

## KUHN AND SCIENTIFIC PRACTICES<sup>1</sup>

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Thomas Kuhn's philosophical work has helped initiate or reinvigorate many of the central themes in the philosophy of science over the past three decades. His claim that appropriate attention to history could decisively transform familiar philosophical of science views was of course prophetic. Kuhn's discussion of meaning change, incommensurability, and the interdependence of theory and observation occasioned both the rise of rationalist metamethodologies and the renewal of scientific realism. Constructivist sociologies of science and various social epistemologies also trace a lineage to Kuhn's discussion of normal science and the role of scientific communities in sustaining research traditions. The striking growth of philosophies of the special sciences was perhaps foreshadowed as well by Kuhn's reflections on the "ramshackle structure" of normal science in contrast to familiar presumptions of the theoretical unity of the sciences.<sup>2</sup> Yet in my view, one of the most philosophically far-reaching themes of Kuhn's work has yet to receive adequate recognition. Even now, thirty-five years after the appearance of The Structure of Scientific Revolutions, the significance of Kuhn's shift of philosophical focus from scientific knowledge to scientific practices has yet to be fully assimilated.<sup>3</sup>

The difficulty of recognizing Kuhn's turn to practices does not stem entirely from epistemological tunnel vision, however. Kuhn's turn to scientific practices pervades almost every aspect of his work, but nowhere does he reflect upon this theme explicitly. Any discussion of Kuhn on scientific practices must therefore begin by showing how Kuhn's own familiar writings are reconfigured by recognition of his central concern with scientific practices. Since I have discussed these interpretive issues extensively elsewhere, I shall only take them up in the opening section.<sup>4</sup> In the remainder of the paper, I shall address two further questions. The first is prompted by the proliferation of practice talk in philosophy and social theory since the time of The Structure of Scientific Revolutions. In the light of this subsequent work, we can now ask what is distinctive about Kuhn's discussion of scientific research as a kind of practice. The second question stems from Kuhn's return late in his career to the issues raised by incommensurability and scientific revolutions, especially from his reflections upon changes in scientific "lexicons" as a way to construe the incommensurability of competing research traditions. Having started by reading Kuhn's work in terms of a shift to a philosophy of scientific practice, how should we then interpret and assess Kuhn's late work on changes in lexical structure?

### I

Most philosophers of science, before and after Kuhn, have been primarily concerned with the structure and justification of scientific knowledge, and this concern has shaped philosophical interpretations of and responses to Kuhn. I shall not here

rehearse these familiar readings of Kuhn as a philosopher who emphasizes the role of theoretical, instrumental, and evaluative presuppositions in reconfiguring observed evidence and preventing the neutral assessment of competing paradigms. Instead, I shall highlight some distinctive features of Kuhn's work when it is reinterpreted as a philosophy of scientific practices.

Kuhn began by discussing "normal science," a distinctive way of organizing the activity of scientific research. Normal science is the practice of scientists who know their way around in a shared research field. They have a practical grasp of the objects they are dealing with, and the conceptual and material tools at hand to work with those objects. They understand what is a significant question in their field, and generally agree about which questions have been successfully answered. These prior achievements also provide a sense of what would count as an adequate answer to presently unsolved puzzles, and indeed, the practical mastery of these achievements provides the basis for their subsequent work.

Kuhn describes these paradigmatic achievements as "accepted examples of actual scientific practice which include law, theory, application, and instrumentation together."<sup>5</sup> Those familiar with the logical empiricist tradition might say that Kuhn thereby takes "bridge principles" or "correspondence rules" to be more fundamental than the theories they supposedly interpret.<sup>6</sup> A crucial part of Kuhn's point, however, is to distinguish paradigms from explicit principles or rules. Grasping a paradigm is not like holding a belief or following a rule, but is instead acquiring a complex but flexible set of skills. Primarily, these are skills of recognition: the ability to identify successful solutions to problems, and to understand an outstanding problem as relevantly like a familiar solved problem. Such recognition, however, presupposes more basic skills of doing mathematics, applying concepts, building and manipulating apparatus, and so forth. Even when talking about theoretical understanding, Kuhn emphasizes the skillful use of concepts, models, and symbolic generalizations as interconnected tools, rather than the beliefs or judgments that may result from their application. Scientific understanding is dynamic and practical.

