaps just because it was (at the

time) more promising. In place of a simple formulation of the question, Richardson argues instead that logical empiricism and pragmatism were of a piece, that piece being scientific philosophy. Notably, in the course of his argument, Richardson brings to the fore Charles Morris, a figure many contemporary philosophers of science may view as only marginal to the logical empiricist program. Such a recovery of figures who are marginal by our present lights is indeed a theme of recent work in the history of philosophy of science, and one much in evidence in *LENA*.

Howard's extensive analysis of the complex philosophical and historical relationship among philosophy of science, politics, and political life introduces readers to a different but equally significant set of issues in the history of logical empiricism. Noting that "there was rather more politics in prewar philosophy of science than our contemporary image of the discipline usually acknowledges," Howard asks how it is that the philosophy of science became politically *disengaged* in the course of its professionalization (a disengagement Howard himself characterizes as "tragic") and how and why the political engagement of our predecessors was obscured in early histories of logical empiricism. Against the background of political histories of both the Vienna Circle and the journal *Philosophy of Science*, Howard identifies the lack of a "successor paradigm" to logical empiricism and, ultimately, the "loss of the sense of a cultural, social, and political mission" that philosophy of science ought to have as the chief causes of the discipline's political disengagement. Reengagement, Howard suggests, might take place via a reconsideration of "the naturalism of Neurath and Dewey."

Richardson's and Howard's respective essays set the stage for the four chapters that follow, each of which focuses on a figure significant to logical empiricism. Philosophers of science trained since the 1970s will readily and rightly associate the name of Carl G. Hempel with the movement, and they are furthermore likely to characterize his intellectual development over several decades as proceeding *from* logical empiricism and *toward* a view sympathetic to Kuhn's, a trajectory that culminated in his emphasis on "provisoes" in science (Hempel 1988). In a demonstration of how work in the history of logical empiricism can lead to revisions in its "received history," Michael Friedman argues in chapter 3, "Hempel and the Vienna Circle," that Hempel's later pragmatic and naturalistic views in fact had their roots in Hempel's earliest thinking, specifically, in his sympathy with Otto Neurath's position in the Vienna Circle's protocol-sentence debate of the 1930s. In a similar vein, Rudolf Haller's "On Herbert Feigl," chapter 4, reminds us that Herbert Feigl, a young member of the Vienna Circle and among the first of its members to emigrate permanently to the United States (Moritz Schlick had visited Stanford in 1929, a year before Feigl

moved to Cambridge, Massachusetts), defended philosophical views more often associated with a later time, including a view of theories that emphasizes the extent to which they are “free constructions” and a conviction that probability plays a central role in science.

If the stories of Hempel and Feigl are stories of professional success (to which we could add the stories of Carnap, Reichenbach, and several other émigré logical empiricists), Diederick Raven’s contribution to *LENA*, chapter 5, “Edgar Zilsel in America,” reminds us that not all such emigrations were successful. After recounting Zilsel’s life in Europe and the United States (culminating in his self-inflicted death in 1944), Raven attends to the specific and complicated matter of what went wrong for Zilsel and to the more general matter of what his trajectory can tell us about the philosophical and historical dimensions of logical empiricism in North America. In all, these three chapters remind us that the success of the logical empiricists in North America (as well as significant aspects of their philosophical views) was a contingent matter.

The twin issues of contingency and success recur in Thomas Uebel’s chapter 6, “Philipp Frank’s History of the Vienna Circle: A Programmatic Perspective.” Via a comparison of two instances in which Frank told the story of the Vienna Circle and logical empiricism (first in his 1941 *Between Physics and Philosophy* and eight years later in his *Modern Science and Its Philosophy*), Uebel argues that Frank strove, without success, to carve a place for the social-historical concerns that were championed by Neurath but that were not well represented after Neurath’s 1945 death. In the process, we are led not just to Frank’s role in logical empiricism in North America but to Neurath’s as well.

The history of logical empiricism in North America is a history not just of individuals but of cooperative ventures. *LENA*’s next two chapters take up separate cooperative efforts of some significance to logical empiricism. In chapter 7, “Debabelizing Science: The Harvard Science of Science Discussion Group, 1940–41,” Gary Hardcastle recounts the workings of the short-lived Harvard Science of Science Discussion Group (SSDG) and argues that the SSDG reflected a commitment to a particular notion of scientific unity, one best associated with Neurath. Although the group lasted just one academic year, the threads it shared with Frank’s later Inter-Science Discussion Group and ultimately with the Institute for the Unity of Science suggest that an important aspect of Neurath’s thinking did, in fact, make it to North America. In chapter 8, “Disunity in the *International Encyclopedia of Unified Sciences*,” George Reisch gives a detailed history of logical empiricism’s most prominent cooperative effort, the *International Encyclopedia of Unified Science (IEUS)*. He documents the somewhat ironic disunity between the *IEUS*’s editors, Carnap, Neurath, and Morris,

and employs this tension (among others) to explain why the *IEUS* never realized Neurath's extensive plans for it. In Reisch's hands, this example serves to introduce a dispute between what Reisch calls "large-large" and "small-large" (or as Reisch points out, "Neurathian") explanations in the history of philosophical movements. For Reisch, the story of the *IEUS* is small-large; it is the story of specific people and the decisions they made.

