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THE HISTORICAL DEVELOPMENTAL PERSPECTIVE OF THOMAS S. KUHN: A SEARCH FOR FIRST PRINCIPLES IN THE DEVELOPMENT OF SCIENTIFIC KNOWLEDGE OVER TIME

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Abstract

This dissertation reconsiders the work of Thomas S. Kuhn on the basis of what he characterized as the historical developmental perspective. The perspective examines the activities, processes and choices involved in the development of scientific knowledge over time. It reflects case-based, rather than rules-based reasoning and provides an alternative and insightful basis for discerning changes in knowledge, ideas, and assumptions over time.

The historical developmental perspective guided Kuhn's research activities throughout his career, and through those investigations, he developed and refined both the approach and the insights that it provided. Yet the unique aspects of Kuhn's historical developmental perspective were not recognized by many of his readers – nor, to a great extent, were they appreciated fully by Kuhn himself. As a result, much of Kuhn's work has been misinterpreted by his readers and many of his most important ideas have remained unexplored.

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Abbreviation of Selected Works by Thomas S. Kuhn

CR	*The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (1957)
ET-SS	*The Essential Tension: Selected Studies in Scientific Tradition and Change* (1977)
RSS	*The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographic Interview* (2000)
SSR	*The Structure of Scientific Revolutions* (1962/1970a)
SSR-PS	"Postscript" to *The Structure of Scientific Revolutions* (1969/1970a)

AW	"Afterwords" (1990/2000)
CCC	"Commensurability, Comparability, and Communicability" (1982/2000)
CCDP	"Concepts of Cause in the Development of Physics" (1971/1977)
DR	"Dubbing and Redubbing: The Vulnerability of Rigid Designation" (1990)
EC	"Energy Conservation as an Example of Simultaneous Discovery" (1959/1977)
ET	"The Essential Tension: Tradition and Innovation in Scientific Research" (1959/1977)
FM	"The Function of Measurement in Modern Physical Science" (1956/1977)
FTE	"A Function for Thought Experiments" (1964/1977)
HS	"The History of Science" (1968/1977)
HSSD	"The Historical Structure of Scientific Discovery" (1962/1977)
LDPR	"Logic of Discovery or Psychology of Research?" (1965/1970b)
MET	"Mathematical versus Experimental Traditions in the Development of Physical Science" (1976/1977)
MS	"Metaphor in Science" (1977/2000)
OVJ	"Objectivity, Value Judgment, and Theory Choice" (1973/1977)
PW	"Possible Worlds in History of Science" (1986/2000)
RHHS	"The Relations Between History and the History of Science" (1971/1977)
RHPS	"The Relations Between the History and the Philosophy of Science" (1968/1977)
RIT	"Remarks on Incommensurability and Translation" (1999)
RMC	"Reflections on My Critics" (1969/1970c)
RSR-F	"Foreword" to *Reconstructing Scientific Revolutions* (Hoyningen Huene 1989/1993)
RSS	"The Road Since *Structure*" (1990/2000)
RTC	"Rationality and Theory Choice" (1983/2000)
ST	"Second Thoughts on Paradigms" (1974/1977)
TCSC	"Theory Change as Structure Change: Comments on the Sneed Formalism" (1976/2000)
THPS	"The Trouble with the Historical Philosophy of Science" (1991/2000)
WSR	"What Are Scientific Revolutions?" (1980/2000)

Acknowledgments

In making the transition from the theory and practice of corporate strategy to the study of philosophy, I have received inspiration, encouragement and support from a number of individuals across a diverse set of institutions.

During my undergraduate studies at Vanderbilt University, Professor George Graham's courses in Classical Political Philosophy and Foundations of Marxism introduced me both to the history of ideas and to ideas of history. While fascinated by the courses at the time, I have only recently recognized the extent to which they have shaped my interpretations and evaluations of the ways that institutions are established, structured, and changed over time.

Subsequent study and research with Professor Michael Porter at the Harvard Business School refined my understanding of the relationship between the activities of individuals and the corporate, industry, economic, and regulatory structures within which they are practiced. Professor Porter's unique and exceptional ability to combine detailed analysis with an appreciation for the interrelation of forces within a broader system provided my first glimpse of the bridge that can be built between theory and practice. His work on the dynamics of strategy focused my attention on the limitations of established views of change and development.

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There is nothing more difficult to take in hand,

more perilous to conduct,

or more uncertain in outcome,

than to take the lead in introducing a new order of things.

Niccolo Machiavelli

Introduction

"I just want to know what Truth is!"
Thomas S. Kuhn[1]

This exclamation by Thomas Kuhn is puzzling. As the author of *Structure of Scientific Revolutions* (*SSR*, 1962/1970a), Kuhn was praised by sociologists of science as "a man who did as much as anyone to destroy the authority of science" (Brown 1997, 486) and was challenged by philosophers of science for presenting an image of science that was relativistic (Shapere 1964, 392), irrational (Feyerabend 1970, 215) and governed by mob psychology (Lakatos 1970, 178). His views were adopted and extended by self-proclaimed "Kuhnians," who proposed that scientific knowledge was established through political processes of negotiation and possessed no independent, objective, or logical authority. Philosophers of science characterized his work as "anti-humanitarian" (Feyerabend 1970, 197) and as presenting "a danger to science, and indeed, to our civilization" (Popper 1970, 53).

Kuhn, whom one interviewer characterized as a reluctant revolutionary, neither welcomed nor agreed with these interpretations of *Structure*:

> For Christ's sake, if I had my choice of having written the book or not having written it, I would have written it. But there certainly have been aspects involving considerable upset about the response to it. (Horgan 1991, 40)

Rejecting the interpretations of both sociologists and philosophers of science, Kuhn insisted that he was suggesting a different view of science rather than trying to undermine its cognitive authority (AW 1990/2000, 228). While he affirmed *Structure's* emphasis on a broadened conception of science and scientific practice, he claimed that acknowledging the role of sociological and psychological factors in scientific development did not necessitate the wholesale rejection of either scientific progress or "good reasons" for theory choice (RMC 1969/1970b, 234).[2] Kuhn explicitly denounced the attempts by Kuhnians to make their fields more scientific by adopting paradigms and then enforcing them by fiat.[3] Claiming to be fonder of his critics than his admirers (Horgan 1991, 40), he once shouted in frustration, "One thing you have to understand. I am not a Kuhnian!" (Dyson 1999, 16).

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[1] (Baltas et al. 1995/2000, 278).
[2] See (*SSR*-PS 1969/1970a, 186 and 205-7), (LDPR 1965/1970b, 13-21), (RMC 1969/1970c, 259-66) and (AW 1990/2000, 228) for the most detailed rejections.
[3] See (*SSR*-PS, 208-10), (RMC, 245 and 263) and (RSS 1990/2000, 106).

While Kuhn claimed that his work had been widely misinterpreted, his claims generally were not taken seriously. Kuhnians and other scholars in the sociology of science insisted that he did not recognize the full implications of his work, while philosophers of science claimed that he was attempting to avoid its logical consequences. Paul Feyerabend, a philosopher of science who had been trained as a physicist, reflected upon numerous discussions with Kuhn about the interpretation and implications of *Structure*, explaining that,

> …my discussions with Kuhn remained inconclusive. More than once he interrupted a lengthy sermon of mine, pointing out that I had misunderstood him, or that our views are closer than I had made them appear. Now, looking back at our debates as well as at the papers which Kuhn has published since his departure from Berkeley, I am not so sure that this was the case. And I am fortified in my behalf by the fact that almost every reader of Kuhn's *Structure of Scientific Revolutions* interprets him as I do, and that certain tendencies in modern sociology and modern psychology are the result of exactly this kind of interpretation. (Feyerabend 1970, 198)

Despite their different evaluations of *Structure*, sociologists and philosophers of science seem to have had similar interpretations of the work. While the similarities are suggestive and, perhaps, mutually reinforcing, it is important to consider whether the concerns of philosophers of science and the tendencies in modern sociology and modern psychology, were the result of an appropriate interpretation of *Structure* or whether they themselves influenced that interpretation of the work. On the other hand, if Kuhn's work could be misinterpreted so systematically, we must explain why this occurred and the ways in which Kuhn seemed to misunderstand his own work.

Kuhn's rejection of the interpretations suggested by sociologists and philosophers of science was supported by scholars in other fields, including historian Mary Hesse (1964) and computer scientist Margaret Masterman (1970). While acknowledging important limitations in the philosophical implications of *Structure*, they proposed that the work outlined a "new epistemological paradigm" (Hesse 1964, 187) and established Kuhn as "one of the outstanding philosophers of science of our time" (Masterman 1970, 59).

Overview

This dissertation proposes that *The Structure of Scientific Revolutions* and the entire corpus of Thomas S. Kuhn have been misinterpreted in a way that is both systematic and suggestive. Rather than undermining cognitive authority or illustrating the irrationality, relativism and mob psychology inherent in the development of scientific knowledge, *Structure* highlighted problems inherent in traditional views of scientific development and introduced an alternative – if not yet fully developed – approach for examining, understanding, and influencing scientific activity. This approach differs from traditional methods employed in the history and the philosophy of science. Yet it does not therefore necessitate the abandonment of method; reliance on solely sociological considerations; or a relativistic or irrational view of scientific development.

In identifying the misinterpretation of *Structure*, however, we must also recognize important limitations in its presentation. Kuhn's research activities were wide-ranging, his methods were not always clear, and his ongoing revisions and refinements of his theory suggest that his work was not yet developed fully. Given the ambiguity of the central concept of paradigms, as well as Kuhn's lack of clarity and even misunderstanding regarding other important aspects of the account, it is not surprising that it was so widely misinterpreted. On the other hand, the systematic nature of this misinterpretation suggests that there were also important limitations in the approaches of the sociologists and philosophers of science who engaged the work.

As viewed from the perspective of *Structure's* schematic account, both Kuhn's development of his work and the interpretive debates surrounding it can be seen as exemplars of the developmental processes outlined in the account. Both were prompted by the recognition of an anomaly, which drew into question particular aspects of traditional views and approaches. A wide range of further investigations were conducted, and these gradually were broadened beyond boundaries of established assumptions and methods. The resulting accounts reflected fundamental changes in several of these established views and approaches and thus were incommensurable with more traditional views.

Structure represented Kuhn's attempt to explain an anomaly with which he had been struggling for fifteen years; however the account remained highly developmental. The work outlined a new image of the nature of science yet also suggested a new epistemological paradigm that undercut traditional views of the nature of knowledge. Other scholars, struggling with issues of cognitive authority yet employing different assumptions and methods, failed to recognize the distinctive aspects of *Structure's* account and systematically misinterpreted its assertions and implications. Yet this systematic misinterpretation, in turn, suggests an anomaly, that is, a limitation in the traditional approaches of sociologists and philosophers of science. As such, it demands further investigation and, as we will see, indicates areas for reconsideration, and even reconstruction.

The Interpretive Debates Surrounding *Structure*

The interpretive debates surrounding *Structure* focused on the philosophical implications of *Structure's* expansive view of science, in particular, its implications for the cognitive authority of science. The work was published in 1962, during a period in which various types of "authority" were being questioned. Given the "special status" of science as one of the most independent and objective forms of knowledge, *Structure's* focus on the development of scientific knowledge and its implications for the authority of science thus were of particular interest to scholars seeking either to defend or to attack authority.

Structure and the Crisis of Authority

Structure's apparent diminution of the authority of science emerged from its historically based account of the role of sociological and psychological factors in scientific development. In the Introduction, Kuhn proposed that method alone could not account fully for the historical changes in scientific activity, but prior experience, accidents and individual make-up must be understood as "essential determinants of scientific development" (*SSR* 1962/1970a, 4). Kuhn emphasized the role of psychological and sociological factors in situations of theory choice and proposed that the scientific community serves as the producers and validators of scientific knowledge.

Structure's emphasis upon the influence of sociological factors seemed to undermine the philosophical basis and authority of scientific knowledge. In the sociology of science, *Structure's* emphasis on these broader influences at work in scientific activity was applauded, even as Kuhn's reticence about the implications for scientific knowledge was recognized:

> [Kuhn's] work was extended, often in directions which he would have disliked . . . but always with the same tendency – to diminish the authority of science as a way of reaching out to non-human forms of wisdom. Even scientists, he showed, were confined within the assumptions of their time. (Brown 1997, 487)

Philosopher Rom Harré claimed that Kuhn's historical insights established the possibility for a more expansive, sociological view of the philosophy of science:

> Kuhn's work was so influential in so many fields partly because of his historical verisimilitude, and partly because, in the spirit of the times, it opened a space for the sociology of knowledge to find a serious role in philosophy of science, hitherto very much the province of those with a logical interest. (Harré 1997, 485)

The most extreme extensions of Kuhn's work were proposed by "Kuhnians" in the Strong Programme at the University of Edinburgh, who rejected the possibility of independent or objective authority established on the basis of logic and insisted that the "rationality" of science rested solely on negotiation among various political powers. In some respects, however, these views were moderated over time, as indicated by David Bloor's later characterization of Kuhn's work as insisting upon a more expansive conception of rationality: "Kuhn was not calling into question the rationality of science, he was rejecting a certain philosophical account of what rationality consisted in" (Bloor 1997, 499).

Scholars in the philosophy of science acknowledged the valuable historical insights provided by *Structure* but viewed the philosophical implications of the work more critically. Dudley Shapere's 1964 review described *Structure* as an "important book" and applauded its "sustained attack on the prevailing image of scientific change as a linear process of ever-increasing knowledge" (Shapere 1964, 383). Shapere noted that Kuhn's critique of both the historical and philosophical aspects of this view "has much in common with some recent antipositivist reactions among philosophers of science – most notably, Feyerabend, Hanson, and Toulmin" (Ibid.). Yet he proposed that *Structure* went too far, rejecting the traditional, positivist account of scientific development only to establish a relativistic view in its stead:

> If one holds, without careful qualification, that the world is seen and interpreted "through" a paradigm, or that theories are "incommensurable," or that there is "meaning variance" between theories, or that all statements of fact are "theory-laden," then one may be led all too readily into relativism with regard to the development of science. Such a view is no more implied by historical facts than is the opposing view that scientific development consists solely of the removal of superstition, prejudice, and other obstacles to scientific progress in the form of purely incremental advances toward final truth. (Ibid., 393)

Shapere proposed that historians of science must "achieve a more balanced approach to their subject – neither too positivisitic or too relativisitic" (Ibid.) and suggested that Kuhn undertake "more careful scrutiny of his tools of analysis," in particular the logic of his notion of a paradigm (Ibid., 394).

A 1964 review by Mary Hesse, a scholar in the history of science at the University of Cambridge, also described *Structure* as "an important book," noting that,

> It is the kind of book one closes with the feeling that once it has been said, all that has been said is obvious, because the author has assembled from various quarters truisms which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed. (Hesse 1964, 286)

Acknowledging the potentially controversial aspects of this new image of science, Hesse proposed that it reflected a promising new epistemological paradigm:

> The major question for historians of science . . . is whether history bears the interpretation here put upon it. The answer, as in the case of any shift of paradigm, will be partly dependent on impressionistic and non-logical factors and will be subject to the kinds of resistance Kuhn finds in paradigm-change within the sciences. My own impression is that Kuhn's thesis is amply illustrated by recent historiography of science and will find easier appreciation among historians than among philosophers. But it cannot be disputed that this is the first attempt for a long time to bring historical insights to bear on the philosophers' account of science, and whatever the puzzles are that remain to be solved, Kuhn has at least outlined a new epistemological paradigm which promises to resolve some of the crises currently troubling empiricist philosophy of science. Its consequences will be far reaching. (Ibid., 287)

Many of Hesse's predictions were borne out the reception of *Structure*. The linkage established in the work between history and philosophy of science was increasingly questioned, and the divergent interpretations of *Structure's* "new epistemological paradigm" emerged as central concerns in interpretive debates about the work.

Concerns about the authority of science were provoked by *Structure's* discussions of the choice between competing paradigms, in particular, the proposal that such debates are, in Mary Hesse's words, "partly dependent on impressionistic and non-logical factors." Sociologists of science welcomed, explored, and extended this proposal. Philosophers of science rejected its implicit refutation of the cognitive authority of science. Yet historians such as Hesse seemed to suggest that Kuhn's historical investigations suggested a basis for reconsidering established views of science without necessarily undermining its authority. While acknowledging that the philosophical authority and implications of *Structure* were not yet developed fully, she insisted that the promise of the work merited its consideration and further development.

In considering these various views and their associated response to the crisis of authority, Paul Feyerabend's comments are suggestive. Affirming the "irrationality" of science that Kuhn's work suggested, he proposed that "the question raised by Kuhn is not whether *there are* limits to our reason; the question is *where* these limits are *situated*" (Feyerabend 1970, 218). While claiming that the development of scientific knowledge occurs outside the boundaries of rationality, Feyerabend insisted that the determinations of science are not, therefore, relativistic (as, he proposed, *Structure* seemed to imply). Rather, through aesthetic judgments, judgments of taste and subjective wishes, incommensurable views may be evaluated according to standardized criteria of form, rather than content:

> Incommensurable theories, then, can be *refuted* by reference to their own respective kinds of experience (in the absence of commensurable alternatives these refutations are quite weak, however). Their *content* cannot be compared. Nor is it possible to make a judgment of *verisimilitude* except within the confines of a particular theory. None of the methods which Popper wants us to use for rationalizing science can be applied and the one that can be applied, refutation, is greatly reduced in strength. What remains are aesthetic judgments, judgments of taste, and our own subjective wishes. (Ibid., 227-8)

According to Feyerabend, the occurrence of incommensurability in scientific development reminds us that science is a human, rather than an ideal, enterprise. Although the traditional philosophical goal of verisimilitude or objectivity may be unachievable with regard to the content of scientific theories, we are not thereby abandoned to relativism. Rather, we must look to those alternative processes by which we might work to compare, to improve, and to argue until we identify the most appealing formulation and direction for our scientific work.

Competing Interpretations and Evaluations

In "Reflections on My Critics," the response to a symposium at the 1965 International Colloquium in the Philosophy of Science,[4] Kuhn asserted that a gestalt switch separated his views from those of Karl Popper and his philosophical colleagues (RMC 1969/1970c). In the opening lines of the essay, he insisted that the philosophers of science had misinterpreted his work in a systematic and fundamental way:

> I am tempted to posit the existence of two Thomas Kuhns. Kuhn_1 is the author of this essay and of an earlier piece in this volume. He also published in 1962 a book called *The Structure of Scientific Revolutions*, the one which he and Miss Masterman discuss above. Kuhn_2 is the author of another book with the same title. It is the one cited repeatedly by Sir Karl Popper as well as by Professors

[4] The colloquium was organized jointly by the British Society for the Philosophy of Science and the London School of Economics and Political Science, under the auspices of the Division of Logic, Methodology and Philosophy of Science of the International Union of History and Philosophy of Science. It was held at Bedford College, Regent's Park, London, from 11 to 17 July 1965, and was intended to be a tribute to Sir Karl Popper. During the Colloquium, a symposium was held on "Criticism and the Growth of Knowledge," in which Professors Kuhn, Feyerabend, and Lakatos were to be the main speakers. Neither Feyerabend nor Lakatos were able to submit their contributions for the symposium, thus Kuhn became the primary speaker (Feyerabend and Lakatos were replaced by John Watkins, who responded to Kuhn's paper). Those also presenting papers included Professor Sir Karl Popper, who chaired the session, Professor Steven Toulmin, Professor Pearce Williams, and Miss Margaret Masterman. The proceedings of the symposium, along with later contributions by Lakatos and Feyerabend (as well as the cited response by Kuhn) were published as *Criticism and the Growth of Knowledge* (Lakatos and Musgrave 1970).

> Feyerabend, Lakatos, Toulmin, and Watkins. That both books bear the same title cannot be altogether accidental, for the views they present often overlap and are, in any case, expressed in the same words. *But their central concerns are, I conclude, usually very different*. As reported by his critics (his original has unfortunately been unavailable to me), Kuhn_2 seems on occasion to make points that subvert essential aspects of the position outlined by his namesake. (RMC, 231, emphasis added)

According to Kuhn, the philosophers of science who had participated in the symposium had responded to *Structure* on the basis of views that "often overlap[ped]" or at least, were "expressed in the same words" as those outlined in the work. Yet their central concerns were "usually very different" from his own, and on occasion, their points "subvert[ed] essential aspects" of his position. The only member of the symposium who seemed to examine and to address the concerns of Kuhn_1 was Margaret Masterman.

These comments highlight two competing interpretations of *Structure*, which are evident not only in the 1965 colloquium but also in broader debates about the interpretation and evaluation of *Structure*. The first interpretation, supported by both sociologists and philosophers of science, proposed that *Structure* presents an account that draws into question the cognitive authority of science. While philosophers of science vociferously rejected this (sociological) image of the nature of science, sociologists of science readily adopted it and attempted to expand it further. The second interpretation, expressed by Kuhn and supported by historian Mary Hesse and computer scientist Margaret Masterman, suggested that *Structure* does not undermine the authority of science but outlines a new image of science that addresses emerging issues in empiricist philosophy of science and avoids the "aetherialism" of traditional philosophical accounts.

These contrasting interpretations reflect the differing orientations, approaches, and assumptions of the scholars who considered the work. Philosophers of science conducted focused critiques of *Structure's* central concepts based on the logical principles by which science *should be practiced*, whereas historians and scientific practitioners examined Kuhn's broader, historical developmental accounts of how science *has been practiced*.[5] To historians and practitioners, Kuhn's examination of historical scientific activity provided an appropriate basis for *Structure's* schematic account and its (implicit) normative claims about science. Yet to philosophers of science (including those who were part of the so-called "historicist turn"),

[5] Some philosophers, such as Hempel, also expressed support for Kuhn's view of his work against the interpretation of philosophers of science (Hempel 1993, 7-8). As we will see in Chapter Five, Hempel's interest in *Structure* reflected a concern with issues of concept formation and theory choice.

historical accounts could properly be used only to illustrate or to refine philosophical assertions – not to serve as the basis for them.

What seems to be at issue in these competing perspectives is the relationship between the history and the philosophy of science in understanding, explaining, and directing the conduct and the investigation of science. Kuhn, Hesse (1964), and Masterman (1970) seemed to see the history of science as playing an insightful and even directive role in establishing imperatives for scientific activity. L. Pearce Williams (1970) and Paul Feyerabend (1970) also recognized this role for history; however, they expressed caution about the philosophical strength of the conclusions to be drawn from an historical account. Popper's followers in the philosophy of science, on the other hand, viewed the history of science as a means of illustrating or, at most, falsifying the universal, logical imperatives established by scientists' bold conjectures and philosophers' critiques (Lakatos and Musgrave 1970). In their view, the "particular" aspects of even the most rigorous historical examination did not provide an adequate basis for "universal" claims or generalizations.

These contrasting interpretations and evaluations of *Structure* were based on competing views of science as an inescapably human activity (Kuhn, Feyerabend, Masterman and Williams) or as the highest example of a purely logical and objective field of inquiry (Popper, Watkins, Toulmin and Lakatos). In the first instance, history plays an important role in constituting the nature of science; yet in the second instance, granting too much of a role to history risks the introduction of subjective or temporal factors. On this account, the introduction of such factors risks clouding the otherwise logical and objective investigations that are based on the hard-won achievements associated with the discovery of natural laws, the development of scientific theories, and the determination of criteria. Thus while the focus of investigations by Kuhn and others was the conduct of scientific activity, philosophers of science focused their work on the logic of scientific theory.

Examinations of *Structure* and the Surrounding Interpretive Debates

Despite the vast array of secondary literature addressing the issues raised by Structure, there have been only two systematic examinations that have considered the work within the context of its surrounding, interpretive debates (Fuller 2000, 4). *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy* *of Science* (1993), written by Paul Hoyningen-Huene with Kuhn's oversight and approval, provides a detailed reconstruction of both the major concepts of Structure and further developments of Kuhn's thought. The second systematic work, Steve Fuller's *Thomas Kuhn: A Philosophical History for Our Times* (2000), examines the sociological and political context of *Structure*, tracing elements of Kuhn's thought throughout the history of philosophy and across the theoretical landscape of its time. At the most general level, Hoyningen-Huene's text represents an in-depth, internal account of the theory presented in *Structure*, whereas Fuller's work provides an equally detailed, external account of the context within which it was written and developed.

The two studies are each valuable in their own right yet lacking in what the other highlights. Hoyningen-Huene offers a careful, textually focused analysis of the theory presented in *Structure* and Kuhn's further development of his ideas through 1989. The work takes seriously Kuhn's rejections of other interpretations and attempts to refute them on a point-by-point basis, applying Kuhn's theory as he developed and refined it over time. Yet the work explicitly neglects the contextual factors surrounding its development and reception, including a systematic understanding of the reception of the work and the reasons underlying interpretive debates about its implications (Hoyningen-Huene 1993, xviii-xx).[6] Perhaps as a result of these (understandable yet unfortunate) omissions, it has proven relatively ineffective in redirecting the terms of debate surrounding *Structure*.

In contrast, Fuller provides an in-depth analysis of the external, contextual factors at play in the creation, interpretation, and reception of *Structure.* Fuller's work highlights the background influences and the underlying assumptions of *Structure* that have remained unrecognized and unacknowledged, perhaps, the author notes, even by Kuhn himself. He characterized these influences and assumptions as a reflection of the "Cold War mentality" exemplified by Kuhn's mentor, James Conant. He described *Structure* as an expression of this mentality "in a more abstract and hence more portable form" (Fuller 2000, 6). In its central concerns, Fuller proposed that *Structure* reflects only the most recent installment of a long history of philosophical ideas that present "a Platonic concern for the peculiar position of elites in a democratic culture" (Ibid., 92).

[6] Hoyningen-Huene notes: "The neglect of these topics is motivated not by lack of interest but rather by the concentration necessary in pursuit of the reconstructive and critical goals of this work" (Hoyningen-Huene 1993, xx).

While Fuller's examination of the impact of *Structure* emphasized its reflection of a Cold War mentality, he placed special emphasis upon Kuhn's claims of misinterpretation, commenting that "the monumental fecundity of misreadings attached to *Structure* begs for explanation" (Ibid., 4). In attempting to account for these misreadings, however, he noted that, "it may be that Kuhn seems to be so frequently 'misunderstood' because his readers are really not trying to understand him to any great extent but to use his text as a token in some ongoing disputes" (Ibid., 5n). Fuller attributed this widespread misreading to "the fortuitous timing and placing of its appearance" (Ibid., xii) and suggested that it resulted from Kuhn's failure to recognize the influences that guided his work. He described *Structure* as a "servant narrative" and characterized Kuhn as a "culturopath" who had benefited unduly from the "weak ties" of his association with Harvard yet failed to recognize his place within the culture (Ibid., 398).

Fuller's evaluations of Kuhn and his contextualization of *Structure* within particular lines of thought in the history of philosophy frequently are based upon interpretations that conflict with textual accounts in *Structure*, with Kuhn's earlier work and with descriptions of the General Education program in which Kuhn taught.[7] Thus while the work provides an extensive and expansive account of *Structure*, as contextualized within its time and within the history of philosophy, Fuller's account is not consistent with the ideas, accounts and texts that it considers. These contradictory interpretations may, in fact, prove to be appropriate, and it is important to recognize that Kuhn may not have been aware of the influencing factors or implications of his work. Yet Fuller's recognition of interpretive issues is not balanced by a justification of own interpretation or a consideration of alternatives. As such, his work seems to beg the question of how *Structure* should be interpreted, rather than to resolve it.[8]

[7] These (mis-) interpretations occurred with respect to each of Kuhn's major concepts, including paradigms (Fuller 2000, 187, 192, 193, 249 and 269), normal science (Ibid., 187, 192, 194 and 195), communities (Ibid., 211-3, 221, 301, 303 and 306), and incommensurability (Ibid., 201-5, 309 and 380) as well as Kuhn's methods (Ibid., 193, 203, 206, 210, 302 and 316) and objectives (Ibid., 379-80).

[8] This dissertation reflects a general agreement with Fuller's emphasis upon and explanation for the widespread misunderstandings of Kuhn and the need for a broader analysis of the context in which *Structure* has been interpreted. Yet this contextual analysis must also take into consideration a more serious consideration of Kuhn's claims of misinterpretation than was provided by Fuller. In this respect, our historiographic and historical developmental investigations suggest that – far from being a culturopath – Thomas Kuhn was actively engaged in and provided important contributions to some of the most important philosophical problems of our times.

The Limitations of *Structure*

In considering the confusing state of secondary literature surrounding *Structure*, it is important to understand the extent to which Kuhn must bear some responsibility along with his respondents. That is, we must consider the extent to which the ongoing misinterpretations of *Structure* and the dearth of systematic studies of the work are a result of Kuhn's approach both to the work and to the various interpretations and criticisms of it. Although Kuhn borrowed quite extensively from a wide range of fields, he did so selectively and without acknowledging (or recognizing) the unique objectives, methods, and issues that characterized those fields. In this respect, he rightly may be accused of "promiscuous borrowing," perhaps to the point of recklessness (Fuller 2000, 195n).

Interestingly, Kuhn did not distinguish his own objectives and methods from those of the scholars from whom he borrowed and whose concerns he sought to address. He often neglected subtle yet important nuances that separated his work from that of philosophers of science. The breadth of Kuhn's investigations extended his work into fields in which he seemed to have limited technical knowledge, thereby rendering some of his statements either naïve or even incorrect from the perspective of scholars in those fields. Finally, both the schematic nature of Kuhn's essay and his ongoing clarification of its key concepts seemed to foster additional problems of understanding, interpretation, and attribution. In these respects, it is perhaps understandable that other scholars dismissed Kuhn's claims of misinterpretation, given that he did not clearly articulate (or, perhaps, even recognize) the unique character of his work vis-à-vis their own.

* * *

It seems highly ironic and perhaps even unimaginable that what is, arguably, one of the more influential works of the last century could be misinterpreted in such an extensive and continuous way. And yet we are left with Kuhn's explicit rejections of the dominant interpretations of his work, his puzzlement at the interest of scholars in other fields, and the non-completion of his book on incommensurability that was to present a final resolution of the issue.

Even if the dominant interpretations of *Structure* do not reflect the reported intentions of the author, it is reasonable to ask whether such (mis-) interpretation and (mis-) appropriation are relevant or even important. If Kuhn did not *intend* to suggest that science is irrational or relativistic yet his work can be *interpreted* as making such a suggestion, how are we to adjudicate between the two perspectives? Such questions are important; yet they are also premature. What they do suggest, however, is a need to understand more clearly Kuhn's intentions and his desired interpretation of *Structure*. This interpretation must then be subjected to evaluation and critique, both on its own terms and from other perspectives. At that point, the implications and evaluations of the various interpretations can be addressed more clearly.

An Historiographic Reconsideration of *Structure*

Although detailed investigations of *Structure* were conducted in the various fields, our preliminary investigation suggests that both sides of the interpretive debates may have overlooked important aspects of the competing views, rendering each of their respective interpretations incomplete. By focusing on a particular area of interest, each group neglected other areas. Thus while philosophers of science tended to underestimate or to overlook the concrete aspects of Kuhn's approach, historians and practitioners seemed to neglect its philosophical implications.

These differences in orientation might very well support interpretations of the conclusions and implications of *Structure* that are systematically different. Given the broad sweep of Kuhn's investigations, it is possible that each group responding to *Structure* within the context of their own field has only a partial view of the work. Kuhn's characterization of the various interpretations of his work as separated by a gestalt switch rather than a disagreement thus seems to be appropriate. Yet it is also possible, if not probable, that the scholars' concerns about *Structure* are valid with respect to their specialized fields, and thus must be examined and addressed carefully. In short, there seems to be a need for further investigation of the interpretive debates surrounding *The Structure of Scientific Revolutions*. Yet it is important to evaluate both the debates and the work itself, considering not only Kuhn's stated intentions for the work but also alternative views and concerns about its theory and implications.

In investigating the interpretive debates surrounding *Structure*, we first conduct an historiographic examination of Kuhn's research activities in an attempt to identify the "historical integrity" of the theory presented in *Structure* (**Chapter One**). In doing so, we follow the historiographic method employed by Kuhn in his investigations of scientific practice. This examination focuses on the research activities through which Kuhn developed the theory of *Structure*, examining both the development of his ideas over time and the contextual or personal factors that may have influenced that development. Based on these investigations, we examine in detail the theory of *Structure*, highlighting areas in which the theory presented is similar to, different from, or more refined than the ideas developed through Kuhn's earlier research (**Chapter Two**). Finally, we evaluate the concerns expressed by philosophers of science, considering the responses of Kuhn, Hesse, and Masterman as well as the insights provided by our historiographic investigations (**Chapter Three**).

Our historiographic investigation suggests that the historical development of *Structure* began with Kuhn's Aristotle experience of 1947. This experience suggested to Kuhn that something was wrong with traditional (positivist) views of the nature of science as independent, objective knowledge of nature. In particular, the experience indicated that the historical development from Aristotelian to Newtonian physics entailed a change not only in scientific theories but also in scientists' fundamental conceptions of nature. Furthermore, it seemed that scientific theories and conceptions of nature were so deeply interrelated that major changes in scientific theory necessitated changes in conceptions of nature. The transition from Aristotelian to Newtonian physics thus seemed to reflect a change both in the practice of science and the "world" within which science was practiced.

Fascinated by his Aristotle experience, Kuhn switched from physics to the history of science and began to explore fields such as psychology, philosophy, and linguistics, in which similar problems were being investigated. These subsequent historiographic studies and related explorations confirmed the issues raised by Kuhn's Aristotle experience and suggested that they extended beyond science to traditional views of the nature of knowledge, as characterized by independent and objective application of logical dichotomies and distinctions. It seemed that traditional distinctions between subject and object, descriptive and normative, practice and theory, or discovery and justification were interrelated and interdependent. Furthermore, these "distinctions" seemed to be linked closely to the questions and issues to which they were applied. Revolutionary changes such as the transition from Aristotelian to Newtonian physics seemed to prompt changes in the logical distinctions that were employed in evaluation as well as the questions or issues that were to be evaluated.

In order to highlight and to account for these issues, Kuhn first conducted detailed historiographic investigations of revolutionary discoveries. Based on these case studies, he began to identify shared characteristics, underlying patterns, and apparent structures and began to develop historical developmental accounts of various types of scientific activity. Gradually, these accounts and Kuhn's associated investigations extended beyond scientific discoveries to encompass all aspects of scientific activity.

Structure represented the culmination of this fifteen year period of historiographic and historical developmental research. It was presented as a schematic account of scientific development, which was based upon the historical investigation of actual scientific practice. In the first lines of the Introduction, Kuhn proposed that history could provide the basis for a new image of the nature of science, and he outlined this proposed image in terms of four implications of the new historiography. Yet as presented in *Structure*, these four implications – paradigms, normal science, anomalies, and scientific revolutions – were introduced and interrelated in an order that was inverted relative to the order in which they had been developed. As a result, their basis in Kuhn's earlier, historiographic investigations was concealed and their apparent authority was thereby transformed. Furthermore, while the concept of normal science was developed more fully in *Structure's* schematic account than in Kuhn's earlier publications, the notion of paradigms was more ambiguous, in part because many important nuances gleaned from earlier investigations were not included. This ambiguity was particularly consequential, given the status of paradigms as the central concept of *Structure's* account.

Given the ambitious nature of *Structure's* account, the ambiguity of its central concept of paradigms, and the importance of its implications for the development and authority of science, it is not surprising that the work encountered a wide range of interpretations and evaluations. Based on their understanding of science, both sociologists and philosophers of science interpreted the notion of paradigms as a dominant theory, whose authority was established and overturned by the dictates of the scientific community. As a result of this interpretation, they proposed that *Structure's* account undermined the cognitive authority of science. Yet historians and scholars in other fields emphasized the function of paradigms within the conduct of actual science and viewed the notion as a valuable interrelation of the empirical and theoretical aspects of scientific activity. On this interpretation, *Structure's* account offered a new image of science that promised to resolve some of the epistemological problems facing philosophers' "aetherial" approaches to scientific development. To the extent that the notion of paradigms served to relate the empirical and the theoretical aspects of scientific activity, it would seem that it also offered an alternative to sociologists' views of science as a purely "political" activity.

Based on our historiographic investigations of Kuhn's research, it seems that sociologists and philosophers of science misconstrued the notion of paradigms and thus misinterpreted the theory and implications of *Structure*. This misinterpretation seems to be both systematic and suggestive. In neglecting the function of paradigms as an interrelation of the empirical and the theoretical aspects of science, they neglected the central insight of Kuhn's Aristotle experience and of the new historiography, more generally. To a great extent, this "neglect" was understandable – the result of their sociological and philosophical approaches to the interpretation of *Structure*. Yet this explanation provides an even more important suggestion: that their misinterpretation of *Structure* reflected not only limitations in their interpretation of *Structure* but also limitations in their approaches to investigations of science and the development of scientific knowledge. It would seem, then, that understanding *Structure's* new image of the nature of science requires making adjustments to traditional sociological and philosophical approaches to science and its development. To some extent, it may also require the adjustment of traditional views of cognitive authority.

An Historical Developmental Account of *Structure*

When viewed from the perspective of Kuhn's research activities, the theory and concepts outlined in *Structure* suggest an interpretation of the work that is dramatically different from the one proposed by philosophers of science. From this historiographic perspective, Kuhn's research can be understood as an investigation of the anomaly presented by his Aristotle experience. *Structure* represents an attempt to explain the anomaly within the historical context of scientific development, rather than an effort to undermine the authority of science. To the extent that Kuhn challenged and adapted traditional views of the nature of science, he did so in order to reconcile those views with the actual scientific practice.

Yet simply proposing this alternative interpretation of *Structure* is not a sufficient response either to the concerns of Kuhn's most vocal critics in the philosophy of science or to the interests of his (unwelcome) advocates in the sociology of science. For in adapting existing theories of scientific development to explain actual scientific practice, Kuhn simultaneously threw into question the epistemological basis for scientific authority. He provided a few references to the epistemological implications of his work and a few hints as to the resolution of the challenges that they raised for traditional views. Yet he did not provide an epistemological basis for the normative aspects of his schematic account. Nor did he adequately communicate the ways in which his analyses and conclusions could serve to refute those who would deny the cognitive authority of science. These failures haunted Kuhn's work for the remainder of his career, and they still threaten to undermine the legacy of his project, as he conceived it.

Although Kuhn's "failures" are evident with the benefit of hindsight, if one applies the historical developmental perspective outlined in *Structure* to our historiographic evaluation of the work and its reception, these shortcomings can be understood quite differently. They can be seen as understandable, if unfortunate events within the longer-term development of an alternative paradigm for the nature of science.

The period of "discovery" that supported *Structure's* schematic account extended over a long period of time and across a wide range of investigations. It involved a highly complicated and changing set of relationships between Kuhn and the various communities with which he was involved. In each of these cases, Kuhn was presented with a specific problem for which, in his judgment, his "community" did not seem to have an adequate or complete answer. He then began to examine the problem in a more focused way and to look outside his community for analogies or possible explanations. In some cases, he found these in the work or problems of other communities. In other cases, he developed his own analogies or resolutions, adapting borrowed concepts from other fields as they proved relevant.

The greatest challenges of Kuhn's career emerged during his battle for acceptance, that is, in the responses of various communities to the publication of *Structure*. The schematic nature of the account and the borrowed nature of some of its central concepts seemed to render its theses accessible, relevant, and applicable to some fields yet problematic for others. By not reflecting or addressing the underlying perspectives and problems of these various fields, the account also fostered mistrust, misunderstanding, and misinterpretation. These were compounded by the failure of the account to address the challenges to cognitive authority that it raised. As a result of these issues, Kuhn was not able to establish his new "paradigm" for the nature of science and his potential "revolution" did not occur. At least, that is, not the revolution that he had intended.

Kuhn's theory thus seems to describe the broader historical development of his activities, including his failure to establish the theory. Our investigation suggests that the theory possessed an explanatory power that has not been acknowledged by ongoing debates because it has not been recognized by traditional approaches to the work. On this account, Kuhn's research activities were guided by the puzzle suggested by an anomaly. He developed, proposed, refined and defended a theoretical resolution to this puzzle that undermined key assumptions of traditional theories. Yet the distinctive aspects of his work were not recognized explicitly, and scholars in several fields misconstrued his central notions and systematically misinterpreted his schematic account. As a result, Kuhn's proposed image of the nature of science was not understood fully and its challenges to traditional views were not recognized clearly.

In an ironic twist of fate, the failure of Kuhn's "revolution" thus seems to support some of the most provocative elements of his theory, including the rise of anomaly; the role of paradigms; revolutionary breaks in development; and incommensurability. Yet it also points out the gaps in that theory with respect to the ambiguity of those central concepts; their underlying dynamics and interrelation; and their (as yet unaddressed) implications for the cognitive authority of science and the nature of knowledge. In important respects, the theory presented in *Structure* was not yet developed fully, certainly not to the point that it presents a fully defensible challenge to traditional views and investigations of science. This is particularly the case in terms of the notion of paradigms and the philosophical implications of the work. Furthermore, it is not clear that Kuhn's wide-ranging investigations and varied methods are themselves defensible. In evaluating *Structure's* challenge to the authority of science (or to sociological and philosophical investigations of science), we must also evaluate the authority of *Structure* and its historical developmental approach.

Kuhn's Further (Philosophical) Development

During the thirty four years between the 1962 publication of *Structure* and his death in 1996, Thomas Kuhn attempted to clarify and to develop many of the ideas introduced in the work. As he investigated the sociological, psychological and philosophical aspects and implications of *Structure*, he came to understand more clearly the interpretive debates, concerns, and challenges that had characterized its reception. During the years immediately following the publication of *Structure*, he provided a number of clarifications and elucidations of its theory (**Chapter Four**). In particular, he attempted to address concerns regarding the notion of paradigms and the role of the scientific community in the development of knowledge.

In the mid-1970s, Kuhn began to conduct focused investigations of the philosophical implications of *Structure* (**Chapter Five**). He was drawn, in particular, to the causal theory of reference and its emphasis upon identification of referents through historical acts of dubbing. Attempting to account for the (historical) mechanism that attaches language to nature, he emphasized the relationship between the study of exemplars drawn from experience and the development of a lexicon, or conceptual network of kind terms. While these later studies did not explicitly address the schematic account presented in *Structure*, they implicitly addressed the philosophical issues and concerns that emerged from that account. Viewed within the context of Kuhn's underlying philosophical interests, they represent a series of attempts to develop an alternative basis for the cognitive authority of science.

During the last ten years of his life, Kuhn claimed to have made great progress in exploring the philosophical implications of his work. Unfortunately, the articles published during this period were relatively disparate, and the book that was to present a systematic view of his position was only two-thirds complete upon his death in 1996. Yet even in this later work, Kuhn remained focused on the aims and objectives that had emerged from his Aristotle experience and that had provided the basis for his earlier research. These investigations were more narrowly focused within the realm of philosophy yet still reflected Kuhn's characteristically distinctive approaches as well as his creative challenges to traditional views.

Based on our historiographic investigation of the objectives of Kuhn's project, our historical developmental consideration of the interpretive debates surrounding *Structure*, and our examination of Kuhn's subsequent, philosophical investigations, several important insights and areas for further investigation are evident (**Chapter Six**). These include Kuhn's later emphasis on the historical perspective as supporting a dynamic conception of science, and his claim to have identified (along with other scholars in the historical philosophy of science) the "first principles" of this perspective. In addition, Kuhn's investigations suggest important elements of a "dynamic" form of rationality, which supports the determinations made within the historical (developmental) perspective. Finally, the developmental aspects of Kuhn's work – many of which were not recognized by him fully – suggest that the influence of judgment over time plays a central and consequential role in scientific activity, knowledge, and development.

The central insight of Kuhn's later work is the existence of a deep interrelation between observations or experiences and the conceptual network by which these are understood, examined, and extended. This interrelation is evident only within the context of the historical developmental perspective, that is, the perspective that examines and attempts to account for the change and development of activities and ideas over time. Through the dynamic reconceptualization of science that is suggested by this perspective, revolutionary changes are shown to occur not only in conceptions of science but also in conceptions of nature. These changes are shown to be interrelated, in that revolutionary changes of "science" cannot occur without corresponding changes of "nature."

The interrelated nature of these conceptions suggests a dual-function generalization, by which both the definitional and the legislative aspects of scientific investigations are established simultaneously. These generalizations are to be distinguished from those that rely upon established definitions, that is, from those that are purely legislative. Because they establish definitions of natural phenomena, they must be drawn from observations or experiences and cannot be developed on the basis of laws, theories or rules alone. In order to establish both the definitional and the legislative aspects of these phenomena, they require the juxtaposition of multiple exemplars. This juxtaposition establishes determinations and constraints of both experience and logic, such that the dual-aspects of the generalization (definitional and legislative) are established simultaneously, are mutually determinative and thus are interdependent.

This special type of generalization, or interrelation, provides an alternative basis for first principles such as laws, theories, and rules. It suggests that these conceptual or logical determinations are not independent and objective, but are integrally linked with observations and experience. Furthermore, the revolutionary change from one interrelated set to another, fundamentally different set, serves to undermine the traditional authority of first principles, laws, etc. as reflecting a direct correspondence to truth, or "what is really there." Instead of direct correspondence, the comparative authority of one set vis-à-vis another is based on determinations regarding the comparative "fit" of the observations or experiences that it defines with the conceptual network that it establishes to explain them. This fit is established, refined, evaluated, and overturned by a dynamic form of rationality. Dynamic rationality thus exerts substantial influence through the determinations or judgments that result from its operation over time. It is these determinations that constitute the progress of scientific development.

Our investigation suggests that we must reconsider and revise the legacy of Thomas Kuhn. We must understand the interpretive debates surrounding *Structure* as a broader engagement with emerging issues of cognitive authority and we must acknowledge the role of an historical developmental perspective, the operation of dynamic rationality, and the influence of judgment over time. Contrary to established views, the work of Thomas Kuhn is best characterized as a search for first principles. Kuhn's Aristotle experience drew into question established views of the cumulative development of science and suggested that an alternative account must be based in observation and experience. His subsequent investigations represented his attempt to investigate the issues raised by his Aristotle experience and to develop an alternative account.

Although Kuhn's investigations developed gradually and his resulting descriptive and explanatory accounts were characterized by revisions, retractions, and further refinements, these also presented distinctive and valuable insights regarding both the limitations of established views and the elements required of an alternative account. In this respect, then, Thomas Kuhn should be viewed not as a culturopath, who was blind to the issues and underlying tensions of his day, but as an active and attentive, if, perhaps, overly ambitious scholar, who took the lead in engaging and attempting to address some of the most difficult and fundamental philosophical challenges of his times.

Chapter One

An Historiographic Investigation of Kuhn's Research Activities

In the Preface to *Structure*, Thomas S. Kuhn described the work as "[i]n some part … an attempt to explain to myself and to friends how I happened to be drawn from science to its history in the first place" (*SSR* 1962/1970a, v). He traced his original conception of the project to an experience 15 years earlier, when he was working on his Ph.D. dissertation in physics:

> A fortunate involvement with an experimental college course treating physical sciences for the non-scientist provided my first exposure to the history of science. To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special success. (Ibid.)

Kuhn claimed that the ideas prompted by this experience not only "proved a source of implicit orientation and of some problem structure for much of my more advanced teaching" but also guided his research throughout the remainder of his career (Ibid.). What he later described as his Aristotle experience led to,

> a drastic shift in my career plans, a shift from physics to history of science and then, gradually, from relatively straightforward historical problems back to the more philosophical concerns that had initially led me to history (*SSR*, vii).

It is these problems and concerns that Kuhn sought to address in *Structure*, and it is the development of these research activities into the theory of *Structure* that we examine in our own historiographic/historical developmental investigation.

Overview

Kuhn's research in the history of science was both prompted by and directed toward investigating the puzzle presented by his Aristotle experience. This experience suggested to Kuhn that something was wrong with established views of the nature of science. In particular, it drew into question established views of science as the incremental and cumulative development of knowledge about nature, or "what is really there."

The questions raised for Kuhn by his Aristotle experience centered on the nature of the conceptual changes associated with discoveries of "new" phenomena. Many such discoveries were not simply an incremental addition to the collection of known phenomena, but required the reconstruction of earlier views of nature. Departing from established views, Kuhn proposed that such discoveries are not simply the result of genius, inspiration, or intuition. Nor are they simply the piecemeal correction of past mistakes. Instead, they result from highly directed scientific activity and involve the systematic reconstruction of a wide range of established beliefs about both nature and science. Furthermore, it seemed to Kuhn that these directed investigations exhibited an underlying structure, by which they could be understood within the broader ebb and flow of the development of scientific knowledge over time.

To understand the philosophical implications of his Aristotle experience, Kuhn conducted detailed, historiographic research of notable scientific discoveries. As his research progressed, he began to integrate these detailed case studies into broad, historical developmental accounts of science. These historical developmental accounts encompassed not only the dramatic (often revolutionary) achievements of scientific discovery but also the more incremental (and cumulative) exploration of those discoveries.

Section One

The Aristotle Experience[9]

Thomas Kuhn spent the summer of 1947 preparing a case study on the development of Newtonian mechanics for an experimental, science course in Harvard's General Education program. The course, an overview of science for non-scientists, had been conceived and was to be taught by James B. Conant, President of Harvard University and a chemist by training. The experimental nature of the course lay in Conant's desire to combine the history of science with "an analysis of the various methods by which science has progressed" (Conant 1957, xvi). Rather than provide students with "an overall survey of the history of science in the last 300 years," Conant sought,

> to develop in the student some understanding of the interrelation between theory and experiment and some comprehension of the complicated train of reasoning which connects the testing of a hypothesis with the actual experimental results (Ibid., xvii).

In order to reveal the "complicated train of reasoning" involved in the practice of science, Conant undertook an unusual approach.[10] For the course text, he assigned his book, *On Understanding Science*

[9] Kuhn's autobiographical statements about the formative role of his Aristotle experience included the Prefaces to *Structure* (*SSR* 1962/1970a, v) and *The Essential Tension* (*ET-SS* 1977, xi-i) and the essay "What are Scientific Revolutions?" (WSR 1980/1987/2000, 13-32). His mention of the Aristotle experience in interviews included (Baltas et al. 1995/2000, 255-323). These accounts, written over the course of a 30-year period, differ in their emphasis and detail regarding various aspects of Kuhn's experience. It is probable (if not certain) that their respective emphases and accounts reflect the areas of Kuhn's interest and concern during the periods in which they were written. As such, they may be particularly vulnerable to retrospective rationalization by Kuhn. Nonetheless, the accounts generally are consistent. To the extent that they are used in our historiographic investigation in order to develop an alternative account of Kuhn's work on his own terms, any anachronistic biases will occur with respect to the development of Kuhn's ideas over time. As such, they will prove problematic with respect to the consistency of Kuhn's views but not with respect to our alternative account of his work vis-à-vis other interpretations. Furthermore, these accounts will be integrated with many other types of information, thus any substantive biases are likely to be mitigated by the broader range of information.

[10] In understanding the aims and approaches of this course, is important to remember Conant's positions as a university administrator, a chemist, and an advocate for science policy at the national level. In the Foreword to Kuhn's first book, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (1957), Conant outlined his concern with defining more clearly "the place of the physical and biological sciences in the [university] curriculum" (Conant 1957, xvi). Noting that traditional first-year courses in the sciences had proved inappropriate for students who did not intend to enter into intensive study, he noted that "more emphasis on the history of science has been recommended and this recommendation I have heartily joined" (Ibid.). As we will see, Conant's objective for his experimental course was to use the history of science in order to develop students' appreciation for the complexities of scientific practice.

(Conant 1947), which proposed a case-based method for teaching science to non-scientists. To provide insight into scientific practices, he augmented the text with case studies of major scientific achievements.[11]

The case study method of teaching, which Conant adopted from Harvard's Graduate School of Business Administration,[12] presents students with a stylized recreation of the scientific reasoning that surrounded and supported selected achievements:

> … an original scientific paper is reprinted and forms the basis of the case; the reader is guided by comments of the editors to follow as far as possible the investigator's own line of reasoning. It is left to the professors who use these [case studies] to fit the case in question into a larger framework of the advance of science on a broad front. (Ibid., xvii)

Consistent with Conant's aim for his course, the case study method differs from traditional, textbook pedagogy in outlining the considerations involved in the *development and progression* of scientific reasoning, rather than providing technical descriptions of the scientific achievements in their final, logical, and refined form. It involves extensive class discussion, led by the instructor, whereby students consider the various factors that influence and directs scientific activity, both positively and negatively, toward a given achievement. Students thus learn not only the technical aspects of the achievement itself but also the processes by which it develops over time.[13]

The case study method was particularly well-suited for Conant's experimental course, which was targeted toward students who were non-scientists and, predominantly, World War II veterans. These individuals were older and more experienced than traditional undergraduate students. They were viewed as the rising managerial class, who would come to oversee the country's largest private and public institutions. In Conant's view, it was crucial that these individuals develop an ability to understand and to evaluate scientific activities. For although the war had established science as a major force in society, its position within society and its autonomy from government were still very much in question. Conant was at the

[11] These case studies were developed explicitly for the course. A collection was later published in a series of pamphlets entitled, "Harvard Case Histories in Experimental Science" (Conant 1966).

[12] Interestingly, the case method developed and employed at Harvard's Business school was inspired by the work of William James and Alfred North Whitehead.

[13] As we will see, the case study method seems to provide an interesting, pedagogical alternative that may avoid some of the problems that Kuhn later attributed to textbook science. It has also been established within the broader approach of discussion-based learning, which presents an alternative to the lecture-based method of teaching. For a broader discussion, see (Christensen, Garvin and Sweet 1991).

forefront of science policy discussions, and he saw an important role for universities in educating not only practicing scientists but also the non-scientists who would evaluate (and fund) scientific activities.[14]

Kuhn was one of two assistants whom Conant had asked to assist in the course. At the time, Kuhn was working on his Ph.D. dissertation in theoretical physics, yet he had increasing doubts about pursuing a career within the field. He was unsure about what else he might want to do; was faced with growing "questions about [his] ability;" and felt that he "really had to focus [his] attention in one spot and give [his] full energies to it" (Baltas et al. 1995/2000, 273). It was at this point that Kuhn was asked to assist Conant, an offer which he "accepted with alacrity," noting, "Who the hell wouldn't have taken the chance to work with Conant for a semester?" (Ibid., 274).

In preparing a case study on the development of Newtonian mechanics, Kuhn decided to read Aristotle's *Physics* and some later works that had been derived from it. In his own words, "[w]hen Conant asked me to do this case, which is my first work in history of science in a sense, I started by reading Aristotle to find out what the beliefs had been *before*" (Ibid., 285).[15] Yet in reading Aristotle's *Physics*, Kuhn found that early beliefs about mechanics had little relation to those of the 17th century:

> Like most earlier historians of science, I approached these texts knowing what Newtonian physics and quantum mechanics were. Like them, too, I asked of my texts the questions: How much about mechanics was known within the Aristotelian tradition, and how much was left for seventeenth-century scientists to discover? Being posed in a Newtonian vocabulary, those questions demanded answers in the same terms, and the answers then were clear. Even at the apparently descriptive level, the Aristotelians had known little of mechanics; much of what they had to say about it was simply wrong. No such tradition could have provided a foundation for the work of Galileo and his contemporaries. They necessarily rejected it and began the study of mechanics all over again. (*ET-SS* 1977, xi)

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[14] For a more in-depth discussion, see Conant's Foreword to *The Copernican Revolution* (1957), his autobiography (Conant 1957) and Chapter 3 of Fuller (2000).

[15] It is important to note Kuhn's interest in understanding the beliefs that preceded mechanics. In his own words,

> Conant, in case histories of his own and in his teaching, never I think saw to the extent that I did the need to say what people believed *before*. He would always start in more or less with the beginning of the work. There would be something about it, but there was very little preparation for getting to the person. I always felt you had to do more; and that meant you had to do a stage set, *within another conceptual framework*, in order to get at those things (Baltas et. al. 2000, 275-6, second emphasis added).

As will be discussed subsequently, the source of this approach was, perhaps, Kuhn's earlier study of Kant, whom Kuhn described as "a revelation" and whose notion of the preconditions of knowledge "just knocked me over…" (Ibid., 264). Further, Kuhn's interest in the type of intellectual history presented in A. O. Lovejoy's *The Great Chain of Being* (1936) also contributed to his emphasis upon the broader context of the development of ideas.

For purposes of the historical case study, then, it seemed that Aristotle's theories were not just uninformed or incomplete, but "simply wrong."

Kuhn acknowledged that "[g]eneralizations of that sort were widely current and apparently inescapable" (Ibid.). Yet he would not accept such a judgment at face value:

> But [these generalizations] were also puzzling. When dealing with subjects other than physics, Aristotle had been an acute and naturalistic observer. In such fields as biology or political behavior, his interpretations of phenomena had often been, in addition, both penetrating and deep. How could his characteristic talents have failed him so when applied to motion? How could he have said about it so many apparently absurd things? And, above all, why had his views been taken so seriously for so long a time by so many of his successors? The more I read, the more puzzled I became. Aristotle could, of course, have been wrong – I had no doubt that he was – but was it conceivable that his errors had been so blatant? (Ibid.)

Kuhn was thus presented with a puzzle: how could he reconcile Aristotle's apparent failure as a physical scientist with his tremendous reputation for logic and observation?

With a curiosity and tenacity that ultimately would redirect his career and form the basis of his future work, Kuhn began to search for alternative interpretations that might preserve the Greek's intellectual stature and justify his historical legacy:[16]

> I could easily believe that Aristotle had stumbled, but not that, on entering physics, he had totally collapsed. Might not the fault be mine rather than Aristotle's, I asked myself. Perhaps his words had not always meant to him and his contemporaries quite what they meant to me and mine.
>
> Feeling that way, I continued to puzzle over the text, and my suspicions ultimately proved well-founded. (WSR 1980/2000, 16)

The answer to Kuhn's puzzle came on "[o]ne memorable (and very hot) summer day" (*ET-SS*, xi):

> I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out of the window of my room – the visual image is one I still retain. Suddenly, the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said, and what his authority had been. Statements that had previously seemed egregious mistakes now seemed at worst near misses within a powerful and generally successful tradition. (WSR, 16-7)

This "experience" transformed Kuhn's assessment of Aristotle as a physicist; his understanding of Aristotle's physical theory; and his recognition of the authority on which that theory had been based. In

[16] As we shall see, several of Kuhn's most important insights derived from this willingness to investigate seeming contradictions between (negative, historical) interpretations of scholarly work and perceived achievements in other realms (or from other perspectives). That is, Kuhn was willing to put aside (retrospective) historical evaluations in order to consider the works within their own historical context. This willingness seems to mirror the practices of the "historiographic revolution" within which Kuhn placed himself in the Introduction to *Structure* (*SSR* 1962/1970a, 3). In the terms of the historiographic method, however, Kuhn differed from his predecessors and contemporaries in seeing the various situations as anomalies.

Kuhn's own words, "strained metaphors often became naturalistic reports, and much apparent absurdity vanished. I did not become an Aristotelian physicist as a result, but I had to some extent learned to think like one" (*ET-SS*, xii).

The "Mechanics" of Kuhn's Aristotle Experience

In his "discovery of a way of reading Aristotelian physics . . . that made the texts make sense," Kuhn identified consequential differences in the basic terms and general assumptions of Aristotelian and Newtonian physics (WSR, 17). Most notable among these differences were Aristotle's definition of "motion" (as it relates to various categories and subcategories of change), and the centrality of qualities (rather than matter) within Aristotle's theory:

> When the term "motion" occurs in Aristotelian physics, it refers to change in general, not just to the change of position of a physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of a number of subcategories of motion for Aristotle. Others include growth (the transformation of an acorn to an oak), alterations of intensity (the heating of an iron bar), and a number of more general qualitative changes (the transformation from sickness to health). As a result, though Aristotle recognizes that the various subcategories are not alike in *all* respects, the basic characteristics relevant to the recognition and analysis of motion must apply to changes of all sorts. In some sense this is not merely metaphorical; all varieties of change are seen as like each other, as constituting a single natural family. (WSR, 17)

Thus Aristotle's definition of motion encompasses a broader range of phenomena than that employed by Galileo and Newton. As a result, the characteristics that Aristotle attributed to motion differ in ways that, ultimately, held important consequences for his conception of nature.[17] In addition to this broader definition, Kuhn noted a second and related[18] aspect that seemed to distinguish Aristotle's theory:

> A second aspect of Aristotle's physics – harder to recognize and even more important – is the centrality of qualities to its conceptual structure. By that I do not mean simply that it aims to explain quality and change of quality, for other sorts of physics have done that. Rather I have in

[17] For a discussion of Galileo's presentation of the paradox presented by Aristotle's broad conception of motion, see "A Function for Thought Experiments" (FTE 1964/1977, 240-65, especially 249-61). As indicated by the title of the article, Galileo's presentation was in the form of a thought experiment outlined in his *Dialogue concerning the Two Chief World Systems*. It is important to note that Newton's conception of motion as change of a physical body was a sub-category of Aristotle's conception of motion as a change of quality. Furthermore, Newton made the case for his conception by using a thought experiment to highlight the paradox presented by the Aristotelian conception in particular (problematic) situations.

[18] Kuhn's Aristotle experience involved simultaneous recognition of *both* of these differentiating aspects and of their interrelation. It was the interrelation among the two aspects that provided an alternative (and holistic) basis for Aristotle's theory. As such, it was neither simply Aristotle's broader definition nor his ontological hierarchy but the way in which these two conceptions fit together and formed an holistic world view.

> mind that *Aristotelian physics inverts the ontological hierarchy of matter and quality* that has been standard since the middle of the seventeenth century. In Newtonian physics a body is constituted of particles of matter, and its qualities are a consequence of the way those particles are arranged, move, and interact. In Aristotle's physics, on the other hand, matter is very nearly dispensable. (Ibid., emphasis added)

The distinguishing aspects of Aristotle's theory that underlay Kuhn's "experience" were thus the Greek's definition of motion and the ontological hierarchy of matter and quality, which formed the "conceptual structure" of his physics.

Yet Kuhn noted that it was not singular definitions or assumptions themselves that had provided his moment of insight. Rather, it was the way that

> . . . as one recognizes these and other aspects of Aristotle's viewpoint, they begin to fit together, to lend each other mutual support, and thus to make a sort of sense collectively that they individually lack. In my original experience of breaking into Aristotle's text, the new pieces I have been describing and the sense of their coherent fit actually emerged together (WSR, 18).

Kuhn described this experience of "the pieces suddenly sorting themselves out and coming together in a new way" as "the first general characteristic of revolutionary change" (WSR, 17). He emphasized that this is not a gradual process, which occurs one step at a time. Rather, it is a singular, momentary experience[19] that is truly revolutionary:

> …the central change cannot be experienced piecemeal, one step at a time. Instead, it involves some relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before.[20] (Ibid.)

Kuhn's Aristotle experience was thus a "sudden and unstructured transformation" in which he came to understand Aristotle's physics in a new, and previously inaccessible,[21] way.

Given the immediacy, extent and importance of this transformation, it is crucial to understand the basis on which it occurred and upon which it might be justified.[22] Kuhn described his own experience as the simultaneous determination and "locking together" of two interdependent parts, namely, Aristotle's definition of motion and his attribution of ontological primacy to quality rather than matter:

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[19] Kuhn likened the experience to a "gestalt shift," a concept which came to play an important, albeit ultimately problematic role in his theory. For later discussions of holistic change, see (WSR 1980/2000, 4n) and (CCC 1982/2000, 52 and 57). For corrections to prior discussions of gestalt shift, see (PW 1986/2000, 88-9) and (AW 1990/2000, 242).

[20] It is important to note that Kuhn did not mention one of the most salient aspects of this experience: that it is undergone by an individual. We will return to a consideration of the relationship between individuals and communities in scientific development (see Chapter Four).

[21] As we will see, the seeming inaccessibility of Aristotelian physics when viewed from a Newtonian perspective is directly related to Kuhn's notion of incommensurability.

[22] These questions will later prove to be crucial to Kuhn's defense of his theory against accusations of "irrationality" and "relativism."

> . . . it is precisely seeing motion as change-of-quality that permits its assimilation to all other sorts of change – acorn to oak or sickness to health, for examples. That assimilation is the aspect of Aristotle's physics from which I began, and I could equally well have traveled the route in the other direction. The conception of motion-as-change and the conception of a qualitative physics prove deeply interdependent, almost equivalent notions, and that is a first example of the fitting or the locking together of parts. (WSR, 18)

The two elements of Aristotle's theory that Kuhn initially described are thus interdependent in a way that is not only mutually supportive but also collectively constitutive of the bases for an holistic re-interpretation of Aristotle's theory. That is, Aristotle's definition of motion and his ontology, once locked together, can then be "assimilat[ed] to all other sorts of change." Combining these, or "*seeing* motion *as* change of quality" constituted the central change that transformed Kuhn's understanding of Aristotelian physics.

Kuhn suggested that once he had come to see this alternative pattern of relationships, other, seemingly disparate or nonsensical aspects of Aristotelian physics were thereby rendered consistent and comprehensible:

> If that much is clear, however, then another aspect of Aristotle's physics – one that regularly seems ridiculous in isolation – begins to make sense as well. Most changes of quality, especially in the organic realm, are asymmetric, at least when left to themselves. An acorn naturally develops into an oak, not vice versa. . . . One set of qualities, one end point of change, represents a body's natural state, the one that it realizes voluntarily and thereafter rests. The same asymmetry should be characteristic of local motion, change of position, and indeed it is. The quality that a stone or other heavy body strives to realize is position at the center of the universe; the natural position of fire is at the periphery. That is why stones fall toward the center until blocked by an obstacle and why fire flies to the heavens. They are realizing their natural properties just as the acorn does through its growth. Another initially strange part of Aristotelian doctrine begins to fall into place.
>
> One could continue for some time in this manner, locking individual bits of Aristotelian physics into place in the whole. (WSR, 18-9)

Kuhn ended this discussion by considering Aristotle's doctrine of the vacuum or void, suggesting that "[i]t displays with particular clarity the way in which a number of theses that appear arbitrary in isolation lend each other mutual authority and support" (WSR, 19):

> If position is a quality, and if qualities cannot exist separate from matter, then there must be matter wherever there's position, whatever body might be. But that is to say that there must be matter everywhere in space: the void, space without matter, acquires the status of, say, a square circle.
>
> That argument has force, but its premise seems arbitrary. Aristotle need not, one supposes, have conceived position as a quality. Perhaps, but we have already noted that the conception underlies his view of motion as change-of-state, and other aspects of his physics depend on it as well. If there could be a void, then the Aristotelian universe or cosmos could not be finite. It is just because matter and space are coextensive that space can end where matter ends, as the outermost sphere beyond which there is nothing at all, neither space nor matter. That doctrine, too, may seem dispensable. But expanding the stellar sphere to infinity would make problems for astronomy . . . . The presence of matter is what provides space with structure. Thus, both Aristotle's theory of natural local motion and ancient geocentric astronomy are threatened by

> an attack on Aristotle's doctrine of the void. *There is no way to "correct" Aristotle's views about the void without reconstructing much of the rest of his physics.* (WSR, 19-20, emphasis added)

In considering these examples, Kuhn noted,

> Those remarks, though both simplified and incomplete, should sufficiently illustrate the way in which Aristotelian physics cuts up and describes the phenomenal world. Also, and more important, they should indicate how the pieces of that description lock together to form an integral whole, one that had to be broken and reformed on the road to Newtonian mechanics (WSR, 20).

It is thus, in Kuhn's view, important to understand the ways in which Aristotle's theory simultaneously "cuts up and describes the phenomenal world" while also "lock[ing those pieces] together to form an integral whole." Moreover, Aristotle's particular way of "cutting up" and "locking together" cannot be added to nor simply applied to the "phenomenal world" as conceived by Newtonian mechanics. Rather, the pieces of Aristotle's phenomenal world must be "broken up and reformed on the road to Newtonian mechanics."

When considered from the perspective of Kuhn's Aristotle experience, the path of scientific reasoning from Aristotle to Newton is not cumulative but involves a consequential restructuring. Within each theory, definitions of phenomena and underlying ontology combine to "cu[t] up and describ[e] the phenomenal world" in a way that forms an integral whole. Yet while both the Aristotelian and the Newtonian physics present holistic accounts of the phenomenal world, these accounts differ in their most fundamental conceptions of both that phenomenal world and the behavior of the phenomena within it.

The differences between Aristotelian and Newtonian views include both the conception of motion and, the interrelated, ontological relationship of quality and matter. These central differences are interrelated with other descriptions and conceptions of the phenomenal world. As such, they do not stand independently, cannot be isolated, and thus cannot be changed indiscriminately in the transition from Aristotelian to Newtonian physics. To the contrary, they entail (and also constrain) a number of other assumptions and conceptions that can be inferred on the basis of these assertions and further phenomenological investigations. Thus they form not only the basis for descriptive accounts nor even the foundation of their respective theories. Instead, they form the central core around which the Aristotelian and Newtonian conceptions of the phenomenal world are established: a center whose interlocking pieces are mutually constituting, mutually reinforcing, and, to some extent, mutually justifying.

The Context of Kuhn's Aristotle Experience

Throughout his career, Kuhn repeatedly reflected on the formative influence of his Aristotle experience. These reflections reveal that while the Aristotle experience led Kuhn to shift his career plans from physics to history of science, his interests in doing so were not historical but philosophical. In investigating the philosophical basis of Kuhn's interests, it is thus important to understand (as Kuhn typically sought to do) what his beliefs were *before* his Aristotle experience.

In an in-depth interview conducted in October 1995, Kuhn identified two courses from his freshman year at Harvard that had proven to be particularly influential (Baltas et al. 1995/2000, 261-4). The first was a "rapid physics" course, designed for future majors; the second was a course in the history of philosophy. To his surprise, Kuhn initially had difficulty with both courses; however, the ways in which he dedicated himself to improving his grades strongly influenced his future approach to scholarship. In responding to his physics professor's admonition to do better on exams, Kuhn "started really learning how to do problems. We called them problems, I call them puzzles" (Ibid., 262). This problem-solving approach came to be a predisposition for Kuhn: "I think part of my emphasis on solving problems, puzzles, may come from, or have been affected or prepared by this" (Ibid., 261).[23]

In philosophy, Kuhn's initial difficulties spurred him to "really lear[n] something more about how to study. I went back and made detailed notes and really pinned myself to it. . . I was quite fascinated by this stuff although I didn't understand it terribly well" (Ibid., 263-4). Kuhn was accepted into the honors section of the course during his spring semester, and found that while "Spinoza didn't hit me very hard . . . [and] I could understand [Descartes and Hume] easily; Kant was a revelation" (Ibid., 264). In particular, he was fascinated by "Kant's notion of the preconditions for knowledge. Things that had to be the case because you wouldn't be able to know things otherwise. . . . it just knocked me over, that notion. . ." (Ibid.).

[23] The influence of this predisposition can be seen in Kuhn's proposal in *Structure* that normal science was a "puzzle-solving" activity (*SSR* 1962/1970a, Chapter IV). In comparing his views with those of Popper, Kuhn proposed that puzzle-solving ability is the distinguishing characteristic of science (LDPR 1965/1970b, 7). This proposal distinguished Kuhn from the philosophers of science (responding to logical positivism), who proposed that the demarcation point of science is testability, verification, or falsification (Watkins 1970, 29-30).

As we will see, this sensitivity to the preconditions for knowledge likely brought an added dimension to Kuhn's focus on solving (scientific) problems: namely, the view that it is not sufficient merely to solve problems but one must also understand the preceding conditions that are (ostensibly) required for their solution. Through the course of his research, Kuhn combined these Kantian considerations with an historiographical or developmental approach, seeking not only to understand preceding conditions but also tracing changes in those conditions in order to outline the processes involved in the (sometimes revolutionary) development of ideas over time. Kuhn later noted that his first encounter with Kant was "an important story because I go round explaining my own position saying I am a Kantian with moveable categories" (Ibid., 264).

Kuhn's studies of both physics and philosophy were interrupted by the advent of World War II. His physics courses were re-directed from the traditional physics curriculum to the study of electronics, and his undergraduate program was accelerated so that he graduated from Harvard in three years (including two summer courses). As a result of the accelerated program, Kuhn took fewer non-science subjects than he had planned.[24] Yet he satisfied his broader interests through extracurricular activities, most notably his involvement in the Signet Society[25] (of which he was President during his senior year) and the student paper, the *Harvard Crimson* (of which he headed the Editorial Board as a senior). These outside activities both served as an outlet for Kuhn's interests and raised his visibility as a student who "was known [by the administration] for this range of interests" (Ibid., 275). Kuhn later surmised that it was for this reason that Conant had requested his assistance in the General Education science course, widely described as "physics for poets."

After graduating from Harvard, Kuhn began working on radar countermeasures for the Radio Research Laboratory. Working at the Laboratory on the Harvard campus and later, at various locations in Europe, Kuhn found the work "fairly dull" and began developing "a bad taste of what it was going to be like to be a physicist" (Ibid., 271-2). Yet particularly during his time in Europe, Kuhn "had what was basically a 9 to 5 job; and I suddenly had time to read. And I started reading what I took to be philosophy

[24] Kuhn took only one history course, on British nineteenth century history and sat in some of Sarton's lectures on the history of science, which he found "turgid and dull" (Baltas et al. 1995/2000, 275).
[25] The Signet Society was an elected group, which was "sort of an intellectual discussion society" generally filled with students in the humanities rather than the physical sciences (Baltas et al. 1995/2000, 268). Kuhn noted "…I was probably the only physicist who was ever president of the Signet" (Ibid.).

of science" (Ibid., 305). Kuhn's readings at the time were drawn from the English logical empiricist tradition, including Bertrand Russell's *Knowledge of the External World*, work by von Mises, Philip Frank, and (selected works of) Rudolf Carnap.[26]

Following the victory in Europe, Kuhn returned to work at the Research Laboratory in Cambridge, expecting to be shipped to Japan in short measure. While waiting for the expected orders (which never came), he enrolled in physics courses at Harvard. He soon decided to complete graduate work in physics at Harvard, although the decision was one of convenience and continuity rather than dedication and interest:

> Because I had taken so much physics as an undergraduate, because I was back at Harvard where I hadn't planned to go to graduate school – but it would have been silly not to, given the continuity – it would have cost me at least a year to pick myself up and go somewhere else." (Ibid., 272-3)

Although Kuhn enrolled in graduate work in physics, it was clear that his interests lay elsewhere. He petitioned the physics department to take half of his first year courses in other fields in order "to explore other possibilities . . . philosophy in particular" (Ibid., 273). He took two courses in philosophy yet found that,

> . . . there was just a lot of philosophy I hadn't been taught, and didn't understand, and was not finding it very palatable to pick up this way. I didn't know quite why people were doing the things that they were doing. And I fairly rapidly decided, yes, I was interested in philosophy, but my God, I was a graduate, I had been through war in some sense or other, I couldn't go back and sit still for that undergraduate chicken-shit and go on from there. (Ibid.)

Thus Kuhn decided to get his degree in physics and to end his formal study (if not his underlying interest) in philosophy.[27]

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[26] These readings formed Kuhn's perspective of positivism, a perspective which he later suggested was incomplete:

> the question as to where I got the picture that I was rebelling against in *The Structure of Scientific Revolutions*. And that's itself a strange and not altogether good story. Not altogether good in the sense that I realize in retrospect that I was reasonably irresponsible." (Baltas et al. 1995/2000, 305)

Kuhn later attributed his "irresponsibility" to the limited nature of his understanding of positivism: "And it was against that sort of everyday image of logical positivism – I didn't even think of it as logical empiricism for a while – it was that that I was reacting to when I saw my first examples of history" (Ibid., 306). He later acknowledged important similarities between the theory of *Structure* and the later work of Rudolf Carnap; however, he maintained that Carnap continued to miss the "constructivist aspect" and did not pay sufficient attention to developmental considerations (AW 1990/2000, 227) or to the significance of language change.

[27] Kuhn noted that, ". . . in certain respects I'm extremely glad I didn't [study undergraduate philosophy], because I would have been taught things that would have given me a cast of mind which would have, in many ways, helped me as a philosopher, but they'd have made me into a different sort of philosopher" (Baltas et al. 1995/2000, 281). When viewed from *Structure's* account of development, formal study of philosophy might have hindered Kuhn's ability to see the limitations of traditional views. Yet based on the responses to *Structure* by philosophers of science, it is clear that formal study of philosophy might have helped Kuhn to make a stronger, clearer, and more defensible case for his alternative to those views.

Kuhn's (abandoned) foray into the study of philosophy occurred in the fall of 1945. For the next year and half, he rededicated himself to graduate work in physics; however, his interest in the field began to flag considerably. He focused on completing the degree and took the most expedient path to do so:

> By the time I decided on a thesis topic, I was quite certain that I was not going to take a career in physics, and I didn't want to prolong my time in graduate school. Otherwise I would have shot for a chance to work with Julian Schwinger [a famous Harvard physicist], but there were a lot of things I didn't know, and would have had to study, if I were to do it that way. I wanted the degree – it would have been stupid to have gone that far and not to get the walking papers that would result. But I didn't want to do the amount of extra training. (Ibid., 274)

Kuhn described himself during this period as being

> . . . very much of two minds; partly it was simply I didn't know what I would do if I didn't do physics. I was looking at thinking about other things, none of which turned me on all that much. . . . And then, of course, I had this extraordinary experience which I've talked about, of being asked by Conant to assist in his course" (Ibid., 273-4).

With this contextual aside completed, we return to Kuhn's Aristotle experience and its implications.

The Implications of Kuhn's Aristotle Experience

Kuhn's Aristotle experience was important to him not simply for the recognition that Aristotle's physical theory differed from Newtonian mechanics in fundamental ways but, more importantly, for the philosophical implications of that insight. In this respect, Kuhn's studies in the history of science and the development of scientific ideas were a means to a (philosophical) end. He admittedly "had no real interest in history," and even his Aristotle experience "didn't really get [him] *interested* in history of science" (Baltas et al. 1995/2000, 275-6). Rather, Kuhn emphasized later that

> . . . my objectives in this, throughout, were to make philosophy out of it. I mean, I was perfectly willing to do the history, I needed to prepare myself more. I wasn't going to go back and try to be a philosopher, learn to do philosophy; and if I had, I'd have never been able to write that book! But my ambitions were always philosophical. (Ibid., 276)

By the end of his semester teaching with Conant, Kuhn had decided to redirect his career from physics to the history of science: "At the end of that time I knew what I wanted to do. I wanted to teach myself enough history of science to establish myself there in order to do the philosophy" (Ibid.).

"The philosophy" that Kuhn sought to do centered on "'what it is to *be* true,'" a question which his Aristotle experience "certainly made . . . problematic" (Ibid., 278).[28] For how could Aristotle's physics, on one reading, reflect "egregious mistakes" yet on another reading, present "at worst near misses within a powerful and generally successful tradition" (WSR, 16-7)? The first assessment is, Kuhn noted, "widely current and apparently inescapable," particularly from the perspective of philosophy of science at that time (*ET-SS*, xi). Yet the second assessment, which requires important conceptual reconfigurations, suggests that Aristotelian physics possesses a form of "authority" that typically goes unrecognized and does not correspond to traditional notions within the philosophy of science (WSR, 16). How can these drastically different assessments be explained? What are their implications for philosophy of science in general and the "authority" of science in particular? At an even broader level, what does the reconfiguration that is required to move between Aristotelian physics and Newtonian mechanics suggest about scientific development and "what it is [for a scientific theory] to *be* true?" As we will see, these questions guided Kuhn's investigations throughout his (now-redirected) career.

[28] In discussing this interest, Kuhn recounted a cocktail party in New York in which, "I was heard saying (including by me), 'I just want to know what Truth is!'" (Baltas et al., 278). Kuhn commented on the event, "that's what it meant to me to be studying philosophy or to have philosophical ambitions, that was the type of thing it meant to me" (Ibid.). Yet he also pointed out, "remember, when I said that, I wasn't saying that I want to know what is true; I was saying I want to know what it is to *be* true. And that's not something that one gets to through physics" (Ibid.). In making the distinction between what is true and what it is to *be* true (and linking the latter to philosophy, rather than physics), Kuhn was, perhaps, making an indirect reference to his fascination with Kant and the preconditions of knowledge.

Section Two

Study and Research in the History of Science

To investigate the implications of his Aristotle experience, Kuhn switched his field of research from physics to the history of science, in particular, the emerging field of internalist historiography, which was being developed by Alexandre Koyré and other scholars interested in the history of ideas and its application to scientific knowledge.[29] The new historiography examined historical scientific achievements within the context of the knowledge, theories, beliefs, instruments, and assumptions of their time, independent of contemporary knowledge and beliefs. This was accomplished by investigating the conduct of actual scientific activity, as revealed by the scientific writings of the period. The field was developed in order to highlight and to resolve the "Whiggish" biases found to be present in earlier approaches, which viewed historical scientific achievements through the lens of contemporary knowledge and beliefs.

