History and Philosophy of Science After the Practice-Turn: From Inherent Tension to Local Integration

1. Introduction: The Myth of Inherent Tension

Over the past several decades, a pernicious myth has taken hold. This is the myth that history and philosophy of science are *intrinsically opposed* to one another: as if the two fields have timeless essences that tug against one another in equally timeless tension.[1] The myth has a certain fascination for purists on both sides, and even speaks an important truth about the present configuration of disciplinary standards. But it is a myth nonetheless, and one that stands in the way of a more productive analysis of the value of history of science for philosophy of science.

My goal in this essay is to dismantle this myth, and in so doing, to indicate a more fruitful way of analyzing the value of history of science for philosophy of science. Crucial to my argument is a particular conceptual shift. Too often, discussions of the "HPS problem" have concentrated on *global integration*: on articulating a single model that captures how history and philosophy of science ought to interact. But this project encounters many of the difficulties that confront the project of articulating a singular scientific method. In particular, by cleaving too narrowly to the ideal of generality, philosophers are left without much to say—at least much that is all that interesting (see Nickles 1987). But philosophers *do* use historical sources and information in their practices, in myriad productive and legitimate ways. This suggests a different focus for methodological reflection: one that starts by asking how philosophers *use* history to accomplish their heterogeneous philosophical ends. Rather than search for a single model of how history and philosophy of science are related, we should instead try to characterize

[1] In this essay, I will use the expression "history and philosophy of science" as a shorthand for "history of science and philosophy of science" and also to refer to the discipline of HPS. The intended meaning should be evident from context clues (my title employs the former meaning).

how different methodological approaches in philosophy of science engage productively with history, including both primary sources and historical scholarship.

My argument is set out in three broad sections. In the first, I explore in greater detail the "myth of inherent tension": the notion that history and philosophy of science are intrinsically opposed to one another. The crux of this myth is the supposed opposition between philosophy's "normative and universalist orientation" and history's uncompromising particularism (Stadler 2014, 762). Because these perspectives are set at cross-purposes, no reconciliation between the two disciplines is thought to be possible—or even desirable. I show that this claim rests on a faulty assumption, as well as a descriptively inadequate conception of philosophical practice. Replacing this with a more adequate conception is crucial to gaining traction on the focal question: what is the value of history of science for philosophy of science? But the reigning conception is deeply ingrained, and for that reason difficult to see around. It is the task of Section 3 to say why this is the case, and ultimately to point the way towards a more acceptable picture of philosophical practice. Finally, in Section 4, I begin the project of characterizing how different methodological approaches in philosophy of science use historical sources and information to achieve their philosophical ends. The paper concludes with a brief discussion of philosophical normativity, and suggests that normativity be regarded, not as a constitutive part of philosophy's disciplinary identity, but rather as a desideratum of certain projects within philosophy.

2. History, Philosophy and the Roots of a Problem

The relationship between history and philosophy of science has been a subject of conversation for over fifty years, as well as an inspiration for many melodramatic conference titles. (Two favorites: "Do the History and Philosophy of Science Have a Future Together?"

(2006) and "Do the History and Philosophy of Science Have Anything to Say to Each Other?" (2008).) At issue has usually been the right way of conceptualizing the relationship between the disciplines of history of science and philosophy of science. The trouble, as David Marshall Miller observes, is that these disciplines seem "inherently at odds with one another" (Miller 2011, 38). Philosophy of science, at least in the Anglophone world, is a normative discipline concerned above all with understanding how science succeeds in predicting and explaining features of the natural world. History of science is a descriptive enterprise concerned to understand scientific activities in context: in relation to the range of factors that influence research agendas and affect the content and reception of scientific ideas. This difference in methodological orientation generates a tension between the two disciplines, which the historian Kenneth Caneva describes as follows:

> We historians deal with time-bound particulars, and our truths lie in those particulars. In contrast, philosophers seek timeless truths from which the historical particulars have been distilled off. (Caneva 2011, 51)

Not for historians the sanitized abstractions of philosophers! Historians wade about in the swamp of scientific and cultural practice. Theirs is a discipline of concrete details and human intrigue; of power dynamics and political entanglements. Philosophers, in contrast, like to pretend that science is "like the Persian God Khoda, whose source is only itself" (Proctor 1991, 271). In light of these differences, could it be any more evident that history and philosophy of science have nothing to say to each other?[2]

[2] Jouni-Matti Kuukkanen tries to establish the inherent tension between history of science and philosophy of science another way. He argues that the "failure to [properly] integrate history and philosophy of science" owes to the incompatible metaphysical assumptions of historians and philosophers, respectively. Whereas historians tend to be "historicists, who assume that all objects of research are variable in principle," philosophers tend to be essentialists, on the prowl

These differences in methodological orientation are reflected by differences in standards. Philosophers seek—if not always, at least in the main—to generalize, simplify and reveal unity in evident diversity.[3] They delight in showing that apparently disparate things are connected by some hidden thread; that all scientific explanations share a logical structure, or that all theories are just families of models. Apropos of these aims, philosophers place great value on "clarity and precision, generalizable or abstract concepts and arguments, properly justified decisions, and ideal epistemic situations" (Schickore 2018, 197). Together these guide philosophers round the shoals of concrete detail which menace the passage into wider spaces and (it is hoped) deeper waters.

