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## The silence of the norms: The missing historiography of *The Structure of Scientific Revolutions*



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### ABSTRACT

History has been disparaged since the late 19th century for not conforming to norms of scientific explanation. Nonetheless, as a matter of fact a work of history upends the regnant philosophical conception of science in the second part of the 20th century. Yet despite its impact, Kuhn's *Structure* has failed to motivate philosophers to ponder why works of history should be capable of exerting rational influence on an understanding of philosophy of science. But all this constitutes a great irony and a mystery. The mystery consists of the persistence of a complete lack of interest in efforts to theorize historical explanation. Fundamental questions regarding why an historical account could have *any* rational influence remain not merely unanswered, but unasked. The irony arises from the fact that analytic philosophy of history went into an eclipse where it remains until this day just around the time that the influence of Kuhn's great work began to make itself felt. This paper highlights puzzles long ignored regarding the challenges a work of history managed to pose to the epistemic authority of science, and what this might imply generally for the place of philosophy of history vis-à-vis the problems of philosophy.

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In history...expectations are far less precise, and there is correspondingly less agreement than in science about whether expectations 'fit the facts' and about the sorts of data relevant to their evaluation...The historian's problem is not simply that the facts do not speak for themselves but that, unlike the scientist's data, they speak exceedingly softly. Quiet is required if they are to be heard at all. (Kuhn, 1980, p. 183)

### 1. Introduction

A philosophical mystery, one cloaked by a methodological irony, shrouds a key development in contemporary philosophy of science. The mystery? How to account for the logic of explanation that underwrites the influence and status of Kuhn's widely celebrated and extensively studied *The Structure of Scientific Revolutions* (hereafter cited as SSR). What makes this a mystery? Consider the following irony: despite SSR's status, there exists no generally

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accepted specification of those features that an historical explanation ought to possess.<sup>1</sup> In this key respect, fifty years of debate regarding the merits of SSR has proceeded virtually without mention of the philosophical void regarding the topic of historical explanation. Almost all readers of SSR have started this mystery in the face for 50 years now without taking notice of or commenting on it.<sup>2</sup>

Indeed, the fact that the entire topic of historical explanation fell off the map of (analytic) philosophy decades ago compounds the mystery of how SSR could have been influential and the irony of its enduring impact. Just at a moment when philosophy of history arguably should have “taken off” as a core philosophical discipline, riding a wave of professional concern one might have expected Kuhn’s work to generate, discussion instead effectively ceases and the topic disappears. And even those reporting “the naturalists return” record no sightings or mentions of philosophy of history.

In this sense, the mystery runs deep. For it has managed (or so it seems) to elude detection even by those supposedly highly sensitized and trained to identify, analyze, and evaluate standards of explanation and argument. But why pursue this philosophical cold case—the unsolved and allegedly worrisome mystery of a philosophical topic gone missing? Why worry about the silence that surrounds questions regarding the norms of historical explanation? I suggest the following answer: breaking the silence should prove key to exposing still existing and important questions about the relation of history and philosophy, ones that presently go unasked and ignored in polite philosophical company. In order to reanimate interest in this mystery, I assemble reminders of its connection to a basic task of philosophy—to clarify for ourselves the grounds for what we take to be true, and so apparently shared but still implicit standards for rationally warranting beliefs.

## 2. The rise of SSR and the disappearance of analytic philosophy of history

Towards this end, it helps to recall how analytic philosophy of history comes to exist as a subfield. If one were to construct a type of genealogical chart, it would show analytic philosophy of history as the runt of a litter of topics that sprung from philosophy of science in its youth. Birth could plausibly be dated to coincide with

the publication of Hempel’s classic article, “The Function of General Laws in History.” (Hempel, 1942; see also Nagel, 1979[1961], especially the chapter “Problems in the logic of historical inquiry.”) Hempel there stipulates as a condition for the scientific/logical adequacy of an historical explanation that it contain a covering law. This effectively mandates the *de facto* exile of academic history from the realm of the legitimate sciences.<sup>3</sup> Analytic philosophy of history, for the 25 years following Hempel’s article, by and large consists in critiques or defenses of the applicability of this model to historical explanation.<sup>4</sup>

Regarding the historical/philosophical context at the point when SSR first appears requires situating Kuhn’s work relative to Hempel and to Quine. Reading Hempel’s classic 1950 article, “Problems and Changes in the Empiricist Criterion of Meaning” alongside of Quine’s (1951) “Two Dogmas of Empiricism,” a striking feature emerges. One finds in Hempel’s article (see especially §5) not merely a recognition of the type of the holism that Quine so (in)famously promotes, but actually an embrace. Both acknowledge that holism radically broadens what counts as the unit of empirical significance with regard to explanation and testing in science as then philosophically conceived. Ironies abound here. On the one hand, Hempel betrays no anxiety that holism ultimately represents any principled problem for his favored analysis of the logic of science. On the other hand, Quine hypothesizes that holism makes it impossible to philosophically vindicate the verification criterion of meaning at the philosophical heart of positivism.

Kuhn for his part explicitly acknowledges Quine’s critique in “Two Dogmas” as a key influence, particularly the holism it ushers into philosophical prominence. (Kuhn, 2012, p. vi; see also Zammito, 2004) Kuhn’s particular narrative of a history of science powerfully illustrates how this shift in a conceptualization of the unit of empirical significance effectively upends all prevailing accounts of what supposedly explains the rationality of theory change in science. And one might then imagine that precisely this turn of events—the emergence of a narrative of the history of science that profoundly alters and constrains any philosophical account of how rational evaluation of scientific reasoning could proceed—would galvanize philosophical concerns about and research into the nature of historical knowledge and historical explanation. Yet, as Arthur Danto wryly notes, nothing of the sort happens.