While Kuhn adopted the historiographic approach, he differed from other historiographers in his underlying interest in the philosophical implications of his Aristotle experience. By focusing his research on discoveries, in particular, discoveries of "new" phenomena, Kuhn sought to study situations that were similar to his Aristotle experience. Thus even in his earliest work, Kuhn's objectives were not simply to discern the historical integrity of a particular scientific achievement but to understand the underlying structure or factors that seemed to trigger all achievements of that (general) "type." Each of his early publications presents a highly focused, detailed, and systematic historiographic examination of a particular scientific discovery. Yet viewed collectively, this early work also reflects an orientation toward the more generalized and philosophical issues of scientific discovery and development raised by his Aristotle experience.

The Transition to the History of Science

After deciding to switch his career from physics to the history of science, Kuhn asked President Conant for a recommendation to Harvard's Society of Fellows in order to accomplish the transition successfully and

[29] Many of these scholars were trained in the Continental tradition of philosophy rather than the history of science or even history.

relatively seamlessly. The Society included 24 Junior Fellows, with 8 fellows elected each year for a three-year period of research. In his application, Kuhn indicated that,

> . . . there was important philosophy to come out of [this study of the history of science]; but I needed first to learn more history, to do more history, and to establish myself professionally as a historian before I let the cat out of the bag" (Baltas et al. 1995/2000, 281).

Kuhn was admitted to the program and, after finishing his Ph.D. dissertation in physics, entered the Society in November 1948.

Perhaps surprisingly, Kuhn had limited interaction with scholars in the history of science during his three years in the Society of Fellows. Although Harvard had no formal department in the history of science at the time,[30] several prominent scholars were at the university, namely, George Sarton and Bernard Cohen. Sarton had authored a comprehensive survey of the history of science (1937); however, his views of science and its history differed from Kuhn's interests in important respects:

> My notion was that there was a sort of history of science to do that Sarton wasn't doing. . . he certainly was a Whig historian and he certainly saw science as the greatest human achievement and the model for everything else. And it wasn't that I thought that it was *not* a great human achievement, but I saw it as one among several. I could have learned a lot of data from Sarton but I wouldn't have learned any of the sorts of things I wanted to explore. (Baltas et al. 1995/2000, 282)

Bernard Cohen's approach to the history of science differed from Sarton's, and Kuhn noted that he had "done a lot of good for the history of science" (Ibid., 283). Yet while Cohen had recognized some of the problems implicit in Sarton's approach, Kuhn noted that he "is not someone who thinks about development at all in the way that I do" (Ibid.).

The Problems of Whig History

The situation that faced Kuhn at Harvard reflected broader changes that were occurring in the history of science, in particular, growing recognition of the problems inherent in traditional approaches to the study of history. In his 1968 essay, "The History of Science" (HS 1968/1977, 105-26) and his 1971 essay, "The Relations Between History and the History of Science" (RHHS 1971/1977, 127-61), Kuhn outlined the two main traditions that had dominated the history of science until the beginning of the twentieth century. The first, within which George Sarton was still a prominent figure (following Condorcet, Comte and Dampier),

[30] At the time, there were only two history of science programs in the world: University College in London and the University of Wisconsin (Baltas et al. 1995/2000, 285).

viewed science as "the triumph of reason over primitive superstition, the unique example of humanity operating in its highest mode" (RHHS, 148). Kuhn noted, perhaps a bit unfairly, that,

> . . . the chronicles which this tradition produced were ultimately hortatory in intent, and they included remarkably little information about the content of science beyond who first made which positive discovery when." (Ibid.)

While noting that historians of science owe these scholars "an immense debt for [their] role in establishing their profession," Kuhn stated that "no contemporary historian of science reads [their works]"[31] (Ibid.). Even further, he asserted that "the image of their specialty which [Sarton and others] propagated continues to do much damage even though it has long since been rejected" (Ibid.).

The most influential critique of this tradition was presented in Herbert Butterfield's *The Origin of Modern Science* (1949/1957), which noted the problems associated with what he termed "Whig history." According to Butterfield, traditional approaches to the history of science, such as those undertaken by Sarton, presume that science reflects cumulative development toward the truth. As such, their investigations examine history as presenting an inevitable, accumulative path of progress. Their works are, then, essentially retrospective rationalizations that function less as in-depth scholarship than anachronistic justifications of current practices.

The second major tradition, represented at Harvard by Bernard Cohen, was dominated at that time by practicing scientists, who prepared histories, "as a means to elucidate the contents of their specialty, to establish its tradition, and to attract students" (RHHS, 148). While noting that the resulting works are often technical and thus still valuable for specialists with "different historiographic inclinations," Kuhn suggested that seen as history, this approach has two major shortcomings (Ibid.). The first is that these histories are exclusively internal, with little consideration of the "context for. . . [or] external effects of, the evolution of the concepts and techniques being discussed" (Ibid.). As such, Kuhn concluded, they have limited relevance for historians, with the possible exception of historians of ideas.[32]

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[31] This comment and profile of the development of the history of science is interesting in light of Kuhn's proposal in *Structure* that advocates who continue to support an overturned paradigm are simply "read out" of the field, in that their works are no longer deemed relevant and are thus no longer studied (*SSR* 1962/1970a, 159).

[32] Kuhn justified the possible exception of historians of ideas in stating that, "That limitation [of an exclusively internal focus] need not always have been a defect, for the mature sciences are regularly more insulated from the external climate, at least of ideas, than are other creative fields" (RHHS, 148-9). This exception becomes important when considering the work of Alexandre Koyré and other scholars of

Kuhn proposed that the tradition is limited in a second respect in that these "scientist-historians . . . characteristically imposed contemporary scientific categories, concepts, and standards on the past" (RHHS, 149). This anachronistic reconceptualization not only is historically misleading but also disguises important elements of historical development and interpretation:

> . . . they usually treated concepts and theories of the past as imperfect approximations to those in current use, thus disguising both the structure and integrity of past scientific traditions. Inevitably, histories written in this way reinforced the impression that the history of science is a not very interesting chronicle of the triumph of sound method over careless error and superstition. (Ibid.)

The problems that Butterfield had raised with respect to "Whig history" thus were also apparent within this tradition of the history of science.

In Kuhn's view, the transgressions of scientist-historians were less biased than those of Sarton's tradition because they reflected a deeper understanding of the internal workings of science. As such, their errors were more specific. Rather than highlighting science as the triumph of reason over superstition, they highlighted the triumph of *sound (scientific) method*.[33] Kuhn noted that the problems associated with their work lie both in their characterization of earlier views as mere "superstition" and in their attribution of "triumph" to method:

> In short, ideas which the ["Whig"] historian dismisses as superstitions usually prove to have been crucial element in highly successful older scientific systems. When they do, the emergence of novel replacements will not be understood as the consequence merely of good method applied in a favorable intellectual milieu. (RHHS, 140)

In Kuhn's view, this second tradition, while an improvement over the first, nonetheless suffered from inherent weaknesses in its approach to the history of science.

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internalist historiography (who established a third tradition in the history of science at the beginning of the twentieth century).

[33] This emphasis seemed appropriate given scientist-historian's objectives. For it is important to note that the history of science was, at the time, small and struggling not only for acceptance but also for definition. Within universities, history of science courses shifted back and forth between departments of history and science. Kuhn noted that in the early 1970s, most history of science programs were based, rightly, within history departments. Yet he puzzled over the facts that the majority of students in these courses were scientists or engineers and that the impetus for providing such courses typically emanated from the sciences or philosophy rather than from history (RHHS, 129).

The history of science's institutional homelessness and search for acceptance seemed to reflect the field's search for relevance as well as the emergence of competing objectives and approaches. With the growing influence of science came greater emphasis on general education in the sciences for non-scientists. It was within this broader context (and with these broader objectives) that James Conant developed his course in science for non-scientists. As we will see, Conant's approach proved extremely influential in defining Kuhn's views of and approaches to the history of science, although it did not exert a long-term impact on the field itself.

The "Whiggish" aspects of these two traditions in the history of science were drawn into question by Kuhn's Aristotle experience. From that experience, Kuhn came to see Aristotle's physics as neither simply "wrong" nor wholly embraced by the theories of Newtonian mechanics. The transition from Aristotle to Newton thus did not seem to illustrate either the triumph of reason over superstition, or even the triumph of sound, scientific method. Rather, the transition seemed to reflect processes that are more complex, yet typically suppressed when considering Aristotle from a contemporary, Newtonian perspective.

Given Kuhn's interest in understanding the philosophical implications of both this transition and his Aristotle experience, his reticence to study the history of science with either Sarton or Cohen is more comprehensible than it might seem initially. This reticence also provides an indication of Kuhn's single-mindedness with respect to his intellectual pursuits.

The Historiographic Revolution

The approach to the history of science that seemed to suit Kuhn's interests most closely was developed by Alexandre Koyré. Koyré had been trained in philosophy rather than history, and was a part of the European post-Kantian tradition that considered the relationship between philosophy and history.[34] Perhaps for this reason, his work extended beyond the traditional boundaries of the history or the philosophy of science as established in the Anglo-American tradition of the time. Koyré sought not only to describe the history of science but also to explain the internal factors[35] underlying its development. It was this aspect of Koyré's work that resonated particularly strongly with Kuhn:

> And very shortly after [reading Aristotle], and it was at Bernard Cohen's suggestion, I picked up Koyré's *Études Galiléenes*, I loved them. I mean, this was showing me a way to do things, but I just hadn't imagined it was there. In a sense it wasn't quite so strange as it might have been, because I had read and admired a good deal Lovejoy's *Great Chain of Being*. But that you could do that with *science* had not quite occurred to me, and this is what Koyré was in some sense showing me. And that was important. (Baltas et al. 1995/2000, 285)

[34] See (*ET-SS* 1977, xv) and (HS 1968/1977, 108) for further details.

[35] It is important to note here that Koyré's method was strongly "internalist," that is, focused on the development of ideas within the field. Yet, as we will consider in detail subsequently, Koyré later noted of Kuhn that he had "'brought the internal and external histories of science, which had previously been far apart, together'" (Baltas et al. 1995/2000, 285). Kuhn noted of this comment, "Now, I hadn't thought of that at all as what I was doing. I saw what he meant, and coming from him it was particularly agreeable because he had been so anti-external history; his gifts were as an analyst of ideas." (Ibid., 286).

In the work of Koyré, Kuhn thus found a way to combine the history of science with an investigation into the philosophical development of scientific ideas within their own time.

Also during his tenure at the Society, Kuhn encountered the work of Emile Meyerson,[36] Hélène Metzger, and Anneliese Maier, who, like Koyré, exerted a tremendous influence on Kuhn's subsequent work. In the Preface to *Structure*, Kuhn outlined the contributions of these scholars:

> More clearly than most other recent scholars, [Alexandre Koyré, Emile Meyerson, Hélène Metzger, and Anneliese Maier] ha[ve] shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today. Though I increasingly question a few of their particular historical interpretations, their works, together with A. O. Lovejoy's *Great Chain of Being*, have been second only to primary source materials in shaping my conception of what the history of scientific ideas can be. (*SSR*, vi)

In considering this quote, it is important to note Kuhn's emphasis on the history of scientific ideas, rather than simply the history of science or of scientific achievements. As noted previously, the scholars who developed the historiographic approach were distinguished by a guiding interest in the history and development of ideas.

Toward the end of his second year, Kuhn and his wife traveled to Europe in order to meet with colleagues in England and France. During the trip, he met Mary Hesse, A. C. Crombie, Mackie, Heathcote and Armytage, who taught in the history of science program at University College (Baltas et al. 1995/2000, 285). He also met with Stephen Toulmin in England and with Bachelard, to whom Alexandre Koyré had recommended him, in France.[37]

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[36] Kuhn was referred to Meyerson's book, *Identity and Reality*, by Karl Popper in his third year at the Society. He later noted, "I didn't like [Meyerson's] philosophy at all. But boy, did I like the sorts of things he saw in historical material. He went into those briefly and I mean he didn't do it as a historian but he was getting it right in ways that were different from the ways that history of science was being written" (Baltas et al. 1995/2000, 287). In examining developments in the history of science, Kuhn noted that Meyerson, along with Brunschvicg and "a small group of neo-Kantian epistemologists" had played a formative role. In particular, Kuhn commented that their "search for quasi-absolute categories of thought in older scientific ideas produced brilliant genetic analyses of concepts which the main tradition in the history of science had misunderstood or dismissed" (HS, 108).

Kuhn's later description of himself as a "Kantian with moveable categories" reflects both his affinity to and his differences from Meyerson: he sought to apply Meyerson's genetic approach to a search for the *changing* categories of thought in historical scientific ideas (Baltas et al. 1995/2000, 287). Thus Fuller's indictment of Kuhn as an advocate of Meyerson's absolutist conclusions neglects not only Kuhn's explicit statements to the contrary but also the nature of Kuhn's work and interests [see (Fuller 2000, 392-5)].

[37] Kuhn noted that Koyré was not in France during this trip; however, he had met him earlier during a visit to Harvard. Of the meeting with Bachelard, Kuhn later commented, "he was a figure who was seeing at least some of the thing. He was trying to put it into too much of a constraining . . . . He had categories, and methodological categories, and moved the thing up an escalator too systematically for me. But there were things to be discovered there that I did not discover, or did not discover in that way" (Baltas et al. 1995/2000, 285).

During his third year in the Society, Kuhn met Karl Popper, who had been invited to Harvard for the William James Lectures. Of this first encounter, Kuhn later recounted,

> I had had reason to think I was going to like these [lectures] and I was clearly interested in it. I was introduced to Popper at a fairly early stage and we saw a little bit of each other. Popper was constantly talking about how the later theories *embrace* the earlier theories, and I thought that was not just going to work out quite that way. It was too positivist for me. (Ibid., 286)

Interestingly, Kuhn's comments seemed to reflect his Aristotle experience, which suggested that Newton's theories do not *embrace* those of Aristotelian physics but require a consequential restructuring of its most fundamental definitions and its ontology.

"Random Exploration" of Related Fields

During his post-graduate fellowship at Harvard, Kuhn devoted his attention not only to the study of the history of science but also to the exploration of fields "without apparent relation to history of science but in which research now discloses problems like the ones history was bringing to my attention" (*SSR*, vi). Kuhn noted that this "random exploration" led him to works to which he was "indebted . . . in more ways than I can now reconstruct or evaluate" (*SSR*, vii):

> A footnote encountered by chance [in Robert Merton's graduate thesis] led me to the experiments by which Jean Piaget has illuminated both the various worlds of the growing child and the process of transition from one to the next. One of my colleagues set me to reading papers in the psychology of perception, particularly the Gestalt psychologists; another introduced me to B. L. Whorf's speculations about the effect of language on world view; and W. V. O. Quine [who was a Senior Fellow in the Society of Fellows] opened for me the philosophical puzzles of the analytic-synthetic distinction. That is the sort of random exploration that the Society of Fellows permits, and only through it could I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel 1935), an essay that anticipates many of my own ideas. (*SSR*, vi)

As we will see, these various scholars formed an important link in the development from Kuhn's Aristotle experience to the ideas presented in *Structure*. Influenced heavily by the issues raised by his Aristotle experience, Kuhn's investigations were not necessarily as "random" as he suggested.

With respect to Jean Piaget, Kuhn noted in particular, *The Child's Conception of Causality* (1930) and *Les notions de mouvement et de vitesse chez l'enfant* (1946) "because they displayed concepts and processes [of transition from one world to the next] that also emerge directly from the history of science" (*SSR*, vi). Kuhn outlined these concepts and processes of transition in his 1971 essay, "Concepts of Cause in the Development of Physics" (CCDP 1971/1977, 21-30), an invited address to an audience of child psychologists. In that essay, Kuhn highlighted, "the ineradicable traces of Piaget's influence" (CCDP, 22) on his work, noting that, "[p]art of what I know about how to ask questions of dead scientists has been learned by examining Piaget's interrogations of living children:"

> His perceptive investigations of such subjects as the child's conception of space, of time, of motion, or of the world itself have repeatedly disclosed striking parallels to the conceptions held by adult scientists of an earlier age. (CCDP, 21)

Kuhn later noted, ". . . these children develop ideas just the way scientists do, except – and this was something I felt Piaget did not himself sufficiently understand, and I'm not sure that I realized it early – they are being taught, they are being socialized, this is not spontaneous learning, but learning what it is that is already in place. And that was important" (Baltas et al. 1995/2000, 279).[38] As we will see, it is in this respect that the scientific community plays an important role in the education of students and new members.

In studying the psychology of perception, Kuhn did not specify which of the Gestalt psychologists he had read. In *Structure*, he characterized the immediacy with which a new paradigm is perceived as a "gestalt shift" and suggested that it typically accompanied scientific discovery (*SSR*, 111). Yet he later qualified this early characterization, distinguishing between the historian's experience of a gestalt shift in coming to understand an out-of-date text and the slower, gradual process by which scientific discovery occurs (CCC 1982/2000, 57). In later reflections, Kuhn acknowledged that initially he had leaned too hard on gestalt switches (as an explanation of the shift that occurred with scientific discovery) because he didn't know enough about meaning (Baltas et al. 1995/2000, 229).

[38] In particular, Kuhn emphasized the importance of Piaget's distinction between a narrow conception of cause (characterized by an active agent and similar to Aristotle's efficient cause) and a broad conception, described as "the general notion of explanation:"

> To describe the cause or causes of an event is to explain why it occurred. Causes figure in physical explanations, and physical explanations are generally causal [in the narrow sense]. Recognizing that much, however, is to confront again the intrinsic subjectivity of some of the criteria governing the notion of cause. Both the historian and the psychologist are well aware that a sequence of words that provided an explanation at one stage in the development of physics or of the child may lead only to further questions at another. . . . A specified deductive structure may be a necessary condition for the adequacy of causal explanation, but it is not a sufficient condition. When analyzing causation, one must therefore inquire about the particular responses, short of *force majeur*, that will bring a regress of causal questions to a close" (CCDP, 23).

Piaget's distinction was important for Kuhn in that it acknowledges the role of a narrow conception of causality as a necessary condition, yet also highlights the importance (and necessity) of a broader, more general conception, which delineates sufficient conditions and whose criteria are, at least in some respects, subjective.

B. L. Whorf's work, later collected in *Language, Thought, and Reality – Selected Writings of Benjamin Lee Whorf* (Carroll 1956), was among Kuhn's first introductions to the effects of language on world views. Kuhn's Aristotle experience had introduced him to (what he initially characterized as) two opposing "world views," separated in part by differences between Aristotle and Newton in their definition of "motion." Over the course of his career, Kuhn increasingly emphasized the role of language within the context of such problems.

W. V. O. Quine was a Senior Fellow during Kuhn's tenure in the Society of Fellows and published "Two Dogmas of Empiricism" (1953) during Kuhn's third year in the program. Kuhn later noted two ways in which Quine had proven influential:

> . . . [Quine] had a considerable impact on me because I was wrestling already with the problem of meaning, and at least to discover that I didn't have to be looking for necessary and sufficient conditions was extremely important. Quine has been important to me for that piece, and [later] for the problems that *Word and Object* [(1960)] presented me in trying to figure out why I was so sure it was wrong . . . where he was going off the rails. (Baltas et al. 1995/2000, 279-80)

Quine's paper on the analytic-synthetic distinction was important to Kuhn because it helped to undermine aspects of the theory-observation distinction which his Aristotle experience had drawn into question. Later, as Kuhn began to characterize his project in terms of the problem of meaning, Quine's views on translation in *Word and Object* were especially provocative.

Finally, Kuhn encountered a reference to Ludwik Fleck's *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* [*Genesis and Development of a Scientific Fact* (1979)] while reading Hans Reichenbach's *Experience and Prediction* (1938). The reference proved to be an important confirmation of Kuhn's investigations:

> I said, my God, if somebody wrote a book with that title – I have to read it! These are not things that are supposed to have … they may have an *Entstehung* [Genesis] but they are not supposed to have an *Entwicklung* [Development]. I don't think I *learned* much from reading that book, I might have learned more if the Polish German hadn't been so very difficult [Kuhn supported its translation into English, for which he authored the Foreword]. But I certainly got a lot of important reinforcement. There was somebody who was, in a number of respects, thinking about things the way I was, thinking about historical material the way I was. (Baltas et al. 1995/2000, 283)

In the Preface to *Structure*, Kuhn described Fleck's work as "an essay that anticipates many of my own ideas" (*SSR*, vii). Further, he noted that "together with a remark from another Junior Fellow, Francis X.

Sutton, Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community." (Ibid.).[39]

The Lowell Lectures of 1951

In March 1951, during his final year at the Society, Kuhn presented his ideas in a series of public lectures for the Lowell Institute in Boston, under the title, "The Quest for Physical Theory." He intended for the lectures to combine his readings in the history of science and other fields with his philosophical interests. Yet Kuhn found the undertaking too ambitious for the occasion: "I had a dreadful time preparing [the lectures] and I nearly cracked up. But I got through them. What I was trying to do was to write the *Structure of Scientific Revolutions* in three lectures" (Baltas et al. 1995/2000, 289).

The lecture series had two important outcomes for Kuhn. First, he rededicated himself to the study of the history of science:

> …the primary result of that venture was to convince me that I did not yet know either enough history or enough about my ideas to proceed toward publication. For a period that I expected to be short but that lasted seven years, I set my more philosophical interests aside and worked straightforwardly at history. (*ET-SS*, xvi)

A second outcome was both more substantive in nature and more encouraging in its immediate effects. One of Kuhn's three lectures traced the role of atomism in the development of science, in particular, what Kuhn took to be its "transforming influence in the seventeenth century" (Baltas et al. 1995/2000, 289).

In the lecture, Kuhn traced the development from Epicurean and Democritean atomism to the atomism of the seventeenth century. He described the former as "those ancient and medieval atomisms which took the atoms to be indivisible *but which had built into them Aristotelian qualities . . . so that atoms are fire, air, earth, and water. . .* " (Ibid., 290, emphasis added). In contrast, atomism in the seventeenth century was "a matter and motion atomism," which had no built-in qualities and which thus provided "a natural basis for transmutation" (Ibid.). To trace the historical development of these theories, Kuhn looked to Boyle (1952) and to Newton's thirty-first query (1951), investigations that formed the bases of his first

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[39] It is interesting to note, however, that Kuhn later claimed that he was "bothered" by Fleck's notion of the "thought collective," as a group that was modeled on the mind and the individual. (Baltas et al. 1995/2000, 283)

two published articles. In both cases, Kuhn found that the theorists identified and isolated an anomaly, which, when pursued, led them to propose the vastly different account of nature.

Perhaps most importantly, however, Kuhn noted of these early investigations, "what I gradually discovered was that nobody knew nearly as much about this problem as I did" (Ibid., 291). As suggested by these two articles, the "problem" with which Kuhn was concerned was the occurrence of revolutionary transformations or conceptual reconstructions of scientific theory. These investigations suggested that such transformations were not limited to the shift from Aristotelian to Newtonian physics but could also be found in the historical development of other scientific theories. Furthermore, it seemed that they had not been recognized previously nor investigated fully with respect to their nature, underlying processes, or implications.

The Tenure Track at Harvard

During his third year as a Junior Fellow (1950/1951), Kuhn returned to teaching, taking over Conant's general education course, "Science for the Non-Scientist." For the next five years, Kuhn taught the course with Leonard Nash, a chemist by training, who had also been selected by Conant.[40] Kuhn and Nash continued to use the case method approach that Conant had established, and they began the course with lectures that Kuhn had developed on the Copernican revolution.[41]

After his fellowship ended in 1951, Kuhn remained at Harvard as an Instructor for one year and as an Assistant Professor until 1956. His primary teaching assignment remained the general education course; however he also developed an advanced undergraduate course, "The Development of Mechanics from Aristotle to Newton." In this course and his ongoing research efforts, he continued to emphasize the importance of understanding the processes underlying the development from one set of beliefs to another:

> Sometimes you have to go way back in order to find the starting point, to write something that indicates how powerful these prior beliefs were and why they ran into trouble. And [for the Copernican revolution book,] I couldn't have started earlier than prehistory; I had to go practically back that far. (Baltas et al. 1995/2000, 291)

[40] During this period, Nash became a valued confidante for Kuhn, who described Nash in the Preface to *Structure* as "an . . . active collaborator during the years when my ideas first began to take shape" (*SSR*, xi).
[41] These lectures ultimately formed the basis of Kuhn's first book on the Copernican revolution.

Given this focus, Kuhn increasingly expanded the scope of his historical investigations, yet did so with an eye toward the broader developmental processes involved.

A second area that Kuhn began to emphasize in his teaching and research was the holistic nature of the conceptual frameworks used in science. He proposed that such frameworks heightened the visibility of anomalies while anomalies, in turn, highlighted areas in which those frameworks were incomplete or inadequate. These interrelationships – and the extent to which anomalies might prompt the shift to a different conceptual framework – were evident in Kuhn's Aristotle experience and his investigations of the history of science:

> I had gotten [the gestalt switch aspect of it . . . the conceptual framework aspect of it] from the Aristotle experience. I had it on other occasions, too. When I taught Galileo, I used to teach it in a way in which key things in it were the relatively anomalous things. I thought I understood why . . . . It's a standard mistake in the early history because people didn't used to have both the notion of accelerated motion and . . . this depends upon medieval [notion of 'latitude of forms'] . . . *So it was those questions of frameworks which illuminated anomalies, which were right at the center of what I was doing*. (Ibid., 293, emphasis added)

Kuhn's early investigations into the history of science thus were focused, detailed, and systematic. They were also directed toward the more comprehensive (and philosophical) issues of scientific development raised initially by his Aristotle experience.

Kuhn's early research explored these ideas through detailed case study investigations of important scientific discoveries. His early publications in the history of science included six reviews and five highly focused investigations.[42] The latter included the two articles cited above, an article in *Isis* on "The Independence of Density and Pore-Size in Newton's Theory of Matter" (1952), and a series of two articles on the Carnot Cycle in the *American Journal of Physics* (1955). Kuhn's book on the Copernican revolution was described by Kuhn as "an extended case history" that was "modeled very precisely" on his lectures in the general education course (Baltas et al. 1995/2000, 291).

While working on the book, Kuhn was asked by Charles Morris, author of the *Encyclopedia of Unified Science*, to contribute a volume on the history of science. Kuhn, who had been suggested to Morris by Bernard Cohen, agreed and intended to "use [the opportunity] to produce the first version, a short version of *The Structure of Scientific Revolutions*…" (Ibid., 292).[43] Kuhn applied for and received a

[42] See "Publications of Thomas S. Kuhn" in *The Road Since Structure* (*RSS* 2000, 325-35).
[43] *The Structure of Scientific Revolutions* was Kuhn's contribution to the Encyclopedia; however, it did not appear until 1962, almost 10 years after Morris' initial request. In the Preface to *Structure*, Kuhn expressed

Guggenheim Fellowship (1954/1955) with the dual objectives of finishing his book on the Copernican revolution and writing the monograph for the encyclopedia. Kuhn later commented that he did not finish either work during the fellowship, noting, ". . . something about my unreality with respect to my project and what I could do . . . I mean, I've never been any good in saying how long it will take me to do things . . ." (Ibid., 292). Yet he also suggested that the long-term nature of his project provided ancillary benefits: "thank God, it took me a long time, because I managed to get myself established in other ways meanwhile . . ." (Ibid., 293).

In this context, Kuhn's book on the Copernican Revolution was an important step in getting established. When asked why he had chosen to write his first book on that particular subject, Kuhn replied,

> Oh, I was already writing it – I had been giving it as lectures. I needed a book, I had this material, I could do a book, and I didn't think it was a stupid book to do. I mean, it was not what I mainly wanted to be doing, but it was something worth getting done. But that's why I chose it, because I had been giving lectures on the subject. (Ibid., 292)

Unfortunately, although Kuhn did in fact "need" the book for his tenure review at Harvard, *The Copernican Revolution* was judged to be of insufficient merit. In early November 1955, Kuhn was denied tenure on the basis that he had failed to become a recognized specialist in any field.[44]

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his gratitude: "The editors of that pioneering work had first solicited [the volume], then held me firmly to a commitment, and finally waited with extraordinary tact and patience for a result." (*SSR*, viii)

[44] Fuller noted that the discussion surrounding Kuhn's tenure review was one of the longest in the history of Harvard and that it occurred after Conant's departure from Harvard. It is also relevant to note that the place of science in the General Education program at Harvard was under review. Conant's course was replaced by a course in the practices of a contemporary science (Fuller 2000, 219).

<u>Section Three</u>

Historical Developmental Accounts

After being denied tenure at Harvard, Kuhn accepted a joint position in history and philosophy at the University of California at Berkeley. During his early years at Berkeley, Kuhn's investigations gradually broadened beyond detailed historiographic analysis of scientific discoveries to encompass the full range of scientific activities. With this shift of perspective, his research interests expanded beyond the underlying elements and structural patterns present in discoveries of new phenomena, to the underlying dynamics of scientific development over time. His approach thus shifted from historiographic investigation of particular scientific discoveries to broader, historical developmental accounts of the underlying patterns of scientific activity and development.

The Return to Philosophy

After being denied tenure at Harvard, Kuhn was hired by members of the philosophy department at the University of California at Berkeley, who "wanted to hire a historian of science" (Baltas et al. 1995/2000, 294). Kuhn later noted, however, "[t]hey didn't know that they didn't want [a historian of science], they didn't know that this was not a philosophical discipline" (Ibid.). Nonetheless, Kuhn "jumped at the change, because [he] wanted to do philosophy" (Ibid.). At the last moment, he was also asked to join the history department, thus Kuhn moved to Berkeley in 1956, with a joint appointment in history and philosophy.

Once at Berkeley, Kuhn taught two courses in history and two in philosophy.[45] The courses in the history department were survey courses, and because Kuhn had never given – nor even taken – a survey course in the history of science, "every lecture . . . was a research project" (Ibid., 294). From this experience, Kuhn learned "a lot of history of science" and "how to look at books that didn't feel the way I did" (Ibid.). In addition, he encountered "some of the problems of trying to organize the development of

[45] Kuhn would have preferred to offer cross-listed courses but was not able to do so due to administrative issues, a fact about which he was "sore as hell" (Baltas et al. 1995/2000, 294).

science" (Ibid.).[46] Within the philosophy department, Kuhn taught the course that he had developed at Harvard, "The Development of Mechanics from Aristotle to Newton," as well as a graduate seminar.[47]

One of Kuhn's most valued colleagues at Berkeley was Stanley Cavell. Although Cavell had been a Junior Fellow with Kuhn at Harvard, it was at Berkeley that the two established a close relationship: "[m]y interactions with him taught me a lot, encouraged me a lot, gave me certain ways of thinking about my problems, that were of a lot of importance" (Ibid., 297). In the Preface to *Structure*, Kuhn outlined this importance in more detail:

> That Cavell, a philosopher mainly concerned with ethics and aesthetics, should have reached conclusions quite so congruent to my own has been a constant source of stimulation and encouragement to me. He is, furthermore, the only person with whom I have ever been able to explore my ideas in incomplete sentences. That mode of communication attests an understanding that has enabled him to point me the way through or around several major barriers encountered while preparing my first manuscript. (*SSR*, xi)

Among the "congruent" conclusions reached by Cavell, Kuhn noted in the Postscript to *Structure* the discovery of "important contexts in which the normative and the descriptive are inextricably mixed" [(*SSR*-PS 1969/1970a, 207), citing Chapter I from Cavell's *Must We Mean What We Say?* (1969)].

Discussions between Kuhn and Paul Feyerabend, who was at Berkeley during the final stages of Kuhn's development of *Structure*, were both informal and formal, yet, ultimately (and ironically), incommensurable. The two scholars, who were both trained as physicists, shared an active investigation of issues of incommensurability. They engaged in energetic discussions in Berkeley coffee houses, through written correspondence, and at major conferences. Feyerabend was among the scholars whom Kuhn asked to review *Structure* in its draft form, and Kuhn recognized him in the Preface to *Structure* as one of a small group of scholars "whose contributions proved most far-reaching and decisive" (*SSR*, xii).

[46] Among these problems were "the standard division of history of science – ancient-medieval as one, and then modern science starting in the seventeenth century" (Baltas et al. 1995/2000, 294). Kuhn noted that these divisions "didn't work," although he admitted that the failure to recognize such distinctions formed "a bad aspect of *Structure*" (Ibid., 294-5). Kuhn's resolution of these problems was published in 1976 as, "Mathematical versus Experimental Traditions in the Development of Physical Science" (MET 1976/1977, 31-65).

[47] Of the graduate seminar, Kuhn lamented, "[y]ou couldn't really give graduate seminars in this field at Berkeley. I mean, you got good enough students, but very few of them had the preparation. So, one had to pick an area and then let people work at all sorts of levels and report it" (Baltas et al. 1995/2000, 295). Kuhn later encountered similar problems at Princeton in attempting to teach a course to graduate students from both the history of science and philosophy.

The Road to *Structure*

In the late-1950s, Kuhn once again turned to issues of scientific development and began investigating the ideas that had been prompted by his Aristotle experience:

> Oh look, I had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience. That's why I had gotten into history of science – I didn't know quite what it was going to look like, but I knew the noncumulativeness; and I knew something about what I took revolutions to be. I mean, I think in retrospect I was wrong, in the ways I talked about the other night; but that was what I really wanted to be doing. (Baltas et al. 1995/2000, 292-3)

From this initial focus on noncumulativeness and revolution, Kuhn developed what would become the major theses of *Structure*. As explained in the Preface to that work, Kuhn had directed his early investigations toward understanding the processes that are inherent in the development and acceptance of new scientific theories:

> The same problems and orientation give unity to most of the dominantly historical, and apparently diverse, studies I have published since the end of my fellowship [in the Society of Fellows]. Several of them deal with the integral part played by one or another metaphysic in creative scientific research. Others examine the way in which the experimental bases of a new theory are accumulated and assimilated by men committed to an incompatible older theory. In the process they describe the type of development that I have below called the 'emergence' of a new theory or discovery. There are other such ties besides. (*SSR*, vii)

At least in his own mind, Kuhn's "diverse" studies thus were unified by similar problems and a similar orientation toward the developmental processes and underlying structures of new theories or discoveries.

The Structure of Discoveries

According to Kuhn, "The Historical Structure of Scientific Discovery" (HSSD 1962/1977, 165-77), although written in late 1961 (first published in 1962), nonetheless reflected "the position [his] views had … reached" by the late 1950s:

> Scientific development depends in part on a process of non-incremental or revolutionary change. Some revolutions are large, like those associated with the names of Copernicus, Newton, or Darwin, but most are much smaller, like the discovery of oxygen or the planet Uranus. The usual prelude to changes of this sort is, I believed, the awareness of anomaly, of an occurrence or set of occurrences that does not fit existing ways of ordering phenomena. The changes that result therefore require "putting on a different kind of thinking-cap,"[48] one that renders the anomalous lawlike but that, in the process, also transforms the order exhibited by some other phenomena, previously unproblematic. (*ET-SS*, xvi-ii)

[48] This is a reference to (Butterfield 1957, 17).

In the essay, Kuhn focused on what he described as "one small part of . . . a continuing historiographic revolution in the study of science:" the structure of scientific discovery (HSSD, 165).[49] He proposed that traditional conceptions of discovery as sudden flashes of genius are inappropriate:

> Both scientists and, until quite recently, historians have ordinarily viewed discovery as the sort of event which, though it may have preconditions and surely has consequences, is itself without internal structure. Rather than being seen as a complex development extended both in space and time, discovering something has usually seemed to be a unitary event, one which, like seeing something, happens to an individual at a specifiable time and place. (Ibid.)

Kuhn proposed that the problems inherent in these conceptions – "the historical problem presented by the attempt to date and to place a major class of fundamental discoveries" – are highlighted by the historiographic revolution (HSSD, 166).

The particular "class" of discoveries that Kuhn perceived as presenting the problem are those involving the discovery of "a new sort of phenomenon," which entails recognition "both *that* something is and *what* it is:"

> Observation and conceptualization, fact and the assimilation of fact to theory, are inseparably linked in the discovery of scientific novelty. Inevitably, that process extends over time and may often involve a number of people (HSSD, 171)

Kuhn proposed that when viewed through the lens of the new historiography, such discoveries do, in fact, exhibit an "historical structure," possessing an identifiable internal history as well as a prehistory and post-history (HSSD, 174).

Following in-depth case studies of three examples (the discoveries of oxygen, X-rays, and the planet Uranus), Kuhn highlighted their shared characteristics and schematized the common elements of their structure. In each case, he proposed, discovery began with the recognition of an anomaly that did not conform to expectations. It continued with a struggle to make the anomaly law-like (a struggle that constituted the internal history of the discovery). Finally, the discovery concluded with "a new view of some previously familiar objects and [a simultaneous change in] the way in which even some traditional parts of science are practiced" (HSSD, 175). Kuhn proposed that in each case, this "transformation in the established techniques of scientific practice" resulted from the struggle with anomaly, which provided the discoveries with both their structure and their extension in time (HSSD, 176).

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[49] Kuhn specified that the larger revolution would be discussed in *Structure*, which was forthcoming at the time this article was published.

In the late 1950s, Kuhn published two articles that provided more historically focused examinations of scientific discoveries. "The Caloric Theory of Adiabatic Compression" (1958) provided a "less familiar example" of a discovery involving the gradual and complex identification of a new phenomenon. "Energy Conservation as an Example of Simultaneous Discovery" (EC 1959/1977, 66-104) was even narrower in scope, identifying the unique, situational factors that explained why energy conservation had been "discovered" over a twenty-year period by twelve men through widely varying approaches.

In the article on energy conservation, Kuhn asked, "[w]hy, in the years 1830-50, did so many of the experiments and concepts required for a full statement of energy conservation lie so close to the surface of scientific consciousness?" In posing this question, he noted that, "[t]his formulation has at least one considerable advantage over the usual version. It does not imply or even permit the question, 'Who *really* discovered conservation of energy first?'" (EC, 72). One might also add that, as formulated, the question focused investigative efforts on profiling certain aspects of the internal structure of discovery:

> A contemplative immersion in the works of the pioneers and their contemporaries may reveal a subgroup of factors which seem more significant than the others, because of their frequent recurrence, their specificity to the period, and their decisive effect upon individual research. (EC, 72-3)

Kuhn thus proposed that the question of discovery be redirected, "away from the *prerequisites* to the discovery of energy conservation and toward what might be called the *trigger-factors* responsible for simultaneous discovery" (EC, 73).[50]

In providing an answer, Kuhn proposed three trigger-factors, again emphasizing that these provided the stimulus that had prompted discovery at that particular time:[51]

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[50] In this respect, Kuhn's investigation focused more on the *developmental processes* underlying the discoveries than on their theoretical bases. In a note added to the manuscript after his presentation of this paper, Kuhn emphasized that he did not want to deny the importance of understanding the prerequisites to discovery. Rather, his focus was on explaining the problem of simultaneous discovery: how discoveries could emerge simultaneously from a number of different scientists utilizing different methods (EC 1959/1977, 73). Interestingly, the differences between investigating the prerequisites to discovery and the developmental processes of discovery seem to reflect important differences in the objectives and methods of the philosophy of science, on the one hand, and the (historiographic approach to) the history of science, on the other hand.

[51] As a "stimulus" for discovery, these trigger factors were different both in kind and in effect from the prerequisites to discovery (which are often examined in the philosophy of science as conditions for the possibility). Nonetheless, as a stimulus to actual discovery (i.e., the act of discovery), there seems to be a clear, if as yet unthematized, connection between that stimulus and the presence of those prerequisites. Although Kuhn did not consider it, an examination of the relationship between the (theoretical)

> Since both calorimetry and the new chemistry [both prerequisites to discovery] had been the common property of all scientists for some years before the period of simultaneous discovery, they cannot have provided the immediate stimuli that triggered the work of the pioneers (Ibid.).

In Kuhn's view, these "immediate stimuli" were the availability of conversion processes, the concern with engines, and possibly, the emerging influence of the philosophy of nature. The recognition and classification of conversion processes, which came of age in the 1830s, allowed scientists to proceed "from a variety of chemical, thermal, electrical, magnetic, or dynamical phenomena to phenomena of any of the other types and to optical phenomena as well" (EC, 75).[52] In turn, the interconnections established by conversion processes were provided with a standard of measurement through the concern with engines and the concept of work. While the first stimulus established the relationships that made a concept of energy conservation credible, this second stimulus provided the means for its quantitative formulation.

These technical concepts and experiments seemed to provide a strong scientific basis for the simultaneous discovery of energy conservation. Yet Kuhn noted that "a last look at the papers of the pioneers generates an uncomfortable feeling that something is still missing, something that is not perhaps a substantive element at all" (EC, 94). In attempting to identify this missing piece, Kuhn found that some of the pioneers had "begun with a straightforward technical problem and proceeded by stages to the concept of energy conservation," thereby following the path of discovery as traditionally conceived (Ibid.). Yet others seemed to follow a less straightforward path:

> But in the cases of Colding, Helmholtz, Liebig, Mayer, Mohr, and Séguin, the notion of an underlying imperishable metaphysical force seems prior to research and almost unrelated to it. Put bluntly, these pioneers seem to have held an idea capable of becoming conservation of energy for some time before they found evidence for it. (Ibid.)

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prerequisites and the (contextual) trigger factors might provide a valuable basis for considering the relationship between internal and external approaches or possibly, even philosophical and sociological approaches.

[52] These emerging interrelationships were made manifest by the pioneers through derivations from this network of laboratory conversions (Grove and Faraday); from a single conversion process tracked through the network (Liebig and Joule); and even from a metaphysical idea applied to the network (Mohr and Colding). The approaches of the various pioneers were thus very different; however, their interrelatedness ultimately resulted in a singular theory of energy conservation:

> In short, just because the new nineteenth-century discoveries formed a network of connections between previously distinct parts of science, they could be grasped either individually or whole in a large variety of ways and still lead to the same ultimate result. That, I think, explains why they could enter the pioneers' research in so many different ways. More important, it explains why the researches of the pioneers, despite the variety of their starting points, ultimately converged to a common outcome. What Mrs. Sommerville [in *On the Connexion of the Physical Sciences* (1834)] had called the new connections between the sciences often proved to be the links that joined disparate approaches and enunciations into a single discovery. (EC 1959/1977, 76)

Kuhn noted that each of these pioneers made crucial "jumps" or "leaps" in developing their theories, and commented that, "the presence of major conceptual lacunae in six of our twelve cases is surprising" (EC, 95).

Kuhn proposed that the work of these six scientists was strongly influenced by the philosophical movement, *Naturphilosophie*: "[t]he persistent occurrence of mental jumps like these suggests that many of the discoverers of energy conservation were deeply predisposed to see a single indestructible force at the root of all natural phenomena" (EC, 96).[53] While offering a wide range of support for this proposition, Kuhn acknowledged the tenuous nature of his claim, in particular, the difficulty of distinguishing between the influence of *Naturphilosophie* and that of conversion processes (which were, themselves, a significant source of the views of *Naturphilosophie*). His final assertion remained equivocal: "*Naturphilosophie* could, therefore, have provided an appropriate philosophical background for the discovery of energy conservation" (EC, 99).

Despite this equivocation, it is important to note that Kuhn's identification and consideration of the "philosophical background" of a discovery reflected a unique combination of historiographical awareness and philosophical interest that would become increasingly evident in his work. Kuhn looked beyond traditional (philosophical) investigations of the prerequisites or (logical) conditions for the possibility of discovery in order to consider the trigger factors. This broader view also led him beyond the boundaries of traditional, internalist historiography to consider the external influences that affected the occurrence of a discovery at a particular time and place.

The Function of Measurement

In October 1956, during Kuhn's first year at Berkeley, he prepared "The Function of Measurement in Modern Physical Science" (FM 1956/1977, 178-224) for a Social Science Colloquium at the university. Kuhn described the paper as "a consequential advance in my understanding of my topic," even though it addressed measurement, "a subject [he] had not previously been inclined to consider at all" (*ET-SS*, xvii).

[53] Kuhn noted that this predisposition was noted previously and viewed as "a residue of a similar metaphysic generated by the eighteenth-century controversy over the conservation of *vis viva*" (EC 1959/1977, 96). Yet he suggested that this seemed "an implausible source" because although the "technical *dynamical* conservation theorem has a continuous history . . . its metaphysical counterpart found few or no defenders after 1750" (EC, 97).

From an historical perspective, the essay seems to be one of Kuhn's most important works on the path leading toward *Structure*. Specifically, it provides Kuhn's first discussion of the role of textbooks in science pedagogy; the role of theory in scientific activity; the role of measurement in distinguishing science from social science; and his preliminary conceptions of normal and extraordinary science. This essay also includes Kuhn's initial presentation of the arguments that drew the greatest ire from philosophers of science: the role of theory, measurement, and criticism in scientific activity and theory choice.

Kuhn noted that he found his topic of measurement in the sciences to be "particularly challenging" because his comments were to be directed to social scientists:

> Because physical science is so often seen as *the* paradigm of sound knowledge and because quantitative techniques seem to provide an essential clue to its success, the question how measurement has actually functioned for the past three centuries in physical science arouses more than its natural and intrinsic interest. (FM, 178)

While acknowledging the central role of quantitative methods in the development of the physical sciences, Kuhn proposed that "our most prevalent notions both about the function of measurement and about the source of its special efficacy are derived largely from myth" (FM, 179). He traced the "myth of measurement" to textbooks, which are simultaneously "the sole source of most people's firsthand acquaintance with the physical sciences" and "the unique repository of . . . finished scientific achievements" (FM, 180).

As presented in textbooks, measurement in the physical sciences appears to function, first, as a test, or confirmation of theory and, secondly, as a source of theoretical exploration, or discovery. These functions are suggested in large part by the nature of scientific texts, which are written for purposes of pedagogy "some time after the discovery and confirmation procedures whose *outcomes* they record" (FM, 186). As a result of these defining characteristics, Kuhn proposed, textbooks juxtapose the *actual* relationship between measurements and their associated laws or theories. He described this misdirection as "both systematic and functional:" "though texts may be the right place for philosophers to discover the logical structure of finished scientific theories, they are more likely to mislead than to help the unwary individual who asks about productive methods" (FM, 186-7).

As a corrective to this view, Kuhn proposed an examination of the *actual* function of measurement as viewed through journal literature, that is, "the medium through which natural scientists report their own original work and in which they evaluate that done by others" (FM, 187). He modeled his examination on the approach of the "new historiography," and presented his findings in two separate accounts. The first was completed for the Social Science Colloquium in 1956. A second account, which represented a revision and extension of the previous version, was published in the spring of 1958.

In his first investigation, Kuhn highlighted the rather surprising relationship between measurement and theory that emerges when viewed through journal articles rather than textbooks:

> In textbooks the numbers that result from measurement usually appear as the archetypes of the 'irreducible and stubborn facts' to which the scientist must, by struggle, make his theories conform. But in scientific practice, as seen through the journal literature, the scientist often seems rather to be struggling with facts, trying to force them into conformity with a theory he does not doubt. Quantitative facts cease to seem simply 'the given.' They must be fought for and with, and in this fight the theory with 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. (FM, 193)

Kuhn examined a number of historical case studies and discerned two reasons for this seemingly strange inversion of the relationship between measurement and theory. First, he found that techniques and instruments are rarely available to compare theory with quantitative measures because existing techniques of measurement are often "sufficiently equivocal to fit a variety of quantitative conclusions" (FM, 194). Second, the potential both for error and for variation often is reduced only by comparing results with existing theory.

Kuhn concluded that historically, scientists have tended to let the theory "lead them," so that, "[k]nowing what results they should expect . . . [they] were able to devise techniques that got them" (FM, 196). He summarized his conclusions as follows:

> Because most scientific laws have so few quantitative points of contact with nature, because investigations of those contact points usually demand such laborious instrumentation and approximation, and because nature itself needs to be forced to yield the appropriate results, the route from theory or law to nature can almost never be traveled backward. Numbers gathered without some knowledge of the regularity to be expected almost never speak for themselves. Almost certainly they remain just numbers. (FM, 198)

Kuhn's historiographic investigation thus suggested that theory precedes rather than follows measurement, providing guidance about proper instrumentation as well as methods and detailed expectations about the proper range of results. Given the large amounts of theory required to support detailed quantitative measurement, Kuhn hypothesized that typically in such cases, "the law is very likely to have been guessed without measurement" (FM, 200).[54] In the case of Galileo's inclined plane experiment, for example,

> . . . disagreement between Galileo and those who tried to repeat his experiment was entirely natural. If Galileo's generalization had not sent men to the very border of existing instrumentation, an area in which experimental scatter and disagreement about interpretation were inevitable, then no genius would have been required to make it. His example typifies one important aspect of theoretical genius in the natural sciences – *it is a genius that leaps ahead of the facts*, leaving the rather different talent of the experimentalist and instrumentalist to catch up. (FM, 194, emphasis added)

In such situations, then, measurement seems to follow theory rather than to direct it. Yet if the precise measurement that is available to the sciences plays only a secondary role in the development of the field, then to what can the special success of the sciences be attributed? Can theoretical genius be fostered and developed, or are we left only to hope for its continuing emergence?

Kuhn answered by considering those "abnormal situations" in science, in which "research projects go consistently astray and when no usual techniques seem quite to restore them" (FM, 202). In such cases, he explained, a "crisis state" emerges within the affected fields. This crisis state is "a response by some part of the scientific community to its awareness of an anomaly in the ordinarily concordant relationship between theory and experiment" (Ibid.). Although anomalies are frequently encountered in scientific practice and "few anomalies resist persistent effort for long," situations do arise in which the anomaly cannot be explained adequately by existing theory. In such cases, a scientist will start to "start to search at random" in an attempt to "illuminate the nature of his difficulty" (FM, 203). If the anomaly remains intransigent, however, "he and his colleagues may even begin to wonder whether their entire approach to the now problematic range of natural phenomena is not somehow askew" (Ibid.).

Kuhn proposed that such crises are resolved in one of three ways. First, the discrepancy may be eliminated through refined techniques or instrumentation. Secondly, though Kuhn proposed, not often, it may be left as a "known anomaly, encysted within the body of more successful applications of the theory" (Ibid.). Finally, and most importantly, crises may be resolved by the discovery of a new phenomenon or, occasionally, by a fundamental revision of existing theory.

[54] In "A Function for Thought Experiments" (FTE 1964/1977), Kuhn outlined the ways in which thought experiments may be used in this capacity even before quantitative measurements are available. Notably, Einstein, Newton, Heisenberg, and other innovators made use of thought experiments to highlight inherent paradoxes in existing theory and to introduce the ways in which new theory could resolve those paradoxes. Despite this clear connection to the theses of "The Function of Measurement," Kuhn later noted that this essay, while written before *Structure*, had "little influence" on its shape (*ET-SS* 1977, xx).

The most significant contribution of measurement to the physical sciences is thus, what Kuhn now described as, “discovery by anomaly:” “by displaying serious anomaly, [measurement and quantitative technique] tell scientists when and where to look for a new qualitative phenomenon” (FM, 205). In such cases, quantitative techniques function as tools that direct scientific activity toward such discoveries.[55] Perhaps even more importantly, they do so in a way that is uniquely compelling: “numbers register the departure from theory with an authority and finesse that no qualitative technique can duplicate, and that departure often is enough to start a search” (FM, 206).

In the more extreme case of fundamental revision of an existing theory, Kuhn noted that while “the sources of individual theoretical inspiration may be inscrutable . . . , the conditions under which inspiration occurs are not” (Ibid.). In particular, Kuhn stated, “I know of no fundamental theoretical innovation in natural science whose enunciation has not been preceded by clear recognition, often common to most of the profession, that something was the matter with the theory then in vogue” (Ibid.). He insisted that situations of crises, uncommon yet crucial in the history of science, provide the only examples in which quantitative measurement has played a role in the processes surrounding the discovery and the confirmation of theory. Quantitative indications of the failure (or incompleteness) of a theory thus seem to possess an authority that can overcome even the most rigid of theoretical commitments. The contribution of measurement to discovery is thus indirect and negative, rather than direct and positive: “quantitative discrepancy proves persistently obtrusive to a degree that few qualitative anomalies can match” (FM, 209).

Kuhn proposed that quantitative techniques also play an important role with regard to subsequent questions of theory choice, providing a clear standard for comparison and functioning as “a razor-sharp instrument for judging the adequacy of proposed solutions” (Ibid.). As such, they represent “an immensely powerful weapon in the battle between two theories” (FM, 211). Kuhn noted that “it is for this function . . . and for it alone, that we must reserve the word ‘confirmation’” (Ibid.). Yet in an important divergence from the views of logical positivism, he claimed that, in scientific practice, this choice is always a three-way comparison, “of two theories with each other and with the world:”

> . . . anomalous observations, quantitative or qualitative, cannot tempt [a scientist] to abandon his theory *until another one is suggested to replace it*. Just as a carpenter, while he retains his craft,

[55] Kuhn emphasized that quantitative techniques “usually provide no clues” as to the nature of the phenomenon itself (FM 1956/1961, 205). Instead, such clues must be suggested by the theory that directs and refines measurement techniques.

> cannot discard his toolbox because it contains no hammer fit to drive a particular nail, so the practitioner of science cannot discard established theory because of a felt inadequacy. At least he cannot do so until shown some other way to do his job. (Ibid.)

Thus even in situations of theory choice, measurement techniques do not provide the definitive basis for evaluation of a particular theory. Rather, they are used as a tool for comparing the relative merits of competing theories.

In the competition between two theories, Kuhn proposed that the choice involves both potential gains and potential losses. He thus proposed that "[t]he study of the confirmation procedures as they are practiced in the sciences is therefore often the study of what scientists will and will not give up in order to gain other particular advantages" (FM, 212). The values expressed in such choices thus have a direct effect on the path of scientific development. Noting that "[t]hat problem has scarcely even been stated before," much less investigated in depth, Kuhn claimed that quantitative accuracy is one of the areas (values) from which scientists never retreat: "[w]hatever the price in redefinitions of science, its methods, and its goals, scientists have shown themselves consistently unwilling to compromise the numerical success of their theories" (FM, 212-3). This authority of this quantitative measurement seems to reinforce the value of measurement both in indicating the presence of an anomaly (even if it threatens established views) and in providing a strong basis for choice between competing theories.

After profiling the "actual" role of measurement in the physical sciences, Kuhn turned to consider its role in the development of science over time. He again emphasized that measurement must be preceded by extensive qualitative research, and asserted that one cannot travel the route backward, that is, from a law or theory to a prior quantitative measurement. Yet Kuhn noted that once a theory can be measured and supported quantitatively, the attention of the scientific community can be concentrated on particular problems; the length of scientific controversies can be decreased; and the strength of consensus can be increased. In this way, measurement refines the criteria used in problem selection and increases the effectiveness of verification procedures. While noting the importance of these benefits, Kuhn reiterated that they cannot be achieved through quantitative techniques alone (FM, 221). Rather, he insisted, measurement must be understood as a tool to be used on an already-established path and not as the means by which that path is forged.

In a 1958 revision of the essay, Kuhn redirected his attention from the role of measurement during periods of crisis to "measurement's most usual function in the normal practice of natural science" (FM, 188). The resulting addition to the essay was a section entitled "Motives for Normal Measurement." Kuhn began by noting that, although scientific geniuses such as Newton, Lavoisier or Einstein are recognized for their fundamental reformulation of scientific theory, such "radical reformulations" are extremely rare in the history of science. Moreover, he emphasized that such dramatic events cannot account fully for the development of scientific knowledge:

> The new order presented by a revolutionary new theory in the natural sciences is always overwhelmingly a *potential* order. Much work and skill, together with occasional genius, are required to make it *actual*. And actual it must be made, for only through the process of actualization can occasions for new theoretical reformulations be discovered. (Ibid.)

Kuhn proposed that the work and skill that is required to actualize the potential of a new theory and (thus) to point toward discoveries of new theoretical formulations can be greatly enhanced by quantitative techniques. This process of actualization thus represents the most common scientific function of measurement:

> The bulk of scientific practice is thus a complex and consuming mopping-up operation that consolidates the ground made available by the most recent theoretical breakthrough and thus provides essential preparation for the breakthrough to follow. (Ibid.)

In considering this passage later, Kuhn characterized it as "the first description of what I had . . . come very close to calling 'normal science'" (*ET-SS*, xvii).

Emphasizing the importance and the difficulty of the activities required to actualize a new theory, Kuhn examined current attempts to "actualize" Einstein's general theory of relativity and historical attempts to derive "testable numerical predictions" from Newton's three laws of motion and principle of universal gravitation. In these and a number of other cases, he proposed,

> . . . it proved immensely difficult to find many problems that permitted quantitative comparison of theory and observation. Even when such problems were found, the highest scientific talents were often required to invent apparatus, reduce perturbing effects, and estimate the allowance to be made for those that remained. (FM, 192)

In the normal practice of natural science, then, measurement seems to be focused on improving the measure of "reasonable agreement" in a given theoretical application and opening new areas of application, which carry their own associated measures of "reasonable agreement." Kuhn characterized these activities as “mathematical or manipulative puzzles” and suggested that “[t]his is the sort of work that most physical scientists do most of the time *insofar as their work is quantitative*” (Ibid.).

This view of “normal” scientific activity suggests an image of science and a role for measurement that differs importantly from traditional conceptions. Specifically, Kuhn claimed that,

> . . . these finer and finer investigations of the quantitative match between theory and observation cannot be described as attempts at discovery or at confirmation. The man who is successful proves his talents, but he does so by getting a result that the entire scientific community had anticipated someone would someday achieve. His success lies only in the explicit demonstration of a *previously implicit* agreement between theory and the world. No novelty in nature has been revealed. Nor can the scientist who is successful in this sort of work be said to have ‘confirmed’ the theory that guided his research. For if success in his venture ‘confirms’ the theory, then failure ought certainly to ‘infirm’ it, and nothing of the sort is true in this case. Failure to solve one of these puzzles counts only against the scientist . . . (Ibid.)

These statements anticipate many of the most important and most controversial points of *Structure*. Compared with the earlier version of this essay, the revision examines the role of measurement not only during the “abnormal situations” that lead to scientific discovery but also during “the normal practice of science.” In abnormal situations, measurement highlights discrepancies between theoretical expectations and empirical experiment, whereas in the normal practice of science, it is used to refine established expectations through actual experiments. In both situations, measurement serves as an indicator of either discrepancy or precision and provides no direct support for discovery, testing, or confirmation. Rather, it is always evaluated only in relation to the theory or theories on which it is deployed.

The Final Piece of the Puzzle

“The Function of Measurement” and Kuhn’s other essays illustrate his progress in developing the major theses of *Structure*. Yet the boundaries of these investigations also provide an indication of his remaining challenges. For although Kuhn was beginning to understand the nature of revolutions and the activities of normal science, he did not yet understand fully the developmental linkages between them.

A year spent at Stanford University’s Center for Advanced Studies in the Behavioral Sciences in 1958/1959 provided Kuhn with the final pieces that he needed to resolve the puzzle. He had intended to use the fellowship to write a draft of his book on revolutions and had drafted a chapter on revolutionary change soon after arriving. Yet in beginning this work, Kuhn soon encountered difficulties in accounting for the "normal" period that occurred between revolutions:

> At that time I conceived normal science as the result of a consensus among the members of a scientific community. Difficulties arose, however, when I tried to specify that consensus by enumerating the elements about which the members of a given community supposedly agreed. In order to account for the way they did research and, especially, for the unanimity with which they ordinarily evaluated the research done by others, I had to attribute to them agreement about the defining characteristics of such quasi-theoretical terms as 'force' and 'mass,' or 'mixture' and 'compound.' But experience, both as a scientist and as an historian, suggested that such definitions were seldom taught and that occasional attempts to produce them had often evoked pronounced disagreement. Apparently, the consensus I had been seeking did not exist, but I could find no way to write the chapter on normal science without it. (*ET-SS*, xviii-xix)

In order to explain the activities of normal science, Kuhn had to explain how the members of a scientific community could share a common approach without explicitly agreeing on its particulars.

Kuhn's interactions with other scholars at the Center provided a surprising source of inspiration for resolving this puzzle. Surrounded by a community of social scientists, he observed numerous "overt disagreements . . . about the nature of legitimate scientific problems and methods" and was struck by the contrast with discussions among his own community of physical scientists (*SSR*, viii). An investigator without Kuhn's unique background and underlying concerns might have attributed the difference to inherent differences between science and social science. In characteristic fashion, Kuhn looked for an alternative solution, and that search led to his theory of paradigms:

> Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called "paradigms." (Ibid.)

Kuhn's investigations of the activities of normal science were thus redirected toward differences in the activities of the sciences and those of the social sciences. In early 1959, while exploring the source of these differences, Kuhn realized that,

> . . . no consensus of quite that [traditional, explicit] kind was required. If scientists were not taught definitions, they were taught standard ways to solve selected problems in which terms like 'force' or 'compound' figured." If they accepted a sufficient set of these standard examples, they could model their own subsequent research on them without needing to agree about which set of characteristics of these examples made them standard, justified their acceptance. (*ET-SS*, xix)

Although consensus about "quasi-theoretical" terms such as force or compound was not generally found, Kuhn thus proposed that it was not needed. Instead, standard examples could serve as models for research, without specific agreement about the criteria for their special status. It was these standard examples that Kuhn described as his initial conception of paradigm and with the development of this notion, "a draft of [*Structure*] emerged rapidly" (Ibid.).

Before beginning the first draft of *Structure* and just a month after developing the notion of paradigm, Kuhn authored "The Essential Tension: Tradition and Innovation in Scientific Research" (ET 1959/1977, 225-39). The essay was prepared for a conference in June 1959 on the identification of scientific talent. Kuhn described it as "a modest further development of the notion of normal science" and, more importantly, as the "introduction of the concept of paradigms" (*ET-SS*, xviiii). Responding to the frequent calls for "divergent thinking" in scientific research, Kuhn offered a characteristically contrarian perspective based on his historiographic investigations:

> . . . both my own experience in scientific research and my reading of the history of sciences lead me to wonder whether flexibility and open-mindedness have not been too exclusively emphasized as the characteristics requisite for basic research. I shall therefore suggest below that something like 'convergent thinking' is just as essential to scientific advance as is divergent. Since these two modes of thought are inevitably in conflict, it will follow that the ability to support a tension that can occasionally become almost unbearable is one of the prime requisites for the very best sort of scientific research. (ET, 226)

According to Kuhn, the "essential tension" between tradition and innovation requires a "thoroughgoing commitment to the tradition" (ET, 235) yet, perhaps surprisingly, is "more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved" (ET, 234).

Rehearsing many of the points presented in his earlier essays, Kuhn began by emphasizing the importance of "revolutions" to scientific development. He described these as episodes in which, "a scientific community abandons one time-honored way of regarding the world and of pursuing science in favor of some other, usually incompatible approach to its discipline" (ET, 226). This "abandonment" of old theory is necessitated, Kuhn proposed, by the need to assimilate new discoveries and theories to "the existing stockpile of scientific knowledge:"

> . . . the scientist must usually rearrange the intellectual and manipulative equipment he has previously relied upon, discarding some elements of his prior belief and practice while finding new significances in and new relationships between many others. (Ibid.)

It is in this respect, Kuhn suggested, that "discovery and invention in the sciences are usually intrinsically revolutionary" and thus require "just that flexibility and open-mindedness that characterize, or indeed define, the divergent thinker" (ET, 227).

Yet Kuhn quickly emphasized that within the broader context of scientific development, "flexibility is not enough" in that "revolutions are but one of two complementary aspects of scientific advance" (Ibid.). Echoing many of the historically based conclusions of "The Function of Measurement," he wrote,

> . . . revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them. As I shall indicate below, only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one. (Ibid.)

Kuhn also extended his earlier characterization of most science as a mopping-up operation, now proposing that "normal research, even the best of it, is a highly convergent activity based firmly upon *settled consensus acquired from scientific education and reinforced by subsequent life in the profession*" (Ibid., emphasis added).

To investigate the nature of this consensus, Kuhn first considered the unique nature of scientific education, proposing that, "a rigorous training in convergent thought has been intrinsic to the sciences almost from their origin" (ET, 228). Specifically, he noted that scientific education is, "to an extent totally unknown in other creative fields, . . . conducted entirely through textbooks" (Ibid.). Because textbooks are the primary source of scientific education, they have a direct influence on scientific activities:

> Except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. Rather, these books exhibit concrete problem solutions that the profession has come to accept as paradigms, and they then ask the student . . . to solve for himself problems very closely related in both method and substance. . . . Nothing could be better calculated to produce "mental sets" or *Einstellungen*. Only in their most elementary courses do other academic fields offer as much as a partial parallel. (ET, 229)

In Kuhn's view, then, textbooks teach scientists to recognize and to apply the defined, "concrete problem solutions" that are accepted as "paradigms" by the scientific community.

Kuhn next turned to the role of paradigms in scientific practice, examining the historical development of optics. He proposed that optics education and research began to converge within the scientific community only when consensus emerged about the nature of light. Although the substance of this consensus shifted over time, when it first emerged, "the field began to make rapid and systematic progress" (*ET*, 230). In Kuhn's view, a similar, first consensus also had emerged across a wide range of other fields, produced by paradigms dating back to classical antiquity. That consensus, in turn, had produced “the transition to maturity” for each of those fields (ET, 232).[56]

Kuhn acknowledged that consensus is not necessary for the *practice* of a science, yet he claimed that “without a firm consensus, this more flexible practice will not produce the pattern of rapid consequential scientific advance to which recent centuries have accustomed us” (Ibid.). For during those periods that preceded the achievement of consensus,

> . . . a new man entering the field was inevitably exposed to a variety of conflicting viewpoints; he was forced to examine the evidence for each, and there always was good evidence. The fact that he made a choice and conducted himself accordingly could not entirely prevent his awareness of other possibilities. (ET, 231)

Kuhn characterized the scientist produced by this “earlier mode of education . . . [as generally] without prejudice, alert to novel phenomena, and flexible in his approach to his field” (Ibid.). In contrast, he suggested that, “[e]xcept under quite special conditions, the practitioner of a mature science does not pause to examine divergent modes of explanation or experimentation” (ET, 232).

In explaining these differences and their relation to scientific “progress,” Kuhn returned to many of the concepts developed in his earlier essays. As outlined in “The Function of Measurement in Modern Physical Science,” he proposed that most scientific activity (within a mature science) is directed toward elucidating and extending *existing* theory, rather than producing fundamental discoveries or revolutionary changes. This is accomplished by solving puzzles that can be “both stated and solved within the existing scientific tradition” (ET, 234). On occasion, however, this single-minded focus prompts a crisis in the tradition through the identification of an obdurate anomaly:

> . . . no other sort of work is nearly so well suited to isolate for continuing and concentrated attention those loci of trouble or causes of crisis upon whose recognition the most fundamental advances in basic science depend. (Ibid.)

Kuhn proposed that it is through such highly focused activities that development occurs in the “mature” sciences:

> . . . new theories, and to an increasing extent, novel discoveries in the mature sciences are not born *de novo*. On the contrary, they emerge from old theories and within a matrix of old beliefs about the phenomena that the world does *and does not* contain. Ordinarily such novelties are far too esoteric and recondite to be noted by the man without a great deal of scientific training. (Ibid.)

[56] The relationships among the scientific community, its paradigms, and its practices that are implied by these sentences were not examined closely by Kuhn; however, they proved both important and problematic for later interpretations of his theory.

Thus within the mature sciences, “the prelude to much discovery and to all novel theory is not ignorance, but the recognition that something has gone wrong with existing knowledge and beliefs” (ET, 235).

Kuhn emphasized that recognizing such error requires not only in-depth knowledge but also thoroughgoing and sincere commitment to the tradition. First, the scientist must rely on existing theory to resolve the puzzles of his field, not least because “[t]hat theory alone gives meaning to most of the problems of normal research” (Ibid.). The challenges of those puzzles thus typically lie “less in the information disclosed by their solutions (all but its details are often known in advance) than in the difficulties of technique to be surmounted in providing any solution at all” (Ibid.). Second, anomalies arise so frequently, that practical concerns of “time and talent” demand that only the most obdurate (and thus, potentially, the most illuminating) anomalies be pursued.

With these comments and extensions of his earlier theory, Kuhn sketched an outline of both the productive scientist and the path of scientific development. Of the former, he summarized his thoughts by claiming that, “the productive scientist must be a traditionalist who enjoys playing intricate games by pre-established rules in order to be a successful innovator who discovers new rules and new pieces with which to play them” (ET, 237). Of the latter, he provided only a preliminary view, with the more detailed description to be provided in his next work, *The Structure of Scientific Revolutions*.

<u>Chapter Two</u>

An Historiographic Interpretation of *Structure*

The insights provided by our investigation of Kuhn's research activities provide the basis for an "historiographic" interpretation of *Structure*. This interpretation examines the theory presented in *Structure* and considers its interpretation with respect to the research activities that underlay its development. It highlights both the similarities and points of difference between *Structure's* presentation of theory and Kuhn's earlier research.

This interpretation represents a first step along the path evaluating the interpretive legacy of *Structure*. By examining Kuhn's work within its "historical integrity," we seek to avoid the biases and predispositions of sociological, philosophical, or even historical interpretations and evaluations. Yet we do not simply rely on Kuhn's statements regarding his intentions for the work. Rather, we examine his research activities, earlier publications, and methods of investigation.

While the similarities between Kuhn's research and the theory outlined in *Structure* suggest a clear, developmental connection between research and theory, the differences suggest that connection is not exact. The similarities provide one basis from which to evaluate competing interpretations of the work (there are certainly others, including considerations or implications not considered by either theory or research). The differences, in turn, provide an indication of the developmental aspects of Kuhn's views, that is, those areas that he found to be especially promising or problematic.

Once this broader, research-based interpretation has been developed, we can reconsider the various interpretations and evaluations of *Structure*, highlighting the points of interpretive difference, and the strengths and limitations of the various views. Based on this reconstruction of *Structure's* interpretive debates, we can more clearly evaluate the various interpretations and identify the appropriate legacy of the work in terms of its contributions, limitations, and areas for further development. These further steps will be taken in Chapter Three.

Section One

The Origins, Objectives and Outline of *Structure*

In the Preface to *Structure*, dated February 1962, Kuhn introduced the work as "the first full report of a project originally conceived almost fifteen years ago" (*SSR* 1962/1970a, v)[57]. He described the project as originating from his involvement in James Conant's experimental course and as emerging from his first encounter with the history of science:

> To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special success. (v)

Kuhn identified *Structure* as "the first of my published works in which these early concerns are dominant" and characterized it as "an attempt to explain to myself and to friends how I happened to be drawn from science to its history in the first place" (v). He characterized his "most fundamental objective" for the work as "to urge a change in the perception and evaluation of familiar data" (viii-ix).

In the Introduction, entitled "A Role for History," Kuhn proposed that the history of science suggested a needed transformation in our image of science:

> 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 . . . . This essay attempts to show that we have been misled by [scientific "classics" and textbooks] in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself (1).

Noting the insights already provided by the new historiography, Kuhn noted that, "by implication, at least, these historical studies suggest the possibility of a new image of science" (3). He described the aim of *Structure* as "a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself" (1) and as an attempt "to delineate that [new] image by making explicit some of the new historiography's implications" (3).

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[57] Unless otherwise noted, all citations in this chapter are from the second edition of *Structure of Scientific Revolutions* (*SSR* 1962/1970a). The second edition differs from the first primarily in terms of the addition of Kuhn's 1969 "Postscript" (*SSR*-PS 1969/1970a), which will be denoted, where applicable.

The New Historiography

Kuhn described the new historiography as an attempt to correct underlying conceptual biases in the history of science by rediscovering the historical integrity of scientific activities:

> Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different and often less than cumulative, developmental lines for the sciences. *Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of the science in its own time.* They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint – usually very different from that of modern science – that gives those opinions the maximum internal coherence and the closest possible fit to nature. (3, emphases added)

On this account, the new historiography was distinguished from traditional approaches in investigating scientific achievements within the context of their historical period, rather than from the (retrospective) view of modern science. The various elements within a particular context were examined with respect to their internal coherence and fit to nature, rather than their fit with the tenets of modern science.

The new historiography differed from traditional approaches to the history of science in its objectives, methods and investigative "data." It sought to describe the scientific activities and achievements of a particular scientific achievement in a way that reflected the views and practices of that time. To do so, historiographers investigated multiple facets of scientific practice during the period, including not only original source material, working papers, and written correspondence but also subtle or indirect sources of influence, such as available instrumentation, predecessors, and immediate successors.

Based on their investigations, the pioneers of the new approach raised important questions regarding traditional accounts of scientific innovation and discovery:

> As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. Perhaps science does not develop by the accumulation of individual discoveries and inventions. Simultaneously, these same historians confront growing difficulties in distinguishing the "scientific" component of past observation and belief from what their predecessors had readily labeled "error" and "superstition." (2)

These questions are clearly important. If a scientific discovery does not have an identifiable discoverer or a singular moment of discovery, then how can such achievements be explained? Similarly, if the "scientific" past cannot be readily distinguished from "superstition," and if the "myths" of the past can be "produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge," then on what basis can knowledge be said to be "scientific" (2)?

Historiographic Insights

Kuhn only alluded to the specifics of his Aristotle experience in the Preface and Introduction of *Structure*; however, as already discussed, he highlighted its influence on his work in a number of later publications. From these reports, it is clear that the Aristotle experience provided Kuhn with a puzzle for which *Structure* was the proposed solution. The experience drew into question the prevailing image of science as the incremental and cumulative development of knowledge about the "facts" of nature. It suggested a deep interrelation between conceptions of phenomena and belief, such that both of these interrelated conceptions had to be reconstructed in the transition from Aristotelian to Newtonian physics.

These unique insights were not outlined in the Introduction to *Structure*, the description of the work's objectives, or the discussion of the new historiography. Kuhn's reference to the conception of his project was extremely vague, and he provided little indication of the nature of the "change" that he sought to elicit in either the image of the nature of science or the perception and evaluation of familiar data. His discussions of the new historiography were only slightly clearer, and they failed to address the full range of implications suggested by this "revolutionary" new approach to the study of science. Without more detailed accounts of either his own Aristotle experience and its underlying dynamics or the unique insights of the new historiography, Kuhn's aims and objectives for *Structure* remained rather vague (albeit ambitious and provocative) claims.

Structure's Schematic Account of Scientific Development

In addition to outlining the origin and objectives of his work, Kuhn explained in the Preface to *Structure* that this first report remained only "an essay rather than the full-scale book my subject will ultimately demand" (viii). He attributed the preliminary nature of the presentation to the space limitations of the *Encyclopedia of Unified Science*, for which it had been written. Noting that the result was an "extremely condensed and schematic" presentation, Kuhn suggested that the essay format might, nonetheless, be "both more suggestive and easier to assimilate" for those "readers whose own research has prepared them for the sort of reorientation here advocated" (ix).

In considering the limitations imposed by the schematic nature of his work, Kuhn noted his neglect of a number of areas, including a wide range of supporting historical evidence; promising areas for further research; suggestive problems raised by the work; and the philosophical implications of his historically oriented view of science. More precisely, he acknowledged that the schematic nature of his essay necessarily omitted much available historical evidence, in particular, examples from biological science. Kuhn also identified areas for further historical and sociological research, including the emergence of anomalies and crises, and the change of perspective evidenced in post-revolutionary textbooks and research publications.

Finally, Kuhn listed "a number of major problems" not addressed in the work, such as the distinction between pre- and post-paradigm periods; the role of technological advance and external factors; and the philosophical implications of the work. The last item was, Kuhn proposed, "perhaps most important of all," and he acknowledged that he had "refrained from detailed discussion of the various positions taken by contemporary philosophers on the corresponding issues" (x). Yet in his own defense and with a nod toward the preliminary nature of his work, Kuhn stated, ". . . this essay is not calculated to convince them. To attempt that would have required a far longer and very different sort of book" (x).

Historiographic Insights

Our examination of the development of Kuhn's research activities provides an indication of the extent to which *Structure* is, in fact, a "condensed and schematic" presentation of his earlier work. Not only does the work neglect the specifics of Kuhn's Aristotle experience, but as we will see, it also includes only cursory summaries of the historiographic case studies that formed the substance of his earlier research and the basis for the theory of *Structure*. The development of Kuhn's research investigations over time thus is concealed by *Structure's* report of the outcome of his fifteen-year project. Those cases that are profiled in *Structure* are not considered with respect to either their historical integrity (i.e., the insights provided by an historiographic interpretation) or the common patterns that they collectively suggest. Instead, they seem to be presented as illustrations of a particular concept or a specific aspect of the work's narrative account.

Given Kuhn's proposal regarding the importance of history in transforming our image of science (Chapter I is entitled "A Role for History"), *Structure's* neglect of the historiographic details of Kuhn's research is unfortunate. Readers are told of the insights provided by the new historiography and of the aspects of science that they draw into question (3-8). Yet they are not provided with any indication of the historiographic investigations from which these insights were developed, nor the extent of the differences between historiographic and (traditional) historical accounts of science. The resulting, schematic account thus neglects not only Kuhn's detailed historical studies but also the underlying method and unique insights of his historiographic approach.

This neglect of historiography notwithstanding, *Structure's* schematic, explanatory account of scientific development is perhaps a more appropriate fulfillment of Kuhn's interest in the (philosophical) implications of historiography than a more descriptive, historiographic account would have been. Rather than focusing on the specifics of historical case studies, the work outlines the transformation that historiography suggests for our image of science. While it raises further questions and issues for a number of fields (in particular, the philosophy of science), it does so in virtue of the reoriented perspective that it presents. That the essay format was, in fact, suggestive for those readers who were prepared for such a reorientation is indicated by Mary Hesse's comment that Kuhn had "assembled from various quarters truisms which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed" (Hesse 1964, 286). On the other hand, we might conclude from the wide ranging interpretations and evaluations of *Structure* that the limitations of its schematic presentation were more consequential than Kuhn had envisioned.

In the presentation of *Structure*, Kuhn sought not only to outline a different history of scientific development but also to account for the structure and dynamic processes that underlie that development. For this reason, he looked beyond the insights of particular historical case studies to consider the overarching patterns of scientific development that they seemed to suggest. Rather than focus solely on the historical integrity of science "in its own time," he sought to understand how "science" itself changes over time. As proposed by Maurice Mandelbaum (1977, 449), *Structure* thus is not an historical description of the development of particular scientific achievements but a schematic (and, implicitly, an explanatory and even normative) account of the underlying patterns and structures of scientific development. Furthermore, *Structure's* developmental account extends beyond his initial interest in revolutionary discoveries, such as the transition to Newtonian mechanics, to consider the structures underlying *all types* of scientific activity.

Implications of the New Historiography

In the remainder of the Introduction, Kuhn introduced his proposed new image of the nature of science by outlining the four implications of the new historiography: the notion of paradigms; the activities of normal science; the emergence of anomaly; and the occurrence of scientific revolution.

First, Kuhn proposed that many scientific questions can be answered by "any one of a number of incompatible conclusions" (4). He insisted that method alone is not sufficient to adjudicate among these possibilities, thus prior experience, accidents of investigation and individual make-up must be understood as "essential determinants of scientific development" (4). In this respect, the various "schools" that characterize the early developmental stages of most sciences are distinguished not by their methods but by their distinct views of nature, or what Kuhn called their "incommensurable ways of seeing the world and of practicing science in it" (4). Thus while "[o]bservation and experience can and must drastically restrict the range of admissible scientific belief," the particular beliefs that are selected *within that range*, *by a particular scientific community*, are determined by "an apparently arbitrary element, compounded of personal and historical accident" (4). He proposed the notion of paradigms to reflect this conglomeration of method, experience, accident, individual make-up, and views of nature, and to explain their collective influence on the selection of particular scientific beliefs.

Secondly, Kuhn counterbalanced this discussion of the influence of arbitrary elements with a consideration of the received beliefs that are acquired through scientific education. He noted that "[e]ffective research scarcely begins before a scientific community thinks it has acquired firm answers to questions" of fundamental entities, their relation to each other, their characteristics that can be examined, and available techniques for doing so (4). It is these answers, endorsed by the community, which form the basis of scientific training and education. As such, Kuhn proposed, they "come to exert a deep hold on the scientific mind," thereby accounting "both for the peculiar efficiency of the normal research activity and for the direction in which it proceeds at any given time" (5).

Through scientific education, prospective members of a community are taught to "see the world" in a particular way. Once this education is complete, their research is conducted accordingly. Kuhn thus described the practices of normal science as, "a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education" (5). Kuhn acknowledged the limitations imposed by such an inheritance (which was already shown as arbitrary in important respects). Yet he suggested that such limitations also serve to focus research activities toward (what seem to be) the most promising areas for scientific activity: "we shall wonder whether research could proceed without such boxes, whatever the element of arbitrariness in their historic origins and, occasionally, in their subsequent development" (5).

Normal science, then, is the conduct of scientific activities based on received beliefs about fundamental entities, their relation and characteristics, and appropriate techniques for examination. These received beliefs are acquired through education and serve to establish the conceptual boxes that guide scientific investigations. While the authority of these beliefs remains in question, nonetheless, the beliefs and the boxes that they establish serve to focus scientific research and thus, Kuhn proposed, to facilitate scientific development.

