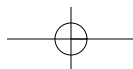
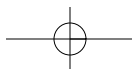




Discourse on a New Method





Discourse on a New Method

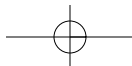
Reinvigorating the
Marriage of History and
Philosophy of Science

EDITED BY
Mary Domski and Michael Dickson

WITH A CONCLUDING ESSAY BY
Michael Friedman



OPEN COURT
Chicago and La Salle, Illinois



Cover Illustration: frontispiece to Voltaire's *Elémens de la philosophie de Newton* (1738).

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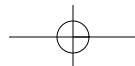
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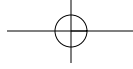
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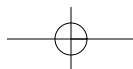


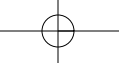
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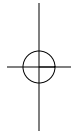


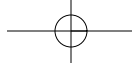


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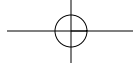
Preface and Acknowledgments

We have chosen a rather grand title for this book, one whose grandness seems only fitting given the rather grand purpose of our project: to examine and honor Michael Friedman's work in the history and philosophy of science. As readers of this volume will quickly discern, our examination does not take the form seen in most festschrifts. We do not simply aim to bring attention to specific claims that Friedman has made over the course of his career (though many of our contributors of course do so in their essays). Rather, our aim is to turn attention to Friedman's work in order that we might glean and assess lessons about how best to approach the interaction between the history of philosophy and the history of science. In brief, our goal is methodological and historiographical.

In our introductory chapter, we spell out what we take to be important and unique about Friedman's approach to the history and philosophy of science and cash out the features of the historiography that Friedman has applied over the past few decades—a historiography which we have dubbed a “new method” in the title of this book. Though Friedman is not the only historian-philosopher to apply the methodology we describe and encourage, we believe that attention to his course of work grants us important insights into how best to understand the complexity and intimacy of the relationship between the history of science and the history of philosophy. Our intention in this volume is to explicate and pursue these insights (though certainly not uncritically), and as our subtitle suggests, we hope to thereby provide a means by which to reinvigorate the very marriage of the history and philosophy of science that scholars have continued to bring into dispute.

Given what we've aimed to accomplish with this collection of essays, it is probably little surprise that this volume has been six years in the making. Considering the number of authors involved, and especially the richness of the issues they have engaged in their essays, six years actually seems a rather short amount of time for the publication of a volume of this depth and range (and with this many pages!). We could not have moved so quickly and with so much success had it not been for the efforts and contributions of various people and groups.



*Preface and Acknowledgments*

First and foremost, our thanks go to the contributors. When asked to contribute a paper to a volume examining and honoring Friedman's work in the history and philosophy of science, each author responded enthusiastically and, over the past six years, they worked within the editorial limits and time frames that we imposed (though perhaps not always with a smile on their faces). We thank each of them for composing rich and interesting papers and for remaining patient with us over the duration of the project.

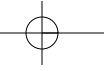
We also extend our thanks to André Carus, President of Open Court Publishing Company. When we approached André with the idea of the volume in 2003, he encouraged us to buck the model of the standard *festschrift* and craft a more programmatic work that would more fittingly honor the impact of Friedman's work in the history and philosophy of science. We followed his suggestion (as detailed in our introduction) and have a higher quality volume to present because of it.

The past couple years, George Reisch of Open Court has played a vital role in getting the papers ready for publication, and we thank him for his attention to the finer details. We would also like to thank Gordon Purves for his assistance in preparing the index.

In October of 2004, we organized a workshop at the University of South Carolina, where many of our contributors presented earlier versions of their papers. The event was a great success and would not have been possible without generous funding from the University of South Carolina Department of Philosophy, College of Liberal Arts, College of Science and Mathematics, Department of Physics, and Department of History, as well as The International Society for the History of Philosophy of Science (HOPOS).

Michael would like to thank his colleagues in the Department of Philosophy and elsewhere at the University of South Carolina, who supported the workshop both financially and intellectually. Mary would like to thank the College of Arts and Humanities at California State University-Fresno, which kindly gave her a course reduction during the 2004–05 academic year so that she could commit more time to work related to the volume. Colleagues at the University of New Mexico also offered welcome encouragement the past few years as the final publication date was nearing. Mary owes a special thanks to her friends, Sarah Pessin, Lee Pike, and David Shumaker, for offering feedback and encouragement at various stages of the volume's production.

Last but certainly not least, our thanks go to Michael Friedman. We of course thank Michael for being a part of the project and contributing his reflections on the essays we assembled to honor his work. But we also thank him for inspiring the very idea of this volume. His work as a scholar, a teacher, and a colleague made this project possible and has, at every step of the way, motivated us to see it through to its completion.



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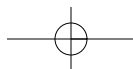
Introduction

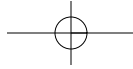
*Discourse on a New Method, or a
Manifesto for a Synthetic Approach to History
and Philosophy of Science*

MARY DOMSKI and MICHAEL DICKSON

Questions about the marital status of history of science and philosophy of science are older than the flurry of new departments, centers, and programs in the history and philosophy of science that emerged in the mid-twentieth century.¹ But quite naturally their creation, and the ongoing joint meetings of the History of Science Society (HSS) and Philosophy of Science Association (PSA), have propelled these questions to the foreground on several occasions.