<sup>1</sup> The University of Chicago Press now has on offer a 50th anniversary edition of *Structure* (Kuhn, 2012), featuring an introduction by Ian Hacking, who himself has been justly celebrated for his historical studies. Yet Hacking nowhere mentions issues of historical explanation in his generally laudatory discussion of Kuhn’s work and influence and despite his explicit acknowledgment of SSR as a work of history. (E.g., Hacking, 2012, p. x) A hint of why Hacking displays no interest in questions of specifically historical explanation emerges in the following remark by Hacking. “Thus I claim that Kuhn leads us into a ‘revolutionary nominalism’ which makes nominalism less mysterious by describing the historical processes whereby new categories come in to being. But I assert that a seemingly more radical step, literal belief in the creation of phenomena, shows why the objects of the sciences, although brought into being at moments of time, are not historically constituted. They are phenomena thereafter, regardless of what happens.” (Hacking, 1985, p. 119) Hacking’s focus on styles of scientific reasoning and processes of categorization suggests (and here I speculate) that, at least with regard to the natural sciences, historical inquiry represents nothing more than a rough analogue to a context of discovery. Styles of reasoning and processes of categorization of natural phenomena play the role for him analogous to a context of justification. On this view, there would then be nothing requiring anything that might be termed a ‘historical explanation’, or even anything (in Hacking’s terminology) characterizable as a “style” of historical reasoning, at least in the sense found in the natural sciences. Regarding the human sciences (however one draws that line), Hacking takes a different view. That is, with respect to categories for human kinds, a history of how these emerge and stabilize *does* constitute a key part of their explanation, unlike laboratory phenomena. Hacking’s scattered remarks on Foucault hint strongly of this view. “I think that we shall lose ourselves in confusion and obscurity for some time yet, in the so-called social and human sciences, because in those domains the distinction between word and thing is constantly blurred.... Here Foucault’s ‘archaeology’ may yet prove useful, ... at least to grasp the interrelations of ‘power’ and ‘knowledge’ that literally constitute us as human beings.” (Hacking, 1985, p. 124)

<sup>2</sup> Work by Alasdair MacIntyre constitutes the sole exception of which I am aware. I discuss his view below. In a review of the 50th anniversary edition that nicely emphasizes Kuhn’s naturalism, Bird (2012) offers in some respects a nice description of Kuhn’s philosophical targets and how SSR addresses them. However, Bird contents himself with observing that Kuhn’s approach to the topics was “multifaceted, involving history, psychology, philosophy, and sociology,” a combination that he acknowledges stood as “unusual” given the then prevailing norms for evaluating arguments in the empirical sciences. As Bird summarizes matters in this regard, “Kuhn’s arguments, especially as regards philosophical consequence, are often more implied than stated explicitly.” (Bird, 2012, p. 865) What Bird nowhere acknowledges, despite his recognition of key portions of Kuhn’s argument as historical, concerns how Kuhn succeeded in challenging and overthrowing other extant narratives of the history of science. For it is not as if SSR brought new facts to light; its novelty lies in its narrative structure regarding historical episodes already known and much studied. Bird’s article might lead an uninformed reader to imagine that no one prior to Kuhn has written a history of science.

<sup>3</sup> The story adumbrated here simplifies the picture by excluding Popper’s influential intervention—*The Poverty of Historicism*. But Popper certainly never promoted history as a science. I address below questions of how debates about historical explanation sit within readings of Wittgenstein propagated so influentially from the late 1950s forward by philosophers such as Peter Winch.

<sup>4</sup> Richard Vann, who served as executive editor of *History and Theory* for many years, uses as the epigraph for (Vann, 1995) the following statement by noted literary critic Frank Kermode: “It seems ... that philosophy of history is the business of those who teach novels.” Vann then observes, “Kermode’s view would have been considered bizarre indeed in 1950 [the hey-day of positivist debate]. In 1968 it was still avant-garde; by 1975 the problems that such a comment raises had moved to the forefront of debate in the philosophy of history in the English-speaking world. I shall try to show how, and in what institutional settings, this happened.” (Vann, 1995, p. 40)

I can think of very little in the philosophy history from the middle-1960s to the present. Somewhere someone sometime in the last decade must have written about explanation, even about historical explanation—but I cannot think of an example offhand.... It is not just that the topic is under extreme neglect. It is, rather, that there is hardly room in the present scene of philosophy for discussion of its issues. So to find someone actively working at them would be almost to encounter a historically displaced person, like someone doing abstract expressionist canvasses as if the whole subsequent history of art had not taken place. (Danto, 1995, pp. 72–73)

The spell cast by positivism conjured analytic philosophy of history into existence. Those caught in the magic of that moment perceived a need to exorcise history of its possession by narrative form. But when the positivist spell breaks, such concerns vanish.

Danto, himself a key player in philosophy of history in its prime, identifies Kuhn as the thinker who forces philosophers of science to rethink the philosophical role that they must accord to history.

