“Has There Been a Second Kuhnian Transformation in the Philosophical Image of Science?”

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Thomas Kuhn and the 21st Century Philosophy of Science
Yafeng Shan, ed.

Thomas Kuhn’s *Structure of Scientific Revolutions* (1962, 1970) has been widely recognized as the centerpiece of a philosophical shift from logical empiricism to a post-empiricist philosophy of science. Kuhn’s book rose to the forefront despite the greater initial philosophical prominence of Hanson (1958), Feyerabend (1962), and Toulmin (1962) as critics of logical empiricism, with Quine (1953), Wittgenstein (1953), and Sellars (1963) even more prominent in the background. The book also influenced the Strong Programme of constructivist sociology of scientific knowledge despite Kuhn’s own ambivalence about that legacy (Bloor 1976, Barnes 1982, Collins 1985). In a series of earlier publications (Rouse 1987, ch. 2; 1998; 2002), I proposed an alternative interpretation of Kuhn’s opening promise of “a decisive transformation in the image of science by which we are now possessed” (1970, 1) and noted at the time that this second Kuhnian transformation in the philosophy of science had not (yet) occurred. Kuhn was not merely advocating a shift to an alternative conception of scientific knowledge, but was implicitly proposing a more significant shift in philosophical focus from scientific knowledge to *scientific practice*. Ronald Giere (1985) offered a somewhat different reading of Kuhn’s significance for philosophy of science as initiating a *naturalized* philosophy of science. Giere recognized that Kuhn’s naturalistic turn was also at that time still generally rejected by his successors in the discipline. The paper’s opening section reviews Giere’s and my earlier interpretations of the programmatic import of Kuhn’s book. The second section then characterizes how both a naturalistic turn in the philosophy of science (Callebaut 1997, Rouse 2023) and a philosophical focus on scientific practice became prominently conjoined within the discipline at the turn of the 21st Century. The concluding section asks whether and to what extent these subsequent transformations in the discipline accord with the programmatic proposals Giere and I previously attributed to Kuhn.

I — Two Projected Kuhnian Transformations of the Scientific Image

I begin by briefly recounting the reinterpretations of Kuhn's project which Giere and I first advanced decades ago. Familiar conceptions of Kuhn's effort to transform the dominant logical empiricist image of science begin with the role of paradigms as theoretical presuppositions shared by "normal scientific" communities. Paradigmatic theoretical presuppositions play a dual role on these readings. First, they indicate a scientific community's commitments concerning the ontological structure of their domain of inquiry, which are not directly open to empirical test. Paradigms specify what kinds of objects and interactions scientists are dealing with and in what terms they should be described and explained. Careful, painstaking empirical research is needed to show in detail *how* to characterize phenomena in these terms, but the basic terms are not questioned on pain of exclusion from a scientific community. Second, paradigmatic presuppositions also specify the norms of evidential and explanatory relevance that govern the assessment of this scientific "puzzle-solving." The norms for what was a legitimate empirical problem in this domain, what could count as evidence for or against a proposed problem solution, and in what sense that solution provided an explanation of the previously problematic phenomena were all derived from theoretical commitments embedded in exemplary problem solutions. Whereas logical empiricists had sought to discern formal-logical relationships to assess evidential and explanatory relevance and the empirical meaning of theoretical concepts in the sciences, Kuhn claimed that norms for theoretical understanding and empirical accountability are packaged with the substantive theoretical commitments of a paradigm.

On these familiar interpretations of Kuhn, paradigmatic commitments are never *directly* accountable to empirical evidence. Anomalous phenomena that resist assimilation in paradigmatic terms instead lead to increasingly divergent efforts to accommodate them and to an explicit emergence of latent differences in how to interpret paradigmatic achievements. The crisis provoked by the resulting internal incoherence of a community's previously shared theoretical commitments does not directly falsify those assumptions. If these crisis-provoking issues are not eventually resolved in traditional terms or the entire field abandoned, the field could only move on via a revolutionary joint reformulation of the field's ontological and normative commitments which is partially incommensurable with prior work. Kuhn's characterization of a resort to rhetorical persuasion to justify theory change in the absence of shared criteria of assessment thereby created a "crisis of rationality" for many philosophers of science (Hacking 1983, 1). Kuhn's account of theoretical change as not justifiable by appeal to shared norms generated divergent responses to the question of how scientific change was legitimated. Lakatos (1978), Laudan (1977), and Longino (1990) proposed meta-methodological norms to govern reasoned choices among competing packages of theoretical commitments and methodological norms. Constructivist sociologists argued for social explanations of scientific changes that could not be justified by rational norms (Bloor 1976, Barnes 1982, Collins 1985, Pickering 1984). Convergent realists replaced rational norms with causal continuities in the reference of the theoretical terms that guided methodological choices (Boyd 1984). This traditional conception of Kuhn's transformation of logical empiricism then reached its culmination in various attempts at moderate resolutions of disputes over whether and how scientific change was a rational process or a realist discovery (Miller 1988, Kitcher 1993, Galison 1998, Longino 2002).

