PHILOSOPHY OF SCIENCE AND HISTORY OF SCIENCE: A TROUBLING INTERACTION

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SUMMARY. History and philosophy complement and overlap each other in subject matter, but the two disciplines exhibit conflict over methodology. Since Hempel’s challenge to historians that they should adopt the covering law model of explanation, the methodological conflict has revolved around the respective roles of the general and the particular in each discipline. In recent years, the revival of narrativism in history, coupled with the trend in philosophy of science to rely upon case studies, joins the methodological conflict anew. So long as contemporary philosophy of science relies upon history’s methodology to construct its case studies, it subjects itself to a paradoxical situation: the better the history, the worse the philosophy. An example of the methodological conflict is presented in the case of Antoine Lavoisier. This example also serves our ultimate conclusion, which is that distinctively philosophical methods of case-study design promise enhanced prescriptive powers for philosophy of science.

Key words: methodology, philosophy of history, historiography, case-history method, interdisciplinary (philosophy, history)

...to epitomize the most celebrated parts of their story, rather than to insist at large on every particular circumstance of it.

–Plutarch, on the historiography of his Lives of Alexander and Caesar

1. INTRODUCTION

Although History and Philosophy are distinct disciplines, beneficial interaction between the two is presumed. Contemporary philosophy of science, especially in its ‘naturalized’ version, is committed to reliance upon fine-grained historical case studies. Absent any distinctive philosophical historiography, philosophers develop their case studies according to standard history of science methodology. But insofar as philosophers continue to rely upon a derivative historiography, they are exposed to the risks of a paradox. This paradox emerges over the typical use to which case studies
may be put in the two disciplines, history and philosophy of science. That is, the central role played by narrative in History necessitates ever greater particularity, at the same time that the normative use expected in Philosophy for case study methodology necessitates generality. Consequently, a case study that successfully serves philosophical ends is uninteresting to the historian, perhaps even methodologically flawed; whereas a case study that satisfies historiographical canons is useless for the philosopher, perhaps even methodologically flawed. We argue that philosophers of science have shortcut the power of philosophical analyses – and will continue to do so – so long as they rely upon history’s methods.

We acknowledge in the course of our argument that some philosophers are making forays in the direction of an applied philosophical methodology of case study construction and analysis. Yet, even these philosophers have not yet enunciated the precepts for distinctively philosophical case studies, which is what we call for below.

2. HISTORY AND PHILOSOPHY

History and Philosophy share many disciplinary components. Subject matter topics, for example, are frequently shared. Lectures featuring, say, Aristotle or Hume occur just as naturally in either department. Many History departments – indeed almost all graduate history departments – offer a course called “philosophy of history”. Texts for this course would be located under exactly the same library catalog number as would the texts for a Philosophy department’s course of the same name. In Philosophy departments, a course called “History of Ancient Philosophy” is routinely offered. It would not be unusual to find as a text for this class historian G.E.L. Lloyd’s Greek Science to Aristotle, which might also be used in the History department’s “The Classic Period” course. But subject-matter overlaps such as these do not exhaust the range of common ground between History and Philosophy. Historians, for example, often concern themselves just as intensely as their philosopher colleagues – albeit each in their own way – with such metaphysical issues as “causality”, such epistemological issues as “evidence”, such methodological issues as “objectivity”, and, finally, with matters of logic or logical issues such as “correct argument.”

Yet, for all their overlapping interests, History and Philosophy are distinct disciplines: each driven by its own sort of curiosity. Historical curiosity asks “What went on? how and why did we get from there to here”; Philosophical curiosity asks “What is really going on here? and what is the best way to know it?”