One such event took place at the University of Minnesota in 1969, at a conference dedicated to the rationale of the union of history of science and philosophy of science.² The conference papers were published under the title *Historical and Philosophical Perspectives of Science* (1970), as Volume 5 of the *Minnesota Studies in the Philosophy of Science* (edited by Roger H. Stuewer). A few years later, Ron Giere wrote a provocative review of the book, entitled “History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?” (Giere 1973).³ Giere considers various arguments for the necessary union of these disciplines—arguments put forward by an esteemed group that includes Herbert Feigl, Paul Feyerabend, Mary Hesse, Ernan McMullin, and Arnold Thackray—and, on the whole, he finds their appeal to history as a remedy for the failings of the logical empiricist movement in the philosophy of science unconvincing. Furthermore, Giere does not simply find fault with the arguments presented in the *Minnesota Studies* volume; he suggests that, although the pairing of history and philosophy of science had already become fashionable, contemporary philosophers of science needn’t worry themselves with appeal to the historian’s ‘internalist’ studies of scientific development or scientific practice. Attention to what he terms “real science”—the science that can be learned in textbooks—will serve their philosophical purposes. As such, he argues that the union of these disciplines “lacks a strong



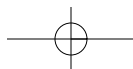


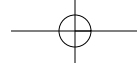
conceptual rationale,” and thus the most (or best) we can say is that the history of science and the philosophy of science share a common interest in science (Giere 1973, 296).

Unsurprisingly, Giere’s forceful remarks have been met with equally forceful replies from historically-minded philosophers. Ernan McMullin offered his direct response to Giere in a 1974 PSA paper entitled “History and Philosophy of Science: A Marriage of Convenience?”⁴ McMullin argued that, despite Giere’s fervent suggestions to the contrary, there are in fact particular issues in the philosophy of science that cannot be adequately addressed without remaining sensitive to the history of science, and especially to the historical development of scientific practices and scientific theories. The principal issues he cites concern the assessment of theories, the nature of scientific growth, and the ontology of theoretical entities. He aims to show that if “an adequate treatment even of the most ‘logical’ of these three questions, viz., that of theory-assessment, cannot avoid reference to the history of science,” then we may safely conclude that the “marriage of the history and philosophy of science is not just one of convenience” (McMullin 1974, 586).

Even without a thorough assessment of McMullin’s argument, our current state of practice in the philosophy of science indicates that McMullin and company have won the debate. For there is no question that philosophers of science have in the past few decades made it commonplace to bring the history of science and the history of the philosophy of science to bear on their philosophical discussions, and they have done so quite effectively. For instance, historical examples have figured prominently in some of the more compelling arguments concerning the nature of scientific growth. Larry Laudan’s (1981) anti-realist appeal to the ‘pessimistic meta-induction’ draws its force from revolutionary periods in the history of science, and John Worrall’s (1989) ‘structural realist’ reply earns credibility from what he understands as Poincaré’s understanding of how scientific theories represent the world.⁵ So much the worse for Giere’s arguments it seems, and so much the better for historically-minded philosophers (and philosophically-minded historians) who can appeal to the continued and successful application of the history of science in the philosophy science as justification that these disciplines are intimately related.

However, the debate cannot be settled on these grounds alone. For when Giere claimed that the history of science has no significant bearing on the philosophy of science, he was not claiming that philosophers should ignore the science and scientific practices of the past. The argument was hardly this simple-minded. Giere was instead, and more pointedly, arguing that the sort of history that *historians of science* take as paradigmatic to their discipline—a fine-grained and often complex history, focused, more or less, on the specific events, context, and influences surrounding the develop-





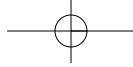
ment and acceptance or rejection of a concept or theory—is not the sort of history from which philosophers can *prima facie* draw useful lessons.⁶ Much philosophical use of history (e.g., as exercised by Laudan) does not involve history of that sort. Surely Giere would have had exactly this point made to *him* by Edward Grant, Richard (‘Sam’) Westfall, and his other historian colleagues at Indiana University at the time.

For Giere, then, the fact that our contemporary philosophical discourse is peppered with appeals to and examples from the past hardly counts as genuine evidence that the history of science and the philosophy of science are involved in an intimate relationship. Any such evidence for this relationship will come from a more reflective consideration of the conceptual ties between these disciplines. In particular, Giere urges that historically-minded philosophers “not be content merely to practice their art but . . . make repeated efforts to explain and argue the rationale for their approach” (Giere 1973, 291), and he solicits from historian-philosophers an account of how “philosophical conclusions may be supported by historical facts.” “Until this is done,” he writes, “the historical approach to philosophy of science is without a conceptually coherent program” (1973, 292).

Our aim in this ‘manifesto’ is in part to provide the sort of historiographical and methodological account that we think Giere rightly urges. The account we offer takes its inspiration from the historiography that has been masterfully applied by Michael Friedman over the past few decades—a historiography which we have dubbed a “new method” in the title of this book. Though Friedman is not the only historian-philosopher to apply the methodology we describe and encourage, we believe that attention to his course of work grants us important insights into how best to understand the complexity and intimacy of the relationship between the history of science and the history of philosophy. Our intention in this volume is to explicate and pursue these insights (though certainly not uncritically), and thereby provide a means by which to reinvigorate the very marriage of the history and philosophy of science that Giere and others have continued to bring into dispute.

“Remarks on the History of Science and the History of Philosophy”⁷

Some twenty years after Giere’s review was published, Friedman (1993) made what perhaps seemed at the time a modest plea to contemporary philosophers of science. Taking for granted that Thomas Kuhn’s *Structure of Scientific Revolutions* (1962) had convincingly shown that philosophers of science ought to take seriously the historical development of science, Friedman suggests that historians of philosophy ought to take seriously the

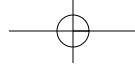


historical role that science has played in philosophy. They should, that is, consider the historical *interaction* between the history of science and the history of philosophy. Relying on Kuhn's historiography of science, and adopting a more or less Kuhnian characterization of scientific revolutions, Friedman argues, in particular, that an important task for the historian of philosophy is to situate the emergence and evolution of philosophical ideas in their revolutionary scientific context.

