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Recovering Thomas Kuhn

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Abstract The interpretive plasticity of Kuhn's philosophical work has been reinforced by readings informed by other philosophical, historiographic or sociological projects. This paper highlights several aspects of Kuhn's work that have been neglected by such readings. First, Kuhn's early contribution to several subsequent philosophical developments has been unduly neglected. Kuhn's postscript discussion of "exemplars" should be recognized as one of the earliest versions of a conception of theories as "mediating models." Kuhn's account of experimental practice has also been obscured by readings that assimilate his views to Quinean holism. Second, three distinctive Kuhnian themes have been insufficiently recognized. Kuhn's challenge to received philosophical views has been domesticated by reading him as offering an alternative conception of scientific knowledge. Kuhn is better understood as rejecting knowledge-centric accounts altogether, in favor of understanding the practice of research. Kuhn's conception of that activity, as conceptual "articulation," has accordingly also not been given its due. Finally, Kuhn's career-long insistence on the mutual accountability of philosophy of science and the philosophy of mind and language calls attention to the extent to which these fields have now drifted apart.

Keywords Kuhn · Models · Experiment · Research · Scientific practice

Thomas Kuhn's (1970) Structure of Scientific Revolutions is among the most widely read and cited academic books of the past half century. The book has also been quite protean, taking on markedly different roles and guises in juxtaposition to other philosophical or social theoretical work. If the book's longevity has been facilitated by its rhetorical independence from its scholarly contemporaries, its plasticity has also been reinforced by recurrent temptations to read it through other philosophical projects.

A brief recap of some of the book's most prominent associations and guises is instructive. Its beginnings were modest. For philosophers, Kuhn initially appeared as a relatively minor figure among more prominent early critics of logical empiricism: Hanson 1958; Feyerabend 1962; Toulmin 1961; Scriven 1962, and Polanyi 1958. For historians, Structure emerged from the rather different historiographic shadows cast by Alexandre Koyre and James B. Conant. The book's initial rise to philosophical prominence was as foil to various self-styled critics of dogmatism and subjectivity (Shapere 1964; Popper 1970; Lakatos 1970; Scheffler 1967), who often paired him with Feyerabend (1962, 1975) despite mutual protest. Apart from the paradigm concept's misleading appeal to social scientists seeking epistemic legitimation, the book's more constructive prominence began with its contribution to the brief heyday of post-Wittgensteinian philosophy of social science (Winch 1958; Taylor 1971, Wilson 1970, MacIntyre 1971, 1980; Toulmin 1972), and other challenges to scientism in the academy. Kuhn has most persistently appeared in retrospect, however, as the principal standard bearer for a "post-empiricism" in the philosophy of science whose doctrinal hallmarks are Quine's (1953) epistemological holism and Hanson's (1958) coinage of "theory-laden observation." This dominant motif in Kuhn's subsequent reception has sounded against other more briefly influential readings: as sympathetic interlocutor with Carnap's (1950) later views on linguistic

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frameworks, reluctant precursor to the Sociology of Scientific Knowledge (SSK), fellow traveler with a cognitive science of science, or end-of-ideology Cold Warrior in Fuller's (2000) revisionist reading of Kuhn alongside Merton 1973; Conant 1961; Price 1963, and Price 1965.

These divergent readings of Kuhn each highlight themes that are indeed present in his work, but none fits without visible seams and gaps. In what follows, I call attention to some themes in Kuhn that I think still deserve greater attention. They fall into two groups. First, in several respects Kuhn is an important precursor to subsequent philosophical work for which his contribution is less commonly recognized. Second, some important themes in Kuhn's work have been unduly neglected, and should be taken up more constructively.