The logical empiricists were, of course, not the only intellectuals forced to flee Europe in the 1930s. Friedrich Stadler's chapter 9, "Transfer and Transformation of Logical Empiricism: Quantitative and Qualitative Aspects," applies the framework of emigration studies to understand logical empiricism. After establishing that the emigration of the Vienna Circle was "manifold and conflicting, involving both success and failure, acculturation and disintegration, diffusion and isolation," Stadler explores a variety of perspectives that might be brought to bear on the emigration and suggests that these perspectives themselves are continuous with philosophy of science.

For many, the most challenging philosophical issue raised by logical empiricism is analyticity. Indeed, if one were asked to locate a point around which logical empiricism has seemed to turn, historically and philosophically, one would be well-advised to select Carnap and Quine's 1950s debate over this very topic. *LENA*'s final two chapters, Richard Creath's "The Linguistic Doctrine and Conventionality: The Main Argument in 'Carnap and Logical Truth'" (chapter 10), and Thomas Ricketts's "Languages and Calculi" (chapter 11), take up the Quine-Carnap debate. Each underscores analyticity's central role in logical empiricism while suggesting that the issue was not, *pace* popular opinion, fruitfully engaged by Quine and Carnap. Creath, for example, explores an early argument of Quine's against the "linguistic doctrine of logical truth," one that has received less attention than the others in Quine's well-known "Two Dogmas of Empiricism" (1951) but that forms, Creath argues, the "basis of much of Quine's subsequent writing." By thinking carefully about Quine's reliance on the claim that logic is "obvious," Creath stakes out two deeply different epistemic perspectives associated with Quine and with Carnap, and he argues that Quine does not argue for his picture so much as presume it. In "Languages and Calculi" Ricketts engages this matter from Carnap's perspective rather than Quine's. After tracing Carnap's account of analyticity as it applies especially to mathematics, Ricketts locates a deep contrast between Quine and Carnap over the relationship of logic to languages, artificial or natural.

LENA thus presents a broad array of work in the history of logical empiricism. Taken altogether, this work raises a number of philosophical and methodological issues, to which we now turn.

Boundary Work, Philosophical Schools, and the Social History of Philosophy

The work represented in *LENA* (indeed, all work in the history of logical empiricism) apparently presupposes that there is something named by the term ‘logical empiricism’. This immediately raises questions about the characteristics, family resemblances, boundaries, and so on of this philosophical school. The canonical logical empiricists—the members of the Vienna Circle, Reichenbach, Hempel—did not, however, all agree on very many things, and the search for defining features or family resemblances has generally been fruitless. Even the movement’s name is vexed: Reichenbach wielded ‘logical empiricism’ in the 1930s in opposition to the ‘logical positivism’ (which was Feigl’s term; see chapter 4) of the Vienna Circle, while Schlick came to prefer to call his philosophy ‘consistent empiricism’, and Neurath occasionally flirted with ‘scientific rationalism’. Moreover, the boundaries of the movement were porous and contested even at the time: Persons now not canonically understood to be logical empiricists, such as Morris, took themselves, and were taken by various others, to be inside the movement (for more on Morris, including a few caveats about his place in the movement, see chapter 8). Others, such as C. I. Lewis, officially distanced themselves from logical empiricism but were nevertheless widely regarded as promoting notions that were certainly within the family of views appropriately denominated as logical empiricist (on Lewis, see chapters 1 and 4; and see chapter 7 for yet other figures at the margins of logical empiricism). There are problems, then, when it comes to fixing the subject matter of the history of logical empiricism in North America. Philosophical, sociological, and historical issues are bound together.

By way of increasing the difficulties surrounding these issues, consider the question of the relations of philosophical schools to each other in the 1930s. Investigating the historical relations of logical empiricism and American pragmatism, for example, requires some sense of who the pragmatists in the 1930s were and what pragmatism in the 1930s was. But, as Richardson documents in chapter 1, these are hard questions. Morris had the appropriate pedigree and claimed to be a pragmatist; Lewis, also, had both pragmatist pedigree and self-identification. Ernest Nagel is *now* understood as a sort of pragmatist, but in the 1930s his views were more closely related to the philosophical position of Morris R. Cohen—a sort of naturalistic realism that found fault with John Dewey’s pragmatist positions (see Cohen and Nagel 1934). If we *assume* that pragmatism and posi-

tivism were opposed, we can write the history of American philosophy in the 1930s and 1940s as pragmatism's betrayal by Morris, Nagel, and other young American philosophers (see Giere 1996). Richardson here rejects that assumption, though, and attempts to write the history as it was understood by both the logical empiricists and the pragmatists while it was happening. He finds a higher level, the level of "scientific philosophy," in which the coming together of the projects makes sense. Howard, in chapter 2, reminds us of some of the costs to both projects resulting from their combination and calls our attention to some remaining political tensions between the projects. Reisch, in chapter 8, meanwhile reminds us of some of Morris's second thoughts about logical empiricism.