Versions of this traditional reading of Kuhn's work still dominate philosophical interpretations of the book. Giere and I nevertheless discerned in Kuhn an aspiration to more far-reaching transformations of philosophical understanding of science. Giere read Kuhn as initiating a *naturalistic* philosopher of science. His account did not involve a detailed reinterpretation of Kuhn's account of normal science and scientific revolutions, but instead focused on Kuhn's own theoretical positioning in offering that account. Logical empiricists had offered a distinctively *philosophical* account of the norms governing scientific knowledge, grounded in commitment to empiricist conceptions of what could provide authoritative grounds for the content and authority of scientific claims and formal-logical derivations of the evidential relevance of observation statements and the explanatory power of laws. Their early post-empiricist critics offered different methodological groundings for scientific knowledge— ideals of natural order or other theoretical commitments (Toulmin 1962, Hanson 1958, Feyerabend 1962), meta-methodological criteria for justifying scientific methodologies (Lakatos, Laudan, Longino), semantic-externalist theories of reference (Boyd), or revisionist empiricisms (van Fraassen 1980)— but persevered in their commitment to a philosophical reconstruction of scientific achievements in order to "make better sense" of their claims to scientific knowledge (van Fraassen 1980, 73). Giere contrasted all of these accounts to Kuhn's claim that philosophical conceptions of scientific methodology are best understood as *empirical* theories of scientific knowledge:

> Rather than being elementary logical or methodological distinctions, which would be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions on which they have been deployed. That circularity ... makes them parts of a theory and, by doing so, subjects them to the same

> scrutiny regularly applied to theories in other fields. ... How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply? (Kuhn 1962, 9)

Giere then noted that Kuhn saw history of science as primarily providing evidence to which empirical theories of science would be accountable, but also allowed for the possibility that science was itself an historical phenomenon whose methods, strategies, and norms changed over time. Giere himself then tentatively proposed cognitivist, evolutionary, and developmental models as possible directions for such empirical theorizing in naturalized philosophies of science.

My alternative reading of Kuhn's projected transformation of the philosophical image of science focused on the locus of that image rather than Kuhn's own theoretical standpoint in developing it. When Kuhn contrasted his account to an image of science drawn from textbooks, his challenge was not just directed against *how* textbooks portrayed scientific knowledge. His aim was to shift philosophical attention away from knowledge as the product of scientific research to the research activity itself, that is, to *scientific practice* as an ongoing, future-directed enterprise. On Kuhn's account, the primary role of normal scientific research was not the formulation and assessment of knowledge claims, but the articulation of conceptual understanding that could guide subsequent research. Paradigms are not theoretical commitments that normal scientific communities hold immune from empirical test. They are instead exemplary applications of theoretical concepts whose content is initially open-ended and sketchy. The principal role of normal scientific research is "paradigm articulation," in which scientists work out just how their core concepts apply to a wider range of empirical situations and thereby further explicate how to speak and reason about phenomena in their domain. Partly this involves the specification of conceptually significant facts but more importantly, it involves working out what these concepts mean concretely in a wider range of situations. The principal difficulty underlying the partial incommensurability of competing paradigm-candidates is how to compare research programs at different stages of conceptual development. How does one assess the *prospects* of two research strategies when is one extensively articulated through a history of protracted success that has now stumbled on recalcitrant problems, while the other has a few tantalizing successes together with an array of unfulfilled conceptual promissory notes? As Kuhn noted, "crucial tests" in which two competing theories generate opposing predictions usually come too late (1977, 327-28; 1970, 156). A detailed empirical comparison of competing programs can often only be formulated well after the collective switch to a new paradigmatic model has already been made on other grounds. Similarly, the scientific textbooks re-written in the wake of a Kuhnian revolution were less a retrospective reconstruction of scientific knowledge, and more a prospective orientation of novice researchers toward the central unsolved problems of the field and the material and conceptual resources they would need to solve them.

Kuhn is often assimilated to other post-empiricists who proposed what Ian Hacking called "theory-dominant" accounts of scientific knowledge (1983, ch. 9). There is no shortage of passages in which Kuhn assigns a leading role for theoretical understanding in guiding scientific research, but his view on this issue is more complicated. Normal scientific communities were united less by a theoretical consensus and more by mutually intelligible research strategies. These strategies, often modeled directly from prior concrete achievements, normally do not require explicit theoretical interpretation and often conceal latent theoretical disagreement that only emerges explicitly in the face of empirical or conceptual difficulties. Paradigms are thus not theoretical presuppositions, but concrete models for how to understand and analyze phenomena in ongoing research. Kuhnian paradigms canonically are “examples of actual scientific practice which include law, theory, application, and instrumentation together” (1970, 10). Kuhn also saw that experiment and measurement were often integral to the specification of conceptual content rather than empirical tests of independently specified claims. Canonical measurement outcomes often

> define “reasonable agreement.” ... An acquaintance with the tables [comparing theory and experiment] is part of an acquaintance with the theory itself. Without the tables the theory would be essentially incomplete. (1977, 185-86)

Structure also included several cases in which experimental practices were integral to Kuhnian paradigms, from cathode ray studies that led to the discovery of X-rays to canonical tests for “the goodness of air” that guided phlogistron chemistry (1970, 57-60). He noted in a different case that,

> Many of the early experiments involving thermometers read like investigations *of* that new instrument rather than investigations with it. How could anything else have been the case during a period when it was totally unclear what the thermometer measured? (1977, 218)

At the outset of research on heat, explicating the concept of temperature and learning to use and understand the thermometer were not separable projects. Paradigms thus combined instruments, procedures, and skills in ways of *using* and projecting theoretical concepts to understand concrete situations through what Sellars (2007, ch. 1-2) had previously identified as material rather than formal inferences.