What makes Kuhn's work *historically* important is the fact that a good many thinkers, whose worlds very largely overlapped Hempel's... were caused by Kuhn's work to turn into thinkers whose world overlapped Kuhn's world instead.... I can remember one of them saying with a cry of anguish that he wished Kuhn had never written that damned book.... [A 16th century scholastic] said, in much the same spirit, that "The wretched Luther had emptied the lecture halls". For a long period there were questions with which scholastic thinkers dealt and with which everyone who shared their world regarded as of the greatest moment. And then, all at once, almost overnight, nobody cared any longer.... [Hempel's theory] just stopped being relevant, the way the whole philosophy of history it defined stopped being. It was replaced with a different set of questions, a world in effect, into which it no longer fit. (Danto, 1995, pp. 84–85)<sup>5</sup>

But Danto, his sophistication with regard to this topic notwithstanding, nonetheless never pauses to ask why philosophy of history fails to rise reborn from the ashes of positivism.

Kuhn advanced a view of history so powerful that history rather than being an applied science, as Hempel holds history to be, history came to be the matrix for viewing *all* the sciences. It all at once became the philosophical fashion to view science historically rather than logically, as an evolving system rather than a timeless calculus, as something whose shifts over time are philosophically more central to its essence than the timeless edifice of theories.... (Danto, 1995, p. 72)

Hempel held that histories only offer "explanation sketches," explanatorily adumbrated accounts that need to be filled out by covering laws in order to achieve fully legitimacy as scientific expla-

nations. But the *de facto* impact of Kuhn's historiographic practice proves false Hempel's attempt to legislate what the form of historical explanation needs to be. In short, Danto gives voice to the fact that Kuhn's work made *passé* all that prior to it had supposedly defined what a science of history had to be. Yet having commented on the surprising result of Kuhn's work—the complete and sudden overthrow of a powerful theory of scientific explanation by means of a work of history that supposedly does not have even *prima facie* status as a scientific explanation—and stared it in the face, Danto (like so many other philosophers) then simply turns away.

Nonetheless, the old questions remain unanswered; the demise of positivist hegemony in philosophy of science only removes any felt pressure to answer them. In short, once positivist-inspired methodological debates cease to have any real point, interest in philosophy of history within analytic philosophy largely disappears. (Vann, 1995) Ironically, then, some time just subsequent to the publication of the first edition of SSR, i.e., as the history of science intrudes itself into a central role in philosophical debate regarding philosophy of science, philosophical discussion of historical explanation effectively ceases. And even as Kuhn's work, as has been widely noted and much discussed (see especially Zammito, 2004), gives impetus and life to a distinctive style of sociology of science and inspires the creation of a new discipline—science studies, questions of what makes for a proper historical explanation remain ignored.

But note the revolution wrought in philosophy at this moment. *SSR effectively reverses the received order of epistemic authority*. Prior to Kuhn a work of history, in order to count as providing a legitimate explanation, needed to conform to a certain standard determined by an ahistorical account of science. Post-Kuhn, philosophers fashion histories to account for which explanatory forms come to prevail and why.<sup>6</sup> Moreover, Kuhn never receives attention as a historiographer or a philosopher of history, even before that philosophical tribe decamps and vanishes. Indeed, his own remarks on historical methodology prove sporadic and mostly unilluminating.<sup>7</sup> If analytic philosophy of history begins, for all intents and purposes, with Hempel's throwing the gauntlet down to historians and daring them to meet the challenge posed by standards of scientific explanation, it ironically ceases just at the point of a miraculous reversal of fortune. Despite decades of exile from the realm of scientific explanation, a work in the history of science overthrows extant accounts of the rationality of theory change in science. But how could this have happened given the absence of any accepted basis for taking a history as explanatory?

### 2.1. Why Did Analytic of Philosophy Disappear?

How then to account for this lack of interest in questions regarding historical explanation just at the moment when they should have been regarded as particularly relevant and pressing? One answer found in the literature can be considered but ultimately rejected, Giuseppina D'Oro's "The Ontological Backlash: Why did

<sup>5</sup> Note a concurrent observation by Vann regarding Louis Mink, a philosopher tied to *History and Theory* from its inception. "As he [Mink] wrote in 1974, 'It could be said without exaggeration that until almost 1965 the critical philosophy of history was the controversy over the covering law model.'" (Vann, 1987, p. 2)

<sup>6</sup> I have in mind Ian Hacking, particularly his important and critical works *The Emergence of Probability* (Cambridge: Cambridge University Press, 1975), *Rewriting the Soul* (Princeton, NJ: Princeton University Press, 1995), and *Historical Ontology* (Cambridge, MA: Harvard University Press, 2002). But Peter Galison's works also provide important examples.

<sup>7</sup> Again, there is a dramatic contrast in the reception by theoretically minded historians between the ignoring of Kuhn and the appropriation of Foucault et al. See Zammito (1993) and Toews (1987). Apart from philosophy, those who do receive extensive discussion for their historicizing turns, e.g., Hayden White and Michel Foucault, offer some (in White's case) or none (so far as I know with Foucault) acknowledgment of any influence by or credit to Kuhn. Foucault's most influential works manifest little or no interest in natural science *per se*, and never seems intent on bringing the sort of questions for which he is famous into the area of scientific change (though, of course, Ian Hacking does precisely this on Foucault's inspiration). This leads, in effect, to about three decades of discussion where those interested in historiography simply assume that nothing that goes on in analytic philosophy could possibly be relevant to their interests. And it is not until the publication of Hacking's key works that any analytic philosopher takes seriously Foucauldian questions regarding the relationship between the historically available stock of ways of self-understanding and the implications of these for other knowledge related activities. Hacking himself, to add the final turn of the screw, neither self-identifies as a philosopher of history nor, more ironically still, ever exercises the sort of methodological reflection on the methods of history that he lavishes on physics or statistics. To the best of my knowledge, the most judicious and comprehensive account of how the intellectual milieu develops in this regard remains (Novick, 1988). See also Vann (1998).