These issues are going to sort themselves out only as philosophers become more adept at social and institutional history. There may not be—in fact, we conjecture that there *is* not—any deep philosophical or conceptual coherence to movements as broad as American pragmatism or logical empiricism. By way of illustration, consider the name of the philosophical program the readers of (and contributors to) this volume are most likely to describe themselves as engaged in; that is, consider 'analytic philosophy'. The term is philosophically opaque. 'Analytic philosophy' denotes a social structure, a group held together not by any substantial philosophical commitments—there is no one metaphysical, epistemological, ethical, or even metaphilosophical project in analytic philosophy—but by an amorphous group of issues, texts, and canonical historical interpretations of great figures in the history of philosophy and, similarly, by a rather murky list of issues, texts, and figures it *excludes*.² Most of this volume's readers would find it philosophically comforting if we could locate a deep coherence to logical empiricism, pragmatism, or (especially) analytic philosophy, but it simply does not seem to be in the cards. As a way of going forward, philosophically, though, we can recognize that this desire for coherence is itself a dimension of analytic philosophy, and we can seek to understand where it came from and why it is ours.

In this regard, the best current tool for understanding 'analytic philosophy' must surely be sociology of knowledge, especially the notion of boundary work (Gieryn 1999). From this perspective, 'analytic philosophy' as a term is used principally for boundary work, and it acquires its meaning in that use. Consider analytic philosophers of mind. They use the term '*analytic* philosophy of mind' in order to distance themselves from "Continental" concerns with mind or with subjectivity, and in so doing they claim for themselves virtues such as intellectual rigor, attention to logical argument, connection to the current sciences of mind, and hardheaded empiricism in order to promote their projects at the expense

of phenomenology, psychoanalysis, and other expressions of Continental tender-mindedness. They will, however, use ‘analytic *philosophy* of mind’ in a different way, namely, to set themselves off from *scientists* of mind, and here they will make much of their place in the tradition of philosophical issues regarding mind (the mind-body problem, the subjective nature of conscious states), their connection to historical figures in philosophy, and, finally, their own distinctive interest in and interpretation of the empirical results of the neurosciences. Arguments aimed at policing these different boundaries cannot be easily combined into one global account of the project of “analytic philosophy of mind,” for reasons Thomas Gieryn (1999) notes and that any rhetorician would point out: The arguments on behalf of “analytic *philosophy* of mind” move the project in the direction of philosophy generally and thus are not easily employed against “Continental *philosophy* of mind.” Conversely, arguments for “*analytic* philosophy of mind” move the project in the direction of scientific accounts of mind and cannot be easily employed against neuroscience. In general, one’s reasons for doing *X* rather than *Y* are typically different from one’s reason for doing *X* rather than *Z*. This fact becomes problematic only in contexts where there is a drive to give a complete list of “the reasons for doing *X*” and present those reasons as the defining features of *X*.³

To return to logical empiricism and American pragmatism, we offer the following suggestion: We are not going to understand logical empiricism, pragmatism, or their relation until we ask questions characteristic of social history. We need to know why terms like ‘pragmatism’ or ‘logical empiricism’ arose in American philosophy, what those terms were introduced to do, and how they came to be banners under which various philosophers gathered. We need to know what sorts of techniques, knowledge, and skills these terms were taken as characteristic of, how they were taught or transferred, how the various contrasts and commonalities changed over time, why they sometimes go out of philosophical discourse, and, finally, why they reemerge in new contexts. We have to seek answers to these questions by looking in detail into the places, times, and contexts in which these things happened, and happened as actions of human beings. The story of analytic philosophy in North America is the story of the appearance, disappearance, or reappearance of crucial philosophical terms, among them ‘pragmatism’, ‘naturalism’, ‘metaphysics’, and ‘the a priori’.⁴ Some of this work is represented in this volume, but there is much more to do. In general, Lorraine Daston’s (1994) and Arnold Davidson’s (2001) “historical epistemology” could find no more fertile field than that offered by recent history of analytic philosophy and philosophy of science.⁵

Philosophical Schools and Their Margins

This volume takes a broader view of logical empiricism than is usual. It contains discussions of, for example, Morris, Frank, Zilsel, and Harvard's SSDG. It also treats of canonical figures such as Feigl, Hempel, and Carnap. Behind this is an interesting tension. The volume seeks to open up new understandings of logical empiricism by attending to the work of logical empiricists who are not now considered to be central figures while at the same time accounting for the historical fact that in logical empiricism some figures and not others came to be canonical. Why, for example, did the more technical work of Carnap, Reichenbach, and Hempel come to capture the attention of the philosophical community and come to be taken as the core of logical empiricist philosophy of science while, for example, Frank's more historical and cultural understanding of philosophy of science came to be more marginalized within it?⁶ This volume contains some partial—and partially competing—answers to this question in particular. For Howard, the construction of the logical empiricist canon depends on professionalization and specialization. Reisch suggests, on the other hand, that Frank simply did not have the local resources to become Neurath's successor at the helm of the Unity of Science movement. Uebel argues that there was a rather important subgroup within the left wing of the Vienna Circle, one that was rooted in the "first Vienna Circle" of the pre-World War I era (of which Frank was a member), and that Frank's efforts to restore this group's project in America in the 1950s failed.