Thirdly, Kuhn considered the arbitrariness that seems to be present in received belief, in particular, the effect of this arbitrariness on scientific development. He proposed that, contrary to what one might expect, such arbitrariness serves to ensure the continuation of scientific development, rather than to inhibit it. For the conduct of normal science is based on a particular view of the world, which typically is defended strongly against any potential challenges. This dedication and narrowness of focus, while suppressing fundamental novelties, also serves to draw attention to anomalies that emerge in the course of normal scientific investigations.

If focused effort cannot account for the anomaly, then received beliefs will be examined to eliminate any "arbitrary" elements that might have gone undetected. These are periods of "extraordinary science," and Kuhn proposed that they are prompted by the emergence of anomaly. In extreme cases, the activities of extraordinary science ultimately may lead scientists to abandon their old set of commitments and to develop a new set that can account for the anomaly by simultaneously establishing "a new basis for the practice of science" (6). Kuhn described such situations as scientific revolutions, "the tradition-shattering complements to the tradition-bound activity of normal science" (6).

Finally Kuhn examined in detail the nature of scientific revolutions and the "new image of science" that they suggest. First, he noted that the widely recognized revolutions associated with Copernicus, Newton, Lavoisier and Einstein each resulted not only in a shift of theory but also in consequent shifts in the problems available for inquiry and in the standards for their solution. In each case, the result was a transformation of "the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done" (6). He proposed that similar inventions of new theory also have occurred within smaller specialist communities, typically prompting a similar "change in the rules governing the prior practice of normal science" (7). Whether large or small, any proposed changes must be assimilated to existing theory and methods, and this effort may require both "the reconstruction of prior theory and the re-evaluation of prior fact" (7). Describing this as "an intrinsically revolutionary process that is seldom completed by a single man and never overnight," Kuhn identified the multi-faceted and interrelated nature of such changes as the source of historians' difficulty in isolating a singular moment when a new theory is "invented" (7).

It would seem, then, that scientific revolutions are more far-reaching than previously understood. They reflect a shift of theory, available problems, and standards of problem-solution, which combine to effect a transformation of the world of scientific activity and even the rules of scientific practice. Yet such changes cannot simply be asserted. Rather, they must be assimilated to existing theory and methods. This assimilation may require the transformation of both scientific theory and scientific "facts." Revolutions are thus extremely complex, and their realization requires substantial time and effort.

In the case of revolutions that are prompted by the discovery of new phenomena, Kuhn proposed that a final insight is provided. In such circumstances, the new phenomena are not simply added to a list of existing phenomena, but must be assimilated to existing theories about the range of possible phenomena in the universe:

> Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal-scientific practice. That is why the unexpected discovery is not simply factual in its import and why the scientist's world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory. (7)

This assertion, perhaps more than any other, represents a clear point of departure for developing a new image of science. Drawing on recent work, such as Quine's 1957 essay, which investigated the relation of fact and theory, Kuhn considered the discovery of new phenomena and the invention of theory. In both cases, new phenomena (or new theory) must be assimilated to both pre-existing facts and pre-existing theory. Yet Kuhn proposed that in some cases, assimilation requires the reconstruction of some part of that theory (or of previous conceptions of particular "facts").[58] It is such situations that seem to provide the basis for the most transformational aspects of scientific revolutions.

Kuhn acknowledged that his broad conception of scientific revolutions (including both the discovery of new phenomena and the invention of new theory) "strains customary usage" (7). Yet he explained that it is the common structure of new discoveries and (traditional) theoretical "revolutions" that "makes the extended conception seem to me so important" (8). Thus in describing "the structure of scientific revolutions," Kuhn was, in effect, attempting to describe the structure of scientific discovery and invention. Yet the changes in both theory and fact associated with these historical achievements suggested that the nature of scientific revolution (and thus the nature of science itself) was characterized by much more discontinuity (in fact and theory) than previously believed.

Historiographic Insights

This brief overview of the four implications of the new historiography presents *Structure's* account of scientific development and, implicitly, Kuhn's proposal for a new image of the nature of science. As both a descriptive and an explanatory account, it begins with the central concept that seems to provide the key point of explanation, that is, the notion of paradigms. This concept is then used to highlight other key aspects of scientific development. As a schematic account, this presentation is appropriate. For as an account of the structure of revolutions and the broader processes of scientific development (not to mention the entry on history of science for the *Encyclopedia*), it was important that the work explain what these were and how they occurred. The details of the research activities by which the account had been developed (i.e., historiographic investigations) were not immediately relevant, nor would they serve to do anything other than complicate the image of science that Kuhn sought to present.

In examining the relationship between Kuhn's research and the theory of *Structure*, an interesting and philosophically important insight emerges. Specifically, the order in which *Structure's* central

[58] In this way, Kuhn seemed to undermine the distinction between the context of discovery and the context of justification, suggesting instead that these are integrally related.

concepts are presented is inverted relative to the order in which they were developed. In *Structure*, the notion of paradigms is introduced in Chapter II, followed by discussions of normal science (III-V), anomaly (VI-VIII), and finally, scientific revolutions (IX-XII). In contrast, Kuhn's research began with his encounter with scientific revolution, and proceeded to the identification of anomaly, the characterization of normal science and, finally, the development of the notion of paradigms.[59]

This inversion seems to support Kuhn's statement that *Structure's* schematic account was a report of his earlier research activities; however, it also illustrates the way in which *Structure* reported the *outcome* of those activities. Instead of rehearsing the detailed historiographic studies that led to the development of the concepts of scientific revolution, anomaly, normal science, and paradigm, Kuhn presented those concepts as the basis of his schematic account. Instead of explaining why the notion of paradigms represented the final piece of his puzzle, Kuhn introduced it as the initial (and implicitly central) concept for understanding scientific development.

Given this inversion, it is not surprising that the presentation of the notion of paradigm in *Structure* was vague and ambiguous. As the final piece of Kuhn's puzzle, many of its characteristics were determined by his previous investigations. Yet as the initial concept presented in *Structure* the notion had to lay the groundwork for the schematic account and the other major concepts. While the 21 uses of the concept identified by Margaret Masterman seems particularly excessive (Masterman 1970, 61), we must understand the ambiguity of the notion as associated with the rather limited period during which Kuhn explored the concept itself. In terms of the presentation of *Structure*, we must also recognize the tremendous demands (implicitly) placed upon the concept as the basis for its schematic account.

The underlying historiographic basis of *Structure's* central concepts and its conclusions were concealed by its (inverted) schematic account. By inverting the order of the central concepts and conclusions, *Structure's* presentation transposed the initial relationship among those concepts and transformed the apparent authority of the "conclusions" that seemed to result (as we will see, the nature of

[59] More specifically, Kuhn's project began with his Aristotle experience and the scientific revolution that was prompted by the "discovery" of Newtonian mechanics. In examining a number of similar scientific achievements, he identified a particular kind of discovery – discovery by anomaly – and outlined the role of anomaly in prompting crisis within a field. Gradually, Kuhn broadened his research beyond discoveries to consider the full range of scientific development, and in doing so, he developed his account of normal science. Finally, he developed the notion of paradigms while at the Center for Advanced Behavioral Sciences in an effort to explain the apparent consensus that seemed to operate during periods of normal science.

this authority also changes with this transformation). The inversion thus affected not only the concepts and the theory of *Structure* but also the apparent authority of its account.

The notion of paradigms was the first concept presented in *Structure*. As such, it seemed to be the basis not only for *Structure's* account of science but also for the other concepts that comprised that account. As presented in the work, the concepts of normal science, anomaly, and revolution, seemed to follow from the notion of paradigm, rather than to precede it, as they did in Kuhn's research.

Perhaps more importantly, the schematic presentation and the inversion of concepts transformed the apparent authority of the concepts, conclusions, and implications of *Structure*. Developed at the Center for Advanced Behavioral Sciences as the final piece of Kuhn's puzzle of science, the notion of paradigms seemed to possess its authority on the basis of its ability to account for the (apparent) consensus of normal scientific activity. Yet presented as the central concept of *Structure* and, implicitly, as the basis for the concept of normal science, the notion required a different, implicitly more self-evident basis for its authority. The inversion, in turn, transformed the apparent authority of the other concepts, which now seemed to be derived from the notion of paradigm and thus dependent upon its authority. Similarly, Kuhn's detailed historiographic case studies, which provided the initial basis for his various concepts, now seemed to be simply illustrations or examples of those concepts.[60] As a result of these inversions and the associated transformation of apparent authority, the theory presented in *Structure* seemed to be vastly different from the research findings from which it had been developed.

Further Implications

Kuhn's final comments in the Introduction suggest that he ultimately was attempting something even more far-reaching than the description of a new image of discovery and invention, or even the nature of science. In answering the presumed challenge that he is here asking too much of historical study, Kuhn noted that his investigation may, itself, serve as a source of phenomena for theories of knowledge. Further, he implied that, as such a source, his work raises important issues (we might even say, anomalies), which extend beyond the image of science to the nature of knowledge:

[60] These impressions are reinforced by the limited number and detail of the historiographic studies included in *Structure*, particularly when compared with the more extensive and in-depth studies researched by Kuhn.

> Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here. An entire arsenal of dichotomies is available to suggest that it cannot properly do so. History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. In the preceding paragraph, I may even seem to have violated the very influential contemporary distinction between the 'context of discovery' and 'the context of justification.' Can anything more than profound confusion be indicated by this admixture of diverse fields and concerns?
>
> Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and I still suppose that, appropriately recast, they have something important to tell us. Yet my attempts to apply them, even *grosso modo*, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply? (8-9)

In these statements, Kuhn defended his method of historiography against the challenges of this "arsenal of dichotomies" with two proposals. First, he proposed that these dichotomies fail to explain "the *actual* situations in which knowledge is gained, accepted, and assimilated" (9, emphasis added). This suggests that an historiographic approach can account for scientific activities and the development of scientific knowledge in a way that is not possible within the constraints of traditional dichotomies and their associated philosophical methods. This seems consistent with Kuhn's second proposition, namely, that the logical and methodological distinctions established by the dichotomies are integrally related to the problems that they would address, and thus to the analysis and answers of those problems. As a result of this intimate interrelation, we must apply the same level of scrutiny to those dichotomies that we would apply to the "theories" examined in other fields. Specifically, Kuhn proposed that we must scrutinize their application to the problems and situations that they would explicate. In this respect, it would seem that Kuhn's historiographic approach suggests not only a new image for the nature of science, but also a new image for the nature of knowledge. It seems to possess important philosophical implications yet also to hold important implications for philosophy itself.

These final paragraphs of the Introduction are admittedly long, involved, and perhaps even a bit confused. Yet their relationship to Kuhn's broader theory and its implications is clear: the new image of science that is implied by the new historiography, in turn, implies the need for a new image of knowledge.

Just as facts and theory are not categorically separable within science, so the "facts" revealed by historiographic study must be considered by (if not assimilated within) established theories of knowledge. As in science, the "facts" of philosophical analysis would seem to be "integral parts of a set of substantive answers to the very questions upon which they have been deployed." Just as the historiographic method reveals similar challenges in the history of science, so, Kuhn proposed, it may be uniquely effective in confronting the similar challenges raised for theories of knowledge.

Placed at the end of the Introduction to *Structure*, this surprisingly ambitious and provocative statement seems to generate more heat than light. While outlining Kuhn's philosophical interests and the way in which they relate to his study of science, the issues raised by the statement are addressed only indirectly in *Structure*. From a philosophical perspective, this is, at best, a missed opportunity. Yet as suggested by the reception of the work, from a practical perspective the failure is substantially more damaging. Kuhn's failure to explore the challenges or resolutions that his work presented with respect to traditional philosophical dichotomies was, to a great extent, the source of philosophers' accusations of irrationality and mob rule. As we will see, that failure also served to support a wide range of interpretations about the conclusions and implications of the work.

Section Two

Four Implications of the New Historiography

Following the introductory discussion of the role of history, Kuhn turned to the central aspects of his new image of science, which he identified as the four implications of the new historiography. In Chapter II, he introduced his notion of paradigms as a unique achievement that accounts for both the efficiency of normal scientific activities and for the direction in which it proceeds. He then examined normal science and its practices in Chapters III through VI. In Chapters VI through VIII, he described the emergence of anomalies and their role in prompting discovery, invention and revolution. Chapters IX through XII outlined the processes involved in scientific revolutions. Finally, in the concluding chapter of *Structure*, Kuhn examined the implications of his schematic account of scientific development for notions of scientific progress.

The new image of science suggested by the concepts of paradigms, normal science, anomaly, and scientific revolution extends beyond scientific theory to encompass the interrelation of changes in scientific theory with the influence of scientific beliefs, activities, and empirical observations. The guiding influence of a paradigm extends not only to method but also to the (seemingly) arbitrary elements of experience, accident, individual make-up and views of nature. Kuhn thus proposed that, contrary to established views, method alone is not sufficient to determine theory choice. Instead, it is the combination of method and this conglomeration of "arbitrary" elements that determines which "among a number of incompatible conclusions" is chosen (4).

Once selected, a paradigm serves as the basis for the conduct of normal science, which is directed toward "the actualization of the promise" of the paradigm (24). This actualization, accomplished through further development and extension of the paradigm, highlights the occurrence of anomalies for which the paradigm cannot account. It is this encounter and the subsequent struggle with anomaly that give rise to the testing and exploration of extraordinary science. This struggle also provides the (historical) structure of scientific discoveries. Finally, the isolation and investigation of anomaly (rather than testing, falsification or verification) prompts the development of an alternative "paradigm" and possibly, the occurrence of a scientific revolution.

Historiographic Insights

The four concepts of paradigms, normal science, anomaly and scientific revolution not only constitute the underlying narrative of *Structure* but also reflect the major conceptual milestones of Kuhn's earlier research. They emerged gradually over the course of Kuhn's investigations, as he attempted to explain his Aristotle experience and the philosophical implications that it seemed to suggest. They were developed through historiographic case studies as well as historical developmental accounts. As a result, they were adjusted, refined, and even extended as Kuhn broadened his investigations from scientific discoveries and revolutions to the whole of scientific practice and development.

In both Kuhn's research and *Structure's* presentation, the four concepts are interrelated, each contributing to scientific development in a way that supports and extends the contributions of the others. Yet as already suggested, the apparent relationship among the concepts and their apparent authority in *Structure* are inverted relative to Kuhn's earlier research. This inversion affected each of the individual concepts, their relation to each other, and the schematic account as a whole. Yet as we will see, its greatest impact was on the central concept of *Structure*: the notion of paradigms.

Paradigms as the Route to Normal Science

In Chapter II, "The Route to Normal Science," Kuhn introduced his notion of paradigms. This "final piece" of the puzzle that had intrigued Kuhn for 15 years was introduced as the basis for the concept of normal science:

> In this essay, 'normal science' means research firmly based upon one or more past scientific achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. (10)

Kuhn here characterized a paradigm as an achievement that provides the foundation for normal scientific practice within a particular scientific community. It serves as a foundation for scientific practice because it "define[s] the legitimate problems and methods of a research field for succeeding generations of practitioners" (10).

In order to provide the foundation for scientific practices, Kuhn explained that a paradigm must gain the acceptance of the scientific community, attracting the members of the community away from competing modes of activity. Yet as the foundation for further scientific practice, it must also be "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve" (10). In this way, a paradigm seems to constitute, to establish, and to support the further development of a particular scientific tradition:

> By choosing [the term "paradigms"] I mean to suggest that some accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research. (10)

A paradigm is thus a set of standard or accepted examples of actual scientific practice, which provide both the foundation for a scientific tradition and the accepted models for its future activities. Paradigms both define legitimate scientific problems and methods and serve as accepted examples of scientific law, theory, application and instrumentation.[61]

Kuhn proposed that scientific education transmits a particular paradigm to the membership of a scientific community, so that those members share a commitment "to the same rules and standards for scientific practice" (11). This commitment produces an "apparent consensus" within the community, and together, these make possible "the genesis and continuation of a particular research tradition" (11). Acknowledging that these remarks raised a number of questions regarding the nature and status of paradigms,[62] Kuhn asserted that, "[t]hat more abstract discussion will depend . . . upon a previous exposure to examples of normal science or of paradigms in operation" (11). [63]

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[61] As we will see, this is an unusual and even confusing dual-characterization.

[62] These questions are predominately philosophical and included the following: "Why is the concrete scientific achievement, as a locus of professional commitment, prior to the various concepts, laws, theories, and points of view that may be abstracted from it? In what sense is the shared paradigm a fundamental unit for the student of scientific development, a unit that cannot be fully reduced to logically atomic components which might function in its stead?" (11).

[63] Kuhn's proposed approach for understanding the notion of paradigms is both interesting and important. He suggests that paradigms can best be understood by first examining case studies of their operation and then considering the more abstract philosophical questions that they raise. This view reflects Kuhn's historiographic approach and suggests the consequential role of historical case studies in the development of his theory (i.e., Kuhn's theory was developed *from* his investigation of those case studies, thus they did not merely exemplify his theory but constituted it). Yet this proposal also suggests the possibility of an (unexplained and unexamined) bias regarding the best way to understand a (certain type of) new concept, e.g., Kuhn's notion of paradigm.

Examples of Normal Science and Paradigms in Operation

In considering the development of optics,[64] Kuhn proposed that the rapid development of the field was characterized by a number of shifts in the conception of light (from particles, to waves, to photons with characteristics of each). Each of these shifts represented a shift in the paradigm of problems and methods guiding optics research. Yet the scientific activities that prompted these various "revolutions" were, he suggested, very different from those conducted during the "pre-paradigm" period, that is, before Newton's particle theory of light:

> No period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were a number of competing schools and sub-schools, most of them espousing one variant or another of Epicurean, Aristotelian, or Platonic theory. (12)

During the pre-paradigm periods of optics research, an optics researcher could "take no common body of belief for granted" and was thus "forced to build his field anew from its foundations" (13). With no standard methods or conceptions of phenomena, researchers' "choice of supporting observation and experiment was relatively free" (13). Yet Kuhn noted that, in practice, this "freedom" reflected the absence of a recognized justification for that choice. While this pattern of pre-paradigm activity is, Kuhn proposed, "not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and innovation," he insisted that it is dramatically different from the pattern of development exhibited by "mature" sciences, such as the field of optics after Newton (13).

In presenting this case study, Kuhn distinguished the scientific activity that is conducted with respect to a shared paradigm from "pre-paradigm" activity. The distinction of these two types of activities rests in the presence of "generally accepted views" about the nature of light and a shared basis for choices of "supporting observation and experience." During the pre-paradigm period of optics research, scientists were "free" to investigate all aspects of optical phenomena. Yet this freedom resulted in a wide range of beliefs and standards, with virtually no agreement on the nature of the fundamental entities to be examined. With Newton's particle theory of light, however, consensus emerged about the nature of light and the standards for investigating it. Scientific activity proceeded relatively rapidly based on the conceptual foundations established by Newton's paradigm. While subsequent revolutions prompted substantial changes in this initial view, those revolutions reflected widespread *changes* in the consensus view, rather

[64] This case study was outlined initially in "The Essential Tension" (ET 1959/1977).

than a return to the previous pattern of disparate views. While discovery and innovation occurred during both periods, Kuhn proposed that – despite, or perhaps because of, the numerous subsequent revolutions – the progress of development was much more rapid once consensus had been established in the field.

As a second case study, Kuhn examined the development of electrical research in order to exhibit the structure that underlies development even during pre-paradigm periods of research. During the first half of the eighteenth century, scientists held a wide range of views regarding the nature of electricity, all of which were derived from existing scientific theories and the mechanico-corpuscular philosophy. Yet despite these similarities and shared communication across the various perspectives, the theories had "no more than a family resemblance" (14). Each view of electricity seemed to be well-grounded and could account for some range of electrical phenomena on its own terms; however, none could account for the full range of observation and experience. With the work of Benjamin Franklin, however, a concept of electricity arose that, "could account with something like equal facility for nearly all these effects and that therefore could and did provide a subsequent generation of 'electricians' with a common paradigm for its research" (15).

What enabled the work of Benjamin Franklin to establish the paradigm for an entire field and thus for all subsequent generations of scientists? What distinguished his work from that of competing schools – and what underlay the resulting shift of loyalty and emergence of consensus across the scientific community? Kuhn proposed that such questions are important for understanding not only the development of electrical research but also the development of knowledge in any "scientific" field:

> I suggest that similar fundamental disagreements characterized, for example, the study of motion before Aristotle and of statics before Archimedes, the study of heat before Black, of chemistry before Boyle and Boerhaave, and of historical geology before Hutton. In parts of biology – the study of heredity, for example – the first universally received paradigms are still more recent; and it remains an open question what parts of social science have yet acquired such paradigms at all. (15)

While acknowledging the rapid development that is made possible by agreement on a paradigm, Kuhn noted that, as suggested by these historical (and contemporary) examples, "the road to a firm research consensus is extraordinarily arduous" (15).

The Role of Paradigms in Scientific Development

Kuhn proposed that approaches developed in the new historiographic study reveal both the reasons for the difficulty in achieving a "firm research consensus" and, implicitly, the means for overcoming it. First,

> In the absence of a paradigm or some candidate for a paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. (15)

Secondly, without clear direction as to the sorts of "facts" that are to be collected, "early fact-gathering is usually restricted to the wealth of data that lie ready to hand" (15). This is not to suggest that such fact-gathering is hopeless. In fact, Kuhn acknowledged that it "has been essential to the origin of many significant sciences" (15). Yet the broad scope of such efforts tends to yield a "morass" with limited basis for distinguishing among the many facts that are collected. Finally, without direction, fact-gathering and the resulting natural histories, are likely to "omit . . . just those details that later scientists will find sources of important illumination" (15).[65] These difficulties explain the reason that, while invention and discovery are possible within pre-paradigm science, they may prove to be extremely difficult to accomplish.

Based on these case studies, it seems that a pre-paradigm community greatly accelerates the momentum of its investigations' development when it achieves consensus about the nature of its most fundamental entities or phenomena:

> No natural history can be interpreted in the absence of at least implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts – in which case more than 'mere facts' are at hand – it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident. (17)

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[65] In these remarks, Kuhn failed to explore the causes and full implications of these limitations. First, the randomness of fact-gathering suggests that there is no clear theory to guide the selection of facts. Secondly, if fact-gathering is limited to the data at hand, then this suggests that there is no instrumentation available to provide in-depth analysis of important facts. Thirdly, the morass of such random data collections suggests that there is no way of discriminating among the results of various fact-finding efforts. Finally, the possibility of omission suggests that there is no way of identifying which details are relevant in developing accounts of the field.

These further implications of Kuhn's statements are valuable in that they provide not only a list of reasons for the difficulties of pre-paradigm research but also an indication of the characteristics that are (and are not) present in such fields. Once these characteristics are identified, one can more clearly demarcate where a particular field lies along the "road to a firm research consensus" and thus take steps to ensure that efforts are directed in appropriate ways.

In this respect, the various schools of a pre-paradigm science typically are distinguished by different theories about the nature[66] of observable phenomena. Scientists thus conduct their investigations, develop instrumentation, and outline explanatory accounts based on their school's particular view of the nature of the phenomena in question: "[n]o wonder, then, that in the early stages of the development of any science different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways" (17).

The surprise, Kuhn continued, is that such differences should ever disappear. He attributed this disappearance to "the triumph of one of the pre-paradigm schools, which, because of its own characteristic beliefs and preconceptions, emphasized only some special part of the too sizable and inchoate pool of information" (17). In the case of electrical research, experimenters who viewed electricity as a fluid invented the Leyden jar in order to conduct more specialized investigations. Kuhn pointed out that this was "a device which might never have been discovered by a man exploring nature casually or at random, but which was in fact independently developed by at least two investigators in the early 1740s" (17). Furthermore, it was Franklin's focused attempts to explain the results gleaned from this specialized apparatus that "provided the most effective of the arguments that made his theory a paradigm" (17).

The triumph of the fluid view of electricity and, ultimately, of Franklin's "paradigm" came, Kuhn proposed, because each "suggested which experiments would be worth performing and which, because directed to secondary or to overly complex manifestations of electricity, would not" (18). The paradigm was particularly effective, Kuhn proposed, because "the end of interschool debate ended the constant reiteration of fundamentals" and, further, "encouraged scientists to undertake more precise, esoteric, and consuming sorts of work" (18). With the establishment of Franklin's paradigm, previous disagreement all but disappeared as scientists directed their work toward more highly specialized activities:

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[66] It is not entirely clear what is meant by the "nature" of phenomena. Kuhn's case study of electrical research suggests that competing theories of the nature of phenomena often diverged in their understanding of what was fundamental. Thus all early concepts of electricity were based on "one or another version of the mechanico-corpuscular philosophy that guided all scientific research of the day" (14). Despite this commonality, various schools proposed that "attraction and frictional generation were the fundamental electrical phenomena" whereas others viewed attraction and repulsion as "equally elementary manifestations of electricity," and a third group viewed electricity as a "fluid" rather than an "effluvium" (14). Thus while the schools of electrical research shared a belief in mechanico-corpuscular philosophy, they differed in their more specific accounts of the nature of electricity. These different views, in turn, affected the "facts" that they would observe. What seems to distinguish members of a pre-paradigm science is thus different views about the nature of the fundamental entities of their field.

> Freed from concern with any and all electrical phenomena, the united group of electricians could pursue *selected phenomena* in far more detail, designing much *special equipment* for the task and employing it *more stubbornly and systematically* than electricians had ever done before. (18, emphasis added)

This greater specialization permeated all aspects of science, so that, "[b]oth fact collection and theory articulation became highly directed activities" (18). Most importantly, Kuhn noted that as a result of this greater specialization, "[t]he effectiveness and efficiency of electrical research increased accordingly" (18).

The Role of the Scientific Community

In reflecting on these two case studies, Kuhn proposed that the establishment of a paradigm influences not only scientific activity but also the nature and structure of the scientific community. Competing schools disappear as new generations become attracted by the emergent paradigm and advocates of alternative perspectives are "simply read out of the profession" (19). With the specialization of scientific activities thus comes a "more rigid definition" of the field and, in some cases, the emergence of a new discipline or profession.

Each "new" scientific community develops specialized journals, specialist societies, and in some cases, new courses in the educational curriculum. Communication within the field become more specialized and is increasingly conducted through journal articles rather than books. Although such developments come at the cost of a "widening gulf that separates the professional scientist from his colleagues in other field," this narrowness of focus is, Kuhn suggested, one of the essential "mechanisms intrinsic to scientific advance" (21). In fact, he insisted that the achievement of a paradigm able to "to guide the whole group's research" is the most convincing "criterion that . . . proclaims a field a science" (22).

Historiographic Insights

As discussed previously, the notion of paradigms was the final concept developed in Kuhn's research investigations yet was presented in *Structure* as the initial (and central) notion for his explanatory account. The inversion of concepts between Kuhn's research and *Structure's* presentation particularly affected the notion of paradigms, whose apparent relationship to other concepts and apparent authority were changed dramatically. Although Kuhn developed the notion through historiographic investigations and within a rather robust developmental account that already included the notions of scientific revolutions, anomalies and normal science, none of these elements were apparent in its presentation in *Structure*. As the central concept of *Structure's* new image of science, the notion of paradigms was asserted as self-evident and as independent of the schematic accounts and the other concepts, which now seemed to be based upon it. To a great extent, then, the nature and authority of that proposed image were dependent upon the self-evident and independent nature of the notion of paradigms.

An interesting indication of the historiographic basis of the notion is indicated by Kuhn's suggestion that paradigms can best be understood by first examining case studies of their operation and then considering the more abstract philosophical questions that they raise.[67] Yet Kuhn did not explain the underlying reasons that "previous exposure to examples of normal science or of paradigms in operation" was required to understand the nature and status of paradigms (11). Nor did he clarify the initial philosophical questions raised by the notion, including the reasons that a paradigm is prior to concepts, laws, and theories or the sense in which a paradigm is a fundamental unit that cannot be reduced to logically atomic components (11). Furthermore, the two case studies that he presented as examples of normal science and paradigms in operation were shorter and less detailed than his earlier, historiographic accounts.[68]

Based on our historiographic investigation, the notion of paradigms as presented in *Structure* seems to be different from the notion as developed in Kuhn's earlier research. It was developed to explain the "apparent consensus" that guides scientists' problem-solving activities and accounts for the transition between periods of extraordinary and normal science. Yet the consensus that was reflected in Kuhn's initial notion of a paradigm was less traditional and less explicit than he had initially supposed. As a consensus about problem-solving (rather than definitions or theories), it operated despite differences in definitions, criteria, and justification for those solutions. Furthermore, the commitments encompassed by

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[67] To the extent that the notion of paradigms should, in fact, be self-evident, it would seem that such examples would not be necessary. We must thus consider whether Kuhn's reliance on these examples is a remnant of his earlier historiographic investigations or if some such exemplary presentation is, in fact, necessary (as in the case of the problems presented in scientific textbooks).
[68] See the study of optics in (ET 1959/1977, xx).

this consensus extended beyond scientific theories or even methods to include instrumentation and other elements of scientific problem-solving.

These unique insights are not evident in the abbreviated case studies presented in *Structure*. In fact, the case studies seem to support several traditional views, which Kuhn's research drew into question. Specifically, the nature of paradigms and of the consensus that supports them seem to derive from agreement upon the fundamental entities, phenomena or "facts" to be investigated, rather than from agreement upon standard problem-solutions. It is telling that the basis for this "agreement" remains unclear in Kuhn's case study accounts (his earlier research suggested that no such consensus existed); however, he emphasized (without further clarification) that the path toward consensus is "extraordinarily arduous" (15).

The abbreviated examples also seem to suggest that agreement is accompanied by acceptance and increased specialization of standard methods and instrumentation. The resulting, "implicit body of intertwined theoretical and methodological belief" is said to permit selection, evaluation, and criticism, thereby supporting more rapid progress and development of the field – even, Kuhn proposed, in the face of subsequent, revolutionary transformations in the initial agreements (17). These statements provide a clearer indication of the central role of problem-solving in the conduct of scientific activity (and the progress of science). Yet it is unclear how theoretical and methodological belief come to be "intertwined," and one might readily infer (in line with traditional views) that theoretical belief (i.e., belief that is derived from agreement upon "facts" or identification of phenomena) is prior to and constitutive of methodological belief.

These points of contrast with Kuhn's research and the ambiguity of explanations regarding the conduct of science are consequential. As the central concept of *Structure's* account, the notion of paradigms seems to establish much of the basis and the authority of other concepts and even of the account itself. To the extent that the notion is ambiguous or even contradictory with respect to Kuhn's earlier research activities (and, based on Kuhn's responses to critics, with respect to his intent), that ambiguity or contradiction will influence other aspects of the work's interpretation and evaluation. To understand the extent to which this occurs, we thus turn to the other major concepts of Kuhn's research and *Structure's* presentation: normal science, anomaly, and scientific revolutions.

Normal Science and its Practices

In Chapters III through V of *Structure*, Kuhn provided a more in-depth discussion of the practices of normal science. In particular, he outlined the relationships between normal science, paradigms, and the "rules" that govern scientific activities.

In Chapter III, "The Nature of Normal Science," Kuhn proposed that normal science should be understood as the "actualization of [the] promise" of a paradigm (24). It is in virtue of its promise, he insisted, that a paradigm is much more than a replication or recitation of definitive examples: "[i]nstead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions" (23). The actualization of this promise is accomplished by relying on the paradigm in undertaking the three different classes of problems typically encountered in normal science:

> . . . by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself. (23)

According to Kuhn, the bulk of normal scientific activity – both experimental and theoretical – thus is directed not toward the identification of *new* phenomena or *new* theories but toward the "articulation of those phenomena and theories that the paradigm already supplies" (24). Compared with the activities of pre-paradigm science, the activities of normal science are thus guided by a paradigm and are directed toward investigation of those facts that are deemed most relevant and most important.

In Chapter IV, "Normal Science as Puzzle-solving," Kuhn explained that while normal science is "not aimed at producing major [unexpected] novelties," the activities of normal science "add to the scope and precision with which a paradigm can be applied" (36). He described the process of articulating a paradigm as requiring "the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles" (36). Kuhn here adopted the "entirely standard meaning" of puzzles as a "special category of problems that can serve to test ingenuity or skill in solution" (36). He proposed that puzzles are distinct from other types of problems in that they possess both "the assured existence of a solution" (37) and "rules that limit both the nature of acceptable solutions and the steps by which they are to be obtained" (38).

Science thus is distinct from other disciplines in that scientific activity is focused only on those problems that can be solved (i.e., those problems that can be treated as puzzles). The selection of such problems is, Kuhn proposed, guided by a paradigm. Those problems that "cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies" will not be investigated because they will not be considered to be a worthy subject for science (37).

Kuhn qualified this characterization of science by noting that, "the problems accessible within a given research tradition display something much like this set of puzzle characteristics," *as long as* one "accepts a considerably broadened use of the term 'rule' – one that will occasionally equate it with 'established viewpoint' or with 'preconception'" (38-9). Under this broader conception, "rules" define the range of admissible solutions for both experimental and theoretical problems and provide "much information about the commitments that scientists derive from their paradigms" (40). Kuhn suggested that the main categories[69] of such rules include:

- Theoretical and conceptual commitments about scientific laws and acceptable puzzle-solutions;
- Instrumental commitments to preferred instruments and their authorized use;
- Quasi-metaphysical and methodological commitments, including the entities that do and do not exist, as well as associated methodological commitments to laws, fundamental explanations, and particular research problems; and
- Commitments inherent to the scientific profession, that is, "to understand the world and to extend the precision and scope with which it has been ordered." (42)

The puzzles of science are thus identified, approached, and resolved through a range of commitments. These extend beyond scientific theories to instrumentation and particular metaphysical entities, methods, and even the guiding principles of the scientific profession.

In these passages, Kuhn presents the case for one of his central theses: that science is best understood as a puzzle-solving enterprise. On this account, science is distinct from other fields in that it deals only with the special class of problems possessing both the assurance of a solution and the rules or commitments that ensure a range of possible solutions. A paradigm provides a particular set of these problems, solutions and rules. The activities of normal science are directed toward solving the puzzles presented by the paradigm and toward articulating the paradigm further. This articulation is the actualization of the paradigm's promise, namely, that it can resolve the puzzles of science.

Kuhn's conception of normal science as a puzzle-solving activity is closely linked with his conception of paradigms as models for further research. With the acceptance of a paradigm, scientists establish (tacit) consensus about the tools and commitments that determine which problems can be solved.

[69] Kuhn credited this question to W. O. Hagstrom and his work in the sociology of science, especially Chapters IV and V of (Hagstrom 1965).

In this respect, a paradigm extends beyond a particular scientific theory. It makes possible the solution of instrumental puzzles (as well as conceptual and mathematical ones) and entails instrumental commitments (as well as theoretical, quasi-metaphysical/methodological, and scientific ones).

Kuhn suggested that the network of commitments established by a paradigm is the source of the rules that direct puzzle-solving activities:

> Because [this network of commitments] *provides rules* that tell the practitioner of a mature specialty what both the world and his science are like, he can concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him. What then personally challenges him is how to bring the residual puzzle to a solution. (42, emphasis added)

While the rules or commitments that are established by a paradigm serve to guide the conduct of science, Kuhn insisted that they do not determine it:

> Though there obviously are rules to which all the practitioners of a scientific specialty adhere at a given time, those rules may not by themselves specify all that the practice of those specialists has in common. Normal science is a highly determined activity, but it need not be entirely determined by rules. That is why, at the start of this essay, I introduced shared paradigms rather than shared rules, assumptions, and points of view as the source of coherence for normal research traditions. Rules, I suggest, derive from paradigms, but paradigms can guide research even in the absence of rules. (42)

Thus while paradigms establish a network of commitments or rules, it seems that they can operate even "in the absence of rules." Yet it is not clear on what basis such commitments can be established, evaluated or adjusted.[70]

The (Ontological) Priority of Paradigms

In the following chapter, "The Priority of Paradigms," Kuhn examined the relationship between paradigms, rules, and normal science. Turning first to history, he suggested that "[c]lose historical investigation of a given specialty at a given time discloses a set of recurrent and quasi-standard illustrations of various theories in their conceptual, observational, and instrumental applications" (43). These "quasi-standard"

[70] This question is central to the cognitive authority of Kuhn's image of science and raises a number of related questions: What are these other shared commitments that guide scientific activities under a paradigm yet lie outside the realm of rules? Furthermore, what is the relationship between the network of commitments and the paradigm? For if all commitments are derived from a paradigm, then how are we to distinguish the criteria for resolving puzzles under *any* paradigm from criteria for resolving the puzzles of a *particular* paradigm? And finally, how can a paradigm provide not only a puzzle but also the criteria and rules for its solution? These unspecified relationships and the questions that result from these ambiguities raise important issues that must be addressed.

illustrations of the various applications of theories are, he proposed, "the community's paradigms, revealed in its textbooks, lectures, and laboratory exercises" (43).

Applying the methods of internal historiography, Kuhn considered the common view that the knowledge of a scientific "tradition" is based on shared rules and assumptions. He proposed that although an historian can readily identify the paradigms shared within a scientific community, the task of identifying shared rules is often "a source of continual and deep frustration" (44).[71] Kuhn suggested that such difficulties undermine the established view of rules as the basis of a given scientific tradition.

In considering the theoretical bases for these claims, Kuhn offered one of his most philosophically important assertions, suggesting that, "the existence of a paradigm need not even imply that any full set of rules exists" (44). Drawing analogies to Michael Polanyi's theory of tacit knowledge[72] and Ludwig Wittgenstein's discussion of family resemblance,[73] he proposed that knowledge within a scientific tradition is defined by paradigms that are not reducible to rules or defined characteristics. Instead, a paradigm establishes a resemblance between its research problems and techniques and those of previous scientific achievements:

> What [the various research problems and techniques within a single normal-scientific tradition] have in common is not that they satisfy some explicit or even some fully discoverable set of rules and assumptions that gives the tradition its character and its hold upon the scientific mind. Instead, they may relate by resemblance and by modeling to one or another part of the scientific corpus which the community in question already recognizes as among its established achievements. (45-6)

In this remarkable statement, Kuhn proposed that the basis of a scientific tradition is not adherence to shared rules but the resemblance of the tradition's problems or techniques with those of established, scientific achievements. In making this statement, he seems to rely on the conception of paradigms as models, suggesting that it is as models that they provide the foundation for scientific activity and the basis

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[71] This is a clear reference to Kuhn's insight at the Center for Advanced Studies in the Behavioral Sciences.

[72] Kuhn summarized Polanyi's theory as the view that, "much of the scientist's success depends upon 'tacit knowledge,' i.e., upon knowledge that is acquired through practice and that cannot be articulated explicitly" (44).

[73] With respect to Wittgenstein, Kuhn highlighted the role of "natural families" as "a network of overlapping and crisscross resemblances" (45). In particular, he emphasized that, "[t]he existence of such a network sufficiently accounts for our success in identifying the corresponding object or activity" (45). That is, no underlying set of shared characteristics is required. In discussing Wittgenstein's work, Kuhn emphasized that, "Wittgenstein . . . says almost nothing about the sort of world necessary to support the naming procedure he outlines" (45). It is the nature of this world (and the implications for the nature of science) that Kuhn is interested in revealing.

for further research. This foundation is based upon resemblance or similarity to established achievements, rather than rules or laws.

Kuhn insisted that the learned resemblance relations that are inherent in a paradigm's model operate independently of rules:

> Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms. And because they do so, they need no full set of rules. The coherence displayed by the research tradition in which they participate may not imply even the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover. (46)

In summarizing the theoretical basis for this assertion, Kuhn proposed that, "[p]aradigms may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them" (46). On this account, the network of relations established by the paradigm is held together by its apparent coherence and effectiveness, rather than by rules. This network of resemblance relations forms the basis for the conduct of science (prior to established rules) and even provides the basis from which scientific rules can be abstracted.[74]

Following this discussion, Kuhn turned his attention to the historical evidence available from actual case studies. First, he again highlighted the difficulties in identifying the specific rules that have guided various scientific traditions. Secondly, he pointed out that scientific education includes not only the teaching of a theory but also the study and repetition of its various applications. This dual approach (of theoretical teaching and practical application) suggests that the applications of a theory may be a source of (some sort of) knowledge, which is independent of the knowledge acquired through abstract study of scientific theory or rules. Thirdly, Kuhn insisted that rules seem to attract the attention of scientists only when the puzzle-solving ability of a paradigm is either undeveloped or drawn into question:

> When scientists disagree about whether the fundamental problems of their field have been solved, the search for rules gains a function that it does not ordinarily possess. While paradigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all. (48-9)

[74] In making these claims, Kuhn implied (and later was explicit) that the resemblance relations established by paradigms were akin to a "natural family;" however, he did not articulate the basis for such a delineation, nor how it could be justified. In the comments of the previous paragraph, he related the resemblance to already-established scientific achievements, although he did not outline the basis or justification for that resemblance, nor the basis for initial achievements. Nor did he explain the parameters of paradigms or their defining characteristics in a way that might provide further illumination of these issues. From a philosophical perspective, these issues have the status of assertions within *Structure's* schematic account and thus must be addressed.

It would seem, then, that rules are scrutinized during the pre-paradigm and crisis periods of a field yet are taken for granted when normal puzzle-solving activities can proceed without difficulty. Fourth and finally, Kuhn proposed that the primacy of paradigms (rather than rules) explains the surprising diversity of scientific fields and activities in a way that is not otherwise possible. Referencing the failed attempts by historians such as George Sarton to present a grand, unified vision of science, Kuhn claimed that science is not "a single monolithic and unified enterprise," as one might expect if all scientific disciplines are guided by a single set of shared rules (49). Instead, he proposed that his historiographic survey of all scientific fields reveals "a rather ramshackle structure with little coherence among its various parts" (Ibid.).

Kuhn explained this diversity by noting that even scientists who were educated with the same paradigms might acquire different paradigms or applications "in the course of professional specialization" (49). Alternatively, they might share a particular paradigm yet apply it to different kinds of situations. In this way, a change in the operative paradigm might affect some applications (and thus may impact some communities) yet not others:

> In short, though quantum mechanics (or Newtonian dynamics, or electromagnetic theory) is a paradigm for many scientific groups, it is not the same paradigm for them all. Therefore, it can simultaneously determine several traditions of normal science that overlap without being coextensive. A revolution produced within one of these traditions will not necessarily extend to others as well. (50)

The extensibility of a paradigm thus supports numerous paths for its articulation and specialization. As such, it supports further development of a range of overlapping applications that are not necessarily reducible to a common or even collectively coherent set of rules.

In an attempt to illustrate the extent of this specialization, Kuhn highlighted the fundamental nature of the differences that might emerge among the commitments held by individual members (or sub-groups) of a scientific community:

> An investigator who hoped to learn something about what scientists took the atomic theory to be asked a distinguished physicist and an eminent chemist whether a single atom of helium was or was not a molecule. Both answered without hesitation, but their answers were not the same. For the chemist the atom of helium was a molecule because it behaved like one with respect to the kinetic theory of gases. For the physicist, on the other hand, the helium atom was not a molecule because it displayed no molecular spectrum. Presumably both men were talking of the same particle, but they were viewing it through their own research training and practice. (50-1)

It seems, then, that the defining characteristics of a molecule will differ between physics and in chemistry. This apparent absurdity suggests a number of possible resolutions. At the most basic level, one might propose that either the physicist or the chemist is "wrong" about their conception of a molecule. Alternatively, one might propose that the proper conception is some blending of their conceptions, or adjustment under various conditions. Given the fundamental nature of the disagreement, however, we must consider carefully how and on what basis the divergent views might be compared or evaluated.

Historiographic Insights

As with the notion of paradigms, the concept of normal science presented in *Structure* differs from the concept as it was developed through Kuhn's earlier research. In the case of normal science, however, *Structure's* presentation is more detailed and comprehensive. In "The Function of Measurement" (FM 1956/1977, 178-224) Kuhn characterized normal science as the extended period that occurs between scientific revolutions and that involves "mopping up" the potential insights of the most recent revolutionary change. In "The Essential Tension" (ET 1959/1977), he proposed that this elucidation and extension of past achievements serves to isolate anomalies and thus to indicate areas for further development, and possibly, subsequent revolution. He emphasized that these periods of convergent research are characterized by settled consensus, which is achieved through education (in particular, the combination of theory and applications that are presented in scientific texts). Yet he insisted that this convergent thinking and commitment to tradition is as necessary to scientific progress as the flexibility and openness of divergent thinking.

Given the inversion of concepts between Kuhn's research and *Structure's* presentation, normal science is introduced in *Structure* not as the period of activity occurring between scientific revolutions but as the period during which the promise of a paradigm is actualized. This occurs not only through "mopping up" operations but also through extension and further articulation of the paradigm. Normal science is thus a period of puzzle-solving in which scientists act with the assurance that the problems they investigate can be resolved. This assurance is provided by the achievement of a paradigm, which encompasses shared commitments and rules that limit (but do not determine) accepted methods and possible solutions. Furthermore, as a basis for puzzle-solving, the paradigm is ontologically prior to definitions, laws, or rules because puzzle-solutions can be modeled on concrete examples. It is these examples – and their associated commitments – that a paradigm provides.

In later reflections, Kuhn noted that,

> Though I had recognized for some years that periods governed by one or another traditional mode of practice must necessarily intervene between revolutions, the special nature of that tradition-bound practice had in large part previously escaped me. (*ET-SS* 1977, xvii-xviii)

The "special nature" of this practice as problem-solving became clearer to Kuhn following his 1958 revision of "The Function of Measurement." This clarity seems to have been closely related to his development of the notion of paradigms (during 1958/1959) as a consensus of problem-solving, rather than definition. The development of that notion, in turn, was guided by his interest in explaining the consensus that seemed to characterize tradition-bound periods of normal science.

The notion of paradigms and the concept of normal science thus were closely linked in Kuhn's research and in *Structure's* presentation. Both were developed through his attempts to outline historical developmental accounts of all periods of scientific activity. The concept of normal or tradition-bound science guided Kuhn's conceptualization of the notion of paradigms. Once that notion was developed, it allowed him to articulate the concept of normal science more fully (i.e., as puzzle-solving). In the case study illustrations presented in *Structure*, however, consensus seemed to be based on the definition of phenomena, rather than the standard examples for problem-solving.

Although Kuhn emphasized the problem-solving characteristics of normal science in subsequent chapters and asserted the ontological priority of paradigms (as established by the relation of standard examples, rather than theories or definitions), the subtle yet consequential aspects of these assertions were not recognized by many readers of *Structure*. As we will see, many readers interpreted both the notion of paradigms and the concept of normal science according to traditional views of science and traditional approaches to investigations of scientific activity. In this respect, they understood the activities of normal science as guided by the "agreement on fundamentals" that was established through consensus around a (theoretical) paradigm.

Anomalies as the Route to Discovery, Invention, and Revolution

In Chapters VI through VIII, Kuhn turned from the profile of normal science to the anomalous observations that direct scientists onto the road of discovery, invention, and, occasionally, revolution. This discussion outlines how the activities of normal science, which do not aim at producing novelties of fact or theory, can be reconciled with the repeated discovery of new phenomena and the invention of radical new theories. In considering this paradox, Kuhn proposed that, "research under a paradigm must be a particularly effective way of inducing paradigm change. . . . Produced inadvertently by a game played under one set of rules, their assimilation requires the elaboration of another set" (52).

In considering the role of normal science in prompting discovery and invention, Kuhn examined a series of historical case studies involving either the discovery of a new phenomenon or the invention of a new theory.[75] He identified an underlying pattern that seemed to characterize each of the case studies and suggested that this "regularly recurrent structure" is common to the developmental processes surrounding the research activities of *both* a new discovery and a new theory (52 and 66). According to Kuhn, the first step along either path is the awareness of an anomaly. This initial awareness is followed by investigation of the anomaly and "the gradual and simultaneous emergence of both observational and conceptual recognition" (62). Finally, the discovery or invention (or revolution) is completed, through a "consequent change of paradigm categories and procedures [that is] often accompanied by resistance" (62). The underlying pattern or structure of research activities for both new scientific discoveries and the invention of new theories is thus awareness of anomaly, followed by an alternative way of observing and conceiving phenomena and finally, assimilation through changes in established means of recognition.

The Discovery of New Phenomena

In the case of research involving the discovery of new phenomena, Kuhn described the observation of anomalous phenomena as "the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science" (52-3).[76] This step is the growing awareness of a discrepancy between observations and conceptions, or between "fact" and the expected fit between that fact and existing theory. In this respect, Kuhn emphasized that traditional conceptions of "discovery" are misleading:

[75] These case studies, along with the following discussion of new phenomena were initially presented in Kuhn's 1962 publication, "The Historical Structure of Scientific Discovery" (HSSD 1962/1977).
[76] These expectations may be theoretical, conceptual, or instrumental in that they are implicit in the design and the interpretation of all standardized procedures (59).

> Though undoubtedly correct, the sentence, "Oxygen was discovered," misleads by suggesting that discovering something is a single simple act assimilable to our usual (and also questionable) concept of seeing. That is why we so readily assume that discovering, like seeing or touching, should be unequivocally attributable to an individual and to a moment in time. (55)

In contrast to this traditional view of discovery as a sudden, complete, and straightforward identification, Kuhn proposed that "discovering a new sort of phenomenon is necessarily a complex event, one which involves recognition both *that* something is and *what* it is" (55).

Discovery of new phenomena thus involves both observational and conceptual recognition, both the identification of a "fact" and the assimilation of that fact to a theory of nature.[77] This is the second stage of discovery, which Kuhn proposed is "gradual and simultaneous" emergence that possesses a clear structure:

> If both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery [of new phenomena] is a process and must take time. Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering *that* and discovering *what* occur effortlessly, together, and in an instant." (55-6)

This second period of discovery thus involves adjustment of both observations and concepts to that point of simultaneous fit: "[o]nly as experiment and tentative theory are together articulated to a match does the discovery emerge and the theory become a paradigm" (61).[78]

In considering the processes involved in a recognition that is *both* observational and conceptual, it is important to distinguish the (observational) discovery of (expected) phenomena that is already conceived and the discovery of (new) phenomena for which there are no previously established expectations or conceptual categories. In the case of the former, discovery is immediate and effective because the conceptual categories are already in place. Yet without such categories, discovery must, necessarily, be a process that takes time to be completed fully.

Completion of the discovery occurs when a paradigm (and its associated commitments) are able to account for the anomalous phenomena: "conceptual categories are adjusted until the initially anomalous

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[77] It is important to recognize that Kuhn was analyzing the discovery of *new phenomena* in the fullest sense. Such discoveries involved both new facts and new conceptualizations of those facts.

Clearly, there are a number of "discoveries" that involve the *identification* of new facts already predicted by existing theory. They could be incorporated within the parameters of existing theory and thus would be "additive" to that theory, requiring no sort of reconceptualization. As such, they would not, under Kuhn's terminology, represent the discovery of *new phenomena*.

[78] It is important to consider the basis on which such simultaneous adjustments may be made. As suggested by Kuhn's earlier comments, this basis seems to be that of (mutual) coherence and fit. Given the crucial nature of this question, however, detailed consideration will be postponed following further investigation.

has become the anticipated" (64). With this adjustment and assimilation to existing theory, Kuhn proposed, normal science can again commence, guided by the problems and methods outlined by the newly established paradigm. Furthermore, he noted that any newly adopted scientific practices generally will be more specialized than their predecessors, involving more elaborate equipment, more esoteric vocabulary, and more refined conceptualizations of phenomena. It is as a result of this greater specialization that scientific practices increase in detail and precision over time.

Kuhn emphasized that this specialization of scientific practice serves an important, if not widely recognized, role in that it ensures that scientists' activities will remain highly focused and that any emergent anomalies "will penetrate existing knowledge to the core" (65). In this way, the rigidity that characterizes the activities of normal science facilitates, rather than inhibits, the discovery of new phenomena:

> . . . novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong. Anomaly appears only against the background provided by the paradigm. The more precise and far-reaching that paradigm is, the more sensitive an indicator it provides of anomaly and hence of an occasion for paradigm change. (65)

Thus while the discovery of new phenomena is neither sought nor achieved within the defined parameters of normal science, it is, somewhat counter-intuitively, facilitated by the highly directed nature of the activities of normal science.

The Invention of New Theories

Kuhn proposed that the structure underlying the development of new theories is similar to that of scientific discoveries. In this case, the continued occurrence of the anomaly prompts "pronounced professional insecurity. . . . generated by the persistent failure of the puzzles of normal science to come out as they should" (67-8). Continued gradually, this seeming "failure of existing rules" may prompt a crisis within the field, thus serving as "the prelude to a search for new [rules]" (68).

Drawing from investigations in the history of science, Kuhn asserted that the invention of a new theory typically is "a direct response to crisis" (75). Contrary to the claims of philosophers of science, scientists typically do not develop alternative "theoretical constructions" to explain a given collection of data unless their existing ones prove problematic:

> So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science – retooling is an extravagance to be reserved for the occasion that demands it. The significance of crisis is the indication they provide that an occasion for retooling has arrived. (76)

It would seem, then, that the invention of a new theory is not the result of either relentless criticism or proliferation (as is suggested by philosophers of science). Instead, it is prompted by the emergence of a crisis that draws into question the effectiveness and appropriateness of some portions of the existing theory. As such, it draws attention away from the application of that theory to the areas that seem to require adjustment, refinement, or replacement..

Kuhn pointed out that for a crisis of confidence to develop to the point that scientists begin to look for alternate rules, the situation must present "more than just an anomaly" (82). What is required is nothing short of a breakdown in the fundamental generalizations of the paradigm; the heightened importance of problematic applications; or the introduction of contradictory developments from other fields. In such situations, the anomaly gradually becomes worthy of focused investigation because it increasingly seems to suggest that something is deeply wrong with the current paradigm.[79] Kuhn suggested that it is at this point, when "an anomaly comes to seem more than just another puzzle of normal science, [that] the transition to crisis and to extraordinary science has begun" (82).

Kuhn proposed that there are two effects once such a crisis becomes apparent. First, scientists begin to test increasingly divergent solutions in an attempt to explain the anomaly and thus to return to their productive practice of "normal" science. These "ad hoc adjustments" reflect a gradual loosening of established rules and raise increasingly urgent questions about once-standard applications of the paradigm (88).[80] Kuhn noted that this type of research is similar to the divergent and rather random activities of the pre-paradigm period, yet he emphasized that "the locus of difference is both smaller and more clearly defined" (84).

[79] At the core of the breakdown, then, is the failure of the existing paradigm to explain the anomaly. Kuhn noted that external factors, such as social pressures, may play a role in the timing of this breakdown; the ease with which it is recognized or the area in which it first occurs. Yet he emphasized that historically, the driving forces of change have been internal to the community (69).

[80] As with discoveries, these adjustments often involved both theory and experiments, that is, both theoretical and instrumental standards.

The second effect involves the resolution of the crisis, which, Kuhn proposed, occurs in one of three ways: the refined application of the traditional paradigm; the abandonment of the investigations after repeated failure; or the development of a new paradigm. The last, most extreme resolution – the development of a new paradigm – prompts a comparison of the two competing paradigms, a battle for acceptance of one or the other, and, if the new paradigm is chosen to replace the established one, a scientific revolution.

Among these three alternatives, the particular resolution of a crisis will depend upon the activities of scientists. Kuhn emphasized that the activities of extraordinary science exhibit a clear and consistent pattern, which is different from the pattern or underlying structure of normal scientific research. First, scientists attempt to isolate the anomaly and "to give it structure" by testing the rules derived from the old paradigm to see where they work and where they falter (86). Such testing represents what Kuhn described as the prevalent (but mistaken) image of a scientist: seemingly searching at random for a great discovery or new theory. Yet he proposed that even these activities are conducted within a narrowly focused area, that is, the area in which the anomaly has been isolated. The activities are also guided by some sort of paradigm, albeit one that is speculative rather than established and proven.

After isolating and investigating the anomaly, scientists seek to explain it, "turn[ing] to philosophical analysis as a device for unlocking the riddles of their field" (88). These rare investigations into the underlying assumptions of the rules and "standard procedures" of scientific research represent an attempt to identify the faulty assumptions or to reveal the internal contradictions that might be responsible for the occurrence of the anomaly. Importantly, the effect of these (philosophical) investigations is not only to isolate and to examine particular parts of the old theory, but also "to weaken the grip of a tradition upon the mind and to suggest the basis for a new one" (88). Kuhn noted that historically, analytical thought experiments have proven to be especially effective in facilitating this reorientation because they are "perfectly calculated to expose the old paradigm to existing knowledge in ways that isolate the root of crisis with a clarity unattainable in the laboratory" (88).[81]

Kuhn noted that attempts to isolate an anomaly both experimentally and theoretically are often sufficient to prompt discovery or invention:

[81] Kuhn's earlier essay, "A Function for Thought Experiments" (FTE 1964/1977) provides more detail regarding the activities and processes involved in such efforts.

> By concentrating scientific attention upon a narrow area of trouble and by preparing the scientific mind to recognize experimental anomalies for what they are, crisis often proliferates new discoveries. (88)

The "preparation" of the scientific mind "to recognize experimental [or theoretical] anomalies for what they are" thus is accomplished by both the circumstances and the activities that are associated with the crisis: "crisis simultaneously loosens the stereotypes and provides the incremental data necessary for a fundamental paradigm shift" (89).

Kuhn noted that in some cases, "the shape of the new paradigm is foreshadowed in the structure that extraordinary research has given to the anomaly" (89). Yet more often, he suggested, the development of the new paradigm is rather abrupt:

> More often no such structure is consciously seen in advance. Instead, the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. (89-90)

Kuhn could not account for the processes involved in the sudden development of a new paradigm, that is, the invention of "a new way of giving order to data now all assembled" (90). He acknowledged that the process "must here remain inscrutable and may be permanently so," yet he noted that those scientists who develop new paradigms typically are either young or new to the field in question (90). As such, he proposed, they tend to be less committed to established rules and traditions and can more readily "conceive another set that can replace them" (90).

In considering the revolutionary transition to a new paradigm, Kuhn proposed that the seeming equivalence of revolution and extraordinary science – along with their characterization as "non-normal science" – is not as "circular" as one might expect. Rather, this circularity is qualified, in that a breakdown of normal science neither depends upon nor necessarily entails a subsequent revolution:

> Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. It is upon their existence more than upon that of revolutions that the notion of normal science depends. (91)

The progression from the breakdown of normal science to revolution thus is not inevitable but depends on how the various symptoms of the crisis (e.g., the proliferation of theories, the debate over fundamentals, etc.) are resolved through the research activities of scientists. It is thus the nature of research activities, rather than the occurrence of revolution, that distinguishes extraordinary science from normal science.

Extraordinary science is characterized by a situation in which scientific activities are no longer limited (or guided) by the full range of constraints typically operative in normal science. Such activities may or may not lead to the development of a new paradigm, let alone the revolutionary overturning of the old paradigm. Regardless of their outcome, however, these activities, attitudes, and expectations will be those of extraordinary, rather than normal, science.[82]

Historiographic Insights

Kuhn's understanding of anomalies developed over almost the entire period of his investigations, and the account presented in *Structure* is very similar to that of his later publications. At an early stage in his research, Kuhn noted that discoveries often emerged from the investigation of anomalies, and he focused intently on the nature of the frameworks that illustrated those anomalies. As his work progressed, he noted the role of obdurate anomalies in suggesting that something was wrong with established theories, knowledge, or beliefs. Investigation of anomalies seemed to establish the structure underlying scientific discoveries, leading Kuhn to assert that while the sources of genius or inspiration may be inscrutable, the conditions under which they occur are not (FM, 206). Furthermore, the emergence of anomalies suggested that new theories or discoveries were not born *de novo* but were developed with respect to established theories, knowledge, or beliefs.

Structure's account of anomalies presents few new insights relative to Kuhn's earlier work. The primary difference lies in his discussion of anomalies as prompting a change in scientific activities, that is, as prompting a change from normal to extraordinary science. This slight shift of emphasis derives from his earlier discussions of paradigms and normal science, as well as his characterization of normal science as puzzle-solving. Given this account of scientific activity, (obdurate) anomalies thus can be understood as

[82] As we will see, the seeming incongruity of Kuhn's comments at the end of this chapter reflected an important and distinctive aspect of his investigation. His concern with the circularity of his argument seemed to be directed toward a philosophical audience concerned with the logical relations among (universal) concepts. Yet Kuhn's investigation was based on the *historical development* of science. The structures that he derived from scientific activities are thus imbued with temporal aspects of scientific activities that are not present in the logical structures proposed by philosophers of science. The circularity that might exist in a solely logical conception of science thus is addressed by Kuhn's emphasis on the research activities of extraordinary science, which may or may not lead to revolution. We have, then, an historiographic basis for Kuhn's theory that differs in consequential ways from the more logical or theoretical basis common in the philosophy of science.

presenting a challenge to existing puzzle-solutions. The account of normal science as a puzzle-solving activity thus provides an explanation of why anomalies occur and why their emergence prompts a gradual loosening of established traditions and, perhaps, revolution.

Scientific Revolutions

Chapters IX through XII outline the nature of scientific revolutions; the reasons they are necessary for scientific development; the processes by which they come about; the changes that they foster; the reasons for their invisibility; and the ways in which they may be resolved.

The Nature of Revolutions

In Chapter IX, "The Nature and Necessity of Scientific Revolutions," Kuhn introduced scientific revolutions as "non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (92). Considering the appropriateness of applying the metaphor of "revolution" to the sciences, he outlined two aspects of scientific revolutions that parallel political revolutions.[83] First, he proposed that both political and scientific revolutions occur when institutions cease to function adequately. In science, this sense of a malfunction occurs when a particular scientific community senses that its paradigm "has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way" (92).[84]

Secondly, Kuhn explained that revolutions occur when this malfunction leads to the abandonment of the existing institution and the construction of an alternative one. In a political context, the revolution typically is prompted by "partially extra-political or extra-institutional events" (94), and the parties involved often "differ about the institutional matrix for evaluation and achievement of political change" (93). As a result, "political recourse fails" and the parties must often resort to "techniques of mass persuasion, often including force" (93). Kuhn here adapted the three stages of discovery and invention

[83] These aspects seem to represent the processes that underlie institutional change. Kuhn did not address the ways in which this change reflects development in the sense of either evolution or progress.
[84] The community in question may be either large or small, as the revolution is generally confined to those whose paradigms are affected. To outsiders, interestingly, the revolution may seem to be simply "normal parts of the developmental process" (93).

(awareness, recognition and assimilation) to the comparison of scientific and political revolutions. The most interesting aspect of this analogy is the proposition that science, like politics, is constituted by a series of institutions that both support and rely upon it. The scientific community thus oversees its central institution (i.e., its paradigm), adjudges its effectiveness and when necessary, reforms it.

As in politics, the scientific community cannot rely on its central institution when its viability is questioned but must resort to alternative techniques and points of support. Kuhn proposed that the resolution of scientific revolutions depends partially upon persuasion and "cannot be made logically or even probabilistically compelling:"

> Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. (94)

On this account, the "issue of paradigm choice can never be unequivocally settled by logic and experiment alone" because these are determined in part by one or another paradigm (94). Just as the authority of laws or political structures is drawn into question with political revolution, so the authority of established scientific laws and structures may also be questioned. The resolution of a scientific revolution also must depend, then, upon the "techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists" (94).[85] Kuhn proposed that these "special groups" function as the ultimate arbiters of paradigm debates: "[a]s in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community" (94).

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[85] In these statements, Kuhn does not preclude the operation of logic and experiment but proposes that these cannot be the sole means by which a paradigm is chosen. More precisely, because each paradigm will suggest its own "rules", there may be no sufficient basis *outside each of the paradigms in question* on which to make a choice. Other factors will thus play a role.

One might suppose that these "other" factors will exert their influence within the parameters proscribed by the logic and experimentation of one or the other paradigm. Kuhn does not make such a claim and this proposal requires further investigation. Such a proposal would, however, serve to address at least some concerns that *Structure* undermines the cognitive authority of science.

The Necessity of Revolutions and the Changes that They Prompt

Before examining the ways in which scientific communities have chosen between two competing theories historically, Kuhn asked whether scientific development is inherently revolutionary or if scientific revolutions are simply the correction of previous mistakes:

> Granting that paradigm rejection has been a historic fact, does it illuminate more than human credulity and confusion? Are there intrinsic reasons why the assimilation of either a new sort of phenomenon or a new scientific theory must demand the rejection of an older paradigm? (94-5)

This question is an important one for Kuhn's project in that it attempts to shift his discussion from historical accounts of scientific case studies to more philosophical claims about the nature of science. This shift is crucial because Kuhn's proposed new image of science can be justified only if revolutions are shown to be an integral (and perhaps even inevitable) part of scientific development, rather than simply the piecemeal correction of previous error.

In answering this question, Kuhn acknowledged that the *logical structur*e of scientific knowledge does not require revolutionary development:

> . . . compatible relationships between old and new theories can be conceived. Any and all of them might be exemplified by the historical process through which science has developed. If they were, science would be genuinely cumulative. New sorts of phenomena would simply disclose order in an aspect of nature where none had been seen before. In the evolution of science new knowledge would replace ignorance rather than replace knowledge of another and incompatible sort. (95)

Kuhn noted that "many people" hold this cumulative view of science, yet he also pointed out that such a view is "closely . . . entangled with a dominant epistemology that takes knowledge to be a construction placed directly upon raw sense data by the mind" (96). Postponing the epistemological discussion implied by this remark and providing no further elucidation, Kuhn claimed that while logically possible, "cumulative acquisition of novelty is not only rare in fact but improbable in principle" (96).

For science to develop through the recognition of new phenomena and the development of new theories, Kuhn proposed, existing views must undergo revolutionary change that extends to the central institution that guides scientific activity. A scientific achievement that reflects either the discovery of new phenomena or the invention of new theory thus will differ from its predecessor in ways that are "necessary and irreconcilable" (103). These differences may be substantive, methodological, or instrumental:

> Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or of energy. These are the substantive differences between successive paradigms, and they require no further illustration.

> But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the sources of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. (103)

This passage suggests that a revolutionary shift in the substantive aspects of a paradigm may also prompt changes in the instrumental or methodological aspects of scientific theory. This relationship is highly complex, in that a given "science" may achieve a paradigm only to have it influence (and possibly, even overturn) the substantive, methodological, and instrumental aspects of that science.

By constituting the substantive aspects of a science, a paradigm serves as the "vehicle for scientific theory," providing a "map whose details are elucidated by mature scientific research" (109). This map of nature provides direction to the scientific research that is conducted under that paradigm: "since nature is too complex and varied to be explored at random, that map is as essential as observation and experiment to science's continuing development" (109). This represents one of the ways in which a paradigm influences science: "[t]hrough the theories they embody, paradigms prove to be constitutive of the research activity" (109).

On the other hand, through its link with methods, problem fields, and standards of solution, a paradigm also provides scientists with "some of the details essential for map-making" (109). As such, it provides more than just a theory – it also provides a way to understand, to apply and even to evaluate that theory:

> In learning a paradigm the scientist usually acquires theory, methods, and standards together, usually in an inextricable mixture. Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions. (109)

Because these scientific "tools" are provided by a particular paradigm, they prove relatively ineffective in the choice between competing paradigms: "[t]o the extent . . . that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms" (109). Once a new paradigm is established, these methodological and instrumental differences may render it incommensurable with its predecessor:

> . . . the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or

> mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before. (103)

Given the incommensurability of normal scientific activities conducted in two different periods, a paradigm is not only a product of science but actually "constitutive of science" (110).

Revolutions as Changes of World View

Given the influence of paradigms on both the substantive aspects of nature and the theoretical, methodological and instrumental aspects of science, Kuhn proposed that a change in paradigms represents a change in the community's "world of research-engagement" (111). This change entails the development of new theories and/or instruments as well as the opening of new areas for examination. Yet even more importantly, "scientists see new and different things when looking with familiar instruments in places they have looked before" (111). Kuhn concluded that this last change is perhaps the best indication that,

> The world . . . is not . . . fixed once and for all by the nature of the environment, on the one hand, and of science, on the other. Rather, it is determined jointly by the environment and the particular normal-scientific tradition that the student has been trained to pursue. Therefore, at times of revolution, when the normal-scientific tradition changes, the scientist's perception of his environment must be reeducated. . . (112)

It seems, then, that revolutions prompt not only changes in method and instrumentation but also – through their changes in the substantive aspects of a science – changes in the community's world view.[86]

Because this world view is, at least in part, a product of education, a change of paradigm entails an associated change or reeducation in the scientist's world view. This change permeates scientists' subsequent observations at such a fundamental level that they come to see new things using familiar experimental and observational techniques. Examining the discovery of Uranus, which was consecutively identified as a star, a comet, and finally, a planet, Kuhn noted that discoveries of new phenomena typically prompt changes in "the perceptual categories (star or planet) provided by the paradigm that had previously prevailed" (116). With these changes in perceptual categories, the discovery also prompted the re-

[86] Although this shift of perception is similar to a gestalt shift, Kuhn noted that the analogy is incomplete: ". . . gestalt experiments illustrate only the nature of perceptual transformations. They tell us nothing about the role of paradigms or of previously assimilated experience in the process of perception" (112). In this respect, psychological studies of perception "are suggestive, [but] they cannot, in the nature of the case, be more than that" (113).

examination of similar phenomena, leading to "the rapid discovery, after 1801, of the numerous minor planets or asteroids" (116).

Kuhn noted that similar "paradigm-induced changes in scientific perception" have occurred not only in astronomy but also in electricity, chemistry, and physics (116). Perhaps the most dramatic of these was Galileo's consideration of the pendulum as "a body that could repeat the same motion over and over again *ad infinitum*" (119). This view contrasted with the traditional Aristotelian characterization of a pendulum as a body "simply falling with difficulty" (119). Yet the resulting observations proved to be extremely productive for Galileo, leading to "his only sound arguments for the independence of weight and the rate of fall, as well as for the relationship between vertical height and terminal velocity of motions down inclined planes" (119).

Kuhn proposed that while Galileo's laws could not have been discovered by an Aristotelian (123), this was not necessarily because his observations were more accurate or more objective (119).[87] Rather, what characterized Galileo's "genius" was "the exploitation . . . of *perceptual possibilities* made available by a medieval paradigm shift" [i.e., the impetus theory (119, emphasis added)]. Yet Galileo's arguments were not the first of their kind. Moreover, his expectations preceded his experiment, in that he "developed his theorem on the subject together with many of its consequences before he experimented with an inclined plane" (125). In this way, Kuhn explained, Galileo's theorem was "another one of the *network of new regularities* accessible to genius in the world *determined jointly* by nature and by the paradigms upon which Galileo and his contemporaries had been raised" (125, emphasis added).

Considering again "[t]he traditional epistemological viewpoint that has most often guided Western philosophy for three centuries," Kuhn argued that sensory experience is not simply fixed and neutral, nor are theories simply interpretations of given data (126).[88] To the contrary, the observations gleaned from scientific research are not independent or objective but are the hard-won results of substantial effort: "[t]he

[87] In fact, Kuhn emphasized, the measurements made or observations discussed by an Aristotelian would have been focused in different areas (123). Moreover, he suggested that the conclusions drawn by Galileo were relatively straightforward once the paradigm shift was accomplished: "[r]egularities that could not have existed for an Aristotelian (and that are, in fact, nowhere precisely exemplified by nature) were consequences of immediate experience for the man who saw the swinging stone as Galileo did" (124).

[88] Kuhn proposed that traditional views of scientific theories as interpretations of fixed data are "an essential part of a philosophical paradigm initiated by Descartes and developed at the same time as Newtonian dynamics" (121). Yet he noted that this epistemological paradigm is encountering increasing difficulties as a result of the insights provided by research in the new historiography as well as philosophy, psychology, linguistics and art history.

operations and measurements that a scientist undertakes in the laboratory are not 'the given' of experience but rather 'the collected with difficulty' (126). In this respect, Kuhn proposed that scientific activity tends to be directed toward those areas in which the community's paradigm seems to fit with nature:

> Science does not deal in all possible laboratory manipulations. Instead, it selects those relevant to the juxtaposition of a paradigm with the immediate experience that that paradigm has partially determined. As a result, scientists with different paradigms engage in different concrete laboratory manipulations. (126)

It seems, then, that the observations of scientific research are influenced by the activities that yield them in ways that have not yet been acknowledged fully. By pursuing activities that are "relevant" (126) for fitting the paradigm with experience, the scientist may obtain observations that are more "productive" (119) for her research.

Based on his historiographic investigations, Kuhn proposed that, "[w]hat occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable data" (121). First, he stated that the data themselves are "not unequivocally stable" (121). Second, the transition from one conception of the data (e.g., Aristotle's pendulum as falling with difficulty) to another (e.g., Galileo's pendulum as a body repeating the same operation over and over) permeates the entity, its characteristics, and its relationships. As such, it involves more than a shift in interpretation. In this respect, it seems, the observational "facts" of science are not necessarily "mere" facts.

Kuhn acknowledged that the proliferation of different interpretations of facts has been prominent in the history of scientific research, however, he asserted that

> . . . each of these interpretations [e.g., by Galileo, Aristotle, Musschenbroek, and Franklin] presupposed a paradigm. They were parts of normal science, an enterprise that, as we have already seen, aims to refine, extend, and articulate a paradigm that is already in existence. (122)

In this way, Kuhn insisted that interpretive activities are relevant only within the context of normal science because the "interpretive enterprise . . . can only articulate a paradigm, not correct it" (122). A shift of paradigms thus entails more than just a change in the interpretation of fixed data or concrete phenomena. To the contrary, Kuhn proposed that it involves the transformation of "large portions" of experience:

> Scientists . . . often speak of the 'scales falling from the eyes' or of the 'lightning flash' that 'inundates' a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution. On other occasions the relevant illumination comes in sleep. *No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born*. Though such intuitions depend upon the old experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of that experience as an interpretation would be. Instead, they gather up large portions of

> that experience and transform them to the rather different bundle of experience that will thereafter be linked piecemeal to the new paradigm but not to the old. (122-3, emphasis added)

Although the processes underlying these "flashes of intuition" remain obscure, Kuhn's characterization of intuition is quite telling. On this account, intuition gathers up "large portions" of experience and transforms them into a "different bundle" of experience. Whereas interpretation is merely linked to distinct objects of experience, intuition seems to be capable of both engaging and transforming experience in a way that both transforms the experience and reconfigures (some of) its objects into entirely different bundles.

The contrasting means by which intuition and interpretation operate is also significant. Intuition gathers up "large portions of experience" to develop paradigms, which thereby "determine large areas of experience at the same time," (129). Once these areas of experience are determined, interpretation then fits the (resulting) observations into already-prepared conceptual boxes: "questions about . . . the consequences of particular laboratory manipulations presuppose a world already perceptually and conceptually subdivided in a certain way" (129). Kuhn proposed that "paradigm-embodied experience" is the most elementary form of experience and precedes the perceptual and conceptual subdivisions that accompany subsequent laboratory manipulations:

> . . . the scientist who looks at a swinging stone can have no experience that is in principle more elementary than seeing a pendulum. The alternative is not some hypothetical 'fixed' vision, but vision through another paradigm, one which makes the swinging stone something else.
>
> All of this may seem more reasonable if we again remember that neither scientists nor laymen learn to see the world piecemeal or item by item. Except when all the conceptual and manipulative categories are prepared in advance – e.g., for the discovery of an additional transuranic element or for catching sight of a new house – both scientists and laymen sort out whole areas together from the flux of experience. (128)

On this account, the most elementary forms of experience are not the interpretation or identification of particular objects but the intuitive "bundling" of whole areas out of the "flux of experience."

A paradigm shift thus involves a re-bundling of large areas of experience, rather than piecemeal change. This shift may, in turn, require an adjustment of those conceptually and instrumentally interpretive "boxes," which were suggested and supported by the previous paradigm:

> After a scientific revolution many old measurements and manipulations become irrelevant and are replaced by others instead. One does not need to apply all the same tests to oxygen as to dephlogisticated air. (129)

Kuhn emphasized that even in cases of revolution, this change is not total. Many of the same concepts, descriptions, methods, and instruments may be incorporated directly into the new paradigm. Those that must be changed may or may not be among the transformed or reconstructed parts of the paradigm. It is in this way, then, that the adoption of a new paradigm can prompt the development of innovative insights from long-familiar concepts and methods. It is in this way that a paradigm shift can be revolutionary with respect to some of its rules and commitments yet evolutionary with respect to others.

Historiographic Insights

Structure's presentation of scientific revolutions is dominated by discussions of paradigms with no mention of Kuhn's Aristotle experience or of the reasons for his 15-year investigation. Revolutions are defined as the replacement of an older paradigm when it has ceased to function adequately, such that commitments of the substance, methods and instrumentation of a science are replaced by "incompatible" new ones. In this respect, revolutions are non-cumulative yet they are also necessary. Furthermore, they prompt a change not only in the practices of science but also in the way that scientists view the world. This is because "paradigm-embodied experience" is the most elementary form of experience and involves the use of intuition to "sort out whole areas together from the flux of experience" (128).

Based on our historiographic investigations, in particular, our examination of Kuhn's Aristotle experience, scientific revolutions can be understood as involving a change in the deeply interrelated conceptions of phenomena (i.e., "motion") and the underlying ontology (i.e., relation of qualities and substance). While these two aspects may be discerned from the presentation of *Structure* (particularly once they have been clearly identified and distinguished), they are neither readily apparent nor exceedingly clear in the discussions outlined above. The difficulty lies in *Structure's* presentation of scientific revolutions in terms of paradigms without clear articulation of the nature of paradigms or of the reasons for the associated change. The ambiguity of the notion of paradigms is thus particularly problematic for these discussions of scientific revolutions because it renders the nature and impetus for revolutionary change equally ambiguous. Furthermore, *Structure's* account of scientific revolutions relies upon not only their relationship to the notion of paradigms but also upon the authority of that notion. Particularly given the challenges presented to traditional epistemology, this reliance renders *Structure's* account of revolutions potentially even more problematic than its account of paradigms. The inversion of both the apparent relationship and the apparent authority of these various concepts thus is consequential for our understanding, interpretation, and evaluation of them and of the account presented in *Structure*.

* * *

While our historiographic consideration of Kuhn's research clarifies important aspects of the concepts of paradigms, normal science, anomaly and scientific revolution, the presentation of *Structure* provides a more detailed and comprehensive account of their interrelationship. To some extent, this more detailed interrelation is to be expected, given the explanatory nature of *Structure's* account as compared with the collection of descriptive historiographic studies that constituted Kuhn's earlier work. Yet the resulting, extended account of the developmental processes in scientific research also served to introduce a number of subtle yet important distinctions. While many of these points were noted in Kuhn's earlier work, they took on a more consequential role in *Structure's* explanatory account.

First, Kuhn distinguished the puzzle-solving activities of science from the more generalized view that scientific activities are directed toward increasing the truth-content of scientific theories. The difference lies, he suggested, in the (implicit) guarantee that the given problems possess an achievable solution.[89] This guarantee allows a scientist to focus her efforts on those problems (puzzles) that can be solved using current theory, methods, and techniques. By relying on these existing tools, she can explore the more esoteric aspects of a puzzle to a level of detail not otherwise available. Convinced that a solution exists, she also may adjust (gradually) even the most fundamental of the rules that seem to govern it. Kuhn proposed that this adjustment is made possible through "special commitments," which can be used to question or to adjust existing rules. These special commitments are part of the "arbitrary" elements that exist within any given paradigm.

Secondly, Kuhn explicitly distinguished the discovery of new phenomena from the discovery of phenomena that is already expected or conceived. In the case of new phenomena, observational and conceptual categories must emerge simultaneously (otherwise the phenomena would not be "new"), whereas expected phenomena are anticipated by existing theory. Anticipated phenomena thus have

[89] Kuhn proposed that this guarantee is based on the presumption of the adequacy of accepted puzzle-solutions, rather than the presumption that the "truth" may be identified for a particular problem.

conceptual categories already prepared and waiting for (confirming) observations. In fact, such phenomena are often actively sought out in an effort to extend or to confirm the theory that suggested them. Thus once the conceptual categories are posited, observations are directed toward identification or "discovery" of the phenomena.

In the case of new phenomena, however, Kuhn proposed that observation and conceptualization are integrally intertwined and cannot be separated from either a practical or a logical perspective. In rough philosophical terms, the conceptualization, or "what" of the discovery, occurs simultaneously with the observation, or "that" of the discovery. This discovery, while simultaneous with respect to these defining categories, nonetheless involves substantial effort and investigation, as scientists struggle to explain an anomaly. While this struggle involves the isolation and investigation of anomaly, Kuhn suggested that the discovery itself is more similar to intuition than it is to interpretation. Specifically, observational and conceptual recognition emerge together as an holistic resolution of the problem that the anomaly presents for established expectations. Yet the processes by which this occurs – and the authority of what results – remain unclear.

The distinction of discovery by anomaly is important because it highlights the role of philosophy and the existence of two types of scientific activity. During periods of normal science, a community's paradigm operates effectively and scientists follow its puzzle-solving rules successfully. During such periods, science progresses by actualizing the promise of the paradigm and discoveries that are anticipated in advance by the paradigm are identified and confirmed through empirical observation. Scientists' observations generally match the expectations and the conceptualizations established by the paradigm.