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These sorts of differences carry through to the more specialized disciplines of history of science and philosophy of science. Historians of science cast their gaze backward and ask what went on in a given science, in a given era, and (perhaps) how did we get here from there. Philosophers of science look at a given science and ask what is really going on here, and what is the best way to know it – and, in some instances are in a position to make recommendations about how best to conduct science in future. There are, however, further and significant differences between the historian and philosopher of science, in contrast to the historian and philosopher in general. Historians of science, for the most part, are not interested in whether or not a given science, in a given era, really got it right about the world. That is to say, unlike philosophers, historians are unlikely to be focussed upon justification. Rather, historians content themselves in getting it right about the way the scientists thought, tout court. But philosophers of science, for their part, do interest themselves in the issue whether or not the scientists got (or are getting) it right about the world and the philosophical means to justify scientific belief. Although science and scientific thinking are intrinsically interesting, over and above an historian’s analytic role, many, if not most, philosophers of science are propelled by the belief that science provides the best means of discovering truths about, or, at minimum, correctly describing, Nature. It is this belief which underlies the attraction philosophers of science feel toward normative contexts.

Finally, each discipline has developed its own issues and questions. Historians speak in terms of the presumptions and choices that historians must make in order to write their histories. Every history must have a subject matter, a point-of-view or perspective, and it must selectively examine data, as directed by the subject matter and the perspective of the historian. By contrast, philosophers focus upon problems, which are subjected to logical and conceptual analysis, in order that their underlying general principles may be exposed. The general principles serve many functions, among the most important of which may be normative recommendation, a basis for initiating, modifying, ending or justifying practice. In this normative vein, philosophers’ analyses, unlike historians’ histories, are not functionally limited to the provision of explanations; they may also provide philosophical descriptions, or, in some cases, philosophical clarifications; and most significantly, when used epistemically, philosophical analyses provide justifications. Our focus in what follows is upon normative use of general philosophical principles.

Despite a range of shared contexts, even our brief comparison of the two disciplines’ methodologies reveals little overlap. What we show in the next section to be, the dominance of narrativist methodology in contemporary
history (and mutatis mutandis, history of science) is traced in no small part to the question of the correct methodological role of generalizations. The paradoxical relation between today’s philosophy of science and history is one direct consequence of the victory of narrativism in the disciplinary battle about generalizations. For this reason it is useful to explore some of the key incidents that led to narrative’s dominant role at present.

3. METHODOLOGICAL CLASH: COVERING LAWS AND THE RISE OF NARRATIVISM

Contemporary philosophy of history may justly be said to begin with the publication of Walsh’s *An Introduction to the Philosophy of History* (Walsh, 1951). The major methodological dispute between history and philosophy is tied to Walsh’s book and a now-famous article “The Function of General Laws in History”, published in 1942 by Carl Hempel. Here, Hempel challenged historians, at the risk of their academic credibility, to pay heed to the philosophical underpinnings of their discipline.

As Hempel argued, historians were to reject narrative ‘explanations’ in favor of a “covering law model” of explanation. The critical point of Hempel’s paper was not lost on his historian audience: “if historical knowledge was cognitive knowledge at all it had to conform to the paradigm of explanation provided by [the natural] sciences...” (Murphey, 1986, p. 44). And, Hempel’s thinly veiled threat to the intellectual disciplinary integrity of the field was well-remarked: “either history is a science, in the natural-science sense, or it provides no cognitively meaningful knowledge at all.” (Murphey, 1986, p. 44) Nine years later, Walsh featured Hempel’s claim in his *Introduction to the Philosophy of History*. The response was immediate: historians and historiographers ranged themselves on both sides of the issue, becoming ever more deeply divided: “It could be said without exaggeration that until about 1965 the critical philosophy of history was the controversy over the covering law model.” (Mink 1973, p. 196, emphasis in original). In the end, the debate drove a methodological wedge between philosophers and historians: Hempel’s demand had the singular result that “historians have almost unanimously rejected the covering-law model.” (Mink, 1973, p. 171) And this with impunity, it was said, since otherwise historiography would be nothing more than “an odd positivist fossil in the contemporary intellectual world.” (Ankersmitt, 1986, p. 27)

Although apparently prescient, historians’ refusal to stand as “positivist fossils” in the academy has the marks of a case of ‘right answer, wrong reason’. This because historians’ rejection of the model was not based upon “internal criticism of it, but on the grounds of its irrelevance to their
understanding of the practice of their own inquiry.” (Mink, 1973, p. 171) That is, according to the narrativists, qua historians/historiographers, generalizations are not only unneeded, they are unwanted. With the covering-law model set aside, narrativism dominated historiography, “and the writing of narrative history has revived.” (Roberts, 1996, p. 35) Indeed, “we have witnessed across the whole spectrum of the human sciences over the course of the last two decades a pervasive interest in the nature of narrative [and] its epistemic authority.” (White, 1987, pp. x–xi).