By contrast, historians delight in differences—in teasing apart history's tapestry to reveal strands of complex texture. This has not always been the case (see Mauskopf and Schmalz 2011); but at least since the 1980s, the focus in academic history has shifted from the streamlined and synoptic "to the dense and detailed" (Daston 2012, 117). Contextualist approaches in particular have demonstrated the importance of making synchronic connections between scientific activities and the contextual factors that elucidate their meanings in particular times and places (Maza 2017). In fact, such approaches have become so much the norm that it has registered in "the adjectives [used] to praise lectures," which have gone from "Bauhaus...to Baroque: good papers are 'rich', no longer 'acute' or 'incisive'" (Daston 2012, 117). Philosophers, of course, remain unrepentant modernists, with a taste for right angles and sharp reliefs. Or so the story goes.

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for "permanent and essential qualities" (Kuukkanen 2016, 3). From this he concludes that there "does not seem to be any middle ground between historicist history and essentialist philosophy" (6), and that historical and philosophical studies "simply pull in different directions" (8).

[3] This is what I'm doing right now!

This is the myth of inherent tension in a nutshell, and if I've told my story well, parts of it should ring true. Historians and philosophers *do* have vastly different aims and standards, which makes collaboration across disciplinary lines difficult. More importantly, divergent aims and standards render much work in history of science irrelevant to philosophers of science (and vice versa). But it's also true that this wasn't always the case—and ironically this is the root of our present predicament. There was a time, not so long ago, when history and philosophy of science were sufficiently aligned to suggest a new disciplinary venture: *history and philosophy of science* (HPS). Beginning at Leeds in the 1950s, HPS gained a modest institutional toehold during the next two decades in Great Britain and the United States (Laudan and Laudan 2016). But just as the process of consolidation had begun, the two halves of HPS began to drift apart. The story of how this happened is a familiar one (see, for instance, Pinnick and Gale 2000; Steinle and Burian 2002; Miller 2011; Daston 2012), yet it is a story worth telling, as certain features of it serve to stabilize and support the myth of inherent tension.

The shortest version of the story is told by Lorraine Daston, and goes like this. Soon after the disciplinary consolidation of HPS, "historians of science became historians," and in so doing parted ways with philosophers, with whom they had previously shared a broad intellectual orientation (Daston 2012, 119). Whereas earlier historians of science had exalted scientific ideas (much to the liking of their philosophical colleagues), beginning in the 1960s, a wave of historiographical discontent broke across the young discipline.[4] Ironically, it was Thomas Kuhn's *Structure of Scientific Revolutions* that set the wave rolling. Kuhn was a great enthusiast of scientific ideas (Dear 2012), but it was his suggestion that scientific change is not entirely rational that had the greatest influence on subsequent disciplinary history (Miller 2011). The

[4] Here I refer to the discipline of history of science, although historians employed in HPS departments were no less affected.

debate touched off by this suggestion had the effect of knocking science off its cultural pedestal (now it was just one institution among many, subject to ordinary forms of social determination), and in the span of several decades, intellectual history of science had fallen out of vogue (Laudan see 1993). But since intellectual history of science was *the kind of history relevant to philosophical research*, the Kuhnian wave had the unintended effect of throwing history and philosophy of science out of alignment. And this is the state they remain in today, with philosophers pursuing their philosophical goals and historians pursuing their historical goals in relative isolation (Steinle and Burian 2002).

Two aspects of this story warrant highlighting. The first and most important is the suggestion that what threw history and philosophy of science out of alignment was the fact that—sometime during the 1970s and '80s—historians of science became historians. This implies that, prior to this time, historians of science were not (yet) historians; and this is what allowed them to link arms with philosophers. Now grown-up and professionalized, history of science works at cross-purposes with philosophy, and there is no reason to hope for a reversion.

There is a whiff of teleological thinking in the claim that history and philosophy of science were drawn apart when history of science achieved its 'mature' disciplinary form—that is, when it came to adopt the standards and practices of late-twentieth century professional history. But this is not my worry here. Rather, my worry is that the story, as I have presented it, presupposes a static conception of philosophy of science: so while history of science matured from an intellectualist to a contextualist orientation, philosophy of science remained unreformed. Maybe the thought is that philosophy of science settled into its mature disciplinary form earlier than history of science. But setting aside the idea that there *is* such a thing as the mature disciplinary form of philosophy (or history) of science, what are we then to make of the

considerable changes in philosophical practice that have taken place since the 1970s (Soler et al. 2014)? Today, few philosophers of science fit the image drawn by Kuhn way back in 1976:

> The philosopher...aims principally at explicit generalizations and at those with universal scope…His goal is to discover and state what is true at all times and places rather than to impart understanding of what occurred at a particular time and place. (Kuhn 1977, 5)[5]

It is simply not true that philosophers of science are uninterested in imparting an understanding of what occurred at particular times and places, as my examples in Section 4 illustrate. Moreover, while generalizations of wide scope remain a desideratum in many areas of philosophy, philosophers of science are increasingly recognizing that generality is just one virtue among many, and that it may not be attainable in many (most) cases. As James Woodward writes:

> When one considers the enormous variety of activities and disciplines that fall under the heading of 'science' and the great variation these exhibit over time, it seems implausible that there will be many interesting exceptionless truths about how science always works. (Woodward 2011, 171)

I will have more to say about recent methodological developments in philosophy of science in Section 3. For now, let us raise a skeptical eyebrow at the suggestion that, while historians of science became historians during the 1970s and '80s, philosophers of science remained unreformed.