In contrast, discovery by anomaly represents a specialized type of discovery and highlights the existence of a second type of scientific activity. Over time, Kuhn proposed that the development of a paradigm through the conduct of normal science inevitably gives rise to anomalous observations that cannot be explained or resolved with the existing theory or puzzle-solutions. If the anomaly continues, the puzzle-solving effectiveness of the current paradigm may be drawn into question, thereby prompting a crisis within the scientific community. In such cases, the "puzzle-solving" rules of the paradigm will be loosened gradually in an attempt to regain stability, that is, to assimilate anomalous observations with (conceptual) expectations. During such periods, progress occurs either through piecemeal refinement of the existing rules (as in normal science) or in the (more extensive) elaboration of another set of rules. Extraordinary science thus may culminate either in a return to normal science under the previous set of rules or in a revolution (typically prompted by discovery-by-anomaly and subsequent assimilation to established theory). In the case of a revolution, a new paradigm will be established, along with a new set of rules and puzzle-solutions for future scientific activity.[90]

[90] We might characterize this return to normal scientific activities under a new paradigm as "new normal".

Section Three

The Historical Development of Science

While the four implications of the new historiography each hold important implications for the conduct of science, collectively they suggest a new image of scientific development. In the final chapters of *Structure*, Kuhn outlined the role of paradigms, normal science, anomalies and revolutions in the development of scientific knowledge and attempted to account for the special nature of scientific progess.

The Invisibility of Revolutions

After considering various revolutions "by illustration," Kuhn considered why such events "have customarily been viewed not as revolutions but as additions to scientific knowledge" (136). He proposed that previous, cumulative views of scientific development were developed and supported by "an authoritative source that systematically disguises – partly for important functional reasons – the existence and significance of scientific revolutions" (136). This authoritative source, he suggested, is the scientific textbook, along with the subsequent popularizations and philosophical works that may be modeled on it:

> All three of these categories [textbooks, popularizations, and philosophical works] – until recently no other significant sources of information about science have been available except through the practice of research – have one thing in common. They address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are writing. (136)

This claim highlights two important characteristics of textbooks: they are one of the few sources of information about science; and they include problems, data *and* theory, all of which have already been articulated fully.

Whether used to educate scientists, to present scientific issues to the public, or to outline the logical structure of science for philosophers, Kuhn noted that these authoritative sources "record the stable *outcome* of past revolutions and thus display the bases of the current normal-scientific tradition" (137). Given this purpose and their relevance to the contemporary context, they need not outline the processes by which scientific achievements have been developed over time. In fact, he suggested, "[i]n the case of textbooks, at least, there are even good reasons why, in these matters, they should be systematically misleading" (137).

As the “primary pedagogic vehicles for the perpetuation of normal science,” Kuhn noted that scientific textbooks are rendered obsolete by any shifts that occur in the language, problem-structure, or standards of science. When a new paradigm becomes established, existing textbooks must be rewritten in order to present contemporary views of science in their fullest and most developed form. These new textbooks present their paradigm as definitive. In doing so, they repress or reject earlier, alternative accounts, thereby masking the processes of revolution:

> For reasons that are both obvious and highly functional, science textbooks (and too many of the older histories of science) refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the texts’ paradigm problems. Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific. (138)

Thus by neglecting or rejecting earlier language, problems, or standards of scientific research, “revised” textbooks *systematically* render invisible not only earlier forms of scientific research but also the underlying processes of scientific revolution.

Since these (ahistorical) textbooks are the primary source of both scientific pedagogy and more general discussions of science, Kuhn emphasized that the historical aspects of science typically are hidden from students, practitioners, and scholars of science alike:

> Unless he has personally experienced a revolution in his own lifetime, the historical sense either of the working scientist or of the lay reader of textbook literature extends only to the outcome of the most recent revolutions in the field. (137)

Noting that the “temptation to write history backward is both omnipresent and perennial,” Kuhn suggested that the temptation is especially strong in the field of the physical sciences (138). This is because the results of scientific research are presumed to be independent of their historical context. This presupposition is, in turn, fundamental to current conceptions of the development of science:

> More historical detail, whether of a science’s present or of its past, or more responsibility to the historical details that are presented, could only give artificial status to human idiosyncracy, error, and confusion. Why dignify what science’s best and most persistent efforts have made it possible to discard? (138)

The contemporary position of science thus appears to be secure and free from error and confusion in all instances other than crisis and revolution. Apart from such (rare) instances, what basis or benefit can there be in questioning it that position or its apparent authority?

Kuhn explained that the pedagogical role of textbooks traditionally has been combined with the "generally unhistorical air of science writing" and "occasional systematic misconstructions" to support a cumulative view of scientific development (140). The implicit yet misleading impression that results from this view is that "science has reached its present state by a series of individual discoveries and inventions that, when gathered together, constitute the modern body of technical knowledge" (140). According to this image of science, scientists are engaged in a collaborative and mutually supportive enterprise: "[o]ne by one, in a process often compared to the addition of bricks to a building, scientists have added another fact, concept, law or theory to the body of information supplied in the contemporary science text" (140). In this respect, Kuhn proposed that textbooks have played a crucial role in supporting a "Whiggish" view of scientific history.

Drawing from his own investigations, Kuhn proposed that recent historiographic studies suggest a very different image of scientific development:

> Many of the puzzles of contemporary normal science did not exist until after the most recent scientific revolution. Very few of them can be traced back to the historic beginning of the science within which they now occur. Earlier generations pursued their own problems with their own instruments and their own canons of solution. Nor is it just the problems that have changed. Rather *the whole network of fact and theory* that the textbook paradigm fits to nature has shifted. (140-1, emphasis added)

Kuhn thus proposed that textbooks mislead by suggesting that current scientific problems and even current "facts" have been consistent throughout the history of science. What seems to occur in the course of scientific practice is, rather, the gradual and simultaneous adjustment of both scientific theories and scientific facts:

> Those theories . . . do "fit the facts," but only by transforming previously accessible information into facts that, for the preceding paradigm, had not existed at all. And that means that theories too do not evolve piecemeal to fit facts that were there all the time. Rather, they emerge together with the facts they fit from a revolutionary reformulation of the preceding scientific tradition, a tradition within which *the knowledge-mediated relationship between the scientist and nature* was not quite the same. (141, emphasis added)

By failing to articulate the linkages between scientific fact and scientific theory, textbooks thus suppress the extent to which the relationship between a scientist and nature is mediated. Yet this knowledge-mediated relationship changes over time in ways that are concealed by the "re-writing" of scientific texts.

To illustrate these points, Kuhn examined the traditional textbook account of Robert Boyle's development of the contemporary conception of an element. The pedagogic lessons that typically are drawn from this historical example are that "chemistry did not begin with the sulfa drugs . . . [and] that one of the scientist's traditional tasks is to invent concepts of this sort" (142). While acknowledging the value of these lessons, he claimed that "as history, the textbook version of Boyle's contribution is quite mistaken" (142). Specifically, Boyle's definition paraphrased the classical conception of an element (rather than proposing a new one) and, in fact, was used to argue that no such thing as a chemical element exists.

Kuhn acknowledged that in a broader scientific context, this mistaken attribution is trivial, yet he emphasized that,

> What is not trivial, however, is the impression of science fostered when this sort of mistake is first compounded and then built into the technical structure of the text. Like "time," "energy," "force," or "particle," the concept of an element is the sort of textbook ingredient that is often not invented or discovered at all. (142)

In Kuhn's view, any scientific concept can be understood fully only when it is considered in relation to the other aspects of scientific practice that also are present at that time:

> The scientific concepts to which [definitions point] gain full significance only when related, within a text or other systematic presentation, to other scientific concepts, to manipulative procedures, and to paradigm applications. (142)

Boyle's contribution thus was not his development of a conception of elements but his relation of the traditional conception to chemical manipulation and chemical theory. It was this more (broadly considered and contextualized) change that (only later and after further research and consideration) "transformed the notion into a tool quite different from what it had been before and transformed both chemistry and the chemist's world in the process" (143).

According to Kuhn, scientific revolutions reflect changes in the relationships, networks, and constituting factors that are collected within a paradigm. Once these are reconstituted under a new paradigm, earlier theories are adjudged "special cases;" "illogical;" or simply "wrong" with respect to "new" facts. Such judgments are appropriate within the context of the new paradigm. Yet from an historiographic perspective, it is clear that they presuppose the current, interrelated network of fact and theory. Earlier and fuller conceptions of the previous paradigm are rendered invisible as is the nature of the revolution whereby the new networks of fact and theory became accepted and established.

Historiographic Insights

Kuhn's encounter with the "invisibility" of revolutions began with his Aristotle experience, was repeated through his numerous historiographic investigations and finally, was explained in his essay "The Function of Measurement." In that later essay, Kuhn asserted that scientific textbooks conceal the nature of scientific revolutions and explained that while this is helpful for understanding the logical structure of scientific theories, it is misleading for understanding productive methods. In *Structure*, he extended these assertions beyond scientific texts to the popularizations and philosophical works that are based on them. He noted that problems, data, and theory are already articulated fully in all three of these "authoritative sources." Yet they are rendered obsolete by revolutions, which shift the "whole network of fact and theory." What is concealed, then, is not simply the nature of scientific revolutions but more fundamentally, the "knowledge-mediated relationship between the scientist and nature" (141).

Unfortunately, *Structure* provided little clarification of the relationship that is constituted by the network of fact and theory, how it could be discerned, the way in which it served to mediate the relationship between the scientist and nature, or how it provided the basis for scientific knowledge. Substantially more detail was provided, however, in Kuhn's earlier research and publications. His Aristotle experience highlighted the deep interrelation between fact and theory by revealing the fundamental change that occurred with scientific revolutions. The historiographic approach provided a method whereby such changes could be identified and the "Whiggish" biases of more traditional approaches could be eliminated. During these early periods, Kuhn characterized the recognition of these changes as involving a "gestalt shift," yet he noted that while gestalt psychology provided a basis for describing such changes, it could not account for them.

In "The Function of Measurement in Modern Physical Science" (FM 1956/1977), Kuhn followed the historiographic approach of studying *actual* scientific practices and found that measurement played a very different role in the conduct of science from the one traditionally supposed. In particular, the presumed relationship between measurement and scientific theories seemed to be inverted in the practice of actual science. Rather than using measurement as the basis for discovery or for proof of scientific theories, practicing scientists tended to use established theories to guide their expectations and refinements of measurement. This insight prompted Kuhn's assertions regarding the role of textbooks in scientific pedagogy and ultimately led to his explanation for the invisibility of revolutions.

While not articulated fully in *Structure*, Kuhn's earlier research suggests that the invisibility of revolutions is associated with differences (i.e., inversions) between the practice and the pedagogy of science. As explained in *Structure*, the changes associated with revolutions are changes in the knowledge-mediated relationship between the scientist and nature. These changes – and the knowledge-mediated relationship itself – are rendered invisible by the mechanisms used for scientific education. This occurs through inversion of the "actual" relationship as it is presented and taught to students.

What we must understand, then, is the "actual" relationship between scientific practice (as revealed by historiographic investigations) and scientific pedagogy (as taught through scientific textbooks). As a first step, we turn to Kuhn's discussion of the resolution of revolutions. For it is through such revolutions that the facts, theories, practices and texts associated with an "older" paradigm are replaced and rendered obsolete by those of a "newer" one. By examining how revolutions are resolved, we can understand the processes involved in the establishment and adjustment of the knowledge-mediated relationship between the scientist and nature. In this way, we can more clearly understand the nature of that relationship as it occurs in the practice of actual science.

The Resolution of Revolutions

Before a revolution can be rendered invisible, it must first be resolved. Yet this resolution also will seem invisible when viewed retrospectively, that is, from the perspective provided by the new paradigm. As such, Kuhn proposed that the philosophical views of probabilism, verificationism, and falsificationism suffered from the same retrospective myopia as views of cumulative development. This is because they misconstrued the true nature of the revolutionary shifts that are inherent in such development, failing to recognize the fundamental changes that occur in conceptions of (seemingly objective or neutral) "facts." Kuhn insisted that scientific facts are not simply given, nor can they provide the basis for a neutral observation language or the correspondence of language to nature. Rather, they are constituent parts of a particular paradigm. Without such a basis, scientific facts cannot be used to verify or to falsify a theory independent of the paradigm by which they are established *as* facts. In this respect, neither verification nor falsification is viable as a means for resolving revolutions.

It seems, then, that there are no prospects for a neutral observation language to be used in either verifying or falsifying a particular paradigm. When a new paradigm is being evaluated, it will (to some extent) be incommensurable with respect to the old paradigm because it will differ in its conceptions of what "both the world and . . . science are like" (42). The choice between paradigms thus will be much more complex than traditionally suggested:

> Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations to one another. That is why a law that cannot even be demonstrated to one group of scientists may occasionally seem intuitively obvious to another. Equally, it is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all. (150)

Kuhn acknowledged that such conversions are hard-won. Because "neither proof nor error is at issue," he proposed that the conversion of a community to a new paradigm is a process that "cannot be forced" (151). Historically, even the greatest scientific minds – Copernicus, Newton, Priestly, Kelvin, Darwin, and Planck, among many others – experienced great difficulties and long delays in having their theories accepted by the scientific community.

Kuhn reformulated philosophers' of science traditional reliance upon verification and falsification to consider "how conversion is induced and how resisted" (152). He emphasized that this question is new, in that "it is asked about techniques of persuasion, or about argument and counter-argument in a situation in which there can be no proof" (152). Given the novelty of this approach, Kuhn offered only a "very partial and impressionistic survey," noting that further consideration demands a sort of study not previously undertaken (152). He explained that, contrary to established views, the most productive insights may be gained not from studying the arguments for the various paradigms because individuals "embrace a new paradigm for all sorts of reasons and usually for several at once" (152). Instead, he recommended a sociological approach to the question of conversion, that is, the study of "the sort of community that always sooner or later re-forms as a single group" (152-3).[91]

Kuhn suggested that the complexity and challenges involved in the conversion of a scientific community to a new paradigm are neither an unfortunate aspect of science nor "a violation of scientific standards but an index to the nature of scientific research itself" (151). Those who resist the new paradigm simultaneously exhibit confidence in the problem-solving ability of their current paradigm: "[t]he source of resistance is the assurance that the older paradigm will ultimately solve all its problems, that nature can be shoved into the box the paradigm provides" (151-2). It is this assurance, Kuhn proposed, that "makes normal or puzzle-solving science possible:"

> . . . it is only through normal science that the professional community of scientists succeeds, first, in exploiting the potential scope and precision of the older paradigm and, then, in isolating the difficulty through the study of which a new paradigm may emerge. (152)

Confidence in a paradigm – and resistance to alternative candidates – thus is not a barrier to scientific progress but instead serves as a requirement for its continuation.

Similarly, proponents of the new paradigm "have faith that the new paradigm will succeed with the many large problems that confront it" and that this success will exceed what is possible with the current paradigm (158). They will argue that the new paradigm can resolve the anomalies that prompted the current crisis; that it can predict phenomena unanticipated by the old paradigm; or that it offers a sense of appropriateness or aesthetic not otherwise available. In the earliest stages, Kuhn acknowledged that these claims – and the new candidate itself – will be rather undeveloped and "rough" (156). In this respect, their primary goal must be to attract "first supporters, men who will develop [the new paradigm] to the point where hard-headed arguments can be produced and multiplied" (158). This development requires that the early supporters of a paradigm must work to "improve it, explore its possibilities, and show what it would be like to belong to the community guided by it" (159).

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[91] Kuhn concluded the 1969 Postscript with a similar call for a study into the characteristics of communities in the sciences and in other fields:

> How does one elect and how is one elected to membership in a particular community, scientific or not? What is the process and what are the stages of socialization to the group? What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration? A fuller understanding of science will depend on answers to other sorts of questions as well, but there is no area in which more work is so badly needed. Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it. (*SSR*-PS 1969/1970a 209-10)

On this account, then, the choice between alternative paths of scientific research is determined not by logic alone but also by assurance or faith in the future ability of a given paradigm to resolve scientific puzzles.[92] Given that paradigms will differ in their problems, standards for solution, methods, instrumentation, and views of nature, the issue at stake the community is not simply the relative problem-solving ability of the competing paradigms but "alternative ways of practicing science" (157). Advocates of the old paradigm will presume that it can be adapted to meet the new challenges whereas proponents of the new paradigm will suggest that their proposed replacement holds greater promise. If the perceived strengths of the competing paradigms are comparable, then, Kuhn proposed, the balance will tip toward tradition and continued adherence to the established paradigm.

Given the nature of paradigm choice, Kuhn suggested that a new paradigm will not be accepted through a single conversion of the scientific community. The multitude of factors that influence paradigm choice suggests that no single argument can suffice, either for an individual or for the group as a whole. Rather, as the battle among paradigms continues, "what occurs is an increasing shift in the distribution of professional allegiances" (158). This gradual shift is accompanied by further articulation of the new paradigm and growth in the number of experiments, articles, and books based upon it. The results of these efforts, in turn, affect the conversion of members of the scientific community.

Kuhn proposed that in the case of a scientific revolution, more and more scientists will be converted to the new paradigm, "until at last only a few elderly hold-outs remain" (159). Yet he insisted that even when a new paradigm is established, these hold-outs are not necessarily "wrong" in the traditional sense of the word:

> Though the historian can always find men – Priestley, for instance – who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist. (159)

By definition – that is, according to the new paradigm and its range of rules and commitments – such individuals will no longer be considered to be members of the (now reconstituted) "scientific" community.

[92] Kuhn did not specify here the basis of that assurance or that faith, nor the ways in which it might be justified. He did make such an attempt in several later essays, including "Objectivity, Value Judgment, and Theory Choice" (OVJ 1973/1977, 320-39) and "Rationality and Theory Choice" (RTC 1983/2000, 208-15).

Historiographic Insights

While not explicitly addressed in his earlier research, the question of how revolutions may be resolved was central to his interest in the philosophical implications of the new historiography. To the extent that understanding Aristotelian physics required a "gestalt shift" or entailed a different world view, the question of how such holistic change could occur (or how it could be justified) was central to understanding the nature of science. Kuhn initially directed his attention to the implications of this fundamental change for accumulative views of the development of science. Yet if such views are shown to be problematic – if science does not reflect the gradual accumulation of knowledge through the application of sound method – then one must provide not only an alternative view of development but also an alternative view of its underlying mechanisms.

Kuhn's earliest historiographic studies focused on the occurrence of revolutions and the emergence of anomalies that prompted investigations in particular areas of previous knowledge and beliefs. These studies followed the findings of other scholars in revealing the complexity of scientific discovery and Kuhn emphasized that discoveries were not instantaneous moments of genius but involved substantial preceding activity, and thus possessed a definable structure. Only in his later historical developmental accounts did Kuhn begin to examine the processes and mechanisms underlying the structure of scientific discovery and the changes of knowledge and belief that historiographic study revealed.

In "Energy Conservation" (EC 1959/1977), Kuhn proposed that one must understand not only the (theoretical) prerequisites for discovery but also the (contextual) trigger factors that prompted discovery at a particular time (in this case, relatively simultaneously by several individuals and in noticeably different ways). The three trigger factors included conversion processes, whereby particular insights might be applied to different types of phenomena; engines, which provided a standard of measurement, and the philosophy of nature, which provided a predisposition to a particular explanation of natural phenomena. These three factors relate to the "facts" rather than the "theories" of nature. Clearly, they are not solely logical, but neither are they sociological or purely metaphysical. Rather, they have to do with the way that scientists investigate the facts of nature and, to some extent, what they expect to find there. They have to do with the empirical aspects of scientific investigation.

Similar considerations were highlighted in "The Function of Measurement" (FM 1956/1977) in which Kuhn emphasized that there are often few points of direct contact between the scientist and nature. Measurement thus is guided by theory, rather than vice-versa, and genius leaps ahead of the facts, both guiding experimentation and instrumentation and requiring their explication of proposed views. Neither measurement nor experimentation nor instrumentation can be used to verify or to falsify a particular paradigm. Rather, these can be used only to indicate the relative precision of two competing theories. Choice is thus a three-way, comparative evaluation, rather than the proof or refutation of an individual theory.

None of these considerations was examined in *Structure's* presentation of the resolution of revolutions. Kuhn did not emphasize the way in which the facts of nature are investigated by scientists nor did he consider how empirical investigations of science involve the interrelation of theoretical genius, experimentation, and instrumentation. Finally, in rejecting verificationism and falsificationism, he did not make it clear that the error of these approaches lay in their consideration of the "truth" of an individual theory, rather than the comparison of competing theories. Although Kuhn later emphasized many of these points, their neglect in the presentation of *Structure* prompted a number of readers to conclude that his proposed (sociological) image of science was irrational, relativistic, and subject to mob psychology.

Scientific Progress through Revolution

In the concluding chapter, "Progress through Revolutions," Kuhn considered the broader implications of his "schematic description of scientific development" (160). He noted the problematic nature of providing such a conclusion for the account, which seems to draw into question established views of scientific progress:

> Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call science? The most usual answers to that question have been denied in the body of this essay. We must conclude it by asking whether substitutes can be found. (160)

Kuhn noted that traditionally there have been "inextricable connections between our notions of science and of progress," which have been both semantic and practical, particularly during the Renaissance (161). He suggested that his reconceptualized image of science demanded a similar examination – and, presumably, a complementary reconceptualization – of its progress. Thus to the extent that the practices of science encompass two different types of activities (i.e., normal and extraordinary science), it follows that the development of science thereby exhibits two different types of progress.

Progress in Normal Science

Kuhn first considered how progress might occur in normal science, that is, when the members of a community share a common paradigm. He suggested that from the perspective of this community, progress is an inherent part of the creative research conducted under the paradigm. As such, it is the inevitable result of an "apparently united group" working together toward a "defined goal:"

> No creative school recognizes a category of work that is, on the one hand, a creative success, but is not, on the other, an addition to the collective achievement of the group. If we doubt, as many do, that non-scientific fields make progress, that cannot be because individual schools make none. Rather, it must be because there are always competing schools, each of which constantly questions the very foundations of the others. (162-3)

In this sense, progress in normal science lies "in the eye of the beholder," that is, in the paradigm-guided views of a particular scientific community (163). This sort of "progress" is the inevitable result of normal scientific activity. It is inevitable, first, because the scientific community shares paradigm-induced standards for scientific achievement (and thus for scientific progress). Secondly, agreement on fundamentals promotes the increased effectiveness and efficiency of scientific activity. Normal scientific research focuses on particular areas, and its results are evaluated by members of a community, who share (roughly) the same values, beliefs, and standards. Finally, educational processes in normal science provide a focus and rigidity that support not only normal scientific activity but also the identification of anomalies, which indicate areas requiring further development. Kuhn concluded that in normal science, "a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Further, the result of solving these problems must inevitably be progress" (166).

Progress in Extraordinary Science

Turning to the prospects for progress in extraordinary science, Kuhn asked, "[w]hy should progress also be the apparently universal concomitant of scientific revolutions?" (166). He again looked to the scientific community and considered “what else the result of a revolution could be” (Ibid.). As in the achievements of normal science, he pointed out that the resolution of a revolution is both interpreted and evaluated by the community that ultimately proves successful in either defending the old paradigm or establishing a new one. Furthermore, by reestablishing or revising the textbooks that educate new members, the community is “in an excellent position to make certain that future members of their community will see past history in the same way” (166). Previous revolutions thus are rendered invisible, and scientific development is understood as a cumulative path leading toward the now-dominant paradigm.

Kuhn admitted that this characterization suggests an Orwellian view of scientists as “the victim[s] of a history rewritten by the powers that be” (167). He noted that the analogy is appropriate in that revolutions involve both losses and gains, scientists being “peculiarly blind” to the former (167). Yet Kuhn also insisted that the progress that accompanies scientific revolutions is more than a self-satisfied appellation imposed by the scientific equivalent of “Big Brother,” where “might makes right” (167). In considering scientific progress through revolution, he emphasized that one must also consider “*the nature of the process and of the authority* by which the choice between paradigms is made” (167, emphasis added).

For Kuhn, the scientific community is the center of authority in ensuring progress through revolution: “the very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community” (167). The requirements for membership in such a community include a concern with solving detailed problems about the behavior of nature and the acceptance of solutions on the basis of community agreement, rather than personal views. This second requirement posits members of the scientific community as “the sole possessors of the rules of the game or of some equivalent basis for unequivocal judgments”(168). To deny such a shared basis for evaluations is, Kuhn proposed, “to admit the existence of incompatible standards of scientific achievement” (168). In conclusion, he insisted that, as with normal science, in extraordinary science, “[t]he scientific community is a supremely efficient instrument” – in this case, for “maximizing the number and precision of the problems solved through paradigm change” (169).

In considering the choice between two competing paradigms, Kuhn outlined a number of factors influencing the community. First, he stated that the most basic unit of scientific achievement is the solved problem. For this reason, a scientific community will be loath to adopt a new paradigm that undercuts previously resolved scientific problems: "nature itself must first undermine professional security by making prior achievements seem problematic" (169). Thus secondly, the new paradigm must seem to resolve such issues in ways that are unavailable under the old paradigm. Finally, the new paradigm must support, if not reinforce, the bulk of concrete problem-solutions already achieved by its predecessors. Thus new paradigms, while involving some loss of previous problem-solving ability, must also preserve many past achievements and promise many future achievements.

These considerations illustrate the extent to which scientific communities both preserve and expand their scientific achievements. Specifically, they highlight one of the key characteristics of paradigm choice: "a community of scientific specialists will do all that it can to ensure the continuing growth of the assembled data that it can treat with precision and detail" (169-70). Although the community may sustain losses in some of its prior achievements and selected old problems may be deemed irrelevant, the new paradigm will typically provide a "narrower scope of professional concerns," which increase specialization and thus decrease communication with other groups (170). In this way, Kuhn explained,

> Though science surely grows in depth, it may not grow in breadth as well. If it does so, that breadth is made manifest mainly in the proliferation of scientific specialties, not in the scope of any single specialty alone. Yet despite these and other losses to the individual communities, the nature of such communities provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem-solution will grow and grow. (170)

Thus just as a community will "progress" through revolutions that reflect greater and greater specialization, so science as a whole will "progress" through the proliferation of focused specialties.[93]

A Different Sort of Progress

Kuhn proposed that these considerations point toward "a more refined solution of the problem of progress in the sciences" (170). Specifically, progress should be understood as "a process of evolution *from* primitive beginnings – a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature" (170). This conception of progress involves development *from* a beginning rather than *toward* a specified end.

[93] One might also conclude (although Kuhn did not) that just as the choice of a new paradigm involves losses as well as gains, so the gains in precision that result from increased specialization of a field may be offset by losses in the ability to comprehend the field in its entirety.

Science thus is not, as traditionally conceived, an enterprise that moves ever closer toward truth or some other goal "set by nature in advance" (171). Rather, Kuhn proposed that no such goal is required for science to function effectively and to exhibit progress. Considering the (similar) proposition put forward by Charles Darwin, Kuhn noted that evolution was not directed by truth nor by god, but,

> Instead, natural selection, operating in the given environment and with the actual organisms presently at hand, was responsible for the gradual but steady emergence of more elaborate, further articulated, and vastly more specialized organisms. (172)

In the same way, he proposed, science does not require a teleological conception of truth to guide the activities of scientists along a path of development. Rather, it needs only the careful and informed choices of the relevant scientific community:

> The process described in Section XII as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar. (172-3)

This "evolutionary view of science" suggests that progress through revolution is possible because of the special nature of the scientific community and of "[t]he world of which that community is a part" (173). Much of Kuhn's essay represents an attempt to illuminate the former. Of the latter, he noted that the question of the special nature of the world "is as old as science itself, and it remains unanswered" (173).

Historiographic Insights

Our historiographic investigations provide relatively limited insight into *Structure's* concluding account of development and progress in the sciences. In many ways, this absence is itself revealing, for it suggests that the assertions were not drawn from Kuhn's detailed historiographic investigations nor even from his historical developmental accounts. Rather, the assertions seemed to emerge as the implications of the schematic account presented in *Structure*. In this respect, they provide insight into the central aspects of that account; however, they also suffer from its limitations.

In introducing this presentation, Kuhn emphasized the interconnection between conceptions of science and of progress. To the extent that *Structure* outlines a different image of science, it thus would seem to suggest a different image of progress. Just as normal science differs from extraordinary science, so normal "progress" would seem to differ from extraordinary (or revolutionary) "progress." *Structure's* discussions of progress relied upon the notions of paradigms, normal science, anomalies, and revolution, as well as insights regarding the nature of scientific education, the primacy of problem-solving, and the role of the scientific community. Yet as we have seen, important aspects of these notions and insights remained unclear and even confusing in *Structure's* presentation.

First, the notion of paradigms was less fully developed in the presentation of *Structure* than in Kuhn's earlier research. As presented in *Structure*, the notion was neither as clear nor as self-evident as one might expect from the central concept of a descriptive or explanatory account. Its unique characteristics and the basis for its authority could not be discerned readily, that is, without the prior introduction of other concepts or illustration through (more detailed) historiographic case studies. As presented, it seemed to be vague or ambiguous and without clear authority. This ambiguity and absence of justification, in turn, affected the strength and authority both of *Structure's* other concepts and of its schematic account.

Secondly, the choice between competing paradigms was more fully explicated in Kuhn's earlier research than in *Structure's* presentation. While his earlier work emphasized the struggles with the network of fact and theory by which scientists attempted to establish and to adjust their relation to nature, *Structure* provided only limited accounts of these activities. The work thereby emphasized the sociological aspects of scientific activity yet neglected many of the empirical aspects investigated in Kuhn's earlier research.

These limitations are evident in *Structure's* presentation of both types of scientific progress. "Normal" progress is characterized as somewhat relative (i.e., as in the eye of the beholder) and as subject to the determinations of the scientific community (i.e., as inevitable based on the community's agreement on fundamentals and the nature of scientific education). While denying the Orwellian aspects of these activities, Kuhn provided no basis by which such tendencies could be avoided. As we will see in his subsequent clarifications, a more precise account of paradigms as providing concrete puzzle solutions (and of normal science as characterized by puzzle-solving) might have mitigated, or at least more clearly addressed, these implications of relativism or of mob psychology.

Structure's consideration of "extraordinary" or revolutionary progress also is affected by the limitations of its account of extraordinary science. Interestingly, the discussion of paradigm choice with respect to progress includes a much more detailed account of the role of puzzle-solving and the importance of precision than the earlier (primarily sociological) account of choice. This greater detail provides important illumination regarding the "special" nature of the scientific community, that is, the nature of the processes and authority by which choice is made. Yet even with this detail, the discussion fails to offer an adequate philosophical consideration of the basis and authority for choice.

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Throughout this (historiographic) interpretation of *Structure*, we have identified important points of difference between Kuhn's earlier research and the theory presented in *Structure*. Our historiographic perspective has highlighted omissions, ambiguities, and further developments that provide the basis for a deeper understanding of *Structure's* (schematic) account. It suggests that the account was not complete with respect to either Kuhn's earlier research or the subject matter that it would explain. In this respect, the "inversion" of concepts between Kuhn's research activities and the presentation of *Structure* was incomplete, or perhaps, premature. As the seemingly "central" concept of *Structure*, the notion of paradigms was especially vulnerable to this incompleteness; however, its vulnerability also extended to the entirety of *Structure's* schematic account. Similarly, the discussion of paradigm choice was not yet sufficiently developed such that it could account for the philosophical implications of Kuhn's new image of science. As we will see, these limitations were consequential for the interpretation and evaluation of *Structure*.

<u>Chapter Three</u>

The Interpretive Legacy of *Structure*

Our historiographic investigation of Kuhn's early research and our reconsideration of the theory presented in *Structure* provide a basis for a reconsideration of its interpretive debates. This reconsideration is organized around the three central concerns Kuhn identified in his reflections on the 1965 colloquium: concerns about method; differences in views of normal science; and questions regarding the change from one normal-scientific tradition to another (RMC 1969/1970c). These concerns are examined, along with the direct or indirect responses outlined by Kuhn, Hesse, and Masterman. Both the concerns and responses are then evaluated and implications for the interpretive debates are considered.

When viewed from the perspective of their approach to the work, interpretations and evaluations of *Structure* by philosophers of science were systematic and their accusations of irrationality, relativism and mob psychology were appropriate. In particular, they identified in *Structure* a number of important conceptual weaknesses, logical gaps and potential threats to the normative conduct of science.

From the perspective of our historiographic examination of Kuhn's research activities, however, philosophers of science seem to have misconstrued and even to have overlooked important aspects of *Structure*. In particular, Kuhn's historiographic approach seems to provide a distinctive view of scientific activity that highlights aspects of scientific development that are not otherwise apparent. While these points of distinction were not clear in the theory presented in *Structure* and many of the work's central concepts remained vague and ambiguous, both considerations support an interpretation of the work that differs from the views of philosophers of science in important ways.

Section One

Questions of Method

Philosophers of science expressed concerns about the validity of the philosophical conclusions that Kuhn had developed from his historiographic investigations. Specifically, they were concerned that Kuhn had confused the appropriate relationship between the historical, descriptive accounts developed in the history of science and the logical, normative conclusions developed in the philosophy of science. It seemed that Kuhn would use the vagaries, politics, and sociology of actual science to guide scientific activities. From a philosophical perspective, such an approach threatened to undercut the most fundamental objectives of not only the philosophy of science but also science itself. Stated bluntly by John Watkins, "methodology, as I understand it, is concerned with science at its best, or with science as it should be conducted, rather than with hack science" (Watkins 1970, 27).

While applauding the use of history as a basis for illustrating or testing philosophical claims, these scholars questioned the viability of deriving normative claims from historical research.[94] They insisted that history alone was an inadequate basis for drawing philosophical or normative claims and expressed particular concern regarding Kuhn's rejection of the possibility for objective criticism of such claims. While valuing the insights provided by the so-called historiographic revolution, they did not attribute any philosophical import to historiographic investigations of particular scientific achievements. In their view, normative conclusions about science could be gained only from a philosophical approach derived from first principles.[95]

Responses to Concerns about Method

In responding to concerns of method, Kuhn insisted that he and the philosophers at the colloquium "are scarcely to be distinguished by our methods" (RMC 1969/1970c, 233). Emphasizing the distinctions between historicist and positivist approaches to philosophy of science, Kuhn noted that both he and the

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[94] See in particular, (Shapere 1964, 393-4), (Popper 1970, 53), and (Feyerabend 1970, 197).

[95] It is interesting to consider the relationship between this view and their proposal that the demarcation point of science is logical criticism. Specifically, did the historicist philosophers view the demarcation point as a cause or as an effect of their presumptions regarding normative conclusions? Alternatively, are these two conceptions deeply interrelated?

Popperians "do historical research and rely both on it and on observation of contemporary scientists in developing our viewpoints" (Ibid.). In both the historiographic and the philosophical perspectives that result, he continued, "the descriptive and the normative are inextricably mixed" (Ibid.). Considering the criteria for identifying the descriptive and the normative aspects of a historical scientific activity, Kuhn again insisted that his approach was similar to that of historicist philosophers of science: "My criterion for emphasizing any particular aspect of scientific behavior is therefore not simply that it occurs nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge" (Ibid., 236-7).

Kuhn proposed that what distinguished him from historicist philosophers was his investigation of "the normal group rather than the normal mind" (Ibid., 241). In particular, he suggested that explaining the success of actual science required understanding not only the logical structure of scientific theories but also the activities of actual scientists, including the factors that influence them as a group. For it is as a group, Kuhn insisted, that individual scientists may share ideals yet also exhibit the differences in behavior that lead to discovery and development. He defended his approach by noting of Lakatos in particular, "his way will not do if he hopes to explain an enterprise practiced by people" (Ibid., 240).

The comments of L. Pearce Williams and Margaret Masterman at the 1965 symposium provide additional insights into these exchanges. Williams, an historian by training, expressed hesitation in accepting the proposals of either Popper or Kuhn without further historical research. In this call for more examples of scientific activity, Williams implicitly asserted that the shift from descriptive to normative accounts requires a broader historical basis than was currently available: "we simply do not know enough to permit a philosophical structure to be created on a historical foundation" (Williams 1970, 50).

While Williams seemed to agree with Kuhn's assessment that his method was scarcely distinguishable from that of Popper, Margaret Masterman (1970) suggested that Kuhn's method reflected a new approach to traditional questions, which philosophers of science had not been able to address previously. On this basis, she described Kuhn as "one of the outstanding philosophers of science of our time" (Ibid., 59). Masterman proposed that Kuhn had discovered a way to examine scientific development from the perspective of actual science, without falling victim to the "aetherialism" that characterized work in the philosophy of science (Ibid., 72). By examining the practice of actual science, she suggested, Kuhn had been able to identify the concrete aspects of scientific activity and to avoid the abstractness that characterized philosophical accounts. She proposed that it was this concreteness that made Kuhn's work attractive to practicing scientists yet confusing to philosophers of science.

Concerns and discussions of Kuhn's method thus focused on the similarities and differences of historical, philosophical, and historiographic approaches, and the viability of the descriptive and normative claims made by each. While Popper and his followers rejected Kuhn's attempt to move from historical, descriptive accounts to philosophical, normative conclusions, Kuhn insisted that their approaches were "scarcely to be distinguished," and that descriptive and normative considerations were "inextricably mixed" in the investigations of both groups. Williams seemed to support Kuhn's view of similarity yet supported Popper's assertion of the shift from descriptive to normative accounts (rather than their intermixture). While Williams recommended further historical investigation by both groups, Kuhn insisted that further study would not bridge the gap between him and Popper. Finally, Masterman claimed that the distinctive, concrete aspects of Kuhn's approach were even more consequential than either he or his philosophical critics seemed to appreciate. It was this concreteness, she proposed, that enabled Kuhn to avoid the aetherialism that characterized work in the philosophy of science and to reveal "the crude forms and early stages of a science" (Masterman 1970, 68).

Distinctive Aspects of Kuhn's Method

Although Kuhn was one of a number of other scholars interested in the philosophical implications of the new historiography, his work was both distinctive and ambitious in its exploration of the linkages between science, history, and philosophy. A physicist by training, he brought to his research a detailed knowledge of (modern physical) science that was not present in the work of either the historians or the philosophers who had pioneered the new historiography. Yet he approached his historical investigations with a guiding interest in their philosophical implications, thus he focused his attention on particular aspects of historical scientific activity and their implications for the historical patterns of scientific development.

Kuhn initially stated that he and historicist philosophers of science were not distinguished by their methods; however, he changed this position shortly after the 1965 symposium. In March 1968, he delivered a lecture, entitled, "The Relations between the History and the Philosophy of Science" (RHPS

1968/1977, 3-20), which initiated what was to become an ongoing consideration of historiography, the history of science and the relation of these fields to other disciplines.[96] In these essays, subsequent publications and later interviews, Kuhn proposed that his most fundamental differences from various interpreters were a reliance on historiographic method and techniques for identifying the underlying conceptual changes that historiography revealed.

History and the Philosophy of Science

In the 1968 lecture on the history and philosophy of science, Kuhn proposed that the objectives and methods typically employed in the history of science differ from those in the philosophy of science in ways that could prove beneficial to the latter. Although Kuhn had described the unique objectives and general approach of the new historiography in the Introduction to *Structure*, he had not previously specified its associated activities and methods. Nor had he compared these to other disciplines or considered the implications of the differences. Particularly in view of Kuhn's growing difficulties in communicating with philosophers, the lecture thus offers valuable insight into potential differences between the approaches of the two fields.[97]

Kuhn proposed first, that an historian of science investigates actual science and its development over time. In doing so, she seeks to describe scientific "facts" and to explain the connections between them (RHPS, 15).[98] In contrast, the philosopher of science

[96] Between 1968 and 1971 alone, Kuhn authored a total of six papers exploring the relationships between the objectives and methods of the history of science and those in philosophy of science, art, and history. These six early essays included "The Relations between the History and the Philosophy of Science" (RHPS 1968/1977, 3-20); "The History of Science" (HS 1968/1977, 105-26); "Comment on the Relations of Science and Art" (1969/1977, 340-51); "Notes on Lakatos," (1970); "Alexandre Koyré and the History of Science: On an Intellectual Revolution," (1970); and "Relations Between History and History of Science" (RHHS 1971/1977, 127-61). Together with six later publications, these essays also addressed the methodological evolution of the history of science as it became more established as an academic discipline.

[97] Unfortunately, this lecture was not published until 1977 and thus provided little practical assistance for Kuhn's early engagements with philosophers of science. The failure to publish this lecture was especially unfortunate in that the differences between the two fields were elided in Kuhn's published response to commentators at the 1965 conference, "Reflections on My Critics" (RMC 1969/1970c).

[98] In making this characterization, Kuhn specified that he was speaking of "that central part of the field that is concerned with the evolution of scientific ideas, methods, and techniques, not the increasingly significant portion that emphasizes the social setting of science, particularly changing patterns of scientific education, institutionalization, and support, both moral and financial (RHPS, 12).

> . . . aims principally at explicit generalizations and at those with universal scope. He is no teller of stories, true or false. His goal is to discover and state what is true at all times and places rather than to impart understanding of what occurred at a particular time and place. (RHPS, 5)

The objective of the historian thus is to understand the practice of science and how it changes over time, whereas the philosopher of science seeks to identify the universal generalizations that constitute the "truth" of science at all times and places.

Given these differences in objectives, Kuhn noted that it is not surprising that historians and philosophers differ in the subject matter that they investigate. The historian generally relies on primary source materials, researching a "multitude of particulars, idiosyncratic details" and placing them within a temporal context (RHPS, 14). In contrast, Kuhn explained that the philosopher's reliance on logical criticism requires rational reconstruction of only those elements that are deemed most essential to scientific knowledge. While the historian presents the results of her investigations in the form of a narrative account, the philosopher provides a highly focused, logical analysis and explication of universal generalizations.

Historical Narratives in the History of Science

Kuhn emphasized that in developing and presenting an historical narrative, "success . . . depends not only on accuracy but also on structure. The historical narrative must render plausible and comprehensible the events it describes" (RHPS, 5). The explanatory force of an historical narrative thus is not carried by its adherence to governing laws, although Kuhn pointed out that it seems to support and to exemplify these in its finished form.[99] Instead, the force of a narrative is established by "the facts the historian presents and the manner in which he juxtaposes them" (RHPS, 16). In this respect, the authority of an historical account lies in its accuracy and the coherence of its structure, that is, in its ability to explain the multitude of historical facts. Yet this authority is concealed in the finished form of the narrative, which presents its constituent facts as (mere) illustrations of the "laws" that initially were established and rendered plausible by that narrative structure.

[99] The presumption that history follows laws of nature and society, described as the "covering law model," thus is incorrect. Historical narratives are not reducible to such laws; nor can those narratives provide an adequate basis for prediction.

Kuhn suggested that this inherent "autonomy and integrity of historical understanding," while not apparent from the finished product of the historical narrative itself, is evident in the personal experience of putting together that narrative (RHPS, 18). Like a child constructing a puzzle, the historian must,

> . . . select from [pieces of "data"] a set that can be juxtaposed to provide the elements of what, in the child's case, would be a picture of recognizable objects plausibly juxtaposed and of what, for the historian and his reader, is a plausible narrative involving recognizable motives and behaviors. (RHPS, 17)

The historical narrative thus must provide not only an accurate description of activities but also a "plausible" explanation of how those activities develop and change over time. It must be both a (recognizably) descriptive and a (plausible) explanatory account of that particular situation.

Importantly, Kuhn noted that, "[l]ike the child with the puzzle, the historian at work is governed by rules that may not be violated," including the avoidance of gaps, discontinuities, or contradictions within the story or with known laws of nature and society (Ibid). These rules serve to limit (yet not to determine) the outcome of the historian's activity, for which Kuhn proposed that the "basic criterion for having done the job right is the primitive recognition that the pieces fit to form a familiar, if previously unseen, product" (Ibid.).

In Kuhn's view, the most fundamental criteria for a "plausible" historical narrative is this recognition of a *primitive similarity relationship*. He insisted that this primitive similarity is not reducible either to prior criteria or to law-like reformulation:

> The child has seen pictures, the historian behavior patterns, similar to these before. That recognition of similarity is, I believe, prior to any answer to the question, similar with respect to what? Though it can be rationally understood . . . the similarity relation does not lend itself to lawlike reformulation. It is global, not reducible to a unique set of prior criteria more primitive than the similarity relationship itself. (Ibid.)

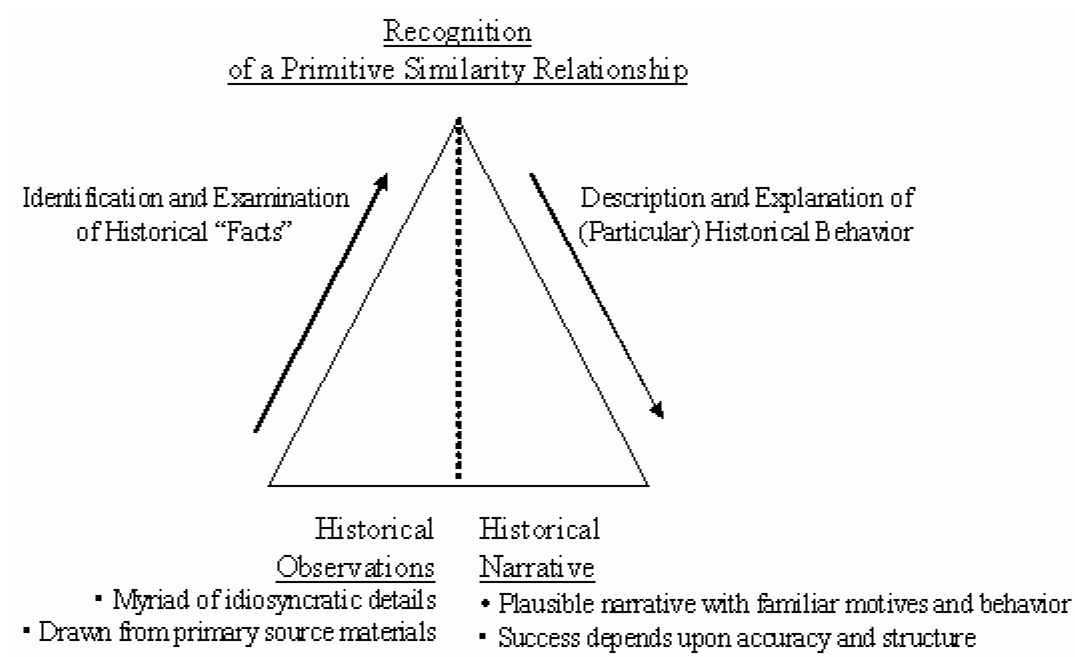
Noting a parallel with the partial dependence of the physical sciences on "the same primitive similarity relation between concrete examples, or paradigms, of successful scientific work," Kuhn proposed that,

> . . . in history that obscure global relationship carries virtually the entire burden of connecting fact. If history is explanatory, that is not because its narratives are covered by general laws. Rather it is because the reader who says, "Now I know what happened," is simultaneously saying, "Now it makes sense; now I understand; what was for me previously a mere list of facts has fallen into a recognizable pattern." (RHPS, 17-18)

An historical narrative, while limited by rules and subject to basic criteria for its preparation, thus cannot be reduced to those criteria nor to any reformulation or reconfiguration more fundamental than the initial recognition of similarity. Kuhn proposed that it is on the basis of this primitive similarity relation that the “multitude of particular, idiosyncratic details” take on structure and may be formed by the historian into an historical narrative (RHPS, 14).

Kuhn’s description of the historical approach can be characterized in terms of the relationship between historical observations and the historical narrative that describes and explains them (**Figure 1**, below). This relationship resembles a pyramid in which the research and development of the narrative involves the identification and investigation of a myriad of observations and details. Selecting from among these, the historian must identify a primitive similarity relationship that connects them to each other in a plausible narrative with recognizable motives and behaviors. This primitive relationship may be refined and expanded through further reconsideration of available observations, such that a fully descriptive and explanatory historical narrative may be developed and presented.

Figure 1: The Development of an Historical Narrative



Note: This figure and the ones that follow are adapted from (Spear 2002).

Although the primitive similarity relationship is not governed by rules or “covering” laws, Kuhn proposed that the narrative that is developed must avoid gaps, inconsistencies, internal contradictions, and contradictions with established laws. Thus while the content of the historical narrative is not governed by laws or rules, the structure of the narrative is constrained, along with the processes by which it is developed. To the extent that these "rules" are followed, the final narrative will be both descriptive and explanatory, that is, it will be a "proper" historical narrative. It is important to note, however, that the explanatory power of the narrative will extend only to the observations and circumstances of that particular historical situation.

Universal Generalizations in the Philosophy of Science

Having outlined the distinctive aspects of an historiographic approach and its resulting historical narrative, Kuhn suggested that the predominant tendencies in philosophy of science "aim at goals and perceive materials in ways more likely to mislead than to illuminate historical research" (RHPS, 11). In particular, he pointed out that philosophers of science seek timeless generalizations about scientific knowledge, rather than an understanding of the primitive similarity relationship that may exist between concrete examples. As a result of this emphasis on universal generalizations (rather than the similarities of concrete examples), philosophers of science understand the development of science as the incremental, cumulative development of scientific theory over time.

Kuhn proposed that, given this approach, philosophers of science often neglect the dynamic or temporal aspects of actual scientific activity. As a result, they misconstrue the nature of science in a way that may be corrected through the more activity-based approaches of the history of science:

> To the philosophically minded historian, the philosopher of science often seems to have mistaken a few selected elements for the whole and then forced them to serve functions for which they may be unsuited in principle and which they surely do not perform in practice, however abstractly that practice be described. (RHPS, 14)

Kuhn traced these difficulties to the separation of philosophy of science from its scientific subject matter. He suggested that the history of science can bridge this gap through its concern with the changes in and the development of scientific activity over time.

In particular, Kuhn proposed that the historian's study of process and development over time highlights an important and consequential distinction between empirical laws and scientific theories:

> Laws . . . to the extent that they are purely empirical, enter science as net additions to knowledge and are never thereafter entirely displaced. They may cease to be of interest and therefore remain uncited, but that is another matter. Important difficulties do, I repeat, confront the elaboration of this position, for it is no longer clear just what it would be for a law to be purely empirical.

> Nevertheless, as an admitted idealization, this standard account of empirical laws fits the historian's experience quite well.
>
> With respect to theories the situation is different. The tradition introduces them as collections or sets of law. . . . it thereafter assimilates theories to laws as closely as possible. That assimilation does not fit the historian's experience at all well. When he looks at a given period in the past he can find gaps in knowledge later to be filled by empirical laws. . . . But the historian seldom or never finds similar gaps to be filled by later theory. *In its day, Aristotelian physics covered the accessible and imaginable world as completely as Newtonian physics later would*. To introduce the latter, the former had to be literally displaced. After that occurred, furthermore, efforts to recapture Aristotelian theory presented difficulties of a very different nature from those required to recapture an empirical law. *Theories, as the historian knows them, cannot be decomposed into constituent elements for purposes of direct comparison either with nature or with each other*. That is not to say that they cannot be analytically decomposed at all, but rather that the law-like parts produced by analysis cannot, unlike empirical laws, function individually in such comparisons. (RHPS, 19-20, emphases added)

On this account, theories are developed as collections or sets of laws that explain the entirety of the accessible and imaginable world. While empirical laws function individually in comparisons and, in retrospect, may indicate gaps in knowledge, theories cannot be analytically decomposed or used individually in comparisons. Instead, theories are *necessarily displaced* when new empirical laws are introduced. This displacement occurs, Kuhn proposed, because new empirical laws draw into question the ability of existing theories to comprehend the (now expanded) world. As such, they demand reconfiguration of those theories.

Kuhn insisted that theories cannot be reduced to individually functioning and analytically separable components.[100] From the historian's developmental perspective,

> . . . theories are in certain essential respects holistic. So far as he can tell, they have always existed (though not always in forms one would comfortably describe as scientific), and they then always cover the entire range of conceivable natural phenomena (though often without much precision). In these respects they are clearly unlike laws, and there are inevitably corresponding differences in the ways they develop and are evaluated. (RHPS, 20)

Philosophers of science (particularly positivists) thus err in treating scientific theories as empirical laws, assuming that they may function as universal generalizations yet also may be separated analytically. By failing to recognize the differences that exist between scientific theories and empirical laws, philosophers of science thus conflate their characteristics and fail to recognize their unique, respective functions in the development of scientific knowledge.

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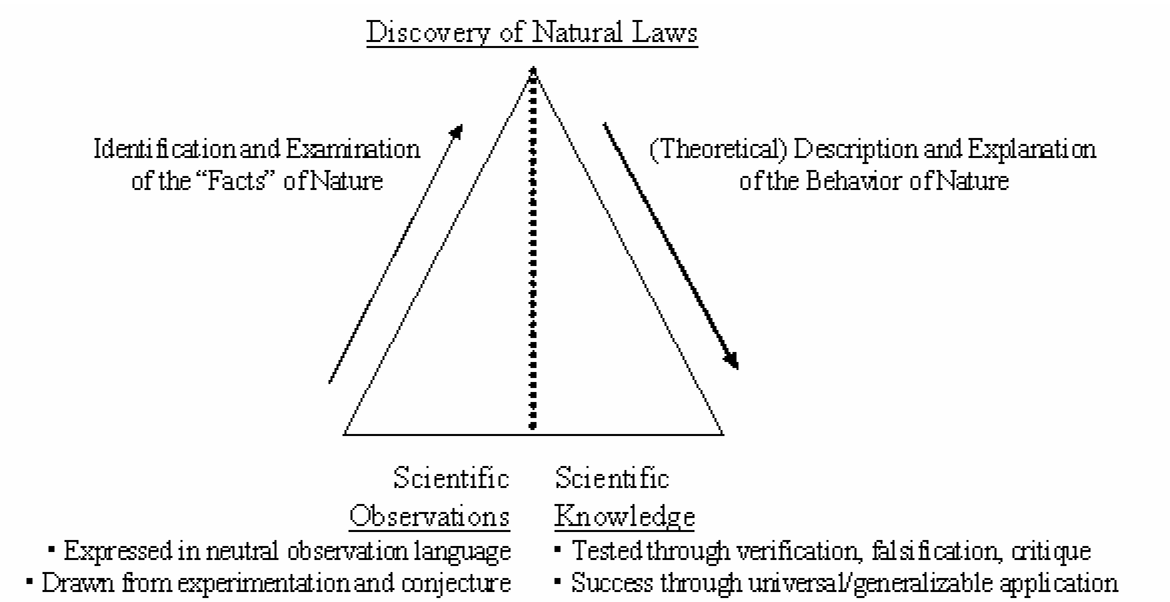
[100] Kuhn did distinguish among various types of theories but implicitly suggested that all types of theories are of this sort. As we will see, this seems to be an excessively general, and potentially misleading, characterization.

Kuhn's description of the development of scientific knowledge (as understood by philosophers of science) can be characterized by a developmental framework that is similar to the one introduced for the development of an historical narrative (**Figure 2**, below). In the philosophy of science, scholars investigate the development from scientific observations to scientific knowledge. In these processes, the "facts" of nature are identified and examined, and theories are developed to describe and to explain the behavior of nature. The discovery of natural laws establishes the first principles that support the transition from "facts" to "theories" and thus provides the authority for the testing and justification of scientific theories. Scientific knowledge thus is the combination of the facts of nature with natural laws and (fully tested) theories to provide "timeless" and "universal" generalizations.

Figure 2: The Development of Scientific Knowledge



Traditional investigations in the philosophy of science typically focus on the activities that are conducted on the right side of the pyramid, relying on the authority of natural laws. The "facts" of nature are identified and examined through measurement, experimentation or conjecture. Discoveries of phenomena or natural laws are viewed as rare achievements of genius, emerging from intuition. Once "discovered," observed facts are expressed in a neutral observation language (or basic vocabulary), so that their expression in the natural laws or theories reflects a direct correspondence between scientific referents and their phenomena. With the identification of natural laws and the development of scientific theories (activities which, Kuhn proposed, are conflated by philosophers of science), the hypothetico-deductive method is applied, whereby laws, theories, or the hypotheses thereof may be tested and critiqued. Through the application of these rigorous and logically based activities, the "knowledge" that results is understood as "scientific" knowledge.

These activities and approaches are both similar to and different from the ones that Kuhn outlined for the historian's development of an historical narrative. For the historian, the identification and examination of "facts" are not at all straightforward, nor are they as individualized as suggested by philosophers of science. The historian does not "discover" individual facts, but "interprets" historical situations. Interpretation conjoins the myriad collection of idiosyncratic facts into a primitive similarity relationship, which is constituted by the juxtaposition of those (previously) disparate facts. Once this primitive relationship has been identified (only loosely is it said to be "discovered" in the same sense as natural laws), it may be expanded to encompass additional facts and developed into a descriptive and explanatory narrative.

The most consequential difference between the development of an historical narrative and philosophers' views of the development of scientific knowledge is the nature and authority of the mechanisms by which identified facts are transformed into explanatory accounts. For the historian, this transformation occurs through the identification of a primitive similarity relationship. For the philosopher of science, it occurs through the discovery of natural laws (and, possibly, scientific theories). Both mechanisms involve the juxtaposition of facts into a descriptive and explanatory account, which encompasses and renders plausible all of the facts that are identified and examined. Yet while the historian's primitive similarity relationship can be extended only to a particular historical situation, the philosophers' natural laws are understood to be universal and generalizable to all situations of that general "type."

It is interesting to consider the source of these differences in application (and, implicitly, in authority). While there are clearly important philosophical differences between the primitive similarity relationship of an historical account and natural laws (namely, the absence of any generalized "law" in an historical account), there are important differences in the nature of the facts that they would describe and explain. The historian's "facts" are considered to be a myriad of idiosyncratic details, whereas the philosopher's "facts" are understood to reflect a direct correspondence to nature. While the historian must interpret, the philosopher or scientist may discover. When the "facts" of an historical or scientific observation are juxtaposed, then, the relationship that is identified (or established) between them thus will possess a different form of authority and extensibility. The primitive relationship of the historian will extend only to the particular interpretation of idiosyncratic "facts," whereas the natural law of the philosopher or scientist will be timeless and universal.

Kuhn's Historiographic Approach

In later reflections, Kuhn noted the accusations of several historians of science that that his approach was not, in fact, typical of the field [See (Mandelbaum 1977) and Reingold (1991)]. In response, he insisted that he was "a rather special narrow sort" of historian, noting particular pride in his ability to "read texts, get inside the heads of the people who wrote them" (Baltas et al. 1995/2000, 276). Kuhn explained further that his unique abilities were adapted from techniques used in psychoanalysis: ". . . I think myself that a lot of what I started doing as a historian, or the level of my ability to do it – 'to climb into other people's heads,' is a phase I used then and now – came out of my experience in psychoanalysis" (Ibid., 280). In particular, he noted the "craft, hands-on aspect" of psychoanalysis, which he described as "intellectually of vast interest" (Ibid.).

In the Preface to *The Essential Tension: Selected Studies in Scientific Tradition and Change* (*ET-SS* 1977), Kuhn outlined his approach for revealing conceptual change through historiographic investigation. Following a discussion of standard historiographic methods (and noting that he had not received formal training in the field), Kuhn explained his particular techniques for "recapturing out-of-date ways of reading out-of-date texts" (*ET-SS*, xiii):

> Trying to transmit such lessons to students, I offer them a maxim: When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning. (*ET-SS*, xii)

On this account, the search for the historical integrity of a text begins by focusing on its apparent absurdities, rather than the passages that seem to make sense initially. Kuhn proposed that this search is accomplished once the absurd passages can be understood. Yet he noted that this process typically entails a change in meaning of those passages that previously had seemed to be both clear and central to the account.

Kuhn characterized this "search for best, or best-accessible readings" as hermeneutic, although he noted that he undertook such readings with a purpose that was different from most historians:

> What I as a physicist had to discover for myself, most historians learn by example in the course of professional training. Consciously or not, they are all practitioners of the hermeneutic method. In my case, however, the discovery of hermeneutics did more than make history seem consequential. Its most immediate and decisive effect was instead on my view of science. (*ET-SS*, xiii)

According to Kuhn, this "hermeneutic" activity reveals the historical integrity of a text by suggesting the changes that are required in order to make the text make sense. Within the context of standard historiographic investigations, this change of meaning represents the rediscovered historical integrity of the passage, which was previously concealed by an anachronistic, "Whiggish" interpretation. It is thus the adoption of an historiographic perspective that renders this change of meaning visible and thereby draws into question established views of science.

In considering Kuhn's description of his method as attempting to account for the absurdities of a text, Ian Hacking noted, "Long before R. G. Collingwood, this was good, solid counsel, but hardly 'Kuhnian' in the sense one might infer from *Structure*. Methodologists of history may find some of these discussions a little naïve" (Hacking 1979, 226). Furthermore, Hacking "deplored" Kuhn's "casual taking up of the word 'hermeneutics'," noting that "[a]ll he means is careful reading" (Ibid.). Providing his own interpretation of Kuhn's approach, Hacking noted that,

> His instructions to his pupils are entirely positivist in character. We are to frame an hypothesis that explains seemingly incoherent passages and test it by seeing if it makes good sense of passages we had previously understood in other ways. Perhaps one will do this best if one has the same kind of mind and quality of reading as might be deployed by a good hermeneuticist, but the point of the exercise and the tests of success are not in general the same. (Ibid)

These comments suggest that Kuhn did not appreciate fully the methodological aspects of his investigation. Ironically, Hacking went so far as to propose that Kuhn's method of reading old texts was positivist, involving the generation then testing of hypotheses. Given that Kuhn had written *Structure* as a rejection of positivist views and had explicitly rejected Popper's conception of testing, this characterization, if accurate, would undercut important aspects of Kuhn's theory.

Based on our historiographic investigation, however, it seems that Hacking's comments misconstrue the most distinctive and important aspects of Kuhn's research.[101] For the processes underlying Kuhn's Aristotle experience were more complicated than the positivist instructions to "frame an hypothesis that explains seemingly incoherent passages and test it by seeing if it makes good sense of passages we had previously understood in other ways." In fact, the most intriguing and important aspect of the experience for Kuhn was the fundamental conceptual reconstruction that his Aristotle experience entailed. This was more than the development and testing of an hypothesis, although each of these activities was involved:

> Suddenly, the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he said, and what his authority had been. (WSR 1980/2000, 16)

What was important to Kuhn, then, was the "new way" in which the various fragments of Aristotle's theory of motion were sorted out. This was not simply a matter of making "good sense of passages we had previously understood in other ways," but required a *simultaneous* reconstruction of the categories of facts or the organization of idiosyncratic details into the elements of a descriptive and explanatory account. It was this change in the "factual" categories of an historigraphic account – quite different from hypothesis generation and testing – that seemed to hold important philosophical implications for the nature of science.

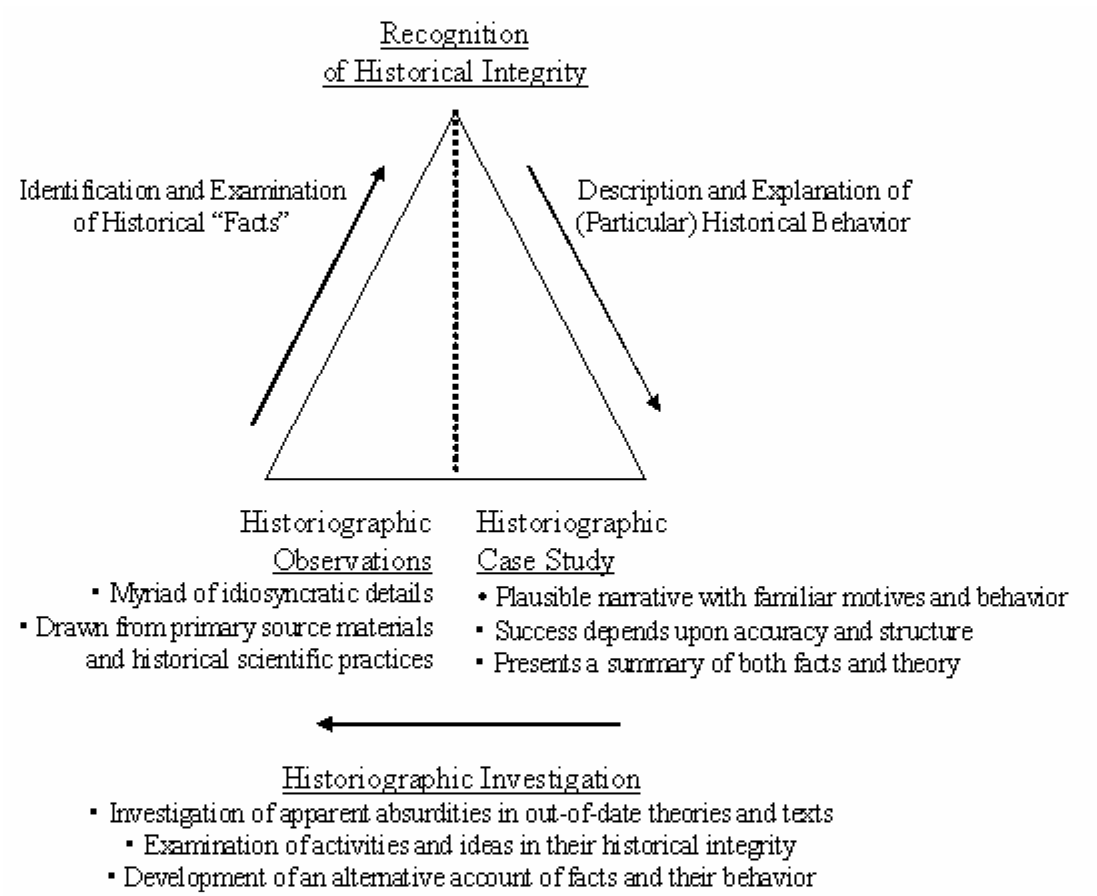
Returning to the developmental framework previously introduced, Kuhn's historiographic approach can be understood as an adaptation of the processes involved in the development of an historical narrative (**Figure 3**). The apparent absurdities of an out-of-date theory or text provide an indication that there is something wrong with account and, possibly, with the primitive similarity relationship by which its historical facts are understood. In attempting to account for those absurdities, the historiographer develops a more appropriate historical narrative based on the "historical integrity" of scientific activity during the period being investigated.

As with the development of historical narratives, this process involves the identification and examination of "facts," the identification of a primitive similarity relationship, and the development of a descriptive and explanatory account. The difference rests in the means by which historical "facts" are

[101] Admittedly, Hacking was not attempting to account for the full scope of Kuhn's activities in these passages. Yet if we are to understand the problems and benefits of Kuhn's attempt to bridge the history and the philosophy of science, then we must understand the distinctive processes and activities of his historical developmental method more clearly.

identified and examined, such that both scientific texts and scientific practices are examined. With the development of an historical narrative that is more appropriate to the ideas and practices of the time, Kuhn proposed that previous absurdities can be explained, yet previously clear, central passages of the text will have changed their meaning.

Figure 3: The Development of an Historiographic Case Study



The historiographic approach thus involves the identification and examination of historical "facts" that differ from those presumed through contemporary views. It reveals the contemporary biases of "Whig" history, that is, history developed on the basis of contemporary views of facts and their primitive relation. The alternative facts identified by historiographic investigations thus constitute an alternative primitive similarity relationship and are described and explained through an alternative account or narrative. Substantial reconstruction thus is required vis-à-vis the initial historical narrative, and this reconstruction involves both fact and theory.

The fundamental nature of this reconstruction holds important implications for traditional philosophical views of science and its development. It suggests that neither the "facts" of science nor the theories that describe and explain them are as independent, as objective, or as directly related to nature as is typically presumed. It suggests that the authority of natural laws does not necessarily lie in their expression of a direct correspondence to "what is really out there." Furthermore, it suggests that by relying on the combination of a neutral observation language or basic vocabulary and the hypothetico-deductive method, philosophers of science may fail to discern the fundamental conceptual change of "facts" that an historiographic approach reveals. The errors or absurdities identified by the philosopher of science may remain just that, but for the historiographer, they will point toward the possibility of a fundamentally different conception of facts of nature.

Kuhn's Historical Developmental Accounts

Three years after his 1968 essay on the relation between the history and the philosophy of science, Kuhn considered "The Relationships Between History and the History of Science" (RHHS 1971/1977). In this essay, he proposed that the new historiography was "potentially a [more] fully historical enterprise" than that of preceding traditions (RHHS, 150). In elaborating on this comment, he called for historians to extend their work beyond the bounds of internalist historiography:

> I say 'potentially' because that model too has limitations. Though it has extended the proper subject matter of the historian of science to the entire context of ideas, it remains internal history in that it pays little or no attention to the institutional or socioeconomic context within which the sciences have developed. (Ibid)

In Kuhn's view, then, the typical, internalist approach of historiography should be combined with considerations of external, contextual influences in order to render its investigations of science more "fully historical." While acknowledging that external factors typically are not as influential in an established or mature science (RHHS, 148-9), he insisted that a broader historical perspective provides investigations with the greatest degree of historical integrity. This broader view is particularly important during the early stages of a science and during times of crisis, that is, during periods in which the internal aspects of science are in question and thus are placed under greater scrutiny.

The integration of internal and external considerations is evident in even the earliest of Kuhn's historiographic investigations. His training in physics provided a strong basis from which to consider the subtle or specialized differences in the scientific activities or underlying conceptual presumptions of different periods. This training provided him with specialized insight into the contradictions inherent in previous historical accounts, such as Boyle's supposed development of the concept of an element.[102] Furthermore, Kuhn's interest in understanding scientific development also served to direct his interest toward the role of external, contextual factors, such as the role of *Naturphilosophie* as a trigger factor in the simultaneous discovery of energy conservation.

In his historical developmental accounts, Kuhn attempted not only to *reveal* the changes that occurred in scientific activities but also to *account* for them and to *explain* the development of those changes over time. These later accounts extended beyond particular scientific activities or achievements to account for generalized "types" of scientific development. It was particularly in virtue of these broader objectives that Kuhn advocated the consideration of external as well as internal influences on scientific activities and ideas. Even more importantly, this wider scope extended the relevant area of Kuhn's investigations to encompass (and, implicitly, to explain) all aspects of the conduct, authority, and progress of science over time.

Returning to our developmental framework, these broader, historical developmental accounts can be characterized in terms of both historical and philosophical approaches (**Figure 4**). Historiographic investigations of "similar" activities or achievements provide the "facts" for historical developmental accounts, and those accounts describe and explain the shared patterns and underlying structure of those generalized "types" of scientific practice. The primitive similarity relationship typical in the development of particular historical narratives thus is broader and more specialized, in that it reflects similarity in the patterns and structures of historiographic cases, rather than in historical facts. These case studies thus serve to constitute the shared patterns and structures that they (later) are said to illustrate.

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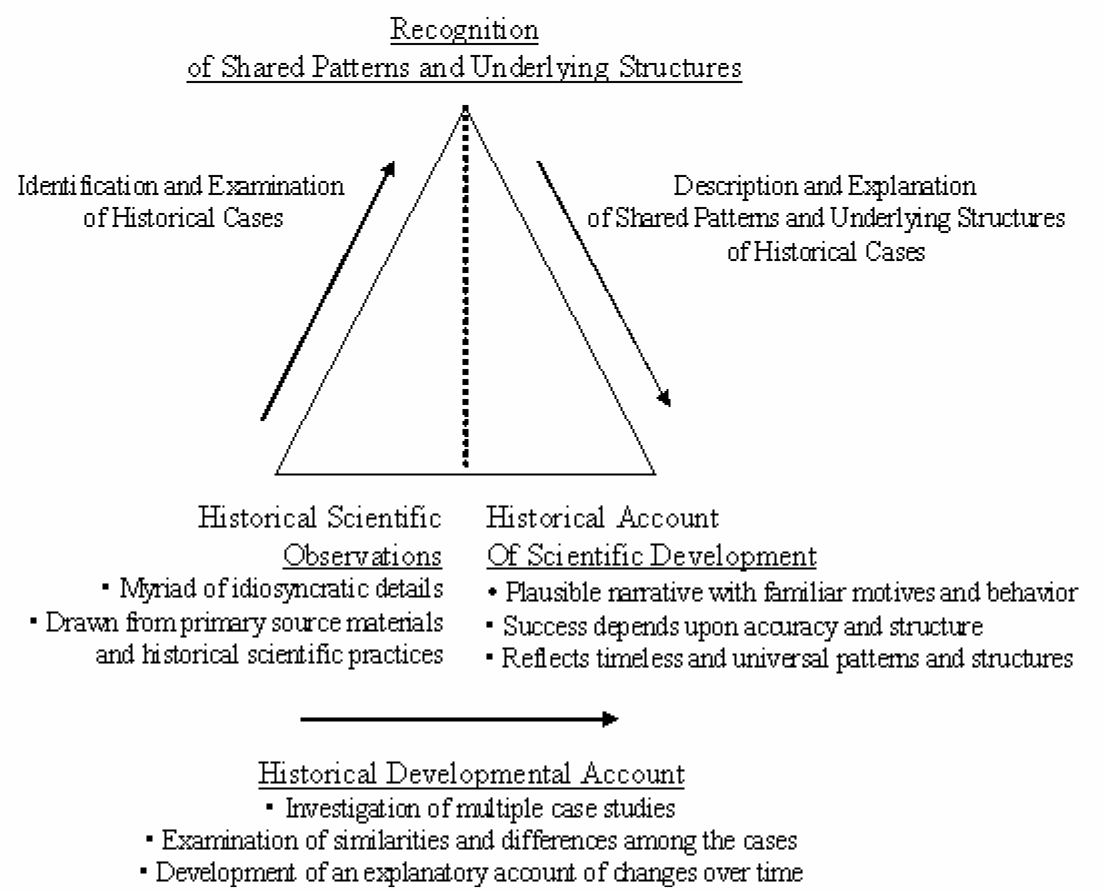
[102] Kuhn later identified the lack of specialized knowledge on the part of historians of science as limiting their ability to discern the finer points of distinction in historical scientific practice and theory (1984).

Figure 4: The Development of an Historical Developmental Account



The resulting narrative or descriptive/explanatory account is not simply "historical" but is understood to be "historical developmental," that is, it is said to account not only for the particular group of case studies under examination but also for more generalized (and timeless) patterns of scientific activity. In this respect, it presents a descriptive and explanatory account of how science *actually is practiced*, as well as an implicit, normative account of how science *should be practiced*.

The Transition from Historiographic Case Studies to Historical Developmental Accounts

To a great extent, our investigation suggests that Kuhn's historical developmental accounts were guided by different objectives, developed through different methods, and based on a different form of authority than his historiographic studies. Yet this is not to suggest that an historical developmental account may be separated from the historiographic studies on which it based. To the contrary, the account is constituted by its exemplary cases, in particular by the specific aspects of those cases that are deemed relevant (or irrelevant) for the account. On the other hand, to the extent that those case studies are conducted for the purpose of developing a broader, developmental account, their selection and examination will be guided by the particular type of "development" that is to be explained. Thus while an historical developmental account may differ from its historiographic basis, these differences serve to distinguish, rather than to separate, their interrelated approaches, findings, and possibly, even their authority.

In understanding and attempting to justify Kuhn's transition from historiographic studies to historical developmental accounts, we must consider the relationship between the case studies to be examined and the type of development to be explained. This relationship is not one of association, deduction, or even induction because it is not a relationship that is established by the movement *from* historiography *to* an historical developmental account. Rather, it is an *interrelation*, in which historical developmental objectives influence historiographic investigations, just as the findings of those investigations inform historical developmental accounts. What we must understand is the nature and the authority of that relation.

From a methodological perspective, the transition is not simply a shift from descriptive to prescriptive approaches, or even from the history to the philosophy of science. Rather, philosophical concerns guide the application of historiographic methods just as historiographic insights inform the philosophical analyses that are conducted and the philosophical implications that are drawn. Thus this is not a *shift* or break from one distinct method to another, completely different one. Rather, it is a developmental transition in methodological emphasis, interest, or, perhaps, of methodological primacy.

The subtleties and complexities of this intimate interrelation aside for the moment, it is important to recognize that some sort of change, transition, or transformation does occur. As discussed above, this is a transition of objectives, methods, relations, and authority. It is a transition that, once completed, is not readily apparent. If the transition is completed successfully, the normative claims will be established as the basis for the account. Only by, once again, undertaking an historiographic perspective will the developmental nature of the account be rendered visible. Yet this reacquired historiographic perspective will, as a consequence, render invisible the underlying influence (and intimate interrelation) of the associated historical developmental interests. Thus it is only in considering the transition itself that the two-sided, intimately interrelated aspects of the historiographic-historical developmental accounts will be apparent.

Implications

The "method" underlying *Structure's* schematic account was much more complex and varied than either Kuhn or his philosophical interpreters recognized. While Kuhn's early research was historiographic, his later publications (including *Structure*) were historical developmental. Even his initial, historiographic work was guided by the puzzle presented by his Aristotle experience and by his interest in the new historiography's philosophical implications. His later work drew from these initial case studies but attempted to draw general, even normative conclusions from the collection of particular, historiographic accounts.

Kuhn's formal training was in physics, rather than history, historiography, or philosophy, and scholars in each of these fields pointed out that Kuhn's approaches did not reflect their own. He provided no justification for his conglomeration of methods, nor, it seems, did he recognize fully the distinctive and varied aspects of his investigative approaches. His investigations seemed to be guided by his interest in exploring the conceptual change exhibited in his Aristotle experience, rather than commitment (or even adherence) to a particular method. Conceptual changes were revealed by emerging methods in the new historiography; however, that approach was not sufficiently "historical" to account for the full range of (internal and external) influences underlying the changes. Even this broader perspective was relevant only to the particular accounts to which it was applied, such that Kuhn had to adjust his methods further in order to develop a general account of scientific development over time. In these respects, it is clear that the investigations that were suggested by Kuhn's underlying interests guided his methods, rather than vice-versa.

How, then, are we to evaluate the concerns of the philosophers of science or the praise of Masterman? How might we justify the wide-ranging nature of Kuhn's investigations and approaches? On the one hand, the philosophers' concerns about the normative implications of *Structure's* schematic account seem to have been well-founded. The basis by which Kuhn's historiographic case studies were collected into an historical account of scientific development was unclear. There was no justification for the account itself, and its central notion of paradigms was vague and ambiguous in important respects.

On the other hand, Kuhn's historiographic investigations highlighted aspects of historical scientific activity that were not revealed by traditional, philosophical approaches. To the extent that the philosophers of science relied upon contemporary views of the "facts" of nature, they risked misinterpretation of the historical theories and texts that had been developed on the basis of (now out-of-date,) alternative "facts." Kuhn's work suggested that this potential misinterpretation was consequential not only for their understanding of historical scientific views and approaches, but also for their understanding of the nature of science. Taken to its logical extreme, it presented challenges to philosophical conceptions of the nature and authority of natural laws and thus, to the cognitive authority of science.

The Nature and Authority of an Historical Developmental Account

In order to evaluate the legacy of *Structure* and its interpretive debates, we thus must understand the nature and authority of an historical developmental account. We must understand the transition from collections of particular, historiographic case studies to a sweeping, descriptive, explanatory, and even normative, general account of shared patterns, underlying structures, and the mechanisms supporting change and development over time. We must understand if and how such a transition can be accomplished and the (philosophical) basis on which it may be justified. In this way, we may gain greater insight into the authority of *Structure's* schematic account. Even further, we may gain valuable insight into the broader and more general question of the relationship between historical (or historiographic) narratives and philosophical (or developmental) accounts.

To the extent that *Structure's* schematic, historical developmental account may be viewed as an historical narrative with an expanded, philosophical scope, its authority would seem to rest (at least in part) on its adherence to the rules for constructing such a narrative. That is, the account must be complete, encompassing the full range of (identified) scientific activities. It must offer a plausible account of the conduct and development of science over time. In this respect, what seems to be at issue is the nature and authority of the "primitive similarity relationship" that is inherent in *Structure's* schematic account.

As developed in the context of an historical narrative, this primitive relationship is constituted by the collection of "facts" (i.e., historiographic cases) examined in *Structure* and provides the basis for the descriptive and explanatory "theories" that it presents. Yet given Kuhn's broad, philosophical ambitions for the work, the nature and authority of this primitive relationship (or of the facts that constitute it) must justify a timeless and universal (quasi-philosophical) account, rather than a temporal and particular (historical) narrative. Furthermore, as a developmental account, the primitive similarity relationship must be established with respect to the underlying patterns and structures of change, rather than (changeable) "facts" or "theories."

If we are to understand either the authority of *Structure's* historical developmental account or the philosophical implications of Kuhn's historiography, we must understand more about both the substantive and the philosophical aspects of this primitive relationship and its changes over time. We must understand the basis for its authority, in particular, its relationship to the authority that is established in the form of the natural laws or first principles that provide the authority for "philosophical" accounts. For these purposes, we turn to concerns regarding Kuhn's conception of normal science (that is, his new image of the nature of science) and the philosophical implications of *Structure's* account.