The availability of several answers to the question of Frank's apparent marginalization in the United States in the 1950s suggests the not very interesting fact that the scholarly community does not yet know why it happened. There are, however, more interesting issues. Howard, Uebel, and Reisch all seek to broaden our understanding of the range of positions taken within logical empiricism, but they all suggest that that wider range of positions cannot be combined into a coherent program the way the logical empiricists had hoped. Yes, these accounts claim, Neurath and Frank were logical empiricists, and so were Carnap and Reichenbach, but the moral to be drawn is that *the movement was deeply divided and not philosophically coherent from the start*. In retrospect, philosophy of science proceeded under the influence of the Carnap-Reichenbach wing of logical empiricism. Howard, Uebel, and Reisch speculate about a short-lived philosophy of science that attended (or attends) to the Frank-Neurath wing, and they wonder why its life was not longer.

There are other ways to tell the story. One could argue, as Peter Galison (1998) has, that Frank was caught up after World War II in a new notion

of scientific unity and was in fact promoting, under the rubric “unity of science,” a *different* project from any the logical empiricists had promoted before the war. Or one might attempt to argue that Neurath and Frank, to the end of their lives, took their main philosophical mission to be the promotion of the “scientific world conception,” whereas within the American context of the 1940s and 1950s, Carnap, Reichenbach, Hempel, and Feigl all came to do work inside that conception, no longer expending much effort arguing for it. On such an account, Frank finds a place within a certain division of labor in logical empiricism, a division that explains why he did not do much of the detailed work in philosophy of science that the students of Carnap, Reichenbach, and Hempel would find important or useful. Frank’s philosophical task, on this account, concerned *arguing for* something that the mainstream of philosophers of science came simply to take for granted.

The point is not simply to proliferate narratives but to point to a crucial aspect of historical work in philosophy of science. Which narrative, if any, comes to be accepted regarding the marginalization of Frank and Neurath in philosophy of science will inform future understanding of who Frank and Neurath were as philosophers. On some narratives, they provide the road not taken (at least not taken yet) in philosophy of science. On other narratives, they are an older generation whose philosophical work paved the way for Reichenbach and Carnap and who were naturally superseded by the more mature and technical philosophy of science they made possible. On yet other narratives, Frank and Neurath were forward-looking and creative thinkers who constantly remade the Unity of Science movement. The narratives constructed will set the conditions under which Frank’s and Neurath’s work will be approached and used in the coming years. We write them back into the history of philosophy of science by writing stories of how they were written out of it. And depending on how we do that, they look like very different philosophers.

Attending to such figures as Frank, Morris, and Zilsel raises further questions surrounding the facts our historical accounts are expected to explain. Is Morris a key figure in the demise of American pragmatism at the hands of logical empiricism? Are Zilsel and Frank figures whose life histories indicate that not all European logical empiricism was moved successfully to North America? Was logical empiricism already insulated from science in the 1930s, or was it deeply engaged with science right through the 1950s?

Here, again, Frank is an interesting figure to think with. Arguably, Frank was no marginal figure in the American academic scene after World War II. Frank organized and participated in various activities with important long-term effects in philosophy of science, including laying the foundation for

the Boston Colloquium for Philosophy of Science and the series Boston Studies in Philosophy of Science (see chapter 8). Frank organized and participated in many influential conferences, including the Conferences on Science, Religion, and Philosophy held in the 1940s and 1950s, several conferences on science education, his own conference on the validation of scientific theories, and the 1955 Conference on History, Philosophy, and Sociology of Science sponsored jointly by the American Philosophical Society (APS) and the National Science Foundation (NSF).⁷ Only one other philosopher of science in America appears to have been present in such venues in anything like this frequency: Ernest Nagel.

But how are we to construe these facts? Are these the activities of a man who had become alienated from professionalized logical empiricist philosophy of science and who, in light of these activities, was rendered marginal in philosophy of science? Or are these the actions of a leading logical empiricist, actions that give the lie to any claim that logical empiricism was disconnected from history and sociology of science and from the larger social and cultural contexts of science and its philosophy?

These questions are important because, among other things, Frank, his courses in philosophy of science (which he taught in the Physics Department at Harvard), and his books were well known to James B. Conant and, at least through Conant, to the young Thomas Kuhn. He was also well known to George Sarton, a colleague of Percy Bridgman, and a key mentor of Gerald Holton, Marx Wartofsky, and Robert S. Cohen. Frank was, that is to say, the member of the Vienna Circle who worked most closely with physicists, historians of science, and sociologists of science in the American context. It is enormously important to figure out, therefore, whether his relations with those groups were part of a *turning from* logical empiricism or part of a *commitment to* logical empiricism. The issue is central to understanding the place of logical empiricism not merely in the history of philosophy of science and not merely in the history of philosophy but in the history of our culture's twentieth-century attempts to understand science as a human activity.