Paradigms belong to (and to some extent demarcate) the scientific communities that use them. Kuhn insisted, however, that scientific communities are not characterized by consensus. No set of beliefs is sufficient to insure membership within a scientific community, which is best understood as a community of practitioners (although Kuhn did not quite say so, he clearly understood a scientific community to include only those actively doing research in the field, not the wider group whose training or study enables them to understand it). What matters is the ability of community members to recognize one another's work as satisfying norms of competent scientific practice, measured against the paradigmatic

achievements that define the research field. Such mutual recognition is quite compatible with divergent beliefs about how the field is structured: Kuhn maintains that "scientists can agree in their identification of a paradigm without agreeing on, or even attempting to produce, a full interpretation or rationalization of it."<sup>7</sup> Similarly, they can agree in identifying appropriate work from a paradigm, without requiring such an underlying interpretive consensus. Misunderstanding might have been avoided had Kuhn ascribed paradigms not to communities (which might be thought to share a creed), but to scientific sub-cultures sharing cultural forms, activities, and aspirations. This difference was partly obscured in Structure by Kuhn's appeal to Gestalt psychology, which suggested similarities in the mental lives of the individual members of scientific communities.<sup>8</sup> Such psychological features contrast, however, with Kuhn's primary identification of communities in terms of publicly accessible tools, practices, and norms.

A crucial but rarely noticed feature of normal scientific practice on Kuhn's account is its reflexivity. The focus of normal scientific research is "paradigm articulation," that is, the specification, extension, and disambiguation of the exemplary achievements of the field, and the conceptual and instrumental tools that derive from them.<sup>9</sup> Scientists explore and disclose the world by attending to and working out their own familiar but not fully articulated practical and conceptual grasp of that world. Kuhn's account of normal science thus appropriated Quine's rejection of the analytic/synthetic distinction: one learns what one's concepts and theories mean by learning what the world is like, and vice versa.<sup>10</sup>

The reflexive focus of scientific practice upon the articulation of its own concepts and tools throws a different light upon Kuhn's notorious acknowledgement that the concept of truth had no significant place within his account of science.<sup>11</sup> Kuhn did not thereby entertain skeptical doubts about whether the sciences accurately disclose the world, as his subsequent protests amply reminded us. Kuhn's view instead proceeded in complete confidence that those scientific disciplines that have successfully attained to normal science are "in the truth," i.e., genuinely disclosive of the world. The crucial remaining question, indeed the question that continually animates normal scientific practice itself, is what the truths are that that practice and its accomplishments uncover. Scientific practices thus articulate the meaning of their own concepts and apparatus by using them to disclose the world more extensively, consistently, or precisely.

Recognizing the priority Kuhn accorded to meaning over truth clarifies a great deal about his discussion of anomalies and crises. Kuhn's insistence that scientists never test theories, that outstanding puzzles or anomalies are not counterinstances or

refutations, and that normal science often proceeds in the face of widespread anomaly, does not attribute to normal science an indifference to truth or a dogmatic adherence to received views. For Kuhn, the only difference between a puzzle and an anomaly is in scientists' practical orientation. Puzzles are discrepancies or lacunae in scientists' practical grasp of their field which they confidently anticipate resolving by constructively extending their present conceptual and instrumental resources. Such discrepancies or lacunae become anomalous when that confidence wanes. Anomalies are not yet counterinstances, however, because what initially falters is not the acceptance of a paradigm but the interpretation of the anomaly: when a fact or situation no longer makes sense in familiar terms, that fact "is not quite a scientific fact at all."<sup>12</sup> Its meaning and significance, including to some extent its status as a fact, are open questions. Crisis arises at the point when anomaly cannot be localized, when scientists' sense that they no longer quite know how to go on becomes more general.<sup>13</sup> Crisis is debilitating precisely because it cannot be localized as the falsification of some beliefs; it is instead the incoherence of research practice. The alternative to normal scientific practice being "in the truth" is not error, but confusion.

Scientific revolutions are first and foremost reconfigurations of scientific research. After a revolution, scientists work in a different world. Kuhn did not thereby claim that new concepts change what happens in the world (except to the limited extent that subsequent experimental work produces novel events). His claim was that revolutions reorganize the practical field within which the world is manifest in scientific research. The consequences of such practical/conceptual reorganization are far-reaching, however, because it is the world thus practically configured that the sciences articulate. Scientific research provides not a view from nowhere, but an engagement from a particular way of approaching and dealing with the world. Only in the light of changed practices does it even make sense to ask whether and how scientists' beliefs about the world have changed. Indeed, as we shall see in the final section, an upshot of Kuhn's discussions of incommensurability is that the latter question may not be straightforwardly answerable.

Kuhn might well have been ambivalent about the blurring of the distinction between normal science and revolution that is one consequence of reading him as a practice theorist. The distinction blurs in part for reasons he acknowledged: scientific communities are located within larger communities, so that a revolutionary transformation of the practices of smaller community may be more readily assimilable within a larger group whose everyday practical commitments are not fundamentally challenged by the new developments.<sup>14</sup> But it blurs for a deeper reason as well: different scientists successfully working within the same practical field need not share an interpretation of that field, and a

particular development that is a wrenching reorganization of the field for one researcher may seem more like paradigm articulation to another. The explicit interpretation of scientific practices is strongly underdetermined by their successful development as a normal scientific tradition.