II — Naturalism and the “Practice Turn” in the Philosophy of Science

Writing initially in the 1980's, Giere and I respectively saw a Kuhnian legacy in our arguments for naturalism in the philosophy of science and for a shift in philosophical focus from the structure and character of scientific knowledge to scientific understanding as embedded in research practices. We also each acknowledged that these philosophical programs we attributed to Kuhn were as yet minority positions in the philosophy of science, and often not even widely recognized there as serious possibilities. In the ensuing three decades, that situation has changed significantly. Naturalism in the philosophy of science is now not merely a dominant view; it has become so firmly grounded in the field's self-understanding that it need not be explicitly defended or even noted apart from introductory remarks in passing (Rouse 2023). A philosophical turn toward understanding science in practice has also become widespread, although it remains more controversial and hence is more frequently discussed and justified (Soler et al. 2014). We nevertheless need to examine these shifts within the discipline more carefully, in order then to assess how they stand in relation to the prospective transformations that Giere and I discerned previously in Kuhn.

To understand the version of naturalism characteristic of most contemporary philosophy of science, one must recognize that the naturalism advanced there is distinct from naturalism as widely espoused in other areas of philosophy. For all its divergent forms, naturalism elsewhere in philosophy denotes a commitment to treating philosophy as continuous with science and situating philosophical work within the horizons of a scientific conception of the world. In philosophy of science, however, naturalism involves a rethinking of the very idea of a scientific conception of the world, and where and how it can be discerned. This latter philosophical reorientation arguably had its beginnings in the emergence of philosophy of biology as a separate sub-field. Philosophers of biology gradually stopped asking the question of whether the life sciences fit standard philosophical models of explanation, empirical confirmation, theoretical structure, or ontological commitment, and instead asked how these issues were actually addressed in various biological inquiries. If the best work in biology, which was the locus of many of the most exciting scientific developments of the late 20th Century, did not fit standard philosophical models of scientific knowledge largely drawn from the physical sciences, so much the worse for philosophy. This approach to the biological sciences gradually infiltrated much of the field, which increasingly aimed to understand what Nancy Cartwright succinctly summarized as "science as we know it: apportioned into disciplines, apparently arbitrarily grown up; governing different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; here and there, once in a while, corners that line up, but mostly ragged edges; and always the cover of law just loosely attached to the jumbled world of material things" (1999, 1). There is no better science than the science we have as an ongoing, historically developing research enterprise.

This implicit challenge to philosophical authority over the practices and achievements of the sciences was made explicit in Arthur Fine's (1986a,b,c; 1991; 1996) proposal for a "Natural Ontological Attitude (NOA)." Fine challenged all sides in the post-empiricist debates among realists and anti-realists. These debates mostly supplanted earlier discussions of scientific rationality, which were thereby repositioned as versions of anti-realism. NOA suggests that,

> ... what the scientific enterprise has to offer is actually sufficient to satisfy our philosophical needs. It urges us to explore what happens philosophically when we approach science with trust, and openly; i.e. without rigid attachments to philosophical schools and ideas, and without intentions for attaching science to some ready-made philosophical engine. (1986c, 177)

Fine's deflationary approach to familiar empiricist and post-empiricist accounts merged with a gradual rapprochement between empirical studies of scientific work by sociologists, historians, anthropologists, or feminist theorists and philosophical approaches to a science's conceptual content, experimental practices, or norms and forms of modeling, reasoning, explanation, or data analysis. Early adherents of "social constructivism" had often waded into philosophical debates over rationality or realism by advancing alternative answers to those questions. Partly in response, books such as Philip Kitcher's (1993) *Advancement of Science*, Richard Miller's (1988) *Fact and Method*, or John Zammito's (2004) magisterial history of the period developed or promoted more modest but still systematic defenses of scientific rationality or realism. By the 1990's, however, several prominent anthologies (Pickering 1992, Galison and Stump 1996) instead displayed movement away from both these questions and various answers to them on all sides. A wide-ranging series of interviews by Werner Callebaut (1993) assimilated the work of diverse philosophers, historians, and sociologists under the heading of "the naturalistic turn" in philosophy of science. By the turn of the millenium, journals had generally stopped publishing articles advocating this naturalistic consensus; explicit commitments to a naturalistic philosophy of science were relegated to the prefaces of books that evidently saw no need for extended justifications of this stance (e.g., Nersession 2008, Solomon 2001, Dupre 2012). The turn to naturalism in the philosophy of science thus seems to exemplify Dewey's dictum that "intellectual progress usually occurs through sheer abandonment of questions together with the alternatives they assume .... We do not solve them; we get over them." (Dewey 1909, 188)

Meanwhile, apart from Kuhn's own advocacy, philosophical focus on scientific practice emerged initially in two related bodies of work. This shift was first more apparent in work that directed attention toward experimentation and instrumentation as integral components of scientific research that were often omitted from philosophical studies of scientific knowledge

(Hacking 1983; Ackermann 1984; Rouse 1987; Ihde 1991). This philosophical resurrection of experimentation as more than a means to empirical observation and testing hypotheses was paralleled by a central focus on experimentation and field methods in the new sociology and anthropology of scientific knowledge (Latour and Woolgar 1979; Knorr-Cetina 1981; Shapin and Schaffer 1985; Traweek 1988; Haraway 1989; Star and Griesemer 1993). In both cases, the concern was to understand material practices and systems, experimenters' skills and judgment, and the creation or discernment of meaningful patterns in the phenomena themselves.