Friedman does not have in mind what he rightly supposed to be, by then, a commonplace in the history of philosophy, namely, the recognition that broad themes from the history of science—and especially the large-scale transformations in philosophical concerns that often accompany scientific revolutions—have some part to play in our understanding of the history of philosophy. In this vein, Friedman mentions the philosophical concerns of figures such as Descartes, whose work can be understood in the context of the shift from Aristotelian-Scholastic natural philosophy to the mechanical philosophy. Other examples spring to mind: the role of the systematization of geometry in the development of various ancient Greek accounts of knowledge; the role of the development of computer science in the development of various theories of mind; and the role of the shift from classical to quantum theory (together with the perceived 'acausality' and attendant 'irrationality' of the latter) in the development of various twentieth century 'irrationalist' philosophies. Recognition of such broad parallels between the history of science and the history of philosophy (and corresponding discussion of the direction of causality, if any) is widespread.

Without discounting the importance of such broad-brushed considerations, Friedman has in mind a finer-grained discussion of the connections between science and philosophy, one that takes account of the specific content of, and especially the central problems driving, the science of the day. More specifically, he emphasizes that many philosophical enterprises are motivated by the attempt to understand (and in some cases provide) the detailed foundations of scientific theories.

In his 1993 paper, Friedman explores the possible fruits of adopting this historiographical perspective in the context of two examples. The first concerns the change from an Aristotelian-Scholastic physical dynamics to a mechanical, ultimately Newtonian, dynamics—a change motivated by the work of figures such as Galileo, Descartes, and, of course, Newton himself. That shift, argues Friedman, raised new foundational problems in science that in turn gave rise to philosophical questions concerning the status and possibility of our knowledge of the natural world. Crucial among the problems emerging from Newton's system of the world was our ability to distinguish states of true motion from states of true rest and, perhaps even more important, states of inertial motion from states of non-inertial (accelerated) motion. Specifically, it is unclear how the new dynamics of the

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Principia Mathematica has any empirical content, in part because the theory is Galilean-invariant, so that there is nothing *given* (such as an Earth at absolute rest, as there was in the older system) relative to which, for example, inertial motion is defined.⁸ But then how does Newton's law of inertia (a law about inertial bodies) have any empirical content? (The second law does not help here, unless one already *knows* all of the forces affecting the system, a point that would much later push Poincaré towards his conventionalism about geometry.)

Friedman suggests that the dialogue that ensued between science and philosophy in the wake of the *Principia's* success and acceptance centered on "this fundamental problem lying at the basis of the new physical dynamics" (Friedman 1993, 41). And this suggestion motivates reconsideration of how to best understand the course of modern thought. For if we take seriously the interaction between science and philosophy and, in turn, approach the evolution of the modern philosophical tradition as successive attempts to come to terms with the fundamental problem of relativity of motion that lay at the basis of the new physical dynamics, then, on Friedman's account,

we are brought to a conception of the evolution of modern philosophy that is more illuminating than the conventional picture of a succession of largely futile attempts to solve the problems of skepticism about the external world. (43)

This re-evaluation of modern thought, centered on the revolutionary character of the history of science, sheds new light on the progress of epistemology and metaphysics. Specifically, Friedman claims that

contrary to the opinion of some contemporary historical writers, the philosophers of the modern tradition are not best understood as attempting to stand outside the new science so as to show, from some mysterious point outside of science itself, that our scientific knowledge somehow 'mirrors' an independently existing reality. (48)

Rather, as Friedman argues, they are best understood as trying to come to terms with the foundations of the new science.

Kant, for Friedman, is paradigmatic here, because he proposes that metaphysics should articulate the a priori conditions of the possibility of experience. Starting from the so-called synthetic a priori forms of knowledge expressed in Euclidean geometry and Newtonian dynamics, Kant sought not to justify this knowledge but to investigate those very conditions of human cognition that make such synthetic a priori knowledge possible. By grounding the project and goal of metaphysics in mathematics



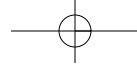
and natural science, Kant at the same time established “a reorganization and reinterpretation of metaphysics itself” (49), and thus instigated his own ‘revolution’, not in science, but in philosophy. On Friedman’s account, then, Kant’s great service to philosophy was to bring metaphysics down from its lofty place ‘above’ human knowledge precisely by taking pure mathematics and pure natural science as the starting point of his metaphysical investigations.

In his second example, Friedman points out that a similar concern with our most successful claims to scientific and mathematical knowledge emerges in the early twentieth century as philosophers faced another revolution in the natural sciences: the special and general theories of relativity grounded on non-Euclidean geometry. Remaining sensitive to their historical situation—and, in particular, to the ongoing scientific revolutions of the early twentieth century—Friedman contends that we can understand logical positivists as

attempting philosophically to come to terms with the profound conceptual revolutions that initiated twentieth-century science. These thinkers should be seen not as attempting to justify twentieth-century science from some sterile and futile external vantage point but rather as once again refashioning the basic concepts and principles of philosophy so as to accommodate and comprehend the new scientific developments. That is, their aim is not to justify twentieth-century science from some supposed “higher” standpoint but rather to provide a *rational reconstruction* of that science and to find thereby a new, non-metaphysical task for philosophy. (49)

With the supplanting of Newtonian dynamics and its framework of Euclidean geometry—and thus with the possibility of scientific knowledge as understood by Kant no longer on center stage—the logical positivists sought to trace the development of scientific ideas and scientific theories by embedding the development of scientific theories in a logical language in order to exploit the rational interconnections among our scientific claims.