The final point to make about the interpretation of Kuhn's work as a theory of scientific practice concerns his account of progress across scientific revolutions. Many philosophers have been troubled by Kuhn's insistence that there can be no measure of scientific progress toward some epistemic telos. Yet Kuhn's own account of scientific progress, as progress from a beginning point rather than progress toward some goal, has too often been discounted or ignored. Perhaps Kuhn's view has been overlooked precisely because it is so thoroughly practice-oriented. As a system of beliefs and values, phlogiston chemistry might have consistently provided its dedicated adherents with a standard according to which the tradition stemming from Lavoisier could be found wanting for a long time. Phlogiston chemistry decisively failed, however, as a practical tool for further exploring and coping with the proliferation of "airs" that emerged within its own research agenda.<sup>15</sup> For the working scientist, an inability to proceed with research in a coherent and informative way must in the end be an insuperable objection to any theoretical program. Scientific revolutions mark progress for Kuhn because they surmount such practical objections.

## II

At one time, making scientific practices the principal focus of interpretation was sufficiently unusual that to make that shift at all would sharply distinguish a philosophical study from others. With practice talk now rampant within science studies, that is no longer enough; we need to ask what, if anything, is distinctive in Kuhn's approach to scientific practices. Although there are now many uses of the term 'practice' in science studies, perhaps the single most widespread and influential use has been to proclaim a considerable degree of autonomy of experimental, instrumental, and other material practices from theoretical interpretation and guidance.<sup>16</sup> In this context, Kuhn's influence is ambivalent. On the one hand, his demarcation of a second scientific revolution in the 18th-19th Century from the earlier canonical Scientific Revolution is an important precursor to the turn to experimental practices. Kuhn's argument more sharply distinguishes the "Baconian sciences" of heat, electricity, magnetism and chemistry, which "owed their status as sciences to the insistence upon experimentation,"<sup>17</sup> from the classical sciences of astronomy, optics, and mechanics. Yet Kuhn's work is also a prominent target for criticism from those who would insist upon the relative autonomy of experiment from theoretical determination.

A brief glance at Kuhn's classic paper on "The Function of

Measurement in Modern Physical Science," the very paper that first noted the second scientific revolution, strongly suggests Kuhn's resistance to claims for the autonomy of experimental and instrumental practices. Kuhn noted that,

Quantitative facts ... must be fought for and with, and in this fight the theory to which they are to be compared proves the most potent weapon. Often scientists cannot get numbers that compare well with theory until they know what numbers they should be making nature yield.<sup>18</sup>

Because of the role of theory in guiding measurement practice, he argued,

the route from theory or law to measurement can almost never be travelled backward. Numbers gathered without some knowledge of the regularity to be expected almost ... certainly remain just numbers.<sup>19</sup>

Kuhn's examples were marshalled to "show how large an amount of theory is needed before the results of measurement can be expected to make sense, ... [at which point] the law is very likely to have been guessed without measurement."<sup>20</sup> If experimental practice sometimes seems to pose its own distinctive problems and develop in ways determined from within its own sphere of skills and practices, this may still show the predominant role accordable to theory: "theoretical genius in the natural sciences leaps ahead of the facts, [to the very border of existing instrumentation,] leaving the rather different talent of the experimentalist and instrumentalist to catch up."<sup>21</sup> Similar sentiments were expressed at many points in The Structure of Scientific Revolutions, and they seem to lead to the conclusion that, for Kuhn, "theories are, even more than laboratory instruments, the essential tools of the scientist's trade."<sup>22</sup>

Despite the ease of finding such passages throughout Kuhn's work extolling the primacy of theory over experiment and empirical measurement, I shall argue that it is a mistake to understand Kuhn's account of scientific practices in these terms. The reason it is a mistake is that such an interpretation too readily assumes an understanding of scientific theory which Kuhn emphatically rejects. While Kuhn certainly does not defend the autonomy of experimental practice, he is also no defender of the autonomy and priority of scientific theories. For Kuhn, theories are not internally closed and coherent representations of the world, which can then be confirmed or challenged by empirical test. Theories only acquire determinate content in their articulation of concrete empirical situations, such that apart from their detailed connection to experimental work, theories are almost vacuous. Kuhn is better understood as offering an account of the sciences as conceptual practices, where "concepts" are not just mental constructions, but articulations of concrete, material engagements with the world, which include experimentation.

A closer look at the context of the passages I quoted from Kuhn's classic paper on measurement shows that Kuhn's comments there apply primarily to precise quantitative measurement. An important part of Kuhn's point is that such measurement is a relatively late development, not merely in relation to the articulation of theoretically derived expectations, but also within the history of experimentation in any particular domain. Kuhn concluded his discussion of theory-guided measurement by noting that,

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.<sup>23</sup>

But what is "qualitative experimentation," and what roles does it play in the development of sciences? Kuhn gives an illuminating example in a passing comment about the history of thermometry.