Mainstream Analytic Philosophy Lose Interest in the Philosophy of History?." (D'Oro, 2008) According to D'Oro, debate in philosophy of history concerns the status of reasons as causes. Unfortunately, D'Oro's narrative runs together and confuses two distinct strands, one emanating from a debate that chronologically predates logical positivism but foreshadows a number of key issues.

D'Oro's telling of the tale begins with the 19th century dispute about the nature of explanation in the natural versus the human sciences—*Erklären v. Verstehen*. This earlier strand, as formulated by Dilthey and others,<sup>8</sup> defends history as a science, but one characterized by its own special methods, methods that were tailored for the reconstruction of meaning structures specific to times and places. In this context, a principled distinction between the natural and the human sciences results from the different types of explanations that the natural as opposed to the human sciences seek to produce—the nomothetic as opposed to the idiographic. Non-reducibility of one science to the other here results from the fact that the human sciences seek the particularity of situations and so cannot generalize. The natural sciences, for their part, abstract from the particularities of time and place in order to identify invariant regularities at work. Explanations required causal laws; causal laws require invariant regularities. The friends of *Verstehen* denied that idiographic accounts yielded regularities of the requisite sort. History was held to reside firmly on the human sciences/*Verstehen* side of this divide. History could not be a science for this reason.

However, D'Oro attempts to weave this together with a second strand of debate, one where Davidson famously intervenes. This involves the dispute over "reasons as causes," a debate that emerges from a particular reading of the later Wittgenstein and *not* from any positivist strictures on explanation. Dilthey and those in this hermeneutic tradition defend history as a science, by which they meant a subject that produces truths by virtue of a special method. Those neo-Wittgensteinians who deny reasons as causes also deny as a matter of principle the possibility of a science of the social, history included. For the neo-Wittgensteinians, reason-giving represents a normative activity, and so cannot be characterized by mere descriptive inquiry. *But idiographic does not equate to normative*. In one case reason explanations prove compatible with history being a science, in the other case not. Ironically, D'Oro mentions a key component of the actual debate but does not recognize it for what it is. (D'Oro, 2008, p. 405)

The problematic as configured by the *Verstehen/Erklären* debate does, to be sure, change with the appearance of logical positivism generally and Hempel's classic paper in particular.<sup>9</sup> The change is this. Hempel does *not* insist that, e.g., economics reduce to the laws of physics. What he does require concerns the *logical* form of scientific explanation. D'Oro overlooks and so misses the logical concerns of positivism and confuses them with the metaphysical views of those alleging the conceptual autonomy of reason explanations.

D'Oro's account thus ultimately mischaracterizes the issues at stake. For having set the narrative line that she does, once Davidson puts to rest doubts that reasons can be causes, the issues switch to metaphysical debates about mental causation. "My key

claim is that the declining interest in the philosophy of history is linked to the return of a metaphysical conception of the task of philosophy." (D'Oro, 2008, p. 404; see also p. 405) These questions were, in turn, appropriated by philosophy of mind. Question of explanation within analytical philosophy of history, on her account, presuppose an account of mental causation. And those issues remain unsettled. But this confuses a metaphysical question about a type of causality and a logical question about the form of explanation. And the logical question alone bears on norms of explanation; the metaphysical question involves issues independent of those of logical form.

In sum, philosophy of history does not have its disappearance accounted for by arguing, *pace* D'Oro, that other areas preempted its core issues. That earlier debate, whatever its interest, does not call into question history's status as a legitimate science. It ties to later debates not via a metaphysics of causation, but in virtue of norms of explanation specific to human sciences. What goes missing when philosophy of history disappears involves a basis for evaluating any imputed action explanation *qua* explanation, whatever the mechanism of action.<sup>10</sup>

### 3. Issues regarding Kuhn's historiography

Bojana Mladenovic offers a thoughtful account that addresses the question of what makes for actual explanation in Kuhn's history of science. (Mladenovic, 2007) Mladenovic examines some recent extended readings of *Structure* from the standpoint of how they treat Kuhn's historiography. While sympathetic to objections she raises to these specific works (e.g., book-length studies by Andersen, Bird, Sharrock and Read, as well as an article by Kindi), a particularly telling criticism she offers of Sharrock and Read bears noting.<sup>11</sup> On their account, the use of history in *Structure* has no explanatory but only a therapeutic intent. The desired outcome on their account would be quietist: "if philosophical therapy is successful [say S & R], it will 'leave science as it is'; history of science, not philosophy of science, will then be the main source of understanding of scientific development." (Mladenovic, 2007, p. 267) So, if Sharrock and Read are to be believed, the explanatory mystery goes away; indeed, it never existed in the first place.

Against this reading, Mladenovic makes the following pointed response: "[P]hilosophy cannot simply 'leave history as it is', because history itself requires substantive philosophical assumptions which ground the individuation of historical phenomena and the selection of explanatory categories used in historical narratives. History, of course, can leave these assumptions unexamined, but that will not make them any less philosophical." (Mladenovic, 2012, p. 268) In short, Sharrock and Read's interpretation confuses a symptom of the philosophical problem with its cause; continued denial does not represent a good therapeutic outcome (philosophical or otherwise).