A turn toward scientific practice also emerged, however, in a new approach to theoretical understanding, which shifted away from analyzing the structure of theories as exemplary of scientific knowledge to consider practices of theoretical modeling (Cartwright 1983; Hacking 1983, ch. 9; Giere 1988; Morgan and Morrison 1996; Teller 2001). The distinctiveness of this conception of theoretical practice as modeling was not initially recognized because it was often conflated with semantic and structuralist conceptions of theories advanced by Patrick Suppes (1967), Joseph Sneed (1971), Wolfgang Stegmüller (1976), and Frederick Suppe (1976). As Paul Teller noted in retrospect, however, this alternative approach to models as mediating between theoretical concepts and empirical phenomena set aside questions about model structure to examine how models are used and revised:

> When people demand a general account of models, an account which will tell us when something is a model, their demand can be heard as a demand for those intrinsic features of an object which make it a model. But there are no such features. WE make something into a model by determining to use it to represent. (Teller 2001, 397)

Mark Wilson's (2006) monumental study of modeling practices in mechanics and materials science noted that the same theoretical formalism and its concepts were often modeled quite differently in different parts of its domain or for different purposes. These different treatments are not seamlessly connected, but instead form patchworks that he analogized to the overlapping facades of maps in an atlas. The shift to scientific understanding in practice also expanded to encompass the ontological issues that had once been the focus of debates between realists and anti-realists. Julia Bursten's succinct summary of how philosophers of science now understand scientific classifications of kinds exemplifies this shift:

> From the problem of species classification in biology to the development of the standard model in particle physics, kinds are useful to science insofar as they can help scientists to determine how to carry out an experiment, confirm a theory, or explain a set of natural phenomena. (2016, 5)

Bursten's treatment of kind classifications in some respects parallels Wilson's study of conceptual specification; in both cases, close attention to scientific practice displays conceptual and ontological divergences at different scales or for different purposes.

Philosophy of science in the 21st Century has clearly moved in a broadly naturalizing direction, and has increasingly focused its attention on a broader range of scientific practices. The key question I now ask, however, is whether these shifts in the field are belated fulfillments of Kuhn's projected "dramatic transformation in the [contemporary] image of science" as Giere and I each described it, or whether they pursued a naturalistic philosophy of science and a turn to scientific practice that diverge from the projections Giere and I previously attributed to *The Structure of Scientific Revolutions*.

III— Scientific Practice, Naturalistic Philosophy of Science, and Thomas Kuhn

Kuhn's philosophical standpoint and conclusions in *The Structure of Scientific Revolutions* and his contemporary papers are complex and multi-faceted. The ensuing 60 years have also seen significant transformations in the sciences along with many different directions taken by philosophers of science in the wake of logical empiricism and its early post-empiricist successors. It would be surprising if there were univocal answers to the question of whether subsequent developments followed Giere's and my projections of an alternative Kuhnian transformation in philosophical understanding of the sciences. Productive conceptual innovations also typically develop further in ways that were not anticipated at their outset. In the remainder of the paper, I consider multiple strands of Kuhn's account and their varied relations to the philosophy of scientific practice and to naturalistic philosophy of science.

I begin my discussion of Kuhn and recent philosophy of scientific practice with the turn of philosophical attention away from the structures of scientific theories to practices of theoretical modeling. This aspect of Kuhn's work is widely recognized as both central to the book and as one of Kuhn's most important innovations. Moreover, the most prominent works that are now seen as initially differentiating a conception of theorizing as model-building from earlier structuralist or semantic conceptions of theories each prominently cites Kuhn's discussion of paradigms as shared examples in section 3 of his 1970 Postscript. Nancy Cartwright's chapter on the "Simulacrum Account of Explanation" begins by sketching a "Kuhnian account" that her chapter pursues further (1983, 143-44); Ron Giere describes his chapter on "Theories and Models" as written in "the spirit of Kuhn's writings both early and late" (1988, 285n3); Ian Hacking builds much of his chapter on theorizing and models jointly around Cartwright's account and Kuhn's notion of "paradigm articulation" (1983, 213-17). Subsequent work in this tradition nevertheless typically refers prominently to Giere and Cartwright, while ignoring Kuhn and their generous acknowledgments of his influence (e.g., Morgan and Morrison 1996, ch. 1; Teller 2001).