However, although the protagonists had, out of historical necessity, abandoned certain aspects of Kantianism in its original form, the spirit of Kant’s project for metaphysics was still alive during the early twentieth century. In particular, Friedman argues that the logical positivists continued to “advocate a *modified* Kantian position according to which there is a fundamental distinction between the spatiotemporal framework of physical dynamics and the empirical laws formulated within this framework” (50). This fundamental distinction is not simply a Kantian distinction between the a priori and the a posteriori; faced with revolutionary changes in math-

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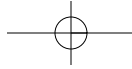
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ematics and science, it was transformed into a distinction between the *relativized* a priori and the a posteriori, between historically dynamic mathematical and physical laws on the one hand, and the understanding of the world that depends constitutively on them on the other hand. Recognizing this Kantian thread running through early twentieth century philosophy of science, Friedman concludes that

although the logical positivists' preoccupation with the a priori did indeed thereby preclude them from using the history of science as a philosophical tool, this did not prevent them from recognizing the profound philosophical significance of conceptual revolutions in science. On the contrary, their effort to articulate a coherent conception of the *relativized* a priori must, I think, count as the most rigorous attempt we have yet seen philosophically to come to terms with precisely such conceptual revolutions. Of course, as we have also seen, this heroic attempt of the logical positivists was in the end a failure. Yet I do not myself think that we will ever progress beyond this point until we possess a fuller appreciation of the historical evolution of our own philosophical predicament. And this means, as I have tried to emphasize throughout, that we must attend more closely to the history of science, the history of philosophy, *and* to the essential interaction between them. (54)

In these concluding remarks, Friedman proposes a project not only for historians of philosophy, but also for philosophers in general. Note, in particular, the word 'essential' in the final sentence. Friedman's implicit claim is that the historical interaction between philosophy and science is not a quirk of history, but an essential fact about the two disciplines. It is of the nature of science and philosophy to interact. Without placing too much weight on this point, or seeking to follow through on this idea in detail (which in any case would require a long discussion of the nature of science and philosophy), we must nonetheless emphasize the distinction between the claim that philosophy *has*, as a matter of historical fact, interacted with science in a variety of ways (as exemplified in Friedman's two examples), and the claim that philosophy by nature does, and *ought*, to proceed in that manner. Friedman quite clearly endorses the former claim, and appears to endorse the latter as well.

Indeed, the two claims are not entirely independent. Intellectual disciplines do not exist in an historical vacuum. The standards, problems, goals, and methods of a discipline provide the context that makes its practices intelligible, and they are in part inherited from its own past. In the case of philosophy, insofar as this context arises from substantial historical interaction with science, understanding that historical interaction is required for



understanding our present practices. Hence, while the two claims that we have thus far attributed to Friedman are indeed distinct, the first does give reason to believe the second.

Nonetheless, one might say that Friedman has taken the role of science in the history of philosophy to an unhealthy extreme. The argument runs thus: Remaining so firmly wedded to a scientific perspective on, say, the modern period provides us at best only half the picture. For example, skepticism was in fact in bloom and helped shape epistemology from Descartes to Kant and beyond.

Though initially persuasive, notice that this sort of argument is grounded on a worrisome either-or: either we examine how the history of science has influenced the history of philosophy, or we examine how skepticism influenced the history of philosophy. We certainly do not deny that Friedman has adopted the history and philosophy of science as the focal point of his investigations. But, as his recent work has illustrated, adopting this strategy does not force us to thereby ignore other factors that contributed to the development of novel philosophical insights. For Friedman's project is not simply one of illuminating how the history of philosophy has helped shape the history of the philosophy of science, and vice versa. Rather, by turning our focus toward the history and philosophy of science, he has shed new light on the standard intellectual concerns surrounding skepticism, metaphysics, and theology that shaped (and continue to shape) the historical progress of philosophy.⁹ Thus, what some might take as the overly radical nature of Friedman's work we take, more sympathetically, as a strategy for drawing attention to an interaction between philosophy and science that has, until very recently, attracted inadequate scholarly attention, as well as a fruitful strategy for giving old questions new flavor.

A Tradition of Historical Philosophy and Philosophical History

There is more to Friedman's story than indicated from the above sketch of his 1993 article. From a plea to take seriously the interaction between the history of science and the history of philosophy, Friedman arrives not only (apparently) at the conclusion that philosophy and science *ought* to interact in a certain way, but also (clearly) at a Kantian, or better, neo-Kantian, conception of the nature of that interaction, which rests on the relativized a priori.¹⁰ If one finds the science-influenced story that Friedman tells hard to digest, then his Kant-influenced story about the history of science and the history of philosophy may be poison for the palate. But we need to take care, as always, not to throw the baby out with the (Kantian) bath-

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water, and ask whether embracing the sort of historiography of philosophy that Friedman promotes in fact demands that we at the same time embrace a Kantian or neo-Kantian understanding of the growth of scientific knowledge and the methodology of philosophy. From our perspective, the two are not inextricably bound. In fact, there is a long tradition of pursuing the sort of historically informed philosophy and philosophically informed history that has become a hallmark of Friedman's work. Seeing Friedman's own work in this context will help us to understand, first, how specifically he differs from his predecessors, and also how one might pursue a program broadly sympathetic to Friedman's approach without necessarily ending up a neo-Kantian.¹¹

On the side of history, Friedman stands in a tradition of philosophical historians that includes Richard Westfall, who approached the historical development of ideas with an eye to the philosophical issues that loomed over seventeenth-century science. Westfall's *Force in Newton's Physics* (1971) provides an example. In his masterful explication of Newton's formulation of the second law of motion, Westfall puts less emphasis on the attempt to formalize the law ($F = ma$) and more emphasis on the philosophical context that allowed Newton to ask the right questions in the first place. It was Newton's proposed resolution to "the metaphysical question of the ontological status of force" that, according to Westfall, brought him to the notion of force that lay at the heart of his mature physical dynamics. In a similar fashion, Friedman has brought us to a deeper appreciation for the philosophical questions that enabled Einstein to develop relativity theory. On Friedman's account, Einstein's attention to the philosophical questions surrounding the newly discovered non-Euclidean geometries, as illustrated by his close reading of Helmholtz and Poincaré, partly allowed him to develop a theory of relativity that stands on the foundation of a non-Euclidean geometry (cf. Friedman 1999, 2001).