Section Two

Different Views of Normal Science

In addition to expressing concerns about Kuhn's historiographic method, philosophers of science held a dramatically different view of normal science. They were particularly concerned with Kuhn's proposal that during periods of normal science, a single paradigm predominates and renders all attempts at critique or testing ineffective or unadvisable. Rejecting this monolithic view of science and the "Myth of the Framework" that it seemed to suggest, Popper and his followers continued to insist that science is best characterized by bold conjectures and critique. Lakatos and Feyerabend acknowledged the importance of looking beyond critique or falsification to the dual principles of tenacity and proliferation; however, they insisted that these principles must operate simultaneously, rather than sequentially, as *Structure* seemed to suggest.

Shapere proposed that the concept of paradigms served to determine Kuhn's view of science yet was vague, ambiguous, mysterious and ultimately, misleading (Shapere 1964, 383). As the central concept of *Structure* from which the rest of Kuhn's account seemed to follow, the concept of paradigms thus was an especially important point of analysis for the philosophers of science. Yet the ambiguity of the notion, combined with its effective rejection of critique and testing, served to raise the suspicions and even the ire of the philosophers of science who considered Kuhn's work. While their concerns differed, they were unanimous in rejecting the suggestion that science is "normally" dominated by a single paradigm.

Responses to Philosophers of Science

In his initial contribution to the 1965 colloquium, Kuhn outlined four "differences of locution" separating him from Popper, including testing, critique, puzzle-solving and criteria for theory choice (LDPR 1965/1970b). Contrary to the proposals of Popper, Kuhn proposed that during "normal" periods of science, it is not the theory that is tested but rather, the scientist. During such periods, the predominant activity is not critique but puzzle-solving. Thus what is at issue is the scientists' puzzle-solving ability, rather than the verification or falsification of the theory. When choices arise among alternative approaches to puzzle-solving, Kuhn proposed that standard (philosophical) criteria are insufficient for making the choice.

Kuhn expanded upon these points of difference in his later reflections on the conference, characterizing the exchanges as reflecting "cross-purposes," rather than disagreements (RMC 1969/1970c, 233). He pointed out that both he and Popper acknowledged the role of a framework in the conduct of science. What Popper seemed to miss, Kuhn proposed, was the special detail and precision with which a framework not only guides scientific activity but also highlights the most productive areas for possible critique. By neglecting the function of breakdowns in this framework, Popper and his followers thus missed an important (guiding) influence on the effective conduct of critique. Kuhn characterized this neglect not as error but as a less effective research strategy.[103] By limiting their critique to those areas in which the framework seems to fail (i.e., areas in which obdurate anomalies arise and cannot be explained), Kuhn proposed that investigators could improve the effectiveness of their critical activities.

In "The Nature of a Paradigm" (1970), Margaret Masterman, a scholar in computer science at the Cambridge Language Research Unit, offered an approach to *Structure* that was notably different from the focused critiques of the philosophers of science. She asserted both the existence and the desirability of normal science, basing her assumption on the practices of actual scientific research:

> That there is normal science – and that it is exactly as Kuhn says it is – is the outstanding, the crashingly obvious fact which confronts and hits any philosophers of science who set out, in a practical or technological manner, to do any actual scientific research. It is because Kuhn – at last – has noticed this central fact about all real science (basic research, applied, technological, are all alike here), namely, that it is normally a habit-governed, puzzle-solving activity, not a fundamentally upheaving or falsifying activity (not, in other words, *philosophical* activity), that actual scientists are now, increasingly reading Kuhn instead of Popper. . ." (Ibid., 60)

Affirming the validity of Kuhn's practice-based approach (yet without presenting justification for the view), Masterman insisted that "science as it is actually done – i.e., science roughly as Kuhn describes it – is also science as it ought to be done" (Ibid.). Rather than rejecting the notion of paradigms, she examined it systematically and attempted to delineate both the proper understanding of the concept and its implications.

[103] By "research strategy," Kuhn seemed to be referring to the choices made in the conduct of research. Interestingly, these are choices of action, rather than choices of theory. While not explored by Kuhn, this seems to be an important point of departure from philosophers of science. Specifically, the research activities of the philosophers seemed to be determined by logic and method, rather than by choice (or something other than method). Theory choice would thus be the sole choice, and it would be made on the basis of logic and method. There would, in effect, be no "choices" to be made with respect to the research activities themselves.

Masterman asserted that the critics of the notion of paradigms had failed to understand it fully, thus their focused critiques of *Structure* were based on a crucial misconception:

> For not only is Kuhn's paradigm, in my view, a fundamental idea and a new one in the philosophy of science, and therefore one which deserves examination, but also, although Kuhn's whole general view of the nature of scientific revolutions depends on it, those who attack him have never taken the trouble to find out what it is. Instead, they assume without question either that a paradigm is a 'basic theory' or that it is a 'general metaphysical viewpoint;' whereas I think it is in fact quite easy to show that, in a primary sense, it cannot be either of these. (Ibid., 61)

To resolve the ambiguity of the notion, she conducted a systematic examination of *Structure* and identified 21 different ways in which Kuhn had used the concept (Ibid.). She categorized these into three characteristic groups: metaparadigms; sociological paradigms; and artefact or construct-paradigms.

Masterman noted that philosophers of science had addressed only metaparadigms and concluded that by neglecting the sociological and artefact senses of paradigms, the philosophers had overlooked important elements of Kuhn's central concept. She insisted that the most important of these overlooked elements was concreteness, which, she proposed, is "the paradigm's basic property" (Ibid., 67). While acknowledging the breadth and ambiguity associated with the multiple definitions of the notion, Masterman noted that its function within the theory *Structure* was evident:

> As has been seen, if we ask what a Kuhnian paradigm *is*, Kuhn's habit of multiple definition poses a problem. If we ask, however, what a paradigm *does*, it becomes clear at once (assuming always the existence of normal science) that the construct sense of "paradigm," and not the metaphysical sense or metaparadigm, is the fundamental one. *For only with an artefact can you solve puzzles*. (Ibid., 70)

If we assume, then, that the activities of normal science are directed toward solving puzzles and that normal science both is made possible and is directed by the achievement of a paradigm, then the primary sense of Kuhn's paradigm must be as a concrete artefact or construct that makes puzzle-solving possible. To accomplish these functions, Masterman proposed that the paradigm must be concrete but also must serve as a model for other (concrete) solutions. It thus must serve two distinct functions, operating as a crude analogy. The nature of a crude analogy and how it can be established thus represent the central issues that Kuhn's work must address.

Structure's New Image of the Nature of Science

The various views of normal science can best be evaluated by understanding more clearly *Structure's* proposed image of the nature of science. As indicated by the wide range of interpretations and the various views of the notion of paradigms, the "new" image presented in the work was not entirely clear – or at least, was not explicit in a way that was evident to its many readers. Yet if we consider the question from the perspective of our historiographic investigation and our reconsideration of *Structure*, three interrelated propositions emerge. First, conceptions of both "science" and "nature" are interrelated and are constructed through scientists' active engagement with concrete phenomena. Secondly, these conceptual constructions (necessarily) undergo two types of change: refinements or extensions of existing constructions and revolutionary transformations to a new set. Third, the "revolutions" that occur in scientific development reflect the transition from one set of conceptual constructions to another, incommensurable, yet more specialized set.

Conceptions of Both "Science" and "Nature" are Constructed

Kuhn's Aristotle experience illustrated what he identified in *Structure* as the first implication of the new historiography: questions of science may be answered by any one of a number of incompatible conclusions (*SSR* 1962/1970a, 114). Even more directly, it reflects the "final" implication presented in *Structure*: that revolutions involve a shift in the theory; the problems available for solution; and the standards for that solution. Through the possibility for incompatible conclusions, theories, available problems, and standards for solutions, revolutions thus effect a transformation of the "world" within which science is conducted.

The changes that occurred in the transition from an Aristotelian to a Newtonian view of physics suggested that the nature of science is more complex than the application of a scientific theory (or group of theories) to an independent, objectively observable nature.[104] The Aristotelian and Newtonian views differed in the conceptions of nature, that is, in their conceptions of motion and the underlying

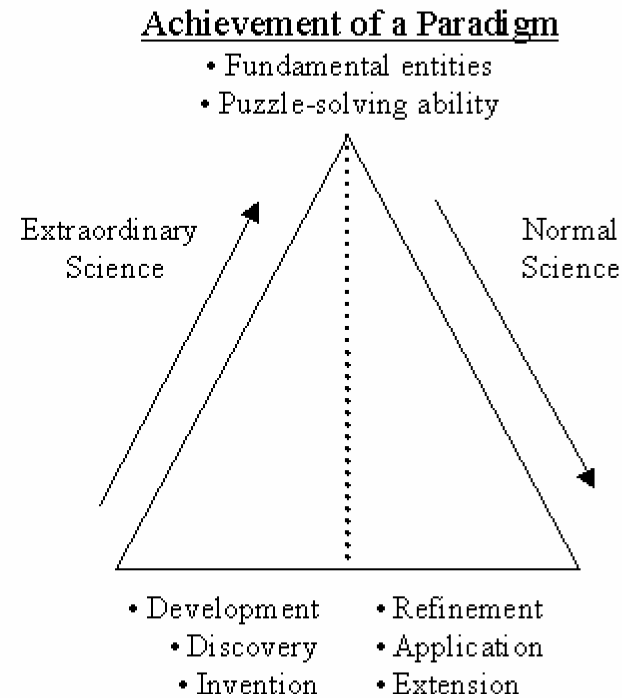
[104] This suggests a problem not only in achieving a neutral observation language but also in establishing any sort of objective or neutral observation of shared, human experience. This does not necessarily preclude communication about shared experience, however, the associated communication can be only partial and need not be associated with a correspondence theory of truth. As we will see, Kuhn's insights led him to question that traditional conception.

metaphysical relationship between qualities and matter. Although each tradition provided a direct linkage between its conceptions of "science" and "nature," a direct or straightforward comparison between the two sets of conceptions did not seem to be possible. Only after acquiring an Aristotelian view of mechanics did Kuhn understand the authority and the integrity of Aristotle's (otherwise "absurd") observations of nature.

Considered independently, conceptual constructions of nature *describe* the underlying phenomena of nature that are to be observed, tested, and measured, whereas constructions of science *prescribe* the techniques, standards and instrumentation whereby scientists come to know their world. Yet a comparison of Aristotelian and Newtonian physics suggests that the constructions are not independent or even separable. To the contrary, they seem to be integrally interrelated, such that the Aristotelian conception of science can be understood as coherent only by adopting the Aristotelian conception of nature. It is this integrated, prescriptive perspective that guides scientific activity.

The developmental framework outlined below provides an illustration of these relationships within the context of Kuhn's theory (**Figure 5**). The achievement of a paradigm reflects both the definition of fundamental entities, which represent basic conceptions of "nature," as well as the identification of puzzle-solutions, that is, basic conceptions of "science." This achievement enables the practice of normal science, which entails refinement, application, and extension of these (now separable) conceptions of science and nature.

Figure 5: Kuhn's Proposed Image of the Nature of Science



According to Kuhn, however, normal science is not the only kind of science, nor are conceptions of science and nature as separable or as stable as they appear in normal scientific activity. Rather, the achievement of a paradigm is preceded and made possible by periods of extraordinary science, in which interrelated conceptions of nature and science are developed, discovered, invented, or defined over time. These activities are characteristic of both pre-paradigm communities and communities in crisis. Subsequent achievement of a paradigm thus may reflect either the establishment of a "mature" scientific community or the occurrence of a scientific revolution.

Like investigations in the philosophy of science, traditional views of science focus primarily on the right-side of this pyramid, relying upon a neutral observation language, basic vocabulary, or "facts" as the basis for application of the hypothetico-deductive method. Kuhn's account introduces the left-side of the pyramid, constructing interrelated conceptions of "nature" and "science" through the developmental activities of extraordinary science. To understand out-of-date texts, such as Aristotle's *Physics*, one must recover historical conceptions of both "science" and "nature" or risk incoherence and even absurdity. This insight represents a key contribution of the new historiography. More importantly for Kuhn, it also highlights the extent to which "nature" can be constructed, thereby drawing into question traditional reliance on an objective, mind-independent nature as the basis for scientific knowledge.

This new image of science denies the sufficiency of the scientific (hypothetico-deductive) method in resolving the questions of science. To the extent that not only science but also views of nature are conceptually constructed, then the aim of science is not to increase the truth content of scientific theories but to increase the fit between its operative constructions of fact and theory. What is at stake, then, is not the correspondence of scientific theory to objective facts but the *coherence* between established theory and (established) facts as they are applied in scientific investigations (i.e., the coherence between conceptions of science and nature). The "data" of scientific investigations thus are neither objective nor independent of the scientific theories that are used to investigate them.

Kuhn sought to explain the nature and authority of this relationship – as well as the way in which it is established – with the notion of paradigms.[105] A mature science is established when scientists agree about the nature of phenomena in their field: the fundamental entities that do and do not exist. Scientific activities can then be conducted within "conceptual boxes," that is, within the now-established constructs of "nature." This makes the possible highly directed activities of normal science. These activities are directed toward the actualization and refinement of the paradigm, that is, improvement of the fit between "science" and "nature."

The puzzle-solving activities that characterize normal science thus can be understood as the application of a particular set of constructions (i.e., a paradigm) to empirical observations that are deemed worthy of scientific investigation. The bulk of normal scientific practice focuses on improving the precision of this fit, by refining scientific theories, expanding the list of known "facts" about "nature," or improving observational and experimental techniques.[106] All of these activities are conducted within the conceptual constructs of "science" and "nature" that are already established and accepted.

This proposal is reflected in the second implication of historiography that Kuhn identified in the Introduction to *Structure*: effective research is linked to answers about fundamental entities; their relations;

[105] Yet as we have seen, this notion remained ambiguous and both the nature and the authority of its inherent relations remained unclear. Given the implications of this proposal for Kuhn's new image of science, we must understand these more clearly in order to determine the appropriate legacy of Kuhn's work.
[106] Yet this is not verifying or falsifying the theory.

characteristics for consideration; and techniques for examination. The goodness of fit between constructions of science and nature thus accounts for *both* the efficiency of normal scientific activities and for the direction in which it proceeds. Yet importantly, these conceptual constructions do not entail – and cannot yield independently – either the invention of new scientific theory or the discovery of new phenomena.[107]

Evolutionary and Revolutionary Change

Perhaps even more important than the proposed interrelation of scientific fact and theory, was Kuhn's proposal that more than one set of these interrelations is possible to answer the questions of science. His Aristotle experience suggested that Aristotelian and Newtonian science operated under different sets of conceptual constructions. The resulting scientific practices were incompatible; however, each possessed an apparent authority that could not be undercut by method alone, because each respective method presupposed its own particular conceptions of the phenomena to which it would be applied.

While Kuhn was able to think like an Aristotelian following his experience, he noted that he did not become one as a result. Thus despite his statements regarding the possibility for a variety of incompatible answers to the questions of science, Kuhn's actions suggest that there remains some (as yet unidentified) means of adjudicating between these competing, incompatible conclusions. This proposal also reflects the second aspect of Kuhn's new image of science, namely, that the operative constructions of science undergo both evolutionary and revolutionary change.

Kuhn proposed that the task of science – to develop knowledge about the natural world – is accomplished most effectively through puzzle-solving. In this respect, he noted that scientists are relatively free to choose the puzzles that they pursue and can focus their efforts on those areas that are most promising. In contrast, investigators in the social sciences often face external pressures to address pressing social, economic or political problems (that may be, as yet, insoluble). On the other hand, in comparison to the "problems" of engineering, which are defined clearly and can be answered readily, scientific "puzzles"

[107] It is important to remember that Kuhn's conceptions of inventions of theory and discoveries of phenomena during periods of extraordinary science (i.e., discovery by anomaly) are narrower than traditional conceptions. Although Kuhn was consistent in his use of this conception, he did not distinguish it from traditional conceptions, nor did he emphasize the importance of his narrower notion.

are often ambiguous in both their form and their resolution. The puzzle-solving capabilities that are available to scientists thus place their activities between the less defined more (socially) "important" investigations of social scientists and the more clearly defined yet (purportedly) value-neutral investigations of engineering. Science thus is relatively unique in the freedom of its practitioners to choose the puzzles that they pursue and in the creativity that is required to outline and to resolve those puzzles.

Kuhn noted that after centuries of scientific activity, some areas of mature scientific investigation have been refined to the point that their puzzles are resolved and their activities are more similar to engineering than to science. Yet he also noted that many areas of scientific investigation still remain in which scientists still seek to improve the "fit" between their expectations and their observations. Even within areas that are relatively well defined or typically successful in their predictions, anomalous or unexpected observations arise consistently and in sufficient numbers that they cannot all be pursued. In most cases, those anomalies that are investigated often require only slight adjustments in scientific theories, categories of phenomena, or methodological or instrumental techniques. Yet occasionally, anomalous observations simply cannot be explained within existing constructs (of either "science" or "nature"). If these circumstances are sufficiently long-lasting and problematic, they may prompt a crisis and encourage investigations that "loosen" the operative constructions. Ultimately, they may lead to a revolution whereby existing constructions of *both* "science" and "nature" are overturned by an alternative set.

The interrelation of conceptual constructions of "science" and "nature" thus is highly complex. Constructions of "nature" inform and serve as the bases for refining constructions of "science." As such, "science" seems to rely upon "nature" for its definition and elucidation. Yet when viewed from a dynamic, developmental perspective, the failure of "science" to account for anomalies ultimately may be deemed a failure of "nature," as it is currently constructed. Such instances thus may prompt the re-construction of the fundamental entities of "nature," and thus of "science," in its turn.

The motivating force for revolutions, then is the occurrence of an anomaly that is unexpected and cannot be explained by existing scientific theory. Although Kuhn described anomalies as variations from expectations, they might more appropriately be described as unexplained (or inexplicable) observations of "nature." For anomalies are not purely theoretical, nor are they "merely" empirical. Rather, they are an empirical indication of an apparent weakness or failure of the operative construction of science (and possibly, that of nature). Anomalies thus suggest a mismatch, between current constructions of "science" and "nature" and the concrete phenomena that they would explain. Most can be resolved with adjustments to the construct of "science" (i.e., theory, instrumentation, etc.). Yet obdurate anomalies may prompt a growing crisis as scientists begin to question their most fundamental conceptions of nature. This reconstruction is prompted by, and in turn, influences, the scientific activities that apply that set of constructs to concrete phenomena. The resolution of an anomaly thus ultimately depends upon the conduct of scientific activity.

These proposals reflect the third implication of the new historiography: that the arbitrary factors that characterize a particular view of the world will ensure the continuation of scientific activity by highlighting the occurrence of anomaly (*SSR* 1962/1970a, 115). Thus while a number of incompatible conclusions to the answers of science are possible (implication #1), the puzzle-solving nature of science and the efficiency and directed nature of scientific activity conducted under a particular set of constructs (i.e., under a particular paradigm, implication #2) combine to limit the range within which scientific activity is conducted. Yet rather than limiting the development of science, this focus seems to enhance development by highlighting those areas in which the operative constructions (which are determined, in part, by arbitrary factors) thus fall short of providing a complete and comprehensive view of nature.

Revolutions Represent the Development *From* One Set of Constructions to Another

From Kuhn's case studies, we can see that revolutions are prompted by the limitations encountered when extending existing scientific theory to more detailed and more specialized observations. They are prompted by anomalies that cannot be explained by existing scientific theory. As knowledge becomes more specialized, expectations become more precise and anomalies occur less frequently (yet are more challenging when they do occur). Thus when a scientist confronts an anomaly that seems worthy of attention, she first will approach it initially with her standard set of methods and constructions.[108] If the anomaly proves obdurate, those scientists who interpret the anomaly as presenting a crisis for their field

[108] To do otherwise is to deny the proven effectiveness of those tools, and to open herself up to a much broader and more random array of possible resolutions. As in the initial choice to adopt the constructions, it always is possible to refuse to apply them.

will adjust their activities, gradually loosening those parts of the constructions (and methods) that seem to stand in the way of resolution and assimilation of the anomaly.[109]

Revolutions thus reflect a transition from one set of constructions (i.e., "science" and "nature") to another set that is incommensurable, yet more specialized. Following a revolution, the new set of constructions will be incommensurable with respect to the old set. This is because in order to explain the anomaly, it must reconstruct not only "science" but also (the fundamental entities of) "nature." If an anomaly can be explained through adjustment of "science" alone (i.e., existing theories or instrumentation), then no revolution will occur: the construction of "science" will be adapted without destabilizing the construction of "nature."

The choice to use a particular set of constructions cannot be the choice for or against a single set but entail the choice between two competing sets. This is because scientists cannot conduct *scientific* activities without operative constructions of both science and nature. Even if a construction is seen as deeply flawed, scientists will continue to use it until an alternative (and generally preferable) set of constructions is developed and selected as a better option. In attempting to account for an anomaly, then, scientists will not abandon their operative set of constructions unless they have another set that they deem more appropriate. They will not simply reject their conceptions of "science" or, more particularly, of "nature" because to do so would be to render either their investigative activities or their very world of existence incoherent.[110]

A new set of constructions that is developed to explain an anomaly will be more specialized than its predecessor, in that it will be able to account for the anomaly. Yet in providing this more specific account, the new set may neglect other, previously specified explanations provided by the old set of constructions. The reconstruction of "nature" will involve the revision of the fundamental entities that do and do not exist, prompting associated changes in the scope of scientific investigations. In this way, a revolution will involve losses as well as gains.

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[109] This loosening can be highly directed and need not reflect an immediate unraveling or total abandonment of constructions and methods. Rather, it can be a gradual testing and reconstitution of the old set of constructions and in some cases, the simultaneous creation of an alternative set. Identity is gained from coherence but may be reconfigured in (analytically) consequential ways over time.
[110] Such a rejection, while possible, would represent the abandonment of "science," that is, the rejection of the very basis of scientific activities: solution of the puzzles presented by nature.

To accept a new set of constructions thus is not to deny the earlier effectiveness of the former set. At the most basic level, it is to acknowledge the importance of explaining the anomaly and to continue the puzzle-solving tradition of scientific activities. More broadly, it is to support a different set of constructions for guiding the range of scientific (puzzle-solving) activities in the future.[111] The perceived importance of explaining an anomaly thus is a matter of judgment, as is the subsequent choice between alternative puzzle-solutions. In this respect, the decision to accept a new set of constructions represents nothing less than the decision of how to practice science.

The "development" of science thus is constituted by (series of) constructions that guide scientific activity. The path(s) of development may change over time, along with the purposes toward which scientific activities are directed. Yet scientific development is decidedly not teleological. Instead, the purposes and paths of scientific development represent the overriding points of concern in the choice to question an established paradigm (i.e., to begin loosening operative constructions of nature) and the choice between competing paradigms. The set of constructions that are chosen, in turn, establishes the prescriptive bases for future scientific activity. Thus the choice between alternative sets of constructions involves a choice between ways of practicing science and an evaluation of the promise of future scientific activity under each paradigm.

The asymmetrical development of scientific activity is a development *from* past achievement, yet it is not a development *toward* (traditional conceptions of) "truth." It is the process of increasing specialization within a defined area through directed (at times, perhaps, fortuitous) scientific activity. The specialization and directedness of scientific activities derives from the holistic, interlocking network of conceptions that are formed by integrated constructions of "science" and "nature." These form a prescriptive collection of techniques, methods, laws, values, and expectations for the *scientific* investigation of nature. Within the realm of scientific activity, then, this prescriptive collection both suggests and makes possible its own notion of ("scientific") progress.

[111] It is important, however, to note that there are some situations in which a revolution does *not* represent the abandonment of the old set of constructions. Thus today, the more easily calculable Newtonian physics is used for more general applications, although it is rejected in more focused cases in favor of the more accurate and precise resolutions provided by Einsteinian physics. This situation suggests that scientists *can* operate in two different "worlds" (i.e., with two different constructions of nature); however, it also suggests that they do so willingly, for specific purposes and within defined parameters.

Implications

Our historiographic investigation suggests that Kuhn's views of normal science emerged from his investigation of scientific revolutions and, explicitly, from his consideration of measurement. As Kuhn broadened his investigations beyond the revolutionary aspects of discovery by anomaly, he came to see that the global change associated with scientific revolutions was counterbalanced by more stable activity during "normal" periods of science. In his 1958 revision of "The Function of Measurement," he developed the concept of normal science to account for the role of measurement in various types of scientific practice. Kuhn proposed that the practice of actual science suggested that measurement played a secondary, rather than a primary, role in scientific discovery (i.e., discovery by anomaly) and proposed that the common view of measurement as guiding scientific activity is, in fact, more typical of the stable periods of science.

Contrary to the suggestions and presumptions of the philosophers of science, the concept of normal science thus was not derived from the notion of paradigms but from his perception of periods of stability occurring on either side of a scientific revolution. Kuhn's view of normal science was not simply the dominance of a single paradigm but the predominance of a particular puzzle-solving tradition, which periodically experienced revolutionary change. As suggested by the changes accompanying scientific revolutions, such "traditions" extended beyond scientific theories to the instrumental, methodological and conceptual elements that supported their puzzle-solving capabilities. They encompassed both descriptive and normative aspects of science, inextricably linked as a puzzle-solution for the field.

Perhaps the most important yet overlooked insight provided by Kuhn's conception of normal science was its linkage with his conception of anomaly, in particular, his identification of discovery by anomaly. Kuhn's early historiographic investigations highlighted this special kind of discovery as prompting the "extraordinary" investigations that occasionally culminate in scientific revolution. In *Structure's* narrative account, Kuhn emphasized the importance of this insight by proposing that the occurrence of anomaly is an inevitable result of the extension and development of a paradigm. Specifically, the "actualization" of the promise of a paradigm through normal scientific activity inevitably leads to the emergence and recognition of anomalies. It does so by extending the paradigm and refining the precision of its expectations to the point that any "failures" in its puzzle-solving abilities are highlighted. While such challenges initially "test" the scientist's puzzle-solving abilities, a sufficient number of failures by respected scientists may prompt the members of a community to declare a "crisis" within their field, that is, it may signal a break-down in their operative, puzzle-solving paradigm.

This account suggests it is the puzzle-solving activities of normal science – augmented by observation of anomaly and targeted application of critique – that provide the most effective impetus for scientific discovery and development. By highlighting the emergence of anomalies, the actualization of a particular puzzle-solving paradigm indicates the areas in which critique may be conducted most effectively. In the absence of such indications, critique could be conducted; however, there would be limited guidance as to the areas in which it might be most effectively employed. By focusing their critique on the anomalies that emerge from scientific puzzles, that is, scientific "problems" that are understood to have a solution, scientists may more readily isolate and investigate problematic areas than if their critique were directed toward more general problems that were not necessarily solvable. This is because the anomaly is not simply something that *cannot* be explained but is something that (given existing tools and theories) *must* be explained. It thus does more than provide an opportunity to improve or to refine those tools and theories – it establishes the need to do so.

The recognition of anomaly thus plays a dual role in scientific development, involving both empirical observations and theoretical/methodological expectations. First, the recognition of anomaly indicates a breakdown in the operative puzzle-solving paradigm by highlighting a break between its expectations and empirical observations. Secondly, it highlights the areas of empirical observation that must be explained in order to reestablish the puzzle-solving capabilities of the scientific community. This dual role is important because it suggests that the exploration and explanation of anomaly will serve to refine the precision and to broaden the scope of scientists' puzzle-solutions over time.

In this respect, the progress that characterizes extraordinary science occurs *by means of* the revolutionary change from one puzzle-solving framework to another. This non-cumulative progress is to be distinguished from the cumulative progress that occurs in the actualization of a paradigm during periods of normal science. Yet the two types of progress are linked just as the two types of science: for it is through the cumulative development or actualization of the paradigm that anomalies arise. By prompting attempts at explanation and assimilation, these anomalies serve as the impetus for the non-cumulative "progression" from the problematic framework to another, more specialized one.

A Reconsideration of Interpretive Debates

In reconsidering the philosophers' concerns about normal science, it seems that they did not appreciate either Kuhn's broad conception of paradigms (as extending beyond scientific theory) or the role of anomaly in directing critique. They did not explicitly acknowledge Kuhn's characterization of normal science as a puzzle-solving activity, yet this neglect was not necessarily a lapse or error in their investigations. Rather, their understanding and interpretation of the theory outlined in *Structure* was fully consistent with standard philosophical approaches and presumptions. Consistent with a philosophical approach, the philosophers of science considered the concepts of *Structure* as they were presented in the work, and inferred the nature and authority of those concepts on the basis of that presentation. They presumed that a paradigm, as the entity that "guides" scientific activity must be a scientific theory. Consistent with their own "historical turn," they viewed *Structure's* case studies as illustrations from the history of science that might be used to test the theories and normative guidelines that were developed through logical, philosophical analysis and critique.

The philosophers' focus on the notion of paradigms as the central concept of the work and their interest in the philosophical implications of the account (as opposed to the account itself) were both consistent with established approaches in philosophy of science. Yet it is clear from our investigation that the notion of paradigms was developed to *explain* the processes that were revealed by Kuhn's historiographic investigations, not to *determine* his interpretation of that history. Furthermore, given its ambiguity in the theory of *Structure*, the notion was not necessarily the most appropriate starting point for either examining the work or its conclusions and implications.

Not surprisingly, philosophers of science focused their investigations on the logic of *Structure's* central concepts rather than their function within the broader developmental processes of science or the historiographic case studies from which they were developed. As a result, they failed to recognize the concrete aspects of Kuhn's conceptions of paradigms and normal science, and the role of (concrete) anomaly in guiding (logical) critique. They also failed to understand the interrelation of his central concepts and the authority of each as they were developed in Kuhn's research.

As a result of their misunderstanding of *Structure's* (admittedly ambiguous) central concepts, philosophers of science misinterpreted its schematic account. In particular, they failed to recognize the distinctive insights revealed by Kuhn's historiographic approach, including the global change that occurred with revolution; the interrelation of descriptive and normative elements in scientific puzzle-solving; the emergence of "new" phenomena; and the existence of two types of science and scientific progress. These distinctive insights emerged from the empirical, activity-based aspects of Kuhn's historiographic approach.

According to Margaret Masterman, Kuhn's strict and detailed focus on the concreteness of historical scientific activities represented a ground-breaking study of the early stages in the development of a "new" science:

> . . . Kuhn does not presuppose anything; not even, initially, his paradigms. He researches into the real history, and broods; he reads scientific teaching textbooks, and wonders. An investigation into the originality of Kuhn, then, is also an investigation into the crude forms and early stages of a science. (Masterman 1970, 68)

On this account, the concreteness of Kuhn's approach reflected the approaches employed in the earliest stages of a science, in which theories initially are developed, evaluated, or chosen. This concreteness freed Kuhn from the presuppositions or biases of speculative philosophical thinking. The resulting insights regarding the conduct of concrete, scientific activities thus rendered his work attractive to science practitioners yet challenging to philosophers of science.

Masterman proposed that Kuhn had taken a crucial step by reintroducing concreteness into philosophical investigations of science. This allowed him to focus initially on what is concrete and then to consider the implications for metaphysical views:

> For instead of asking "How is it that a metaphysical system can be used as a model?" – i.e., instead of asking the question which I said earlier Popperians could not answer [how a new line of research can be established] – Kuhn can now ask: "How is it that a puzzle-solving construct (i.e., a paradigm, sense 3) can be used metaphysically? How, in fact, can a construct-paradigm become a 'way of seeing?'" (Ibid., 73)

The concreteness of Kuhn's approach thus seemed to represent an interesting inversion of philosophers' theoretical focus on generalizations and abstractions. While philosophers of science tended to begin (and often to end) their investigations of science by relying on (established) metaphysical systems, Kuhn's work began with concrete puzzle-solutions and moved from the construct or sociological sense of a paradigm to its associated meta-paradigm. This shift of approach allowed him to examine the earliest stages of a science and led him to ask questions that differed from those typically considered in the philosophy of science.

Masterman proposed that in order for a construct paradigm to become a (metaphysical) “way of seeing” in the context of puzzle-solving, it must be able to be used analogically. Yet she pointed out that such a construct must be more than *just* an analogy:

> . . . for any puzzle which is really a puzzle to be solved by using a paradigm, this paradigm must be a construct, an artefact, a system, a tool; together with the manual of instructions for using it successfully and a method of interpretation of what it does. (Ibid., 70)

In this respect, she proposed, “the real problem, in getting a philosophy of new science, is to describe philosophically the original trick, or device, on which the sociological paradigm (i.e., the set of habits) is founded” (Ibid.). To the extent that Kuhn’s concept of artefact paradigm must have “the inescapable property of crudeness,” she acknowledged that “[t]he heart of the problem is that of envisaging a crude analogy stated in ambiguous words as an artefact; pictures and wire models can be fitted in with comparative ease, after this first central problem has been faced” (Ibid., 81). At the heart of the second required property, that of analogy or extension, lies the question of how a crude paradigm can extend itself by “replication” yet without the introduction of ancillary rules.

While Masterman’s comments present important challenges to the philosophers’ interpretations of *Structure*, they also suggest a number of limitations in the work as it was presented. In this respect, Kuhn must bear a substantial portion of the responsibility for the philosophers’ misinterpretation of his work. His failure to delineate the distinctive aspects of his historiographic approach or to present clearly the concepts and conclusions of his earlier investigations represented important shortcomings. The ambiguity of his concepts and the schematic nature of his presentation allowed philosophers of science to develop interpretations and evaluations that were coherent, comprehensive, and even systematic within the context of their established approaches. Furthermore, the widespread and systematic misinterpretation of his notion of paradigms suggests that the presentation of *Structure* was limited not only by the schematic nature of its account but also by its reliance on concepts and conclusions that were not yet developed fully.

Areas for Further Investigation

With regard to views of normal science and *Structure's* new image of the nature of science, we must address the limitations of interpretations by philosophers of science and the presentation of theory by *Structure*. First, we must recognize the developmental nature of *Structure's* schematic account, including the areas in which its concepts were not fully presented vis-à-vis Kuhn's earlier research and the ambiguity of the notions of paradigms and normal science within the broader, developmental account. Secondly, we must understand the notion of paradigms more clearly, specifically, how a paradigm can provide the basis for a puzzle-solution. Margaret Masterman's suggestion for clarification regarding how a paradigm can become a metaphysical way-of-seeing, that is, how a paradigm can be a concrete construct seems to be particularly promising.

In presenting this view of the interpretive debates and in highlighting these areas for further investigation, we must recognize the broader implications that have emerged vis-à-vis the methods underlying interpretations of *Structure* and of the nature of science, more generally. As discussed previously, traditional philosophical approaches typically view the "facts" of science as reflecting a direct correspondence to nature, subject only to "error" of interpretation or understanding. Most philosophical investigations thus focus on the testing and justification of scientific theories, as applied through the hypothetico-deductive method. Yet Kuhn proposed that due to their reliance on a neutral observation language or basic vocabulary, philosophers of science fail to recognize the fundamental nature of the conceptual changes involved in scientific revolutions. In this respect, their reliance and focus on scientific theories would seem to represent an incomplete, albeit important, approach to investigations of scientific activities. In neglecting potential changes in the "facts" of nature (and their deep relation to scientific theories), traditional approaches to the philosophy of science thus seem to neglect some of the most dynamic and consequential aspects of scientific activity and development.

Similar limitations can be found in the philosophers' approach to the interpretation of *Structure*. They seem to have interpreted that theory according to their traditional philosophical approaches, conducting their investigations from the basis of the seemingly central notion of paradigms and presuming that the dominance of a paradigm reflected the dominance of a theory. In neglecting Kuhn's broader conception of paradigms and the (empirical) role of anomalies in guiding (logical) critique, they neglected the distinctive aspects of Kuhn's approach. Perhaps most importantly, their (philosophically appropriate) presumptions, interpretations, and evaluations of *Structure's* new image of science were not simply incomplete but were systematically different from those evident in Kuhn's earlier research and his subsequent claims of misinterpretation.

It would seem, then, that the limitations inherent in the philosophers' interpretation of *Structure* were closely related to limitations in traditional philosophical approaches. Their "misinterpretation" of *Structure* thus was not only systematic but also suggestive. On the other hand, the fact that this misinterpretation was so extensive (and, to some extent, was even possible) indicated the highly developmental nature of *Structure's* account. Kuhn did not seem to recognize the ambiguities inherent in his work, nor did he seem to appreciate fully the challenges that it presented to traditional philosophical methods. In this respect, Kuhn's work may have represented an important investigation into the "early stages" of a science, but to the extent that it was itself a potentially revolutionary theory, it was still very much in the relatively early stages of its own development. As we will see, this was particularly apparent in its treatment of the change in normal scientific traditions.

Section Three

The Change from One Normal Scientific Tradition to Another

The third area of concern expressed by philosophers of science involved questions about the change from one paradigm (or normal scientific tradition) to another one. While recognizing the "great advances" that had resulted from recognition of "how much more there was to theories that were supposedly overturned and superceded than had been thought," Dudley Shapere proposed that Kuhn had failed to consider the "good reasons" that those theories had been overturned and even had obscured the existence of such reasons (Shapere 1964, 392). Karl Popper was more direct in his assessment, insisting that by denying the fundamental role of logic and criticism, Kuhn had rejected the very demarcation point of science (Popper 1970, 56). Stephen Toulmin proposed that Kuhn had failed to acknowledge the role of selection rules, which provided both the basis for selection of a theory and the point of continuity through revolutionary developments. It seemed to these philosophers of science that Kuhn's new image of the nature of science lacked an objective basis for its cognitive authority and thus rendered science irrational, relativistic, and subject to mob psychology.

Paul Feyerabend provided a different view of these issues. Agreeing with Kuhn that the transition from one tradition to another involves a choice between competing, incommensurable perspectives, he rejected Popper's model of a purely logical approach to truth (Feyerabend 1970, 214-5). With full recognition of the boundaries of rationality and without the hope for objective truth, he proposed that science is more subjective and irrational than Popper and his followers would admit. Yet Feyerabend rejected what he viewed as Kuhn's retreat to relativism, asserting instead the role of aesthetic judgment in choosing between competing theories.

These questions and concerns were consistent with the philosophers' interest in the implications of *Structure*. In particular, they were concerned that the proposed dominance of a paradigm (i.e., a theory) threatened to undermine the philosophical foundations of science. Given their view that the objective of science is to increase the truth content of scientific knowledge of nature, this implied threat to the philosophical foundations of science also threatened the philosophical foundations of knowledge.

Responses to Philosophers of Science

In his 1965 essay comparing his views with those of Popper, Kuhn characterized the change between normal scientific traditions as a choice between alternative ways of solving the puzzles of science (LDPR 1965/1970b). Rejecting Popper's reliance on the possibility of verification or falsification, he insisted that the decision to be made is not the truth or falsity of a particular theory but rather the choice between two competing paradigms. It is a choice between alternative ways of viewing the world, and thus between alternative ways of practicing science. Given the fundamental differences inherent in the alternatives, such comparative choices cannot, Kuhn proposed, be made solely on the basis of either logic or the correspondence of scientific theories to the facts of nature. Rather, they involve choices about scientific theories, the phenomenal world, and how these are interrelated in the community's practice of science. In this way, Kuhn proposed, the choices involve not only an evaluation of the comparative logic of each alternative, but also a range of psychological and sociological factors related to the community and its individual members.

In his later reflections on the conference with Popper, Kuhn expanded upon these earlier views and responded directly to charges of relativism, irrationality, and mob rule:

> To say that, in matters of theory choice, the force of logic and observation cannot in principle be compelling is neither to discard logic and observation nor to suggest that there are not good reasons for favoring one theory over another. To say that trained scientists are, in such matters, the highest court of appeal is neither to defend mob rule nor to suggest that scientists could have decided to accept any theory at all. In this area, too, my critics and I differ, but our points of difference have yet to be seen for what they are. (RMC 1969/1970c, 234)

Kuhn insisted that the philosophical insight suggested by *Structure* was not that science is irrational but that existing theories of science and rationality are problematic when viewed from the perspective of historical scientific activity. If there are "good reasons" for choice that extend beyond the limitations of logic, then logic and critique cannot be either the demarcation point of science or the sole reason for its progress. This is not to suggest that logic and criticism do not play an important role in scientific activity, paradigm choice or the progress of science. Rather, it implies that they may be *augmented* by other reasons and considerations that they do not address themselves.

The "good reasons" that Kuhn proposed included accuracy, consistency, scope, simplicity, and fruitfulness – reasons, he noted, that were the same as those suggested by philosophers. Yet he emphasized that the various members of a scientific community may employ those reasons in different ways, or may select different reasons from among the possible alternatives. Such differences are not solely logical but are influenced by differences in experience and values. Kuhn insisted, however, that this variability is not debilitating but is, in fact, "essential to scientific advance" (RMC, 262).

Rejecting accusations of relativism, Kuhn proposed that scientific development is "unidirectional and irreversible" and that "[o]ne scientific theory is not as good as another for doing what scientists normally do" (RMC, 264). Yet considering what he characterized as a subsequent step typically taken by philosophers of science, Kuhn denied the assertion that scientific theories are "true" representations of nature or even statements about "what is really out there" (RMC, 265). Insisting that neither a neutral observation language nor a basic vocabulary is possible, he proposed, instead, that knowledge of nature is "embodied . . . in the mechanism, whatever it may be, which is used to attach terms to nature" (RMC, 270). It is this mechanism, then, that accounts for the way in which science may, in fact, "progress."

Kuhn's description of an earlier exchange with Mary Hesse provides additional insight into these responses. In a 1995 interview, he reflected on an encounter with Hesse that occurred soon after the publication of *Structure* and that presented him with what would come to be an important, if unexpected, challenge:

> When I next saw her, we were in England, and I remember walking with her and going into the Whipple Museum – it's another one of those imprinted images. She turned to me and she said, "Tom, the one problem is now you've got to say in what sense science is empirical" – or what difference observation makes. And I practically fell over; of course she was right but I wasn't seeing it that way. (Baltas et al. 1995/2000., 286)

An earlier account of this exchange is provided in "Reflections on My Critics," which presented the responses cited above. After emphasizing that, "no part of the argument here or in my book implies that scientists may choose any theory they like so long as they agree in their choice and thereafter enforce it" (RMC 1969/1970c, 263), Kuhn added the following footnote:

> Some sense of my surprise and chagrin over this and related ways of reading my book may be generated by the following anecdote. During a meeting I was talking to a usually far-distant friend and colleague whom I knew, from a published review, to be enthusiastic about my book. She turned to me and said, "Well, Tom, it seems to me that your biggest problem now is showing in what sense science can be empirical." My jaw dropped and still sags slightly. I have total visual recall of that scene and of no other since de Gaulle's entry into Paris in 1944. (RMC, 263n)

These two accounts suggest an interesting evolution in Kuhn's understanding both of his work and of the responses to it. The 1969 account suggests that he was shocked and dismayed at Hesse's suggestion that he had not accounted for the empirical aspects of science. Yet in 1995, he indicated that Hesse had, indeed, been correct in her assessment, although he "wasn't seeing it that way" at the time (Baltas et al. 1995/2000, 286).

The question, "In what sense science can be empirical?", is closely related to Hesse's characterization of paradigm debates as "partly dependent on impressionistic and non-logical factors" (a partial dependence that, she proposed, would likely prove problematic to philosophers of science). To an important extent, what is at issue in both of these cases – and in subsequent debates about the interpretation of *Structure* – is the basis for decisions of paradigm choice. In this exchange, then, we have some indication of an initial disparity between Kuhn's views of the processes of paradigm choice and the theory presented in *Structure*. In his reaction to Hesse's comments, Kuhn seemed to suggest that paradigm choice is related closely to the empirical aspects of science. While initially shocked that Hesse had overlooked this vital point, he later acknowledged that her assessment had been correct.

Similar points were made by Margaret Masterman in her contribution to the 1965 colloquium. She identified as a "vital difficulty" the fact that Kuhn's image of science did not acknowledge the "conception of verification in experience" (Masterman 1970, 61). In this respect, she acknowledged that Kuhn's work was vulnerable to the accusations made by the "philosophical empiricist world" (Ibid.). While Kuhn explicitly rejected the possibility of "verification" in a traditional philosophical sense, Masterman's characterization of a paradigm (i.e., as a concrete construct that establishes a metaphysical way of seeing) suggests a way in which experience or the empirical aspects of science may operate in situations of paradigm choice. What we still must understand, however, is the cognitive authority of such choices. What we still must understand is the cognitive authority of *Structure's* new image of science.

The Cognitive Authority of Science

Kuhn's Aristotle experience and his subsequent historiographic research suggested that in its actual practice, science is not the monolithic, cumulative enterprise that positivist philosophers had presumed. Nor can scientific fact and theory be separated categorically for purposes of falsification, as seemed to be suggested by Popper and his followers. Kuhn proposed instead that science is a puzzle-solving activity, in which conceptions of both fact and theory are developed and fit together, as models of puzzle-solutions that can be refined and extended to new situations. Although fact and theory may be separated within the context of a given puzzle-solution, the global changes that occur with scientific revolutions suggest that multiple puzzle-solutions are possible. These global changes reflect changes in both scientific theory and scientific fact, suggesting that the development of science is much more complex and that revolutions (or achievements) are much more dramatic than suggested by the traditional, cumulative view.

While Kuhn did not explore the philosophical implications of his proposal that science is a puzzle-solving activity (i.e., he did not examine the "authority" of scientific puzzle-solving), the proposal is clearly relevant to the charges of irrationality, relativism, and mob psychology that were lodged against him. A view of science as a puzzle-solving activity does not necessarily suggest that there are an unlimited number of alternative puzzle-solutions, nor that there is no basis for evaluating one puzzle-solution relative to another. To the contrary, there are rules both for evaluation of an individual puzzle-solution and for comparison of alternatives. As such, it is not necessarily either "irrational" or "relativistic." Moreover, these rules must be applied by the community of scientists who are trained to solve a particular range of puzzles. In this respect, the choice between puzzle-solutions is not simply a matter of "mob psychology" but involves a range of commitments held by the scientific community as a "scientific" community.

In considering these accusations in more detail, we turn first to the accusation of irrationality. If empirical observations or scientific activities can exert an influence that extends beyond the limitations of scientific theory, then the change from one normal scientific tradition to another is not necessarily irrational. In considering this proposal, however, it is important to understand clearly what is meant by the terms "irrational" and "rational." Although Kuhn rejected accusations that he presented a view of science that was irrational, he also noted, "[i]f they had said 'arational,' I would not have minded at all" (Horgan 1991, 49). In considering Paul Feyerabend's praise of *Structure's* irrational view of science, he responded:

> Obviously there is much in the last part of Feyerabend's paper with which I agree, but to describe the argument as a defence of irrationality in science seems to me not only absurd but vaguely obscene. I would describe it, together with my own, as an attempt to show that existing theories of rationality are not quite right and that we must readjust or change them to explain why science works as it does. (RMC, 264)

While Kuhn rejected Feyerabend's proposed views of science and the philosophical implications of *Structure*, it is telling that the two scholars shared an emphasis on science as a human activity; a recognition of periods of normal and extraordinary science; and acknowledgment of the incommensurability between the perspectives on either side of a revolution. For both Kuhn and Feyerabend, traditional conceptions of rationality faltered during periods of extraordinary science. The two scholars diverged, however, in their views of how this (periodic) "failure" should be characterized and understood.

Kuhn's proposal that we must readjust theories of rationality to explain the operation of science is similar to Feyerabend's suggestion that "the question raised by Kuhn is not whether *there are* limits to our reason; the question is *where* those limits are *situated* (Feyerabend 1970, 218). Kuhn proposed that existing theories of rationality must be expanded in order to explain the way in which science remains "rational" outside the limits of established theories. Yet Feyerabend insisted that such activity is, by definition, "irrational," although not necessarily relativistic. In their occurrence, operation and effect, Kuhn's value-based reasons for theory choice thus seem to be similar to Feyerabend's aesthetic judgments and judgments of taste. What remains to be understood, then, is how Kuhn's "good reasons" may be "rational" when applied on the basis of something other than logic, and what is the (broader) sense in which the term "rational" may be applied.

With regard to charges of relativism, Kuhn proposed that the change from one tradition to another involves a choice between competing paradigms, rather than the verification or falsification of one paradigm or another. It is thus a choice involving determinations about the scope, precision, and puzzle-solving capabilities of each of the two competing paradigms, as well as judgments about the relative merits and importance of each of these considerations. This is a relative choice in that it relates or compares the capabilities of one paradigm to another, selecting the one that is to be preferred based upon the "good reasons" for theory choice. It is important to note, however, that this type of choice is quite different from the theory choice typically considered in philosophy of science. Even if the "choice" between two theories is made on the basis of "good reasons," the chosen theory will not thereby possess the cognitive authority typically attributed to scientific theories (or, more strongly, to natural laws). The relative strength of one theory with respect to another does not provide an objective basis for its cognitive authority – only for its relative preference within that particular situation. In this respect, then, philosophical charges of relativism must stand.

Finally, to the extent that the choice between competing paradigms is made by individuals whose training renders them part of the scientific community, it is not necessarily vulnerable to mob psychology. Insisting that the role of sociological factors in theory choice does not inhibit the possibility for scientific progress, Kuhn explained:

> Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time. In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a *group* of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, *let them make the choice*. There can be no set of rules of choice adequate to dictate desired *individual* behavior in the concrete cases that scientists will meet in the course of their careers. Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. (RMC, 237-8)

In these statements, Kuhn emphasized the scientific community's dedication to puzzle-solving and to increasing the range of data that it can treat with precision and accuracy. In addition, he noted that differences among individual members emerge in their consideration and evaluation of relative puzzle-solving capabilities and that these differences are crucial for ensuring the ongoing progression of science. While these assertions are consistent with his research and with his theory, they do not address adequately the concerns of philosophers of science. If there are no rules to dictate individual choice, how can the members of a community *avoid* mob psychology, particularly if they are imbued with the value system and ideology that is current within their system? To answer these questions and the charges of philosophers, Kuhn must put forward a more compelling account.

Implications

Structure raised but did not address crucial questions about the cognitive authority of science and by association, presumptions that were taken as central to the philosophy of science and even the nature of knowledge. Philosophers of science did not seem to grasp the full extent of the challenges presented by the new historiography, that is, the fundamental change associated with scientific revolutions. By misconstruing the notion of paradigms and neglecting the critical insights provided by anomalies, they overlooked the insights provided by *Structure's* broader account of actual scientific practice. While their approach and misunderstandings were not surprising given their focus and commitments, they were, nonetheless, limiting with respect to the philosophers' efforts to interpret and evaluate *Structure*.

While Kuhn rejected accusations of irrationality, relativism, and mob psychology, his responses did not refute the challenges fully. Although Kuhn's work drew into question traditional conceptions of rationality as associated with a correspondence theory of truth, it did not provide a clear basis for that dramatic conclusion, nor did it offer an alternative basis for the cognitive authority of science. To a great extent, it seems that Kuhn did not understand the full implications of his work and the extent to which it undercut the very basis for philosophical authority. While he noted that his work drew into question traditional philosophical dichotomies and presented challenges to the nature of knowledge, his failure to account for those challenges or to explore them in detail renders those assertions rather reckless. As with the philosophers of science, Kuhn's limited (philosophical) understanding was understandable given his (historical developmental) focus and commitments; however, it served to undercut the account presented in *Structure* and to prompt a series of philosophical concerns.

To address the limitations of both the philosophers of science and the theory presented in *Structure*, we must understand more clearly the basis for the cognitive authority of *Structure's* new image of science. To the extent that we understand science as involving a change from one normal scientific "tradition" to another, we must understand the philosophical basis for that change. To the extent that there are "good reasons" for the decisions made by the members of a specialist community, we must understand the way in which those decisions avoid potential pitfalls of irrationality, relativism, or mob psychology. If *Structure's* account, more clearly understood, relies upon a broader notion of paradigms as concrete constructs or puzzle-solutions, then we must understand the way in which paradigms are established and overturned. That is, we must understand the basis for their authority and for the selection of one, rather than another. Finally, to the extent that *Structure's* image of science suggests a new conception of rationality, then we must articulate both its nature and its authority. In short, we must further develop the ideas and theories presented in *Structure*. In doing so, we turn first to Kuhn's attempts at clarification and elucidation of the work.

Chapter Four

Clarifications and Elucidations

Throughout the late 1960s, Kuhn attempted to clarify *Structure's* new image of science and to explore its philosophical implications for the nature of knowledge. In "Reflections on My Critics," he highlighted "what I take to be the root of my single most fundamental difference from Sir Karl:"

> He and his followers share with traditional philosophers of science the assumption that the problem of theory-choice can be resolved by techniques which are semantically neutral. . . . For Sir Karl and his school, no less than for Carnap and Reichenbach, canons of rationality thus derive exclusively from those of logical and linguistic syntax. Paul Feyerabend provides the exception which proves that rule. Denying the existence of a vocabulary adequate to neutral observation reports, he at once concludes to the intrinsic irrationality of theory-choice. (RMC 1969/1970c, 234)

Insisting that one might "still hope to preserve good reasons for choosing between" two theories, Kuhn sought to replace the traditional reliance on logical and linguistic syntax with attention to the language-nature link:

> . . . philosophers of science will need to follow other contemporary philosophers in examining to a previously unprecedented depth, *the manner in which language fits the world*, asking how terms attach to nature, how those attachments are learned, and how they are transmitted from one generation to another by members of a language community. (RMC 235, emphasis added)

On this account, the rationality and authority of science do not lie in the logical structure of science and reliance on a neutral observation language, but in the way that language "attaches" to nature. This attachment can be discerned, Kuhn proposed, by examining the processes that establish, support and extend it, namely, "*how* terms attach to nature, *how* those attachments are learned, and *how* they are transmitted from one generation to another."[112]

In considering what could provide "good reasons" for theory choice in the absence of logical or linguistic syntax, Kuhn noted that "paradigms, in one of the two separable senses of the term, are fundamental to my own attempts to answer questions of that sort" (Ibid.). He agreed with critics such as Margaret Masterman that "the term 'paradigm' points to the central philosophical aspect of my book but

[112] Interestingly, Kuhn's proposed investigation of the processes that indicate *how* language attaches to nature is similar to internal historiography's examination of the activities that support particular scientific achievements. In fact, we will see that Kuhn's alternative conception of rationality emerges directly from his historiographic approach and from his objective, outlined in *Structure*, to investigate the implications of the new historiography.

that its treatment there is badly confused" (RMC, 234). Even further, he stated that "[n]o aspect of my viewpoint has evolved more since the book was written" (Ibid.).

In three essays written during the late 1960s,[113] Kuhn attempted to resolve the ambiguity associated with his notion of paradigms. In "Second Thoughts on Paradigms," the first written and last presented of the essays, he stated that were he to re-write *Structure*, he would change its organization and begin by highlighting the community structure of science:

> In the book the term "paradigm" enters in close proximity, both physical and logical, to the phrase "scientific community" (pp. 10-11). A paradigm is what the members of a scientific community, and they alone, share. Conversely, it is their possession of a common paradigm that constitutes a scientific community of a group of otherwise disparate men. As empirical generalizations, both those statements can be defended. But in the book they function at least partly as definitions, and the result is circularity with at least a few vicious consequences. If the term "paradigm" is to be successfully explicated, scientific communities must first be recognized as having an independent existence. (ST 1974/1977, 294-5)

In the 1969 Postscript to *Structure*, Kuhn proposed that, "[s]cientific communities can and should be isolated without prior recourse to paradigms; the latter can then be discovered by scrutinizing the behavior of a given community's members" (*SSR*-PS 1969/1970a, 176). He emphasized that this "analytic separation" focuses investigations of scientific development, first and foremost, on the activities of a scientific community.

Thus while the establishment of a paradigm may provide the demarcation point of science, it is the scientific community that is responsible for the *processes* by which that paradigm is established, developed, and overturned. In considering both the achievement of a paradigm and later questions of (paradigm) choice, one must thus look to the activities and structure of the scientific community.[114]

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[113] The three essays were similar in content yet differed in emphasis. The first written (and last presented) was "Second Thoughts on Paradigms" (ST 1974/1977, 293-319), which provided detailed discussion, clarification, and elaboration of Kuhn's notion of paradigms. In February 1969, Kuhn authored a "Postscript" to the second edition of *Structure* (*SSR*-PS 1969/1970a, 174-210), offering both commentary and clarifications regarding the early reception of the work. In the same year, he completed "Reflections on My Critics" (RMC 1969/1970c, 231-78), a response to the discussion at the 1965 conference with Sir Karl Popper and other philosophers of science. This last of the three essays reflected more refined views than the other essays and dealt explicitly with Kuhn's points of difference from critics in the philosophy of science.

[114] Based on the historiographic investigation presented in Chapter One, Kuhn's clarification remains true to the processes by which he developed the concept of paradigms. That is, he initially developed the concept to explain the tacit consensus that seemed to characterize the activities of members within a scientific community during periods of normal science:

> At that time I conceived normal science as the result of a consensus among the members of a scientific community. Difficulties arose, however, when I tried to specify that consensus by enumerating the elements about which the members of a given community supposedly agreed. In

More specifically, Kuhn emphasized that *specialist* communities function as "the producers and validators of scientific knowledge" (*SSR*-PS, 178). According to Kuhn, specialist communities focus their research within a particular area of science (e.g., the phage group in physics). As such, they are the recognized experts who are responsible for conducting research and training new scientists in that narrow area. Yet members of specialist communities also (implicitly) belong to the community of a particular field as well as the broader community of scientists. Their work and interests thus overlap those of scientists in other communities, and Kuhn noted that individuals even may belong to more than one specialist group. In considering the role of specialist communities in scientific development, there are thus interesting and important interrelationships among individuals and communities.

> order to account for the way they did research and, especially, for the unanimity with which they ordinarily evaluated the research done by others, I had to attribute to them agreement about the defining characteristics of such quasi-theoretical terms as 'force' and 'mass,' or 'mixture' and 'compound.' But experience, both as a scientist and as an historian, suggested that such definitions were seldom taught and that occasional attempts to produce them had often evoked pronounced disagreement. Apparently, the consensus I had been seeking did not exist, but I could find no way to write the chapter on normal science without it. (*ET-SS* 1977, xviii-xix)

The notion of paradigms was thus developed to explain how the members of a (mature) scientific community could agree about "the defining characteristics of such quasi-theoretical terms as 'force' and 'mass,' or 'mixture' and compound.'" It was developed to account for the behavior of a (pre-existing) scientific community.

Kuhn's clarification is also true to his historiographic method, in that it emphasizes the investigation of scientific *activities* rather than scientific theory or subject matter:

> Both normal science and revolutions are . . . *community-based activities*. To discover and analyze them, one must first unravel the changing community structure of the sciences over time. A paradigm governs, in the first instance, not a subject matter but rather a group of practitioners. Any study of paradigm-directed or of paradigm-shattering research must begin by locating the responsible group or groups. (*SSR*-PS 1969/1970a, 179-80, emphasis added)

To understand the activities associated with normal science or revolutions, Kuhn proposed, we must understand the "changing community structure of the sciences over time." It is this community structure – rather than the community's guiding paradigm – that provides the appropriate starting point for investigating the development of scientific knowledge.

Section One

The Community Structure of Science

In the Postscript to *Structure*, Kuhn outlined an "intuitive notion of community," which underlay much of the work and which, he noted, was "now widely shared by scientists, sociologists, and a number of historians of science" (*SSR*-PS 1969/1970a, 176). In particular, he emphasized that a scientific community is comprised of the practitioners of a scientific specialty who share similar educational and professional perspectives:

> To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. Usually the boundaries of that standard literature mark the limits of a scientific subject matter, and each community ordinarily has a subject matter of its own. There are schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints. But they are far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended. As a result, *the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgment relatively unanimous.* Because the attention of different scientific communities is, on the other hand, focused on different matters, professional communication across group lines is sometimes arduous, often results in misunderstanding, and may, if pursued, evoke significant and previously unexpected disagreement. (*SSR*-PS, 177, emphasis added)

A scientific community, then, is a group of specialists who share similar educational training; approach their field in generally the same way; and collectively possess responsibility for the pursuit of shared goals, including training of their successors. According to Kuhn, the members of such a community enjoy "relatively full" communication and "relatively unanimous" professional judgment, whereas members of different communities may experience misunderstandings or even disagreements.[115]

Kuhn noted that these general attributes of a scientific community occur at multiple levels, including the communities of all natural scientists; professional groups (physicists, chemists, etc.); sub-groups within the various professional areas (organic chemists, solid-state and high-energy physicists, etc.); and finally, specialized divisions within those sub-groups. The first three types of community – those of scientists, professionals, and sub-groups – are readily identifiable (Ibid.). Traditionally, they have been the

[115] Kuhn struggled for a number of years to identify the shared characteristics that constitute a "scientific community" and that distinguish one community from another. As discussed in our historiographic investigation, he initially attributed it to community consensus and then to agreement upon a paradigm. In his post-*Structure* work, Kuhn continued to refine the notion in ways that will be addressed subsequently.

focus of investigations by a number of intellectual historians who have identified them on the basis of their subject matter, e.g., the profession of "physicists" or the sub-group of "nuclear physicists."

Kuhn stated that the fourth, most highly specialized community recently had emerged as the focus of investigations by sociologists and historians of science.[116] Although such studies were just beginning, Kuhn noted that such groups were found to be small, consisting of "a hundred members, sometimes significantly fewer" (*SSR*-PS, 178). They tended to have overlapping memberships, in that, "individual scientists, particularly the ablest, will belong to several such groups either simultaneously or in succession" (Ibid.). By delineating specialized groups as the most narrowly defined of the "multiple levels" of community, Kuhn implied a structure of interrelation and association among the various types of community. As we will see, his emphasis on specialist communities and his reference to their implicit relation to "higher level" communities are important, albeit under-emphasized, elements of his theory.

Kuhn acknowledged that identifying the members and defining the boundaries of these narrowly defined communities had been found to present special difficulties. Given that such communities cannot be distinguished by their subject matter nor by their membership, he proposed that scholars investigate the activities and relationships that define them *as* a specialized group. This suggested, in turn, that investigators should "cease to rely exclusively on the techniques of the intellectual historian and use those of the social and cultural historian as well" (RMC 1969/1970c, 252). In particular, Kuhn directed scholars toward the examination of attendance at special conferences; distribution of draft manuscripts prior to publication; and "above all . . . formal and informal communication networks including those discovered in correspondence and in the linkages among citations" (*SSR*-PS, 178).

By investigating the activities and interactions that distinguish a group of specialists, Kuhn proposed that one can isolate the particular theories and tools on which members "must necessarily agree:"

> Theories of matter were not, at least until about 1920, the special province or the subject matter for any scientific community. Instead, they were tools for a large number of specialists' groups. Members of different communities sometimes chose different tools and criticized the choice made by others. Even more important, a theory of matter is not the sort of topic on which the members of even a single community must necessarily agree. The need for agreement depends on what it is the community does. Chemistry in the first half of the nineteenth century provides a case in point. Though several of the community's fundamental tools – constant proportion, multiple proportion, and combining weights – had become common property as a result of Dalton's atomic theory, it

[116] Among the most insightful early studies of community structure, Kuhn cited (Hagstrom 1965), (Price and Beaver 1966) and (Crane 1969). For other citations, see (*SSR*-PS 1969/1970a , 176n).

> was quite possible for chemists, after the event, to base their work on these tools and to disagree, sometimes vehemently, about the existence of atoms. (*SSR*-PS, 180)

The connection between a specialist community and its area of investigation is thus the theory and tools on which members of the community "must necessarily agree," which will "depend on what it is the community does." Kuhn emphasized that these necessarily *shared commitments* represent a subset of all possible theories and tools employed by members of the community in their research activities.[117] Although in some cases, different members may choose different tools or criticize the choices made by others, Kuhn proposed that for each community there is a set of shared commitments that are "necessary" for membership.[118]

Kuhn proposed that his "clearer delineation of community structure" revealed revolutions to be "a special sort of change involving a certain sort of reconstruction of group commitments" (*SSR*-PS, 181). In this respect, he located the distinction between normal science and revolution in the commitments of the specialist community:[119]

> It is . . . with respect to [specialist] groups . . . that the question 'normal or revolutionary?' should be asked. Many episodes will then be revolutionary for no communities, many others for only a single small group, still others for several communities together, and a few for all of science. Posed in that way, the question will, I believe, have answers as precise as the distinction requires. (RMC, 253)

On this revised account, a revolution may affect the shared commitments of a particular specialist group or it may extend to "all of science." Yet Kuhn emphasized that even small-scale revolutions are important: "[i]t is just because this type of change, little recognized or discussed in the literature of the philosophy of

[117] As we will see, the shared commitments that distinguish a specialized community represent the more generalized usage of Kuhn's notion of "paradigm."

[118] In the clearly important yet ambiguous passage above, Kuhn linked the shared commitments of a community with its activities, stating that, "[t]he need for agreement depends on what it is the community does." For chemists in the first half of the nineteenth century, he proposed, Dalton's atomic theory had rendered as "common property" the tools of constant proportion, multiple proportion and combining weights. Yet use of these tools did not require agreement on the existence of atoms, and chemists often disagreed about this point.

In considering this example, it is important to note that recognizing these highly specific details requires an internalist perspective of the "inner workings" of the particular subject matter, theories, alternative views, tools, techniques, and activities involved. This suggests that scholars who rely solely on the general or external aspects of a community (such as its subject matter or established institutions) may have difficulty in identifying a specialist community or in tracing the changes in its shared commitments or community structure. What is required, according to Kuhn, is an appreciation for the theoretical technicalities of the specialty; the cultural and sociological values of the group; and the specialized activities and interactions that occur within the group's formal and informal communication networks.

[119] For the concerns of critics regarding the existence of normal science or its distinction from extraordinary science, see articles by Popper, Toulmin, and Watkins in (Lakatos and Musgrave 1970).

science, occurs so regularly on this smaller scale that revolutionary, as against cumulative, change so badly needs to be understood" (*SSR*-PS, 181).

The Function of Specialist Communities

In the 1969 Postscript to *Structure*, Kuhn explained that specialist communities are "the units that this book has presented as the producers and validators of scientific knowledge. Paradigms are something shared by the members of such groups" (*SSR*-PS, 178). On this account, scientific knowledge develops and is validated within the most specialized areas of its disciplines. It is developed, validated, or – in the case of revolutions – overturned with respect to the shared commitments or paradigm that characterize a specialist community.

When considered in relation to traditional views of science, Kuhn's proposal appears rather strange. On the one hand, the discoveries of Copernicus, Galileo, Newton, or Einstein serve as bases for the traditional view that science develops most dramatically through the inspiration of genius. How can such inspired, individual accomplishments, long recognized as quintessential examples of scientific development, be reconciled with a view of communities as the producers of knowledge? On the other hand, Kuhn acknowledged that specialist communities are highly focused and typically rather small. Even if we grant that communities (rather than individuals) validate and produce scientific knowledge, how can such narrowly defined groups be responsible for something as monumental and far-reaching as the development of science? Even if the genius of an individual exerts a stronger influence within a small, specialist community, how can that influence extend to the broader community of scientists?

Kuhn considered traditional views of the role of individual genius both in his early works and in *Structure*, investigating the problems of early historiographers in supporting the "individual genius" view of discovery (*SSR*-PS, 2). As discussed in Chapter One, Kuhn's research revealed that even in seemingly straightforward historical cases, discoveries are not simply isolated or instantaneous moments of inspiration. Rather, discoverers benefit tremendously not only from the work of their famous forebears but also from the (often unheralded) labor of their contemporaries, immediate predecessors, and early supporters as well as developments within related fields.[120] Kuhn concluded that what characterized Galileo's "genius" was "the exploitation . . . of *perceptual possibilities* made available by a medieval paradigm shift" [i.e., the impetus theory, (*SSR*-PS, 119, emphasis added)].

This is not to suggest that individuals do not play an important role in the development of scientific knowledge. As we will see, their role is of crucial importance, as is their relation to various scientific communities. Yet in response to Popper and other philosophers, Kuhn explained that with respect to scientific knowledge, we cannot rely solely on the actions of isolated individuals:

> [Communities] could not, of course, function without individuals as members, but the very idea of scientific knowledge as a private product presents the same intrinsic problems as the notion of a private language. . . . Neither knowledge nor language remains the same when conceived as something an individual can possess and develop alone. (RMC, 253)

More succinctly, Kuhn later explained that, "though science is practiced by individuals, scientific knowledge is intrinsically a group product" (*ET-SS* 1977, xx). As he had noted at the conclusion of the *Postscript* to the second edition of *Structure*, to understand the processes and activities that are involved in the development of scientific knowledge, we must thus understand the scientific community that develops that knowledge.

Even if we accept that communities produce and validate scientific knowledge, we must ask how *specialist* communities can fulfill this role. It would seem that scientific knowledge can be developed most readily by the larger groups of scientists working within a sub-group, profession, or the sciences as a whole. At least some portion of scientific knowledge would seem to depend upon the efforts of scientists who are more broadly focused or upon the interactions of scientists communicating across different disciplines. On what basis can small groups of narrowly focused specialists hold primary responsibility for the scientific development? Alternatively, how can narrowly focused specialists transcend the interests of their individual disciplines to collaborate or even communicate within the larger group of scientists?

Kuhn neither justified nor explained his claim that scientific knowledge is produced by specialist communities; however, he did suggest that the focused nature of "specialized" activities (guided by detailed frameworks) renders them highly effective in extending existing knowledge and in identifying and isolating

[120] Kuhn's historiographic investigations suggested to him that "discoveries" possess a discernible, temporal structure that encompassed the "struggle with anomaly" as well as the (typically lengthy) attempts to refine the discovery and to assimilate it within the body of existing knowledge. As such, the "work" of discoverers was heavily influenced by the surrounding context (often by the existence and intensity of crisis) and by the past, present, and future members of related communities.

anomalies (*SSR*-PS, 52). Given the narrow range of their focus, the investigations and expectations of specialist communities thus reflect a degree of precision that generally is not available to more broadly based examinations. This precision, in turn, supports a more defensible basis for agreement within the community. It also heightens the visibility and the problematic nature of anomalies so that the members of a specialist community are more likely to identify and to investigate the ones that are most problematic. Given the precision of their expectations, subsequent investigations are more easily focused on the problem area in which the anomaly occurs. Finally, the small size of communities and the existence of both formal and informal communication networks support more rapid and more detailed sharing of information, experiments, and results.[121]

Relationships Among Communities

What, then, is the relationship between specialist communities and these "other potential actors" and how do these relationships affect the development of scientific knowledge? Kuhn's discussion of the multiple levels of community provides several suggestions. First, changes to the scientific knowledge of a community must be validated and justified by the members of specialist communities in their simultaneous capacities as specialists, as members of their profession, and as members of the scientific community. The development of scientific knowledge within a specialist community will thus be influenced by members' tacit conceptions of what is acceptable as specialized knowledge, as knowledge within the profession, and as scientific knowledge. Changes that threaten to undermine any of these conceptions will be subject to heightened scrutiny and if accepted, may sever the related community affiliations.[122]

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[121] The effectiveness of specialized activities has been studied by economists who have found that the most successful companies in the world tend to be clustered in localized geographic areas (Porter 1990). This localization supports investment in specialized infrastructure and supporting industries, and facilitates the flow of human resources, capital, and information. In particular, they note the benefits provided by "spillovers," whereby information, experiments or results are readily shared, developed, and adapted due to the close interrelationships.

[122] In this respect, it is possible that the members of a community may agree to changes that effectively reject other, more "basic" commitments to its profession or the scientific community. Such groups may continue to operate as a (specialist) community; however, they may no longer be considered as part of "higher-level" professional or scientific communities. Clearly, such considerations hold important implications for the nature of knowledge. These will be examined once we have developed a clearer conception of the community structure of science and the nature of a community's shared commitments.

Secondly, the shared commitments of a specialized scientific community include not only the unique commitments, interests or predispositions that distinguish them *as* a specialized group but also the broader commitments shared with a professional sub-group, the profession, and the community of scientists. Thus some of a community's shared commitments are unique to the group, while others overlap those of other, related communities. Because of these interrelationships, a community's commitments must be consistent and coherent not only within the context of its own activities but also with respect to the commitments held by other, related communities.[123] The adjustments required to assimilate an anomaly thus may have a cascading effect, influencing the original, specialist community first, then other related communities, then communities related to previously affected communities, and so on. Issues of assimilation and, in extreme cases, theory choice, thus may affect a number of communities that seem to be only tangentially related. The influence and importance of adjustments may vary among the affected communities, and the effects of these changes may reverberate back and forth as each community struggles to resolve its own issues and to respond to those raised by others.

We might thus revise Kuhn's assertion to state that scientific knowledge is produced and validated through the activities of specialist communities *and* their interlocking relationships with other communities.[124] While specialized commitments support a precision of expectation that is not available from more broadly focused investigations, overlapping commitments establish a relationship with the knowledge and activities of other communities. Commitments shared with other communities provide a basis for shared communication as well as a constraint to change or adjustment.[125] Knowledge produced in one community may readily influence other communities (in the form of either support or constraint), as

[123] The requirements for a community's commitments (and implicitly, for changes to those commitments) are closely related to issues of the cognitive authority of science. These requirements and issues will be examined throughout the remainder of this chapter.
[124] We still must understand the crucial questions of *how* it develops and the basis for its cognitive authority. These questions will be examined in Chapters Five and Six.
[125] Kuhn credited the concept of "constraint" to the formalism of Sneed-Stegmüller (TCSC 1976/2000, 179). As we will see, he emphasized that when modeling a theory to similar applications, pairs or sets of partial models provide constraints as well as direction. The structure of sets of similarity relationships thus involves not just likeness within a class but the differences of each from others in that class. According to Kuhn, this additional aspect establishes a triadic rather than a dyadic relationship, thereby avoiding a number of previously perceived philosophical problems (TCSC, 194).

may either successful or failed attempts at justification.[126] Similarly, individuals may be involved with several specialist communities, and this overlapping membership facilitates communication and constrains revision even further. Thus although specialist communities produce and validate scientific knowledge, they do so not as singular, isolated entities but as a multi-level, overlapping network of shared commitments and shared members.[127]

Given the interlocking relationships among the community's set of commitments and those that it shares with other communities, we can see that changing a community's structure or set of shared commitments presents epistemological, sociological and political challenges. Changes to scientific knowledge will be hard-won, and the difficulty in enacting them will be directly (if not exponentially) proportional to their influence and impact. In order for changes to be accepted by a specialist community, they not only must be integrated into the established network of commitments[128] held by that community but also must be accepted by the community as a whole.

These difficulties will be compounded for changes in commitments shared with other communities.[129] Other communities may not experience the anomaly, and thus may have no reason to accept the proposed changes (particularly if the proposed changes threaten existing commitments). If they reject these changes, there will be a resulting disparity in the shared commitments of the communities, and this may hamper future communication and further development among the communities. Thus it is important to understand how knowledge is produced (and changed) by specialist communities; how it is assimilated with the existing knowledge of that community; and how it comes to be accepted and assimilated by the various communities affected.[130]

[126] In this respect, Kuhn noted in the Postscript to *Structure* that "crises need not be generated by the work of the community that experiences them and that sometimes undergoes revolution as a result" (*SSR*-PS, 181).
[127] In this respect, the development of scientific knowledge is not only sociological but also political.
[128] This includes commitments to their specialty, their sub-group, profession, and the community of scientists. All of these commitments impact the development of scientific knowledge, yet the "shared commitments" of the scientific community would seem to be of a different sort from those of a specialist community. The nature and substance of such commitments will be examined subsequently.
[129] Kuhn would suggest that the heightened difficulty of more wide-ranging reconfiguration is appropriate.
[130] We still must address *how* the various members of communities make decisions of theory choice (or how they evaluate proposed changes to shared commitments). Further, we must understand how the decisions of individual members of a community are related to the (ultimate) "decision" of the community and how the decision processes of various communities are related.

This profile suggests important insights regarding the development of scientific knowledge over time. First and foremost, it suggests that the development of scientific knowledge has a variety of epistemological, sociological and political elements. The recognition of a discovery as "scientific knowledge" requires the acceptance and, in some circumstances, the refinement or extension of individual work by a community or set of communities. Those who would examine scientific development must consider not only the genius' moment of insight but also the community's preceding "struggle with anomaly" and subsequent efforts to incorporate the new "knowledge" into the interlocking commitments of their community (and possibly, other communities as well).

These considerations both broaden and delimit the locus of responsibility for the activities that support the development of scientific knowledge. Having identified specialist communities as "the producers and validators of scientific knowledge," we then qualified that statement by noting the importance of their interlocking relationships with other (specialist and higher level) communities. The mutual support and constraint provided by these relationships thus suggest that additions or revisions to scientific knowledge are made within relatively defined parameters.

Yet it is still unclear how such changes occur either within a specialist community or in "higher level" communities. What are the mechanisms that support changes in scientific knowledge and, given our activity-based approach, what are the processes whereby such changes are proposed, negotiated and accepted by the relevant communities? How does theory choice occur, either within a specialist community that is facing such a choice or in other, related communities that may be affected by it? Furthermore, what can be the cognitive authority of the decisions of theory choice made by a scientific community? As the sole audience and authority for a particular area of scientific knowledge, a community of scientific specialists would seem to be best suited to make decisions and determinations about that knowledge. Yet there must be some basis for those decisions other than the group's authority *as a group*. Even if the path to research consensus is extremely arduous – and our discussions above indicate that, particularly in more loosely configured communities, the challenges are severe – cognitive authority cannot lie simply in the achievement of consensus. For if it did, then science might be subject (as Kuhn's critics suggest) to accusations of "mob rule," "might makes right," or some form of irrationality or subjectivity.

We seem to have returned once again to the question of the cognitive authority of science and the proper basis for theory choice. To begin to answer these questions, we first must examine more closely Kuhn's conception of community, in particular, the relationship between individuals and their communities. As we will see, this relationship provides the basis for understanding the internal, developmental dynamics that take place within a scientific community. As such, it provides an indication of some of the processes involved in decisions of theory choice.

The Interrelation of Individuals and Communities

To understand how knowledge develops and how changes in community structure occur, it is important to articulate more carefully Kuhn's (implicit) conception of the scientific community and its differences from traditional conceptions. Through the course of Kuhn's investigations, this conception became more refined and more clearly articulated. In the Foreword to Paul Hoyningen-Huene's *Reconstructing Scientific Revolutions* (1989/1993), he clarified the relationship between individuals and communities in his theory:

> In *Structure* the argument repeatedly moves back and forth between generalizations about individuals and generalizations about groups, apparently taking for granted that the same concepts are applicable to both, that a group is somehow an individual writ large. . . . Groups do not have experiences except insofar as all their members do. And there are no experiences, gestalt switches or other, that all the members of a scientific community must share in the course of a revolution. Revolutions should be described not in terms of group experience but in terms of the varied experiences of group members. Indeed, that variety itself turns out to play an essential role in the evolution of scientific knowledge." (RSR-F 1989/1993, xii-i)

In this passage, Kuhn overstates the error of his earlier position. For in *Structure* and his publications of the late 1960s, he was adamant in emphasizing the value of individual differences in the activities of a scientific community.[131]

Kuhn thus rejected traditional conceptions of community as either an independent entity abstracted from a group of individuals or an aggregation of individuals. These traditional notions emphasize the community *in toto*, attempting to eliminate differences among individual members of that community. Such attempts are intended to eliminate subjective elements, such as individual psychology or values, which may not be relevant to an analysis of a community *as a whole*. By eliminating these subjective elements,

[131] See (*SSR*, 158), (*SSR*-PS, 200), and (RMC, 240).

however, Kuhn proposed that they also eliminated sources of individual difference that are central to the development of a community over time.

Although Kuhn rejected traditional conceptions of community, his comments above indicate that remnants of those views remained in his early work, specifically, in his discussion of the processes underlying gestalt switches and scientific revolutions.[132] The correction outlined above thus serves as a refinement of Kuhn's concept of communities rather than a revision of it. This distinction is important in suggesting a clarification of Kuhn's theory as opposed to a more fundamental change. In fact, the theory articulated in *Structure* is quite clear in rejecting traditional conceptions of community and, as we will see, it suggests an alternative conception that makes a clear distinction between a community and its individual members.

Kuhn proposed that individual differences play an important role in accounting for the forces at work within a community, thus traditional views cannot account for certain types of changes in community structure over time. If a community were simply an abstraction or an aggregation of its individual members, then the only types of change that a community could undergo would be those that are influenced by external factors. Yet historical case studies suggest that communities also undergo changes in the *internal* structure of their beliefs and commitments. In Kuhn's Aristotle experience, for example, he found that the change that occurred in the transition from Aristotelian to Galilean/Newtonian mechanics was not external but internal, specifically, in the conception of motion and the ontological relation of quality and matter.

Traditional conceptions generalize in order to eliminate difference, yet Kuhn's work suggests that in doing so, they neglect internal sources of change. As such, they provide only a partial and incomplete account of development over time. In a sense, they reflect a "Whiggish"[133] conception of community, whereby the current structure is projected onto other historical (or future) periods. Such generalizations

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[132] Kuhn's failure to address this implicit retention of older views earlier may be explained by his early emphasis on paradigms rather than community structure. Specifically, his early discussions of gestalt switches and revolutions were focused on the processes that are related to changes in a paradigm rather than those associated with a community and its members.