Without a doubt, Kuhn engaged in a struggle against a received reading of the history of science, a reading that functioned very much as an unacknowledged prop for the nascent philosophy of

<sup>8</sup> Two very good historical overviews of how this debate develops and the form it takes with the ascension of logical positivism are Habermas (1988) and Apel (1984).

<sup>9</sup> I shall follow D'Oro in using Hempel's essay as exemplifying the logical positivist position. However, inasmuch as I read the essay somewhat different than does she, readers might find it helpful to reference as well, e.g., Nagel (1979). Whatever some differences of detail, these works harmonize on the key point on which I insist, viz., the methodological unity of the sciences, and how the methodological unity entails the requirement of the use of covering laws. Neither Hempel nor Nagel ever claim that reasons cannot be causes.

<sup>10</sup> A more Kuhn-centric answer to the disappearance question owes to Pinto de Oliveira, who goes so far as to suggest that Kuhn offers a "new historiography of science." (Pinto de Oliveira, 2012, p. 115) On this account, philosophy of history as it was disappears because replaced by a new historiography crafted by Kuhn. This odd account rests on a reading of Kuhn's texts that might readily be contested. I mention it only to note that although it claims to analyze Kuhn as a historiographer, it does nothing of the sort. For it addresses none of the questions one might have regarding what makes for an adequate historical explanation no matter which general view of the history of science one reads into Kuhn.

<sup>11</sup> Although its title suggests a strong relevance to the subject under discussion, I ignore here Kindi's discussion. For one, she offers no textual evidence for the view she finds in Kuhn. (Kindi 2005) In addition, as I go on to develop, Kuhn's own remarks on historiography simply cannot be reconciled with Kindi's "transcendental" reading and her attribution to Kuhn of certain corresponding "first principles." Mladenovic offers a detailed and compelling debunking of Kindi (Mladenovic, 2007, pp. 278–282).

science. This received reading—science as cumulative, and the history of science as one of progress—it should be noted, involves the same absence of a philosophical base. For the received historical account never receives any more scrutiny qua explanatory model than does its Kuhnian alternative. But this gets ahead of the story.

Mladenovic attributes (correctly, as I shall argue) a high degree of self-consciousness to Kuhn regarding the historiographic challenge he faced, whatever his lack of reflection on the mode of argument that history constitutes. His problem, as she notes, is that “one cannot *argue* against an image, or a metaphor.... Kuhn couldn’t hope to be successful in erasing that image by producing specific arguments, however sound, against particular historiographical or philosophical claims and assumptions. Deeply entrenched images of this sort don’t just fade away when deprived of evidence to support them, for the simple reason that images are not *supported* by evidence.” (Mladenovic, 2012, p. 268) On this basis, she labels Kuhn’s endeavor as metaphilosophical. This seems apt, since Kuhn’s debate with the received view within the history of science (and, implicitly, within the philosophy of science as well) requires a recasting of the relationship between the history and philosophy of science. (Mladenovic, 2012, p. 275)

Now Mladenovic has her own account of how Kuhn proposes to “support” (her term, Mladenovic, 2012, p. 269) his model of scientific change, one she draws from Weber’s theory of ideal types. But as she understands them, an ideal type functions for Weber only as “a methodological tool, and its use is strictly heuristic.” (Mladenovic, 2012, p. 270) Think here of an actual Calvinist as embodying in the flesh Weber’s ideal type of the Protestant ethic. But she then goes to attribute ideal types a role in explanation.

Kuhn’s selection of ideal-type concepts is a reflection of his explanatory interest: he wanted to understand what and how science develops, and what the changes in that development imply from a philosophical point of view.... Nevertheless, ‘revolution’ is an (*sic*) useful ideal-type concept which accentuates incommensurability as the highly relevant feature for our understanding of scientific change; its presence explains rational disagreements among scientists. (Mladenovic, 2012, p. 273)

But what explanatory work could her proposed types actually provide? What would be needed to fill out her story would be an account of revolutions analogous to how the Protestant ethic helps explain a link between actual Calvinists and a newly crafted theological license for achieving material success. Otherwise, the ideal type heuristic offers absolutely no explanatory purchase.

Part of the way in which Mladenovic’s suggestions strains credibility here concerns the fact that Kuhnian revolutions simply do not “explain rational disagreements among scientists.” Rather, they signal precisely the point at which “rational disagreement” ceases to be possible. In addition (and certainly not determinatively), Kuhn himself gives no evidence that he takes the term to be more than a metaphor for the changes he hopes to characterize (e.g., SSR 92). More importantly, while the Weberian sense of ideal type provides explanatory insight (to the extent that it does) by approximation of actual cases to an analytic ideal, Kuhn simply has no ideal type of revolution on offer with respect to which actual cases can be illuminated. Rather, and not unlike those who unwittingly or not instigate such changes in theoretical views, he finds himself groping for a language which will allow others to see accepted facts in new ways. The novelty of SSR, in short, does not reside in Kuhn’s application of social science to the history of science, but precisely in recasting relatively well-known historical data into a very different narrative structure.