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?<sup>24</sup>

Thermometers are hardly unique in this respect. Any instrument that opens a new domain of investigation, or makes a familiar domain accessible in a strikingly new way, needs to be assimilated within one's prior grasp of the world. The task is less developing theories than it is making a domain accessible to theoretical reflection. This role for qualitative experimentation is comparable to what Hacking has labeled as the "creation of phenomena"; Kuhn's point is that a crucial task undertaken in such experimental work is conceptual articulation.<sup>25</sup> An important part of what the initial experimental work with thermometers accomplished was to begin to disentangle multiple conceptual dimensions of the pretheoretical notion of "degree of heat."

Even when conceptual development has progressed to the point at which precise quantitative measurement makes sense, experimental work does not lose its articulative conceptual role. What a theory says about the world depends in significant part upon what it can say, given the experimental and instrumental practices that connect words and things. Naively, we might suppose that experiment tests theory, and indicates whether it is true according to whether theoretical prediction and experimental outcome agree. But theoretical prediction from idealized and reasonably general models could in principle be articulated mathematically with a degree of precision that would lose touch with the things modeled. What constitutes "agreement" depends upon what the theory actually says about the world, and that is not fully specified by theoretical talk alone, apart from its material circumstances of application.

Kuhn concludes that the canonical outcomes of experimental measurement

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.<sup>26</sup>

We can now see more clearly why Kuhn's developed view abjured detailed discussion of theories in favor of paradigms or "exemplars," which "include law, theory, application, and instrumentation together." The Received View of theories carefully distinguished the sentences composing the theory proper from the correspondence rules that connected them to observed data. But for Kuhn, there is no theory proper apart from applications to concrete empirical situations; better put, the open-ended pattern exemplified by its canonical applications is the theory.<sup>27</sup> Theories for Kuhn are not already-developed semantic structures with a definite content, but ongoing practices of articulating concepts in relation to one another in specific contexts. What the theory says about the world is not yet fully determinate, but only emerges over time in the concrete uses of its concepts, uses which are embedded as much in material practices as in talk and calculation.

Understood in this way, Kuhn's concept of 'exemplars' is the ancestor of Nancy Cartwright's and Ronald Giere's conception of theories as families of models rather than the superficially similar semantic conception of theories.<sup>28</sup> The semantic conception identifies theories with formal structures expressible in terms of model theory; to say that a theory is true (or empirically adequate) is just to say that the actual world (or its appearances) conforms to one of its many possible models. Cartwright's and Giere's (and Kuhn's) views instead incorporate the actual practices of interpretively modeling the world within the theory itself. The theory is not a formal structure, but a family of concretely modeled situations. In Cartwright's sense, then, Kuhn is not a "fundamentalist" for whom theories are universal generalizations or model theoretic structures of unlimited scope.<sup>29</sup> The domain of a theory (paradigm) is not specified in advance by its syntax or semantics, but is only disclosed through the ongoing conceptual practice of paradigm articulation (a practice that is simultaneously theoretical and experimental). To use Cartwright's example, the domain of mechanics on this view is not of universal scope: there are "forces" only where a force function can be developed and applied within the limits of "reasonable agreement."

Yet if Kuhn is not a fundamentalist, he is also not an instrumentalist, i.e., not one for whom the content of theories reduces to its actual domain of canonically successful empirical application. Theories are tools for disclosing the world through their own conceptual articulation in the reflexive practice of research. What a live scientific theory says is always more than



can actually be articulated, even if its scope is not already universal. While this point is implicit in Kuhn's primary identification of theories with what is employed in normal science rather than what is represented in texts, it is perhaps clearest in Kuhn's reflections upon thought experiments.

In some respects, Kuhn took thought experiments to be comparable to qualitative experimentation in their role in conceptual development: they articulate concepts by concretely working out their application to novel situations or manifestations. Thought experiments, however, presuppose the prior articulation of the relevant concepts within some domain, for as Kuhn notes,

one condition of verisimilitude [constraining thought experiments is that] the imagined situation must be one to which the scientist can apply his concepts in the way he has normally employed them before.<sup>30</sup>

Typically, the role of thought experiments is to disentangle multiple dimensions of a set of concepts, which are compatible within the situations to which they have been familiarly applied, but which come into tension or even contradiction in the imaginatively extended situation. Kuhn was inclined to deny, however, that a successful thought experiment reveals that the theoretical concepts it uses are self-contradictory: they "run the risk of contradiction" without actually being in contradiction.<sup>31</sup> The reason for his hesitation is that Kuhn took these concepts to be consistent within their familiar domain of application, i.e., as an interpretation of the practical setting within which they were heretofore articulated. What the concepts say, and what they are about, follow from their actual deployment in a range of concrete situations.