On the side of philosophy, Friedman attests to his own position in a tradition of historical philosophers, most notably, Thomas Kuhn. Quite clearly, their respective philosophies of science take revolutionary changes in the history of science as their touchstone. Where Kuhn (and followers) see, in a scientific revolution, an opportunity to draw important generic philosophical lessons about general philosophical issues, such as the nature of scientific knowledge, Friedman sees in them an opportunity to understand the specific problems that drove the development of philosophy at the time of the revolution and beyond.

Friedman also draws significant motivation from another historically-minded philosopher, Ernst Cassirer. Cassirer famously attempted to preserve a Kantian understanding of science and knowledge in the wake of the discovery of non-Euclidean geometry and Einstein's theory of relativity. Relying heavily on Kant's notion of "the regulative use of reason," Cassirer



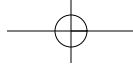
presented a (Marburg neo-Kantian) history of science defined by the successive and continual development of our ideas, a development that would purportedly find its limit in an absolute truth. Though abandoning the “logical idealist” notion of an objective end to the progression of our ideas, Friedman maintains along with Cassirer the notion of a constancy and permanency in the course of scientific progress.¹² For Friedman, the permanency is not found in the limit of our ideas but in the relativized a priori. Both Cassirer and Friedman are thus in a clear sense *motivated* by Kantian themes and embrace Kant’s insistence on the unity of scientific knowledge, though, clearly, this unity expresses itself differently in Friedman’s and Cassirer’s respective neo-Kantianisms. (The unity peculiar to Friedman’s historiography of philosophy and science is discussed in greater detail below.)

But beyond the traces of Kantian inspiration evident in their work, there is a more significant methodological similarity between Cassirer and Friedman, especially if we accept Friedman’s reading of Cassirer in his *A Parting of the Ways* (2000). Here Friedman offers an account of how contemporary philosophy has reached a state in which analytic and continental philosophers sit on different sides of a disciplinary divide. He traces this division back to a single historical moment: the 1929 Davos disputation. The event brought together Carnap, Heidegger, and Cassirer, with Carnap in the audience and Heidegger and Cassirer lecturing and ultimately debating how best to understand the importance of Kant’s *Critique of Pure Reason* (1781/1787). The disputation was marked by a culminating face-off between Heidegger’s newly developed phenomenological-metaphysical reading of Kant and the “logical idealist” approach associated with the Marburg School. However, on Friedman’s reading, the event’s broader significance lies in its aftermath. For after the encounter in Switzerland, the disciplinary tree begins to branch, with Carnap steering an analytic course, Heidegger a continental course, and Cassirer adopting a middle path between the two.

Friedman’s own appreciation for Cassirer’s middle path—and especially for Cassirer’s serious consideration of the great revolutions in the foundations of mathematics and mathematical physics that marked the early twentieth century—emerges quite clearly in the final chapter of *Parting*, where he writes that

. . . aside from Husserl himself, Cassirer is the only significant twentieth-century philosopher to make a serious effort to comprehend both these developments within the exact sciences and the contemporary turmoil taking place in the foundations of the historical and cultural sciences. And it is in this respect, in particular, that Cassirer here emerges as a quite central twentieth-century figure. For he alone attempts to do





justice, once again, to both sides of Kant's original philosophical synthesis. And he alone is thus in a position to mediate the increasing intellectual tension between the now notorious "two cultures"—between the *naturwissenschaftliche* and *geisteswissenschaftliche* intellectual orientations. (Friedman 2000, 151–52)

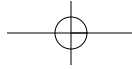
Keeping with the spirit of Cassirer's project, Friedman has committed himself to treading a path between history and philosophy, a path that, as demonstrated through his own work, can bring us to a deeper appreciation of the dialogue between history and philosophy that continues to shape our understanding of science. Or perhaps being more faithful to his general project, we should say that his work lies less on a road *between* two disciplines as it does at the *intersection* of the history and philosophy of science and mathematics. The synthesis of the knowledge, concepts, terminology, and methods emerging from these disciplines has fueled an influential corpus that has blossomed over recent decades.

Now, of course, we face non-trivial questions: What precisely does this synthesis entail? Why do we call it a 'synthesis' in the first place? Given his Kantian motivations, one might take Friedman's synthesis of knowledge simply as a 'going beyond' or amplification, so that his goal is to bring philosophy to bear on history and history to bear on philosophy in order that we might 'see beyond' what either can offer alone. Surely there is this somewhat Kantian tinge to Friedman's general project. But taking an even broader view, the synthesis Friedman promotes is premised not merely on an amplification but on a *unity*—a unity that holds between the intersecting disciplines and a unity that we, as scholars, share with the history of philosophy and the history of science.

Synthesis and 'Synthetic History'

As we just said, the "new method" for examining the history of philosophy, to which we refer in the title of this volume and which is illustrated, in various ways, by the essays contained in this book, may appropriately be called 'synthetic'. Now, among historians generally, the phrase 'synthetic history' has come to mean something like a history that offers 'the big picture', incorporating lessons from various more detailed histories, themselves narrowly circumscribed in subject matter, time period, geographical area, and the like. We have something quite a bit more specific in mind, and in fact the sort of history of philosophy that we describe here (and that we take to be exemplified by essays in this volume) is not synthetic in this sense.

To get a handle on the sort of synthesis that we have in mind, it is useful to recall the philosophical origins of the term 'synthesis', and the



related term ‘analysis’ as found in Aristotle’s two treatises on ‘analytics’. These initiated a long tradition of philosophical and scientific usage of these terms.¹³ A cursory review of this tradition might easily lead one to conclude that ‘analysis’ has come to mean nothing more than ‘taking apart’ or, more philosophically, ‘reducing to the fundamental parts’, as illustrated, for example, by Francis Bacon (1620) when he writes, in the Preface to *The Great Instauration*:

Now what the sciences stand in need of is a form of induction which shall analyze experience and take it to pieces, and by a due process of exclusion and rejection lead to an inevitable conclusion. (Bacon [1620] 1999, 78)

The emphasis in such uses of the term ‘analysis’ is on the idea that the object of analysis is naturally conceived in terms of fundamental parts, so that the conception in those terms, achieved by analysis, amounts to a foundational understanding of it. Our contemporary (philosophical) use of the term may often appear to be along similar lines. For example, an ‘analytically true’ proposition is true in virtue of the meanings of its constituent terms, so that, for example, understanding the proposition as composed of its constituent terms is the same as understanding the proposition.