Paul Hoyningen-Huene voices a concern that more closely connects to themes of central concern in this paper when he states that he wants “only to show that and how the sociology and philosophy of science are dependent on the history of science. The upshot is: the history of science already determines, among other things, the realm of questions that can, in a sociological or philosophical perspective, be sensibly asked with respect to science.” (Hoyningen-Huene, 1992, p. 490) However, Hoyningen-Huene also records without comment a clear tension in Kuhn’s account at precisely this point. For, as he characterizes Kuhn’s discussion of the received historiography, Kuhn maintains that “we were possessed by a deceptive image of science” and that the challenge becomes one of understanding (again speaking for Kuhn) “how can we gain an undistorted image of past science”. (Hoyningen-Huene, 1992, p. 489) But if the philosophy and sociology of science presuppose the history, then what marks a history of science as being of the requisite “undistorted” sort?

Just here Hoyningen-Huene’s account fails to be on Kuhn’s behalf sufficiently self-reflexive (just as Kuhn himself turns out to be). For Hoyningen-Huene notes that, with regard to the rationality of theory choice, the cognitive values in play themselves turn out to be artifacts of a scientist’s historical situation. What else could they be? Hoyningen-Huene suggests that just here Kuhn follows Hempel insofar as it “seems to me that Hempel and Kuhn agree on the *possibility* of a justification of cognitive values, and perhaps also on the fundamentals of the *means* of justification.” (Hoyningen-Huene, 1992, p. 497) But what could this means be? Certainly not by an abstraction of scientific method from the practice of science. Indeed, logical positivism just was a program predicated on this idea, and this in turn tied to accounts of logic that themselves did not prove out.<sup>12</sup> In short, no good reason exists for taking as free of historical determination those norms of science that philosophers hold near and dear.

Hoyningen-Huene notes, in fact, just this concern, but only appears to hold science accountable to it.

What I find most fascinating about this approach is the prospect of a solution of a related problem in which sociological and philosophical aspects are also intertwined. It is the problem of the *change* of cognitive values in time, and of their difference in different scientific communities at the same time. Kuhn has described change and difference of cognitive values, but I think he has not answered the question how change and difference of cognitive values can be understood. (Hoyningen-Huene, 1992, p. 497)

Indeed, as Hoyningen-Huene explicitly recognizes, “the question arises how this change of values can be understood as a consequence of theory change, and whether such a change may count as justified. The latter question asks whether this sort of value change may be rational.” (Hoyningen-Huene, 1992, p. 498) Ironically, he then goes on to suggest that the metaphilosophical issue—the rationality of value change, can be answered by examining the goal(s) of science! “Thus, how should theory change justify value change? The puzzle dissolves once one pays attention to the fact that cognitive values relate to the ultimate goal of science which was, in Hempel’s words, ‘an increasingly comprehensive, systematically organized, world view that is explanatory and predictive.’” (Hoyningen-Huene, 1992, p. 498) But now his whole explanatory move has collapsed back on itself.

The problem began as one of the history of science predetermining what questions it made philosophical sense to ask. But when asked what to count as an “undistorted” view of the history of science, the answer on offer turns out to be one that a prior

<sup>12</sup> See especially Quine (1969) and Friedman (1993, 2002).

philosophical account of science tells the historian determines what counts as 'science'. This conflicts with a lesson from Kuhn that all accept, viz., that what to count as science has no historically stable boundaries. So Hoyningen-Huene's answer proves no answer at all. It simply restates the problem with which he began.

In accord with a previously noted point from Danto, Hoyningen-Huene also finds in Kuhn an ambivalence about embracing the role for history he so famously forges. Indeed, in his book that so closely examines the details of SSR, Hoyningen-Huene characterizes Kuhn's procedure in SSR explicitly in terms of the construction of a narrative. (Hoyningen-Huene, 1993, p. 20) He there also introduces two notions that underline the problematic character of the very nature of the history he goes on to so carefully explore, what he terms "narrative" and "pragmatic" relevance. "The moment of narrative relevance selects for material which must be taken into account if the resulting text is to be a proper narrative... Finally, the moment of pragmatic relevance selects for material without which the pragmatic goal of a historical narrative cannot be realized. Thus the content of a historical narrative is determined in part by the audience to which it is addressed and in part by the effect it is meant to have on this audience." (Hoyningen-Huene, 1993, p. 14; see also Hoyningen-Huene, 2012, pp. 282–283) But this says nothing about the critical issues regarding what makes for a "proper narrative" and the notion of "audience" to whom to give the explanation.<sup>13</sup>

#### 4. Kuhn, MacIntyre, and Mink on historical explanation

Kuhn himself also characterizes a history as a narrative. In one of the very few places he directly addresses this topic, he offers the following gloss on the notion of a narrative explanation:

The final product of most historical research is a narrative, a story, about particulars of the past. In part it is a description of what occurred... Its success, however, depends not only on accuracy but also on structure. The historical narrative must render plausible and comprehensible the events it describes. In a sense to which I shall later return, history is an explanatory enterprise, yet its explanatory functions are achieved with almost no recourse to explicit generalizations. (I may point out here, for later exploitation, that when philosophers discuss the role of covering laws in history, they characteristically draw their examples from the work of economists and sociologists, not of historians. In the writings of the latter, lawlike generalizations are extraordinarily hard to find.) (Kuhn, 1977, p. 5)

This remark could hardly be more explicit in rejecting the then prevailing Hempelian account of explanation as descriptive of or prescriptive for historical work. Yet it also unabashedly endorses history as an "explanatory enterprise," one built on narrative structure. "But I do claim that, however much laws may add substance to an historical narrative, they are not essential to its explanatory force. That is carried... by the facts the historian presents and the manner in which he juxtaposes them." (Kuhn, 1977, p. 16) But what marks some "manner" of juxtaposition as explanatory?