Yet thought experiments can nevertheless reveal a conceptual incoherence, in part by introducing it. In explicitly extending concepts beyond their range of familiar application, successful thought experiments simultaneously reveal something about the world in which their concepts have applied, and articulate what those concepts can say. For although concepts acquire sense within the contexts of their concrete application, their sense is not confined to that context. They are tools for exploring a world that is only intelligible through their articulation. In retrospect, we might say these concepts were used in ways that implicitly presupposed substantive assumptions about the world, assumptions that were later revealed to be false. Yet Kuhn thought that to be an anachronistic misinterpretation. That those concepts had a range of application extending beyond their familiar instances was an implication of their open-ended use in scientific practice. What they said beyond that range, however, and how their extension affected their familiar use, were not yet determinate.

Kuhn is often regarded as a theorist of meaning change. Yet

that phrase suggests that there was a determinate meaning to a set of concepts at one time, and a different determinate sense at another. Kuhn's account of conceptual practices instead introduced a rather more fluid understanding of the "meaning" of scientific concepts. That conceptual understanding involves a more dynamic grasp of a practical field of words and things is evident in Kuhn's late reflections upon historiography. Kuhn argued that historical narratives of scientific practices must begin with a quasi-ethnographic scene-setting, an explicit interpretation of how a group of scientists understood and dealt with the world within some particular time period. Yet this historical "ethnography" is to some extent an artifact of the historian's framing, an extraction of determinate structure from an ongoing, transformative encounter with the world. Kuhn's return, later in his life, to the questions raised by the emergence of incommensurable conceptual practices, can be best understood as a reflection upon the relation between structure and dynamics of the conceptual life of the sciences.

### III

Late in his professional life, Kuhn began to rework his earlier discussions of conceptual change, incommensurability, and world changes.<sup>32</sup> Influenced but not fully convinced by the development of causal theories of reference and possible worlds semantics, he sought to articulate his original insights about the historical interpretation of scientific practices in ways that would not engender the problems raised by the notion of changes in meaning. Kuhn's unrepentance was clear in his insistence that,

The heavens of the Greeks were irreducibly different from ours, ... [comparable to the differences] between the social practices of different cultures. In both cases, the difference is rooted in conceptual vocabulary. In neither can it be bridged by description in a brute data, behavioral vocabulary. [Thus], any attempt to describe one set of practices in the conceptual vocabulary, the meaning system, used to express the other, can only do violence.<sup>33</sup>

Kuhn's strategy was to consider the structure of scientific "lexicons," i.e., the systematic relations between kinds of objects and properties ascribable within the discursive practices of historically situated scientific communities or sub-cultures. Historians aiming to understand those practices must either acquire effective use of this lexicon themselves, or must learn how to translate it into terms that they already understand.<sup>34</sup> Kuhn's argument concluded that such translation is not possible without unacceptable loss or compromise, and hence that there is no substitute for the historian's practical appropriation of the lexicon.

'Lexicons' in Kuhn's sense are structured vocabularies acquired and used in specific settings. They are thus not merely verbal, but are rather an inextricable configuration of words and

things; mastering the lexicon is acquiring the skill to recognize its appropriate application in various settings, and to encounter the world in those terms. The intelligibility of the world through the use of a lexicon is less a presupposition than a practical commitment. The sense in which acquisition of a lexicon confers or constitutes a practical grasp of situations appropriate to its use perhaps accounts for Kuhn's temptation to speak of it as providing a set of accessible possible worlds.<sup>35</sup> The crucial point is that one learns one's way around a practically configured situation and acquires a structured vocabulary simultaneously; or rather, those are different descriptions of the same acquired capacity. Of course, one can share a lexicon, using words in commensurable ways, without having acquired it in quite the same way, and without agreeing about what to say in its terms:

Two people could share a concept without sharing a single belief about the feature or features of the objects or situations to which it applied.<sup>36</sup>

Kuhn thus continues to block the inference from shared practices to shared beliefs or experiences.

Donald Davidson, among others, has objected that Kuhn's own achievements as an historian belie his insistence that past scientific understanding is incommensurable with ours.<sup>37</sup> We can only reveal the strangeness of past practice by making it nevertheless accessible. Kuhn's eventual response was to distinguish understanding a lexicon from being able to translate it without serious loss into familiar concepts. The historian can understand another lexicon by acquiring it, painstakingly recognizing and defamiliarizing the contexts in which it was employed so as to reconfigure the world practically. Part of what one thereby learns is supposedly the untranslatability of the sentences one nevertheless can correctly formulate and apply.