How do we decide these issues? With painstaking and subtle historical work, of course. We must, we propose, determine whether Frank understood himself to be acting as a logical empiricist in undertaking his work and whether he was understood by his colleagues and readers—both those who described themselves as logical empiricists and those who did not—as a logical empiricist. On the first issue, there is good reason to believe that through the end of his life, Frank understood himself to be a logical empiricist. As chapter 6 argues, Frank's revisionary histories were in support of an alternative vision of logical empiricism, not in support of overthrowing, transcending, or renouncing logical empiricism. As late as *Relativity*:

A Richer Truth (1950), Frank seems both to be presenting logical empiricist doctrines and presenting them *as* logical empiricist doctrines. The book is one of those in which strong connections are made between logical empiricism, operationalism, and pragmatism, and Frank mentions all three movements approvingly and by name. Indeed, all are named in the titles of chapters of the book: pragmatism in chapter 5, operationalism in chapter 6, and logical empiricism in chapter 7.

The evidence is, admittedly, more ambiguous in Frank's 1957 textbook, *Philosophy of Science: The Link between Science and Philosophy*. Much of what Frank says in the book is unobjectionable from the point of view of Carnapian or Reichenbachian logical empiricism, and Frank cites Carnap, Reichenbach, and Richard von Mises with greater frequency than he cites any other twentieth-century philosophers. But here Frank rarely, if ever, uses the term 'logical empiricism', and his historical concerns seem more aligned with Conant's Harvard Case Histories in Experimental Science, the key texts in the Harvard General Science Education program as taught by Conant, Kuhn, Leonard Nash, and others, than with the technical concerns of Carnap or Reichenbach in the 1950s. Moreover, Frank hinted at dissatisfaction with recent logical empiricist work in the book's preface:

Presentations in [philosophy of science] have very often started from a concept of science that is half vulgar and half mystical. Other presentations have linked science with a philosophy that has actually been a mere system of logical symbols without contact with the historical systems of philosophy. But these very philosophies have served as support for ways of life and, specifically, for religious and political creeds. (1957, iv)

The import of this statement becomes clear only at the end of the book, when Frank talks of "extrascientific" reasons for the acceptance of "scientific theories of high generality" (342–60). Frank there employs underdetermination to argue that the historical philosophies have provided "extrascientific" reasons for the acceptance of high-level theories, such as theories of causality. Thus, the historical philosophies have to be at least a topic for philosophy of science, which Frank conceived of as a part of a general science of science that includes a "sociology of science" or a consideration of the "humanistic background of science" (Frank 1957, 359).⁸

Regarding the second issue—the reception of Frank's work—we note that a preliminary reading of some reviews of Frank's books indicates that he was understood to be a logical empiricist, even a vulgar logical empiricist, but, more interestingly, that logical empiricism during its alleged era of philosophical preeminence received plenty of bad reviews in important journals. Frank was routinely condescended to by his reviewers, and his *Relativity: A Richer Truth* was especially pilloried. A young Stephen

Toulmin thought the book naive, writing that “its sub-title would perhaps best be: Logical Empiricism told to Children” (1951, 181), and A. P. Ushenko used his review to condemn the whole of logical empiricism:

I am urging students to read the book on account of Part One. . . . because its simplicity and clarity of presentation exposes the inadequacy and confusion of the author’s philosophical affiliations where the more technical writings are protected by a camouflage of symbolic notation or pedantic belaboring of detail. (1951, 587)

These reviews suggest a corrective to any view that logical empiricism dominated American philosophy in the 1940s and 1950s. Ushenko’s review even announces a “decline in [the United States]” of logical empiricism (587). A preliminary reading of some of the reviews of Frank’s books suggests that logical empiricism was not only not accepted but was, at least at times, deeply resisted and even resented by philosophers working in North America in the 1940s and 1950s.

Narrative Structure and Historical Explanation in Philosophy

The historical work in logical empiricism has also raised large historiographic issues, two of which we will mention here. The first has already been exemplified in some of the concerns raised; the second is raised explicitly by Reisch in chapter 8 in his remarks on Peter Galison’s 1996 “Constructing Modernism.”

Consider the terms in which historical accounts in philosophy, especially philosophy of science, are often given. The terms in which the career of logical empiricism is treated seem taken from an inquest: “When did it die and how? Was there foul play?” Richardson took the death of logical empiricism to be “a social fact of analytic philosophy” and cited as reasons for this view the further fact that “few, if any, consider themselves to be continuing the project” of logical empiricism (1996, 13 n. 4). But such comments do not suffice to explain why the use of “death” is helpful in historical investigation of discontinued social practices or intellectual movements. We think of logical empiricism as having a life cycle—it is born, it flourishes, and it dies—but that may be the *wrong* way to think, especially if it is embedded in a Hobbesian discourse of the war of all against all, where a philosophical project flourishes at the expense of other projects and dies at the hands of younger, more vital projects.