The second aspect of the story I want to highlight is the emphasis it places on disciplines. What Daston wants to explain is how the *disciplines* of history of science and philosophy of science drifted apart—something she attributes to a divergence in disciplinary standards during

[5] This quotation is drawn from a lecture first delivered in 1968, and revised in 1976.

the second half of the twentieth century. But while a disciplinary focus is useful for explaining a certain centrifugal tendency in HPS, it is by no means clear that history's value for philosophy of science is best analyzed in disciplinary terms.[6] Focusing on disciplines leads us to ask, 'what is the appropriate way for history and philosophy of science (conceptualized as broad methodological monoliths) to interact?' But history and philosophy of science are not methodological monoliths; they are highly diverse intellectual enterprises that must be integrated locally, if at all. This local integration will be my focus in Section 4, when I turn my attention to the relevance of historical sources and information for particular methodological approaches in philosophy of science. Before coming to this, however, it will be useful to reflect on how philosophical practice has changed since the 1970s, and to ask why this hasn't been reflected in discussions of the relationship between history and philosophy of science.

3. HPS Before and After the Practice-Turn

Ever since Ronald Giere's influential article (1973), the preferred idiom for discussing the relationship between history and philosophy of science has been that of romantic relationships. Are history and philosophy of science *truly committed* to one another, and if so, does their relationship have the "passionate involvement and deep communication" needed to sustain a marriage (283)? Giere's answer, delivered through a provocateur's grin, is 'no'. HPS is at best a "marriage of convenience," institutionally advantageous but intellectually gratuitous (296). While philosophers require an anchoring in scientific practice, there is no obvious reason to prefer the science of the past to the science of today. At any rate, it isn't obvious that history of science *as practiced by historians* "has anything essential to contribute to the content of

[6] I take it that Schickore (2011) is making a similar point when she says that "In my view...the recent debate about how to 'combine' history and philosophy is misguided from the outset" (471).

contemporary philosophy of science" (286). Because philosophy is a normative enterprise, and history merely descriptive, attempts to extract philosophical lessons from the historical record are menaced by the naturalistic fallacy (see also Hanson 1962). From this, Giere concludes that the "historical approach to philosophy of science" presently lacks "a conceptually coherent programme" (Giere 1973, 292).

Unsurprisingly, Giere's views met with strong resistance from those committed to the conceptual coherence of HPS (e.g., McMullin 1976; Burian 1977; Shapere 1977; Krüger 1982). Responses typically took the form of arguments seeking to establish that history of science is indispensable to some activity taken to be central to philosophy of science. So, for example, Richard Burian writes that "historical considerations" are needed to evaluate "proposed philosophical norms for the evaluation of the degree of support for theories and the worth of explanations" (Burian 1977, 1). Since "the validation of knowledge claims" was one of three problem areas identified by Giere as central to philosophy of science (the other two were the structure of scientific knowledge and the strategy and tactics of research), history of science emerges as an important symbiont of philosophical analysis, albeit not necessarily an obligate one.[7]

I am not presently interested in re-litigating "marriage debates"; rather, I want to highlight an assumption that Giere shares with a majority of his critics. This is the assumption that philosophical research targets a small number of highly abstract and general problems (e.g., the structure of scientific knowledge, the nature of scientific rationality, models of scientific

[7] Giere had already denied that history of science has a role to play "[in] validating an account of empirical validation," since the naturalistic fallacy precludes the use of descriptive historical information to support normative claims (Giere 1973, 294).

change), and evaluates them using empirical data.[8] For Giere, these data are best gleaned from contemporary scientific practice; for his critics, it is history of science that supplies the most informative data set. But wherever the data come from, their *use* is basically the same: evaluating and supporting highly abstract and general claims about the nature and workings of the sciences.

This means that the debate touched off by Giere is a debate between people who share a broadly similar view of what philosophy is. It is a discipline characterized above all by the highly abstract and general nature of its questions. Notably, this agrees with an image of philosophy that has long been popular among Anglophone philosophers, and which some have suggested played an instrumental role in solidifying philosophy's professional identity (Kuklick 2001; Doyle 2014; Smith 2016). In the words of Hans-Johann Glock:

> Philosophy as a distinctive intellectual pursuit is constituted at least in part by problems of a peculiar kind. These problems are *supremely abstract and fundamental*, and they include questions such as 'Can we acquire genuine knowledge?', 'How is the mind related to the body?', and 'Are there universally binding moral principles?' (Glock 2008, 872, emphasis added)[9]

While abstractness and generality are probably the most important good-making qualities of a philosophical problem, they are not the only such qualities. In addition, philosophical inquiry is frequently held to be *normative*: philosophy should concern what ought to be the case, not simply what happens to be the case. HPS was founded on the conviction that history of science can teach us something about how the sciences ought to operate, not simply how they do operate

[8] Jutta Schickore describes this conception of philosophical practice as the "confrontation model" of HPS, because it involves confronting general philosophical claims with historical data (Schickore 2011, 2018). Schickore—who is highly critical of the confrontation model—claims that it "dominated the debates about HPS" for nearly four decades.