I suggest three reasons why Kuhn's account of paradigm articulation as developing analogical relations among families of models has largely dropped out of the citation history of what is now widely viewed as the Cartwright-Giere account of theorizing or the conception of models as mediators promoted by Morgan and Morrison (1996). The first reason arises from subsequent responses to Kuhn's later distinction between two senses of 'paradigm'. Kuhn recognized that the sense of paradigms as exemplary problem solutions governed his choice of the term, and he suggested that the more encompassing sense of paradigms as multi-factorial disciplinary matrices was derivative from paradigmatic exemplars. For many readers, however, the more inclusive sense of paradigms as disciplinary matrices and its association with Kuhn's account of incommensurability remains the more prominent conception. Consider for example Mark Wilson (2006), whose richly detailed account of the patchiness of conceptual development in applied mechanics and materials science is a recognizable, sophisticated descendant of Kuhn on exemplary problem solutions. He nevertheless refers to Kuhn almost solely as a philosophical antagonist, conjoined with Quine on webs of belief and the indeterminacy of translation as preeminent advocates of what Wilson disparages as a "hazy holism" about concepts. A second, more speculative reason is Wolfgang Stegmüller's (1973) association of Kuhnian paradigms with Sneed's structuralist account of theories, and Kuhn's own (1976) positive appreciation of that parallel. In retrospect, the divergence of the Cartwright-Giere conception of theoretical modeling from structuralist or semantic accounts of theories is more salient than the parallels. Cartwright and Giere turn away from a model-theoretic conception of models, from isomorphism as a defining relation between model and target, and more generally, from a concern with the structure of theories toward dynamic practices of theorizing. In this respect, however, the initial comparison of Kuhn's view to Sneed or Stegmüller is misleading, and Giere and Cartwright were right to acknowledge Kuhn as their predecessor.

A third consideration yields a more ambivalent outcome. Kuhn's account of paradigms was conceived as an alternative to a logical empiricist conception of theories, and he explicitly chose the term 'paradigm' to circumvent the familiarity of that received view (1970, 182, 187-88). In arguing for this role for exemplary problem solutions, however, Kuhn seemed merely to reverse the traditional account: the cognitive content *of the theory* was embedded in the concrete problem solutions *rather than* the symbolic generalizations that seem to encompass them. Subsequent work has challenged the residual role still assigned to a supposedly general theory. Wilson portrays this as a further aspect of hazy holism:

> As the implausibilities of logical empiricist doctrine became apparent, most philosophers of science, encouraged by Quine and Kuhn, decided that language still worked "kind of like theories but not quite so formalized"—e.g., that scientific predicates are somehow buoyed semantically aloft by "paradigms," "webs of belief" or the "practices of a scientific community." ... In rendering "theories" into schematic T's and T`'s, our [theory T] syndrome puffs the humble word "theory" into something quite grand, without it being exactly clear in what its grandeur consists. (2006, 127)

Wilson urged careful attention instead to the details of conceptual development and empirical application within a science, while dispensing with facile generalizations.

Ian Hacking similarly objected to invocations of the "theory-ladenness" of observation as overly unifying and dignifying the wide range of "expectations, prejudices, opinions, working hypotheses and habits" people bring to what we see or what we say (1983, 175). The term 'theory-laden' was more characteristic of Hanson, Feyerabend, or Lakatos than Kuhn, but Kuhn also similarly talked about how scientists' perceptions were shaped by paradigmatic expectations, sometimes invoking Gestalt psychology. In both cases, however, Kuhn's presentation was shaped by the need to address his logical empiricist contemporaries. In this rhetorical context, Kuhn needed to place exemplary problem solutions in the philosophical role previously occupied by scientific theories, even though their sketchily open-ended promissory extension was nothing like a formally structured or even explicitly formulated theory. Similarly, he emphasized the limits of appeals to observation to commensurate judgments about how to pursue further research because for his empiricist critics observation was the gold standard for theory assessment. Only once these battles have been won can one then conclude that, with philosophical focus shifted to theoretical modeling and experimental practice, the *places* previously assigned to formally structured theories as embodiments of knowledge, and to observation as a decisive interface between theory so conceived and "the world," drop out along with their prior philosophical occupants. Margaret Morrison could straightforwardly claim in 1996,

> (1) that it is models rather than abstract theory that represent and explain the behavior of physical systems and (2) that they do so in a way that makes them autonomous agents in the production of scientific knowledge. (1996, 39)

Kuhn himself was not in a position to make either claim in this strong a form, especially not the second, but Morrison's claim is straightforwardly descended from Kuhn's criticism of traditional conceptions of theory and his account of exemplary problem solutions and their roles in guiding subsequent research.

The turn from the structure of theories to practices of theoretical modeling is one aspect of recent philosophy of scientific practice with a clear Kuhnian heritage. Kuhn has a somewhat more complex legacy with respect to a related issue concerning scientific practices. The notion of scientific laws or laws of nature has often been de-emphasized in recent philosophical studies of scientific practice. That diminished role is partly due to the shift from laws to theoretical models, but also to the decline of Hempel's deductive-nomological account of explanation and to increased attention to the evolutionary contingency, complexity, and variation of living systems, which seem to display no simple law-like necessities (Brandon 1996, Mitchell 2003). Kuhn's account also assigned no prominent role to laws, undoubtedly because of his emphasis on a multiplicity of concrete applications of theoretical concepts rather than a more general nomological invariance.