However, neither Aristotle nor indeed contemporary philosophy should be understood in quite this way. Note, for example, that understanding a proposition as containing (conceptually) certain terms does not constitute understanding the (meaning of the, or truth of the) proposition, for at the least, one must also understand *how* those terms compose the proposition. (‘All philosophers are musicians’ differs from ‘all musicians are philosophers.’)

Aristotle is useful on this point, because in his usage, the term ‘analysis’ did not carry, necessarily, the connotation of ‘breaking into fundamental parts’, but involved, rather, the idea of ‘loosening’ (the everyday meaning of the Greek term). Aristotle occasionally uses the term in this everyday sense, as when, for example, he tells us that ‘in most birds, the gut is thin, and simple when loosened out [analyzed]’ (*History of Animals* II.17 509a17). When it comes to propositions—for example, the propositions that science seeks to understand—Aristotle uses the term to refer to the process of finding an argument whose conclusion is the proposition in question. We shall see in a moment how this process is a kind of ‘loosening’.

As Byrne (1997) points out, Aristotle was likely influenced by the emerging ‘method of analysis’ (perhaps in the Platonic Academy) in this more philosophical use of the term. In its mature form, famously (but cryptically) described by Pappus several centuries later, the method of analysis seems to have involved taking the thing to be shown (or con-

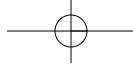


structed) for granted, and working ‘backwards’ from it to something known (or known to be constructible), “and the proof,” as Pappus says, “will be the reverse of the analysis” (*Mathematical Collections*, Book VII). Suppose, for example, that we are faced with the problem of constructing a square equal in area to a given rectangle (Euclid II.14). Using the method of analysis, we assume that we have constructed the square, and we seek a diagram that somehow unites the square and the rectangle in such a way that, from the rectangle, one can construct the diagram, and thence the square.

Aristotle adopts a similar model in his conception of science. Science seeks to understand propositions relating types, for example, ‘All As are Bs.’ Scientific understanding, for Aristotle, amounts to finding an argument whose conclusion is such a proposition. But not just any argument will do. Among the several conditions Aristotle places on the premises, perhaps the most important is that they express the ‘cause’ of the truth of the conclusion, which means, in the context of our simple example, that they contain a middle term that unites the two terms in the proposition, so that the argument we seek has the form ‘All As are Cs; All Cs are Bs; Therefore, All As are Bs.’ In other words, all As are Bs *because* they are all Cs. (For example: All planetary orbits are ellipses because they are the closed paths of bodies governed by gravitational forces.) In Aristotle’s conception, analysis has ‘loosened up’ the proposition, ‘stretching apart’ the extreme terms A and B, showing how ‘between’ them there lies a middle term, C, which is responsible for the truth of the proposition ‘All As are Bs.’ Crucially, the analysis lies not in the observation that the proposition in question is composed of two terms, nor even in any observation about how these terms compose the proposition, but in the observation that the truth expressed by the proposition arises from a particular relationship between those terms. That relationship may be expressed as a kind of ‘synthesis’ of the extreme terms effected by the middle term, C; and analysis is thus the discovery of this ‘synthetic’ relationship between A and B.

We take this conception of analysis and synthesis as the model for the sort of history of philosophy and history of science that we have in mind. Let us see how it plays out in a brief example, discussed by Friedman at the beginning of his contribution to this volume.

It is well known that Newton advocated the view that space is some sort of ‘real thing’. He had clear scientific reasons for doing so, not the least of which was the apparent failure of Descartes’s view of space (which, roughly, identified spatial extension with matter) to yield a coherent account of motion, and a coherent account of *true* motions, in particular. This negative diagnosis of Descartes’s identity of space with corporeal extension is spelled out in Newton’s *De Gravitatione*,¹⁴ where he also claims that Descartes’s system of nature is such that “not even God himself could



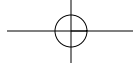
define the past position of any moving body accurately and geometrically” (Newton 2004, 20). In both *De Gravitatione* and later in the General Scholium of the *Principia*,¹⁵ Newton responds to the Cartesian program by adducing several positive arguments for the existence of ‘absolute space’, including the famous ‘bucket’ thought experiment, which purports to show that the real physical effects of acceleration are best explained in terms of an absolute space in terms of which all acceleration is defined.

Newton characterizes this absolute space as the ‘sensorium of God’. As Friedman points out, the doctrine is intimately connected with God’s omnipresence, and God’s ability to create matter at any point in space as an immediate act of the will, in a way that is closely analogous to the ability of a soul (which is present throughout the body) to move its body immediately, as an act of the will. Newton took a position that was thus close to that of the Cambridge Platonist Henry More, whom Newton knew and whose work he read.¹⁶ In his *Enchiridion Metaphysicum* (1671) and elsewhere, More proposed a form of Platonism, in reaction against Cartesian dualism, arguing, among other things, that the properties of space mirror those of God. In particular, space is extended (More believed that all substances, material and immaterial alike, are extended things), indivisible, infinite, immobile, and penetrable (while matter is divisible and impenetrable). For More, as for Newton, God’s omnipresence and God’s ability to create matter at any point in space as an immediate act of the will is the means by which the laws of nature are ‘upheld’.