[9] In an insightful essay, Alan Richardson labels this conception of philosophy *philosophia perennis* (Richardson 2012). According to it, philosophy is a discipline "that constantly considers certain perennial questions," and that is characterized by certain features (a liberal use of abstraction, a concern with argument and truth, etc.) that are stable over time.

(the domain of history of science). In particular, it was founded on the conviction that history can provide a window on the problem of scientific rationality by "enunciating normative principles of scientific inquiry" capable of illuminating the problem of scientific change (Laudan 1977, 147).

This vision quickly fell to pieces. Although the enterprise of HPS did not disintegrate, any hopes of organizing the discipline around a shared concern for scientific rationality and models of scientific change did not survive the 1980s (Schickore 2011). Ian Hacking's "New Experimentalism," in particular, shined a harsh light on philosophy's traditional "obsession with representation and thinking and theory, at the expense of intervention and action and experiment" (Hacking 1983, 131). In addition, a large number of problems arose from an increasing scrutiny of the special sciences, and in particular, from evolutionary biology and genetics (Waters 2013). The joint effect of these developments was that, by the time the Laudans launched their "Scrutinizing Science" project in the mid-1980s, the goal of adjudicating theories of scientific change had become unfashionable.[10] There was simply too much to do—too many horizons beckoning—to re-enter the fray with the likes of Kuhn and Laudan. Alison Wylie provided a "view from the trenches" in 1994:

> As HPS practitioners have scrutinized particular sciences, their diversity has come more clearly into focus and this has generated, in turn, vigorous programs of research that take an increasingly wide range of 'special' sciences—including various life sciences, earth sciences, and social sciences—as a legitimate primary focus of concern. Increasingly these are recognized to be philosophically interesting in their own right, not just a resource for testing (a source of counterexamples), or an export destination for models of "real" science. This has put considerable strain on traditional approaches to HPS... (Wylie 1995, 393)

[10] The project was actually called "Scientific Change," but most will know it from the capstone volume, *Scrutinizing Science: Empirical Studies of Scientific Change* (1988).

This was just the beginning. Throughout the 1990s and continuing into the new century, the trends Wylie identified have only intensified.[11] Today, a great many philosophers profess to ground their work in scientific practice—so many, in fact, that it has begun to generate a backlash. One concern is that by "abandoning theories of [scientific] practice" in favor of "piecemeal description[s] of local procedures and strategies," a major aim of mid-twentieth-century philosophy of science has been lost (Nordmann 2015). Another is that, by aligning themselves more closely with the descriptive aims of sociologists and historians, philosophers risk losing "the prescriptive tendency for which philosophy is famous [or infamous?]" (Sample 2017, 55).

These worries clearly concern the problems practice-based philosophers of science investigate; they seem insufficiently abstract, general and normative to count as *philosophical* in any robust sense. But here a caution is enjoined. For however obvious it may seem that philosophy asks supremely abstract and general questions, this is not something that is inscribed on the soul of the universe. Instead, it is a convention rooted in the recent institutional history of philosophy, and as the example of history of science reminds us, such conventions are mutable. As the historian of philosophy Justin Smith useful remarks, "it is only very recently that the self-conception of philosophers has become entirely separate from that of those who are now called 'scientists'" (Smith 2016, 51). I say this not to enter a methodological plea for naturalism; only to point out that philosophy is not an eternal activity with a stable essence, but rather a highly

[11] A good indication of this is the founding, in 2006, of two professional societies: the Society for Philosophy of Science in Practice (SPSP) and the Committee for Integrated HPS (&HPS). The former seeks "a productive interaction between philosophical reasoning and a study of actual scientific practices, past and present" (SPSP Mission Statement). The latter seeks to foster work on a broad range of questions in history and philosophy of science, and testifies to the staying power of HPS after the breakup of its early research agenda.

pluralistic enterprise that continues to evolve and expand as new problems and methodologies arise.

These observations point to an obvious question. Why have so many discussions of history and philosophy of science failed to track changes in philosophical practice since the early days of HPS?[12] Why do we still read that philosophy is a normative discipline interested only in general knowledge—"timeless truths from which the historical particulars have been distilled off" (Canvea 2011, 51)? Why is 'the philosopher of science' caricatured as an essentialist, interested only in the unchanging features of objects and theoretical concepts (Kuukkanen 2016)? And why do philosophers still refer to "the goal of philosophy of science" as "[articulating a conception of] scientific rationality" (Kourany 2010, 42)? My only guess is that it is a residue from earlier debates (like the marriage debate), or else a product of assimilating too completely philosophy's professional image (Doyle 2014). Whatever the case, attempts to crystalize an essence of the field fail in light of the outrageous thematic and methodological diversity in contemporary philosophy of science. To gain traction on the value of history of science for philosophy of science, the first thing we need to do is face this diversity openly.