It is difficult to overstate the identification present-day narrativists see between the narrative form and history itself. Narrative, according to their view, “is more than a mode of explanation.” (White, 1987, p. 53) Following Croce’s famous dictum “Where there is no narrative, there is no history” (Croce, 1951, p. 3), contemporary narrativists argue that narrative is not a discursive strategy or tactic that the historian may or may not use, according to some pragmatic aim or purpose. It is a means of symbolizing events without which their historicality cannot be indicated. (White, 1987, p. 53)

For this reason, “the narrative account is the form of understanding and expression used by most historians, and accepted by the public as the typical mode of historical explanation.” (Porter, 1981, p. 1)

We note further the fact that narratives get their explanatory power from their particularity:8 historical knowledge concerns the concrete, the particular, the unique... Historians study the battle of the Marne, not battles in general; they seek the causes of the Enlightenment, not of enlightenments in general; they study the rise of Hitler, not of dictators in general. (Roberts, 1996, p. 8)

Although historiographers turned against Hempel and the ‘scientific’ historians, Hempel’s arguments exhibit an abiding, fundamental philosophical aim. This aim is to find generalizations whose suitability is measured in terms of successfully grounding norms of philosophical, methodological or even scientific practice. One of the major post-Hempelian philosophers of science, Imre Lakatos, aimed to integrate history into his philosophical analyses. Yet, Lakatos’ methodological allegiance to generalization led him away from historically-grounded case study. The next section examines this twist.

4. RATIONAL RECONSTRUCTIONS VS. HISTORICAL CASE STUDIES

Imre Lakatos is a pioneer in the use of quasi-historical case studies. After all, it was Lakatos who paraphrased Kant to the effect that ‘History of Science without Philosophy of Science is blind; Philosophy of Science
without History of Science is empty. (Lakatos, 1970, p. 135) His philosophical analyses rely upon apparently historical accounts of individuals, theories and experiments to ground the philosophical conclusions Lakatos draws. But this is only appearance. In fact, Lakatos’ accounts refer not to actual historical entities, but to simulacrum of these entities, famously known as “rational reconstructions.”

Historians of science might very well object that rational reconstructions – since they are counterfeits – are worthless. But for philosophers, this view is too quickly dismissive: it assumes that the philosophical value of case studies is uniquely bound to their historical adequacy. In fact, for the philosopher, historical case studies succeed on two epistemic grounds, not just one, as would seem to be suggested by the historian’s objection. To see this, we will consider how case studies, or, more simply, examples are typically used in philosophy.

Philosophers make rich and frequent use of examples. The richness is a consequence of the fact that philosophers allow virtually no empirical constraints to limit their free construction of examples. And even in cases which sound embodiable, there often is some idealized premise or boundary condition. A prefect example is Kant’s Store Keeper. Here, with perfect foreknowledge that he won’t get caught, a storekeeper is confronted with the decision whether or not to cheat a child. His decision, Kant held, must obey the main dictate of the categorical imperative: generalizability of the maxim of an action. As is well known, for Kant, the action itself is unimportant; what is important is the maxim of the action. It is the norm, or, in Kant’s terms, the maxim of the action which carries the normative force of the example.

Here the obvious question arises: what is a maxim? To a first approximation it is some neutral description of action, given at some level of abstraction from, and idealization of, details of the example. Not to be missed here is that, in formulating any maxim, decisions are required between the amount of detail necessary to give the example its particular bite, and the level of detail beyond which the maxim would lack general application, thereby exhibiting diminishing normative power. But since the perceived prospects for a normative base turned philosophers to case studies in the first place, they typically opt for general applicability. In the end, philosophers inevitably sacrifice particulars for norms. Let us test this, by means again of Kant’s case.