Kuhn took translatability to be problematic for two reasons jointly. First, incommensurability arises when lexicons partly overlap. When that happens, one can understand how things are conjoined in the extension of a concept without being able in practice to project the concept beyond its accepted applications. Kuhn is thus suggesting that Aristotle on matter, form, and the void, or Volta on batteries and electrical current are to us like Nelson Goodman's famous examples of cross-classifying concepts, 'grue' and 'bleen' (where 'grue' means 'green if first examined before time t and blue if first examined thereafter' and 'bleen' the reverse).<sup>38</sup> We can interpret such strange (to us) concepts, but we cannot use them ("project" them, to adopt Goodman's term) within our practically configured world. One might object that such a failure of projectibility does not entail untranslatability: canonically, after all, Goodman first introduced 'grue' by definition in terms of 'green' and 'blue'. But Kuhn insisted that translation fails in the case of actual historical lexicons,

because it does not preserve the truth-value of the original utterances, i.e., their actually having been taken true in the appropriate circumstances. Goodman could thus translate 'grue' and 'bleen' only because they had never actually been projected, and therefore had no truth-values to preserve.

Why should translation preserve truth values in this sense? Presumably we should do so out of respect for the historical evidence: our only grounds for interpreting the lexicon in one way rather than another is recognition of its correct applicability in canonical circumstances. If a lexicon is not just a verbal structure but a practical configuration of similarities and differences in the world, then to translate it in ways that cut across that configuration is to misunderstand its use. We can interpret the words in our terms, but only in ways that systematically misconstrue the practical grasp of the world embedded in their actual deployment. Kuhn's concern here is continuous with his early objection to the claim that thought experiments reveal contradictions within a theory. The subsequent disclosure of phenomena to which the standard uses of a lexicon cannot be adapted without severe strain or incoherence can show its inadequacy as a tool for the further articulation of relevant aspects of the world, but cannot show it to have been incoherent or mistaken in its familiar uses.<sup>39</sup>

How then are the "truth values" of exemplary utterances in a historical lexicon related to their truth or falsity? To answer this question, we must first consider two different roles for historical interpretation. Kuhn came to recognize, alongside the "quasi-ethnographic" historical reconstruction he had long advocated, a legitimate role for a Whig history of scientific practices as a history of manifestations of the world we live in. This latter history, he suggested, sacrificed truth values to preserve a constant truth manifest throughout the past leading to present practices. I think Kuhn's point emerges in comparison to Davidson's suggestion that truth and meaning are two components that must be extracted from a single body of evidence.<sup>40</sup>

Given a set of occasioned utterances, we can either hold truth value constant and interpret meaning, or take meaning for granted in our own terms in order to determine what, if anything, was true in past scientific understanding. Davidson only allows one kind of interpretation, because he thinks that while we have no alternative to understanding others in our own terms, we can only assign an interpretation to what they say by taking them to be mostly truth speakers. Kuhn accepted Davidson's point as a characterization of understanding the past: we must figure out what others were talking about and take them to be mostly right about it. In doing so, however, we cannot map their discursive practices onto ours, because their terms are not projectible for us. Translation is not merely a matter of rendering words, but of disclosing the way the

world hangs together in the use of an interconnected set of concepts. For that, one must capture the ways in which things correctly exemplified concepts, and recognize the ways those concepts were used in ongoing practices.

The reason Kuhn thinks translatability fails is thus not that truth is relative to a conceptual scheme or a "world" of research practice. Only truth-values depend upon conceptual practices, but the upshot of Kuhn's arguments is that many commonplace utterances from past scientific practices do not have truth-values in our modern lexicon. Kuhn's point was captured eloquently in a remark attributed to Oscar Wilde. When asked whether a passage in one of his works was "blasphemous," Wilde supposedly replied, "That is not one of my words."<sup>41</sup> Kuhn thought likewise that the Ptolemaic 'planet', Volta's 'current', or Aristotle's 'matter' are not, and cannot be, among our words, i.e., cannot be projected in ongoing practices without abandoning our own practical conceptual commitments. In drawing the distinction between truth and truth-value, Kuhn was thus still centrally concerned with meaning rather than truth, even while recognizing the inappropriateness of assigning determinate meanings to individual sentences.

Although Kuhn acknowledged a role for such anachronistic translations in the training and identity-formation of scientists, he fiercely insisted upon their inappropriateness as history. Such translations "do violence" to the practices they interpret, presumably by tearing them from the setting in which they had a coherent and intelligible use. Yet there may be an analogous worry about Kuhn's preferred historical practice. Kuhnian narratives of the historical articulation of conceptual practices are framed by the articulation of structured lexicons. At the outset, there is a quasi-ethnographic stage setting; at the conclusion, a silence marking the displacement of the lexicon by incommensurable practices of articulating the world. Both are wrested by the historian from evolving discursive practices. Yet scientists in practice do not employ a lexicon with a definite structure; they talk about things within an ongoing, self-transforming practice of disclosure, a practice not confined to the settings in which it can be coherently systematized. If the Whig historian describes the present as the future of a past that never existed, Kuhn's historical narratives describe the past as the present of a future that never was to be.<sup>42</sup>