One of the most currently influential philosophical conceptions of scientific theorizing emphasizes the mediating role of various kinds of model. Brief histories of this conception (e.g., in Teller 2001) often attribute its origin to Giere (1979, 1988) or Cartwright (1983), and its now-canonical presentation is in Morgan and Morrison (1999).[1] Yet one of the clearest initial formulations of the view, as a challenge to the centrality of laws and a recognition of the patchy development of theoretical understanding, is found in Kuhn's effort to work out a more precise conception of paradigms as "exemplars" in his (1970) postscript:

> It is not quite the case that logical and mathematical manipulation are applied directly to $f = ma$. That expression proves on examination to be a law-sketch or law-schema. As the student or the practicing scientist moves from one problem situation to the next, the symbolic generalization to which such manipulations apply changes. For the case of free fall, $f = ma$ becomes $mg = md^2s/dt^2$; for the simple pendulum it is transformed to $mg\ \sin\theta = -ml\ d^2\theta/dt^2$; for a pair of interacting harmonic oscillators it becomes two equations, the first of which may be written $m_1d^2s_1/dt^2 + k_1s_1 = k_2(s_2 - s_1) + d$); and for more complex situations, such as the gyroscope, it takes still other forms, the family resemblance of which to $f = ma$ is still harder to discover. (Kuhn 1970, 188–189)

Moreover, this broader conception of mediating models has often been mistakenly conflated with the "semantic" view of scientific theories, drawn from model theoretic structures in logic and set theory (Suppes 1967; Suppe 1977; van Fraassen 1980). Even though the semantic view did initially guide some proponents of models-as-mediators, conflating the two conceptions is misleading since the mediating-model approach recognizes an open-ended variety of models (tables, diagrams, physical models, computer simulations, and much more), and emphasizes their concrete uses in scientific practice rather than their abstract structure. Yet the efforts to disentangle these two senses of theories as models were instructively pre-figured in discussions of Wolfgang Stegmüller's (1976) model-theoretic interpretation of Kuhnian paradigms.

My point in highlighting Kuhn's neglected place in the emergence of model-mediated conceptions of theorizing is not to diminish the originality or importance of subsequent work, which has extensively developed and refined the initial idea of theoretical content as mediated by models. We should still recognize this theme as a neglected aspect of Kuhn's own view that is worthy of better understanding, and raise the possibility that reading Kuhn in light of current work on theoretical modeling and vice versa might be mutually illuminating.

The now-familiar assimilation of Kuhn's views to Quinean epistemological holism and observational theory-ladenness has also, I think, blocked recognition of the extent or significance of Kuhn's concern with experimental practice. Hacking's (1983) influential effort to make experiment more central to philosophy of science repeatedly cites Kuhn as exemplifying the theory-dominant approach characteristic of then-current philosophy of science. Kuhn's principal examples of paradigms and paradigm-change are indeed primarily focused on theoretical development and theory change. Yet several important exceptions stand out. Kuhn explicitly and prominently cited the discovery of X-rays as a revolutionary change in instrumental and experimental paradigms whose theoretical import was minimal, at least initially. He followed up with the insistence that "instrumental as well as theoretical expectations have often played a decisive role in scientific development" (1970, 59). The ensuing example of Priestley's and Lavoisier's acceptance of a standard test for the "goodness of air" cannot plausibly be interpreted as exemplifying the theory-ladenness of observation or experiment. On the contrary, it indicates Kuhn's recognition that theoretical concepts and claims can also be "experiment-laden." Nor are these examples of the conceptual-articulative role of experimentation isolated. In an important paper contemporary with Structure, Kuhn emphasized the role of instrumental and experimental capacities for measurement as integral to the determination of what theories say. Kuhn does insist, as received interpretations foreground, that "the route from theory or law to measurement can almost never be traveled backward," and that "often scientists cannot get numbers that compare well with theory until they know what numbers they should be making nature yield" (1977, 197, 193). Yet Kuhn also insists on an integral role for experimental practice in the articulation of theoretical content, and not just a secondary role in either "testing" a theoretical conception, or working out how

[1] None of these texts cites or indexes Kuhn.

to arrive at results already determined on different grounds. Thus, he emphasizes that

> tables of numbers drawn from theory and experiments … define 'reasonable agreement'. By studying them, the reader learns what can be expected of the theory. An acquaintance with the tables is part of an acquaintance with the theory itself. Without the tables, the theory would be essentially incomplete (1977, 185–186).