A variation on the death theme is told in terms of failure rather than

death. In this variant the question becomes, Why did logical empiricism fail? George Reisch in chapter 8 speaks in terms of the failure of the *International Encyclopedia of Unified Science*, and he further articulates what he means by saying that the failure of the project is captured by “the facts that the project never recaptured the success it enjoyed before the war and that only . . . twenty monographs appeared” even though the original plan expected hundreds of monographs. Failure to achieve one’s goals *is* failure of a sort; we are rightly sick and tired of hearing sports commentators tell us that the Canadian competitor, who had set his goal to come in first in the slalom, had great success despite coming in forty-seventh. It is not at all clear, however, that the *Encyclopedia* was or is *less* influential or important for comprising only twenty monographs rather than Neurath’s originally envisioned hundreds. A solid and lean *Encyclopedia* is much more likely to have actually been read than would have been the bloated monster of Neurath’s dreams. As anyone who has written a Ph.D. thesis can attest, sometimes one avoids failure and achieves a modicum of success precisely by setting aside one’s early ambitions.⁹

Leaving aside the particular case of logical empiricism, the more general question is whether we really want to have a history of philosophy in which the historically given philosophical projects or schools either continue to inform (and are known to continue to inform) current philosophical practice or, if not, have died or failed. It is, after all, an unusual narrative structure for fields of human endeavor. Neither accumulationist nor Kuhnian stories of the history of science need say that classical electromagnetism importantly *failed*. Theories do not need to fail before they are improved, no more than do computers or refrigerators.¹⁰ Histories of artistic, literary, culinary, or any other type of achievement need not speak of death and failure in order to recount the movement from one school or style to the next. Moreover, philosophers’ unreflective willingness to speak of their own history in terms of death and failure gives other disciplines both motive and opportunity to find philosophy a very strange and irrelevant discipline, a demoralized discipline giving off the scent of decay. Historians of philosophy come to look like spectators caught up in the grandeur of a historical procession of death and failure; philosophers come to look like cheerful or dutiful marchers *in* that procession. “Join us,” we seem to say to our students, “so that someday soon your work, too, can be seen as dead and failed.”¹¹

Indeed, the pathos of this heroic philosophical failure through thousands of years is unbearable. A history of philosophy that explains why Plato failed, Aristotle failed, Aquinas failed, Descartes failed, Spinoza, Leibniz, Locke, Hume, Kant, Hegel, all failed, all failures, all dead, dead, dead—such a history is the intellectual equivalent of the weary academic

job seeker reciting the history of her years on the market or of the dinner companion whose conversation consists of an exhaustive and exhausting, demoralized and demoralizing, account of why his romantic relationships have all failed. Enough; no more of these images of failure and death and of philosophers' pathetic struggles against certain failure and death. We do not admire the Romans who mocked Christ on the cross, and we ought not happily take up their role with respect to the philosophers, however ungodly they may be.

The final issue we want to scout concerns the levels of historical explanation. Reisch, for example, contrasts his "small-large" account of the troubled career of the *IEUS* with a "large-large" account he sees in Galison's "Constructing Modernism." That is, Reisch argues that the appropriate level at which to explain what happened to the *Encyclopedia* is a fine-grained one, describing in detail the interactions among the editors, between the editors and the authors, between the editors and the University of Chicago Press, and between the editors and their readers, whereas he sees Galison moving in the rarified world of idea complexes that have trouble moving from Europe to America, *zeitgeistige* changes in the cultural landscape, and so on. Reisch attributes Galison's search for expansive explanations of the decline of logical empiricism to a quasi-Cartesian principle of causation: Large effects must have large causes. Reisch himself, conversely, offers a narrative in the manner of the butterfly effect: For want of an editorial team, one might suggest, a philosophical program was lost. Interestingly, in this, Reisch appears as the hardheaded positivist, linking the particular events that constitute the world together into chains of causation. Galison appears as the slippery idealist, trying to pull the wool over our eyes with soothing words and grand schemes.

It is certainly true that particularism and materialism of a sort are in vogue these days in history of science and intellectual history. "Ideas" are now seen as ghostly things to be replaced by practices, texts, and so forth. Grand narratives have given way to minutely detailed descriptions of particular people in particular places doing particular things with particular instruments. But one sort of grand *metanarrative* persists, a *metahistorical* metanarrative that pours value over the move from grand narratives (or the history of disembodied ideas) to localism, particularism, contextualism, and so on by arguing that this move is a methodological or philosophical triumph. Historians of science write fascinating histories, but they may be more fascinating as the particular histories they are than as arguments by example for a certain account of causation or an "ontology."

In fact, history of philosophy of science needs both Reischian explanations and Galisonian explanations. After all, what is at stake is historical understanding, not necessarily causation or "images of the world"

or anything deeply metaphysical, and from this point of view the explanations are more complementary than competing. Consider this volume. It has appeared in the world in its current form due to the individual actions of individual people; it has a history. That history includes a detailed Reischian account of why some people did and others did not submit papers, why the editors took certain actions and not others, and so on. It could suggest different actions that would have led to different volumes, and it would indicate critical points in the volume's construction. The story of *LENA* is the story of those actions. But, of course, those actions do not occur in a cultural void. Larger "standing" conditions would have to be cited in rendering the actions of the actors intelligible: for example, professional considerations such as the relative value of having a paper in an edited volume rather than a journal or having an edited volume on one's curriculum vitae. The rise of historical interest in logical empiricism since the 1980s would also have to be cited as a precondition of the possibility of this volume (as, indeed, evidenced by the first paragraph of this introduction). Such larger features of the situation are, in this case, conditions of intelligible action and are the sorts of features that Galison has sought to elucidate regarding logical empiricism in its places and times.