Kuhn states in his "Preface" to *The Essential Tension* with regard to this essay that this "lecture itself can be read as an effort to deal in somewhat greater depth with the issues already introduced in the preface." (Kuhn, 1977, p. xiv) Presumably this includes the point he makes in that very paragraph, viz., that the interest now

shown by philosophers of science in history "has so far largely missed what I take to be the central philosophical point: the fundamental conceptual readjustment required of the historian to recapture the past or, conversely, of the past to develop toward the present." (Kuhn, 1977, p. xiv) And this in turn needs to be juxtaposed to his cryptic remark, a few pages prior to the one just quoted, that "In history, more than in any other discipline I know, the finished product of research disguises the nature of the work that produced it." (Kuhn, 1977, p. x) Indeed, he clearly suggests that the finished product's disguise consists of the narrative form created by the historian, a form that itself does not reside "in" the world.

I have elsewhere argued that the cognitive content of the physical sciences is in part dependent on the same primitive similarity relation between concrete examples, or paradigms, of successful scientific work... Here I am suggesting that in history that obscure global relationship carries virtually the entire burden of connecting fact. If history is explanatory... it is because the reader who says, "Now I know what happened," is simultaneously saying, "Now it makes sense;... what was for me previously a mere list of facts has fallen into a recognizable pattern." I urge that the experience he reports be taken seriously. (Kuhn, 1977, pp. 17–18)

At this point, it would be germane to note that the only reference found in Kuhn's oeuvre to a card-carrying philosopher of history occurs in this essay. Kuhn makes approving reference to a 1966 essay by Louis Mink, "The Autonomy of Historical Understanding." And although he does not do more than mention the essay in passing, the points just made clearly bear Mink's stamp.<sup>14</sup>

This essay by Mink appears early in what became a series of very distinguished writings on this topic.<sup>15</sup> It would be a hermeneutic folly to read later Mink into early Kuhn. However, that said, it remains the case that Mink's 1966 remarks on the forms of "synoptic judgment" define for him (and by implication for Kuhn) what makes historical reasoning the sort of reasoning it is. Mink emphasizes the sense in which, as he puts it, historical conclusions are "non-detachable." This he identifies as the key factor differentiating the relationship that histories bear to their evidence and structuring narratives as opposed to evidence relation for in explanations in the sciences.

But despite the fact that an historian may "summarize" conclusions in his final chapter, it seems clear that these are seldom or never detachable conclusions; not merely their validity but their meaning refers backward to the ordering of evidence in the total argument. The significant conclusion, on might say, ... are represented by the narrative order itself. As ingredient conclusions they are exhibited rather than demonstrated. (Mink, 1987, pp. 61–88)

"Synoptic judgment" orders and structures the narrative, but the judgment cannot be supported or elucidated independently of the narrative that exhibits it. The narrative constitutes, in this specific sense, its own unique pattern of justificatory argument.

Given the reference to Mink that precedes his discussion of this point, I suggest that Kuhn can be read as endorsing this notion of synoptic judgment when he remarks, "Theories, as the historian knows them, cannot be decomposed into constituent elements for purposes of direct comparison either with nature or with each other.... For the historian, therefore, or at least for this one,

<sup>13</sup> Hoyningen-Huene glosses "narrative relevance" as the process of selecting "for material which must be taken into account if the resulting text is to be a proper narrative. Such material includes those facts by which a historical report gains the narrative continuity that it needs... or facts which make plausible what would otherwise be implausible." (Hoyningen-Huene, 2012, p. 282) But he has nothing of theoretical moment to say about what makes for a "proper" or "plausible" narrative.

<sup>14</sup> Lest it be thought that the considerations just rehearsed represent some passing phase of Kuhn's thought that he later disowns, note that in a biting review essay he publishes in 1980, he approvingly cites his 1968 essay as expressive of his view on the relation between the history and the philosophy of science. See Kuhn (1980, p. 183, fn. 1).

<sup>15</sup> Mink's 1966 work only hint at views that he will later develop, most notably in his 1978 piece, "Narrative Form as a Cognitive Instrument."

theories are in certain essential respects holistic.” (Kuhn, 1977, pp. 19–20) Now although talking about scientific theories, these remarks come at the conclusion of his Minkian speculations about the nature of historical explanation, and in what the autonomy of such historical explanation consists.

The familiar Kuhnian story about theory-ladenness, in short, applies not just to accounts of Aristotle’s physics, but characterizes as well the narrative structure historians deploy in trying to make this physics comprehensible to a later audience. In the penultimate paragraph of his essay, Mink puts his view this way:

I have tried... to ask whether “history” differs from “science,” not because it deals with different kinds of events and *not* because it uses models of explanation which differ from... the received model of explanation in the natural sciences, but because it cultivates the specialized habit of understanding which converts congeries of events into concatenations, and emphasizes and increases the scope of synoptic judgment in our reflection on experience.

Now synoptic judgment is not a substitute for a methodology, any more than “empathy” is a substitute for evidence;... So far it is only an attempt to identify what distinguishes sophisticated historical thinking from both the everyday explanations of common sense and the theoretical explanations of natural science. (Mink, 1987, p. 88)

Nothing here, of course, functions to unpack what Mink indicates as the mark of “sophisticated historical thinking.”