Moreover, he explicitly repudiates claims that "theory must always lead experiment" or that "[experiment] has a decidedly secondary role":

> If we had been discussing the qualitative experimentation that dominates the earlier developmental stages of a physical science and that continues to play a role later on, the balance [between theory and experiment] would be quite different. … We would certainly find vastly more symmetry and continuity in the ongoing dialogue between the two (1977, 201).

Kuhn remained far from Hacking's (1983) insistence that philosophy must acknowledge how "experiment has a life of its own," but he is also far from the usual portrait that emphasizes the Duhem-Quine thesis, or a neglect of experimental work in favor of observation, taken as theory-laden. We still await, I think, an adequate account of the place of experiment and instrumentation in Kuhn's philosophy of science, and that account will not support facile characterizations of Kuhn as a post-empiricist advocate of theoretical holism.

Despite these early contributions to understanding theoretical modeling and the diverse philosophical roles of experiment, I look to three different themes for Kuhn's primary import for philosophy of science a half century later. These three themes are a shift of philosophical attention to science away from epistemology, a correlated shift of attention toward the dynamics of conceptual articulation in the sciences, and a resolute insistence upon the integral engagement of philosophical reflection on science with philosophical work on mind, language, and metaphysics.

I have already explored the first theme in some detail in previous retrospective work on Kuhn (Rouse 2003; 1998), so I will be brief. Kuhn's talk of scientific revolutions has often been extended reflexively to characterize the shift away from logical empiricist philosophy of science to which the book contributed. Even if the break from logical empiricism was a philosophical "revolution," I insist that a Kuhnian revolution in the philosophy of science has yet to occur. Kuhn introduced Structure as a repudiation of conceptions of science that are best exemplified by textbooks. His point has been most widely understood as advocating a different conception of scientific knowledge than the cumulative ones familiarly drawn from reading textbooks, of which the logical empiricist "image of science by which we are now possessed" (Kuhn 1970, p. 1) is a more sophisticated version. The result would be a historically and cross-disciplinarily discontinuous conception of knowledge as "problem solutions" framed by occasionally shifting paradigms.

Kuhn instead posed a more radical challenge to any epistemological orientation within philosophy of science. He was offering not an alternative conception of scientific knowledge, but a philosophical conception of science that is not primarily concerned with knowledge products. Emphasizing the practice of research did not provide a different standpoint from which to describe and assess scientific knowledge. Kuhn presented research practice as the proper target of philosophical explication, and argued that the epistemological terms that have typically guided such explication lead philosophical inquiry astray. The book was written as a quest for "a viable alternate to the traditional epistemological paradigm" which was "a philosophical paradigm initiated by Descartes and developed at the same time as Newtonian dynamics… [The] example of Newtonian dynamics indicates [that] even the most striking past success provides no guarantee that crisis can be indefinitely postponed. Today research in [many disciplines] all converge to suggest that the traditional paradigm is somehow askew" (1970, 121).

Kuhn's intent to challenge any epistemological conception of science can be seen in some of the book's most prominent claims. Thinking of Kuhnian paradigms as an alternative conception of knowledge runs up against the problem that their function circumvents any effort to say what that knowledge is: "Scientists can agree … in their identification of a paradigm without agreeing on, or even attempting to produce, a full interpretation or rationalization of it" (1970, 44). Kuhn is often enlisted among those who extended philosophical concern with scientific knowledge beyond justification to include discovery, but in fact Kuhn adamantly rejected both concepts as appropriate to the scientific enterprise. The primary point of his extended discussion of the emergence of oxygen as an intelligible constituent of the world was to show that "clearly we need a new vocabulary and concepts for analyzing events like the discovery of oxygen" (1970, 55). Kuhn's insistence that normal science neither tests nor justifies a paradigm is better known, but has also too often been similarly domesticated. His point was not that scientists accept paradigms dogmatically, without justification, but that paradigms are not appropriately seen even as candidates for justification. Science seeks not to justify its claims and practices but to move beyond them, through the process of "paradigm articulation," and ultimately through the replacement of paradigms that have ceased to sustain