Both sorts of history are crucial to breaking the hold of a sort of textual hegemony in history of logical empiricism. Philosophers typically think they can "read the texts" and "figure out the arguments" and, on this basis, construct the rational history of philosophy. Logical empiricism, on such views, declined because it was refuted by Quine or Kuhn or because it was wrong about the nature of scientific theories, confirmation, realism, naturalism, or whatever. Reisch reminds us of the extent to which the individual actions and commitments of individual people *actually* form the history of logical empiricism in America. The *Encyclopedia* was not refuted; it was abandoned. Or, if you prefer, abandonment of projects and alteration of commitments while citing arguments as the reasons for such abandonment is the empirical face of refutation: If no one were to change their philosophical commitments or projects after "Two Dogmas of Empiricism," it would not have "refuted" anything. Galison, on the other hand, reminds us that patterns of such actions and commitments—and also the texts themselves—make sense only when placed in larger contexts. Indeed, only by combining the cultural and the individual can the poignancy of the actions taken or not taken become manifest.

The Role for History

Earlier, we adapted to our own purposes the most famous sentence in the philosophy of science in the second half of the twentieth century, the first

sentence of Thomas Kuhn's *Structure of Scientific Revolutions*: "History," Kuhn wrote, "if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" ([1962] 1996, 1). Kuhn obviously took himself here to be arguing not against science but against an *image* of science. Moving to philosophy of science, it seems quite clear that there is no *image* of philosophy of science that currently possesses philosophers. What philosophy of science is, what its topics, methods, and epistemic status are, are matters of dispute and dissent. If there is a role for history of philosophy of science, it is not a role that mirrors Kuhn's "role for history" of science in our understanding of science.

We suggested above that the history of philosophy of science has a deeper role: It can transform philosophy of science itself. Philosophers of science might, while lacking an image of their own practices, nevertheless be fairly considered to possess a philosophy of science: a set of unarticulated working methods, canonical topics, intellectual values, and ways of arguing. It is perhaps characteristic of today's analytic philosophy more generally that it consists of highly specialized sets of practices, argumentative procedures, and textual traditions without a compelling account of the meaningfulness of those practices, procedures, or traditions.¹² History of philosophy of science can transform contemporary philosophy of science by tying past practice in philosophy of science to the actions of individual philosophers and scientists set in their cultural contexts and by exploring the relationships between the culturally available understandings of science and of philosophy throughout our history. History of philosophy of science does not only suggest that this is the way for contemporary philosophy of science to acquire a focused image of itself; it also suggests that the very process of acquiring that image of our own activities will alter those activities. It will do this by making the cultural task of philosophy of science an explicit topic of consideration.

Those in the postphilosophical camp have argued that there are no roles or tasks for philosophy these days. Often this view is presented as the conclusion of a historical argument that claims to lay bare the nature of the philosophical project and indicate its poverty, impossibility, or irrelevance. Interestingly, those historical stories have been some of the worst caricatures of the historical projects in philosophy that anyone has ever written. But despite this reliance on bad history, the postphilosophers *might* be right. After all, it is implausible that every human culture has had a role for philosophers within it. Those of us engaged in serious history of philosophy of science look to the historical figures not to discover and take up a cultural mission more pertinent to times past but to forge new cultural missions and philosophical projects. We must be prepared to fail

in these endeavors, but we must be, therefore, all the more committed to succeeding in them.

In 1946, Neurath was writing an essay that was left unfinished by his sudden death. It concludes with this uncharacteristically simple sentence: “The difficulty is that, whatever changes we accept, it is a kind of venture” ([1946] 1983, 246). The changes we create are even more difficult than those we simply accept, and *our* philosophical venture may yet come to nothing. But if it does, we will let down more than just ourselves. Historical and philosophical awareness induces a knowledge of our obligations to the past and fills us with our sense of obligation to the future. The interest we current philosophers have in logical empiricism can perhaps be explained by another image from Neurath: Wanderers lost in a forest do not purposefully return to their previous places, but they do well to remind themselves of the advice they have received in the past regarding the best way out of the forest. This is no time to return to logical empiricism, but it may well be time to return to the social spirit of philosophy of science of the 1930s. Perhaps that is our best philosophical venture in a world of anxious social and technological Maybes.

Notes

1. Early exemplars of the new historical interest in philosophy of science are Michael Friedman’s review of the collected papers of Moritz Schlick (Friedman 1983a) and the first chapter of his book on space-time theories (Friedman 1983b). In both these places he began to tell a very different story of the relation of logical empiricism to relativistic physics than had become by then canonical. In the Austrian context, 1982, the centenary year of Schlick and Otto Neurath, brought new historical interest in philosophy of science (see Haller 1982). In addition to this literature, the historical turn has institutional and organizational superstructure. In 1990, the History of Philosophy of Science (HOPOS) Working Group was founded; in 1991, the Vienna Circle Institute was founded at the University of Vienna. These two entities came together for the HOPOS 2000 conference hosted by the Vienna Circle Institute, a conference that matched the number of presentations offered at the 2000 Philosophy of Science Association meetings in Vancouver.