Kuhn's insistence that only a quasi-ethnographic reconstruction of untranslatable lexicons can count as history may thus mark a residual commitment to a semantic realism about the discursive practices of past science. But Kuhn himself often reminded us that as philosophers we can dispense with the rhetoric of correspondence to already determinate facts without thereby doing away with the sciences' accountability to how the world is manifest within their ongoing practices. Historical interpretation

is in this respect not significantly different. For if the lexicons of past science are not already determinate structures, applicable only to a limited domain of things correctly characterizable in their terms, the same is true of the language we now deploy in making sense of past science. Understanding the world and understanding our concepts and practices always go hand in hand. In making sense of the past, we simultaneously articulate our own concepts and practices, and resituate ourselves in the world. That is why there will always be a role for history, and why Kuhn's words in the beginning still remain vital for us in the end: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."<sup>43</sup>

## NOTES

1. This paper is dedicated to the memory of Thomas Kuhn, scholar and friend.
2. Thomas Kuhn, The Structure of Scientific Revolutions, Second Edition (Chicago: University of Chicago Press, 1970), p. 49.
3. It is symptomatic of the predominant philosophical readings of Kuhn's work that Paul Hoyningen-Huene, Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science (Chicago: University of Chicago Press, 1993), an impressive and authoritative reconstruction of Kuhn's philosophical project, has no index entry for "practice" or "scientific practice."
4. Joseph Rouse, Knowledge and Power: Toward a Political Philosophy of Science (Ithaca: Cornell University Press, 1987), ch. 2.
5. Kuhn, Structure, p. 10.
6. Kuhn's otherwise puzzling use of 'rules' as the contrast term to 'paradigm' in chapter 5 of Structure becomes clearer if one recognizes an allusion not only to Wittgenstein's discussions of rule following, but also to Carnap's use of "correspondence rule" to denote the interpretation of specific empirical tests of a theory.
7. Kuhn, Structure, p. 44.
8. Thomas Kuhn, "Afterwords," in World Changes: Thomas Kuhn and the Nature of Science, ed. Paul Horwich (Cambridge: MIT Press,

1993), p. 328, remarks that he later came to see his early emphasis upon perceptual Gestalts as misleading.

9. Kuhn lists three foci of normal scientific research, but characterizes paradigm articulation to be most important. In fact, the first two foci, the determination of revealing facts or facts that can be directly compared with theoretical prediction, can be seen as special cases of paradigm articulation. Both are attempts to extend or spell out more carefully what the paradigm tells us about the world. Indeed, Kuhn explicitly includes the theoretical work that generates the empirical comparison as a type of paradigm articulation.

10. The claim that Kuhn treats science as a reflexive practice may seem to be in tension with the sharp contrast Kuhn draws between scientists' and historians' attitudes toward past scientific practice, and his resolute defense of the ahistoricism of science pedagogy. There is no tension, however. Kuhn's claim is not that scientific practice is unreflexive, but that its reflexive focus upon its own concepts and theories is ahistorical. The difference is between the historian's attempt, as observer, to ascertain what the concepts and practices are at any point in time, and the scientist's concern, as participant, to make them into something different.

11. Kuhn, Structure, p. 170-73.

12. Kuhn, Structure, p. 53.



13. Of course, as Alasdair MacIntyre, "Epistemological Crises, Dramatic Narrative, and the Philosophy of Science," in Paradigms and Revolutions: Applications and Appraisals of Thomas Kuhn's Philosophy of Science (Notre Dame: University of Notre Dame Press, 1980) reminds, they then know a great deal about how things once made sense, and where that prior understanding seemed to come apart.

14. Kuhn, Structure, p. 58-59, cogently discusses why the discovery of X-rays "staggered" the small community of cathode ray researchers, even though other physicists found X-rays readily assimilable within Maxwellian electromagnetic theory.

15. Compare Ian Hacking, "Working in a New World: The Taxonomic Solution," in World Changes, ed. Horwich, p. 296-97, on the inability to write a new volume of Paracelsian medicine, however sympathetically one is able to interpret its claims and norms.

16. Among the most prominent contributors to this experimentalist philosophy and historiography of scientific practices are Ian Hacking, Representing and Intervening: Introductory Topics in the Philosophy of Natural Science (Cambridge: Cambridge University Press); Robert Ackermann, Data, Instruments, and Theory: A Dialectical Approach to Understanding Science (Princeton: Princeton University Press, 1985); Allan Franklin, The Neglect of Experiment (Cambridge: Cambridge University Press, 1986); Peter Galison, How Experiments End (Chicago: University of Chicago

Press, 1987); and David Gooding, Experiment and the Making of Meaning (Boston: Kluwer Academic, 1990); David Gooding, Trevor Pinch, and Simon Schaffer, ed., The Uses of Experiment: Studies in the Natural Sciences (Cambridge: Cambridge University Press, 1989).

17. Thomas Kuhn, "The Function of Measurement in Modern Physical Science," in The Essential Tension: Selected Studies in Scientific Tradition and Change (Chicago: University of Chicago Press, 1977), p. 214.