that process effectively (in this respect, he was addressing the functional role of revolutions rather than the aims or intentions of those scientists whose work brings them about). Kuhn was neither a skeptic nor a relativist nor a subjectivist nor a dogmatist nor a social constructivist about scientific knowledge; all of these stances presuppose the epistemological paradigm that Kuhn sought to supplant with a philosophy of scientific research. Philosophy and sociology of science have indeed moved closer to Kuhn's project in recent decades, through a growing focus on scientific practice. Yet even here, arguably, we are still churning in the wake of Kuhn's aspirations rather than bringing them to fruition. The new society that explicitly attends to such work still identifies itself as a Society for the Philosophy of Science in Practice rather than of scientific practice. For too many philosophers, "practice" still indicates only a different context for posing and answering familiar questions about scientific knowledge, rather than different questions and different aims in asking them.

My second theme for how to look forward from rather than back to Kuhn is a direct correlate of the first. If science does not aim at knowledge, what does it aspire to? Kuhn argued that normal science undertook three primary functions: determining significant facts, comparing the paradigm with nature, and "paradigm articulation." Yet the first two functions are clearly ancillary to the third. The first provides more specific, often quantitative articulation to concepts already integral to research practice, and the second is not a test that a paradigm could fail, but a diagnostic test that identifies the locus and target for subsequent articulation of the paradigm. Paradigm articulation is the working out of conceptual understanding, by showing how those concepts apply in detail to situations in the world. Contrary to the familiar image of Kuhn as a theoretical holist, such articulation involves a simultaneous adjustment of concepts and their application, instruments and their use, and experimental phenomena and the procedures that introduce and refine them. Paradigms in the primary sense involve "law, theory, application and instrumentation together" (1970, 10), and paradigm articulation is the mutual adjustment of all of them in order to extend and refine the sciences' conceptual grip on significant aspects of the world. Paradigm change is then a conceptual and practical reconfiguration of a scientific domain, as a "work-world" in which the primary scientific task is the further development of a conceptual grip.[2]

In a forthcoming book (Rouse forthcoming), I develop this Kuhnian theme more explicitly as part of a revisionist-naturalistic re-conception of "the Scientific Image" (Sellars 2007, ch. 14). For Sellars, the sciences produce a comprehensive, composite, theoretically articulated conception of the world, a body of theoretical knowledge that takes explanatory priority over empirical-correlationist and practical-normative conceptions. I locate scientific aspirations and achievements somewhat differently: the sciences produce not a unified and comprehensive "image" within the Sellarsian "space of reasons," but an ongoing refinement and reconfiguration of the space of reasons itself.[3] The sciences do not produce a consensus of beliefs, but an open-ended conceptualization within which beliefs, practices, and explorations are formulated, contested, refined and assessed. There are no bounds to what can be understood scientifically, but the sciences focus on some issues and aspects of the world rather than others, and scientific understanding points beyond and outruns any determinate formulation of belief. The sciences configure the world as a research field rather than as object of belief.