2. Indeed, the *exclusionary* practices of analytic philosophy may come first. Hilary Putnam comments on this in an interview: “What happened to me, as to many other young American philosophers, was that in graduate school one learned what *not* to like and what not to consider philosophy” (Borradori 1994, 57). John McCumber (2001) comments on the larger social conditions attending such exclusionary practices as well as on their detrimental effects.

3. And here the move to “family resemblances” helps not a whit. Boundary concepts are typically terms of contrast rather than resemblance—which means that they combine in a way that either rules out nothing or rules out everything. That is, either analytic philosophy of mind is not phenomenology, not neuroscience, and so on, or else thought about one way, it sort of looks something like phenomenology, but thought about another way, it is sort of like neuroscience, and so on.

4. This is, in fact, something of a metaphilosophical exemplar of the noble history

of words that McCumber offers as a way for philosophy to climb out of “the ditch” (2001, 132–56).

5. An example of why issues such as those considered in this volume demand institutional and sociological perspectives, and yet of how halfhearted the reliance on a robust version of sociology of philosophy can be, is provided by Robert Butts. Butts’s question concerns “the reception of German scientific philosophy”—the reception, that is, of logical empiricist accounts of science—in the United States. His answers divide into the internal and the external: His internal answer is that logical empiricist philosophy of science has “no proper alternative” (Butts 2000, 200), and his external answer is that “by the time that logical empiricism moved to the United States, the soil had been already well-prepared” (Butts 2000, 201) by American philosophical movements that shared an interest in science. As it happens, what matters to Butts is to argue for the truth of his internal reason for the positive reception of logical empiricism: Logical empiricism has, he argues, no proper alternatives. Butts’s essay thus trades upon a fatal ambiguity in the notion of “proper alternative”: His own argument is that logical empiricist accounts of theories are true and thus lack properly true alternatives, but this notion of “proper alternative” cannot play any explanatory role in the history of philosophy. After all, if logical empiricists were right about theories, then there never has been a proper alternative in this sense to their views. But that putative fact does not at all explain why these views only became canonical in the 1930s rather than the 1830s or the 1630s. If “proper alternative” is to play any explanatory role in the history of logical empiricism, it must mean something like “alternative that could at the time be adopted without losing one’s standing as a philosopher.” But then the question of the reception of logical empiricism becomes the question of how logical empiricism came to have no proper alternative, and Butts’s account is seen as containing a canonical *petitio principii* of history of philosophy: He takes his more sociologically robust version of the problem to be a cause to be cited in the problem’s solution.

6. After Neurath, Frank is perhaps the best (but not the only) example of a major yet marginalized figure in the received history of logical empiricism. Morris is a close second, and perhaps Zilsel comes in third. For this reason, and to demonstrate the fruitfulness of considering his work in the context of the history of philosophy of science, we draw attention to Frank in this introduction.

7. Frank’s *Relativity: A Richer Truth* (1950) is derived from his talks at the Conferences on Science, Philosophy, and Religion in the 1940s, and he continued attending the conferences and publishing in their proceedings into the 1950s (Frank 1953, 1954). The conference on validation of theories in science had a proceedings volume (Frank 1956). His remarks at the APS/NSF conference are in Frank 1955. His views on science teaching can be found, for example, in Frank 1947.

8. Interestingly, the very slender Frank papers in the Harvard University Archives (UAP 4406.xx) consist largely of several hundred heavily edited typescript pages of an unpublished book manuscript by Frank entitled “The Humanistic Background of Science.”

9. It could be argued that the size constraints of the individual monographs of the *Encyclopedia* greatly contributed to the success of its most famous one, Thomas Kuhn’s *Structure of Scientific Revolutions*. If one keeps track of all Kuhn’s claims in the text regarding things he wished to include but for which was not given the space, one sees a very different book emerge. Indeed, the book Kuhn *wanted* to write looks more like a prize-winning “thick description” of the 1990s than the most significant and widely read book in history and philosophy of science in the 1960s.

10. Popper’s insistence that science progresses through repeated failure under severe testing is a philosophers’ history par excellence. It overturns the value judgments implicit in canonical history of philosophy and the history of science by making failure the ultimate

epistemic virtue. Significantly, Popper's "enemies of the open society" are also supremely confident philosophers who do not think failure of prediction is the hallmark of epistemic success. Friends of the open society have a distinctly Protestant air about them. Falsificationism inscribes a narrative that involves endless yearning after the truth but certain knowledge that one has fallen short of it.

11. For a recent example that shows how natural this way of framing an historical narrative of philosophy is among analytic philosophers, see Stroll 2000; for a review of Stroll's book that takes its narrative structure as topic, see Richardson 2001.

12. McCumber, interestingly, begins his account of the current "dysfunction" of American philosophy by noting the "ongoing and *general* absence of reflection on the discipline" in American philosophy (2001, 8), as emblemized by the change in format of the American Philosophical Association presidential addresses from occasions to reflect on philosophy to occasions to engage in philosophy.

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