For one thing, it is quite irrelevant what kind of storekeeper is involved: he might have been a candystore keeper; or he might have been a toystore keeper. In the end, it makes no difference. Also irrelevant is that the storekeeper be a “he” rather than a “she”, or be named Helmut rather than Lisa.
Questions like “In what century?” and “In what country?”, “Where was the storekeeper educated?”, and “Whom did he meet in street as he opened the shop?” are not just irrelevant in this case, they are – or should be considered to be – silly. Indeed, were these sorts of details added, the example would have less and less generalizability, thereby limiting more and more its normative domain. At the limit, were an historically full, rich, robust description – a Leibnizian complete concept – to be offered, the maxim’s normative force would apply to a unit case: the one described. Philosophers would find such a pass methodologically inadequate, and the ‘maxim’ without even a minimal domain of philosophical utility. Nonetheless, such is the stuff of exemplary historiography.

How shall philosophers rein in detail so as to preserve norms? This question illuminates Lakatos’ preference for rational reconstructions: his preference is simply the preference of philosophers in general for example-maxims with large normative domains. But obvious sacrifices aside, in most philosophical use of examples, the targeted domain is not an historico-empirical one. Ethical examples take as their domain the choices of human beings; other types of examples, say, logical or metaphysical ones, are perhaps not so clearly normative as are ethical examples, yet that their respective domains are non-historico-empirical is obvious. But this is manifestly not the case for philosophers of science: their domain, after all, is an historico-empirical one, namely, the behavior – complete with its successes or failures – of scientists seeking to discover natural states of affairs. For this reason, Lakatos’ disregard for the actual, particular historical facts discomfits present day philosophers of science just as much as did the Logical Positivists’ parallel disregard for empirico-historical considerations. How then are philosophers of science to remake their discipline around the rallying cry “History Counts!”?

5. HISTORICIZED PHILOSOPHY OF SCIENCE

The historically-based case study method has been touted in philosophy of science only since the collapse of Logical Positivism. As recently described, the method aims to reformulate traditional epistemological questions to reflect on the cognitive enterprise (including the ventures of science), on its history and on the capacities of those who participate in it, to achieve corrigible formulations of the goals of the enterprise and corrigible accounts of promising strategies for achieving those goals. (Kitcher, 1994, p. 58)

The goal to reformulate traditional questions to reflect the history marks a clean break from Logical Positivism. Historicized philosophy of science should rely upon confrontation with an ‘empirical’ situation to justify
its theories. Yet, here again, Lakatos’ programme should be recalled, for “empirical” does not have quite the same meaning for historicized, case-study-using philosophers of science that it does for the natural scientist.\(^{17}\)

Most philosophical uses of example are in domains, e.g., logic, metaphysics or ethics, where actual historical facts are irrelevant or otiose. By its own design, however, historicized philosophy of science is different. It connects to the historico-empirical world in a manner that other philosophical sub-disciplines typically do not imitate. This is for the reason that normative maxims in philosophy of science get their force from two sources. First, their normative force is grounded in their generalizability, their ability to extend to, to have interesting new things to say about, as yet unexamined cases. This is precisely as in other branches of philosophy. But, secondly, unlike other branches of philosophy, maxims in philosophy of science depend upon their empirical grounding in the historical record.\(^{18}\) Because of this latter aspect, normative force in philosophy of science is grounded in experience, not pure or formal reason. This clarifies the manner in which Lakatosian rational reconstructions differ from the case histories desired by presentday philosophers of science. Rational reconstructions provide normative force via generalizability, just as do examples in other areas of philosophy; but they provide no historico-empirical warrant whatsoever. Thus, the present-day philosopher of science must go beyond Lakatos’ simulacrum of history and into genuine history.

But moving into genuine history makes strong demands upon the philosopher’s historical practice. Meeting or beating the professional canons of contemporary historiography places the philosopher on all fours with the historian: the philosopher is hamstrung by narrativism. Until now philosophers of science have adapted case histories produced by their historian colleagues. But this practice puts the philosopher at risk.\(^{19}\)

6. A DISTINCTIVE METHODOLOGY?

Our argument has proceeded on the assumption that philosophers must adopt historical standards.\(^{20}\) But is this necessarily so? Is it not possible that philosophers might have their own means for producing case studies? We wish to make two responsive points, and then draw our conclusion using a particular historical case.