As but one of many ways in which such a broadly Kuhnian reconception of "the Scientific Image" changes philosophical understanding, consider its implications for understanding recent political challenges to evolutionary biology and climate science within the United States. Advocates of "creation science" or "intelligent design" and skeptics about anthropogenic climate change often insist that evolution is "just a theory" and global climate projections are "just a model" rather than well-justified knowledge claims. They also highlight scientific dissensus on specific details, and sometimes speak in pseudo-Kuhnian terms of alternative paradigms that are unjustly excluded from normal scientific consensus, in order to maintain the illusion of ongoing scientific controversy.[4] Defenders of scientific work in these areas too often share with their opponents an image of science as striving for epistemic consensus, and when challenged to identify the consensus beliefs, often must oscillate uncomfortably between vague common denominators of scientific belief, and more detailed but contestable formulations. Yet what any serious scientific understanding of climate dynamics or evolution shares is not specific beliefs, but a conceptual and practical field of concepts, strategies, issues and

[2] Hence, the infamous quotation about Lavoisier, for which commentators have usually given insufficient emphasis or comprehension to the verb: "after discovering oxygen, Lavoisier *worked* in a different world" (1970, 118, my emphasis).

[3] The classic introduction of this phrase was in Empiricism and the Philosophy of Mind: "in characterizing an episode or a state as one that of knowing, we are not giving an empirical description of that episode or state; we are placing it in the logical space of reasons, of justifying and being able to justify what one says" (1997, 76).

[4] Oreskes and Conway 2010 show how the strategic political uses of manufactured "controversy" have been integral to conservative resistance to environmental and health regulations in the United States for the past four decades. Similar strategies have also informed the post-1960 s renewal of efforts to restrict the teaching of evolutionary biology in American public schools.

research directions.[5] Here, of course, the contrast between evolutionary biology or contemporary climate research and their critics becomes especially clear: the critics have no research program, and no conceptually open framing of how to articulate an understanding of these domains as "problems" or issues that can be coherently posed and resolved. In that sense, it is the utter absence of anything like an alternative Kuhnian paradigm that most clearly exposes the intellectual bankruptcy of these political challenges to scientific understanding.

My third and final theme highlights Kuhn's commitment to coupling his detailed work within the philosophy of science with exploration of its implications for philosophical understanding of language, mind, and knowledge. When Kuhn began his philosophical career, leading philosophers of science such as Carnap, Hempel or Sellars were also developing a broader philosophical vision, and their conceptions of science were influential throughout philosophy. As philosophy of science has become more specialized, both in its disciplinary orientation within the sciences and in its accountability to contemporary scientific research, the field has become less central to other philosophical concerns, ironically so given the concurrent shift from empiricism to naturalism as the predominant philosophical stance toward the sciences. A resistance to any such marginalization of philosophy of science animated Kuhn's work throughout his career, not only in Structure (where chapters 5 and 10 highlighted the possible broader philosophical import of the book for epistemology and philosophy of language) but also in his subsequent efforts to rethink and develop its concerns in response to subsequent philosophical work on language, cognition and conceptualization (Kuhn 2000). I share the widespread recognition that Kuhn's efforts in this direction were not successful. John Haugeland's (1998) efforts to build upon Kuhnian themes in philosophy of mind and metaphysics are much more promising, albeit not yet sufficiently recognized or addressed. Yet my point here concerns not Kuhn's achievement but his aspiration. For Kuhn, the sciences exemplify the most sophisticated forms of cognition and conceptualization, and philosophical work on both topics needs to be mutually accountable.

I conclude with the reminder that, a generation ago, logical empiricism's philosophical commitments and their limitations were thought to be all too well understood. Subsequent scholarship has challenged that calcified reading of the Vienna Circle and their followers, and initiated a renewed, productive philosophical engagement with their work. The place of The Structure of Scientific Revolutions within our received disciplinary histories and our current philosophical imaginary now seems comparably rigid and limiting. Fifty years later, perhaps the time has now come for a comparably fresh renewal of philosophical engagement with Kuhn's philosophical writings on science.

[5] The problem with lowest-common-denominator formulations, e.g., that species evolve in part by natural selection over a multi-billion year history of the earth or that there has been significant and growing anthropogenic climate change since the beginnings of the Industrial Revolution, is not that they are false, but that they understate the sophisticated and detailed articulation of scientific understanding in these domains, even though more specific formulations would yield dissent on details within a common space of scientific reasoning.

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