But in an essay published shortly after the one Kuhn cites, Mink adds a point of significance to understanding the respects in which a finished narrative disguises its explanatory intent. In a typical history, unlike a novel, the reader knows in advance (more or less) how the story turns out. The historian’s craft consequently does not consist in surprising the reader with twists of plot or nuances of character development. Rather, it manifests what Mink comes to call a “configurational” mode of understanding, i.e., the significance of an historian’s “emplotment” of the facts into narrative form. In this respect, Mink thus comes to argue, narratives “are in an important sense primary and irreducible. They are not imperfect substitutes for more sophisticated forms of explanation and understanding.... Stories are not lived but told.... There are hopes, plans, battles, and ideas, but only in retrospective stories are hopes unfulfilled, plans miscarried, battles decisive, and ideas seminal.... [B]ut it is from history and fiction that we learn how to tell and understand stories, and it is that stories answer questions.” (Mink, 1987, p. 60)<sup>16</sup> Alasdair MacIntyre, who shares Mink’s appreciation of the primacy of narrative structure in matters related to historical understanding, finds in Mink’s and Kuhn’s account his own special set of worries. Interestingly, these bear comparison to Kuhn’s own later concerns arising from the sociology of science that sprang from certain readings of SSR. For the stress on the primacy of narrative threatens, in MacIntyre’s pungent phrase, to fate Kuhn to being “the Kafka of the history of science.” (MacIntyre, 1980, p. 73)<sup>17</sup>

But not everyone shares MacIntyre’s worries here about the autonomy of narrative structure as a form of understanding. Norton Wise, himself a distinguished historian of science, claims that the “autonomy of written language [makes it]... a vehicle of critical reflection and creative imagination. This is as true in history and in science as it is in literature... their creative function reflects in part, I will argue, the capacity to support narrative of particular

kinds about the objects of science. The narratives take on different forms in different areas and they change over time.” (Wise, 2011, p. 351) Indeed, Wise goes on to complain that with respect to physics the “deductive structuring of the course of events has long defined what constituted an explanation in physics. The explanatory emphasis, however, has been on the deduction, to the exclusion of the attached narrative, and with that, the exclusion of anything like historicity in explanation.” (Wise, 2011, p. 355) He references in the course of this article Hempel’s critique of historical explanation, but like Kuhn did decades ago, dismisses it because it “does little to illuminate how narratives are related to explanations in natural science that do not depend on general laws.” (Wise, 2011, p. 371) Moreover, although mentioning in a footnote what he terms Kuhn and Hanson’s “historicizing ‘revolution’” against the Hempelian model, he goes on to complain (unfairly, I would say) that this revolution “did not stress science as narrative, nor did it attack deduction as explanation.” (Wise, 2011, p. 371 fn. 16) Alas, Wise’s own acquaintance with contemporary philosophy of history, at least as he records it in this piece, proves spotty at best and offers him no apparent resources to address what makes narratives explanatory even on the assumption (that I share) that they are.

My goal has been just to raise puzzles and questions that have oddly gone so long unasked and unexamined despite the massive influence narratives exert on how core issues in philosophy come to be understood. In this respect, the foregoing reflections only echo and elaborate frustrations voiced by John Zammito. “[W]hat seem to be lacking here is recognition that the problems of validity the philosophers stress in their theories about natural science apply with equal force to the utterly fallible, ineluctably empirical endeavor of history.” (Zammito, 2004, p. 100)<sup>18</sup> A willed blindness to this influence of history on philosophical thought also caught Stephen Toulmin’s attention more than four decades ago: “In both sociological theory and philosophy of science... questions about historical change were set aside at the turn of the century, in reaction against the historicism of the German idealist tradition and against the misconceived ‘evolutionism’ of Spencer and his successors. What we now have to do it to take up the discussion once again at the point where it broke off some 60 years ago.” (Toulmin, 1971, p. 63) By Toulmin’s calculation, analytic philosophy has passed the century mark in its refusal to reengage with these issues. Yet like the return of the repressed, unacknowledged historiographic issues continue to manifest themselves, haunting and hampering efforts to evaluate what to count as rational because of an ongoing refusal on the part of philosophers to examine just how works of history exert their undeniable power and influence. Kuhn’s example enduringly albeit ironically testifies to the hold that narratives exercise even on the philosophical imagination. Philosophical therapy (like other forms) can commence only by first admitting to and then attempting to comprehend the sources of this grip. As philosophers world-wide do homage to the 50th anniversary of the publication of Kuhn’s rightly celebrated book, surely the time has also come to also confront the detrimental effects of the discipline’s strategy of denial with regard to the historiography of SSR.

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<sup>16</sup> A related appreciation of the primacy narrative, though cast in a more critical tone for reasons not examined here, can be found in MacIntyre (1980).

<sup>17</sup> Virtually alone among philosophers, MacIntyre has consistently raised issues of SSR as a type of philosophy of history, and has pursued questions of the historiography needed to account for SSR as rationally persuasive. See especially MacIntyre (1980, 1985). I have elsewhere rehearsed my disagreements with MacIntyre and my reasons for favoring Mink’s views to his.

<sup>18</sup> With regard to Kuhn’s impact on and uptake by academic historians outside the sphere delimited by historians of science, see the discussion in Novick (1988). For a thoughtful and interesting utilization of Kuhn’s account as a format for examining changes in historiography, see Wise (1980).



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