First, despite the possibility of doing so, philosophers have not pursued a method of case-study design. Lakatos (regardless of his ultimate success) appears to have been a unique instance of selfconscious regard for historiographic methods that might be put to philosophical use. Even \(a\) post-Lakatosian moves to inderdisciplinarity and \(b\) flourishing HPS
Departments only signal philosophers’ tendency to adopt whole the historiographic techniques of their historian colleagues. Secondly, although it may be difficult to conceive how a new philosophical method would differ from the old historical method, this difficulty does not amount to a (non-)existence proof. The paradox should motivate philosophers to reconsider the propriety of those methodological presuppositions currently in use. Indeed, as our final point we wish to advance an argument in favor of a distinct methodology, for we think that the long-term success of philosophy of science calls for just this, despite the fact that the project has dropped off the agenda since Lakatos’ influence waned.

To move to our final point, we consider the case of Antoine Lavoisier’s feature role in one of the most exciting episodes of modern science – the overthrow of the phlogiston theory and subsequent acceptance of the oxygen theory. This scientific change accounts in no small way for Lavoisier’s frequent appearance in case studies authored by both historians and philosophers. We wish to press into service once more his scientific role, with the aim to 1) illustrate the paradox, and 2) motivate our conclusion that philosophers need their own case-study methodology.

First, sufficient to show the methodological cross-purposes described above to be a paradox, we point to a major element of Lavoisier’s life which could not be left out of an historian’s explanatory narrative, but could (perhaps, should) be left out of a philosopher’s normative analysis. Arthur Donovan’s biography of Antoine Lavoisier Antoine Lavoisier: Science, Administration, Revolution has been well received. As clearly indicated in his book’s subtitle, Donovan has an interest in more than Lavoisier’s scientific life. He is interested in Lavoisier’s career as an administrator and as a figure in the French Revolution. Both are legitimate historical topics, both fairly standard to any historical narrative about Lavoisier. For example, the role of Lavoisier’s administrative theories and practices may work well to explain Lavoisier’s predisposition to look for equations as ‘balance sheets’ in his chemical discoveries.

Donovan writes that in Lavoisier we find “a career unified by two themes: an ‘18th-century version of positivism’ based upon reasoning by experiment, and a driving, almost ruthless ambition.” No philosophical study of Lavoisier should ignore his “‘positivism’ based on reasoning by experiment.” And historical accounts, in all likelihood, would feature Lavoisier’s ‘positivism’ as well. On the other hand, no historical narrative justly omits Lavoisier’s “driving, almost ruthless ambition”, whereas philosophers are enjoined from adverting to personal styles in any philosophically acceptable analysis of an idea’s long-term justificatory merit. This opposition shows the paradox.
But our second point is more challenging – and, we believe, more important. Can the paradox support a call for distinctively philosophical methods of case-study design? We hope to show that not only does the paradox raise such a call, but also that such methods promise an enriched philosophy of science.

Lavoisier's ambition is irrelevant to a philosophical study of his contributions to, or role in, this critical episode of scientific change. It is a philosophically irrelevant factor because, philosophically speaking, the genesis of scientific ideas bears in no way on short-term or long-term justification. This is not to deny that there are other–sociological–implications of Lavoisier's driving ambition. For example, without such ambition, it is difficult to imagine Lavoisier (or anyone else, for that matter) to have achieved what he did in one truncated lifetime. Perhaps Lavoisier's driving ambition might even have been responsible for his initial choice to enter science. That is, he could conceivably have asked himself 'where can I make a name for myself, fast?' and answered 'science'. But this sort of sociological-cum-psychological information is of no consequence to philosophers and philosophers of science seeking to use history to reveal epistemic forces at work.