4. History of Science in Philosophical Practice

It is time at last to tackle the focal question of this essay: what is the value of history of science for philosophy of science? As I indicated in section 1, my approach puts philosophical practice front and center. This means that I will take an extremely liberal view of what counts as

[12] Not all discussions have ignored the turn to practice (see, for example, Arabatzis and Schickore 2012; Chang 2011; Miller 2011; Schickore 2011, 2018). Nonetheless, it remains disconcertingly common—even in articles that acknowledge the heterogeneity of philosophical practice—to conceptualize philosophy as an abstract, normative enterprise for the purpose of interrogating the relationship between history and philosophy of science (e.g., Miller 2011, 38, 41; Schickore 2018, 197).

history of science. By "history of science," what I mean is *historical sources and information*, which I take to include everything from primary documents and artifacts to secondary and tertiary sources. These needn't be terribly old (if they are primary sources or artifacts) or concerned with old events (if they are secondary or tertiary sources). History does not become less historical just because it is recent history. Moreover, historical sources and information do not become historical only when they are metabolized by a professional historian. There is no *in principle* reason why a philosopher's historical research should count for less than a historian's, especially if the philosopher executes her research with a particular philosophical goal in view (Schickore 2018). By this I intend no slight to historians (and no flattery to philosophers). I only mean to endorse the strategy of pursuing historical and philosophical researches in tandem.

Let me also be clear about what I mean by "philosophy of science." In the last section I argued that attempts to crystalize an essence of philosophy of science fail in light of the diversity of goals and research practices found within the field. Because of this, I recommended abandoning the notion that there is a single thing, "philosophy of science," whose relationship to "history of science" we are interested in ascertaining. A better approach is to ask how philosophers of science engage with historical sources and information in their research practices. Since philosophical aims are various, this question can be better formulated as follows: how do philosophers with different aims use historical sources and information to achieve these aims? This is the question I attempt to answer in the present section, albeit in a partial and preliminary manner.

My approach is empirical. I begin by articulating three distinct methodological approaches in practice-based philosophy of science, and show how each uses historical sources

and information to achieve its ends.[13] For each approach, an exemplar provides concrete illustrations and philosophical guidance; these are supplemented, when necessary, with references to other works. Together they make a start on the important-but-neglected task of elucidating specific methodological approaches in practice-based philosophy of science. The three approaches I discuss are the following:

1. the *functional-analytic approach*, exemplified by C. Kenneth Waters's work on the "structure and functioning of classical genetics" (e.g., Waters 2004, 785),
2. *integrative history of the recent*, exemplified by Sabina Leonelli's work on data-centric biology (Leonelli 2016), and
3. the *phylogenetic approach*, exemplified by James Lennox's work on the concept of fitness in evolutionary biology (Lennox 2001).

In the remainder of this section I will discuss these in order, using the exemplars to elucidate general research strategies, as well as to throw light on the role historical sources and information play within those strategies.

The Functional-Analytic Approach

I have called the first approach "functional-analytic," because its (proximate) goal is to ascertain how the major cognitive elements of a body of scientific knowledge *function together* in investigative and explanatory practices. A model of this approach is provided by Waters's (2004) study of classical genetics, which seeks to articulate "a philosophical account of the

[13] Exemplars are here understood after the Kuhnian fashion, as instances of successful research that provide a model and standard for future research projects (Kuhn 1962).

content and structure of classical genetics that elucidates how it functioned as a body of knowledge" (786). Waters criticizes past philosophical accounts of classical genetics for presupposing a view of how knowledge is structured in "well-established" areas of science, like genetics. According to this view, knowledge is structured by explanatory reasoning associated with a powerful central theory, and in particular, by the need to "fill out [the] central theory and expand its explanatory range" (783). So for example, Philip Kitcher (1984), while acknowledging the importance of "experimental procedures and methodological rules" in classical genetics, nonetheless ranks these activities as subordinate to the *ur*-task of testing theoretical explanations of inheritance patterns (352). But this will not do, Waters thinks. As the historian Robert Kohler argues in his book, *Lords of the Fly*, classical genetics was "organized around efforts to investigate *a broad range of biological phenomena* (including development), not just [inheritance patterns]" (Waters 2004, 785, emphasis added). This implies that "we cannot understand the overall structure and functioning of classical genetics by viewing it through the traditional theory-based lens of philosophy," since this suggests that it was primarily a science of inheritance patterns, when in fact it was much more than this.