[133] Just as the approach of internal historiography is appropriate for uncovering differences obscured by a "Whig" approach to history of science, our investigation highlights possible ("Whiggish") biases in traditional conceptions of community, community structure, and possibly, even scientific development. These biases derive from generalized views of community, structure, or scientific development, which function as totalizing abstractions of their constituent parts.

perform an important function in facilitating those pedagogical and investigative activities that are focused on current structures (and which form the bulk of most scientific activity).[134] Yet when considered from an historian's developmental perspective, they obscure important differences that may have previously existed within a community. Kuhn proposed that these internal differences are an important source of change in the shared commitments and structure of a community over time.

In order to gain a comprehensive view of development, one must thus consider not only the external factors influencing change but also the "internal dynamics" at work within a community. These internal dynamics are the result of differences that emerge and develop among the members of a community over time, including their relationships to each other and to their shared commitments. Kuhn proposed that these differences occur with respect to the interpretation, application, and extension of shared commitments. From a common basis, then, a number of varying approaches, interpretations, applications, and perspectives will emerge. Some of these may be employed by isolated individuals; some by sub-groups or segments within the community.

Kuhn's conception of community is based on shared commitments that bring together the members of a community yet also allow for differences among them. As a reflection of shared commitments, the basis of a community is positive, defined, and specific. Yet he proposed that these commitments need not be totalizing. Furthermore, differences among the members of a community will be constrained by the commitments that they share with their community and by the interlocking relationship of their community with others.

The Function of Individual Difference

A scientific community that encompasses individual differences can be identified by the shared commitments of its members, but it cannot be reduced to them. To remain a "member" of the community, a specific set of commitments must be shared, yet within those boundaries there will be a range of different relationships (to the commitments, to the community, and to other members). Some members may interpret those commitments more rigidly or apply them more rigorously, while others may interpret them pragmatically and apply them more loosely. Finally, members may use different techniques or tools for

[134] These generalizations are thus similar to what Kuhn describes as "textbook science."

interpretation and application. These differences are usually inconsequential but particularly during periods of crisis, they may be an important (internal) source of changes in community structure. Kuhn insisted that this variability of judgment is essential to scientific advance.

The most dramatic impact on community structure occurs when some members undercut and, ultimately, unseat some of the commitments shared by the community, prompting a revolutionary reconfiguration of group commitments. In such cases, the activities are likely to be conducted by a subset of members who bring a unique perspective to their investigations. This smaller group's activities will be influenced not only by the shared commitments of the community but also by the distinctive ways in which they interpret, apply, or extend those group commitments. As such, the relevance of their activities may hold immediate relevance only for themselves. Even further, this relevance may not extend to all members of that smaller group, and the results may be interpreted differently by different members.[135] To the extent, however, that a specialists group remains relatively focused, its membership will be relatively small and the differences among group members will be more apparent.

Implications of the Community Structure of Science

From this conception of community structure, we can see that not all communities will be alike. Some may have a broad range of differences among their members, while others may have a narrower range of diversity. The activities of each community will reflect these degrees of difference. The members of communities characterized by greater diversity will conduct a broader range of loosely related activities, whereas those in closer agreement with other members will conduct activities that are more narrowly focused and more relevant to each other.[136] The "results" of activities will vary correspondingly, with more

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[135] In understanding the processes involved in the development of knowledge, we must thus consider not only the conduct of activities by the collective members of a specialist community nor simply the results and relevance of those activities. Rather, we must also consider the internal processes whereby what is relevant to some subset of members may be deemed relevant to the shared commitments of the community *as a whole*. That is, we must understand the internal processes whereby the activities of a subset of members (which are guided but not fully determined by the shared commitments of the community) can, in turn, influence the commitments that are shared by all members. As outlined above, this influence may extend not only to the community of specialists but also to communities within the sub-group, the profession, or the field of the natural sciences.

[136] It is likely that within diverse communities there will be a number of smaller sub-groups with shared views about issues on which the community as a whole does not agree. This raises questions of the proper

wide-ranging results emerging from the activities conducted within diverse communities and more specific and precise results achieved by highly focused communities.

Similarly, the character and the activities of a community will vary over time as its members' activities reinforce, adjust, or undercut the shared commitments that define them as a community. Some of the activities conducted by members of the community may yield results with important implications for their shared commitments. In some cases, they may reinforce those commitments. In others, they may extend them to new situations. Yet in some cases, the results of activities may conflict with or seem to undermine those commitments. Whatever the circumstance, the activities conducted by various members of a community will not only be influenced by their shared commitments but also will, in turn, impact those commitments themselves. As such, they may ultimately impact the community structure from which they derive – and, in extreme cases, impact the commitments and structure of other communities as well.[137]

New Perspectives on the Transition to Maturity

Given these considerations, we can deepen our view of a community's transition to maturity. In the pre-transition period, the shared commitments of a community may extend only to the investigation of a particular part of nature. Competing schools will differ in their interpretation of even the most basic conceptual categories of nature (e.g., example). In this pre-transition period, the most effective experimentation, evaluation, and communication of results will occur within the various "schools" and communication between schools will be difficult at best. In a situation characterized by such diversity, it is difficult for a single subgroup to influence the community as a whole. It will thus be difficult for the members of a highly diverse community to overcome the differences that separate them.

This explains why the transition to maturity – the point at which a community's shared commitments can support puzzle-solving research – is such an important yet difficult achievement. With

points of demarcation for a specialist "community" relative to the various "schools" that may exist within that community.

[137] The flexibility that characterizes the members of a specialist community may also apply to the relationships among related communities. That is, communities may be similar with respect to some set of shared commitments; however, they may differ in their interpretation or application of them. In this way, the "collections" of communities at various levels will be mutually supportive yet not deterministic. Moreover, the degree of support (or challenge) among communities may vary depending on the diversity that exists between them.

the transition to maturity, the number of competing schools is eliminated when one school succeeds by emphasizing a particular part of the wide range of available "facts." The views of this school are adopted as the shared commitments of the community as a whole. These shared commitments are of a different nature from those of the pre-transition period in that they can support "puzzle-solving" activities (i.e., "it is the nature of the paradigm that changes").[138] Differences of interpretation and application still exist among the members of a community; however, these, too, are of a different sort.

The transition to maturity thus reflects a shift in the nature of a community's shared commitments and in the nature of its differences. The commitments shared by a mature scientific community extend to the basic conceptual categories of nature. As such, they guide a community's puzzle-solving activities and enrich its expectations with a detail and precision not previously available. They do so, however, in a way that is not totalizing and deterministic but preserves differences in interpretation and application.

Within a mature scientific community, the differences in the interpretations and applications of various sub-groups are neither as apparent nor as decisive as in the pre-transition period. Yet when a crisis arises, they become more evident and more important as the various groups attempt to resolve the crisis in different ways. As in the pre-transition period, such differences may prove crucial to resolving the crisis, in that one sub-group may propose a way of interpreting or applying the community's shared commitments that can explain the anomaly. This process may well parallel that by which competing schools are eliminated in the transition to maturity.

Outstanding Issues

In positing communities of scientific specialists as "the producers and validators of scientific knowledge," Kuhn seemed to suggest that the processes involved in the development of scientific knowledge can be understood by examining the community structure of science. The community structure of science, he proposed, is related to the shared commitments of the members of a specialist community (albeit in an as-yet-undetermined way). Yet his discussions raised a number of important issues that must be addressed if we are to link these processes with the cognitive authority typically attributed to science.

[138] This different nature of shared commitments (and the relation to Kuhn's notion of paradigm) will be examined in the next section.

Specifically, we must understand how the activities of a specialist community can be established *as* scientific knowledge. Although Kuhn's discussion of the community structure of science addresses the processes involved in the development of scientific knowledge, he failed to specify the epistemological basis of those activities as leading to the establishment of scientific knowledge. His later works on incommensurability can be seen as an attempt to address this issue; however, they did so only indirectly. To answer this question, we must understand the group commitments that constitute a given scientific community and the scientific knowledge purportedly produced and validated by those commitments.

Section Two

The Disciplinary Matrix

Kuhn proposed that the "analytic separation" of specialist communities and their paradigm highlights two distinct usages of the term (*SSR*-PS 1969/1970a, 174-5).[139] The first of these is the community's set of shared commitments, or "the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community" (*SSR*-PS, 175). Kuhn described this as the sociological usage of paradigm and re-dubbed it the disciplinary matrix: "'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification" (*SSR*-PS, 182). The second usage, which Kuhn now termed exemplars, is a particular type of shared commitment within the disciplinary matrix. He proposed that exemplars, or shared examples, represent the more appropriate and the more philosophically important usage of the term "paradigm."

Through the late 1960s and the early 1970s, Kuhn explored the notion of exemplars, characterizing them as concrete examples that support puzzle-solving activities. While each exemplar represents a concrete puzzle-solution, Kuhn proposed that the study of (multiple) exemplars yields a knowledge of nature that is unavailable from theories, laws and rules. Specifically, he proposed that the study of exemplars yields a "way of seeing" such that the student can solve not only the scientific puzzles with which she is presented but also new puzzles presented in a slightly different form.

Kuhn suggested that the basis of puzzle-solving ability is not the direct application of generalized laws or theories but the recognition of a primitive similarity resemblance among a set of exemplars, such that problems are thereby transformed to puzzles. In this way, he suggested, the content of science enters not only "at the bottom," that is, through the identification of facts as referents for the terms of a symbolic generalization, but also "at the top," through the study of exemplary puzzle solutions. The study of exemplars thus serves as an additional source for the cognitive content of science and provides an alternative to the application of theories, laws, rules, or their associated symbolic generalizations.[140]

[139] These distinctions (implicitly) represent Kuhn's reinterpretation of Masterman's 21 usages.

[140] As we will see, the content provided by exemplars is not simply the content of individual "facts" but also the relationships that exist among them. It is these relationships (discerned in a way remains

In its comprehensive collection of all factors that influence scientific activity, the disciplinary matrix was Kuhn's answer to the limitations of traditional, philosophical conceptions of "theory" as that which guides scientific investigations: "[a]s currently used in philosophy of science. . . 'theory' connotes a structure far more limited in nature and scope than the one required here" (Ibid.). The disciplinary matrix thus not only establishes the views, instruments and standards of scientific activity but also includes the arbitrary beliefs and points of difference that exist within the scientific community.

While the elements of the disciplinary matrix "form a whole and function together," Kuhn insisted that "[t]hey are, however, no longer to be discussed as though they were all of a piece" (*SSR*-PS, 182). In particular, he identified elements of particular importance and proposed that "[a]lterations in any one can result in changes of scientific behavior affecting both the locus of a group's research and its standards of verification" (ST 1974/1977, 298).[141] Consideration of these key elements thus is important not only for purposes of inclusiveness but also for explanation of the changes that occur in the development of scientific activities and standards over time. Kuhn identified these influential elements as symbolic generalizations; ontological and heuristic models; values; and exemplars (*SSR*-PS, 182-7).[142] These elements provided the focus for his subsequent investigations and underlay his research activities for the remainder of his career.

Symbolic Generalizations

Kuhn characterized symbolic generalizations as "formal or readily formalizable components of the disciplinary matrix," which provide the points at which members attach logical and mathematical techniques to their observations and experiments (*SSR*-PS, 182). These generalizations, which he also described as "laws" or "law-schema," may be represented either in symbolic form ($f = ma$) or in linguistic form ("elements combine in constant proportion by weight").

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unidentified) that transform problems to puzzles and thus serve as a (non-theoretic) basis for "scientific" activity.

[141] Following this important claim, Kuhn stated that he did not "attempt to defend a thesis quite so general" but intended to focus his discussion on exemplars (Ibid.). The failure to provide even a broad defense of this thesis is unfortunate given that it reflects a central point of his argument, namely, that the investigations of sociology or history can present important (philosophical) implications for a community's research and standards of verification.

[142] In the first written (but last presented) of the three essays, "Second Thoughts on Paradigms," Kuhn outlined only three elements of the disciplinary matrix, omitting the consideration of values (ST 1974/1977, 298).

In what seemed to be a casual observation, Kuhn explained that certain types of these generalizations often function not only as laws of nature but also as definitions of some of the symbols that they introduce or represent:

> These generalizations look like laws of nature, but their function for group members is not often that alone. Sometimes it is: for example the Joule-Lenz Law, $H = RI^2$. When that law was discovered, community members already knew what H, R, and I stood for, and these generalizations simply told them something about the behavior of heat, current, and resistance that they had not known before. *But more often . . . symbolic generalizations simultaneously serve a second function, one that is ordinarily sharply separated in analyses by philosophers of science. Like $f = ma$ or $I = V/R$, they function in part as laws but also in part as definitions of some of the symbols they deploy.* Furthermore, the balance between their inseparable legislative and definitional force shifts over time. (*SSR*-PS, 183, emphasis added)

It seems, then, that there are two distinctive types of symbolic generalizations, which are not differentiated by philosophers of science. The first type, exemplified by the Joule-Lenz Law, serves only a legislative function, relying on previously defined terms. In contrast, generalizations of the second type, such as Newton's Second law or Ohm's Law, also function as definitions of some of the terms that they employ. As such, they perform a *dual legislative-definitional function* that occurs simultaneously. While the dual function of these more specialized generalizations occurs simultaneously, the respective *force* of those functions shifts over time. Yet importantly, the analytic distinctions typically made by philosophers of science serve to separate these functions, thereby concealing their interrelation and the shifts that occur over time in the balance of their respective forces.

Kuhn noted that philosophically, the distinction between these two types of symbolic generalizations holds important implications for scientific development:

> In another context these points would repay detailed analysis, for the nature of the commitment to a law is very different from that of commitment to a definition. Laws are often corrigible piecemeal, but definitions, being tautologies, are not. For example, part of what the acceptance of Ohm's Law demanded was a redefinition of both 'current' and 'resistance'; if those terms had continued to mean what they had meant before, Ohm's Law could not have been right; that is why it was so strenuously opposed as, say, the Joule-Lenz Law was not. (Ibid.)

For symbolic generalizations that are solely legislative (i.e., that rely on previously defined terms), changes, revisions, or adjustments may be made piecemeal. Yet those generalizations that perform both legislative and definitional functions are, according to Kuhn, tautologies that must be either accepted or rejected *as a whole*. Furthermore, to the extent that this second type *redefines* previously defined terms (as in the case of Ohm's Law reconceptualization of "current" and "resistance"), the acceptance or adoption of such generalizations may impact the viability of previously established laws. As such, these generalizations may present challenges to existing views based on their now-reconfigured terms. These dual-function generalizations may be "strenuously opposed" in ways that are not experienced for those that are solely legislative.

Kuhn noted that to some extent, the dual function of specialized generalizations renders them tautologous with respect to their self-defined terms. Changes in these dual legislative-definitional generalizations thus serve as the impetus for revolution: "I currently suspect that all revolutions involve, among other things, the abandonment of generalizations the force of which had previously been *in some part* that of tautologies" (*SSR*-PS, 183-4, emphasis added).

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To the extent that we seek to understand the intimate interrelation of facts and theories in scientific activities, it would appear that this relation is constitutive of those (specialized) generalizations that serve both definitional and legislative functions. To investigate this insight further, we must understand how and with what authority this "dual" function is established. For if the force of these generalizations is "in some part that of tautologies," which are abandoned in the course of scientific revolution, then in order to avoid the challenges of relativism in theory choice, we must understand what constitutes the *other* basis of their force. As we will see in Kuhn's discussion of exemplars, these specialized generalizations are established only by consideration of actual illustrations. Thus although such generalizations are tautologous with respect to the (logical) interrelation of their legislative and definitional functions, they are not self-positing but are established, and to a great extent, constrained, by empirical observations and scientific standards for the establishment and acceptance of exemplars.[143]

[143] From an investigative perspective, it is also important to note that distinguishing between Kuhn's two types of symbolic generalizations requires in-depth understanding of the historical context in which a symbolic generalization is introduced. This is required to determine whether a newly introduced generalization relies upon previously established definitions or introduces its own definition for some of its operative terms. This task requires detailed understanding of both the technical and the historical aspects of scientific activity, yet it is obstructed by the anachronistic tendencies of a "textbook" approach to the history of science, a bias highlighted by the "new" historiography. Further, as Kuhn later pointed out, it is also obstructed by the external focus that later came to dominate the study of the sociology and history of science.

Heuristic and Ontological Models

The second element of Kuhn's disciplinary matrix was "beliefs in particular models," including both heuristic and ontological models (*SSR*-PS 1969/1970a, 184). Kuhn described heuristic models as analogies or metaphors that explain the behavior of phenomena, for example, "the electric circuit may be regarded as a steady-state hydrodynamic system" or "the molecules of a gas behave like tiny elastic billiard balls in random motion" (Ibid.). Ontological models also function as analogies or metaphors, yet they do so by connecting observable phenomena with their metaphysical bases. Kuhn described them as "shared commitments to such beliefs as: heat is the kinetic energy of the constituent parts of bodies; all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void, or, alternatively, to matter and force, or to fields" (Ibid.).

Kuhn noted that heuristic and ontological models differ in important ways, most notably in the strength of group commitment. Yet he insisted that "all models have similar [cognitive] functions" and thus, for his purposes, their influence within the disciplinary matrix is the same:

> Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each. (Ibid.)

Beliefs in models thus provide a scientific community with a way to understand the behavior of phenomena; to identify and to prioritize "unsolved puzzles;" and to specify the required explanation or solution. Through these functions, they support the application of symbolic generalizations to a range of important (yet purportedly soluble) problems.[144]

[144] Kuhn's discussions of these models were limited, and he did not explore fully either the distinction between ontological or heuristic models or their relationship to the rest of the disciplinary matrix. This neglect is surprising in several respects. First, substantial research in the philosophy of science focused on the role of heuristic models in the conduct of scientific activities. It would seem that Kuhn would address the major points of this research or at least direct the reader toward more detailed discussions. Secondly, in combining heuristic and ontological models, Kuhn conflated what were, at the time, two distinct points of investigation for philosophers of science. His justification on the basis of their similar "cognitive functions" is supported by his activity-based approach; however, a more in-depth argument would seem to be needed. Finally, even if the two types of models have similar cognitive functions, differences in the level of group commitment would seem to be important, particularly given Kuhn's emphasis on the community structure of science.

Values

The third element of Kuhn's disciplinary matrix was the set of values shared by the members of a scientific community.[145] Kuhn proposed that the values of a community and its members influence their scientific activities in ways that range from debates about the social contributions of science to decisions of theory choice. Further, he suggested that shared values function as an integral point of connection among the various types of scientific communities: "[u]sually they are more widely shared among different communities than either symbolic generalizations or models, and they do much to provide a sense of community to natural scientists as a whole" (*SSR*-PS 1969/1970a, 184). As we will see in Chapter Five, these points of connection (and influence) are particularly important in situations of theory choice.

Although the values of a community "function at all times," Kuhn emphasized that, "their particular importance emerges when the members of a particular community must determine whether an anomaly is sufficiently problematic as to provoke a crisis or, later, must choose between competing yet incompatible ways of practicing their discipline" (*SSR*-PS, 184-5). Positing the importance of shared values at these crucial stages of development, he commented, "I now think it a weakness of my original text that so little attention is given to such values as internal and external consistency in considering sources of crisis and factors of theory choice" (*SSR*-PS, 185).

Kuhn proposed that despite the inevitability of individual differences with respect to values, one should not conclude that shared values are therefore inconsequential. To the contrary, he insisted (in response to philosophers of science) that "shared values can be important determinants of group behavior even though the members of the group do not all apply them in the same way" (*SSR*-PS, 186). Furthermore, he claimed that shared values play a crucial role in determining the direction of scientific development:

> It is vitally important that scientists be taught to value these characteristics [of good theory choice] and that they be provided with examples that illustrate them in practice. If they did not hold values like these, their disciplines would develop very differently. Note, for example, that the periods in which the history of art was a history of progress were also the periods in which the

[145] Values are not addressed in "Second Thoughts on Paradigms" (ST 1974/1977) the first written and last published of the late-1960s essays. They are introduced as the third key element of the disciplinary matrix in both the "Postscript" (*SSR*-PS 1969/1970a) and "Reflections on My Critics" (RMC 1969/1970c). In the last essay, the role of values in situations of theory choice is emphasized. The discussion of theory choice is the focus of a later article, "Objectivity, Value Judgment and Theory Choice" (OVJ 1973/1977), which represents an extension and refinement of the arguments presented in the earlier essays.

> artist's aim was accuracy of representation. With the abandonment of that value, the developmental pattern changed drastically though very significant development continued. (RMC, 261-2)

Thus when viewed as values, the criteria associated with good theory choice influence scientific activity and development, even if they do not determine it.

Given the influence of shared commitments, we must also recognize the influential role played by the community structure of science from which they derive. Kuhn described shared commitments as "ideological commitments which scientists must share if their enterprise is to succeed" and concluded, "[t]hey are therefore irreducibly sociological. . ." (RMC, 240). Yet he emphasized that in acknowledging the influence of shared commitments, we must continue to give precedence to the role of specialist communities. Both the situations of choice and the shared values of these groups will be more precise than those of "higher level" communities. For these reasons, Kuhn emphasized that

> . . . unlike most disciplines, the responsibility for applying shared scientific values, must be left to the specialist group. It may not even be extended to all scientists, much less to all educated laymen, much less to the mob. If the specialists' group behaves as a mob, renouncing its normal values, then science is already past saving. (RMC, 263)

Yet while this statement highlights the role of specialist communities, it also suggests the importance of the (tacit) influence exerted by other, "higher level" communities.[146]

Exemplars

The final element of the disciplinary matrix was exemplars, which Kuhn characterized as the more specific and the more appropriate use of the term "paradigm." Within the context of scientific development, Kuhn defined exemplars as "concrete problem-solutions that students encounter from the start of their scientific education" (*SSR*-PS 1969/1970a, 187). These are the technical problems included in laboratory experiments or textbooks, and in the periodical literature. Kuhn emphasized that these "technical problems" are not

[146] Given the community structure of science, the shared values employed by the members of a specialist community will include some values that are shared with other related and "higher-level" communities. These connections with the values of other communities play a particularly important role in situations of theory choice, in which some of the community's own shared commitments may be at issue. In such situations, commitments shared with other communities may play an especially important role in the delineating values, weighing their relative importance, and interpreting them at a high level. This influence and the nature of the "other" community structures will be examined in Chapter Five's consideration of theory choice.

merely problems but have identifiable solutions. As problem-solutions, they operate like puzzles in that they are guaranteed to have an acceptable solution. He identified these "puzzle" solutions as the shared examples of a community, noting that "the notion of paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect" of *Structure* (Ibid.).

Kuhn pointed out that within the philosophy of science, puzzle-solving exercises typically are perceived as mere illustrations or examples of a particular theory. Yet he insisted that these technical puzzle solutions play a much more important and distinctive role in the actual conduct of science:

> Philosophers of science have not ordinarily discussed the problems encountered by a student in laboratories or science texts, for these are thought to supply only practice in the application of what the student already knows. He cannot, it is said, solve problems at all unless he has first learned the theory and some rules for applying it. Scientific knowledge is embedded in theory and rules; problems are supplied to gain facility in their application. I have tried to argue, however, that this localization of the cognitive content of science is wrong. After the student has done many problems, he may gain only added facility by solving more. *But at the start and for some time after, doing problems is learning consequential things about nature*. In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content. (*SSR*-PS, 187-8, emphasis added)

These statements suggest that Kuhn's alternative view of the *cognitive authority* of science is based on a different view of its *cognitive content*. In the initial stages of study, students learn about nature not by learning a scientific theory and the rules for its application but by doing problem-solutions. Thus "at the start and for some time after," students acquire knowledge of nature by studying exemplars and doing the problem-solutions presented by their teachers and their textbooks.

In establishing exemplars as the source for acquiring the cognitive content of science, Kuhn insisted that they are distinct from symbolic generalizations and cannot be reduced to or subsumed by scientific laws, theories, or rules. Even further, he suggested that the example of taxonomy indicates that "a science can exist with few, perhaps with no . . . generalizations," implying that the knowledge of nature that is provided by exemplars is more fundamental than the knowledge that is provided by generalizations (ST 1974/1977, 298).[147] In this respect, the cognitive authority of science is not established by the logical

[147] It is important to note that the example of taxonomy differs from exemplars in that the former is the "classification of plants and animals on the basis of their natural relationships" whereas the latter are exemplary puzzle solutions (Webster 1977). Kuhn provided an initial sketch of this relationship by noting that "[t]he herbaria, without which no botanist can function, are storehouses for professional exemplars, and their history is coextensive with that of the discipline they support" (ST 1974/1977, 313).

As Kuhn's work developed, he began to highlight the taxonomic aspects of exemplars as particularly important for the development of scientific knowledge and proposed a "taxonomic solution" to the problem of meaning change. This development and its implications will be examined in Chapter Five.

structure of scientific generalizations (i.e., by laws, theories, or rules) but must be provided (in some as yet unspecified way) by a community's shared examples. This is not to suggest that generalized laws or theories do not play an important role in the development of scientific knowledge. To the contrary, Kuhn agreed with "the widespread impression that the power of a science increases with the number of symbolic generalizations its practitioners have at their disposal" (ST, 299). Yet while the proliferation of symbolic generalizations heightens the power of a science, he proposed that such generalizations themselves rely upon a (n alternative) type of knowledge that is provided (at least initially) only by study of the community's exemplars.

Kuhn thus proposed that concrete exemplars, or shared examples, have a distinctive cognitive function, which renders them an important and irreducible part of the disciplinary matrix. Yet he did not provide a clear account of the knowledge of nature that is provided by exemplars, nor of the processes by which this knowledge can be established or attained *as* knowledge. Although Kuhn proposed that exemplars provided at least some of the cognitive content of science, he failed to explain how this content might serve as the basis for the cognitive authority of science (and thus for determinations of theory choice). Thus while exemplars represented a needed clarification of the notion of paradigm outlined in *Structure*, their role in the development of scientific knowledge remained rather vague.

Section Three

Exemplars and the Language-Nature Link

In order to understand the nature of exemplars and their role in the development of scientific knowledge, we must address three questions. First, we must understand what can be the cognitive content or knowledge of nature that is gained from doing exemplary problems. Secondly, we must understand why this “knowledge” is not available from symbolic generalizations alone. Finally, we must understand the epistemological authority of this knowledge, vis-à-vis traditional views.

Margaret Masterman’s comments on the construct sense of paradigms provide a valuable starting point for these considerations (Masterman 1970). Based on her examination of the function of paradigms within *Structure*, Masterman proposed (and Kuhn later confirmed) that the primary sense of the notion of paradigms was as a concrete artefact or construct that makes puzzle-solving possible. In expanding upon this proposal, she pointed out that a construct paradigm must both provide a concrete puzzle-solution and serve as a model for other such solutions. This dual function, she proposed, could be accomplished only by a *crude analogy*.

In highlighting the unique aspects of the notion of paradigms, Masterman noted that Kuhn had reintroduced concreteness into philosophical investigations of science by inverting the established philosophical approach. While philosophers of science attempted to show how a metaphysical system might be used as a model, Kuhn initially investigated concrete situations and then attempted to show how these might be used metaphysically. The issue raised by Kuhn’s work thus was how “a construct-paradigm [can] become a ‘way of seeing’” (Ibid., 73).

To address this issue, Masterman outlined two areas for consideration. First, she proposed that Kuhn must “describe philosophically the original trick, or device, on which the sociological paradigm (i.e., the set of habits is founded,” that is, how the crude analogy can be established (Ibid., 70). Secondly, she noted that in order for a paradigm to function *as* an analogy, Kuhn must also explain how it could extend itself by “replication” yet without the introduction of ancillary rules.

In order to understand the function of exemplars in the development of scientific knowledge, we thus must understand how they provide cognitive content or knowledge of nature that is not available from symbolic generalizations alone. Kuhn proposed that once established, this knowledge guides the way that symbolic generalizations are applied to concrete situations, specifically, "the manner in which the symbols, individually and collectively, are to be correlated with the results of experiment and observation" (ST 1974/1977, 299). He insisted that this knowledge cannot be provided by the generalizations themselves but is provided by something akin to ostension, which establishes a connection between symbolic generalizations and concrete, natural phenomena. Yet unlike mere ostension, this connection also serves as a model or basis for further articulation.

Kuhn's first attempts to clarify the notion of paradigms and to specify the function of exemplars were presented in the three essays of the late 1960s. In these essays, he affirmed Masterman's proposal regarding the various senses of the term "paradigm," and emphasized the role of construct paradigms in supporting the puzzle-solving activities of science. He also attempted to address how a construct paradigm might be established (initially) and extended (later) without recourse to ancillary rules.

In "Second Thoughts," Kuhn highlighted the role of exemplars in providing cognitive content and knowledge of nature that is unavailable from symbolic generalizations alone. In the "Postscript," prepared for the second edition of *Structure*, Kuhn claimed that exemplars "can replace explicit rules as a basis for the solution of the remaining puzzles of normal science," characterizing the knowledge of nature gained from the study of exemplars as a community-licensed "way of seeing" (*SSR*-PS, 175). Finally, "Reflections on My Critics" introduced a shift away from psychological and visual metaphors (which he had used in *Structure* to explain this way of seeing), and toward a reliance on language, in particular, the "language-correlated or language-coordinated way of seeing the world" (RMC 1969/1970c, 274). Expanding on Masterman's earlier conception of the notion of paradigm, Kuhn proposed that the study of exemplars establishes the language-nature link in a way that "transforms problems to puzzles and enables them to be solved even in the absence of an adequate body of theory" (RMC, 273).

"Second Thoughts:" Exemplars and the Cognitive Content of Science

In "Second Thoughts on Paradigms," Kuhn proposed that exemplars function as a source for the cognitive content of science, thereby providing knowledge of nature that is unavailable from scientific theories, laws, or rules alone. He began by noting that philosophers of science typically view the laws that are associated with scientific theories as uninterpreted formal systems, and as analogous to the laws of a pure mathematical system. Yet he proposed that this view cannot account for the adjustments in those generalizations that may be required in applying them to various concrete situations:

> When an expression like $f = ma$ appears in a pure mathematical system, it is, so to speak, there once and for all. If, that is, it enters into the solution of a mathematical problem posed within the system, it always enters in the form $f = ma$ or in a form reducible to that one by the substitutivity of identities or by some other syntactic substitution rule. In the sciences symbolic generalizations ordinarily behave very differently. *They are not so much generalizations as generalization-sketches, schematic forms whose detailed symbolic expression varies from one application to the next*. For the problem of free fall, $f = ma$ becomes $mg = (md^2s/dt^2)$. For the simple pendulum, it becomes $mg\sin\Theta = -ml(d^2\Theta/dt^2)$.[148] For coupled harmonic oscillators it becomes two equations, the first of which may be written $m_1(d^2s_1/dt^2) + k_1s_1 = k_2(d + s_2 - s_1)$. More interesting mechanical problems, for example, the motion of a gyroscope, would display still greater disparity between $f = ma$ and the actual symbolic generalization to which logic and mathematics are applied; but the point should already be clear. Though uninterpreted symbolic expressions are the common possession of the members of a scientific community, and though it is such expressions which provide the group with an entry point for logic and mathematics, it is not to the shared generalizations that these tools are applied, but to one or another special version of it. In a sense, each such class requires a new formalism. (ST 1974/1977, 299-300, emphasis added)

According to Kuhn, the symbolic generalizations that are associated with scientific laws differ from those of pure mathematical systems in that they may require a "special version" in order to attach to concrete situations in nature. Such generalizations[149] thus are more precisely conceived as generalization-sketches. Without further specification, their "attachment" to nature would remain indirect and thus incomplete.

Interestingly, Kuhn pointed out that scientists often identify the appropriate special formalism "in advance of directly relevant empirical evidence" and that "with remarkable frequency . . . the community's judgments prove to be correct" (ST, 301). Thus while concrete specification of the generalization is required, this specification need not be provided by direct exposure to the specific, concrete situation. On the other hand, Kuhn proposed that these hypothetical specifications cannot be made on the basis of rule-

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[148] This quote is taken from "Second Thoughts," the first written of the late-1960s essays, and it appears in more condensed form in the other two essays. Comparison of the three references reveals that the formula for the simple pendulum is incorrect in this first essay and is corrected in the later two essays. The quote above has been adjusted to reflect this correction.

[149] Kuhn did not distinguish among various types of generalizations in making these proposals; however, he consistently used Newton's Second law to illustrate the issues and implications involved. As mentioned previously, this particular law is distinctive in that it has a dual definitional-legislative function. It is with respect to such dual-function generalizations that Kuhn's comments are clearest and most defensible vis-à-vis traditional views in the philosophy of science. In this case, generalizations whose terms are antecedently available (such as Joule's law) would require no special versions for their application because the associated definitions do not change in their application to new situations. This is an important point of difference between the two types of generalizations; however, those distinctions often remained unnoted in Kuhn's statements and the importance of those differences seems to have been unrecognized in much of his work.

governed criteria, operational definitions, or necessary and sufficient conditions because these cannot be applied directly to the newly encountered situation. No established standards or rules are available in ways that might prove decisive for determining the specialized formulation required for the new situation. Nor can the range of all possible empirical applications be embedded within the basic vocabulary of the generalizations themselves.

Kuhn concluded that, "no conjunction of particular symbolic forms would exhaust what the members of a scientific community can properly be said to know about how to apply symbolic generalizations" (ST, 301). He proposed that this (specialized) knowledge can be acquired only through the study and usage of the community's concrete exemplars. In considering the need for this "alternative source of cognitive content, it is also important to consider the extent to which the resulting knowledge about how to apply the symbolic generalization represents an "alternative" kind of knowledge.

The Limitations of a Basic Vocabulary

Kuhn proposed that the knowledge of nature required to develop specialized formalizations extends beyond the direct application of symbolic generalizations to the way that these may be linked with and applied to different situations. Asking the question, "How do scientists attach symbolic expressions to nature?", he explained that "[s]ince the abandonment of hope for a sense-datum language, the usual answer to this question has been in terms of correspondence rules" (ST, 301-2). Kuhn insisted, however, that the linkage between symbolic generalizations and the situations they would explain is neither as direct nor as unproblematic as the correspondence theory of truth would suggest:

> It is, I think, remarkable how little attention philosophers of science have paid to the language-nature link. Surely, the epistemological force of the formalists' enterprise depends upon the possibility of making it unproblematic. One reason for the neglect is, I suspect, a failure to notice how much has been lost, from an epistemological standpoint, in the transition from a sense-datum language to a basic vocabulary. While the former seemed viable, definitions and correspondence rules required no special attention. "Green patch there" scarcely needed further operational specification; "benzene boils at 80 degrees centigrade" is, however, a very different sort of statement. (ST, 303n)

The phrases invoked by sense-datum language, such as "green patch there," imply a direct and unproblematic relationship that is not as straightforward in the statement, "benzene boils at 80 degrees centigrade." While the former implies a direct correspondence between observed "facts" and scientific theories, the latter involves a more complicated view of the "facts" presented. Direct correspondence thus is more difficult to establish.

In considering the limitations of these views, Kuhn proposed that there is an alternative source for the cognitive content of science:

> Those philosophers who exhibit scientific theories as uninterpreted formal systems often remark that empirical reference enters such theories from the bottom up, moving from an empirically meaningful basic vocabulary into the theoretical terms. Despite the well-known difficulties that cluster about the notion of a basic vocabulary, I cannot doubt the importance of that route in the transformation of an uninterpreted symbol into the sign for a particular physical concept. But it is not the only route. Formalisms in science also attach to nature at the top, without intervening deduction which eliminates theoretical terms. Before he can begin the logical and mathematical manipulations which eventuate with the prediction of meter readings, the scientist must inscribe the particular form of $f = ma$ that applies to, say, the vibrating string or the particular form of the Schrödinger equation which applies to, say, the helium atom in a magnetic field. Whatever procedure he employs in doing so, it cannot be purely syntactic. *Empirical content must enter formalized theories from the top as well as the bottom.* (ST, 300, emphasis added)

Acknowledging the importance of the "bottom-up" movement of empirical reference (i.e., from a basic vocabulary into theoretical terms), Kuhn thus proposed that there is an additional route by which "formalisms. . . attach to nature at the top," without relying on either a basic vocabulary or associated theoretical terms. In the case of Newton's Second law, for example, one cannot apply the terms of the generalization $f = ma$ directly to the concrete situations presented by free fall, the pendulum or the gyroscope, but each situation requires its own, specialized formalization before standard tools of logic or mathematics can be applied effectively. Kuhn proposed that the development of these specialized formalizations requires empirical content that cannot be provided by a symbolic generalization, such as $f=ma$.

How can formalisms attach to nature "at the top?" From Kuhn's comments, it seems that empirical content is introduced through the (as-yet-unspecified) way in which the special version of a symbolic generalization is developed. Yet Kuhn proposed that this empirical content need not be direct exposure to the specific (empirical) situation in question. Instead, he suggested that the knowledge required to develop the specialized generalization can be provided by studying (relevant) exemplars. Over time, a student who studies these exemplars develops an "acquired perception of analogy" (ST, 307). By extending this analogy to the new situation, the student may develop a special version of the generalization, with terms that ostensibly attach to nature "at the top" (Ibid.). The student thus may learn by doing problems (rather than applying theory), yet this does not necessarily suggest that exemplars may function *prior* to the establishment of theory. This central part of Kuhn's argument thus has yet to be proven.

The Knowledge of Nature Provided by Exemplars

To understand the role of exemplars in the development of scientific knowledge, Kuhn examined the processes involved in correlating symbolic expressions with concrete problem-situations:

> Students of physics regularly report that they have read through a chapter of their text, understood it perfectly, but nonetheless had difficulty solving the problems at the end of the chapter. Almost invariably their difficulty is in setting up the appropriate equations, in relating the words and examples given in the text to the particular problems they are asked to solve. Ordinarily, also, those difficulties dissolve in the same way. The student discovers a way to see his problem as like a problem he has already encountered. Once that likeness or analogy has been seen, only manipulative difficulties remain. (ST, 305)

Students' difficulties in doing problems suggest that applying the theory learned in chapter texts to specific problem situations requires something in addition to comprehension of the material. Kuhn suggested that, in resolving this difficulty, students typically discover an analogy or likeness between the problem at hand and one previously encountered (and resolved). Specifically, the students learn how to link the current problem to an exemplary problem-situation that they have already encountered. Having established this relationship, they then must relate the various elements of the two problems; formalize the new problem; and resolve it in a way that is analogous to the formulation and the resolution presented by the exemplar. These processes yield knowledge of the complex interrelation of facts in a way that links their identification with explanation of their behavior. Through this interrelation the exemplar yields knowledge of nature that is both descriptive and normative, although the authority and means of acquiring this knowledge have yet to be explained.

In the case of Newton's Second law, students first must see the situations of free fall, the simple pendulum, coupled harmonic oscillators, or the gyroscope as analogous to each other or to a similarly relevant, known exemplar.[150] They then must relate the concrete elements of the two problem-situations and develop the formalization that is appropriate for resolving the situation at hand. What is most important, from Kuhn's perspective, is that the special version is not developed either from a generalized

[150] This initial identification of similarity provides legislative guidance about the structure of the solution but does not (as yet) provide definitional determination of the elements involved.

theory or a basic vocabulary. Rather, it is developed from the application of a concrete, exemplary problem situation that is deemed similar and thus relevant to the resolution of the current problem.[151]

On this account, the knowledge required for a student to solve a newly encountered problem is provided not only by comprehension of the theory or generalization outlined in the chapter text but also by the discovery of an analogous, concrete exemplar on which to model the problem and its solution. Kuhn noted that similar processes underlie the research and discoveries of practicing scientists, who "model one problem solution on another, often with only a minimal recourse to symbolic generalizations" (ST, 305) For example, Huygens' calculation of the center of oscillation of a physical pendulum and Bernoulli's discovery of the speed of efflux were each modeled on Galileo's incline experiments (and for Bernoulli, also on Huygen's calculation) without direct reference to Newton's laws of motion.

Kuhn concluded that the group-licensed resemblances that are learned through the study of exemplars provide an alternative to correspondence rules in guiding the puzzle-solving activities of science:

> . . . I suggest that an acquired ability to see resemblances between apparently disparate problems plays in the sciences a significant part of the role usually attributed to correspondence rules. Once a new problem is seen to be analogous to a problem previously solved, both an appropriate formalism and a new way of attaching its symbolic consequences to nature follow. Having seen the resemblance, one simply uses the attachments that have proved effective before. That ability to recognize group-licensed resemblances is, I think, the main thing students acquire by doing problems, whether with pencil and paper or in a well-designed laboratory. (ST, 306)

By doing exemplary problems, then, a student gains the ability to recognize "group-licensed resemblances" among various problem-situations. This recognition of resemblance, in turn, provides a basis from which the student can develop the appropriate specialized formalization. The "resemblance" thus extends throughout the entirety of the problem-situation and ultimately translates into a "way of seeing" similar situations.[152]

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[151] Note here that the analogy is established first with respect to the problem-situation and then is extended to the elements of that situation and the resolution of its respective problems. In this way, it connects to existing knowledge "at the top" rather than "at the bottom," through the correspondence to a basic vocabulary.

[152] Exemplars thus provide something akin to ostension, pointing out the relationship between a set of elements within a particular concrete situation and suggesting the way in which the problem-situation may be resolved. Empirical content enters "at the top," that is, through the perceived similarity of a concrete problem-situation and an exemplary puzzle solution. Yet exemplars can be effective in such situations only to the extent that they are viewed by students or scientists *as* exemplars. That is, they must function as a means of conveying to students or to the members of a community – in concrete terms – the elements of the problem situation and how they are related in a puzzle solution. By doing exemplars, students learn to recognize what the community already knows about both the nature and the behavior of the phenomena they would investigate.

Through the study of exemplars, Kuhn proposed that students learn how their community defines and resolves the concrete problem-situations that it typically encounters and investigates:

> Acquiring an arsenal of exemplars, just as much as learning symbolic generalizations, is integral to the process by which a student gains access to the cognitive achievements of his disciplinary group. Without exemplars, he would never learn much of what the group knows about such fundamental concepts as force and field, element and compound, or nucleus and cell. (ST, 307)

By assimilating the (set of) exemplars that are licensed by the community, a student develops a "learned similarity relationship, an acquired perception of analogy" that reflects much of the community's knowledge of nature (Ibid.). By studying multiple exemplars along with their associated generalizations, she develops knowledge of the group's "fundamental concepts," such as force, field, element, etc., as they are applied across a range of concrete situations. This knowledge will allow her to identify and to resolve similar problem situations.

The Study of Exemplars and Primitive Similarity Resemblance

Kuhn insisted that the recognition of resemblance between two situations (or between a situation and an exemplar) is "a learned but nonetheless primitive perception of similarity and difference" (ST, 312). He proposed that it involves the perception of similarity among concrete problem-situations, rather than an identification of correspondence between a generalized theory and observations of nature. As such, it centers on identifying the relationships of similarity and difference that exist between two concrete problem situations, such that the situations are seen as similar in both their structure and their solution.[153] Kuhn proposed that no intervening theory or generalization is required for this perception of similarity, nor must any correspondence be posited between the observations and a generalized theory or criteria for solution.

According to Kuhn, studying exemplars and resolving the problems that they present provides a student with knowledge of nature because "while acquiring the perception [of similarity/dissimilarity], he has learned something about nature." (Ibid.). More specifically, by examining two problem-situations that are presented as analogous, the student learns to discern (selected) similarities and differences among their concrete elements and to develop an appropriate formalization that both can identify and can resolve the

[153] The existence of some form of similarity and difference is implicit in the juxtaposition of multiple exemplars.

new problem-situation.[154] The identification and resolution of the specialized formalism will be similar to those of the exemplary situation, yet it also will be specialized with respect to the distinctive aspects of that (new) situation.

Kuhn proposed that we recognize an alternative cognitive process by which knowledge of nature is developed by specialist communities. Through the study and practice of exemplary problems, students and scientists gain knowledge of their community's fundamental concepts of nature. It is this (specialized) knowledge, embedded in the similarities and differences that are perceived and established through the study of exemplars, rather than the rules and criteria of generalized laws or theories, that accounts for much of a group's "unproblematic conduct of research" (ST, 318). Thus it is in this way that the members of a community come to agree on the fundamental categories of nature and the way that they may be understood through scientific terminology and generalizations.

Kuhn noted that once a specialized version of a symbolic generalization is developed, philosophers may succeed in identifying a set of correspondence rules or criteria that seem to have been operative in its development and that determine its application. He cautioned, however, that while these attributed correspondence rules may explain past behavior, they will not necessarily be able to account for future behavior in newly encountered situations. He added that to consider correspondence rules as the basis for the knowledge of nature that is, at least initially, gained through group-licensed resemblances is to obscure not only the source of that knowledge but also its nature:

> The philosopher is at liberty to substitute rules for examples and, at least in principle, he can expect to succeed in doing so. In the process, however, he will alter the nature of the knowledge possessed by the community from which his examples were drawn. What he will be doing, in effect, is to substitute one means of data processing for another. Unless he is extraordinarily careful he will weaken the community's cognition by doing so. Even with care, he will change the nature of the community's future responses to some experimental stimuli. (ST, 314)

A community's knowledge of nature thus is derived not only from the application of its generalizations but also from the study of concrete problem-solutions that it identifies as concrete exemplars of that knowledge. This knowledge is not embedded in generalizations of the cognitive content of science but in specialized exemplars of that content. The nature of this knowledge thus is the identification not of correspondence to "what is really out there" but of a primitive similarity relationship posited among

[154] It will be important to consider the way in which the "knowledge of nature" gained from the study of exemplars (i.e., the ability to discern similarities and differences and to develop specialized formalisms) varies from the knowledge acquired through the application of a theory.

concrete situations. To rely solely on correspondence rather than this primitive similarity is to limit the source of a community's cognitive content and thus to restrain its cognition. This limitation is of particular concern with respect to the community's future response to newly encountered situations.

The 1969 *Postscript*: A Community-Based "Way of Seeing"

Kuhn expanded his discussion of exemplars in the 1969 Postscript to the second edition of *Structure*, describing them as "concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (*SSR*-PS 1969/1970a, 175). As indicated by this description, he heightened the importance of exemplars vis-à-vis the discussion in "Second Thoughts," suggesting that exemplars are not simply an alternative to rules but actually replace them. They do so, moreover, not only with respect to the knowledge of nature that is required for applying generalizations to concrete situations, but even further, as the basis for resolving the puzzles of normal science.

According to Kuhn, exemplars can be found not only in the textbook problem-solutions encountered by students but also in the technical problem-solutions of the periodical literature read by practicing scientists. Within the context of scientific practice, exemplars "show [scientists] by example how their job is to be done" (*SSR*-PS, 187). Repeating much of his earlier discussions in "Second Thoughts," Kuhn noted that although exemplars receive little attention from philosophers of science, "at the start and for some time after, doing problems is learning consequential things about nature" (*SSR*-PS, 188).

Kuhn claimed that in developing a specialized formalization, "[t]he law-sketch, say $f = ma$, has functioned as a tool, informing the student what similarities to look for, signaling the (legislative) gestalt in which the situation is to be seen" (*SSR*-PS, 189). On this account, symbolic generalizations function as tools or as signals indicating the proper context ("gestalt") for scientific investigations. By doing exemplary problem-solutions, a student gains the "ability to see a variety of situations as like each other, as subjects for $f = ma$ or some other symbolic generalization" (Ibid.). Kuhn proposed that these acquired similarity relations inform the way that the student views the situations that he encounters and thereby form the basis of his acceptance into the specialist community:

> After he has completed a certain number, . . . he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. For him they are no longer the same situations he had encountered when his training began. He has meanwhile assimilated a time-tested and group-licensed way of seeing. (Ibid.)

Thus what is important is not only the acquired similarity relationships – the ability to see different situations as similar to each other – but also the fact that this ability can be extended systematically to new situations not previously encountered. Kuhn concluded that this "way of seeing" represents consequential knowledge of nature, which is "acquired while learning the similarity relationship and thereafter embodied in a way of viewing physical situations rather than in rules and laws" (*SSR*-PS, 190-1).

Generalizations such as Newton's Second law gain meaning for students only when they learn to recognize force, mass, etc., "as ingredients of nature, and that is to learn something, prior to the law, about the situations that nature does and does not present" (*SSR*-PS, 191). This way of seeing is the ability to discern distinct "situations" within nature as similar to those previously encountered. Having gained this knowledge, students can then discern new problem-situations as soluble within a Newtonian "gestalt" and can develop the appropriate (specialized) formalization. In this way, exemplars actually "replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (*SSR*-PS, 175).

Kuhn proposed that the knowledge of nature that results from considering problem-situations "comes as one is given words together with concrete examples of how they function in use; nature and words are learned together" (*SSR*-PS, 191).[155] He insisted that this knowledge cannot be developed through the study of generalizations alone nor by exclusively verbal means. Instead, it is "'tacit knowledge,' which is learned by doing science rather than by acquiring rules for doing it" (Ibid.). Kuhn insisted that knowledge gained in this way is not simply the result of individual intuition. To the contrary, it is "the tested and shared possessio[n] of the members of a successful group" and is "not in principle unanalyzable," although it carries its own implicit means of evaluation or analysis (Ibid.). This knowledge may be programmed through education, that is, through the study of exemplars.[156]

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[155] In this reference, we see the operation of dual-function generalizations, although Kuhn did not specify it as such, nor did he distinguish it from other types of generalizations.

[156] As we will see, Kuhn gradually deemphasized the psychological elements of his discussions, focusing instead on the knowledge that is embedded in language, in particular the language-nature link. It is also

Kuhn proposed that the exemplars that are passed from one generation to the next constitute an extensive legacy of "experience and knowledge of nature embedded in the stimulus-to-sensation route" (*SSR*-PS, 196). In order to be passed along within a community (and in order for the community itself to survive over time), these exemplars must withstand the tests and standards that are presented by its individual and collective activities and experiences:

> To say that the members of different groups may have different perceptions when confronted with the same stimuli is not to imply that they may have just any perceptions at all. In many environments a group that could not tell wolves from dogs could not endure. Nor would a group of nuclear physicists today survive as scientists if unable to recognize the tracks of alpha particles and electrons. It is just because so very few ways of seeing will do that the ones that have withstood the tests of group use are worth transmitting from generation to generation. Equally, it is because they have been selected for their success over historic time that we must speak of *the experience and knowledge of nature* embedded in the stimulus-to-sensation route. (*SSR*-PS, 195-6, emphasis added).

The "success" of a community thus depends upon the effectiveness of its exemplars in directing its activities and research over time. Only those ways of seeing that have "withstood the tests of group use" and that "have been selected for their success over historic time" will be passed along to future generations.

The ways of seeing that are passed along (and the exemplars that embody them) are thus time-tested by members of the scientific community. To the extent that they comprehend the full range of a community's past experience, they will be systematic. Yet they also will be corrigible, in that they may prove to be unsuccessful with respect to future experiences of the community. In the language of *Structure*, anomalies may arise that draw into question the community's long-standing way of seeing. The authority of a community's way of seeing and its associated set of exemplars thus derives from past experience and past success. Yet these past successes do not guarantee future success when encountering new situations.

Scientific Revolutions as Changes in Exemplars

According to Kuhn, a revolution occurs within a particular community when its members' shared "way of seeing" undergoes a fundamental change. He now proposed that the underlying source of this change lies in the group's most primitive similarity relations:

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important to note that the "education" through which the integrity of perception may be programmed is exposure to exemplars, rather than the learning of rules. We must thus understand the distinctive aspects of this (alternative) type of education, which is characterized most directly by the study of exemplars.

> The practice of normal science depends on the ability, acquired from exemplars, to group objects and situations into similarity sets which are primitive in the sense that the grouping is done without an answer to the question, "Similar with respect to what?" One central aspect of any revolution is, then, that some of the similarity relations change. Objects that were grouped in the same set before are grouped in different ones afterward and vice versa. Think of the sun, moon, Mars, and earth before and after Copernicus; of free fall, pendular, and planetary motion before and after Galileo; or of salts, alloys, and a sulphur-iron filing mix before and after Dalton. Since most objects within even the altered sets continue to be grouped together, the names of the sets are usually preserved. Nevertheless, the transfer of a subset is ordinarily part of a critical change in the network of interrelations among them. (*SSR*-PS, 200)

Revolutions thus represent a change in the primitive similarity sets into which the members of a community group the objects examined in their observations and investigations. Although the names of these sets are often preserved, the particular phenomena that are included within them change. What results is a consequential change in the community's knowledge of nature, specifically, in the way in which selected scientific terms and phenomena are connected.

The change in knowledge that occurs with revolution thus is a change in the language-nature link that is not simply linguistic. In fact, if the names of the sets are preserved, then the change may not be evident in language at all. For example, before Copernicus, the sun, moon, Mars and earth were all considered to be planets; however, after Copernicus, the term "planet" – now defined as an object rotating around a sun – could only be applied to Mars and earth. The change occurred with respect to the language-nature link and was functional rather than linguistic. Nonetheless, it reflected a consequential (and in some cases, even a revolutionary) shift in the scientific community's knowledge of nature.

"Reflections:" Experience and Knowledge Embedded in Language

In "Reflections on My Critics," the last written of the late 1960s essays, Kuhn responded to the papers and presentations of the 1965 International Colloquium with Karl Popper and his colleagues in the philosophy of science. The ideas, clarifications and refinements that he presented in this essay were more developed than those of the two preceding essays and reflected his heightened concern with issues in both philosophy and philosophy of science. Most notably, Kuhn's preference in *Structure* for visual metaphors in characterizing the development of scientific knowledge – a "way of seeing," the habitation of "different worlds" and the "gestalt switch" accompanying scientific revolutions – was replaced by detailed consideration of the operation and limitations of language. Similarly, he deemphasized much of his earlier account of the neural processes involved in the movement from stimulus to sensation and paid greater attention to the language-nature link. These moves, in turn, reflected a shift in Kuhn's focus from the psychological and sociological aspects of scientific development to the underlying, philosophical implications of *Structure*.

In considering the role of language in the development of scientific knowledge, Kuhn repeated his earlier arguments against the positivist proposal that a basic vocabulary attaches to nature in ways that are unproblematic. He noted that, historically, scientific terms have often changed their meaning or the conditions of their applicability in subtle ways, yet in doing so, have changed in the way that they attach to nature. To understand the knowledge of nature that is embedded in the language-nature link, Kuhn proposed that we must look beyond positivists' reliance on definitions, words, or sentences:[157]

> These procedures for language-nature learning are . . . purely linguistic. They relate words to other words and thus can function only if we already possess some vocabulary acquired by a non-verbal or incompletely verbal process. Presumably, that part of learning is by ostension or some elaboration of it, the direct matching of whole words or phrases to nature. (RMC 1969/1970c, 270)

Expanding on his earlier proposals, Kuhn reiterated his claim that the knowledge of nature gained in this way is distinct from the (solely linguistic) knowledge provided by generalizations, laws or criteria:

> When I speak of knowledge embedded in terms and phrases learned by some non-linguistic process like ostension, I am making the same point that my book aimed to make by repeated reference to the role of paradigms as concrete problem solutions, the exemplary objects of an ostension. When I speak of that knowledge as consequential for science and for theory-construction, I am identifying what Miss Masterman underscores about paradigms by saying that they 'can function when the theory is not there.' (RMC, 271)

What Kuhn called "language-nature learning" thus cannot be accomplished by linguistic methods alone, but requires a non-linguistic process like ostension. Yet as concrete problem-solutions, exemplars not only augment the knowledge provided by scientific theories but can "function" even when the theory is not there.

[157] Kuhn here referenced Carnap's work showing that by encountering words in a variety of sentences "we acquire laws of nature together with a knowledge of meanings" (RMC 1969/1970c, 270). As we will discuss, the work of Sneed and Stegmüller suggested (in line with Kuhn's proposal and in contrast to the presumed sufficiency posited by Carnap) that the initial development of a theory requires multiple examples.

The Function of Exemplars as Scientific Puzzle Solutions

As in his earlier accounts, Kuhn proposed that doing exemplary problems provides knowledge of nature, now describing the process as "learning the language of a theory and acquiring the knowledge of nature embedded in that language" (RMC, 272). He also extended his earlier discussions by agreeing with Margaret Masterman's characterization of a paradigm as "fundamentally an artefact which transforms problems to puzzles and enables them to be solved even in the absence of an adequate body of theory" (RMC, 273). In the two preceding essays, he had proposed first, that exemplars provide an alternative path to gaining knowledge of nature and then, that they replace laws as the basis for solving the remaining problems of normal science. The assertion here is more specific: the study of exemplars transforms problems to puzzles. That is, they not only present the problem at hand but also provide the tools that make possible its solution.

Acquiring the exemplars that are licensed by a community yields two types of capabilities. The first is the ability to recognize and to resolve the particular exemplars that are taught by the community. For students of Newtonian physics, this may include Galileo's incline experiment for situations of free fall, Huygens' pendulum experiments, etc. By doing these experimental puzzle solutions, students learn to identify the force, mass, and acceleration for the particular situations and to specify the required special version of Newton's Second law. Secondly, and perhaps even more importantly, by doing a number of such problems, students acquire a way of seeing the world in terms of Newtonian physics. That is, they learn (through an as-yet-unthematized process) to identify force, mass, or acceleration for situations that they (and perhaps other members of their community) have not encountered previously. This second capability thus extends beyond the particular exemplars that are shared by the members of a community to a way of understanding new situations and identifying new exemplars that is, at least tacitly, reflective of a group-licensed way of seeing.

Kuhn proposed that the transformation from problems to puzzles is accomplished by the dual function of textbook problem-solutions: by contemplating exemplary problem-solutions, students learn both how scientific terms attach to nature and how the world behaves. That is, they learn not only how particular terms function within nature but also, by extension, how nature itself behaves. This "dual function" of exemplary problem-solutions is strikingly similar to Kuhn's earlier consideration of the dual (legislative-definitional) function of generalizations such as Newton's Second law.[158] Given that exemplars are required to provide (these types of) generalizations with their required empirical content, their dual function, taught by the study of exemplars, seems to provide the basis for the source of student's ability to transform problems to puzzles.

While not articulated by Kuhn, it also seems that the cognitive content that is developed through the study of exemplars (yet unavailable from theories or laws) is the content embedded in the dual-function of textbook problems or dual-function generalizations. To the extent that these specialized generalizations *require* empirical observation, exemplars may join, or perhaps even replace, theories as the basis for solving puzzles.

[158] As proposed above, the duality of definition and legislation is a crucial aspect of Kuhn's theory that was inadequately thematized. Kuhn here introduced what remained implicit in the two earlier essays; however, he mentioned duality only briefly and described it as the "role" of textbook problems. The recognized importance of this duality and its relation to certain types of symbolic generalizations emerged only gradually in Kuhn's work, as will be outlined in subsequent pages.

Chapter Five

Exemplars, Incommensurability and Lexical Structure

Following the refinement of *Structure's* central concepts in the late-1960s and early-1970s, Kuhn still was not prepared to address the philosophical implications of the work. He recalled the situation in a later interview, providing insight into the objectives of his investigations, the frustrations that he encountered, and the path that he began to follow:

> There was an increasing influence of a different approach to historical examples among philosophers of science gradually. . . . I don't think that the people [in philosophy] who were doing history, by and large, saw everything in it that I was seeing in it. They were not coming back asking "What does this do to the notion of truth, what does it do to the notion of progress," or if they did they were finding it too easy to find answers that seemed to me superficial. It isn't that I knew the answers, but I didn't think that theirs were ones that were going to withstand the scrutiny that they needed. I was worrying about it, I mean I was back to writing history for a change [i.e., the book on Max Planck and black-body theory, (Kuhn 1978)[159]]. But I wanted nothing more than to get back and straighten out these problems – and I really didn't know how to do it – and I kept saying, it's like going around on a stage set, opening doors, to see which ones have just a painted canvas behind them and which one will lead into another room. Well, I gradually found some that led into another room, or part way into another room: the causal theory of reference. (Baltas et al. 1995/2000, 311-2)

In the causal theory of reference and its approach to the problem of meaning change, Kuhn perceived a way to address the philosophical issues and implications raised by his Aristotle experience and his historiographic studies of scientific revolutions. In particular, he was interested in the proposal that a tag or label may be attached to a referent and later used for purposes of identification by tracing the lifeline back to the (historical) point of dubbing. This attachment of a proper name with its referent (rather than the traditional approach of identification through definitive description) seemed to parallel Kuhn's emphasis on the mechanism that links language to nature and operates without the need for "defining" theories, laws, or rules.[160]

Beginning in the mid-1970s, Kuhn recommenced his earlier investigation of the philosophical implications of his new image of science. He focused his attention on the language-nature link, proposing that it is a central mechanism for the establishment of knowledge that typically is neglected in the philosophy of science. Prompted by the causal theory of meaning and his acquaintance with Carl G. Hempel, he began to examine the issues of his earlier research using analytic tools in philosophy and

[159] Kuhn began research for this book immediately following the publication of *Structure*.
[160] Yet as we will see, extending the causal theory of reference from proper names to the (kind) terms used in science raises a number of important considerations.

philosophy of language. These studies continued throughout the remainder of Kuhn's career, leading him to consider the philosophical problems associated with meaning change, language change and possible worlds, and culminating in his theory of the lexicon.

Kuhn's philosophical studies formed the basis of a book on incommensurability, which he worked on from the early 1980s until his death in 1996. Although the book was never completed,[161] a series of published articles[162] outlined several of its major propositions and indicated the development of Kuhn's thought during the period. Characterizing the book as a return to the philosophical problems left over from *Structure*, Kuhn noted that while it addressed issues of rationality, relativism, realism and truth, it was primarily about incommensurability:

> No other aspect of *Structure* has concerned me so deeply in the thirty years since the book was written, and I emerge from those years feeling more strongly than ever that incommensurability has to be an essential component of any historical, developmental, or evolutionary view of scientific knowledge. Properly understood – something I've by no means always managed myself – incommensurability is far from being the threat to rational evaluation of truth claims that it has frequently seemed. Rather, it's what is needed, within a developmental perspective, to restore some badly needed bite to the whole notion of cognitive evaluation. It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of postmodernist movements like the strong program. (RSS 1990/2000, 91)

On this (rather surprising) account, incommensurability is not a "problem" for the rationality of science but is the way to combat the challenges that arise from an "historical, developmental, or evolutionary view of scientific knowledge." The theory associated with incommensurability does not support postmodernist movements like the strong program (e.g., self-named "Kuhnians") but provides a way to defend truth and knowledge from their excesses.

These final attempts to address the philosophical issues emerging from the theory of *Structure* within the context of the theory of meaning led Kuhn into a complicated thicket of problems in the philosophy of science and the philosophy of language. In doing so, it brought him face-to-face with the theories developed by Quine, Putnam, Davidson, and others. Kuhn thus not only had to defend his views

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[161] Early drafts of the book were based on three important lecture series, which remain unpublished. This material is currently being edited and published by James Conant and John Haugeland, who edited the collection of Kuhn's later publications, *The Road Since Structure* (2000). The lecture series included "The Natures of Conceptual Change" (Perspectives in the Philosophy of Science, University of Notre Dame, 1980), "Scientific Development and Lexical Change" (The Thalheimer lectures, Johns Hopkins University, 1984), and "The Presence of Past Science" (the Shearman lectures, University College, London, 1987).

[162] References to the book are made in "Possible Worlds in the History of Science" (PW 1986/2000); "Afterwords" (AW 1990/2000); "The Road Since Structure" (RSS 1990/2000); "Dubbing and Redubbing: The Vulnerability of Rigid Designation" (DR 1990); and "Remarks on Incommensurability and Translation" (RIT 1999).

from their challenges but also to develop his own views in relation to their proposals. The difficulties typically associated with such efforts were compounded by a lack of technical training in philosophy, the philosophy of science, or the philosophy of language. These limitations manifest themselves in a tendency toward generalization, a failure to provide detailed argumentation, and frequent neglect of the implications of one line of investigation for other, related aspects of his developing theory.

To some extent, however, the limitations of any single article are addressed by considering the series of articles published by Kuhn from the early 1980s until his death in 1996. While the articles span a wide range of concerns, collectively they reflect a gradual convergence around a number of issues and ideas. They also reflect Kuhn's gradual development of both his views and his knowledge of the more technical, philosophical aspects of the issues with which he was engaged. Thus rather than evaluating each article on the basis of its individual merits or demerits with respect to clarity, argumentation, and implications (although these will be noted, as appropriate), the following pages will attempt to discern and to evaluate the development of Kuhn's views over time.

Section One

The Philosophical Basis and Authority of Exemplars

The three essays that Kuhn authored in the late-1960s provided a rather schematic account of the function of exemplars in acquiring and extending scientific knowledge; however, they did not address the philosophical basis and authority of exemplars or the knowledge that they seemed to provide. Even if students do, in fact, acquire (extensible) knowledge of nature by doing exemplary problems, it is important to note that they are presented with those problems (either by their teachers or by their texts) *as* exemplars. We thus must examine not only the (pedagogical) function of exemplars but also the processes involved in their development and refinement over time. Understanding these processes is especially important if we are to understand the processes underlying scientific discovery and revolution.

In some cases, Kuhn proposed that these exemplars are presented as exemplars *of* a particular theory. His statements also implied that students may learn to discern primitive similarity relationships without the introduction of a theory (i.e., the exemplars may be juxtaposed presented and learned by the student as a set of *similar* problem-solutions). In his early discussions of exemplars, Kuhn proposed that their philosophical basis and authority lies in the primitive similarity resemblance that can be discerned from the study of multiple exemplars. Yet we must still understand the philosophical and investigative processes whereby that resemblance may be discerned and how it may form the basis of a scientific theory. That is, we must understand the relationship between exemplars and the theories that they exemplify.

Even if exemplars can be established independently of scientific theory, we must still understand their authority *as* exemplars. That is (as Margaret Masterman proposed), we must understand not only how they may be *established as a concrete construct* but also how they may be *extended as an analogy* in a way that supports puzzle-solving (and thereby provides knowledge of nature). The first question raises issues regarding the relationship between exemplars and theories, along with the philosophical problems associated with identification of theoretical terms. In considering the second question, we are immediately confronted with the skepticism of David Hume and long-standing philosophical concerns regarding the (epistemological) limitations of empirical observation. We are also confronted once again by the challenges of philosophers of science regarding the (possible) irrationality, relativism, and mob psychology inherent in Kuhn's proposed image of the nature of science.

In order to provide an adequate *philosophical* account of Kuhn's *schematic* account of the nature of science and the function of exemplars, we thus must confront issues that are central not only to the philosophy of science but also to philosophy itself. In doing so, we must consider not only Kuhn's new image of the nature of science but also the (alternative) image of the nature of knowledge that his work would seem to suggest.

The Relationship of Theories and Exemplars

In 1976, Kuhn published "Theory Change as Structure Change: Comments on the Sneed Formalism" (TCSC 1976/2000, 176-195), which examined the work of J. W. Sneed on the relationship between a theory and its applications.[163] Sneed's work differed from traditional formalisms in that it analyzed the logical structure of scientific theories by applying set theory, rather than relying on identification of individual theoretical terms. Kuhn proposed that through its set-theoretic approach, the new formalism "captures significant features of scientific theory and practice notably absent from the earlier formalizations known to me" (TCSC, 178). In particular, he cited Sneed's frequent consideration of how theories are presented to students and used by them, noting that this eliminated "artificialities that have in the past often made philosophical formalisms seem irrelevant to both practitioners and historians of science" (Ibid.).[164]

Sneed identified three classes of models to which a theory may be applied. The first two classes included potential partial models (or M_{pp}'s) and models (M's). As in traditional formalisms, potential partial models represented "the entities to which a given theory might be applied by virtue of their description in the nontheoretical vocabulary of the theory," while models were "the subset of M_{pp}'s to which, after suitable theoretical extension, the laws of the theory actually do apply" (Ibid.). Sneed also introduced a third class of models, which typically were not posited in traditional formalisms. These were partial models (M_p's), "the set of models obtained by adding theoretical functions to all the suitable

[163] Kuhn also considered a number of proposals made by Wolfgang Stegmüller, who had attempted to identify the implications of Sneed's work for the theory outlined in *Structure*.
[164] Kuhn's interest in the insights to be gained from Sneed's set-theoretic approach were tempered by the difficulties associated with its relatively "unknown and altogether forbidding language" (TCSC 1976/2000, 176). Nonetheless, he proposed that, "[i]f only simpler and more palatable ways of representing the essentials of Sneed's position can be found, philosophers, practitioners, and historians of science may, for the first time in years, find fruitful channels for interdisciplinary communication" (TCSC, 178).

members of M_{pp}, thus completing or extending them *prior to the application of the theory's fundamental laws*" (Ibid., emphasis added).

By means of a set-theoretic approach, Sneed identified a subset of potential partial models that possessed the functions of a given theory but were not yet fully completed applications of it. This contribution was important because it introduced an intermediate step between the identification of potential partial models (i.e., all entities that could be described in the nontheoretical vocabulary of the theory) and models (i.e., the subset of entities to which the laws of the theory could actually be applied). By giving these partial models "a central place in the reconstruction of theory," Kuhn proposed that Sneed had added "significant verisimilitude to the structures that result" (Ibid.):

> Lacking time for an extended argument, I shall be content here with three assertions. First, teaching a student to make the transition from partial potential models to partial models is a large part of what scientific, or at least physics, education is about. That is what student laboratories and the problems at the ends of chapters of textbooks are for. The familiar student who can solve problems which are stated in equations but cannot produce equations for problems exhibited in the laboratory or stated in words has not begun to acquire this essential talent. Second, almost a corollary, the creative imagination required to find an M_p corresponding to a nonstandard M_{pp} (say, a vibrating membrane or string before these were normal applications of Newtonian mechanics) is among the criteria by which great scientists may sometimes be distinguished from mediocre. Third, failure to pay attention to the manner in which this task is done has for years disguised the nature of the problem presented by the meaning of theoretical terms. (TCSC, 178-9)

These points echo Kuhn's earlier consideration of the specialized formalizations of scientific theories. A student's effort to "produce equations for problems exhibited in the laboratory" is similar to the specialized formalization of a scientific theory. What we must understand, then, is how to account for these processes.

Although Sneed did not specify the general processes for making the transition to a partial model, he examined in detail the case of fully mathematized theories. Just as Kuhn had proposed the juxtaposition of exemplars as a means of developing a specialized formalization, so Sneed used previous applications of the theory as guides for specifying theoretical functions. Unlike Kuhn, however, Sneed provided a specific account of the basis on which this "guidance" might be provided (Kuhn had simply asserted the identification of a primitive similarity resemblance). In what Kuhn identified as "the central conceptual innovation of Sneed's formalism" (TCSC, 179), Sneed proposed that guidance in the transition to partial models is provided by the *constraints* that arise from conjoining multiple applications:

> If a theory, like Newtonian mechanics, had only a single application (for example, the determination of mass ratios for two bodies connected by a spring), then the specification of the theoretical functions it supplies would be literally circular and the application correspondingly vacuous. But, from Sneed's viewpoint, no single application yet constitutes a theory, and, when

> several applications are conjoined, the potential circularity ceases to be vacuous because distributed by constraints over the whole set of applications. As a result, certain other, sometimes nagging, problems change their form or disappear. (TCSC, 181)

On this account, connecting *multiple* applications of a theory not only indicates their points of similarity as applications of a particular theory or law, but also delimits the range of empirical content (or types of situations) to which those various applications of the laws may apply.

Kuhn noted that within this set-theoretic structure, individual applications play a double role that is similar to the role of reduction sentences at a pre-theoretic level:

> . . . when applications are tied together by constraints, as reduction sentences are tied together by the recurrence of a theoretical term, they prove capable simultaneously of specifying, on the one hand, the *manner* in which theoretical concepts or terms must be applied and, on the other, some *empirical content* of the theory itself. (TCSC, 180).

Within the context of a set-theory, the conjoining of multiple applications thus plays a role that is similar to the role of reduction sentences before a theory is established. Furthermore, at least some of[165] the conventional elements present in individual applications would be eliminated by the constraints imposed by their combination. This would seem to address the philosophical problems typically associated with the processes of introducing theoretical terms (i.e., the basis on which conventional and empirical elements – in Sneed's terms, theoretical and nontheoretical elements – may be distinguished).

Kuhn insisted that Sneed's "new formalism" highlighted the ways in which "the adequate specification of a theory must include specification of some set of exemplary applications" (TCSC, 179). Noting that, "roughly speaking," Sneed represented a theory as "a set of distinct applications" (TCSC, 180),[166] Kuhn emphasized "that learning a theory is learning successive applications in some appropriate order and that using it is designing still others" (Ibid.).[167]

Kuhn also applauded the extent to which Sneed's unique approach to formalization "lends itself to the reconstruction of theory dynamics, the process by which theories change and grow" (TCSC, 181). In particular, he noted Sneed's identification of two distinct types of theory change: those in which the "theory core" (and at least some exemplary applications) remains fixed and those involving a change in the "core"

[165] Although not stated by Kuhn, it would seem that the degree to which conventional elements are eliminated will depend upon both the number and the character of the applications that are conjoined.
[166] Kuhn's characterization of Sneed's "rough" representation of theories is not without some justification; however, it is important to note that Sneed followed the formalist view that a theory is initially developed (albeit in preliminary form) before its various applications.
[167] As in the considerations above, this statement concerns the *acquisition and extension* of a theory but does not address its *development*.

itself. Yet in agreeing with this general characterization of theory dynamics, Kuhn noted that "[t]hough the Sneed formalism does permit the existence of revolutions, it does virtually nothing to clarify the nature of revolutionary change" (TCSC, 182).

Returning to the notion of constraints as a means of completing scientific theories, Kuhn proposed that constraints might play an even more fundamental role than Sneed's new formalism suggested. Rather than following his process of selecting a theory on the basis of strict identity criteria; distinguishing nontheoretical and theoretical functions; and then introducing constraints to allow the specification of theoretical functions, Kuhn proposed that this process might be inverted:

> Could one not, that is, introduce applications and constraints between them as primitive notions, allowing subsequent investigation to reveal the extent to which criteria for theory identity and for a theoretical/nontheoretical distinction would follow? (TCSC, 183)

Kuhn's (by now, characteristic[168]) inversion thus established the introduction of multiple applications (along with their associated constraints) as the basis for establishing the structure, criteria, and distinctions of a theory. In doing so, it eliminated traditional reliance on the theory and, in doing so, dissolved a number of philosophical problems associated with the identification (or elimination) of theoretical terms. On the other hand, it raised the challenge (outlined by Masterman) of how to identify the various applications that are to be examined and how to justify them as a group of (similar) applications.

In Sneed's "new formalism" we thus find a way of considering in more detail the relationship between traditional formalisms and the function of exemplars or applications in establishing scientific knowledge. Like Kuhn, Sneed examined the processes and activities of science, emphasizing the functional aspects of theories that are revealed by consideration of their various applications. His notion of constraints provided an important insight regarding the dual-function of applications (or exemplars) and their role in full identification of scientific theories.

[168] As shown in our investigations, Kuhn's work was often characterized by an inversion of established positions. These inversions typically were associated with Kuhn's attempt to make a text make sense and frequently involved close (historiographic) examination of the activities underlying an achievement in order to discern its historical integrity. In his Aristotle experience, Kuhn tried to reconcile the Greek's reputation as a detailed observer with the errors and "bad science" that (a Newtonian reading of) his text seemed to suggest. In "The Function of Measurement," Kuhn examined the role of measurement by examining the way that it was used in scientific activities, as indicated by the scientific journals and writings of the time. Finally, in considering these various inversions it is also important to note Margaret Masterman's theoretical inversions of the positions taken by philosophers of science.

Yet Sneed's account remained a *formalist* account, in which theories provided the basis for conjoining multiple applications. On the basis of the empirical content provided by constraints, Kuhn proposed to invert the order of the processes involved in the development of theories, identifying applications and constraints (rather than theories) as the primitive basis of theory development. Unfortunately, this proposed inversion remained a rather vague and even hypothetical assertion and failed to address the questions of identity and justification that it raised. Our initial question of how an exemplar may be established as a concrete construct, that is, how multiple exemplars may be introduced as primitive notions, thus remains unanswered.

Exemplars and the Metaphor-like Process in Science

At a 1977 conference entitled "Metaphor and Thought," Kuhn presented "Metaphor in Science" (MS 1977/2000, 196-207) a commentary on a paper by Richard Boyd.[169] The paper examined the causal theory of reference as the basis for positing a "central metaphorlike process in science" whereby scientific terms are introduced, learned, and deployed (MS, 196). Still trying to develop a philosophical account of the knowledge provided by the study of exemplars, Kuhn suggested that the causal theory of reference provided important insights about how the language-nature link is established. Yet as we will see, determining the referents of scientific theories (i.e., natural kinds) was much more philosophically complex than determining referents for proper names (as outlined in the causal theory).[170]

The causal theory of reference claimed that the referents of proper names need not be identified by definite descriptions, that is, by the distinction of their necessary and contingent attributes from among their various intensional qualities. Instead, reference could be established solely through extensional determination, that is, simply by attaching a tag or label to the individual at a point in time through the process of "dubbing." Once this attachment is established, one can trace the lifeline of the individual back to the point of dubbing and thereby establish reference. The attachment thus was simply a product of

[169] The proceedings of the conference were published as *Metaphor and Thought* (1979), edited by Andrew Ortony.

[170] As we will see, a number of scholars in both philosophy and the philosophy of science attempted to make the transition from proper names to natural kinds; however, their approach in doing so differed from Kuhn's in a way that later proved to be consequential.

history established by convention, with no need for the definition or distinction of necessary or contingent attributes. Yet once established, the referent attached to the lifeline of the individual and thus remained attached to the individual at all points along that (life-)line.

Given the effectiveness of the causal theory in dealing with the problem of determining the referents of proper names, a number of efforts were undertaken to extend it to determination of the referents of natural kinds. Scholars had long struggled with the difficulties associated with distinguishing the appropriate boundaries of a "natural" family or group and were particularly challenged by Wittgenstein's proposal that various (individual) members might share only a "family resemblance." No singular definition could suffice for such determination, thus the causal theory seemed to be particularly promising.

In considering the challenges associated with extending the causal theory to natural kinds, Kuhn pointed out that, "[w]hen one makes the transition from proper names to the names of natural kinds, one loses access to the career line or lifeline which, in the case of proper names, enables one to check the correctness of different applications of the same term" (MS, 199). This loss of access to the range of possible applications was troubling to Kuhn because the generalized terms of scientific theories often required specialized formalizations for their various applications. He thus proposed that the referent be determined with respect to the full range of its various applications, that is, by recognizing that the act of dubbing might be *repeated* in a way that was consequential for determination of the referent. On this account, a referent might be determined by associating that determination with a contemporaneous act of dubbing.[171] This could be done, he proposed, by considering the act of dubbing as an act of ostension, such that one might identify a referent by examining multiple exemplars or applications.

Drawing from his earlier work and the insights provided by Sneed's notion of constraints, Kuhn proposed that the identification of natural kind terms can be made (and even, that it requires) a number of acts of ostension. Refining his earlier statements regarding the primitive similarity resemblance that is discerned through the study of exemplars, Kuhn now asserted that,

> Only through a multiplicity of . . . exposures can the student acquire what other authors in this book (for example, Cohen and Ortony) refer to as the *feature space* and the knowledge of *salience* required to link language to the world. (MS, 200-1)

[171] Translated into the terminology of his earlier work, the act of dubbing establishes an (historically based) attachment or linkage between language and the world.

The feature space and knowledge of salience that are acquired through the juxtaposition of multiple examples allow "the language learner" to "discover the characteristics with respect to which [two exemplars] are alike, the features that render them similar, and which are therefore relevant to the determination of reference" (MS, 201). In this respect, Kuhn proposed, "the juxtaposition of examples calls forth the similarities upon which the function of metaphor or the determination of reference depends" (Ibid.).

In considering this proposal, it is important to examine in more detail the nature of the "similarities" that are "called forth" by the juxtaposition of examples. These "primitive" similarities, it would seem, provide both the feature space and the salience that are required to establish the language-nature link. Kuhn noted that, as with the determination of reference in metaphor, so in the determination of reference for natural kinds, the "end product" is "nothing like a definition" because "[n]o lists of that sort exist" (Ibid.). In response to this long-recognized problem for determining reference, Kuhn proposed, perhaps surprisingly, that "no loss of *functional precision* results:" "[b]oth natural-kind terms and metaphors do just what they should without satisfying the criteria that a traditional empiricist would have required to declare them meaningful" (Ibid.emphasis added). Thus while knowledge of salience is, in itself, incomplete as a definite description of the referent, *when combined with identification of the feature space*, that combination supports determination of the referent with the same functional precision as that envisioned by attempts to establish necessary criteria.

Considering the broader concept of metaphor employed by other scholars at the conference, Kuhn characterized it as "essentially a higher-level version of the process by which ostension enters into the establishment of reference for natural kind terms" (Ibid.). In particular, he insisted that in order for a metaphor to be established, one already must have established the referents that are thereby juxtaposed. More specifically, one must possess the knowledge of salience that is required to highlight both the features (of similarity) that support the juxtaposition in metaphor and the features (of difference) that establish the referents as separate natural families.