"Driving ambition", as an example, has not been shown to carry epistemic force, i.e., to further epistemological aims. We hasten to add that the question of epistemological force in the case of “driving ambition” (and perhaps other marks such as gender diversity, geographical location, political allegiance, birth order, caffeine consumption, etc., etc.) should be viewed as open, or as not having been ruled out, given the lack of empirical studies of such issues. It is a wasteland out there, save for Donovan et al. (1988) and now, more recently, (Sonnert et al., 1996) (Wennemars et al., 1997) and (advisedly) (Sulloway, 1996). Many more studies of this sort are needed before philosophers can even begin to theorize upon the epistemological significance (or lack thereof) of the various marks that might, and certainly have been, claimed to carry epistemic force.26

Furthermore, only on the basis of such studies might philosophers of science adjust their use of history. In the end, the strongest use to which philosophers can put history is to learn how to learn prospectively. In the case of philosophy of science, the strongest use is to identify factors which justify and ultimately underwrite prescriptive generalizations concerning science. Methods which would single out, or narrow the range of successful past learning strategies would be of enormous philosophical use for evaluating present and future scientific strategies and programs.27

We find here some indicators of what a philosophical methodology might require. One clear requirement is a criterion (or criteria) which could
be used to rule out the purely idiosyncratic features of the scientists or their situations. For example, although philosophers would proceed (at least initially) on a case-by-case basis, it would be disastrous to fall into the narrativist trap embodied in the methodological rule that each case is essentially unique. That is, it is of course empty to say “There can be only one Lavoisier.” This criticism alone suffices to rule out maxims along the lines of “To be a good chemist, be another Lavoisier.” However, at the other end of the scale, it is by no means entirely trivial to remark what Donovan calls “Lavoisier’s ‘18th Century Positivism’” and thereby conclude “To be a good chemist, reason from experiment.” If we take these two maxims to define opposite ends of a scale, then some aspects of an historiography for philosophers emerge. For example, while there cannot be another Lavoisier, there most certainly can be another scientist who reasons from experiment. This move itself reveals possible avenues for further investigation. For example, in the historical situation, it was essential that Lavoisier was male. Might empirical trials show gender to be essential, inessential, or to have any measurable influence on goal-achievement in science? There are no a priori answers to this.

7. CONCLUSION

Philosophy of science relies on a case-study method but present method is antithetical to philosophical goals. While this finding alone may motivate the conclusion to devise our own method(s)\(^2\), it is fairly apparent that an even stronger motivation sits in the historical record of science itself. In saying this, we own up to what has operated as an implicit assumption in the above. What is on offer is sustained by a certain theory concerning the value of methodology. Namely, disciplinary progress coincides with disciplinary consensus about method. This theory is underwritten by history: a state of rival methods is coincident with disciplinary dissension, even disarray, and lack of progress; conversely, intradisciplinary consensus on method focusses problems and solutions, and is coincident with progress.\(^3\) Our ultimate conclusion should be obvious. By extension, it is reasonable to expect that real progress in historicized philosophy of science is to be had from reaching a methodological consensus. But, given the paradox, this requires devising our own method.

Obviously, our arguments raise only initial considerations involved in developing a philosophical methodology of historical case studies. However, modest as they are, the conclusions we draw are borne out by the successful limited efforts made so far to design empirical tests of philosophical analyses. There are potential philosophical, rather than just his-
torical, fruits to be had from renewed attention to method. History does count, but not much without method.

NOTES

1 For critical remarks on earlier versions of this paper we thank the Philosophy and STS faculties at Virginia Tech, Eric Palmer, and participants in the Pittsburgh Center for Philosophy of Science’s Third Quadrennial International Fellows Conference.

2 “The tension between the particularity of historians’ case studies and the philosophical imperative for generality and for norms remains unresolved.” (Gooding, 1997, p. 121) Of course not all philosophy of science is normative, nor is all history of science narrative. But that is not our concern here. Rather, we are interested in the typical uses to which case studies are put in the two disciplines. And those typical uses are clearly normative and narrative, respectively.