In developing his positive account of classical genetics, Waters isolates three important cognitive elements: "pools of specialized knowledge," "patterns of explanation" and "patterns of investigation." The first he dissects into practical "know-how" (embodied knowledge about, for example, experimental techniques and methods of material production), "descriptive knowledge" (of genetic structures and causal regularities), and "evaluative knowledge" (concerning the relative value of individual mutants for genetic research). Waters notes that, in T.H. Morgan's Fly Room at least, explanatory reasoning *depended on* practical knowledge, "because [explanations] appealed to causal regularities that were understood in light of knowledge about

the procedures used to observe them" (Waters 2004, 802). Moreover, investigative reasoning required an understanding of explanatory knowledge, since researchers were often forced to reason 'on the fly' (as it were), setting up breeding experiments and interpreting provisional results in light of theoretical principles. To reflect this complex interdependence of reasoning styles, Waters concludes that research in classical genetics was pursued by patterns of *investigative* reasoning, whose implementation depended on patterns of *explanatory* reasoning, which in turn drew heavily on pools of specialized knowledge—and that none of these elements can be (fully) understood in isolation from the rest.

It is obvious that Waters makes ample use of historical sources and information in crafting his account. The reason is that the object of his analysis *is a historical community*—T.H. Morgan's research group, which formed at Columbia University around 1910, and moved to Caltech in 1928. To understand the practices of group-members during this period, Waters scrutinizes a range of publications, including some rather dense monographs published during the heyday of the Fly Room (e.g., Bridges and Morgan 1919, 1923). In addition, he creatively appropriates Kohler's account of the material culture of classical genetics from *Lords of the Fly*. From Kohler, Waters ascertained that one can "provide a compelling account of the central research agendas of classical genetics without mentioning the central theory that explained inheritance patterns" (Waters 2004, 785). This puts a strong challenge to philosophers that place the transmission theory at the heart of classical genetics, almost to the exclusion of everything else. But equally important, it leads Waters to seek a more articulated account of the structure and functioning of classical genetics, for Kohler comes near to erring in the opposite direction (pretending that Fly Room geneticists had no substantive theoretical interests at all). This sent him back to the monographs, where a blend of investigative and explanatory reasoning was on

display. Geneticists *were* using theoretical concepts and principles to advance their research projects, but these agendas were not uniformly geared towards the explanation of inheritance patterns (as theory-centric philosophers were led to suppose). Classical genetics thus emerges as a challenge to the philosophical notion that scientific knowledge is organized by explanatory reasoning associated with a powerful central theory, as well as a guidepost on the way to a more adequate conception.

Time to take stock. The functional-analytic approach interrogates how knowledge and inquiry are structured in an area, as well as how elements of epistemic relevance function jointly in investigative and explanatory practices. An orienting question is, what gives epistemic communities their coherence, and structures their knowledge and ongoing research activities? To answer this question, the philosopher attempts to dissect elements of epistemic relevance in relation to their use (see Woody 2014). These can be any elements *X*—examples include strategies (e.g., idealization), activities (explanation), abstract entities (standards), or pools of tacit or explicit knowledge. For any element, the investigator is interested in discerning the role(s) *X* plays in the functioning of a community. Here, primary and secondary historical sources provide guidance, especially when the focal community is a historical community. These are used to identify elements of epistemic relevance, determine the goals and standards operative in the community, and elucidate the significance of epistemic elements in relation to the identified goals and standards.

Integrative History of the Recent

The second approach I wish to highlight is exemplified by Leonelli's account of "data-centric" biology (Leonelli 2016). Its goal is to elucidate 'history on the wing', or if you prefer, history-in-the-making (as opposed to the merely synchronic features of a situation, which are the

focus of the functional-analytic approach). I am tempted to call it "history of the present," but that phrase has already been taken, and anyways is not entirely appropriate. Still, integrative history of the recent *is* a tool for understanding the present—in Leonelli's case, the ongoing shift in the organization and practice of the life sciences associated with the rise of big data.

Leonelli lists three goals for her study of data-centric biology: to show what data-centric biology consists in, to show "how deeply it is rooted in twentieth-century scientific practice," and to examine its implications "for how we understand scientific epistemology" (Leonelli 2016, 9). Her overarching aim is to produce a philosophical account of data-centric biology that highlights both its novel features and its continuity with ongoing projects in the life sciences. Her aim is *not* to produce a narrative that traces the emergence of data-centric biology through the frenzied history of the late twentieth and early twenty-first centuries. This is what makes her project "*integrative* history," as opposed to history *simpliciter*. The role of history in *Data-Centric Biology* is to add texture to a framework designed to highlight "the epistemological challenges involved in processes of data gathering, classification, and interpretation," as well as "the multiplicity of conceptual, material, and social structures in which such processes are embedded" (Leonelli 2016, 2). To be more precise, it is to situate these challenges and structures within "the longer historical trajectory [of the life sciences], thus highlighting both the continuities and the ruptures that data-centrism brings with respect to other normative visions for how research should be carried out and with which outcomes" (4).

At the heart of Leonelli's project is a "relational framework" for understanding scientific data. This is not just an account of what scientific data *are*—in addition, it is a tool for studying how data are valued by different users in different local settings. According to this framework, data are essentially portable objects, whose status and meaning are dependent on "who uses

them, how, and for which purposes" (Leonelli 2016, 78). This means that the same data can potentially serve as evidence for different substantive claims in different investigative settings. To unlock the evidential value of data, however, these data must be rendered capable of travel. And this involves a tremendous amount of labor and social coordination. The novelty of data-centric biology, Leonelli argues, lies not in "the emergence of big data and associated methods"—instead it has to do with the increased attention paid to data handling and dissemination practices (1). The reason is that these practices are what enable scientists to leverage the evidential flexibility of data to its fullest extent; that is, to make a particular collection of data maximally relevant to a maximally wide group of potential users.