18. Kuhn, Structure, p. 193.

19. Kuhn, "Function of Measurement," p. 198.

20. Kuhn, "Function of Measurement," p. 200.

21. Kuhn, "Function of Measurement," p. 194.

22. Kuhn, "Function of Measurement," p. 208 (my emphasis).

23. Kuhn, "Function of Measurement," p. 201.

24. Kuhn, "Function of Measurement," p. 218.

25. Hacking, Representing and Intervening, ch. 13.

26. Kuhn, "Function of Measurement," p. 185-86.

27. Kuhn, Structure, p. 182, noted that he would have happily used the more familiar word 'theory' in lieu of 'paradigm' except for his desire to avoid confusion with its then-established philosophical connotations. I am suggesting that the principal connotation to be avoided is the separation of the theory itself as a semantic structure from the practices through which it is concretely interpreted as about specific kinds of situation in

the world.

28. Nancy Cartwright, How the Laws of Physics Lie (Oxford: Oxford University Press, 1983); Ronald Giere, Explaining Science: A Cognitive Approach (Chicago: University of Chicago Press, 1988), ch. 3; classic expositions of the semantic conception of theories include Patrick Suppes, "What Is a Scientific Theory?" in Philosophy of Science Today, ed. Sidney Morgenbesser (New York: Basic Books, 1967), p. 55-67, and Bas van Fraassen, The Scientific Image (Oxford: Oxford University Press, 1980).

29. Nancy Cartwright, "Fundamentalism vs. the Patchwork of Laws," Proceedings of the Aristotelian Society 94 (1994): 279-92; Kuhn, "Afterwords," p. 335-36, distinguishes his "reluctant pluralism" from Cartwright's criticism of fundamentalism, but I read Cartwright's view differently than he does, in ways that bring her closer to his view that it is "effability, not truth, that [is] relative to worlds and practices" (p. 336).

30. Thomas Kuhn, "A Function for Thought Experiments," in Essential Tension, p. 242.

31. Kuhn, "Thought Experiments," p. 254.

32. The discussion that follows draws upon Thomas Kuhn, "Possible Worlds in History of Science," in Possible Worlds in Humanities, Arts, and Sciences: Proceedings of Nobel Symposium 65 (Berlin: Walter de Gruyter, 1989), p. 9-32, 49-51; Thomas Kuhn, "The Natural and the Human Sciences," in The Interpretive Turn:

Philosophy, Science, Culture, ed. David Hiley, James Bohman, and Richard Shusterman (Ithaca: Cornell University Press, 1991), p. 17-24; Kuhn, "Afterwords," p. 311-341; as well as "The Presence of Past Science," The Shearman Memorial Lectures, University College, London, 23, 24, 25 November 1987 (my copy of the latter transcript is marked "DRAFT: Not for distribution, Quotation or Paraphrase," presumably because Kuhn anticipated its replacement by a more adequate version; since that version will never be written, I have drawn upon the lectures while respecting Kuhn's hesitancy to commit himself to their formulations).

33. Kuhn, "Natural and Human Sciences," p. 21.

34. One change in emphasis between Kuhn's earlier and later discussions of incommensurability is a shift from accounting for contemporary scientists' understanding of competing candidate paradigms to accounting for a historian's understanding of past practice.

35. For example, see Kuhn, "Possible Worlds."

36. Kuhn, "Natural and Human Sciences," p. 20.

37. Donald Davidson, "The Very Idea of a Conceptual Scheme," in Inquiries into Truth and Interpretation (Oxford: Oxford University Press, 1984), p. 183-98.

38. The classic discussion of projectibility and the grue example, is of course Nelson Goodman, Fact, Fiction and Forecast (Cambridge: Harvard University Press, 1954).

39. Kuhn might thus be seen to reinterpret the logical empiricist doctrine that a theory is correct within the domain in which it is well-confirmed as a claim about interpreting the history of science, although he rejects it as a methodological constraint upon subsequent theorizing in science.

40. Davidson, "The Very Idea," p. 196; Davidson's far-reaching program in the philosophy of language, which marks a fundamental shift from a representationalist to a discursive semantics, turns on treating 'truth' as a semantic concept that enables speakers or other agents to interpret one another, rather than an epistemological concept that would mark whether already determinate meanings corresponded to an already semantically determinate world. For a brief exposition of the Davidsonian program and its relevance to understanding the sciences, see Joseph Rouse, Engaging Science: How to Understand its Practices Philosophically (Ithaca: Cornell University Press, 1987), ch. 8.

41. I owe this example to Robert Brandom.

42. I adapt this description from Hans-Jorg Rheinberger, "Experimental Systems: Historiality, Narration, and Deconstruction," Science in Context 7 (1994): 65-81.

43. Kuhn, Structure, p. 1.