On this account, it would seem that even before metaphor, one must first have some sort of ostension that establishes the referents juxtaposed by the metaphor. In considering what sort of ostension might play such a role, Kuhn referred briefly to the "metaphorlike process" involved in establishing a primitive similarity relationship (or, in Sneed's formalism, in establishing constraints) and then followed Richard Boyd in distinguishing between *constitutive* metaphors and those that are merely *exegetical*:

> Because I take it to be both less obvious and more fundamental than metaphor, I have so far emphasized the metaphorlike process which plays an important role in fixing the referents of scientific terms. But, as Boyd quite rightly insists, genuine metaphors (or, more properly, analogies) are also fundamental to science, providing on occasions "an irreplaceable part of the linguistic machinery of a scientific theory," playing a role that is "*constitutive* of the theories they express, rather than merely exegetical." (MS, 202).

In introducing the distinction between constitutive and exegetical metaphors, Kuhn first emphasized that it is important to recognize the subtlety yet importance of the need to determine the metaphorical referents that are to be juxtaposed. Thus in examining the role of metaphor in science, one must either acknowledge the underlying "metaphorlike process" that fixes the needed referents prior to the application of metaphor, or specify that the metaphor in question is *constitutive* and thus establishes its own referents.

The Authority of Exemplars

Kuhn's comments regarding constitutive metaphors provide an important clue regarding how to establish the authority of exemplars. To the extent that genuine or constitutive metaphors (Kuhn says, more properly, analogies[172]) serve as the means by which their referents are established, they establish a linkage between their terms and the associated referents. To the extent that this is the *sole* means by which those referents may be established, those constitutive exemplars, metaphors or analogies would be concrete constructs, (capable of) operating independently of any theory, rule, or law. While they might, once established, be communicated in conjunction with a generalized theory (i.e., for purposes of pedagogy), they would not depend upon that theory for their determination or concreteness.

By definition, these constitutive metaphors (or "constitutive analogies") not only define some of their referents but also serve to legislate the relationship among their various referents (i.e., those that they constitute and those that are antecedently available). By virtue of this analogical relationship, they embody a legislative or prescriptive structure that extends across their defined terms. They thus establish *both*

[172] Despite Kuhn's identification of constitutive metaphors as analogies, it is not clear that analogies are understood to be constitutive. For purposes of clarification, we will retain the adjective and refer to "constitutive" analogies.

descriptive and prescriptive determinations within the construct that they establish, and these elements are integrally interrelated.

These insights suggest how we might resolve at least some of our questions regarding the philosophical basis and authority of exemplars. First, to the extent that an exemplar serves to constitute (i.e., to define) *at least some of* its terms, it would appear to possess at least some basis for identification as a concrete construct. By constituting the definitions of some of[173] the referents that it presents, it will place its various referents in relation to each other (although the processes by which this occurs must still be delineated). Secondly, in order to function as an analogy that transforms problems to puzzles, the metaphorical or analogical relationship that the exemplar establishes among its various referents must have the structure of a puzzle solution. To the extent that this puzzle solution (structure) is established without the aid of theories, laws, or rules, it must be based on (similar) referents that are established by multiple exemplars and, possibly, those that are antecedently available.[174] Thus we must still understand the basis on which both the definitional and legislative functions of exemplars may also be established as concrete.

To the extent that an exemplar functions as a primitive notion, it must do so through the exercise of this dual, legislative and definitional function. What seems to be required, then, is an exemplar that both establishes a puzzle solution and determines (at least some of) the referents of that problem-solution. Similar dual-functions have arisen in several parts of our investigation. First, in outlining the components of the disciplinary matrix, Kuhn highlighted a specialized type of symbolic generalization, which serves both definitional and legislative functions:[175]

> These generalizations look like laws of nature, but their function for group members is not often that alone. Sometimes it is: for example the Joule-Lenz Law, $H = RI^2$. When that law was discovered, community members already knew what H, R, and I stood for, and these generalizations simply told them something about the behavior of heat, current, and resistance that they had not known before. *But more often . . . symbolic generalizations simultaneously serve a second function, one that is ordinarily sharply separated in analyses by philosophers of science. Like $f = ma$ or $I = V/R$, they function in part as laws but also in part as definitions of some of the*

[173] It is not yet clear how many of referents must be established by an analogy in order for it to be considered "constitutive." This will prove to be an important point for future consideration.
[174] While a constitutive exemplar must establish (at least some of) its referents in order to be a concrete construct, it is not necessary that its puzzle solution relies solely on those referents that it establishes. In considering this issue, Kuhn later expressed agreement with Carl Hempel's reconception of the distinction between referents provided by observation and those provided by the theory as those that are antecedently available and those that are not. In this way, Kuhn proposed, Hempel introduced a valuable, developmental perspective into the associated discussions.
[175] Kuhn noted that philosophers tend to clearly distinguish definitional and legislative functions and thus overlook the special class of generalizations with these dual functions.

> *symbols they deploy.* Furthermore, the balance between their inseparable legislative and definitional force shifts over time. (*SSR*-PS 1969/1970a, 183, emphasis added)

In comparing these two types of generalizations, Kuhn noted that while legislative generalizations (which operate as laws) might be revised or changed in a piecemeal fashion, dual-function generalizations (which operate as both laws and definitions) are, to a great extent, tautologies, which must be accepted or rejected in their entirety. Furthermore, Kuhn proposed that such wholesale changes in dual-function generalizations typically serve as the impetus for scientific revolution.

Kuhn also identified a dual function in Sneed's new formalism, attributing it to conjoined applications and noting that a similar dual function occurs in reduction sentences:

> . . . when applications are tied together by constraints, as reduction sentences are tied together by the recurrence of a theoretical term, they prove capable simultaneously of specifying, on the one hand, the *manner* in which theoretical concepts or terms must be applied and, on the other, some *empirical content* of the theory itself. (*SSR*-PS, 180)

In considering Kuhn's attribution of dual functions to conjoined applications, it is important to note his proposed inversion of the processes that Sneed had outlined for theory development. While Sneed's formalism proposed that the first step involves the selection of a theory on the basis of strict identity criteria,[176] Kuhn suggested that one might instead introduce applications and constraints as primitive. Thus while conjoined applications may serve a dual function with or without the prior identification of a theory, to assert that they so independently requires that they be (shown to be) primitive

These various discussions suggest that we must reconsider the relationship between exemplars and theories. To the extent that a dual-function generalization such as Newton's Second law defines (some of) its own terms, it must be learned or discovered in terms of both its definitional and its legislative functions. This suggests that it first must be learned or discovered *as* a concrete exemplar *of* a (potential or contemporaneously presented) generalization. Dual-function generalizations such as Newton's law or the theories considered in Sneed's set-theoretic formalism thus require exemplars (or applications) for their empirical content. The law, its terms, and its exemplars are constitutively interrelated.

[176] The processes outlined by Sneed reflect those of the hypothetico-deductive method. To the extent that we accept Kuhn's proposed inversion, we must understand the underlying (alternative) method that is involved. Given the long-established effectiveness and widespread use of the hypothetico-deductive method, we must thus tread carefully in proposing that it be changed (or replaced) in such a substantive way.

The legislative concreteness of a constitutive exemplar can be established by adapting Sneed's notion of constraints and by requiring that the analogical relationships established by our constitutive exemplars possess the structure of a problem-solution. As pointed out by Kuhn's discussion of Sneed's theory, to the extent that a theory possesses only one application, it will appear to be circular and its content vacuous. Yet multiple applications will establish constraints for the range within which the theory may be applied. As applied to our consideration, if a constitutive exemplar defines only one of its referents, the problem-solving structure presented by that exemplar (i.e., the relationship established among its referents – both constituted and antecedently available) will appear to be circular and its content vacuous. In such cases, the constitutive exemplar will be concrete in terms of its definitional function (i.e., it will have succeeded in introducing a new referent and presenting itself as a concrete construct. Its legislative function, however, will appear to be only that of a definition. On the other hand, if a constitutive exemplar introduces multiple referents, this multiplicity will establish constraints for the relationships among referents in the problem-solution.

Thus if we require that an exemplar is constitutive with respect to at least two of its referents, the constraints that are established by conjoining those (concretized) referents within the exemplar will provide some (empirical) content to the problem-solving structure. This will establish at least some aspects of the legislative function of the exemplar as concrete. The degree to which a constitutive exemplar is (understood to be) a concrete construct thus will depend upon the number and nature of the referents that it establishes as well as the nature of its problem-solution.

All else being equal, an exemplar will be more concrete (as a construct) to the extent that it constitutes more of those referents that are central to its puzzle solution. It must be noted, however, that this greater concreteness (as a self-constituting construct) may be counterbalanced by a greater challenge in communicating its relevance and relationship to the group of referents and theories that are antecedently available. In the context of the practice of science by the scientific community, it is likely that at least one antecedently available referent will be needed in order to connect the exemplary problem-solution to existing referents and theories, that is, to identify it *as* a problem that requires solution.

Once a constitutive exemplar is recognized as a concrete puzzle solution, initial efforts to develop it as a generalization may focus on either its definitional or its legislative functions. Attempts to apply its legislative functions to situations that are deemed similar will result in "specialized" formalizations of its definitional terms. Following Sneed's theory of constraints, conjoining multiple applications over time will establish constraints that can be used to develop generalized definitions. In this way, a constitutive exemplar may be transformed (over time) into a dual-function generalization such as $f = ma$.

This (highly schematic and preliminary[177]) account seems to explain Kuhn's view that scientific theories are more appropriately "generalization sketches" than generalizations. It also seems to support his proposal that specialized generalizations are developed through the study and usage of the community's exemplars. Furthermore, this suggests that in the case of dual-function generalizations, it is not the generalization that is primitive but the identification of a set of exemplars. This proposal is a striking one, for it suggests that the processes involved in the development of a dual-function generalization are inverted relative to those outlined in the hypothetico-deductive method (just as Kuhn had suggested in considering the processes underlying Sneed's new formalism). To understand the basis of a dual-function generalization, we must thus understand the processes by which its various exemplars are identified as such.

In considering this inversion, however, it is important to note that once a dual-function generalization is established, its basis in an identified set of exemplars will not be apparent (except, that it must always be *translated* into a specialized formalization). In particular, once the definitional functions of a concrete problem-solution are established in a generalized form, the exemplars that were initially constitutive parts of the problem-solution will become identified as referents. The generalization will appear to present a direct correspondence between its (generalized) "laws" and its (identified) referents.

The establishment of a (dual-function) generalization thus represents an important, but potentially misleading, achievement. It is important because the "generalization" is recognized as the basis for solving a range of scientific puzzles through specialized formalizations. The terms of the generalization may now be used in the development of other theories. They may be studied by students and their applications may be grouped together *as* exemplars of the *same* theory. The resulting network will embody the development and expansion of scientific knowledge and will be deemed both appropriate and valuable from the

[177] This account focuses on the processes involved in the development of a concrete problem-solution into a (dual-function) generalization. It neglects a number of crucial considerations, in particular, the way in which these activities are conducted by (various members and groups within) the scientific community.

perspective of the study and practice of science. From an historical, developmental or evolutionary perspective, however, the achievement is misleading because the apparent nature and authority of the various aspects of its development (i.e., concrete problem-solutions, constitutive referents, antecedently available referents, and generalizations) will be transformed, along with the apparent relationships that previously existed among them.

Even in its schematic and preliminary form, this (quasi-historical developmental) account of dual-function generalizations and their origin in concrete puzzle solutions provides substantial elucidation of Kuhn's notions of paradigms and exemplars. The achievement of a dual-function generalization (i.e., a paradigm) emerges from the study of concrete puzzle solutions (i.e., exemplars). This achievement reflects agreement on fundamental entities (definition of terms) and establishes a basis for puzzle-solving (legislation of terms). In this respect, then, the juxtaposition of exemplars will render them concrete constructs. To the extent that this juxtaposition reflects in the achievement of a dual-function generalization, the construct will function as a concrete puzzle solution.

Section Two

Incommensurability and the Theory of Meaning

Within the philosophy of language, the causal theory of meaning was applied to natural kinds as one of several ways to address the problem of meaning or, in related yet somewhat specialized variations, the problem of referent determination and the problem of meaning change. The problem of meaning highlighted the philosophical challenges inherent in determining the referents of generalizations through the use of definite description or other intensional indicators. In particular, challenges identified in the (positivist) distinction between analytic and synthetic aspects of a theory, or (later) between theoretical and observational terms drew into question the basis on which one might determine the referent of a generalization by means of its intensional characteristics. Several approaches were developed by which a referent could be determined extensionally, without confronting the difficulties of intensional distinctions. Among the most widely recognized (and for Kuhn, the most relevant) approaches were those outlined by W. V. O. Quine, Saul Kripke, and Hillary Putnam.[178]

The problem of meaning change presented a challenge in the determination of a referent either at different points in time or for (different) generalizations that shared the same referent. The problem of incommensurability, presented both by Kuhn and by Paul Feyerabend in slightly different forms, suggested that the meaning of theoretical terms differed in various applications or iterations of scientific theories, thereby rendering the evaluation or comparison of these "incommensurable" theories problematic. Yet the conclusions drawn by Kuhn and Feyerabend – that evaluation or comparison of incommensurable generalizations must be conducted on the basis of something other than logical or linguistic analysis – seemed to challenge the very foundations of established views of rationality. For scholars who were loathe to give up this established position, incommensurability needed to be explained and the problem of meaning change that it presented needed to be addressed. Feyerabend proposed that their concerns might be addressed through reliance on aesthetic judgments and evaluation of the form (but not the content) of scientific theories. Kuhn sought to defend his views against the charges of irrationality lodged by Feyerabend and other philosophers of science by proposing that the problem of meaning change

[178] Kuhn noted, in particular, (Kripke 1972) and (Putnam 1975). While noting that Putnam had subsequently changed his views, he proposed that "few philosophers have followed him. The views discussed below are very much alive" (PW 1986/2000, 78n).

highlighted the operation of "good reasons" for theory choice, even in cases of incommensurability. Yet as we will see, the differences between exemplars of the "good reasons" identified by Kuhn and the aesthetic judgments of Feyerabend were not as substantive as either of the two scholars suggested.

Although Kuhn's conception of incommensurability changed throughout the course of his research, it remained at the center of the issues that he investigated. In *Structure*, incommensurability was used to characterize the non-cumulative break between successive scientific traditions and was described as a gestalt switch or change in scientists' "ways of seeing" the world. Following the publication of *Structure*, Kuhn refined the conception, linking it to issues and investigations in the philosophy of language:

> When writing the book on revolutions, I described them as episodes in which the meanings of certain scientific terms changed, and I suggested that the result was an incommensurability of viewpoints and a partial breakdown of communication between the proponents of different theories. I have since recognized that "meaning change" recognizes a problem rather than an isolable phenomenon, and I am now persuaded, largely by the work of Quine, that the problems of incommensurability and partial communication should be treated in another way. Proponents of different theories (or different paradigms, in the broader sense of the term), speak different languages – languages expressing different cognitive commitments, suitable for different worlds. Their abilities to grasp each other's viewpoints are therefore inevitably limited by the imperfections of the processes of translation and of reference determination. Those issues are currently the ones that concern me most, and I hope before long to have more to say about them. (*ET-SS* 1977, xxii – i)

In these initial refinements of the notion of incommensurability, Kuhn thus began to characterize it in terms of the problem of meaning. In particular, he characterized it as a problem of partial communication that could be explained by recourse to the philosophical processes involved in translation and reference determination.

In considering Kuhn's extension of his investigations to issues and approaches in the philosophy of language, it is important to note his interest not only in language itself but also in the language-nature link and its relation to exemplars:

> . . . in learning [a specialized] language, as they must to participate in their community's work, new members acquire a set of cognitive commitments that are not, in principle, fully analyzable within that language itself. Such commitments are a consequence of the ways in which the terms, phrases, and sentences of the language are applied to nature, and it is its relevance to the language-nature link that makes the original narrower sense of "paradigm" [i.e., construct paradigm or exemplary problem solution] so important. (*ET-SS*, xxii)

To the extent, then, that Kuhn sought to examine the problem of incommensurability within the context of philosophy of language and the problem of meaning, he was interested not simply in changes of language but, more precisely, in changes of the language-nature link.[179] Such changes were, he proposed, integrally linked with the knowledge gained from exemplars.

The "Double-Faced Character" of Scientific Language

In "What Are Scientific Revolutions?" (WSR 1980/2000, 13-32), Kuhn proposed that "the central character of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and thus prior to anything quite describable as description or generalization, scientific or everyday" (WSR, 32). Following detailed study of the revolutions prompted by Newton (mechanics), Volta (electronic battery) and Planck (quantum theory), he offered a refined conception of meaning change, first as a change in the way that referents are determined and then, more specifically, as a change in "several of the taxonomic categories prerequisite to scientific descriptions and generalizations" (WSR, 30). Noting that his views were still developing, he proposed that,

> …roughly speaking, the distinctive character of revolutionary change in language is that it alters *not only the criteria by which terms attach to nature but also, massively, the set of objects or situations to which those terms attach*. What had been paradigmatic examples of motion for Aristotle – acorn to oak or sickness to health – were not motions at all for Newton. In the transition, a natural family ceased to be natural; its members were redistributed among preexisting sets; and only one of them continued to bear the old name. (WSR, 29-30, emphasis added)

Revolutionary change in language thus involves not only a change in criteria but also a change in the underlying taxonomic structure by which a particular "natural family" is determined. As such, the changes that occur thus are not simply linguistic or logical but, more fundamentally, taxonomic. In this respect, "language" itself is not "simply" linguistic or logical but is linked with a particular taxonomy in some (as yet undetermined) way.

Emphasizing that the redistribution of objects and situations among taxonomic categories is a redistribution among multiple, interdefined categories, he concluded that "this sort of alteration is necessarily holistic" (WSR, 30). Furthermore, he proposed that the holistic nature of the change is,

> . . . rooted in the nature of language, for the criteria relevant to categorization are *ipso facto* the criteria that attach the names of those categories to the world. Language is a coinage with two

[179] Kuhn's application of the causal theory of reference through multiple acts of dubbing is similar to this consideration of the role of exemplars in establishing the language-nature link and the incommensurability that results from changes in the language-nature link.

> faces, one looking outward to the world, the other inward to the world's reflection in the referential structure of the language. (Ibid.)

In making this assertion, Kuhn posited language as establishing a connection between the determination of a referent and a particular referential structure.

Examining this connection from the opposing perspective – how to determine a referential structure (as opposed to a particular referent) – Kuhn noted that the changes of meaning that occur with scientific revolutions are typically accompanied by changes of model, metaphor or analogy. Furthermore, he suggested that these "metaphor-like juxtapositions" are "central to the process by which scientific and other language is acquired:"

> When the exhibit of examples is part of the process of learning terms like "motion," "cell," or "energy element," what is acquired is knowledge of language and of the world together. On the one hand, the student learns what these terms mean, what features are relevant for attaching them to nature, what things cannot be said of them on pain of self-contradiction, and so on. On the other hand, the student learns what categories of things populate the world, what their salient features are, and something about the behavior that is and is not permitted of them. *In much of language learning these two sorts of knowledge – knowledge of words and knowledge of nature – are acquired together, not really two sorts of knowledge at all, but two faces of the single coinage that a language provides*. (WSR, 31, emphasis added)

Once again, we encounter the "double-faced character" of scientific language, this time from the perspective of language learning and, implicitly, the development of a referential structure.

Kuhn proposed that this reappearance provided "an appropriate terminus" for his paper, providing no further elaboration of either the distinctive character of "scientific language" or the basis on which its "double face" may be established (or changed). Given the subject of scientific revolutions, Kuhn's neglect of a detailed investigation of the dual-nature of scientific language and the basis for establishing (or changing) its dual aspects may have been appropriate; however, it was unfortunate from the perspective of the development of his broader theory.[180] Focusing solely on the changes that occurred with revolutions, he stated simply,

> If I am right, the central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and that is thus prior to anything quite describable as generalization, scientific or everyday. (WSR, 31-2)

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[180] As we will see, several of Kuhn's subsequent publications dealt with only one or the other aspect of this duality without acknowledging its relation to the other aspect. While this detailed focus is perhaps understandable, it provides only a limited view by neglecting the broader, holistic perspective. Some indication of this broader view is, however, provided in two of Kuhn's final publications.

As we will see, the double-faced character of scientific language served as an important focal point for Kuhn's subsequent investigations. It provided a basis from which he extended established approaches to the problem of meaning change (i.e., changes in the criteria used for reference determination) to consider the problem of incommensurability (i.e., changes in the underlying referential structure).

Translation and Interpretation

Kuhn's initial attempts to characterize incommensurability as a problem of translation (RMC 1969/1970c) encountered substantial opposition from scholars in both the philosophy of science and the philosophy of language. In "Commensurability, Comparability and Communicability" (CCC 1982/2000, 33-57), he revised this earlier proposal and reconsidered the associated philosophical challenges. Specifically, Kuhn proposed that incommensurability is the absence of a common measure, such that direct translation is impossible without residue or loss (CCC, 35). Responding to two distinct lines of criticism outlined by Hillary Putnam (1981), he insisted that untranslatability need not imply the impossibility of comparison: what is required in such situations is interpretation. Furthermore, he suggested that the failure of translation and the need for interpretation serve as important clues for better understanding the nature of scientific language and the structure of scientific theories.

In reconsidering the notion of incommensurability, Kuhn characterized the discussions surrounding it as depending upon "the literally correct but regularly overinterpreted assumption that, if two theories are incommensurable, they must be stated in mutually untranslatable languages" (CCC, 34). He now proposed the more precise definition of incommensurability as lack of a common measure:

> Remember briefly where the term "incommensurability" came from. The hypotenuse of an isosceles right triangle is incommensurable with its side or the circumference of a circle with its radius in the sense that there is no unit of length contained without residue an integral number of times in each member of the pair. There is thus no common measure. (CCC, 35)

By "common measure," then, Kuhn meant (even more precisely), the ability to measure (or to translate) "without residue." Thus the relationship between the hypotenuse of a right triangle and its side (typically rendered as $a^2 + b^2 = c^2$, or in the case of an isosceles right triangle, $2a^2 = c^2$ or $2b^2 = c^2$) cannot be reduced to common unit of comparison. Instead, it must be considered as a complex interrelationship of terms.

In considering two theories that are incommensurable,[181] one thus cannot rely upon a common measure, or more precisely, upon a common language as a basis of direct comparison or perfect translation, that is, translation without residue. In the case of scientific theories, Kuhn proposed that problems of translation (i.e., problems of incommensurability) typically are limited to a small group of interrelated terms and sentences. The other terms – which are common to the theories and can be translated – thus provide a basis for (interpretive) comparison: "[t]he terms that preserve their meanings across a theory change provide a sufficient basis for the discussion of differences and for comparisons relevant to theory choice" (CCC, 36).

In considering these proposals with respect to earlier challenges by philosophers, Kuhn noted that Davidson, Kitcher, and Putnam, among others, had examined the technique of interpretation, described its outcome as translation (or a translation schema), and concluded that even local incommensurability was impossible. In response, Kuhn suggested that these investigations – along with other discussions of translation in philosophy of language – failed to distinguish between translation and interpretation. Tracing this activity to Quine's *Word and Object* (1960), he insisted that,

> . . . interpretation . . . is not the same as translation, at least not as translation has been conceived in much recent philosophy. . . . Recent analytic philosophy has concentrated exclusively on one and conflated the other with it. (CCC, 37)

To the extent, then, that incommensurable theories are without common measure, Kuhn would propose that they cannot be translated, *that is, translated without residue*. Yet to the extent that they can be interpreted (such that interpretation is distinguished from translation), they may be compared. In the difference between translation and interpretation, we thus have a means of comprehending the "untranslatable residue" that exists between incommensurable theories and that provides the basis for their comparison. Yet this comparison seems to involve the use of creative, rather than purely logical, judgment.

Kuhn defined translation as "something done by a person who knows two languages:"

> Confronted with a text, written or oral, in one of these languages, the translator systematically substitutes words or strings of words in the other language for words or strings of words in the text in such a way as to produce an equivalent text in the other language. What it is to be an "equivalent text" can, for the moment, remain unspecified. Sameness of meaning and sameness of reference are both obvious desiderata, but I do not yet invoke them. Let us simply say that the

[181] It is important to note that the "problem" of incommensurability is associated with a comparison of two theories, rather than an evaluation of a single theory on the basis of its truth or correspondence to truth [See (Ibid., 36n).

> translated text tells more or less the same story, presents more or less the same ideas, or describes more or less the same situation as the text of which it is a translation. (CCC, 38)

On this account, translation involves working between two known languages, such that the words or phrases of one are rendered in the other. In considering this activity, Kuhn emphasized, first, that both languages existed (and were known to the translator) prior to the translation. On this basis, he proposed that "[t]he fact of translation has not, that is, changed the meanings of words or phrases. It may, of course, have increased the number of known referents of a given term, but it has not altered the way in which those referents, new and old, are determined" (CCC, 38). Citing a second and related point, he continued by noting that "the translation consists exclusively of words and phrases that replace (not necessarily one-for-one) words and phrases in the original" (Ibid.). Acknowledging that glosses and translators preferences are often used in accomplishing this "replacement," he noted that these are not part of the translation itself and would not be required of a "perfect" translation.[182]

Turning to interpretation, which he characterized as the enterprise conducted by historians and anthropologists, among others, Kuhn noted that, "[u]nlike the translator, the interpreter may initially command only a single language. At the start, the text on which he or she works consists in whole or in part of unintelligible noises or inscriptions" (Ibid). To the extent, then, that an interpreter succeeds,

> what he or she has in the first instance done is to *learn a new language*, perhaps the language in which "gavagai" is a term, or perhaps an earlier version of the interpreter's own language, one in which still current terms like "force" and "mass" or "element" and "compound" functioned differently. Whether that language can be translated into the one with which the interpreter began is an open question. *Acquiring a new language is not the same as translating from it into one's own.* Success with the first does not imply success with the second. (CCC, 38-9, emphases added)

Kuhn proposed that the difference between learning a new language and translating between two known languages is a difference of knowing the distinctive ways in which each of the two languages structures the world. To the extent that such structural linguistic differences exist, they may be learned. Once learned (that is, once the structures of the two languages have been interrelated), the terms associated with the various structures may be translated from one language into another. Yet before such translation is possible, the terms of one structure must be interpreted using the terms of the other. This process of interpretation is the means by which different linguistic structures are learned and their (localized) points of

[182] Kuhn added, "[I]f they are nonetheless required, we shall need to ask why. Doubtless, these features of translation seem idealizations, and they surely are. But the idealization is not mine. Among other sources, both derive directly from the nature and function of a Quinean translation manual" (CCC 1982/2000, 38).

incommensurability may be compared. In this respect, interpretation is the answer to the "problem" of incommensurability, is essential to language learning, and is a prerequisite for translation.

Considering the activities involved in interpretation, Kuhn examined both the attempts of historians of science to understand out-of-date scientific texts and the means by which students of science learn new theories. With respect to understanding out-of-date texts, Kuhn noted that the greatest challenges typically arise in determining the referents of out-of-date terms, such as the term "phlogiston," as used in eighteenth-century chemistry. Strictly speaking, such terms "do not refer" within the context of twentieth-century chemistry, yet given their role during the earlier period, they must be rendered if the out-of-date text is to be understood. Furthermore, Kuhn noted that such terms are integrally interrelated with sets of other terms, only some of which may be included in modern usage – and then in a somewhat altered form.

Thus the attempt to develop a coherent translation of an eighteenth-century text on the phlogiston theory would encounter difficulties not only with respect to the individual term phlogiston but also – in attempting to render that term – with respect to other out-of-date terms, such as "principle" and even modern terms, such as "element." To complete a coherent translation, Kuhn proposed that one must understand these terms as constituting "an interrelated and interdefined set that must be acquired together, as a whole, before any of them can be used, applied to natural phenomena" (CCC, 44). In doing so, one would come to understand not only the phlogiston theory but also eighteenth-century chemistry, distinguishing the latter as "a discipline that differed from its twentieth-century successor not simply in what it had to say about individual substances and processes, but in the way it structured and parceled out a large part of the chemical world" (Ibid.).

Turning from the historian's study of out-of-date texts to the way in which a student learns a new scientific theory, Kuhn again emphasized the challenges and insights provided by identifying an interrelated and interdefined set of concepts and terms. In considering Newtonian mechanics, he noted that standard formalization obscures both the need to acquire the terms "mass" and "force" together and the consequential role of Newton's Second law in their acquisition:

> In formalizing mechanics, one may select either "mass" or "force" as primitive and then introduce the other as a defined term. But that formalization supplies no information about how either the primitive or the defined terms attach to nature, how the forces and masses are picked out in actual physical situations. Though "force," say, may be a primitive in some particular formalization of mechanics, one cannot learn to recognize forces without simultaneously learning to pick out masses and without recourse to the second law. That is why Newtonian "force" and "mass" are not

> translatable into the language of a physical theory (Aristotelian or Einsteinian, for example), in which Newton's version of the second law does not apply. To learn any one of these three ways of doing mechanics, the interrelated terms in some local part of the web of language must be learned or relearned together and then laid down on nature whole. They cannot simply be rendered individually by translation. (Ibid)

On this account, the formalization and its various terms cannot be learned individually but must be acquired together. Similarly, translating Newtonian mechanics into Aristotelian or Einsteinian forms requires something other than direct translation from one theory to the next.

It would seem, then, that both the historian of science and the student must find some way to bridge the gap between those terms of their "known" language that are directly applicable to the new language (i.e., rendered without difficulty) and those that are not. Kuhn proposed that this gap must be bridged in order for the newly acquired language or theory to be rendered coherent, and he insisted that doing so involved a set of interrelated and interdefined terms that must be acquired together. The first step in bridging the gap thus must be to identify the set of terms that are problematic. The distinctive structure that these terms collectively establish within the language or theory to be acquired then must be understood, that is, their use in the language must be learned. It is only at that point that the attempt may be made to interpret one language in terms of the other, that is, to relate their distinctive structures. Yet this relationship will not be exact and thus will be accomplished through the creative judgment of interpretation, rather than the logical judgment of translation.

In considering this process of interpretation, Kuhn noted that it was widely discussed under the rubric of hermeneutics, as outlined by Charles Taylor in "Interpretation and the Sciences of Man" (1971). While expressing general agreement with Taylor's views, he rejected the proposal that the descriptive language of the natural sciences (or the behavioral language of the social sciences) is fixed and neutral (CCC, 45n). As suggested by the examples provided above, the situations that demand interpretation (rather than translation) are those involving localized differences in language, that is, sets of interrelated and interdefined terms that reflect differences in aspects of the structure of the various languages. According to Kuhn, it is with respect to these localized differences in interrelated terms that two languages or theories are incommensurable and thus untranslatable. Interpreting or learning another language thus involves first identifying these localized differences and then attempting to account for them. The creative judgments of interpretation thus are two-fold: the identification of differences, followed by attempts to bridge those differences.

Once the similarities and localized differences between the two languages are identified and understood – that is, once an individual *has learned the new language*, Kuhn proposed that the earlier difficulties are overcome and often forgotten. While an historian of science will introduce an out-of-date text with passages outlining how scientists understood and described the world of their time, once these "glosses on phlogistic texts" are comprehended the text itself will seem "merely translations, or perhaps merely texts" (CCC, 45). Through the (interpretive) processes involved in learning the new language, "systematically transformed uses of terms" will have been incorporated into the old language, in large part by means of the overlap provided by those terms that the two languages share (Ibid.).

Based on these analyses, Kuhn proposed that established views of translation in terms of extensional or referential semantics must be reconsidered (CCC, 47-53). He suggested that Quine's radical translator is, in fact, a language learner, and that the translation manual that he establishes fails to indicate the ways in which the other language structures the world. Rather than directing attention to the "clusters of interrelated terms" that must be learned together, establish a distinctive structure within a particular language and thus serve to illustrate incommensurability between languages, Quine's radical translator presents one-many linkages (i.e., those linkages that are not direct or one-to-one) as context specifiers. Also included as context specifiers are genuine cases of linguistic ambiguity, thereby diluting the extent to which clusters of related terms might be identified and used in the learning process:

> By treating the one-many linkages in his translation manuals as cases of ambiguity, Quine discards the intensional constraints on adequate translation. Simultaneously, he discards the primary clue to the discovery of how the words and phrases in other languages refer. Though one-many linkages are sometimes caused by ambiguity, they far more often provide evidence of which objects and situations are similar and which are different for speakers of the other language: they show, that is, how the other language structures the world. Their function is thus very much the same as that played by multiple observations in learning a first language. (CCC, 49)

It would seem, then, that the intensional constraints that are provided by one-many linkages or by multiple observations are central to discovering "how the other language structures the world."

To the extent that languages structure the world differently – a point that Kuhn supported by reference to the historian and the student of science – we can understand those structural linguistic differences only by understanding the intensional aspects of the languages or theories that we would compare. That is, we must identify their localized differences before we can attempt to account for them. This suggests that theories of translation based on extensional semantics are incomplete:

> What is it that translation must preserve? Not merely reference, I have argued, for reference-preserving translations may be incoherent, impossible to understand, while the terms they employ are taken in their usual sense. That description of the difficulty suggests an obvious solution: translations must preserve not only reference but also sense or intension. (CCC, 50)

Noting the well-known challenges to the "meaning invariance" implicit in this proposal (challenges that Quine had sought to address), Kuhn averred by noting that the position, "is by no means merely wrong, but it is not quite right either, an equivocation symptomatic, I believe, of a deep duality in the concept of meaning" (Ibid.).

Rather than confronting the problem of meaning invariance directly, Kuhn turned to the problem of reference determination, noting that a wide range of criteria might be considered and these might differ among the members of the community with no effect on determination of the appropriate referent. Drawing from his earlier analyses, Kuhn identified two themes that had emerged to explain the difficult yet important question, "What must speakers with disparate reference-determining criteria share in order that they be speakers of the same language, members of the same language community?" First, he proposed that a language community is characterized by "local holism," such that the members of a community share "sets of terms that must be learned together by those raised inside a culture, scientific or other, and which foreigners encountering that culture must consider together during interpretation" (CCC, 51-2). Secondly, Kuhn reiterated his assertion that "different languages impose different structures on the world" (CCC, 52). The localized, holistic aspects of one language thus may differ from those of another such that reference determination is not possible (i.e., interpretation is required).

Providing only a brief sketch of what would become his theory of the lexicon, Kuhn attempted to explain how one might reconcile differences of criteria with meaning invariance, local holism, linguistic structure, translation and interpretation:

> Imagine, for a moment, that for each individual a referring term is a node in a lexical network from which radiate labels for the criteria that he or she uses in identifying the referents of the nodal term. Those criteria will tie some terms together and distance them from others, thus building a multidimensional structure of the world which the lexicon can be used to describe, and it simultaneously limits the phenomena that can be described with the lexicon's aid. If anomalous phenomena nevertheless arise, their description (perhaps even their recognition) will require altering some part of the language, changing the previously constitutive linkages between terms. (Ibid.)

What is preserved by the "homologous structures" that are shared by members of the same language community is thus "the taxonomic categories of the world and the similarity/difference relationships between them" (Ibid.). Thus what the members of a language community share is homology of lexical structure:

> Their criteria need not be the same, for those they can learn from each other as needed. But their taxonomic structures must match, for where structure is different, the world is different, language is private, and communication ceases until one party acquires the language of the other. (Ibid.)

Incommensurability thus occurs between languages (or language communities) with different taxonomic structures. In such cases, translation is not possible because reference determination is not the same. What is required, then, is interpretation or language learning. To understand and to address situations of incommensurability, we thus must turn not to translation but to interpretation.

Interrelated Terms, Possible Worlds, and the Causal Theory

Kuhn both deepened and expanded his earlier considerations of interpretation, translation, and lexical structure in his 1986 presentation at the 65th Nobel Symposium, "Possible Worlds in the History of Science" (PW 1986/2000, 58-89).[183] In introducing the essay, he noted that many of the issues discussed were central to the book on which he was currently working yet arise in the book "only after much prior discussion has led to conclusions I must here present as premises" (PW, 58). With that caveat provided, Kuhn began the essay with a delineation of the interpretive activities of historians of science; a reconsideration of the limitations of referential semantics; and a proposal for a (new) theory of meaning. He then provided a detailed examination of how the terms of Newton's Second law are interrelated with each other and with the terms themselves. Finally, Kuhn concluded by identifying the implications of his discussions for possible world semantics and the causal theory of reference.

In introducing his discussion of interpretations in the history of science, referential semantics and the theory of meaning, Kuhn summarized his views as follows:

> . . . to understand some body of past scientific belief, the historian must acquire a lexicon that here and there differs systematically from the one current in his own day. Only by using that older lexicon can he or she accurately render certain of the statements that are basic to the science under

[183] The proceedings of the symposium were published as *Possible Worlds in Humanities, Arts and Sciences* (1989).

> scrutiny. Those statements are not accessible by means of a translation that uses the current lexicon, not even if the list of words it contains is expanded by the addition of selected terms from its predecessor. (PW, 58-9)

In these statements, we have an abbreviated version of the theory of the lexicon introduced in the 1982 essay, "Commensurability, Comparability, Communicability," which was examined in the previous section. According to this account, the "acquisition" of a new lexicon is necessitated by the need to "accurately render certain of the statements that are basic to the science under question." Yet while "systematically different" from the historian's current lexicon, those differences occur only "here and there."

Examining these processes in more detail, Kuhn outlined how historians of science attempt to understand out-of-date scientific texts:

> A historian reading an out-of-date scientific text characteristically encounters passages that make no sense. That is an experience I have had repeatedly whether my subject was an Aristotle, a Newton, an Volta, a Bohr, or a Planck. It has been standard to ignore such passages or to dismiss them as the products of error, ignorance, or superstition and that response is occasionally appropriate. More often, however, sympathetic contemplation of the troublesome passages suggests a different diagnosis. The apparent textual anomalies are artifacts, products of misreading.
>
> For lack of an alternative, the historian has been understanding words and phrases in the text as he or she would if they had occurred in contemporary discourse. Through much of the text that way of reading proceeds without difficulty most terms in the historian's vocabulary are still used as they were by the author of the text. But some sets of interrelated terms are not, and it is failure to isolate those terms and to discover how they were used that has permitted the passages in question to seem anomalous. *Apparent anomaly is thus ordinarily evidence of the need for local adjustment of the lexicon, and it often provides clues to the nature of that adjustment as well.* An important clue to problems in reading Aristotle's physics is provided by the discovery that the term translated "motion" in his text refers not simply to change of position but to all changes characterized by two end points. Similar difficulties in reading Planck's early papers begin to dissolve with the discover that, for Planck before 1907, "the energy element *hv*" referred not to a physically indivisible atom of energy (later to be called "the energy quantum") but to a mental subdivision of the energy continuum, any point on which could be physically occupied.
>
> These examples all turn out to involve more than mere changes in the use of terms, thus illustrating what I had in mind years ago when speaking of the "incommensurability" of successive scientific theories. (PW, 59-60, emphasis added)

Kuhn here expanded his earlier discussions of historiographic method. In contrast to those earlier discussions, he did not simply present his method as a way of explaining how he developed the substantive aspects of his theory of scientific revolutions. Nor did he seek to highlight the methodological points of difference between the history and the philosophy of science. Rather, in this case, Kuhn emphasized both the substantive and the methodological implications of the new historiography by delineating the steps taken and the insights revealed by the historian of science. Thus what is highlighted (albeit as yet, only tacitly) is the distinctive nature of the insights provided by historiography. To make these points more directly, we thus might expand upon the italicized sentence above to assert that the apparent anomaly, which is revealed and explained by the historian, provides evidence of a local adjustment of the lexicon *that is not apparent to (or even accessible through) established philosophical approaches*.

Indeterminacy of Translation and Universality of Understanding

In asserting that the incommensurability that is revealed in the history of science is different from translatability as understood in the philosophy of science (or the philosophy of language), Kuhn once again considered Quine's views of translation. In this case, however, he examined Quine's arguments for the indeterminacy of translation. Inverting Quine's explanation of the basis for this indeterminacy, Kuhn proposed that, "instead of there being an infinite number of translations compatible with all normal dispositions to speech behavior, there often are none at all" (PW, 61). On this account, one might well assert the possibility for untranslatability.

Considering how such an argument might be made within the bounds of Quine's theory, Kuhn located it in the choice posited between meaning and the universality of language:

> In [Quine's] view, one must either entirely abandon traditional notions of meaning, of intension, or else one must give up the assumption that language is, or could be, universal, that anything expressible in one language, or by using one lexicon, can be expressed also in another. His own conclusion – that meaning must be abandoned – follows only because he takes universality for granted, and this paper will suggest that there is no sufficient basis for doing so. (Ibid.)

Drawing from his earlier distinctions between translation and the interpretation that accompanies language learning, Kuhn proposed that conceptions of universality should not be based upon universal translatability but upon universal understanding:

> What has made the assumption of universal translatability so nearly inescapable is, I believe, its deceptive similarity to a quite different one, in this case an assumption that I share: anything that can be said in one language can, with imagination and effort, be *understood* by a speaker of another. What is prerequisite to such understanding, however, is not translation but language learning. (Ibid.)

Based on this discussion, Kuhn concluded, "[t]hough learnability could in principle imply translatability, the thesis that it does so needs to be argued" (Ibid.).

Given the influence of Quine's theory, we must extend Kuhn's comments to consider the implications of his proposal more deeply. Specifically, Kuhn's proposals suggest that the Quinean choice between intensional notions of meaning and universality of understanding is a false dilemma. Traditional notions of meaning could be taken to suggest either an infinite number of translations or untranslatability, yet neither of these proposals need contradict the universality of understanding. In this respect, then, we must distinguish notions of both meaning *and* translatability from the notion of understanding. Furthermore, we must specify what can account for understanding independent of meaning or translation.

In considering the problems encountered in translating scientific texts, Kuhn proposed that these are similar to the challenges presented in the translation of literature:

> In both cases the translator repeatedly encounters sentences that can be rendered in several alternative ways, none of which captures them completely. Difficult decisions must then be made about which aspects of the original it is most important to preserve. Different translators may differ, and the same translator may make different choices in different places even though the terms involved are in neither language ambiguous. Such choices are governed by standards of responsibility but they are not determined by them. In these matters there is no such thing as being merely right or wrong. The preservation of truth values when translating scientific prose is very nearly as delicate a task as the preservation of resonance and emotional tone in the translation of literature. Neither can be fully achieved; even responsible approximation requires the greatest tact and taste. (PW, 62)

Kuhn emphasized that translations of scientific texts require such choices with respect to all passages in a text, not only those with explicit reference to the theory. Insisting that scientific texts typically exhibit both a literal and a figurative use of language, he noted that the two uses of terms "are alike in their dependence on preestablished associations between words" (Ibid.). To the extent that these associations are important for understanding aspects of the text that are directly related to the theory, they are also central for understanding the text's more descriptive aspects.

Based on these considerations, Kuhn outlined two aspects of a theory of meaning. First, he proposed that "knowing what a word means is knowing how to use it for communication with other members of the language community within which it is current" (Ibid.). This conception of meaning as knowledge of the *use* of a word is to be distinguished from identification of the *individual* meaning of the word because "[w]ords do not, with occasional exceptions, have meanings individually, but only through their association with other words within a semantic field" (PW, 63). Meaning change thus extends beyond the word itself: "[i]f the use of an individual term changes, then the use of the terms associated with it normal changes as well" (Ibid.). Secondly, Kuhn proposed that "[t]wo people may use a set of interrelated terms in the same way but employ different sets (in principle, totally disjunct sets) of field coordinates in doing so" (Ibid.). On this account, the use of a word and even its associated terms may be shared by two individuals who determine those words in different ways. Yet while using different "metrics" or criteria for that set of terms, those must be chosen in such a way as to “preserve the structural geometrical relations” among them (Ibid.). Noting that this second proposal is “both less standard and more consequential” than the first, Kuhn attempted to illuminate it by illustration. For this he turned, once again, to language learning in Newtonian mechanics.

Learning the Lexicon of Newtonian Mechanics

In considering how students learn the “lexicon” of terms and theories that constitute Newtonian mechanics, Kuhn proposed that there are five aspects that are of particular importance. First, a “considerable antecedent vocabulary” must be in place, including a vocabulary for referring to physical, spatio-temporal objects; a mathematical vocabulary encompassing trajectories, velocities, and accelerations; and, at least implicitly, a notion of extensive magnitude (PW, 66).

Secondly, in acquiring the Newtonian terms of “force,” “mass,” and “acceleration,” students must be provided with examples of their use. These may be either actual exemplars or “stipulative descriptions,” which are “drawn primarily from the antecedently available vocabulary but in which the terms to be learned also appear here and there” (PW, 66-7). In both instances, Kuhn proposed, students learn “simultaneously and inseparably about both the substance and the vocabulary of science, about both the world and the language” (PW, 67).

Third, students must be exposed to a number of varied examples as well as contrasting examples, which appear to be similar yet in which the relevant terms do not apply. In considering this aspect of the learning process, Kuhn noted that “[t]he terms to be learned . . . are seldom applied to these situations in isolation but are instead embedded in whole sentences or statements, among which are some usually referred to as laws of nature” (Ibid.).

Fourth, multiple new terms must be learned, such that the learning process “interrelates a set of new terms, giving structure to the lexicon that contains them” (Ibid.).

Finally, Kuhn noted that individuals may learn Newtonian mechanics through exposure to a (limited) group of various situations and thus by means of different possible paths. Yet despite these differences, Kuhn proposed that “individuals can in principle communicate fully even though they acquired the terms with which they do so along very different routes” (Ibid.). In this respect, while individuals may acquire or develop a definition of the various terminology within the Newtonian lexicon, these definitions need not be shared.

Following the delineation of these five aspects involved in learning Newtonian mechanics, Kuhn provided a highly detailed account of how students learn Newton's Second law, including the specific processes followed; the various alternatives available at each step of the process; and the implications of the various possible paths for determining the epistemic status of the terms and theories and for responding to situations of theory choice (PW, 67-74).

In learning Newton's Second law, Kuhn proposed that students first are exposed to situations that exemplify either force, mass, or acceleration. The particular situations will differ depending upon the term, and multiple options may exist for individual terms. Thus in learning the term "force," students may be exposed to,

> . . . muscular exertion, a stretched string or spring, a body possessed of weight (note the occurrence of another of the terms to be learned), or finally, certain sorts of motion. The last is particularly important and presents particular difficulties to the student. As Newtonians use "force," not all motions signify the presence of its referent and examples which display the distinction between forced and force-free motions are therefore required. Their assimilation, furthermore, demands the suppression of a highly developed pre-Newtonian intuition. (PW, 68)

In these examples and contrast sets, then, students may not only be required to learn a new term but may need to "learn" to suppress existing intuitions.

Once a student grasps the meaning (i.e., the proper usage) of a particular term, she is introduced to examples in which that term is placed in relation to another new term. In the cases of "weight" and "mass" (and unlike "force"), Kuhn noted that "the qualitative features by which one picks out the [associated] referents . . . are identical with those of pre-Newtonian usage" (PW, 68-9). Yet while qualitative identification is relatively straightforward, the quantification of the terms that is required by Newtonian theory (including the quantification of force, as well as weight and mass) "alters both their individual uses and the interrelationships between them" (PW, 69). In order to learn the terms in a way that supports quantification, students once again must encounter "the juxtaposition of statements involving the terms to be learned with situations drawn directly or indirectly from nature" (Ibid.).

In order to learn the "quantitative concept" of force, students must learn to measure forces with a spring balance (developed for use in Newtonian mechanics, replacing the preexisting pan balance). Yet in order to acquire the proper measure of force using the spring balance, a student must have recourse both to

Newton's third law ("the force exerted by a weight on a spring is equal and opposite to the force exerted by the spring on the weight") and Hooke's law ("the force exerted by a stretched spring is proportional to the spring's displacement"). Kuhn proposed that these laws, while acquired as part of the learning process, are also constitutive parts of that process:

> Like Newton's first law, these are first encountered during language learning where they are juxtaposed with examples of situations to which they apply. Such juxtapositions play a double role, simultaneously stipulating how the word "force" is to be used and how the world populated by force behaves. (Ibid.)

In this discussion, we have, at last, a specific example of the "double role" played by generalizations. Yet it is important to note that the laws do not play this role independently but are integrally interrelated with the other terms and theories encountered in the learning process.

Turning to the quantification of "weight" and "mass," Kuhn noted that there are several alternative sets of examples, the choice among which proves consequential for a student's understanding and characterization of the various elements of Newtonian theory. The "standard" approach is to quantify "mass" as "inertial mass," presenting students with the second law as a way of acquiring the appropriate, quantified conception of the term. This is made possible by the use of a centripetal force apparatus, such that the mass of a body is understood in relation to its acceleration under a known force. The law of gravity can then be introduced as an "empirical regularity:"

> Newtonian theory is applied to observation of the heavens and the attractions manifest there are compared to those between the earth and the bodies resting on it. The mutual attraction between bodies is thus shown to be proportional to the product of their masses, an empirical regularity that can be used to introduce the still missing aspects of the Newtonian term "weight." "Weight is now seen to denote a relational property, one that depends on the presence of two or more bodies. It can therefore, unlike mass [i.e., identified as "inertial mass"], differ from one location to another, at the surface of the earth and of the moon, for example. That difference is captured only by the spring balance, not by the previously standard pan balance (which yields the same readings at all locations). What the pan balance measures is mass, a quantity that depends only on the body and on the choice of a unit measure. (PW, 70)

In this standard approach, "force" is acquired as a quantitative concept through the use of the spring balance. "Mass" is understood as "inertial mass" and is acquired along with Newton's second law. Weight is then identified as an empirical regularity, whose differences can be measured in an appropriate, quantitative way, by the spring balance.

An alternative route for establishing the quantitative usage of "mass" and weight" also begins by quantifying "force" with the spring balance. In this case, however, "mass" is introduced as "gravitational mass," rather than "inertial mass" (thus placing it in a different relation to "weight"). The notion of gravity is introduced as a "stipulative description," that is, as "a universal force of attraction between pairs of material bodies, its magnitude proportional to the mass of each" (PW, 71). In this case, "weight" is explained as a relational property, that is, the force that results from gravitational attraction. With quantitative conceptions of both "mass" and "weight," Newton's second law can then be introduced as "empirical, a consequence simply of observation:"

> For that purpose, centripetal force apparatus is again appropriate, but no longer to measure mass, as it did on the first route, but now rather to determine the relation between applied force and the acceleration of a mass previously measured by gravitational means. (Ibid.)

In this alternative route, then, "force" is measured as before, yet "mass" and "weight" are introduced as "gravitational mass" and a relational property of gravitational attraction, respectively. Newton's second law thus appears to result from observation as an empirical consequence of these (quantified) conceptions of force, (gravitational) mass, and acceleration.

In considering these various paths, Kuhn noted that the particular path that is chosen holds important implications for a student's characterization of the various terms and theories of Newtonian mechanics:

> The two routes thus differ in what must be stipulated about nature in order to learn Newtonian terms, what can be left instead for empirical discovery. On the first route the second law enters stipulatively, the law of gravitation empirically. On the second, their epistemic status is reversed. In each case one, but only one, of the laws is, so to speak, built into the lexicon. I do not quite want to call such laws analytic, for experience with nature was essential to their initial formulation. Yet they do have something of the necessity that the label "analytic" implies. Perhaps "synthetic a priori" comes closer. (Ibid.)

This rather striking passage suggests that differences in how a set of interrelated laws and terms are learned (or applied) may lead to consequential differences in the (individual) epistemic status of those laws or terms. This reinforces the proposal that the laws and terms must be understood as integrally interrelated and are not distinguishable individually (from the perspective of either how they are learned or their epistemic status). Furthermore, Kuhn's proposal that this interrelation is best described as "synthetic a priori," emphasizes the extent to which the interrelated set is constitutive yet also dependent upon empirical observations for its content.

This detailed examination of the interrelation of Newtonian terms and laws and the differences in their epistemic status that result from the way that they are learned provides a clear illustration of Kuhn's earlier assertions regarding changes in scientific theories. First, it illustrates the way in which two individuals may use different criteria yet still identify the "same" referent. The various possible paths that are associated with acquiring the quantitative terms of Newton's second law (and even with acquiring the law itself) are all viable paths for learning Newtonian mechanics. Yet, secondly, challenges to the interrelations that exist among these terms and laws may lead to a breakdown in the associated lexicon. A challenge to Hooke's law or a revision of Newton's second law may present a challenge not only to the laws themselves but also to the vocabulary in which they are stated. Finally, responses to challenges in the interrelated set may differ among individuals, depending upon their view of the epistemic status of the terms drawn into question. This possibility for difference within a (delimited) range of problem solutions suggests a role for creative as well as logical judgment in the study of exemplars.

In considering this last point, Kuhn noted that such differences are usually not relevant to the practice of science: "[w]hile the world behaves in anticipated ways – the ones for which the lexicon evolved – these differences between individual speakers make little or no difference" (PW, 73). Yet with the occurrence of anomaly, individuals' attempts to explain the anomaly will be guided by the way that they understand the interrelated set of terms and laws and (at least initially) will be based on the epistemic status that they attribute to the various terms and laws:

> Imagine that a discrepancy is discovered between Newtonian theory and observation, for example, celestial observations of the motion of the lunar perigee. Scientists who had learned Newtonian "mass" and "weight" along the first of my two lexical-acquisition routes would be free to consider altering the law of gravity as a way to remove the anomaly. On the other hand, they would be bound by language to preserve the second law. But scientists who had acquired "mass" and "weight" along my second route would be free to suggest altering the second law but would be bound by language to preserve the law of gravity. A difference in the language-learning route, one which had no effect while the world behaved as anticipated, would lead to differences of opinion when anomalies were found. (Ibid.)

Extending this example even further, Kuhn noted that if both of these attempts to explain the anomalies fail, then scientists will attempt to alter both laws together – something that is not possible within the interrelated construct of the lexicon. Although Kuhn did not characterize it as such, it is clear that such situations (i.e., when Newtonian theory seems to break down) present not simply a challenge but a crisis for the field of Newtonian mechanics.

Kuhn proposed that dealing with such situations outside the boundaries of the lexicon may be successful; however, he emphasized that this requires,

> . . . recourse to such devices as metaphorical extension, devices that alter the meanings of lexical terms themselves. After such revision – say, the transition to an Einsteinian vocabulary – one can write down strings of symbols that *look like* revised versions of the second law and the law of gravity. But the resemblance is deceptive because some symbols in the new strings attach to nature differently than do the corresponding symbols in the old, thus distinguishing between situations which, in the antecedently available vocabulary, were the same. They are the symbols for terms whose acquisition involved laws that have changed form with the change of theory: the differences between the old laws and the new are reflected by the terms acquired with them. Each of the resulting lexicons then gives access to its own set of possible worlds, and the two sets are disjoint. Translations involving terms with the altered laws are impossible. (PW, 74)

Here, then, we have a detailed explication of the processes underlying the transition from one lexicon to another. While attempting to adjust the Newtonian lexicon so that it might explain the anomaly, scientists gradually adjust first one aspect of its interrelated terms and laws and then another. Meeting with no success, they resort to techniques such as metaphorical extension, which alter the "meanings of lexical terms" (where "meaning" is understood as the use of the term within the community). To the extent that such techniques are successful, the set of terms and laws will be adjusted to reflect the (metaphorical) alteration; however, this adjustment will represent a change the lexicon because it will reflect a change in the interrelation of the terms and laws that constitute the lexicon. While some elements of the two lexicons will overlap, there will be subtle yet consequential differences that render the two sets disjoint and thus establish localized differences in the referents of the altered laws. In those areas in which the referents differ, translation will be impossible.

While this explanation provides detailed support for Kuhn's earlier claims regarding incommensurability, lexical change, and untranslatability, it still presents the problem of meaning change as a challenge to realism. Acknowledging that the resulting threat to realism is of deep concern, Kuhn insisted that views of possible world semantics as providing a basis for "ever closer specification of a single world, the actual or real one" are undercut by changes in lexical structure:

> . . . a lexicon which gives access to one set of possible worlds also bars access to others. (Remember the Newtonian lexicon's inability to describe a world in which the second law and the law of gravity were not simultaneously satisfied.) And scientific development turns out to depend not only on weeding out candidates for reality from the current set of possible worlds, but also upon occasional transitions to another set, one made accessible by a lexicon with different structure. Once such a transition has occurred, some statements previously descriptive of possible worlds prove untranslatable in the terminology developed for the subsequent science. (PW, 76)

Because some of the individual terms and statements of an older lexicon cannot be rendered in the new one, attempts at translation from one lexicon to the other will encounter localized problems.

Yet Kuhn proposed that the challenges for judging the truth values or other epistemic status of *individual* terms of statements need not apply to the lexicons taken in their entirety:

> Faced with untranslatable statements, the historian becomes bilingual, first learning the lexicon required to frame the problematic statements and then, if it seems relevant, comparing the whole older system (a lexicon plus the science developed with it) to the system in current use. Most of the terms used within either system are shared by both, and most of these shared terms occupy the same positions in both lexicons. Comparisons made using those terms alone ordinarily provide a sufficient basis for judgment. But what is then being judged is the relative success of two whole systems in pursuing an almost stable set of scientific goals, a very different matter from the evaluation of individual statements within a given system. (PW, 77)

On this account, meaning change reflects a change in the lexical structure, such that the epistemic status of individual terms or laws cannot be evaluated independently, nor can those terms and laws be translated from one lexicon to the next. These limitations may be overcome, however, by comparing the two lexicons (i.e., the one in place before the change and the one emerging from the change) in their entirety. Yet while these lexicons overlap in many areas, some portion of their respective sets of terms and laws will be disjoint.

The interdependence of the individual terms and laws within a lexicon, combined with the occurrence of lexical change (or meaning change), seem to undercut standard views of realism:

> Evaluation of a statement's truth values is, in short, an activity that can be conducted only with a lexicon already in place, and its outcome depends upon that lexicon. If, as standard forms of realism suppose, a statement's being true or false depends simply on whether or not it corresponds to the real world – independent of time, language and culture – then the world itself must be somehow lexicon-dependent. Whatever form that dependence takes, it poses problems for a realist perspective, problems that I take to be both genuine and urgent. (Ibid.)

In this respect, Kuhn noted that the "problem of meaning change" may be recharacterized as a "problem of lexical dependence." Where "meaning" is the use of a term and that use is the basis on which the term is interrelated with other terms and laws within the lexicon, "meaning change" will reflect a change in the lexicon. Yet while Kuhn (long) perceived this change as central to the nature of science – and thus something to be considered carefully even while confronting the associated challenges to realism – philosophers of science and of language continued to suggest that (solely intensional) changes were cognitively and epistemologically inconsequential within the context of the (presumably) increasing and accumulative development of knowledge. Kuhn thus concluded by examining attempts to dismiss these problems, in particular the causal theory of reference.

The Causal Theory of Reference

Although Kuhn had developed and applied his own approach for applying the causal theory to natural kind terms in several publications,[184] he had had not previously addressed the approaches undertaken by philosophers. Kuhn now examined the causal theory of reference, as developed by Saul Kripke and Hillary Putnam, characterizing it as the most influential among the theories proposing that, 1) truth values depend solely on reference, and 2) reference may be considered independently of (intensional) meaning. In considering this proposal, Kuhn focused on Putnam's views, which dealt more explicitly with issues of scientific development.

As discussed earlier, the causal theory of reference was developed for determining the referents of proper names and involved an initial act of "dubbing," such that reference might be established by tracing the lifeline of the individual back to that original, historical act. Applying the theory to determination of referents for natural kinds required a modification of this approach because (from the perspective of the subject-object distinction,) a "natural family" does not possess a readily identifiable life-line in the same sense as an individual substance. Kuhn proposed that this challenge might be addressed by examining multiple exemplars, which examination would indicate the feature space and provide knowledge of salience required for reference determination. Identification of these exemplars would represent an act of "historical" dubbing; however, this dubbing would occur in the (historical) period in which the referent was to be determined. Multiple acts of dubbing thus were possible over different historical periods, and the potential consequences of "redubbing" could be identified by examining (any) changes in the associated exemplary referents.

Putnam (along with other "causal theorists") suggested an alternative approach. Rather than relying on multiple acts of dubbing, he followed the original causal theorists in relying upon an "original act" of dubbing to establish an original sample, along with a primitive relationship, sameness-of-kind. He then traced the relationship between the term and its sample as a "lifeline" that might be followed in order to determine reference:

> Thus, some samples of a naturally occurring yellow, malleable metal were once baptized "gold" (or some equivalent in another language), and the term has since referred to all samples of the same stuff as the original whether they displayed the same superficial qualities or not. . . . Theories about what makes the samples the same are, on this view, irrelevant to reference, as are the

[184] See (MT 1977/2000) and (RTC 1983/2000).

> techniques used in identifying further samples. Both may vary over time as well as from individual to individual at a given time. But the original samples and the relation sameness-of-kind are stable. If meanings are the sorts of things that individuals can carry around in their heads, then meaning does not determine reference. (PW 1986/2000, 79)

On this account, the reference of a natural kind term is not affected by changes in the "meaning" of the term or even in the characteristics that render those terms "the same." What is important is only the connection between the original name and the original sample, as these are traced historically. When traced in this way, modern science may then be applied to specify the common essence of the referents. In this respect, reference determination can be combined with the full knowledge of modern science, avoiding the challenges (and, for determination purposes, the irrelevancies) posed by historical changes of meaning or categorization.

As we might expect, Kuhn challenged this view, arguing that changes of meaning are consequential for science in ways that are not recognized by emphasis on reference alone. Examining Putnam's proposal that referents of the term "water" as best determined by identifying samples of (what modern science identifies as) H_2O, Kuhn explained that according to this view:

> The extension of "water" is determined by the original sample together with the relation sameness-of-kind. That sample dates from before 1750 and the nature of its members has been stable. So has the relation sameness-of-kind, though *explanations* of what it is for two bodies to be of the same kind have varied widely. What matters, however, is not explanations but what gets picked out, and identifying samples of H_2O is, according to causal theory, the best means yet found to pick out samples of the same kind as the original set. Give or take a few discrepancies at the margins, discrepancies due to refinement of technique or perhaps to change of interest, "H_2O" refers to the same samples that "water" referred to in either 1750 or 1950. Apparently causal theory has rendered the referents of "water" immune to changes in the concept of water, the theory of water, and the way samples of water are picked out. (PW, 81)

While similar analyses seemed to be appropriate for determination of "gold" as "having atomic number 79," Kuhn proposed that in the case of "water," "difficulties arise:"

> "H_2O" picks out samples not only of water but also of ice and steam. H_2O can exist in all three states of aggregation – solid, liquid, and gaseous – and it is therefore not the same as water, at least not as picked out by the term "water" in 1750. The difference in items referred to is, furthermore, by no means marginal, like that due to impurities for example. Whole categories of substance are involved, and their involvement is by no means accidental. (PW, 81-2)

It would seem, then, that identifying samples of H_2O is not sufficient for determining the proper referents of "water." Yet modifying the means of identification to specify "liquid H_2O" is to expand reference determination from a single essential property to multiple properties, and thus to return to the problems that causal theory was designed to resolve.

In considering this case, Kuhn proposed that the lexicon required to identify essential properties is "rich and systematic," including not only essential properties but also a number of "so-called superficial properties [which] are no less necessary than their apparently essential successors:"

> To say that water is liquid H_2O is to locate it within an elaborate lexical and theoretical system. Given that system, as one must be in order to use the label, one can in principle predict the superficial properties of water . . ., compute its boiling and freezing points, the optical wavelengths which it will transmit, and so on. If water is liquid H_2O, then these properties are necessary to it. If they were not realized in practice, that would be a reason to doubt that water really was H_2O. (PW, 83)

In arguing for the importance of superficial properties, Kuhn insisted that just because a particular property is superficial does not mean that it is contingent (PW, 84). He noted that in a theory that posits essential properties must also be able to predict (at least some) superficial properties, and proposed that superficial properties are often relied upon for discriminating among the various kind terms that are comprehended by new theories.[185]

Considering the transformations of "natural kind terms" throughout the history of science, Kuhn noted that while the terms themselves often remain the same, "the changes in membership of the sets of items to which those enduring terms refer are often massive, and they affect not just the referents of an individual term but of an interrelated set of terms between which the preexisting population is redistributed" (PW, 85). Such changes create a divide between the referents of the term, as it is applied before and after the change, and these changes in referents – as with "water" and "H_2O" can be consequential for science. By failing to recognize the consequences of these changes, causal theory thus falls short.

In a revision of this essay, entitled "Dubbing and Redubbing: The Vulnerability of Rigid Designation" (DR 1990), Kuhn praised the important advances that provided by causal theory yet also noted that the examples chosen from scientific development "quite regularly fail in ways that are both consequential and illuminating" (DR, 309). Drawing parallels between the discussion of acquiring the language of Newtonian mechanics and the considerations of the causal theory, Kuhn noted two important similarities:

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[185] Kuhn noted that what is at issue in all of these situations is how proper boundaries may be established for natural kinds and pointed out that similar issues arise in determining individual members of the "same" species (PW 1986/2000, 84n).

> First, the role played by actual examples in anchoring terms of the lexicon to the world recurs in causal theory's emphasis on an original act of dubbing, which supplies canonical examples for later use. Second, the emphasis on language learning – on the way the lexicon is transmitted from one generation to the next – is duplicated in causal theory's emphasis upon the chain linking later users of a term to the canonical sample. (DR, 314)

Kuhn proposed that these similarities are particularly important because they highlight the central point of difference between his own views and those proposed by causal theorists:

> Dubbing is here seen as a process that recurs again and again through history. The putative "original sample" may mark the beginning of the chain, if any beginning is needed, but there is nothing privileged about its membership. The sets of canonical examples used in transmitting the lexicon change in the course of time, and not all the changes can properly be viewed as mere adjustments. (Ibid.)

In these statements, we can see Kuhn's assertion of the consequence of meaning change and the value of recognizing the adjustments that occur through "redubbing" of the samples over time.

In considering these changes, Kuhn once again insisted that acts of redubbing are influential not only with respect to their associated samples but also in relation to the larger lexicon of which those samples are a part:

> In short, dubbing and the procedures that accompany it ordinarily do more than place the dubbed object together with other members of its kind. *They also locate it with respect to other kinds, placing it not simply within a taxonomic category but within a taxonomic system*. Only while that system endures do the names of the kinds it categorizes designate rigidly.
>
> A lexicon that embodies such relationships between terms necessarily also embodies knowledge of the world those terms can be used to describe, and that knowledge may be placed at risk. (DR, 314-5, emphasis added)

In considering the act of dubbing that establishes the language-nature link, we must thus recognize that it not only categorizes but also systematizes. In this respect, each act of redubbing is not simply an addition or refinement to established categories but is, potentially, a threat to the established taxonomic system:

> As time goes on and new demands are placed on the lexicon, conditions may be encountered that defy description. The Newtonian lexicon could not, without internal contradiction, describe a world in which both Newton's second law and the law of gravity were violated. Prior to the eighteenth century, the lexicon of chemistry could not provide a coherent description of a world in which a sample of liquid could change its state of aggregation without simultaneously changing its chemical kind. But these changes, both in mechanics and in chemistry, nevertheless came about, together with the change of lexicon they required. (DR, 315)

In considering the implications of this redubbing, we must thus recognize that changes in the way that samples are identified may lead to changes in taxonomic categories and even in taxonomic systems. "Rigid designation" of scientific terms (as proposed by causal theorists) thus must be understood as vulnerable to the consequential changes that occur in the course of scientific development.

Section Three

Interrelations and Implications

The series of articles, addresses, and interviews emerging from the 1980s until Kuhn's death in 1996 indicate the gradual development of his views for the book on incommensurability. Among the most important aspects of this development was a growing awareness of an interrelation among his conceptions of the knowledge of nature that is acquired through language learning; the structural change that is revealed by incommensurability; and the distinctive insights of an historical or developmental perspective. Although Kuhn did not specify the interrelation of these concepts explicitly, the nature, import and implications of this interrelation emerge from his discussions of kind terms in the lexical structure, his insistence on the cognitive bite of incommensurability, and his characterization of his views as "post-Darwinian Kantianism."

Kind Terms, Lexicons, and the Lexical Structure

The lexicon and change in the lexical structure emerged as central points of explanation in Kuhn's attempts to elucidate the notion of incommensurability. Yet in his initial discussions of the theory of the lexicon, he did not clearly delineate either the elements that constituted a lexicon; the boundaries that defined it; or the relation that it may have shared with other lexicons. In his paper, "Commensurability, Comparability, Communicability" Kuhn introduced the theory of the lexicon in order to explain the role of intensionality in reference determination and thus to explicate the notion of incommensurability (CCC 1982/2000, 52). He characterized the referents of theoretical terms as nodes in the lexical network that might be identified by individuals using different criteria, yet he also proposed that the referents and the structure itself could change.

Four years later, in "Possible Worlds in History of Science," Kuhn applied the theory of the lexicon to explain the emergence of so-called "new worlds." In this case, however, he noted that his concern was not simply with the lexicon, terms, and statements, but "with conceptual or intensional categories more generally, e.g., with those which may be reasonably attributed to animals or to the perceptual system" (PW 1986/2000, 60n). In his speaker's reply to commentary on the paper, Kuhn introduced a distinction between the lexical structure of conceptual categories shared by a community and the individual lexicons used by its members, emphasizing the importance of "separating the concepts appropriate to the description of groups from those appropriate to the description of individuals." (Ibid., 89). In particular, he noted that the processes by which a community undergoes conceptual change must be recognized as different from those experienced by an individual:

> Communities do not have experiences, much less gestalt switches. As the conceptual vocabulary of a community changes, its members may undergo gestalt switches, but only some of them do and not all at the same time. Of those who do not, some cease to be members of the community; others acquire the new vocabulary in less dramatic ways. Meanwhile, communication goes on, however imperfectly, metaphor serving as a partial bridge across the divide between an old literal usage and a new one. To speak, as I repeatedly have, of a community's undergoing a gestalt switch is to compress an extended process of change into an instant, leaving no room for the microprocesses by which the change is achieved. (PW, 88)

As a point of departure for further investigation and clarification of these distinctions, Kuhn referenced his statement in the original essay that, "Reference is a function of the shared structure of the lexicon but not of the varied feature-spaces within which individuals represent that structure" (PW, 77n). In order to understand the determination of reference and the problem of meaning to the fullest extent, then, one must distinguish between the conceptual structures of the community (which establish reference) and the conceptualizing activities of its individual members (which reflect criteria or meaning):

> A number of classical problems of meaning, I am suggesting, may be seen as a product of the failure to distinguish between the lexicon as a shared property constitutive of community, on the one hand, and the lexicon as something carried by each individual member of the community, on the other. (PW, 89)

In making this statement, Kuhn returned – albeit in a clearer and more precise manner – to the sociological dimensions of scientific activity, positing the relationship between the community and its members as the basis for understanding the complexities of conceptual change. Even with this distinction, however, substantial issues remained regarding the nature, characteristics, and interrelation of (what he later came to call) the community's lexical structure and the lexicons of its members.

In "Afterwords" (AW 1990/2000, 224-252), Kuhn's reply to papers presented at a May 1990 conference in his honor, he characterized the lexicon as "the module in which members of a speech community store the community's kind terms" (AW, 229). Referring to his initial discussions of the lexicon as populated by natural kinds, he noted that what is needed to resolve the problems posed by incommensurability, "is a characteristic of kinds and kind terms in general" (Ibid.). In considering the implications of expanding the lexicon to include all conceptualizations of kinds and kind terms, Kuhn linked the lexicon's operation as a "mental module" of kind terms to the neural mechanisms that are used in reidentifying substances:

> In the book [on incommensurability], I will suggest that this characteristic [of kinds and kind terms in general] can be traced to, and on from, the evolution of neural mechanisms for reidentifying what Aristotle called "substances:" things that, between their origin and demise, trace a lifeline through space over time. What emerges is a mental module that permits us to learn to recognize not only kinds of physical object (e.g., elements, fields, and forces), but also kinds of furniture, of government, of personality, and so on. (Ibid.)

Thus to the extent that the lexicon is considered to be a mental module of kind terms, those terms have their basis in the reidentification of spatio-temporal "substances," in the broadest sense of that term.

Responding to a paper by Ian Hacking that explored the "kinds" that were included in Kuhn's lexicon (Hacking 1993), Kuhn noted that the "required generality" of his position prohibited his adoption of a nominalist position, namely, that "there are real individuals out there, and we divide them into kinds at will" (AW, 229). More specifically, he asserted, "I need a notion of 'kinds,' including social kinds, that will populate the world as well as divide up a preexisting population" (Ibid.). Furthermore, he noted that his position required aspects of a theory of meaning that could support "talk of change in concepts and their names, in conceptual vocabulary and in the structured conceptual lexicon that contains both kind concepts and their names" (Ibid.). While not identified directly, Kuhn required a notion of "kinds" (and "kind terms") that served a dual function (both definitional and legislative) in its substantive and its linguistic aspects.

In attempting to account for his conception of the lexicon and the way in which it might support these stated requirements, Kuhn identified three sets of properties that must be possessed by kind terms within the lexicon. First, he enumerated four essential elements, drawn from Peter Hempel's work on concept formation:

> Kind terms are learned in use: someone already adept in their use provides the learner with examples of their proper application. Several such exposures are always required, and their outcome is the acquisition of more than one concept. By the time the learning process has been completed, the learner has acquired knowledge not only of the concepts but also of the properties of the world to which they apply. (AW, 230)

This description echoes much of Kuhn's earlier discussion of how students learn the Newtonian lexicon, namely, that a student must be exposed to multiple examples that result in the acquisition of multiple concepts. Through this learning process (and, implicitly, given the nature of the language to be learned), the student acquires not only the appropriate conceptual vocabulary but also appropriate conceptions (or knowledge) of nature.

The second shared property of kind terms outlined by Kuhn was their projectibility: "to know any kind term at all is to know some generalizations satisfied by its referents and to be equipped to look for others" (Ibid.). In discussing these generalizations, he distinguished those that are normic or admit exceptions (e.g., "liquids expand when heated," which fails for water between 0 and 4 degrees centigrade), from those that are nomic, or exceptionless (usually characterized as natural laws). While both types of generalizations are learned as interrelated sets of kind terms, the basis of those relations differ, resulting in different methods for learning the terms and different functions within the lexicon. Normic generalizations, which comprise "the most populous part of the lexicon" (AW, 239), are learned and applied as contrast sets:

> To learn the term "liquid," for example, as it is used in contemporary nontechnical English, one must also master the terms "solid" and "gas." The ability to pick out referents for any of these terms depends critically upon the characteristics that differentiate its referents from those of the other terms in the set, which is why the terms involved must be learned together and why they collectively constitute a contrast set. (AW, 230)

Nomic generalizations, such as Newton's second law, are interrelated not as contrasting terms but as the terms associated with a law of nature.[186] They thus can be learned only by studying exemplars of the law-governed situations in which they occur.

As the third characteristic of kind terms, Kuhn proposed that,

> . . . the expectations acquired in learning a kind term, though they may differ from individual to individual, supply the individuals who have acquired them with the meaning of the term. Changes in expectations about a kind term's referents are therefore changes in its meaning, so that only a limited variety of expectations may be accommodated within a single speech community. (AW, 231)

Expanding his earlier discussions of incommensurability, Kuhn considered the implications of differences in expectations among the members of a community. To the extent that two individuals have "compatible expectations"[187] about the referents of a shared term, they will pick out the same items using different

[186] As outlined in Kuhn's discussion of how the Newtonian lexicon is learned, the law itself may be learned along with its terms.

[187] Later in the essay, Kuhn provided a more specific discussion of situations involving compatible and incompatible expectations:

> Full communication between community members requires only that they refer to the same objects and situations, not that they have the same expectations about them. The ongoing process of communication which unanimity in identification permits allows individual community

criteria and may be able to learn more about the referent by expanding their knowledge of its possible criteria. Yet if individuals have "incompatible expectations," that is, they (occasionally) identify different referents, "communication is then jeopardized, and the jeopardy is especially severe because, like meaning differences in general, the difference between the two cannot be rationally adjudicated" (Ibid.). In such situations, Kuhn proposed, ". . . it is only with respect to social usage that either of them can be said to be right or wrong. What they differ about is, in that sense, convention rather than fact" (Ibid.).

Examining the standard approach to such situations in analytic philosophy, Kuhn considered the proposals that the differences may be characterized as a case of polysemy, that is, the individuals apply the same name to different concepts. In such cases, two names are introduced to distinguish the two (different) concepts. Yet Kuhn insisted that this solution fails to resolve the difficulties inherent to situations involving kind terms because it implies that the differences are purely semantic when they are, instead, differences in expectations about "matters of evidence and fact:"

> Calling an item in the overlap region "water1" induces one set of expectations about it; calling the same item "water2" induces another, partly incompatible set. Both names cannot apply, and which to choose is no longer about linguistic conventions but rather about matters of evidence and fact. And if the matters of fact are taken seriously, then in the long run only one of the two terms can survive within any single language community. (AW, 232)

Offering his own proposal, Kuhn suggested that when a language community employs overlapping kind terms, "either one entirely displaces the other or the community divides into two, a process not unlike speciation and one that . . . is the reason for the ever-increasing specialization of the sciences" (AW, 232-3).

Kuhn explained the need for this displacement or division – and the "problem" of new or different worlds that it implies – as the result of the constitutive role of kind terms in a community's conceptualization of and communication about the world:

> Kind terms supply the categories prerequisite to description of and generalization about the world. If two communities differ in their conceptual vocabularies, their members will describe the world differently and make different generalizations about it. Sometimes such differences can be resolved by importing the concepts of one into the conceptual vocabulary of the other. But if the terms to be imported are kind terms that overlap kind terms already in place, no importation is possible, at least no importation which allows both terms to retain their meaning, their projectibility, their status as kind terms. Some of the kinds that populate the worlds of the two

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> members to learn each others' expectations, making it likely that the congruence of their bodies of expectations will increase with time. But though the expectations of individual members need not be the same, success in communication requires that the differences between them be heavily constrained. (AW 1990/2000, 239)

> communities are then irreconcilably different, and the difference is no longer between descriptions but between the populations described. Is it, in these circumstances, inappropriate to say that the members of the two communities live in different worlds? (AW, 233)

A community's kind terms thus establish the categories by which a community *both* describes and generalizes its world. When differences arise in the kind terms (or conceptual vocabularies) of two communities (or of members within a community), those differences may reflect differences in description or in generalization. In the first case, descriptive categories may be expanded or augmented without negative effect. Yet in the second case – when differences emerge in generalizations about the world and the relations among its categories – descriptive adjustments alone cannot bridge the gap.

In making the proposal that scientific communities can – and implicitly, should – operate in different worlds over time, these statements present a direct challenge to traditional conceptions of realism. As a reiteration of *Structure's* controversial proposal that scientists on either side of a scientific revolution operate in different worlds, Kuhn noted that they also raise the spectre of relativism once again. In responding to these issues and outlining his views and objectives, Kuhn proposed that while the criteria used to evaluate scientific beliefs are epistemic, the assertions of those beliefs are not:

> My goal is double. On the one hand, I aim to justify claims that science is cognitive, that its product is knowledge of nature, and that the criteria it uses in evaluating beliefs are in that sense epistemic. But on the other, I aim to deny all meaning to claims that successive scientific beliefs become more and more probable or better and better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or "external" world. (AW, 243)

While asserting the cognitive authority of scientific practice[188] and the epistemic status of scientific criteria, Kuhn thus rejected claims that the authority of particular scientific beliefs or claims can be based on a correspondence theory of truth.

Linking these considerations with his theory of the lexicon, Kuhn proposed that although a community's lexical structure provides the basis for the assertion and the justification of truth claims, the structure itself cannot be justified on the same "rational" basis:

> A lexicon or lexical structure is the long-term product of tribal experience in the natural and social worlds, but its logical status, like that of word meanings in general, is that of convention. Each lexicon makes possible a corresponding form of life within which the truth or falsity of

[188] This assertion requires a more specific argument regarding the means by which criteria are applied in the practice of science and the necessity for scientific practice to apply those criteria in that particular way. In making this argument, Kuhn returned to the notion of science as a puzzle-solving enterprise (AW 1990/2000, 251). The details of this discussion will be examined subsequently.

> propositions may be both claimed and rationally justified, but the justification of lexicons or lexical change can only be pragmatic. (AW, 244)

Noting that traditional criteria such as simplicity play an important role in scientific choice, he proposed that they are "instrumental rather than epistemic where lexical change is involved" (Ibid.). More specifically, traditional criteria are instrumental with respect to the puzzle-solving activities that constitute scientific practice and it is these activities that provide the linkage between scientific activity and the "real world:"

> . . . what, if not a match with external reality, is the objective of scientific research? Though I think it requires additional thought and development, the answer supplied in *Structure* still seems to me the right one: whether or not individual practitioners are aware of it, they are trained and rewarded for solving intricate puzzles – be they instrumental, theoretical, logical or mathematical – at the interface between the phenomenal world and their community's beliefs about it. (AW, 251)

In this account, the "community's beliefs" about the phenomenal world can be understood as the lexical structure that contains their kind terms, that is, their descriptions and generalizations about the world. Scientific puzzle-solving – like scientific communication – relies upon this common lexical structure yet also presents it with challenges, typically in the form of anomalies, failed expectations, or problems in translation and communication.