How does history figure into this account, which I have called an integrative history of the recent? As noted, its role is to situate data-centric biology within the longer historical trajectory of the life sciences, in order to highlight both its continuity and its discontinuity with past research practices. To demonstrate continuity, Leonelli examines the history of model organism research, and in particular, the development of model organism databases such as The Arabidopsis Information Resource (TAIR), FlyBase and WormBase (e.g., Leonelli 2007, 2010; Leonelli and Ankeny 2012). A conclusion emerging from this inquiry is that the challenges of making data circulate played a major role in configuring biological research during the second half of the twentieth-century, and in incubating innovations that facilitate intellectual and material exchanges across settings. But as Leonelli observes, "research on model organisms...constitutes a close approximation to what may be viewed as 'ideal conditions' for data [circulation]" (Leonelli 2016, 194). Making data circulate under *non*-ideal conditions represents an additional challenge to be surmounted; and it is here that data-centric biology stakes its claim to genuine novelty. Data-centric biology is not just a response to the long-

standing challenges of managing and dissemination copious data. In addition, it is a new "model of attention" within biological research (178)—a new "normative vision of how scientific knowledge should be produced in order for the research process to be efficient and trustworthy" (197). The second role of history in Leonelli's project, then, is to trace the emergence of this vision through its various negotiations, in order to expose what is novel about the present vogue of big data.

This is one way of conceptualizing the role that history plays in Leonelli's project—it is a means of characterizing what is conserved, as well as what is new, about data-centric biology. I call it *integrative history* because these historical contributions do not stand alone; instead they enrich, and are enriched by, philosophical contributions, as well as contributions from sociology of science. I call it *history of the recent* because it concerns a change that is ongoing, and whose significance must therefore be grasped independently of facts about its downstream impact. Of course, integrative history of the past is also possible, as when historians deploy philosophical concepts as historiographic tools in reconstructing past episodes. But it is possible to do integrative history when reconstruction is not the primary aim, and here philosophers seem well-poised to make useful contributions.

The Phylogenetic Approach

The final methodological approach I will consider has already received an explicit treatment from James Lennox (who also named it).[14] For this reason, I will confine my comments here to a few brief words. In addition, I will move quickly through the case history,

[14] Readers interested in more detail should consult Lennox's paper (Lennox 2001), as well as the explication of the phylogenetic approach contained in Love (2005, Ch. 7).

since my purpose in discussing exemplars has been to arrive at a characterization of the approach, and this we already possess.

Lennox calls his approach "phylogenetic" by loose analogy with the activity of phylogenetic reconstruction in evolutionary biology. Phylogenetic reconstruction is a family of evidential practices by which biologists seek to infer patterns of evolutionary descent in and among groups of organisms. By contrast, Lennox's phylogenetic approach is an attempt to reconstruct the history of "puzzles, paradoxes and confusions in the foundations of science generally, and of special sciences specifically," with an eye to providing normative guidance for scientists (Lennox 2001, 3). Historical sources and information provide key guidance in this endeavor, since "the foundations of a particular scientific field, and (though a wider lens is needed) of science generally, are shaped by its history"—often in surprising and profound ways (4). By studying how puzzles and confusions actually arose in the development of inquiry into a subject, the philosopher gains an ability to diagnose "conceptual and methodological assumptions" responsible for current tensions in scientific practice (23). This in turn provides guidance regarding how the focal tension might be resolved, for example, by articulating concepts or posing research questions in a way that avoids the difficulty.

Lennox illustrates these claims by showing how the infamous "tautology problem" in evolutionary theory arose out of tensions inherent in constructing abstract representations of evolutionary processes.[15] After tracing the muddle to its source (a point at which the problem did not exist—in this case, a time before mathematical theories of natural selection were formulated), Lennox proceeds to 'work forward' to show how conceptual confusions can be

[15] The tautology problem concerns the meaning of the term "fitness," which if explicated in terms of reproductive success, renders the phrase "survival of the fittest" tautological (the "fittest" are, by definition, those who survive and reproduce at the highest rates). This is a problem because fitness is commonly taken to be *explanatory* of reproductive success, which it cannot be if it is simply a *measure* of reproductive success.

avoided in contemporary practice. One way is to recognize that variables in "quasi-mathematical" descriptions of natural selection needn't correspond to "any single varying property in nature," and indeed *fitness* corresponds to no such property (see also Ariew and Lewontin 2004). As Ronald Fisher wrote in his *Genetical Theory of Natural Selection*:

> Fitness, although measured by a uniform method, is qualitatively different for every different organism, whereas entropy, like temperature, is taken to have the same meaning for all physical systems. (Fisher 1930/58, 39–40)

Hence, by keeping in view a distinction between fitness-the-physical-property (which is qualitatively different for every organism, and explanatory of reproductive success) and fitness-the-variable-in-quasi-mathematical-theories (which is the result of a uniform measurement procedure, and is *not* explanatory of reproductive success), the tautology problem is avoided.