Work by Marc Lange (2000, 2007), John Haugeland (1998, 2013), and myself (2015 ch. 8-10) nevertheless provides an important counterpoint. Instead of asking which if any sciences discover laws as traditionally conceived, we look to the role of lawfulness in scientific practice in enabling inductive projection, counterfactual reasoning, experimental design, and the constitution of domains of inquiry. "Laws" are whatever plays those roles in scientific research in each scientific domain, and typically their lawful invariance is collectively constituted: lawful patterns are those that would hold under any counterfactual circumstances consistent with the other lawful patterns that collectively mark out a conceptual domain. Kuhn was a prominent influence on Haugeland's version of scientific modalities in particular.[1] Haugeland emphasized Kuhn's (1970, ch. 6) discussion of "fundamental" anomalies as phenomena that are not just unexpected but presumptively *excluded* by commitment to a paradigm. He also further developed a Kuhnian account of the mutually constitutive interplay between the modal invariance of conceptual relations and the normative concerns that govern research in that

[1] Haugeland was co-editor of a prominent posthumous collection of Kuhn's philosophical papers (Kuhn 2000), but also explicitly cited Kuhn at multiple points.

domain (1998, ch. 13; 2013, 260-76). Lange and I develop related arguments without explicit reference to Kuhn, but likewise emphasize that domain-constitutive lawfulness demarcates distinct forms of invariance, such as chemical, biological, or medical necessity in addition to familiar notions of logical and physical necessity. To varying degrees, however, all three proponents of practice-constitutive lawfulness also consider the possibilities for "heteronomic" research and less-systematic accountability to research in other domains, in ways that Kuhn rarely addressed.

Kuhn's legacy for the increased contemporary focus on experimentation and instrumentation is more ambivalent. He came to *Structure* as a scientifically trained intellectual historian in the tradition of Alexandre Koyré, and he was primarily concerned with the development and deployment of theoretical concepts and conceptual relations in physics, chemistry, and astronomy.[2] He was among those, from Hempel and Sellars to Toulmin, Feyerabend, and Putnam, who at mid-century rejected earlier empiricist suspicions about unobservable entities and assigned more robust roles to theoretical understanding. His essays on measurement and thought experiments and several examples in *Structure* were nevertheless important early discussions of experimental practices as distinct from both theory and empirical observation. He also explicitly recognized the importance of instrumentation and experimentation for conceptual development in the sciences. In two evident respects, however, subsequent work on experimental practice has far outstripped this aspect of Kuhn's initial forays into a philosophy of scientific practice. Experimentation and its parallel in field-based research

[2] Despite Kuhn's all-too-traditional focus on the physical sciences, his insistence on the "ramshackle" disunity of scientific understanding (1970, 49) helped open a place for the emergence of an autonomous philosophy of biology.

play a far more central role today than it did in Kuhn's own work. As a second consideration, the center of gravity in such work has shifted toward the life sciences, which were almost entirely absent from Kuhn's own philosophical and historical interests.

In this context, Rachel Ankeny and Sabina Leonelli (2016) instructively chose to contrast their influential account of scientific repertoires to Kuhnian paradigms as alternative conceptions of scientific change and the social organization of research. Ankeny and Leonelli's primary examples are model organism communities and microbiome projects in the life sciences. They nevertheless explicitly cite *Structure* as a starting point for their work before developing several critical contrasts, because of Kuhn's "intertwining of conceptual, social, and material factors in research" (2016, 19). Their first worry is then that Kuhnian paradigms are "static and inflexible," although I think this judgment somewhat misreads Kuhn. He does talk about the *continuity* of a single paradigm throughout periods of normal science, but despite that continuity, paradigms undergo considerable transformation and redirection as their initially sketchy promissory notes lead to further articulation. More significant contrasts arise from Ankeny and Leonelli's focus on collaboration among different research groups rather than communities of individual researchers. They also emphasize that research groups may deploy multiple repertoires in different projects in contrast to the hegemony of a Kuhnian paradigm. These latter contrasts may nevertheless reflect changes in the sciences from Kuhn's historical examples to contemporary funding regimes and more competitive research environments.

Ankeny and Leonelli's account did bring out an overlooked aspect of Kuhn's account that bears on Giere's reading of Kuhn as a naturalist. Neither Kuhn nor Ankeny and Leonelli aspire to a fully *general* account of scientific practices or scientific change. Although Kuhn thought that the rise of paradigm-guided normal science explained the distinctive successes of many sciences, he also recognized that not all sciences do converge on a paradigm, and many may never do so, especially in the human sciences or in more "applied" research. Ankeny and Leonelli similarly regard scientific repertoires as an important and increasingly common form of research organization, but also note that much scientific research proceeds in other ways. As just one example, the repertoires constituted around a comparatively small number of model organisms have grown in influence, but that has not prevented biologists from utilizing other experimental organisms when appropriate. Kuhn's and Ankeny and Leonelli's shared acknowledgement of the diversity of scientific practices and the possibility of changes in how science is practiced across disciplines and over time exemplifies an important aspect of naturalistic philosophies of science..

Ankeny and Leonelli's most significant divergence from Kuhn nevertheless results from their expanded conception of the philosophically significant components of a research repertoire. Despite recognizing Kuhn's "deep awareness" of the material and social dimensions of research, they decry his narrow focus on theoretical aspects of the sciences and his internalism. Ankeny and Leonelli instead insist that

> consideration of what results from research needs to encompass a much wider range of phenomena including technological innovations, data generation, the production of new models and visualization techniques, and novel ways to organize, manage, and support research communities. Thus decisions and strategies concerning funding as well as the management and dissemination of resources and outputs (among other factors) are as scientifically and epistemologically significant as decisions and reasoning about theory, methods, instrumentation, technologies, and models, as well as the types of expertise and compositions of the groups tasked with performing research. (2016, 20)

As experimentation, instruments, and experimental systems become more complex and take on growing importance in both enabling and directing research possibilities, scientific change increasingly involves new materials, technologies, infrastructures, funding sources, and the social *organization* of research that goes beyond simply identification of scientific communities.