3 Philosophers, for example, might ask “What is causality?”, whereas the historian might ask, “What is historical causality?”; similarly, “What is good evidence?” vs. “What is good historical evidence?”, and “What is objectivity?” vs. “What is historical objectivity?”

4 There surely are radical egalitarians who deny that science has any epistemic or methodological privilege. But we will not attempt to combat such denials here. Also, we recognize that philosophers need not believe anything at all about scientific powers or success; we offer no survey of the deontic commitments of philosophers of science. Nevertheless, the evident professional concern with justification, as opposed to description, sets apart philosophers and historians.

5 Of course, there are other reasons as well. An important one is the increasing shift by history of science toward the “particularistic, with closer and closer attention to the local and the situated.” (Nickles, 1995, p. 149) According to the strongest interpretations of these shifts, “every single bit of research is so local and situated, so context-specific, that no general guidelines could possibly be of any use...” (Nickles, 1995, p. 159). Moreover, not only history has experienced the rising tide of narrativism: “...we have witnessed across the whole spectrum of the human sciences over the course of the last two decades a pervasive interest in the nature of narrative [and] its epistemic authority.” (White, 1987, pp. x-xi).

6 Our argument by no means intends to ignore earlier historiographies of science, particularly those of Duhem, Koyré, Dijksterhuis, Meier, and Burtt. Rather, our argument intends to make methodological points about historical writing in general. In any case, Mink certainly speaks for the majority when he says “since the publication in 1951 of An Introduction to Philosophy of History by W.H. Walsh, however, the critical philosophy of history has been for the first time an acknowledged philosophical discipline.” (Mink, 1973, p. 166).

7 The perceived significance of the issue is evidenced further by the Social Science Research Council’s instigation of a 5-year effort to investigate the rôle of generalization in history. (Gottschalk, 1963) Nicely enough, the committee appointed a committed covering-law philosopher of history, Hans Meyerhoff, to be its consultant on philosophical matters. (Gottschalk, 1963, p. vi).

8 “In the last resort the historian’s explanation of what happened is not a piece of general reasoning at all. He explains the French Revolution by describing exactly what it was that occurred; and if at any point we need further elucidation all that he can do is to take us into greater detail, and make us see in still more definite concreteness what really did take
place.” (Butterfield, 1935, p. 72).

9 Lakatos: “one way to indicate discrepancies between history and its rational reconstruction is to relate the internal history in the text, and to indicate in the footnotes how actual history ‘misbehaved’ ” (Lakatos, 1971, p. 120). Of course the relation between the entities in the reconstruction and actual history isn’t entirely accidental. That is, we read, Lakatos’ ‘Bohr’ is how the actual Bohr might have been: “Internal history is not just a selection of methodologically interpreted facts; it may be, on occasion, their radically improved version. One may illustrate this using the Bohrian programme. Bohr, in 1913, may not have even thought of the possibility of electron spin. He had more than enough on his hands without the spin. Nevertheless, the historian, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme. Bohr might have referred to it in 1913. Why Bohr did not do so, is an interesting problem which deserves to be indicated in a footnote.” (Lakatos, 1971, p. 119).

10 Philosophers typically disdain objections such as “But that could never happen”. Some of the most beloved examples from modern philosophy are of this ilk: Bowsma’s ‘brain in a bottle’, Smart + Place’s ‘brain-o-scope”, etc.

11 We choose this example for three reasons. First, Kant’s work is excruciatingly bare of examples; secondly, this example is well-known; and, finally, it is the paradigm of the features of generalization and normative import which here concern us.

12 Compare the philosopher here with the historian: “If a ‘generalization’ were defined as a general law, detachable from its context and applicable to all comparable situations and if this were adopted as an exclusive definition, it would have to be granted that historians do not often make generalizations and probably should not.” (Aydelotte, 1963, p. 147).

13 Of course, there are cases where these types of detail, or particulars, are relevant. But this is exactly the thrust of the paradox. Historians face similar issues in their narratives: they must choose which details to include on the basis of which provide a “fair representation”, a description “of the fortunes of a central subject...in which all major changes to its characteristic properties and relations are described.” (McCullagh 1987, p. 35) Obviously, the problem of what is “characteristic” rears its head here, as duly noted by (Murphey, 1994, p. 296).