According to the point of view adopted here, while it may be initially helpful to parse Newton’s view about space into its scientific, philosophical, and theological aspects, ultimately those aspects must be understood as unified, for surely Newton did not take himself to be doing, at one moment, philosophy, and at another, ‘physics’, and at yet another, theology, nor to be doing these three ‘separate’ things somehow ‘at once’.¹⁷

Here is not the place to suggest specific ‘middle terms’ that reveal the unity, for Newton, of these activities. Our point is simply to set up the problem in such a way that a solution could be recognized. In particular, a solution would show how, and why, Newton himself was able to bring all of these different sorts of considerations to bear in the context of a single discussion. It would render Newton’s struggles concerning the nature of space intelligible by revealing the manner in which Newton’s philosophical, scientific, and theological concerns were, in his own way of thinking, united, and ultimately addressed by his account of space. And notice that even identifying those concerns itself requires historical work, tracing them to More, Descartes, Galileo, and of course many others.

We return, now, to the general account of synthetic history, in order to clarify a few points. First, analysis and synthesis are best conceived not as opposites but as two views of, or two ways of examining, a single phe-



nomenon (such as, Newton's account of space): Analysis reveals the cause of the phenomenon, and synthesis demonstrates, or expresses, or explains, the unity of the various components that constitute the phenomenon. We might just as well, then, have called the conception of the history of philosophy that we have in mind here 'analytic'. Indeed, in some ways, this term would have been more appropriate (if also more misleading), because the fundamental task of such history is to find and explore the causes of specific unities between philosophy and science.

Second, as we said above, analysis is not merely a matter of breaking something down into its constituent parts. Instead, analysis seeks the cause of the unity between the parts, the cause of their constituting one thing. In terms of the synthetic history that we are attempting to describe here, we *presume* the initial distinction between, for example, scientific and philosophical concerns or issues as (in this case, non-exhaustive) constituent parts of something broader, and seek to describe them in a way that makes clear not only their unity, but also the cause of their unity.

Third, synthesis and analysis, while fundamentally about the unified phenomenon, may be (and often are) undertaken with a focus on just one of the parts. For example, as a scientific (as opposed to purely mathematical) activity, the proof that closed paths of bodies under the influence of a gravitational potential are elliptical ultimately serves the purpose of understanding why the orbits of the planets are elliptical (and thus understanding the cause of the 'unity' between 'orbits of the planets' and 'ellipses'); however, the proof may very well be undertaken without an explicit concern for the planetary orbits. That is, the notion of a planetary orbit *per se* does not enter the analysis (though it may well, and historically did, motivate the analysis). Similarly, one may engage in the sort of history of science that we have in mind without explicitly 'doing philosophy'. Hence, for example, we noted above that in his account of the development of the second law of motion, Westfall focuses on the philosophical context that allowed Newton to develop the requisite concepts. We would say that Westfall is doing history of science, not history of philosophy, but he is doing it synthetically insofar as his account helps to reveal the essential unity between philosophical and scientific concerns. Similar remarks hold, *mutatis mutandis*, for the history of philosophy, and indeed for philosophy itself.

Unity and the Growth of Knowledge

Let us return to Friedman's own work, work that we take to be broadly characterized by the synthetic conception of history of philosophy that we have sketched above. We have suggested that Friedman's approach to history and philosophy need not commit us to the Kantian conclusions he

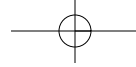


draws about the interaction between the history and philosophy of science and mathematics, conclusions centered on his notion of the relativized a priori.¹⁸ Synthetic history, as we understand it, may be approached from any of a wide variety of frameworks, all of which may be capable of providing insight into the unity of specific intellectual projects that we find emphasized in Friedman's general project. What these frameworks will have in common is their capacity to shed light on that unity. We find, therefore, that Friedman's work brings to light a notion of unity that transcends any Kantian commitments and places us in a dialogue with our intellectual past as well as with the very disciplines we adopt as the objects of our investigations.

In *The Dynamics of Reason* (2001), where the notion of the dynamic and relativized a priori is presented in its most mature form, Friedman gives a reflective account of the inspiration that lies behind his neo-Kantian reading of science. Drawing on the successes and the shortcomings of ideas forwarded by Kant, Carnap, Kuhn, and Quine, Friedman defines more clearly his notion of the relativized a priori as that indelible feature of scientific knowledge that has withstood revolutionary change in the physical sciences and, in so doing, he self-consciously effects a synthesis of many philosophical and scientific ideas of the past.

Moreover, we can see in Friedman's work an illustration of one of the most important rewards of this synthetic approach to the history of philosophy. As suggested in the prefaces and introductions of his most recent books, Friedman's ultimate goal is to put history to work for us by drawing on the resources of the past, specifically by acknowledging both the positive and negative consequences of philosophies gone by. Only in so doing can we draw important lessons about our current situation in the philosophy of science and sustain a hope for progress. (Recall our discussion, above, of how history renders our current intellectual practices intelligible.) In some sense, we might be tempted to say that Friedman is pursuing a natural selection of sorts by embracing those concepts, ideas, and methods that can serve us best in our attempt to delimit the present and future tasks of philosophy in regard to knowledge, and scientific knowledge in particular.

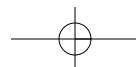
Analogies tend to break down very quickly, and this case is no exception. For though there is a process of selection at the heart of Friedman's project, a Darwinian notion of "survival of the fittest" (which is central to 'Whiggish' forms of history) will not suit our purposes. What Friedman seeks is not simply an appropriation of concepts, methods, and ideas from the past but an *extension, exploration, modification* and above all, a *synthesis* of those concepts, methods and ideas that present the greatest potential for an account of developments in both the history of science and the history of philosophy.