Ankeny and Leonelli's more socially, institutionally, and materially expansive conception of a repertoire brings out an important respect in which other recent philosophy of science also seems to diverge from Kuhn's account of normal scientific practices. An important theme in *Structure* was Kuhn's defense of the normative autonomy of normal scientific practice. For Kuhn, the distinctive success of the (normal) sciences resulted from their ability to focus on questions that they should be able to answer, in terms and according to norms instituted by paradigmatic achievements (1970, 37). The sciences make valuable contributions to issues of broader technological, social, or political import, but can best do so by advancing their own conceptualization and resisting direct accountability to those issues. Other normative concerns would need to be re-defined in scientifically tractable ways rather than the reverse.

Kuhn's commitment to the normative autonomy of scientific work was widely shared throughout much of late 20th Century philosophy of science, but *Structure* provided a different rationale for its stringent internalism, which he regarded as an empirical claim about how science is practiced and how many of its successes were achieved. The most prominent early criticisms of this widespread orientation came from feminist philosophers of science, for whom Kuhn's work offered a conflicting legacy. Feminists resolutely rejected his commitment to insulating the (normal) sciences from external criticism. Kuhn's emphasis on the role of particular scientific communities in establishing and enforcing conceptual and justificatory norms in their field nevertheless provided openings for recognizing and criticizing how sexism and androcentrism could infiltrate the socially instituted norms of predominantly male scientific communities

(Longino 1990, Hankinson-Nelson 1990, Wylie 2002). The resurrection and expansion of Hempel's (1965) conception of inductive risk then provided an entree for a more widespread rejection of Kuhn's normative internalism. Recognition that the need to incorporate assessment of the possible consequences of scientific error infiltrates every aspect of scientific practice undermined the supposed separation of epistemic from non-epistemic norms (Douglas 2000, Biddle 2016, Elliott and Richards 2017, Elliott 2017). In some respects, however, this shift also had much to do with developments in the sciences themselves and their political entanglements that became increasingly evident after 1962. The environmental movement, opposition to the Vietnam War, criticisms of the prospects of nuclear war and the hazards of nuclear power, the emergence of biotechnology, and the massive growth of pharmaceutical medicine challenged any pretension to the intellectual and practical insularity of "basic" science.

Subsequent consideration of the political significance of scientific practice nevertheless has also expanded beyond more narrowly methodological concerns with inductive or epistemic risk. Amid the growth of close philosophical engagement with the life sciences, late-20th Century philosophers of biology found themselves also responding critically to sociobiology, new forms of genetic determinism and scientific racism, and an increasing corporate capture of biomedical research. From a different direction, the superficial deployment of post-empiricist philosophical themes to defend religious alternatives to evolutionary theory, challenge prominent environmental initiatives (Oreskes and Conway 2010), politicize climate science, or regiment the use of evidence in public policy has brought renewed significance to political reflection on science and public policy. These philosophical responses included renewed attention to classic Deweyan, Mertonian, or Popperian analogies between science and democracy and efforts to balance scientific expertise and democratic policy-making (Kitcher 2001, Brown 2009, Douglas

2009); critical attention to the import of causal pluralism for evidence-based policy (Cartwright 2019, Reiss 2015); and more expansive conceptions of norms of justice as integral to scientific practice and understanding (Reardon et al. 2015, Leonelli forthcoming).

These more expansive conceptions of the normativity of scientific practice call attention to a prominent counterpoint to Giere's and my interpretations of Kuhn as philosophically ahead of his times. In distinct ways, we each saw Kuhn's project as aspirationally more philosophically transformative than the initial turn away from logical empiricism from the 1960's to the 1980's. Kuhn has also been prominently read as a philosophically conservative product of his times, however. Historian David Hollinger situated Kuhn's book "in the heart of the period in which both modernization theory and the end of ideology were prominent preoccupations of American social scientific intellectuals" (1996, 451), attributing to him and his generation of science studies scholars a kind of complacent acceptance of the political autonomy of the sciences. Kuhn's conception of normal science as needing to control its own conceptual and normative agenda insulated the sciences from social scrutiny and political challenge. Social philosopher Steve Fuller took this line further, arguing that Kuhn contributed to a conservative legacy in both post-empiricist philosophy and constructivist sociology. "In reducing the voice of reason to that of a scientific insider, *Structure* managed to satisfy an aspiration left unfulfilled by the original logical positivists. ... Kuhn's lesson has exacted its own substantial Kuhn's Loss, as the triumph of his paradigm of scientific change has marginalized the forums for a radically critical rationalism, one that questions the ends of science as well as the means" (1992, 256, 274)

The juxtaposition of a more expansive conception of the political significance of scientific understanding in practice with this critical reading of Kuhn as a philosophically conservative defender of a scientistic end of ideology facilitates a transition to Giere's account of

Kuhn as pioneering a naturalistic philosophy of science. Hollinger's and Fuller's criticism of Kuhn depends on a contrast between the standpoint of insiders to a practice and an "external" critical standpoint, characterized by Fuller as the "voice of reason." That contrast, however, is exactly what is rejected by the naturalizing stance that Giere discerned in Kuhn and that has since become dominant within 21st Century philosophy of science. Although Giere does not directly address Fuller and Hollinger, he would surely assimilate their criticism to other rationalistic philosophies of science that naturalists reject, as

> ... approaches [that] assume the more general principle of rationality that scientists generally strive to make a *rational* choice, however this is defined, ... mak[ing] scant reference to the actual flesh-and-blood scientists who do the choosing. The approach is almost totally "top down." A naturalistic approach to theory choice is explicitly "bottom up." It begins with real agents facing various choices in the course of their actual scientific lives. (1985, 347)

From this naturalistic stance, Hollinger's and Fuller's criticism depends on claiming that "insiders" to a practice share a self-enclosed normative consensus that needs critical assessment from an "external" perspective and/or standard.