14 Of course, the historiographic methods of case studies became entrenched far earlier in History of Science (cf. our fn. 6), but to ends that were avowedly non- (even anti-) philosophical (Dijksterhuis, 1961).

15 Kitcher’s “promising strategies” may or may not dovetail with our normative focus. Although, on its face, “promising strategies” seems to have normative connotations, the phrase is also consistent with straightforward inductivist intentions.

16 We limit our focus here solely to the Logical Positivists’ justificatory arguments. As is well known, many of the Logical Positivist philosophers were themselves familiar with contemporary scientific work; and Mach, of course, immersed himself in the history of mechanics. That such familiarity shaped their ideas cannot be denied; however, in the end, justification depended essentially upon logic and conceptual analysis. This is not to say that some philosophers of science do not, in their work, directly confront the very same empirical world as some scientists. For example, quantum theoretical work done by philosophers is often indistinguishable from that done by physicists.

18 To wit, “Do this, because it has been empirically successful in the past.” Proffering the same reasoning to Kant’s Storekeeper, for example, seems to lose any moral high ground otherwise achieved.

19 This practice of confronting philosophical assertions with typical critical zeal, yet sim-
ultaneously bracketing the historiography and treating it with unquestioned acceptance can have disastrous consequences. See Pinnick (forthcoming, 1997), wherein it is all too clear that philosophers need to do their own historiographic spadework.

We have not argued this assumption, but find evidence of its implicit adoption easy to offer: “philosophy of science, so conceived, is logically and methodologically dependent on the historiography of science because, first, in order to find such principles, the philosopher of the history of science or theoretical historian needs history-of-science explanations.” (Finnochiaro, 1973, 173).

Debate has been going on for some years regarding the precise relationship to be sought between historians of science and philosophers of science. But our point here is controversial only as to the intent of those involved in interdisciplinary efforts; actual practice, as we have maintained, is that philosophers adopt historians’ histories and methods, simpliciter. The opening stages of the controversy are in (Giere, 1973) and (Burian, 1977).

Several philosophers of science have recently produced case studies which embody the initial phases of a distinctively philosophical historical methodology: cf. (Irzik, 1996), (Norton, 1995), and (Lennox, 1996).

The reader must be wondering by now when Larry Laudan’s work will be mentioned. Laudan’s philosophical affinities with Lakatos’ programme, and the indisputable claim that Laudan, in this generation of philosophers, has single-handedly restored methodology to prominence, make his non-appearance in the body of our argument perhaps curious. However, other than Laudan (1977) and Donovan et al. (1988), we are not sure that his focus has ever been on case-study design for philosophers.

Lavoisier pioneered the use of balance-book accounting, in which no sum was ever lost; rather, it moved from one side of the profit-loss equation to the other. Given his belief in the law of the conservation of matter/weight, it seems no accident that similar sorts of equation came to be applied as descriptions of the movements of weights during chemical reactions. Cf. (Gale, 1979).

We here invoke unabashedly a discovery/justification dichotomy and the underlying commitment to objectivity. Our second point below uses this dichotomy to argue for philosophical standards of case-study design.

To sample the risks, see Harding’s claim for feminist epistemic privilege (1992), and Pinnick’s rejoinder (1994).

The practical, or applied, spin-off should be obvious. For example, such methods could guide policy decisions regarding distribution of scarce resources.

Multiple methods are not unlikely; they simply cannot, in the end, be rivals.

Evidence for the theory is easy to hand. Shanks et al. (1994) show the empirical problems physiology experienced due to the controversy between Claude Bernard’s proposed methodology and its rivals. Kraghe (1996, pp. 61–71) demonstrates both the stagnation in modern cosmology during the controversy over Milne’s hypothetico-deductive methods vs. Dingle’s inductivism, and the progress made in the science once methodological consensus had been reached. Searle (1974) points out the gains made in linguistics once Chomsky’s methods had been accepted over those of his behaviorist rivals as the key element of the disciplinary methodological consensus.
REFERENCES


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