How are historical sources and information used in this example? They are *not* used to recognize a conceptual confusion—rather, they are used to *diagnose* it: to trace the roots of a confusion to some crisis that bequeathed to posterity an unresolved tension in scientific practice. With this diagnosis in hand, the philosopher is free to explore ways of formulating the relevant concept or theory that avoids the lingering tension. Failing this, she can simply enter a caveat: that the concept under scrutiny should be used with care, avoiding problematic assumptions, like the assumption that the term "fitness" refers to a single varying property in nature. In any case, the phylogenetic approach does not leverage historical information to make prescriptive recommendations. Instead, it uses historical information to visualize the contours of a scientific puzzle, after which the philosopher can make normative recommendations on independent grounds.

Summary

History of science matters for philosophy of science. But the mode of this mattering—the exact way in which history and philosophy of science interact—varies from project to project. Waters uses historical sources and information to characterize research practices in classical genetics—a plainly descriptive application that nonetheless challenges a widely-held view of the structure of scientific knowledge. Leonelli uses history to situate data-centric biology within the historical trajectory of the life sciences—also a descriptive application, but one meant to illuminate the significance of an emerging normative conception of scientific practice. Finally, Lennox uses history to trace problems to their sources, and ultimately, to provide guidance for independently-derived normative recommendations. This too is a descriptive application, but it is description in the service of diagnosis, with prescription as the ultimate end.

What has this exercise taught us? That philosophers regularly use historical sources and information in their research practices in ways that seem entirely reasonable from a methodological point of view. So why has there been a half-century of infighting about the relationship between history and philosophy of science? A major reason has to do with the notion that philosophy of science should be normative, a subject I turn to in the brief concluding section.

5. Concluding remarks

I titled this essay "History and Philosophy of Science After the Practice-Turn: From Inherent Tension to Local Integration." The reason was to suggest a shift in analytical perspective. In the past, the relationship between history of science and philosophy of science has frequently been analyzed in terms of broad disciplinary approaches ('history describes,

philosophy prescribes'). It is this convention that fosters the myth of inherent tension—the notion that history and philosophy of science are intrinsically opposed to one another. This tension disappears when we focus on the local integration of history and philosophy of science—the use of historical sources and information in pursuit of philosophical goals (and vice versa). Seen in this light, the value of history of science for philosophy of science is manifold; and I have only begun in this essay to document the many roles that history of science plays in philosophical practice.[16]

But the veteran of the marriage debate is unlikely to be impressed. The reason is that a major focus of this debate was how historical philosophy of science can overcome the naturalistic fallacy.[17] In Giere's words, "The general problem is to show that philosophical conclusions may be supported by historical facts and just how this comes about" (Giere 1973, 292). Since philosophical conclusions are normative, and history only describes, any attempt to support philosophical claims with historical facts rests on a fallacy. It is for this reason, Giere argues, that "the union between history and philosophy of science lacks a strong conceptual rationale" (296).

But as my examples from Section 4 illustrate, philosophy of science is not solely concerned with formulating and defending normative claims about the nature of science. In fact, even when prescription *is* on the agenda (as it is for Lennox), a great deal of diagnostic work must typically precede prescription. And here history of science is indispensable. So while Giere is correct that historical facts cannot validate normative claims, this is no reason to think that

[16] I have, for example, ignored the broad school of naturalistic approaches in philosophy of science, many of which (including Nancy Nersessian's "cognitive-historical approach") make productive and creative uses of historical sources and information.

[17] This is not the only focus of the debate, as Schickore (2011) explains. "For many other contributors, the nature and goal of *philosophical* analysis was the main concern," and for still others, the best way of doing history of science was at issue (460).

"the union between history and philosophy of science lacks a strong conceptual rationale" (Giere 1973, 296). Only when the union is characterized with maximal narrowness is this stance justified. Characterized more broadly, in a way that reflects the heterogeneity of philosophical goals and interests, the union between history of science and philosophy of science rests on a firm foundation.

There is an elephant in the room, and his name is Philosophy. Not "philosophy of science," mind you, but Philosophy with a capital-P—philosophy conceptualized after the "professional image" (Doyle 2014). According to this image, philosophy is a discipline concerned with "generalizable or abstract concepts and arguments, properly justified decisions, and ideal epistemic situations" (Schickore 2018, 197). It is a normative discipline whose truths are timeless, and whose practitioners strive for clarity and precision above all else (Caneva 2011). The image is a mirage—at no point in history has philosophy been Philosophy, and never less so than it is today (Smith 2016). But the image continues to function as an off-the-shelf description of what philosophy is *really about*, even for methodological discussions in philosophy of science. This needs to change. Philosophy is a pluralistic endeavor: sometimes prescriptive, sometimes descriptive; sometimes abstract, sometimes detail-oriented. The sooner we admit this, the sooner our methodological reflections will reflect actual philosophical practice, as opposed to mere shimmerings of the air.

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