My own reading of Kuhn as a philosopher of scientific practice (1987, 1996) challenged that conception of practices early on and emphasized the broader political significance of that challenge. That re-conception partially reconciles Kuhn's legacy with the broader normative orientation of recent philosophical studies of scientific practice and also that of the "cultural studies of science" increasingly undertaken by anthropologists, historians, and other science studies scholars (Rouse 1993). Part of what differentiated Kuhn's view both from his post-empiricist contemporaries and from early sociological constructivists was his recognition that the shared practices he attributed to normal scientific communities were not equivalent to shared beliefs and norms. As he prominently insisted, scientists can "agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research" (1970, 44). Kuhn's emphasis on shared practices without shared beliefs was thus an important precursor to a more expansive normative point of view. Although Hollinger and Fuller were not altogether wrong to recognize the influence of a postwar complacency about the politics of science in Kuhn's work, Kuhn at least changed the philosophical status of that complacency. In contrast to his contemporaries' commitment to an ahistorical scientific rationality, Kuhn was making an empirical claim about how science is practiced. Fine's Natural Ontological Attitude similarly "does not prejudge the constitution of the scientific world; that is whether the scientific facts and objects are essentially social or essentially objective, or whatever. Its attitude is to let the chips fall where they may" (Fine 1996, xx). Subsequent work in the philosophy of scientific practice suggests that Kuhn's empirical claim about the normative autonomy of normal scientific research was mistaken, but that recognition is consistent with Kuhn's broader approach to understanding scientific research. That was also why feminist philosophers of science discerned an ambivalent legacy in Kuhn and rightly opted to favor his naturalism over his postwar scientistic internalism.

Giere's reading of Kuhn as initiating a naturalistic philosophical understanding of science brings out an important ambiguity in contemporary philosophical understanding of naturalism, however. I argued earlier in this paper and elaborate elsewhere that naturalism in the philosophy of science needs to be differentiated from naturalism as it is mostly understood elsewhere in philosophy. The most familiar forms of philosophical naturalism situate philosophical work within a scientific conception of the world, but they identify that conception in fairly traditional terms with a body of scientific knowledge or a general scientific methodology. Whereas a more traditional meta-philosophical conception of naturalism often problematizes normativity as only legitimate if "naturalized" with a scientific conception of the world as anormative, recent philosophy of science and its empirical fellow travelers in science studies take scientific understanding itself to answer to a broad range of irreducibly normative concerns (Rouse 2023). The best work in the philosophy of scientific practice increasingly recognizes the historical vicissitudes, disciplinary diversity, institutional embeddedness, material realization, normative complexity, and political engagement of scientific research as integral to how it painstakingly develops and extends conceptual grasp of the world we inhabit.[3] Kuhn himself also had no evident sympathy for more traditional, reductive versions of philosophical naturalism.

In retrospect, Giere's account of Kuhn as progenitor of a naturalized philosophy of science was situated in the midst of this divergence between naturalistic philosophy of science and more traditional meta-philosophical naturalisms. Giere's reading of Kuhn's naturalism situates Kuhn squarely in the midst of a naturalistic emphasis on the sciences as we know them in all of their historical complexity and diversity. He then went on to argue, however, that "if our naturalistic theory of science is not to be *merely* historical, we need a *theory* of theory choice" (1985, 347). In light of subsequent work on scientific practice, he would surely broaden that demand to include a theory of other aspects of scientific practice besides the choices of theories and models. At that time, he tentatively suggested that such a theory might plausibly appeal to

[3] Salient examples include Ankeny and Leonelli 2016; Barad 2007; Bechtel 2006; Cartwright 2019; Douglas 2009; Dupre 2012; Giere 2006; Hacking 2009; Kitcher 2001; Mitchell 2009; Nersessian 2008; Rouse 2015, Part II ;Solomon 2015; Winsberg 2018; Wylie 2002.

evolutionary biology, decision theory and the cognitive sciences more generally, or perhaps a historicized “hybrid strategy” that applied such general scientific theories to shorter, more localized periods in the history of science (1985, sections 5, 8-9). If one thinks about evolutionary theory or the cognitive sciences as they were mostly undertaken in the 1980's, however, the aspiration to understand the normativity of scientific practice in these terms would seem to move Giere’s conception of a naturalized philosophy of science instead closer to a more traditional meta-philosophical naturalism that aims to “naturalize” normativity. I think Giere was nevertheless not wrong to seek to reconcile these two divergent conceptions of philosophical naturalism. In more recent work, I have argued that subsequent developments in evolutionary theory enable a more thoroughgoing reconciliation of naturalistic philosophy of science with a more encompassing but non-reductive philosophical naturalism (Rouse 2015). In our efforts to reconcile these divergent naturalisms, however, Giere and I each move well beyond anything recognizably anticipated by Thomas Kuhn, and the assessment of those efforts must be reserved for another occasion.

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