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In this respect, Friedman's notion of historically informed philosophy reaches beyond the somewhat naive claim that our best philosophy must account for historical episodes. Too often we see this path taken in contemporary scholarship. A philosophical doctrine is put forward and legitimized on the basis of the history of science; history serves to justify philosophy simply in virtue of the scattered intersecting points they share in the course of the history of ideas. However, as Friedman's work illustrates, synthetically pursued history does more than justify or legitimate; at its best, it *informs* contemporary philosophy, offering us a mirror that allows us see where we now stand and what methods and ideas we should pursue in order to improve our current philosophical condition. Recognizing the dialectic between philosophy, science, history, and mathematics, our task is to locate those issues that bind these disciplines together so that we can make the most informed decisions about our own methods and concepts as we attempt to move forward. And in Friedman's hands especially, we've seen that this idea *has* inspired progress, for by granting history, science, and mathematics a voice in the pursuit of philosophical questions, he has produced a corpus of considerable range and depth.

Besides inspiring a particular methodology characterized by synthesis, Friedman has thus inspired a particular perspective on our current state of practice. He has, with elegance and sincerity, taken a seemingly humble stand, bringing us down from our own lofty place 'above' the past. History is not, for him, a tool, or a string of repeated failures that tells us what not to do. Rather, history is our inheritance, an ancestor of sorts, and we can effectively steer its continuing course only by embracing our place in the dialectic between history and philosophy. As seen in Friedman's own work, beyond sensitivity to a dialogue between history and philosophy, he is fully aware of the dialogue we, as contemporary scholars, must continue to share with the science and philosophy that came before us. And thus, the dialectic central to Friedman's work extends beyond an examination of how the past has contributed to our present; placing us in this dialogue, we now have from him inherited a responsibility to examine how the past can best inform our philosophical future.



NOTES

1. Among others, the departments, centers, or programs at Boston University, Cambridge University, Indiana University, the University of Minnesota, the University of Notre Dame, the University of Sydney, Pittsburgh University, and the University of Toronto were created in the mid-twentieth century.

2. The conference was sponsored by the United States National Committee for the International Union of History and Philosophy of Science, itself founded in 1956.

3. Giere's attention to the relationship between history and philosophy of science in 1973 very likely went beyond purely academic interest. As a then member of the Department of History and Philosophy of Science at Indiana University, the question of this relationship was presumably a real issue for him, with day-to-day consequences.

4. There have, of course, been numerous other occasions at which the questions raised by Giere's review article have been addressed. For instance, in 1992 Michael Ruse organized a PSA symposium entitled "Do the History of Science and the Philosophy of Science have anything to Say to Each Other?" which included papers by Robert Richards, Rachel Laudan, and Marga Vicedo. The complete texts can be found in *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* (1992), Volume Two: Symposia and Invited papers, 467–96. A recent installment of this ongoing discussion was a conference with the now familiar title "Do Historians and Philosophers of Science have Anything to Say to Each Other?" held at Duke University on March 23–24, 2007.

5. Laudan's "A Confutation of Convergent Realism" and Worrall's "Structural Realism: The Best of Both Worlds?" are reprinted in D. Papineau (ed.) (1996).

6. More specifically, he contends that the history of science, as practiced by historians of science, has no significant bearing on our understanding of what he takes to be the main problem areas for philosophy of science: the structure of theoretical knowledge, the validation of knowledge claims, or the strategy and tactics of research (Giere 1973, 292–93).

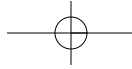
7. This is the title of Friedman (1993), which we discuss below.

8. This, of course, was not a problem for Newton himself, who proposed that the center of the solar system is at rest relative to absolute space. We discuss the relevance of Newton's proposal of an absolute space for his own program of natural philosophy below.

9. The broader scope of Friedman's approach to the history of philosophy and history of science is brought to light in his own contribution to this volume.

10. The relativized a priori emerges as a centerpiece of Friedman's historiography as early as his first book, *Foundations of Space-Time Theories* (1983).

11. Certainly, the tradition of historically informed philosophy and philosophically informed history we refer to includes notable contributors other than those mentioned explicitly. We could have very well focused on others. With limitations of space and time being what they are, we've restricted our attention to cases we find especially illustrative of Friedman's place in this tradition, with no intention of belittling the credit owed to others.



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12. See Friedman (2001), 65–68 for Friedman’s own explicit appeal to Cassirer’s “logical idealism.”

13. For an extended discussion of the use of the term ‘analysis’ in Aristotle, see Byrne (1997). Our discussion in the few paragraphs below follows his chapter 2, though we disagree with Byrne’s view that in the modern period ‘analysis’ came to mean nothing more than ‘reducing to parts’.

14. The full title of *De Gravitatione* is *De Gravitatione et aequipondio fluidorum*. The manuscript first appeared in English translation, with a Latin transcription, in A.R. Hall and M.B. Hall (1962). A new translation by Christian Johnson, with the assistance of Andrew Janiak, appears in Janiak (2004).

15. There is some dispute about the when *De Gravitatione* was written, whether in the 1660s or in the early 1680s, immediately prior to the publication of the first (1687) edition *Principia*. Whatever the case, there’s no dispute that the manuscript was penned prior to the General Scholium, which was added to the second (1713) edition *Principia*.

16. Hall (1948) describes evidence that Newton was reading and studying More possibly as early as 1661. In any case, it is clear that the two knew one another, and that Newton admired More. Newton apparently read other Cambridge Platonists as well, with sufficient interest to take copious notes in at least the case of Ralph Cudworth; see Sailor (1988). For Newton’s broader Cambridge Platonist commitments, see the classic McGuire and Rattansi (1966).

17. On this issue, we (and Friedman) are sensitive to the point, emphasized by several historians of science, that our contemporary disciplinary divisions were foreign to Newton and to early modern natural philosophers in general. See, for instance, Cunningham (1991).

18. For an alternative, though complementary, approach to the generally Kantian features of Friedman’s work, see Mark Wilson’s insightful paper, which is included as the penultimate contribution to this collection.

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