While still implicit in these statements, lexical change can be understood as a change in the basis for scientific puzzle solving. The transition from one lexicon to the next thus does not reflect a movement toward greater truth. Neither does it suggest that scientific truth is merely relative. Instead, lexical change represents the choice of an alternative basis for scientific puzzle-solving.

Given these proposals, we must reconsider the status of kinds and kind-terms within the lexical structure. More precisely, we must reconsider the status of "laws of nature," that is, the nomic kinds that are interrelated in virtue of a particular type of situation and which hold without exception. While their identification as "laws" implies a certain epistemic status, Kuhn's discussions suggest that this status is limited to a particular lexical structure and that there is some degree of flexibility for creative judgment within that structure. This structure is the interrelation of the terms and the laws themselves, combined with the exemplary situations from which they are developed. While such laws also rely upon antecedently available terms to connect them to the world and to support the acquisition process, it is their interrelation that constitutes their ability to describe and to generalize about the world. To the extent that this inherent interrelation is rendered problematic, the terms and their associated laws will become incoherent or at least, increasingly will be drawn into question.

These clarifications provide greater insight into Kuhn's attempt to address concerns of the relativism implied by his views:

> The point is not that laws true in one world may be false in another but that they may be ineffable, unavailable for conceptual or observational scrutiny. It is effability, not truth, that my view relativizes to worlds and practices. (AW, 249)

The "natural laws" of one world may be "ineffable" in another in that they may fail to provide the combination of descriptive and legislative guidance that is required for scientific puzzle-solving in that particular world. To the extent that lexical change presents a choice among two different worlds, the basis upon which *scientists* will make that choice will be the relative puzzle-solving ability that each world makes possible. While individual scientists may differ in their evaluations, *as scientists*, their criteria for making that choice will be unequivocal:

> Accuracy, precision, scope, simplicity, fruitfulness, consistency, and so on, simply *are* the criteria by which puzzle solvers must weigh in deciding whether or not a given puzzle about the match between phenomena and belief has been solved. Except that they need not all be satisfied at once, they are the "defining" characteristics of the solved puzzle. . . . Deployed by trained practitioners, these criteria, whose rejection would be irrational, are the basis for the evaluation of work done during periods of lexical stability, and they are basic also to the response mechanisms that, at times of stress, produce speciation and lexical change. (AW, 251-2)

In understanding lexical change, we must thus understand it within the context of the scientific enterprise. Challenges of relativism are addressed by the application of standard criteria for puzzle-solving. Challenges of realism are addressed by the conception of puzzle-solving as occurring "at the interface of the phenomenal world and [the] community's beliefs about it" (AW, 251). In accepting Kuhn's response to these challenges, however, we must ultimately confront their underlying premise: that nature of science and the scientific enterprise is puzzle-solving.

The Cognitive Bite of Incommensurability

Kuhn's final published papers addressing the notion of incommensurability were "The Road Since Structure" (RSS 1990/2000, 90-104), his Presidential Address to the biennial meetings of the Philosophy of Science Association in October 1990, and "Remarks on Incommensurability and Translation" (RIT 1999), informal remarks prepared for a conference on translation. In each of these presentations, Kuhn traced the history of his interest in and investigations of incommensurability. He also discussed, at varying lengths, the central concepts and concerns included in early drafts of his book on the subject.

Kuhn introduced "The Road Since Structure," his presidential address to the Philosophy of Science Association, as "an exceedingly brief and dogmatic sketch" of the "main themes" of his book on incommensurability (RSS, 90). In describing the book, however, he noted that its scope extended beyond the notion of incommensurability and even beyond the philosophical issues raised by *Structure* to encompass the issues raised by the transition to the historical philosophy of science. Among the issues addressed by the book, Kuhn included "rationality, relativism, and, most particularly, realism and truth" (RSS, 91). While describing these as the "main targets" of the book, Kuhn explained that its primary focus was incommensurability:

> No other aspect of *Structure* has concerned me so deeply in the thirty years since the book was written, and I emerge from those years feeling more strongly than ever that incommensurability has to be an essential component of any historical, developmental, or evolutionary view of scientific knowledge. Properly understood – something I've by no means always managed myself – incommensurability is far from being the threat to rational evaluation of truth claims that it has frequently seemed. Rather, it's what is needed, within a developmental perspective, to restore some badly needed bite to the whole notion of cognitive evaluation. It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of postmodernist movements like the strong program. (Ibid.)

Although these striking assertions were introduced at the beginning of this chapter, we now have a broader perspective from which to evaluate Kuhn's characterization of incommensurability as providing "some badly needed bite" to cognitive evaluation within a developmental perspective. We thus turn to his comments, noting the irony and perhaps, the appropriateness, of their presentation as the presidential address to members of the Philosophy of Science Association.

The Changes Revealed by Incommensurability

Kuhn traced his interest in incommensurability back to his work in the history of science, specifically, to his experiences in trying to understand non-sensical passages in out-of-date scientific texts:

> Ordinarily, they had been taken as evidence of the author's confused or mistaken beliefs. My experiences led me to suggest, instead, that those passages were being misread: the appearance of nonsense could be removed by recovering older meanings for some of the terms involved, meanings different from those subsequently current. (RSS, 91)

In attempting to account for the change in the meanings of scientific terms that historical study revealed, Kuhn explained that he initially described it as language change and characterized the historian's ability to recapture older meanings as a process of language learning, rather than translation, even in its "radical" form.

In reflecting on these earlier views, Kuhn noted that in his current view, the language metaphor seemed "far too inclusive:"

> To the extent that I'm concerned with language and with meanings at all – an issue to which I'll shortly return – it is with the meanings of a restricted class of terms. Roughly speaking, they are taxonomic terms or kind terms, a widespread category that includes natural kinds, artifactual kinds, social kinds, and probably others. (RSS, 92)

Kuhn specified two essential properties of kind terms:

> First. . . they are marked or labeled as kind terms by virtue of lexical characteristics like taking the indefinite article. Being a kind term is thus part of what the word means, part of what one must have in the head to use the word properly. Second – a limitation I sometimes refer to as the no-overlap principle – no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus. (Ibid.)

Kuhn thus condensed his earlier list of requirements – that they populate the world and divide up a preexisting population and that they be expressible in terms of a conceptual vocabulary and a structured conceptual lexicon (AW 1990/2000, 229) – to the first essential property: that the kind term "be" a kind term. The second essential property – the no-overlap principle – may be traced back to his rejection of analytic philosophers' solution to polysemy (AW, 232).

With these requirements for kind terms specified, Kuhn proposed that a shared lexical structure of kind terms is a prerequisite for communication:

> Notice now that a lexical taxonomy of some sort must be in place before description of the world can begin. Shared taxonomic categories, at least in an area under discussion, are prerequisite to unproblematic communication, including the communication required for truth claims. If different speech communities have taxonomies that differ in some local area, then members of one of them can (and occasionally will) make statements that, though fully meaningful within that speech community, cannot in principle be articulated by members of the other. To bridge the gap between communities would require adding to one lexicon a kind term that overlaps, shares a referent, with one that is already in place. It is that situation which the no-overlap principle precludes. (RSS, 93)

Although these statements closely parallel Kuhn's earlier discussions of lexical change (AW, 233), they introduce repeated reference to taxonomies. Such references emphasize the status of kind terms as a special type of linguistic "marker." That is, they highlight the essential property that kind terms "be" kind terms, that is, that they serve as (lexical) markers of (taxonomic) kinds.

What is important, then, is not simply the lexical structure itself but the fact that it is a lexical structure *of kind terms*, that is, a lexical taxonomy. It follows, from Kuhn's essential properties of kind terms, that description of the world is not possible without "some sort" of lexical taxonomy, that is, some way that the members of a language community can collectively identify and refer to the "kinds" that constitute their (individual experiences of the) world. When attempts to communicate about some portion of the world become problematic – that is, when (localized) incommensurability is encountered – the difficulties may reflect differences in the lexical taxonomy.

Incommensurability and the Idea of a Conceptual Scheme

In characterizing his views on incommensurability as concerned with words and with *lexical* taxonomy, Kuhn noted that a more refined characterization would extend to concepts, rather than words:

> What I have been calling a lexical taxonomy might, that is, better be called a conceptual scheme, where the "very notion" of a conceptual scheme is not that of a set of beliefs but of *a particular operating mode of a mental module prerequisite to having beliefs, a mode that at once supplies and bounds the set of beliefs it is possible to conceive*. Some such taxonomic module I take to be prelinguistic and possessed by animals. Presumably it evolved originally for the sensory, most obviously for the visual, system. In the book I shall give reasons for supposing that it developed from a still more fundamental mechanism which enables individual living organisms to reidentify other substances by tracing their spatio-temporal trajectories. (RSS, 94)

In this reference to Donald Davidson's essay, "The Very Idea of a Conceptual Scheme" (1974), Kuhn rejected Davidson's characterization of his position as relying upon a set of beliefs and proposed instead "a particular operating mode of a mental module prerequisite to having beliefs." While Kuhn's response can hardly be considered to be a clarification of his position, it merits further consideration given the widespread influence of the views articulated in Davidson's paper; Kuhn's insistent rejection of Davidson's attempts to undercut incommensurability; and the clear importance of the proposal to Kuhn's (developing) position.

Kuhn introduced the statements above by proposing that the "lexical taxonomy" might more appropriately be called a conceptual scheme. In this respect, we recall his clarification in "Possible Worlds" that his interest in the lexicon, terms, and statements reflected a concern with "conceptual or intensional categories more generally, e.g., with those which may be reasonably attributed to animals or to the perceptual system" (PW 1986/2000, 60). This position was clarified further through Kuhn's later response to Hacking and the expansion of his considerations from natural kinds to “kinds and kind terms in general” (AW 1990/2000, 229). Later in that essay, Kuhn summarized his position regarding the role of kind terms in the lexicon:

> Kind terms supply the categories prerequisite to description of and generalization about the world. If two communities differ in their conceptual vocabularies, their members will describe the world differently and make different generalizations about it. Sometimes such differences can be resolved by importing the concepts of one into the conceptual vocabulary of the other. But if the terms to be imported are kind terms that overlap kind terms already in place, no importation is possible, at least no importation which allows both terms to retain their meaning, their projectibility, their status as kind terms. Some of the kinds that populate the worlds of the two communities are then irreconcilably different, and the difference is no longer between descriptions but between the populations described. (AW, 233)

This passage is particularly important because it presents Kuhn’s argument against Davidson’s proposal that incommensurability can be addressed by addition of terms: if the imported (kind) terms overlap kind terms that are already in place, that importation will draw into question the status of both sets of terms *as kind terms*.

On this account, it is central to the processes involved in a community’s description of and generalization about the world that the terms (of the lexicon, lexical taxonomy, or conceptual scheme) whereby the world is described or characterized are *kind terms*. These are not simply sets of beliefs. Nor are they simply markers or labels attached to a collection of individuals by an original, historical act of dubbing. They are the kind terms of the community. Collectively, they form the basis for communication among the members of the community and, in some respects, for their shared activities. They are the *means by which* the community describes and generalizes about its world. Even further, they are *prerequisite* to such description and generalization.

Yet, according to Kuhn, kind terms *do not correspond* to an objective, mind-independent world. They can – and sometimes do – change. When they do, the world that they describe and generalize changes as well. As kind terms, however, their change must be of a special sort. The change cannot be simply linguistic but must be interrelated with a change in knowledge of the world, that is, a change in its kinds. In response to Davidson, Kuhn insisted that it may not be appropriate to expand the vocabulary of kind terms in order to distinguish between two kinds previously identified by the same kind term. Whether conceived as a mental module, conceptual scheme, lexicon, or lexical taxonomy, kind terms are interrelated with other kind terms in ways that both support and constrain their use. Introducing a new kind term –

particularly if it overlaps an existing kind term – may disrupt these relationships in ways that threaten the integrity of the overall conceptual scheme. Understanding changes in kind terms thus requires understanding the relationships that they share with other kind terms. It requires understanding the conceptual or lexical-taxonomic structure within which they are placed and the sets of beliefs that are supplied by and bound within that structure.

Incommensurability and the Possibility of Enrichment

How, then, are kind terms linked with knowledge of the world, that is, with the kinds to which they would refer? How can the interrelationships among kind terms both support and constrain their use? Even further, how are kind terms established within a conceptual or lexical structure and what is the basis and authority of that structure?

To answer these questions, we turn briefly to Kuhn's "Remarks on Incommensurability and Translation" (RIT 1999), in which he described his attempts to block Davidson's position as providing a foundation for the viewpoint to be articulated in his book on incommensurability (RIT, 35). In outlining Davidson's position, Kuhn noted, first, that Davidson recognized Quine's radical translator as a language learner and agreed that "what he has learned cannot in its entirety be translated into the language he brought from home" (Ibid.). In these respects, then, Davidson and Kuhn would agree. Their difference lay, Kuhn proposed, in how this untranslatability may be resolved:

> . . . Davidson supposes, as I do not, that having come to understand how the presently understandable terms function in the newly acquired language, the language learner can enrich his native language by adding the missing words to it. Enrichment would then have eliminated incommensurability. (Ibid.)

Davidson's position thus would seem to undercut incommensurability and the associated changes of language, meaning, and lexical or conceptual structure that it implies. It thus presents an important challenge to Kuhn's views.

In considering the implications of Davidson's challenge, Kuhn articulated a question that had been central to his project for quite some time:

> . . . if Davidsonian enrichment were possible, that enriched language would project two incompatible images of the same areas of the same world, a consequence that would endanger the community which used it. That is what I take to be the case, but to make the position plausible one must show *how a language can embody knowledge of nature at all*. (Ibid., emphasis added)

Kuhn addressed this question by reconsidering both the processes involved in learning Newtonian mechanics and the cognitive status of the acquired terms and laws:

> There are . . . various different routes which the learning process may traverse, but all of them involve positing the validity of one or more universal generalizations ordinarily described as laws of nature. . . For present purposes the details of the route do not matter. What is crucial, however, is that the acquisition of conceptual vocabulary requires giving to some laws of nature a definitional role that makes their cognitive status like that of Kant's *synthetic a priori*. As other laws are discovered with the aid of those initially posited, they too inherit that cognitive status. Though none of them could exist in the absence of experience (whence synthetic *a priori* rather than *a priori* alone), their experiential and their definitional content are inseparably merged. It is laws that do or could enter the language acquisition process in that way that language thereafter projects back upon the world. (RIT, 35-6)

The processes involved in learning the Newtonian lexicon indicate that language can "embody nature" through the interrelationships that are established between acquired terms and laws and the exemplary situations that are required for their acquisition. The laws that are thereby established are both definitional and legislative. As such, Kuhn proposed, they have a cognitive status like that of Kant's *synthetic a priori*.

In comparing Kuhn's statements with his earlier discussion of how the Newtonian lexicon is acquired, it seems that the cognitive status of *synthetic a priori* should be attributed not simply to the laws but to the interrelated set of Newtonian terms and laws. As Kuhn pointed out in the detailed examination provided in "Possible Worlds," the specific epistemic status of the various Newtonian laws will vary, depending upon whether they are learned using the conception (and exemplars) of "inertial mass" or "gravitational mass:" "[o]n the first route the second law enters stipulatively, the law of gravitation empirically. On the second, their epistemic status is reversed. In each case one, but only one, of the laws is, so to speak, built into the lexicon" (PW 1986/2000, 71). Even if we attribute the status of *synthetic a priori* to both laws, it seems important to preserve some recognition of their relation to each other and the epistemic implications of the alternative paths by which they may be acquired.

As a result of the cognitive status of laws of nature (and those terms, laws, and examples that are associated with them), a change in the law will *necessarily* be accompanied by a change in the corresponding set of terms and examples.[189] Thus the transition from Newtonian to Einsteinian physics entailed not only a change in laws of nature but also a (subtle yet consequential) change in the associated terms and examples (PW, 74). While the terms of one law may be learned and distinguished from those of

[189] In terms of the inverse case, that is a change in the set of terms and examples, a change in the law may or may not be necessary, depending upon the extent to which the change influences the set of interrelations.

another, an Einsteinian vocabulary cannot be "enriched" with a Newtonian vocabulary because the underlying terms, laws, and examples of each view form an interrelated set that cannot withstand dramatic readjustments. In such cases, Davidson's proposal that enrichment eliminates incommensurability is not viable because enrichment is not possible within the constraints established by the lexical structure or conceptual scheme (i.e., the interrelated set of terms, laws, and examples) that characterizes either view.

While these considerations suggest that we may reject Davidson's position and assert the possibility of incommensurability *in the case of natural laws*, the scope of these claims must still be determined. We must also specify more clearly the basis on which two laws (or representatives of other potential cases) may be said to be "incommensurable." In his "Remarks," cited above, Kuhn considered kind terms that are acquired with natural laws and proposed that the constraints deriving from the interrelation of their terms and laws eliminated the possibility of enrichment in cases of untranslatability. In considering the basis for incommensurability, we might narrow this scope to encompass only those natural laws whose acquired terms overlap in some respects yet differ in others. Two laws may be untranslatable yet not necessarily possess overlapping terms, in which case enrichment would not threaten the interrelations established by either law.

On the other hand, we must also consider whether kind terms other than natural laws may present the possibility of incommensurability. In "Afterwords," Kuhn distinguished "nomic," or exceptionless, generalizations (i.e., natural laws) from the more common, "normic" generalizations (e.g., "liquid" or "solid"), which admit exceptions and are learned in contrast sets. While the discussions above suggest that changes in nomic generalizations, or natural laws will result in incommensurability, this is not necessarily the case for changes in normic generalizations. Enrichment might be possible in this second case because normic generalizations are acquired and structurally interrelated as contrasting terms that admit of exceptions. Situations thus may arise in which an untranslatable, "contrasting" kind term can be introduced into a different conceptual scheme without harm to the structure of other, contrasting terms.[190] On the other hand, because these are kind terms that embody knowledge of nature, there may well be some limits to the exceptions that they would admit, suggesting that incommensurability remains a possibility.

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[190] This is particularly likely to be the case in attempting to translate culturally constructed concepts from one language to another, for example, the phrase *lingua franca*.

From these considerations, we can more clearly examine the basis for incommensurability. First, we must revise Kuhn's earlier characterization of incommensurability as untranslatability to specify that it is untranslatability without the possibility of enrichment. Secondly, we must understand enrichment as limited with respect to kind terms whose referents overlap in areas that do not admit of exception. We are left, then, with a much narrower conception of incommensurability as applicable to:

1) Alternative sets of natural laws whose acquired terms differ yet overlap with respect to some portion of the law; and
2) Alternative sets of contrasting kind terms whose terms differ yet overlap in ways that do not admit of (proposed) exceptions.

In these cases, a change in the generalization or in acquired kind terms will result in incommensurability. Enrichment will not be possible within the context of either generalization, given the distinctive and overlapping interrelations between each of the generalizations and their acquired terms.

Although this conception of incommensurability is narrower than the one(s) that Kuhn articulated previously, it is also more precise. As such, it provides a clear indication of the types of change that result in incommensurability, namely, changes in the underlying structures of the natural laws and kind terms that are used in descriptions and generalizations about the world. It indicates a particular type of change in kind terms that is accompanied by a change in the associated referents. As thus conceived, incommensurability indicates a structural change in our knowledge of nature that is not only linguistic but also substantive.

Incommensurability within a Developmental Perspective

With this refined conception of incommensurability in mind, we return to Kuhn's assertion that incommensurability is "what is needed, within a developmental perspective, to restore some badly needed bite to the whole notion of cognitive evaluation" (RSS 1990/2000, 91). In clarifying this claim, he outlined the developmental framework within which incommensurability functions, sketching two elements of the form to be taken by "any viable evolutionary epistemology" (RSS, 94). First, he proposed that,

> On the developmental view, scientific knowledge claims are necessarily evaluated from a moving, historically situated, Archimedean platform. What requires evaluation cannot be an individual proposition embodying a knowledge claim in isolation: embracing a new knowledge claim typically requires adjustment of other beliefs as well. Nor is it the entire body of knowledge claims that would result if that proposition were accepted. Rather, *what's to be evaluated is the desirability of a particular change-of-belief*, a change which would alter the existing body of knowledge claims so as to incorporate, with minimum disruption, the new claim as well. (RSS, 95-6)

In these statements, we can see the conclusions of Kuhn's earlier investigations of the interrelation of terms within the lexical taxonomy or conceptual scheme that is used to describe the world. The localized nature of incommensurability is also evident in the description of evaluation as limited to a particular change-of-belief (and its associated implications for the body of knowledge claims), rather than the body of knowledge claims *in toto*.

The second element of the developmental perspective examined by Kuhn was the specialization and speciation that is exhibited in the development of scientific knowledge over time. Recharacterizing his earlier distinction of normal and revolutionary development, he proposed that the distinction should more appropriately be made between development that does or does not require local taxonomic change. While stating that he did not seek to abandon his earlier notion of revolution entirely, Kuhn noted that the recharacterization supported "a significantly more nuanced description of what goes on during revolutionary change than I've been able to provide before" (RSS, 97). In particular, he emphasized that,

> After a revolution there are usually (perhaps always) more cognitive specialties or fields of knowledge than there were before. Either a new branch has split off from the parent trunk, as scientific specialties have repeatedly split off in the past from philosophy and from medicine. Or else a new specialty has been born at an area of apparent overlap between two preexisting specialties, as occurred, for example, in the cases of physical chemistry and molecular biology. (Ibid.)

Even among related fields, specialization becomes even more pronounced over time as each field acquires and develops its own specialist journals, professional societies, laboratories and even academic departments. In the process, each field gradually develops its own distinct lexicon. Although Kuhn noted that, "the differences are local, occurring only here and there," he insisted that "[t]here is no *lingua franca* capable of expressing, in its entirety, the content of . . . all [specialist fields] or even of any pair" (RSS, 98).

In considering the limitations imposed by specialization and the linkage between specialization and the development of knowledge, Kuhn noted that,

> With much reluctance, I have increasingly come to feel that this process of specialization, with its consequent limitation on communication and community, is inescapable, a consequence of first principles. Specialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools. (Ibid.)

On this account, specialization is concomitant with lexical or taxonomic divergence and results from the "principled limit" that lexical diversity imposes on communication (Ibid.). Yet while lexical diversity (or specialization) imposes limitations across different scientific communities, Kuhn insisted that it also serves to focus the efforts of individual scientific communities in a way that supports the development of knowledge:

> Lexical diversity and the principled limit it imposes on communication may be the isolating mechanism required for the development of knowledge. Very likely it is the specialization consequent on lexical diversity that permits the sciences, viewed collectively, to solve the puzzles posed by a wider range of natural phenomena than a lexically homogeneous science could achieve. (RSS, 98-9)

Thus despite its limitations on communications and community, Kuhn proposed that specialization is "the essential precondition for what is known as progress in . . . the development of knowledge" (RSS, 99).

At last we arrive at the role of incommensurability as "an isolating mechanism" that "reveal(s) the source of the cognitive bite and authority of the sciences" (Ibid.). Tracing Kuhn's two uses of the phrase "isolating mechanism" in the passages cited above, it is clear that it is "lexical diversity and the principled limit it imposes on communication" (RSS, 98) that "reveal the source of the cognitive bite and authority of the sciences" (RSS, 99). Based on our refined notion of incommensurability, we can understand the limits imposed by lexical diversity (i.e., the limits imposed by specialization) as resulting from the limits that are embedded in the language and vocabulary of natural laws.[191] Incommensurability thus provides an indication of the limiting differences that exist between two overlapping laws. As such, it reveals the way in which knowledge of nature is embedded within the language of natural laws. It is this language-embedded knowledge of nature and its associated limitations on scientific development that provide "the cognitive bite and authority of the sciences." We must still understand, however, the processes and authority by which science (and its various specialties) develop over time.

Kuhn's "Post-Darwinian Kantianism"

In the passage characterizing incommensurability as "what is needed, within a developmental perspective, to restore some badly needed bite to the whole notion of cognitive evaluation" (RSS 1990/2000, 91), Kuhn added, "[i]t is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of

[191] These limits to change result from the dual function of natural laws, whereby they not only legislate but also define (some of) the terms that they present. This definitional function limits the extent to which the laws (or the terms that are acquired with it) may change and also inhibits enrichment of its vocabulary by overlapping terms. It is thus in virtue of the unique, interrelated structure of natural laws (and contrasting kind terms) that enrichment of existing knowledge is rendered impossible and either displacement or specialization of knowledge occurs.

postmodernist movements like the strong program" (Ibid.). Reconsidering that statement in light of our investigations of incommensurability, we can identify the basis from which such a defense might be launched. Our investigations suggest it is the dual function of natural laws – the interrelated definitional and legislative structure by which the language of natural laws embodies knowledge of nature – that would form the basis of Kuhn's response to assertions of relativism and rejections of the prospects for truth and knowledge.

In making this claim, however, we must recognize that the "truth" and "knowledge" suggested by our dual-function generalizations differ from established conceptions. Kuhn consistently and continuously rejected the correspondence theory of truth; however, the alternatives that he proposed remained, at best, highly developmental. As his views on incommensurability were increasingly refined, however, he considered these remaining issues with greater attention. While his views on truth and knowledge were never fully articulated in published form, Kuhn's characterization of his position as one of "post-Darwinian Kantianism" is elucidated, albeit incompletely, in several of his final publications.

Describing his developing position as "a sort of post-Darwinian Kantianism," Kuhn considered the role of lexical structures and the processes involved in lexical change:

> Like the Kantian categories, the lexicon supplies preconditions of possible experience. But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another. None of those changes, of course, is ever vast. Whether the communities in question are displaced in time or in conceptual space, their lexical structures must overlap in major ways, or there could be no bridgeheads permitting a member of one to acquire the lexicon of the other. Nor, in the absence of major overlap, would it be possible for the members of a single community to evaluate proposed new theories when their acceptance required lexical change. Small changes, however, can have large-scale effects. The Copernican revolution provides especially well-known illustrations. (RSS, 104)

Presumably, Kuhn's reference to the Copernican revolution was not intended as an indirect reference to the "Copernican revolution" metaphorically attributed to Kant; however, his proposal is interesting and challenging in terms of both Kuhn's views regarding scientific development and Kant's views regarding the operation of pure reason. Perhaps even more illuminating is Kuhn's later characterization of himself as "a Kantian with moveable categories" (Baltas et al. 1995/2000, 264). It would seem that Kuhn would have his conception of a changing, empirically based lexical structure placed in (some as yet undefined) relation to Kant's hard-won and unchanging categories as providing the conditions for the possibility of experience. Yet even if Kuhn is able to establish the occurrence and the importance of incommensurability within a developmental perspective, must we go so far? On what epistemological basis could we (re-) conceive or (re-) establish the prospects for reason, knowledge or truth?

The Preconditions of Possible Experience

Kuhn's first proposal in the statement above is that, "like the Kantian categories, the lexicon supplies preconditions of possible experience." He introduced the first iteration of this proposal[192] in "Afterwords," summarizing the role of kind terms within the lexicon:

> Kind terms supply the categories *prerequisite to description of and generalization about the world*. If two communities differ in their conceptual vocabularies, their members will describe the world differently and make different generalizations about it. (AW 1990/2000, 233, emphasis added)

In "The Road Since Structure," a slightly different proposal followed Kuhn's delineation of the essential properties of kind terms. Rather than outlining the role of kind terms as a prerequisite to description and generalization to the role, this modified proposal posited shared taxonomic categories (presumably, identification of the "kinds" indicated by kind terms) as a prerequisite to "unproblematic communication:"

> Notice now that a lexical taxonomy of some sort must be in place before description of the world can begin [i.e., before kind terms can be applied successfully]. Shared taxonomic categories, at least in an area under discussion, are *prerequisite to unproblematic communication*, including communication required for the evaluation of truth claims. (RSS, 92-3, emphasis added)

In the same essay, while suggesting that a lexical taxonomy is more appropriately called a "conceptual scheme," Kuhn characterized it as "mental module prerequisite to having beliefs:"

> . . . the "very notion" of a conceptual scheme is not that of a set of beliefs but of a particular operating mode of a mental module *prerequisite to having beliefs*, a mode that at once supplies and bounds the set of beliefs it is possible to conceive" (RSS, 94, emphasis added)

In these various versions or modifications, we thus see a range of relationships and dependencies delineated: from the role of kind terms as a prerequisite to description and generalization; to shared taxonomic categories as prerequisite to unproblematic communication; and finally, to a "mental module" or "conceptual scheme" that is prerequisite to beliefs. The proposal that the lexicon supplies preconditions of *experience* – provides an even further expansion.

[192] While not explicitly addressed by Kuhn, this proposal seems to be a further development of his earlier assertion that paradigms supply the puzzle-solving framework for scientific activity.

Based on our earlier investigations, we must further modify these various proposals to assert that *natural laws* supply the preconditions of possible experience *for those parts of the world that they would describe or generalize*. While not as expansive as Kuhn's final proposal, this assertion is more precise, more defensible, and, perhaps surprisingly, more suggestive.

By limiting Kuhn's proposal to natural laws, we can defend his claim that they supply the preconditions of possible experience within the realm of their operation. As Kuhn outlined in detail with respect to Newton's Second Law, natural laws are dual-function generalizations, which are learned along with the terms and examples that they would govern. They thus possess a dual, definitional-legislative structure, such that within the realm in which they operate successfully (i.e., within the realm that is established and bounded by the interrelation of their terms, laws, and examples), natural laws support description and generalization; communication; belief; and even experience.

Within the context of their learning process (we must still ask about their discovery and justification), Kuhn claimed that natural laws have the cognitive status of Kantian *synthetic a priori* (AW, 71). In considering the extent to which this claim is appropriate, it is important to note that natural laws require antecedently available vocabulary (and in some cases, antecedently available laws) in order to establish a point of contact with pre-existing knowledge and experience.

While both the experiential and the cognitive scope of Kuhn's natural laws is more restricted than those of Kant's categories, the empirical and cognitive implications of their status as dual-function generalizations must be acknowledged and considered in detail. As we will discuss in the next chapter, the status of natural laws as *synthetic a priori* provides a basis from which to reconsider Hume's problem of induction with respect to the experiences and situations that are delineated by those laws.

Change Over Time and Across Communities

Kuhn's second proposal was that, "lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another" (RSS, 104). Limiting our examination to natural laws as above, the changes that occur over time and across communities may be understood as changes in natural laws, that is, in the set of interrelated terms, laws, and examples that (collectively) constitute a natural law, such that the law is no longer tenable.

The considerations involved in a change in natural laws are different and more fundamental than the considerations for changes in other parts of a lexical taxonomy or conceptual scheme. The interrelations that constitute natural laws provide both definitional and legislative structure, and in so doing, establish limitations for their application or extension. In the case of two incommensurable laws, the knowledge of nature that is embedded in the structure of interrelations constituting one law will be incompatible with that embedded in the (different) structure of the other law. This incommensurability cannot be addressed by enrichment because to do so would be to address only the definitional structure and to neglect the (integrally related) legislative structure. More specifically, enrichment would "build contradictions about observable natural phenomena *into language itself*," such that the community using the (wrongfully) enriched language "could not successfully deal with nature" (RIT, 36).

When faced with two incommensurable natural laws, a scientific community thus must choose either one or the other as the basis for their descriptions, generalizations, beliefs, and experiences. It is in virtue of the necessity of this choice that "natural laws" may change over time and across communities. What, then, establishes this necessity and prompts the development of an alternative natural law from which to choose?

In considering the function of incommensurability, Kuhn noted that it not only characterizes fundamental changes in scientific language but is endemic to all language. In the sciences, however, he proposed that it "shows with special clarity and special consequences:"

> The practitioners of an enterprise which, like any science, makes use of established laws of nature and aims to discover new ones must normally be able to say with precision when particular phenomena are within the domain of a law and when they are not. Failure of a phenomena within that range to conform to the law need not result in the law's rejection, *but it must be recognizable as a failure, as an anomaly to be accounted for*. Discrepancies which, in another field, could be tolerated or taken for granted play an essential role in the development of science. That is what makes incommensurability in the sciences so especially consequential. For the sciences, indeed, I take it to be constitutive. (Ibid.)

In this interesting yet somewhat troubling passage, Kuhn characterized incommensurability in terms of the recognition of anomaly, that is, as the indication of discrepancies between observations of phenomena and the expectations of natural laws. This usage of the term represents a divergence from Kuhn's earlier discussions of incommensurability and extends the term to a different stage of scientific development. Specifically, it suggests that incommensurability occurs not only with respect to the differences presented by overlapping natural laws but also with respect to discrepancies between observations and expectations.

Kuhn's two uses of the term incommensurability are related yet must be distinguished. The recognition of discrepancy or anomaly in relation to a natural law may prompt the development of another natural law, which is incommensurable with the one that produced the anomaly. Yet it is important to distinguish between anomaly (encountered with respect to one natural law) and incommensurability (revealed in the comparison of alternative natural laws). Similarly, it is important to distinguish the stage in which a natural law is questioned (and an alternative law is developed) from the stage in which an established law is compared with an alternative.

With these distinctions in mind, we must reexamine Kuhn's assertion that incommensurability is constitutive of science:

> Discrepancies which, in another field, could be tolerated or taken for granted play an essential role in the development of science. That is what makes incommensurability in the sciences so especially consequential. For the sciences, indeed, I take it to be constitutive. (Ibid.)

Within the context of these statements, the claims that incommensurability is "especially consequential" for science and even "constitutive" of it, should be expanded to distinguish the role of anomaly and the limitations imposed by the structure of natural laws. For it is the violation of limitations that is recognized in the identification of anomaly. This recognition prompts the development of an alternative law, which is guided by identification and investigation of the anomaly. What is investigated, then, is the way in which the set of interrelations that constitute the established natural law fail to match a situation to which that law was presumed to apply. An alternative will be developed to adjust those interrelations and thus to assimilate the anomaly. If the change in the interrelations is substantive, the new law will be incommensurable with the previous one. Incommensurability, in Kuhn's original sense, thus arises with the development of an alternative law (set of interrelations) that is, it arises in a situation that requires choice.

In this respect, it is not only incommensurability that is "especially consequential" and even "constitutive" of science. Rather, it is the special structure of natural laws, combined with the related abilities associated with recognizing and investigating anomalies; developing alternative laws; and confronting incommensurability through choice. The changes in natural laws that occur over time and across communities are not simply the result of incommensurability but emerge from this combination of factors, as encountered and addressed by particular communities at particular points in time.

How then, are we to understand the "necessary" choices that are reflected in incommensurable laws and made by various communities at various times? If the "categories" of science are moveable, if natural laws are changeable, then what can provide the cognitive authority of science when the scientific community operates outside established categories and beyond the reach of its natural laws? How are we to understand the "development" of science or of scientific knowledge?

Cognitive Authority and the Historical Development of Science

In November 1991, Kuhn gave the inaugural lecture of the Robert and Maurine Rothschild Distinguished Lecture Series, sponsored by the Department of History of Science at Harvard. Kuhn's presentation, "The Trouble with the Historical Philosophy of Science" (THPS 1991/2000, 105-120), reflected a return his historical roots in a number of respects. Addressing himself to the transformation in the image of science that was prompted by the "historicist" turn, or the *historical* philosophy of science, Kuhn noted that the change "has, I think, begun to yield a far more realistic understanding of what the scientific enterprise is, how it operates, and what it can and cannot achieve than was available before" (THPS, 106). Yet he also noted a troubling – yet potentially insightful – by-product of the transformation, namely, the view of science initially emphasized and developed by so-called "Kuhnians:"

> I think their viewpoint damagingly mistaken, have been pained to be associated with it, and have for years attributed that association to misunderstanding. Recently, however, I've increasingly recognized that something central to the new view of science was also involved, and I shall be trying this afternoon to confront it. (Ibid.)

In considering not only the gains achieved by scholars in the historical philosophy of science but also the challenges presented by Kuhnians, Kuhn thus returned to the issues and debates he had confronted following the publication of *Structure*. He returned, that is, to issues of cognitive authority and the development of scientific knowledge.

Kuhn characterized the field of historical philosophy of science as emerging from a concern with "deep though isolated difficulties" in established views of the scientific method, that is, the "method by which scientists discovered true generalizations about and explanations for natural phenomena" (THPS, 107). To investigate these difficulties, scholars examined observations of scientific life and the history of science. Through these investigations, they found that "the supposedly solid facts of observation turned out to be pliable;" "the so-called facts proved never to be mere facts, independent of existing belief and theory;" and "individuals committed to one interpretation or another sometimes defended their viewpoint in ways that violated their professed canons of professional behavior" (THPS, 107-8).

In response to these initial findings, the subsequent generation of scholars began to investigate "the process . . . by which the outcome of experiments is uniquely specified as fact and by which authoritative new beliefs – new scientific laws and theories – come to be based on that outcome." (THPS, 109). To do so, they developed "a new kind of historical and, more especially, of sociological studies:"

> These studies have dealt, in microscopic detail, with the processes within a scientific community or group from which an authoritative consensus finally emerges, a process this literature often refers to as "negotiation." (Ibid.)

Noting that some of these studies were "brilliant" and that, "[t]here can be no question, I think, about either their novelty or their importance," Kuhn suggested that despite these achievements,

> . . . their net effect, at least from a philosophical perspective, has been to deepen rather than to eliminate the very difficulty they were intended to resolve. (Ibid.)

On Kuhn's view, the investigations failed to address how the processes involved in negotiation "can be said to result in either true or probable conclusions about the nature of reality" (THPS, 110). In particular, they failed to address "the way . . . in which nature enters the negotiations that produce beliefs about it" (Ibid.).

Considering the question of how the processes involved in negotiations could yield knowledge of nature, Kuhn identified it as "a serious question" and characterized scholars' "inability to answer it . . . [as] a grave loss in our understanding of the nature of scientific knowledge" (Ibid.). He dismissed the claims of the strong program as "an example of deconstruction gone made" (Ibid.) yet identified "a real philosophical challenge" within their seemingly outrageous conclusions:

> There's a continuous line (or continuous slippery slope) from the inescapable initial observations that underlie microsociological studies to their still entirely unacceptable conclusions. Much that ought not be abandoned has been learned by travelling that line. And it remains unclear how, without abandoning those lessons, the line can be deflected or interrupted, how its unacceptable conclusions can be avoided. (THPS, 111)

The philosophical challenge presented by the strong program thus is to show how the negotiation processes that characterize scientific activity can be said to produce scientific knowledge.

In introducing his attempt to address this challenge, Kuhn noted a remark made to him by Marcello Pera:

> The authors of the microsociological studies are, he suggests, taking the traditional view of scientific knowledge too much for granted. They seem, that is, to feel that traditional philosophy of science was correct in its understanding of what *knowledge* must be. Facts must come first, and inescapable conclusions, at least about probabilities, must be based upon them. If science doesn't produce knowledge in that sense, they conclude, it cannot be producing knowledge at all. It is possible, however, that the tradition was wrong not simply about the methods by which knowledge was obtained, but about the nature of knowledge itself. *Perhaps knowledge, properly understood, is the product of the very processes these new studies describe*. (Ibid., emphasis added)

This final assertion suggests the need for a different conception of knowledge.[193] In particular, it suggests that knowledge might be *developed through* processes of negotiation that occur in science, rather than being the objective or standard *toward which* those processes are directed. What is needed, then, is an even greater understanding of the historical perspective from within which knowledge is developed.

Considering the approach to investigations undertaken by scholars in historical philosophy of science, Kuhn proposed that "[g]iven what I shall call the historical perspective, one can reach many of the central conclusions we drew with scarcely a glance at the historical record itself" (Ibid.). The historical perspective, he explained, is a dynamic conception of science, initially drawn from examination of the historical record yet ultimately derivable from first principles:

> That historical perspective was, of course, initially foreign to all of us. The questions which led us to examine the historical record were products of a philosophical tradition that took science as a static body of knowledge and asked what rational warrant there was for taking one or another of its component beliefs to be true. Only gradually, as a by-product of our study of historical "facts" did we learn to replace that static image with a dynamic one, an image that made science an ever-developing enterprise or practice. And it is taking longer still to realize that, *with that perspective achieved*, many of the most central conclusions we drew from the historical record can be derived instead from first principles. (THPS, 111-2, emphasis added)

Once this historical perspective is in place, Kuhn proposed that science should be approached through first principles rather than further historical investigation:

> Approaching [conclusions] in that way reduces their apparent contingency, making them harder to dismiss as a product of muckracking investigation by those hostile to science. And the approach from principle yields, in addition, a very different view of what's at stake in the evaluative processes that have been taken to epitomize such concepts as reason, evidence, and truth. Both these changes are clear gains. (THPS, 112)

The approach from principle thus seemed to provide a more defensible basis for drawing conclusions about the nature of science and a more suggestive view of cognitive authority than was available from traditional approaches in historical philosophy of science.

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[193] In the next chapter we will consider the proposition that it suggests a (characteristically Kuhnian) *inversion* of traditional conceptions of knowledge and its development.

By approaching the central questions of their field through first principles, Kuhn proposed that scholars in the historical philosophy of science might overcome the challenges that had emerged along with the achievements of their work. Yet in order to do so, he suggested that they must first come to grips with their role in creating those challenges:

> The trouble with the historical philosophy of science has been, I've suggested, that by basing itself upon observations of the historical record it has undermined the pillars on which the authority of scientific knowledge was formerly thought to rest without supplying anything to replace them. The most central of the pillars I have in mind were two: first, that facts are prior to and independent of the beliefs for which they are said to supply the evidence, and second, that what emerges from the practice of science are truths, probable truths, or approximations to the truth about a mind- and culture-independent external world. (THPS, 118)

The insights provided by investigations of the historical record thus proved successful in undermining central pillars of the cognitive authority of science. Unfortunately, further investigations of the same sort had, as yet, failed to reveal adequate alternatives in their stead.

To resolve the situation, Kuhn proposed that rather than viewing these apparent failures *as* failures, scholars understand them as part of the broader developmental processes that the historical perspective reveals:

> What's gone on since the undermining occurred has been efforts either to shore up those pillars or else to erase all vestige of them by showing that even in its own domain science has no special authority whatsoever. I've tried to suggest another approach. The difficulties that have seemed to undermine the authority of science should not be seen simply as observed facts about its practice. Rather they are necessary characteristics of any developmental or evolutionary process. That change makes it possible to reconceive what it is that scientists produce and how it is that they produce it. (THPS, 119)

In other words, Kuhn suggested that what was needed to address the challenges raised by investigations of the history of science was the historical perspective that those investigations had achieved. By adopting this historical perspective, one thus could understand how the negotiation processes that characterized scientific activity could produce scientific knowledge. In doing so, however, one must not only understand negotiation processes but also reconceive established notions of science and scientific knowledge as the products of those processes. To begin this process, he specified three aspects of an historical developmental reconceptualization of science: production and evaluation of change of belief, rather than belief *tout court*; comparative evaluation of competing beliefs; and specialization of scientific activity.

Change of Belief

Kuhn proposed that the "characteristic concern" of the historian is development over time and that the "typical result" of the historian's investigations is embodied in narrative:

> Whatever its subject, the narrative must always open by setting the stage, by describing, that is, the state of affairs in place at the beginning of the series of events that constitutes the narrative proper. . . . That description must make it plausible that the beliefs were held by human actors, for which purpose it must include a specification of the conceptual vocabulary in which natural phenomena were described and in which beliefs about phenomena were stated. With the stage thus set, the narrative proper begins and it tells the story of change of belief over time and of the changing context within which those alterations occurred. By the end of the narrative those changes may be considerable, but they have occurred in small increments, each stage historically situated in a climate somewhat different from that of the one before. And at each of those stages except the first, the historian's problem is to understand not why people held the beliefs that they did, but why they elected to change them, why the incremental change took place. (THPS, 112)

In adopting the historical perspective, then, an historian must begin with a descriptive "opening" of the narrative (i.e., setting the stage) and then trace the progression of changes in belief through various stages.

From a philosophical point of view, the historical perspective thus presents two vastly different formulations: "the rationality of belief versus the rationality of incremental changes of belief" (Ibid.). The rationality of belief was traditionally supported by presumption of the possibility for objective observation of an external, mind-independent world. This type of observation was said to provide a stable, Archimedean platform of "facts" from which to determine truth or to evaluate particular beliefs, laws, or theories. As discussed above, however, this presumption was drawn into question by investigations in historical philosophy of science. Scholars in the strong program took the conclusions of those investigations even further, questioning the very possibility of truth and rational evaluation. The "trouble" then, was that historical philosophy of science had, as yet, failed to establish an alternative basis for either truth or rationality.

Through a series of assertions, Kuhn proposed that an historical perspective supports a reconceptualization of the Archimedean platform and its role in the determination of truth and the rational evaluation of belief:

> The traditional Archimedean platform provides an insufficient base for the rational evaluation of belief, a fact that the strong program and its relatives have exploited. From the historical perspective, however, where change of belief is what's at issue, the *rationality* of the conclusions requires only that the observations invoked be neutral for, or shared by, the members of the group making the decision, and for them only at the time the decision is being made. (THPS, 113)

In adopting the historical perspective, Kuhn thus proposed that a shift of philosophical emphasis from justification of the platform to considerations of the decisions that are made about it. Thus "what's at issue" for the historian or for the historical philosopher – at least initially – is change of belief, rather than belief *tout court*. Rational evaluation or justification of a change of belief thus is limited to the context of the decision in which that possible change is evaluated. More specifically, it is limited to members of the decision-making group and to the time of their decision.

This proposal raises the question of how change of belief can be evaluated and thus seems to reintroduce the need for the Archimedean platform: on what other basis could justification or rational evaluation be conducted? In answer to the question, Kuhn proposed that given the emphasis on evaluation of *changes* of belief, attempts to determine the "neutrality" or rationality of a belief or observation will be limited to the ones that lie within the context, scope and influence of that change. The beliefs (and observations) that remain unaffected by the change thus serve as the basis for rational evaluation or justification of those that are affected:

> . . . the observations involved need no longer be independent of all prior beliefs, but only of those that would be modified as a result of the change. The very large body of beliefs unaffected by the change provides a basis on which discussion of the desirability of change can rest. It is simply irrelevant that some or all of those beliefs may be set aside at some future time. To provide a basis for rational discussion they, like the observations the discussion invokes, need only be shared by the discussants. There is no higher criteria of the rationality of discussion than that. The historical perspective, thus, also invokes an Archimedean platform, but it is not fixed. Rather, it moves with time and changes with community and sub-community, with culture and subculture. Neither of those sorts of change interferes with its providing a basis for reasoned discussions and evaluations of proposed changes in the body of belief current in a given community at a given time. (Ibid.)

From an historical perspective, then, the Archimedean platform changes along with changes in the beliefs and observations of the community. As such, the stability of the platform – the justification and rationality of the platform itself – can be considered only with respect to particular communities at particular points in time.

As a second difference between the evaluation of belief and the evaluation of change of belief, Kuhn explained that "[f]rom the historical perspective the changes to be evaluated are always small:"

> In retrospect, some of them seem gigantic, and these regularly affect a considerable body of beliefs. But all of them have been prepared gradually, step by step, leaving only a keystone to be put in place by the innovator whose name they bear. And that step too is small, clearly foreshadowed by the steps that have been taken before: only in retrospect, after it has been taken, does it gain the status of a keystone. (Ibid.)

In making these claims, Kuhn rejected traditional views of discovery as the inspiration of genius. He also refined previous statements in which he attempted to correct his initial characterization of discovery as involving a "gestalt switch." In retrospect, the final keystone appears to be instantaneous and the path of investigation appears inevitable. Yet from the historical perspective, the moments of discovery that seem to prompt "changes of belief" or even the "scientific revolutions" are simply the culmination of a series of gradual steps. This "culmination" is the identification of a final keystone that (in an as yet unidentified way) serves to complete the series of steps that (in retrospect) constitute the path toward its discovery.

Comparative Evaluation

Assuming that the historical perspective involves changes of belief, rather than belief *tout court*, how are those changes to be evaluated? It would seem, almost by definition, that such changes must involve a *comparative* evaluation between the two beliefs in question. In characterizing evaluations in this way, Kuhn proposed that one might avoid the challenges to the use of traditional criteria when viewed as the basis for evaluating the truth of a belief defined as correspondence to reality:

> . . . look what happens to these same criteria – upon which I cannot much improve – when applied to comparative evaluation, to change of belief rather than directly to belief itself. To ask which of two bodies of belief is *more* accurate, displays *fewer* inconsistencies, has a *wider* range of applications, or achieves these goals with the *simpler* machinery does not eliminate all ground for disagreement, but the comparative judgment is clearly far more tractable than the traditional one from which it derives. Especially since what must be compared are only sets of beliefs actually in place in the historical situation. For that comparison, even a somewhat equivocal set of criteria may over time be adequate. (THPS, 114-5)

In making this proposal, Kuhn noted that greater defensibility of judgments that results from this "change in the object of evaluation" comes at the price of traditional conceptions of truth: "[a] new body of belief could be *more* accurate, *more* consistent, broad*er* in its range of applicability, and also simpl*er* without for those reasons being any tru*er*" (THPS, 115).

In considering the "loss" of truth claims associated with comparative evaluation of changes of belief, Kuhn noted that such loss is an inevitable consequence of the loss of a fixed Archimedean platform:

> Only a fixed, rigid Archimedean platform could supply a base from which to measure the distance between current belief and true belief. In the absence of that platform, it's hard to imagine what such a measurement would be, what the phrase "closer to the truth" can mean. (Ibid.)

On this account, traditional conceptions of truth are integrally interrelated with the presumption of a fixed Archimedean platform. Without a fixed platform, traditional conceptions of truth are incoherent. Yet Kuhn insisted that one need not abandon truth entirely. Instead, one must reconceive traditional notions of scientific development, truth, and reality:

> First, the Archimedean platform outside of history, outside of time and space, is gone beyond recall. Second, in its absence, comparative evaluation is all there is. Scientific development is like Darwinian evolution, a process driven from behind rather than pulled toward some fixed goal to which it grows ever closer. And third, if the notion of truth had a role to play in scientific development, which I shall elsewhere argue that it does, then truth cannot be anything quite like correspondence to reality. I am not suggesting, let me emphasize, that there is a reality which science fails to get at. My point is rather that no sense can be made of the notion of reality as it has ordinarily functioned in the philosophy of science. (Ibid.)

Without a fixed Archimedean platform, one cannot develop toward a fixed goal such as truth or correspondence to external reality. Instead, one can rely only on comparative evaluation, thereby reconceiving development as "a process driven from behind," that is, as a process constituted by a series of comparative evaluations. The result of these evaluations cannot be called truth in the traditional sense of the term, nor can the evaluations be said to be moving closer to external reality in its traditional sense. To the extent that "truth" and "reality" may be conceived within an historical perspective, they must be reconceptualized.

Kuhn noted that his position was "much like that of the strong program – facts are not prior to conclusions drawn from them, and those conclusions cannot claim truth" (Ibid.). Yet unlike the investigations of the strong program, he pointed out that his position was based on "principles that must govern all developmental processes, without, that is, needing to call upon actual examples of scientific behavior" (Ibid.). Moreover, he asserted that "nothing along that route has suggested replacing evidence and reason by power and interest. Of course power and interest play a role in scientific development, but there's room for a great deal else besides" (THPS, 115-6). The question, then, is what "else" is involved in scientific development such that it can play the role traditionally attributed to evidence and reason.

Specialization and Speciation

Acknowledging that his final comments were based on observation rather than first principles, Kuhn proposed that speciation provides a determinate role in the development of science as well as other distinct human practices or specialties. Tracing the patterns of specialization and development in the sciences from antiquity to the changes of the seventeenth century and then to the present time, Kuhn noted that over time, "individual specialties that collectively constitute science . . . acquire their own special societies, journals, university departments and special chairs" (THPS, 116-7). In this respect, he proposed that "human practices in general and scientific practices in particular have evolved over a very long time span, and their development forms something very roughly like an evolutionary tree" (THPS, 117).

Kuhn proposed that the pattern of evolution through speciation provides important insight into the development of scientific knowledge over time:

> What results is the elaborate and rather ramshackle structure of separate fields, specialties and subspecialties within which the business of producing scientific knowledge is carried forward.
>
> Knowledge production is the particular business of the subspecialties, whose practitioners struggle to improve *incrementally* the accuracy, consistency, breadth of applicability, and simplicity of the set of beliefs they acquired during their education, their initiation into the practice. It is the beliefs modified in this process that they transmit to their successors who carry on from there, working with and modifying scientific knowledge as they go. Occasionally the process runs aground, and the proliferation and reorganization of specialties is usually part of the required remedy. (Ibid.)

On this account, scientific knowledge develops through the activities of individual scientists working within particular specialties or subspecialties. These specialists work to improve the beliefs that they acquired during their scientific education, and they do so by applying the criteria to which they are – as scientists – committed. While these criteria may be understood differently by scientists working in different fields and may be applied differently by scientists working within the same, specialized area, Kuhn proposed that, "in the fields where they have once taken hold, they account for the continuous emergence of more and more refined – and also more specialized – tools for the accurate, consistent, comprehensive, and simple descriptions of nature" (THPS, 118). Thus while the criteria guiding scientific activity might differ, the application of those criteria over time constitutes the development of scientific knowledge. Asking, "[w]hat is scientific knowledge and what else would you expect practices characterized by these evaluative tools to produce?" (Ibid.), Kuhn proposed that the "sciences, which must then be viewed as plural" can be seen to possess "a very considerable authority" (THPS, 119).

Chapter Six

Insights and Implications
of Kuhn's Historical (Developmental) Perspective

Our investigation has come full circle: from Kuhn's claims of misinterpretation by philosophers and insistence that he "was not a Kuhnian" to his assertion that the field of historical philosophy, in which he worked along with Feyerabend, Toulmin and others, had neglected important insights of the Kuhnian's once-forsaken proposals. How can Kuhn's initial claims be reconciled with these later assertions? Did his later work reflect a repudiation of the views outlined in *Structure*? Does it suggest that he was, in fact, a "Kuhnian" all along? Or do the first principles of the "historical perspective" developed by Kuhn, Feyerabend and others in historical philosophy of science reflect an achievement that brings together these previously disparate views?

There is not a clear answer to these questions, in part because Kuhn's project remains incomplete. Although his later investigations entailed what he characterized as a series of dramatic breakthroughs and provided a more philosophical basis for the issues that had been at the center of his research, his final publications represent a collection of disparate pieces rather than a comprehensive, explanatory account. This later work is highly suggestive but largely ignored. The book on incommensurability was still in draft form when he died, and an edited version of his early drafts has yet to be published. What, then, are we to make of Kuhn's legacy? Need we be concerned with taking up and investigating further the issues and ideas that he researched and developed?

In considering this question shortly after the publication of *Structure*, Margaret Masterman acknowledged "the obvious difficulties of handling . . . such an entity as Kuhn's crude paradigm has turned out to be (that is, if I am right as to what it has turned out to be)" and reconsidered "the obvious skepticism which even the suggestion that we should take Kuhn's paradigm seriously and philosophically is bound to arouse" (Masterman 1970, 87). She insisted, however, that the difficulties and skepticism be confronted:

> . . . as historians, however much we may cavil at Kuhn's conclusions in detail, we are not going to be able to go back to where we were before Kuhn and his immediate predecessors began to get at us. Their protest against the unconscious dishonesty and the swings of bias with which history of science has been done in scientific textbooks up to now cuts far too deep; and so does their outcry against the oversimple and distorted accumulative view of science which has resulted from reading the textbooks as though they were the real history. (Ibid., 87-8)

Our investigations support Masterman's assertions and suggest that the implications of *Structure* extend beyond science and the history of science, to the philosophy of science and even to philosophy itself. The widespread misinterpretations of *Structure* were both systematic and suggestive, revealing limitations in sociological and philosophical approaches to the work and, by implication, in their investigations of science and development more generally.

In considering Kuhn's legacy, we must confront the fact that what we have identified as "misinterpretations" of *Structure* continue to this day and have become institutionalized as specific disciplines, approaches or views of the irrational, relativistic, or sociological (some would say "Orwellian") nature of science. The various sociologists and philosophers of science who were involved in discussions about the work's implications for cognitive authority now take the "relativism" of Kuhn's views for granted, noting only mild curiosity or disdain at his stubborn rejection of their attributions.

How, then, are we to evaluate Kuhn's legacy, and how can we justify such an evaluation in the face of (what we would consider to be) continued misinterpretation of his work? To the extent that Kuhn's work presents a challenge to established views, specifically, a change in our image of the nature of science and even the nature of knowledge, we must understand the nature and the authority of that work. That is, we must understand Kuhn's "historical perspective" and the basis on which it can be said to present (or to establish) first principles for the conduct and the investigation of science. Secondly, we must understand the "rationality" of that perspective and its relation to the traditional forms of rationality that it is said to undercut. Finally, we must understand the views of scientific progress and the development of knowledge that are suggested by the historical perspective and the cognitive authority of its "rational" determinations.

In conducting these investigations, we must not only seek to understand the insights and implications of Kuhn's historical perspective, we must also attempt to justify them. We must understand the issues that they highlight with respect to traditional views and established interpretations, and the "good reasons" that we might adopt a new perspective of the legacy of Thomas Kuhn.

Section One

Kuhn's Historical (Developmental) Perspective

In describing the "historical perspective," Kuhn suggested that its dynamic conception of science had been developed by scholars in historical philosophy of science, who "were motivated by widely recognized difficulties in the then current philosophy of science, most prominently [difficulties] in positivism or logical empiricism, but in other sorts of empiricism as well" (THPS 1991/2000, 106). This statement is consistent both with Kuhn's characterization of his project and with our historiographic investigation of his research. Yet the statement also implies – an implication continued throughout the rest of the account – that the approach by which Kuhn developed his "perspective" was the same as that of the philosophers of science with whom he was engaged. This suggestion contradicts Kuhn's distinctions between the history and the philosophy of science (RHPS 1968/1977, 3-20) as well as our own examination of the differences in their methods.

We might imagine that the recognition (or achievement) of the "first principles" of the historical perspective would eliminate the relevance of any initial distinctions in the approaches undertaken by Kuhn and the philosophers of science. As such, any discussion of those points of difference would only serve to confuse the larger argument that Kuhn intended to make regarding the "trouble" with the historical philosophy of science. Yet given the importance of the supposed "first principles" to our evaluation of the authority and legacy of Kuhn's work, we must, once again, examine the distinctive methods and approaches underlying that work.

Distinctive Aspects of Kuhn's Historical (Developmental) Perspective

Perhaps the most distinctive aspect of Kuhn's method and approach was his focus on exploring the philosophical implications of his Aristotle experience, through whatever means, methods, and approaches were necessary. This single-mindedness led Kuhn from physics to historiography, "random exploration" of psychology, linguistics and other fields, and finally, to the creation of historical developmental accounts of all aspects of scientific activity. A self-described "physicist turned historian for philosophical purposes" (Baltas et al. 1995/2000, 321), Kuhn's investigations melded together disparate fields of inquiry, and his ideas had greater appeal and wider application than he had intended, or even wanted. His admirers praised his ability to "speak the truly incommensurable languages of physics, philosophy, and history," noting that these were "all necessary to frame and advance his philosophical quest" (Heilbron 1998, 685). Yet his critics challenged his wide-ranging investigations and promiscuous borrowings, characterizing *Structure* as having "a philosopher's sense of sociology, a historian's sense of philosophy, and a sociologist's sense of history" (Fuller 2000, 32).

While highlighting Kuhn's expansive methodological wanderings, our investigation clearly indicates the grounding of his project in historiographic, case-based investigations of scientific activities. The Aristotle experience occurred while Kuhn was developing a case on the development of Newtonian mechanics. His subsequent historiographic investigations were focused on out-of-date scientific texts and the scientific discoveries or achievements that rendered them obsolete (or highlighted the ways in which they were "mistaken"). These case studies highlighted the occurrence of conceptual shifts that were similar to the one he had discerned between Aristotelian and Newtonian physics. Most of these investigations were conducted during Kuhn's nine years teaching in Conant's General Education course in Science. They formed the basis for many of his insights and central concepts regarding scientific development and served as examples or illustrations in his later, historical developmental accounts.

The distinctive aspects of Kuhn's case-based approach and the implications of his case study investigations were conjoined in his unique approach to the causal theory of reference. In "Dubbing and Redubbing: the Vulnerabilities of Rigid Designation" (DR 1990), Kuhn expanded his earlier considerations of the causal theory to highlight the insights that it made available and the limitations in typical applications of it by causal theorists. Praising the "canonical examples" provided by act of dubbing and the recognition of a chain of examples extended over time, he noted the limitations imposed by causal theorists' reliance on an original act of dubbing followed by the identification of "sameness" with a representative current sample (and without regard to the nature of that sameness or any change in the criteria or characteristics of the originally "dubbed" sample).

Kuhn proposed that this "rigid designation" neglected consequential changes of meaning, which extended beyond the sample itself. Instead, he proposed that one should focus on the act of dubbing (and redubbing) over time. In this respect, the referent of a theory would be understood at a particular time and in terms of a particular act of dubbing with its (particular) set of “canonical examples. The parallel to Kuhn’s historiographic investigations thus lies in the proposed interrelation of referent (theory) and examples (exemplars) through the act of dubbing (historiographic narrative) and in the recognition that a change in one part of this tri-partite relationship may necessitate a consequential change in other parts. In contrast, traditional approaches in philosophy of science relied upon a single act of dubbing (“rigid designation”), such that any changes in the relationship between theory and exemplars would be concealed. While Kuhn’s emphasis on multiple acts of dubbing suggests a series of acts, each performing a dual function of definition and legislation (i.e., exemplars and theory), the causal theorists’ focus on a single, original act and identification of (current) “sameness” relied upon an established definition and reflected only the assertion of its legislative aspects.

According to the philosophical perspective (and as with the causal theorists), this concealment is not consequential because changes in the criteria of dubbing or in the associated relationship of theory and exemplars are (incremental) refinements or (purely) definitional improvements. Yet as suggested by Kuhn’s Aristotle experience, his subsequent historiographic investigations, and his critique of Putnam’s application of causal theory to conceptions of water as H_2O (PW 1986/2000, 81-83), changes in the criteria of dubbing or in the theory-exemplar relationship can be revolutionary (that is, non-cumulative) and thus consequential. The underlying presumption of incremental refinement through improved legislation thus seems to present an incomplete view of potential changes.

In his considerations of causal theory, Kuhn proposed that the interrelation of examples (and counter-examples) within a lexical structure provides the basis for identifying these non-cumulative changes. This is because a change in the interrelation reflects changes in the terms of the lexicon. This change extends beyond the particular examples or theory being considered to other aspects of the lexical structure that are related to those terms. In this respect, the lexicon serves as a something akin to an historically situated Archimedean platform, that is, as an all-encompassing and holistic point of reference that provides the basis for identifying and evaluating localized change. The examples of a particular theory or historical act of dubbing thus are relevant not only to that theory or referent but also to the other theories and referents that collectively constitute the entirety of the scientists’ (or scholars’) knowledge and experience.

In this respect, change in a particular part of the lexicon or a particular act of dubbing (i.e., a particular "category" and its examples) is accompanied by broader, systematic change in other parts of the lexicon or in the interrelation of other categories (and their associated theories). The lexicon changes along with its constituting referents and their associated theories and relations to other referents. These interrelated changes can be identified by considering each particular, historical act of dubbing and the changes that it represents and effects. Yet these (dynamic and developmental) points of investigation are dramatically different from the rigid designation applied and traced to the present by causal theorists and, more generally, by philosophers in their evaluation from the presumptive basis of ever-increasing knowledge and the epistemically "privileged" position of the present.

This, then, represents Kuhn's "historical perspective," one which we might more appropriately label "historical developmental," following his later characterizations (Baltas et al. 1995/2000, 319). This more specific characterization emphasizes a specific focus on the developmental aspects of the historical perspective, in particular, the insights provided by a focus on the activities that provide the impetus for that development or dynamism. While it is not clear whether Kuhn recognized the importance of his distinctive, developmental emphasis, it is this unique approach that we must consider when evaluating his claims to have identified first principles. What, then, are we to make of those claims? If acts of dubbing may be identified (and, implicitly, justified) by reference to the lexicon or lexical structure, what can provide a similar basis for identification or justification of the historical Archimedean platform that is to be considered by the historical developmental perspective? What can provide the basis for a comparison of competing views drawn from alternative platforms?

In considering these questions, we must remember that Kuhn's guiding interest was not in the historical insights provided by the new historiography but in its philosophical implications for the nature of science. His research was directed toward a developmental, rather than just an historical (or even historiographic) account of science. He wanted to understand the nature of science not only as a form of knowledge but as an activity that develops over time. Yet in order to understand science as both a form of knowledge and an activity that changes over time, he had to address the question of first principles. That is, he had to understand how historical investigations could provide a basis for normative claims, or, more precisely, how historical case studies could be used as the basis for one developmental (and, implicitly, explanatory) account, rather than another.

A "Near-Trivial" Justification

Responding to challenges regarding the normative claims of *Structure's* historical (developmental) account, Kuhn considered these issues in a 1983 essay entitled "Rationality and Theory Choice" (RTC 1983/2000, 208-15), which was presented at a symposium on the philosophy of Kuhn's friend and colleague, Carl G. Hempel. Recounting his numerous conversations with Hempel regarding theory choice, Kuhn considered the question, "Under what circumstances may one properly claim that certain criteria which scientists are *observed* to use when evaluating theories are, in fact, also rational bases for their judgments?" (RTC, 209). In answer to this question, Hempel had proposed a "near-trivial" justification of theory choice, based on satisfaction of the desiderata that most fully articulate the conception of science:

> Science is widely conceived as seeking to formulate an increasingly comprehensive, systematically organized, world view that is explanatory and predictive. It seems to me that the desiderata [which determine the goodness of a theory] may best be viewed as attempts to articulate this conception somewhat more fully and explicitly. And if the goals of pure scientific research are indicated by the desiderata, then it is obviously rational, in choosing between two competing theories, to opt for the one which satisfies the desiderata better. . . . [These considerations] might be viewed as *justifying* in a near-trivial way the choosing of theories in accordance with whatever constraints are imposed by the desiderata. [(RTC, 210), quoting (Hempel 1983, 91f)]

Noting Hempel's characterization of the proposal as "near-trivial," Kuhn considered the reasons for this qualification:

> He refers to it as "near-trivial" in the passage just quoted, apparently because it rests on something very like tautology, and he finds it correspondingly lacking in the philosophical bite one expects from a satisfactory justification of the norms for rational theory choice. In particular, he underscores two respects in which near-trivial justification seems to fail. "The problem of formulating norms for the critical appraisal of theories may," he points out, "be regarded as a modern outgrowth of the classical problem of induction," a problem that the near-trivial justification "does not address at all" (92). Elsewhere he emphasizes that, if norms are to be derived from a description of the essential aspects of science (my "puzzle-solving enterprise" or his "increasingly comprehensive, systematically organized, world view"), then the choice of the description that serves as premise for the near-trivial approach itself requires justification which neither of us appears to provide (86f, 93). The activities observed by a science watcher can be described in countless different ways, each the source of different desiderata. What justifies the choice of one of these, the rejection of another? [(RTC, 210-11), citing (Hempel 1983)]

In response to these challenges, Kuhn argued that "a particular sort of descriptive premise requires no further justification," concluding that "the near-trivial approach itself is therefore deeper and more fundamental than Hempel supposes" (RTC, 211).

Outlining his own findings with regard to the dual-function of natural laws, Kuhn proposed that a similar duality was embedded in the language of Hempel's near-trivial justification:

> If I am right, the descriptive premise of the near-trivial approach exhibits, within the language used to describe human actions, two closely related characteristics that I have previously insisted are essential features of the language used to describe natural phenomena. (Ibid.)

These two characteristics included, first, the "local holism" by which referring terms must be learned in clusters along with "explicit or implicit generalizations [i.e., "laws"] about the members of the taxonomic categories into which those terms divide the world" (Ibid.). Secondly,

> Once acquired, the member terms of an interrelated set can be used to formulate infinitely many new generalizations, all of them contingent. But some of the original generalizations or others compounded from them prove to be necessary. (RTC, 212)

These dual-function generalizations thus both define their referring terms and legislate the formulation of infinitely new generalizations. Thus in the case of natural laws, knowledge of nature is embedded within the language of the laws themselves, such that they provide their own justification through their definition of the situations that nature presents. In what way, then, does Hempel's near-trivial justification represent a similar dual-function generalization, such that no further justification is required?

Kuhn proposed that theory choice can be justified by satisfaction of the desiderata that best articulate the conception of science because science is part of "the empirically derived taxonomy of disciplines" (RTC, 214). This taxonomy is "embodied in the vocabulary of disciplines and applied by virtue of the associated field of disciplinary characteristics," that is, by virtue of desiderata (Ibid.). The desiderata of science thus function as the referents for science within the broader taxonomy of disciplines:

> Just as access to Newton's second law is required in order to pick out Newtonian forces and masses, so picking out the referents of the modern vocabulary of disciplines requires access to a semantic field that clusters activities with respect to such dimensions as accuracy, beauty, predictive power, normativeness, generality, and so on. Though a given sample of activity can be referred to under many descriptions, only those cast in this vocabulary of disciplinary characteristics permit its identification as, say, science; for that vocabulary alone can locate the activity close to other scientific disciplines and at a distance from disciplines other than science. That position, in turn, is a necessary property of all referents of the modern term "science." (Ibid.)

In this respect, Kuhn suggested that science may be treated as a language game, such that statements that violate the desiderata or vocabulary of science "place the person who makes them outside of his or her language community" (Ibid.). This, Kuhn proposed, is "where the near-trivial approach to justifying norms for theory choice gets its bite" (RTC, 215). To be "scientific," then, is to be committed to the criteria that constitute and govern "scientific" activity.

In making this proposal, Kuhn noted that the near-trivial approach does not resolve the problem of induction, although it does "make contact:"

> Like "mass" and "force," or "science" and "art," "rationality" and "justification" are interdefined terms. One requisite for either is conforming to the constraints of logic, and I have made use of it to show that the usual norms for theory choice are justified . . . Another requisite is conforming to the constraints of experience in the absence of good reasons to the contrary. Both display part of what it is to be rational. (Ibid.)

Hempel's near-trivial justification thus takes steps toward resolving the problem of induction because in linking rationality and justification, it also links constraints of logic with constraints of experience. By "making contact" with experience in ways that both determine and constrain resulting conceptions, Kuhn claimed that Hempel's proposal thereby "acknowledges that we have no rational alternative to learning from experience" (Ibid.). Yet he also noted that, challenged by the problem of induction, it raises the question of "why that should be the case" (Ibid.).

In this way, Kuhn suggested, we might reconceive the problem of induction to ask, "not for a justification of learning from experience but for an explanation of the viability of the whole language game that involves "induction" and underpins the form of life we live" (Ibid.). The question, then, is not on what basis to establish rational criteria for scientific activity or how to justify such criteria. The question is, rather, how to account for the viability of science and, perhaps, the broader empirical taxonomy of ("rational") disciplines in which the problem of induction and the determination of criteria arise as interrelated referents and theories. It is this interrelation – of which the problem of induction is a part – that must be considered. Furthermore, to be considered, its interrelated parts cannot be examined individually but must be understood (and adjusted) *as* interrelated parts of that (potentially reconstructed) whole.

Characterized in this way, we are reminded of Kuhn's application of the causal theory as contrasted with the approach undertaken by philosophers and philosophers of science. As applied to Kuhn's discussion of Hempel's near-trivial justification, proposed reliance on satisfaction of the desiderata of science seems to parallel reliance on the exemplars or referents that are linked by the act of dubbing. The linkage of desiderata with specific "scientific" activities thus is contemporaneous with the determinations of "science" by the scientific community. In contrast, reliance by philosophers of science upon independent and objective criteria (without regard to *actual* scientific activity) parallels their reliance on a single historical act of dubbing. Thus whereas the traditional philosophical approach raises important questions with regard to how such criteria may be established or justified, Kuhn's approach provides an historically situated definition of rationality and justification based on conceptions of science and scientific activity within the broader, empirical taxonomy of disciplines. Specifically, it considers rationality and justification as interrelated with those conceptions and activities that are deemed "scientific."

In discussing the first principles of the historical perspective, Kuhn suggested that the stability of an historically situated platform is provided by the "rational discussion" of members of the community, where the "criteria" for rational discussion is simply that beliefs and observations are "shared by the discussants" (THPS 1991/2000, 113). A similar link back to the nature and operation of the scientific community emerges from consideration of comparative evaluation and how it, too, might be asserted "from principle." In discussing the need for comparative evaluation of competing theories, Kuhn emphasized the continuity of scientific criteria as constitutive of "scientific" activity and thus as central to the development of "scientific" knowledge:

> The characteristics of the members of [the sciences] are, in addition to their concern with the study of natural phenomena, the evaluative procedures I've already described and others like them. I again have in mind such characteristics as accuracy, consistency, breadth of application, simplicity, and so on – characteristics that are passed, together with illustration, from generation of practitioners to the next.
>
> These characteristics are somewhat differently understood in the different scientific specialties and subspecialties. And in none of them are they by any means always observed. Nevertheless, in the fields where they have once taken hold, they account for the continuous emergence of more and more refined – and also more specialized – tools for the accurate, consistent, comprehensive, and simple descriptions of nature. Which is only to say that in such fields they're sufficient to account for the continued development of scientific knowledge. What else is scientific knowledge, and what else would you expect practices characterized by these evaluative tools to produce? (THPS, 118)

It would seem, then, that the basis for the assertion of change of belief and comparative evaluation lies in a conception of the scientific community as the basis for the commitments guiding the conduct of scientific activity over time. A dynamic conception of science and the historical perspective that is associated with it can be examined "from principle" only after the scientific community has been identified and the shared commitments, which constitute it *as* a community of inquirers, have been established.

Implications of the Historical Developmental Perspective

It would seem, then, that the "first principles" of Kuhn's historical developmental perspective (i.e., change of belief and comparative evaluation) may be considered as the desiderata that most fully articulate the conception of that perspective. That is, they both establish objectives and serve as criteria for determination. As such, they represent the establishment of an attachment between the referents of an historical developmental perspective and the broader taxonomy of perspectives. According to Kuhn, first principles are available only once the historical developmental perspective has been achieved, that is, only once it is placed within a larger taxonomic structure, such that it has a dual function and can be extended.

On this account, change of belief and comparative evaluation simply *are* what it means to adopt an historical (developmental) perspective. These "first principles" were identified and developed gradually, through Kuhn's historiographic and historical philosophical investigations of science (it is not clear how similar principles might be achieved through the approaches and "perspective" of philosophers of science). In the earliest stages, these investigations highlighted the anomalies presented by traditional approaches, which focused on belief *tout court* and correspondence to "what is really there." Once these questions were raised, Kuhn began investigating similar historical situations in order to "give structure" to the anomaly in an attempt to account for it. As evident in his work, the associated struggles involved the development of alternative accounts, initially highly schematic in nature, and a gradual consideration of the distinctive aspects of the historiographic or historical developmental approach vis-à-vis traditional views and methods.

While it is not clear to what extent an historical developmental perspective or a dynamic conception of science has been "achieved," Kuhn's discussions would seem to provide an important step toward more widespread recognition, investigation, and adoption of this broader, dynamic view. Furthermore, we might expect that the achievement of the perspective must be simultaneous with the identification of its first principles, as these are integrally interrelated, mutually defining and even self-constituting. Finally, the transition to an historical developmental approach "from first principles" will effect a methodological inversion, by which the initial "authority" of earlier historical developmental investigations will be concealed and the investigations themselves will be employed as illustrations, rather than constitutive exemplars.

Given that an historical developmental perspective and its associated first principles can be "achieved" (or is the process of being developed), how are we to compare these first principles to those of a traditional philosophical perspective, including its methodological tools for investigation and its conceptions of science, knowledge and rationality? To what extent and in what situations does an historical developmental perspective represent a viable and even preferred alternative? Such questions are somewhat premature until the historical developmental perspective is, in fact, developed more fully. Yet our investigations and Kuhn's comments suggest a number of areas in which an historical developmental perspective provides distinctive insights and highlights inherent limitations in a philosophical perspective. In particular, it provides a more consistent and comprehensive account of the historical integrity of out-of-date scientific practices and texts; it accounts for scientific discoveries with greater clarity and precision; and it illustrates the changes that occur over time in the attachments and underlying structures of scientific knowledge.

These insights suggest a reconceptualization of the basis for scientific activity as conducted from an historically situated Archimedean platform and as focused on change of belief and comparative evaluation. They also suggest the need to revise traditional conceptions of the demarcation criteria for science; the basis for applying scientific criteria; and the inevitable progression toward truth that is made possible by the application of such criteria. In these respects, the adoption of an historical developmental perspective involves losses with respect to traditional philosophical views. Yet these are offset by gains, including not only those outlined above but also the reintroduction of empirical observation and activity into philosophical considerations. It provides a vital reconnection between theory and practice, albeit a much narrower connection than previously considered.

What is not yet clear, however, is the basis on which the determinations that are made within the historical developmental perspective may be justified. Traditional forms of rationality are not adequate to address the dynamic aspects of an historical developmental account. Given that this perspective presents a dynamic conception of science, what can be the "dynamic" rationality that justifies the historical developmental activities and investigations of science? In short, how can Kuhn's historical developmental perspective avoid the earlier charges of irrationality, relativism, and mob psychology, which were lodged against his image of science and which dominate current interpretations of his work?

Section Two

The Case for Dynamic Rationality

To understand the historical developmental perspective more clearly, we must understand what kind of "rationality" can operate within the realm of Kuhn's dynamic conception of science. We must also specify the basis on which it can be used to evaluate and to justify changes of belief, or to adjudicate among competing theories. Finally, we must understand the relationship between this "dynamic" rationality and the more traditional conceptions, which underlie established views of science and scientific knowledge.

The "Rationality" of the Historical Developmental Perspective

Our investigations suggest that the dynamic rationality that is required to support an historical developmental perspective must reflect the ability to develop, to extend, or to adjust a conceptual network based on empirical observations or experiences. Based on the analogy to the causal theory of reference, it entails the investigation of various instances of dubbing and redubbing with respect to a particular theory and its referents. That is, it is the investigation of these interrelationships and how they change and develop over time. As suggested by the extension of Kuhn's historiographic research into historical developmental accounts, it is the investigation and juxtaposition of multiple cases into broader, generalized accounts, which are both descriptive and explanatory of "similar" types of cases.

These activities involve the selection and the study of multiple cases, followed by identification of their shared characteristics, underlying patterns, and apparent structures. The juxtaposition of multiple "examples" involves the identification and exploration of their similarities and differences. Characterized as examples, counter-examples or unrelated cases of a particular type of observation or experience, individual cases are grouped into categories, which are interrelated either explicitly or implicitly. Examples within a particular category thus constitute the category by virtue of both their determinations and their implicit constraints.

A group or category of particular case studies functions as a conceptual "set" of observations and experiences. As suggested by Sneed's set-theoretic formalism, the juxtaposition of multiple cases within the set establishes constraints for other, potential members of the set. These are the constraints of both logic (i.e., the limitations of what can be identified as a holistic set) and experience (i.e., the observations and experiences that can be identified as belonging to the set). Yet as suggested by Kuhn (and contrary to Sneed), a set (or collection of interrelated sets) need not be determined by preexisting theory, rule or law but may be constituted through the juxtaposition of examples, counter-examples, and unrelated cases.

The "rationality" that is exercised in this selection, investigation, and juxtaposition of cases differs from traditional, Enlightenment conceptions of rationality in important ways. It is not based on deduction from first principles but involves, Kuhn would suggest, something of the means by which those first principles may be identified. Nor does it involve inductive reasoning from particular "facts" to more general theories, or first principles. Rather, it involves the simultaneous identification and interrelation of "facts" and "theories," that is, both examples and their referents, or both definitions and their explanations. It involves the interrelation of multiple observations and experiences within particular groupings, categories, or types of observations and experiences. Only once these interrelations (later viewed as identifications or determinations) may inductive or deductive reasoning proceed.

Dynamic rationality thus involves case-based reasoning, as contrasted with the rules-based reasoning of Enlightenment rationality [see (Nickles 2000)]. It is based on the examination and interrelation of multiple cases within a complex conceptual network. While its determinations are not derived from rules, they are, nonetheless, governed by them. Just as the development of an historical narrative is governed by the rules and commitments associated with the practice of history, so the determination of categories of scientific facts is governed by the rules and commitments of the practice of science. To a great extent, then, dynamic rationality is both the expression and the application of Hempel's near-trivial justification.

Dynamic rationality will be *dynamic* in that its determination of "facts" can be adjusted – in fact, *must* be adjusted – in order to account for changes in the way that we understand and encounter observations and experiences. While observations and experiences provide the basis for dynamic (i.e., potentially changing) determinations of facts, they provide a "rational" basis for such determinations through their juxtaposition within a broader, conceptual network. The observations and experiences must be identified as particular groups, categories, or types of facts, such that the conceptual network that they constitute both defines what they are and legislates their possible behavior. Dynamic rationality thus entails the development, extension and adjustment of this conceptual network.

The Dynamic Rationality of *Structure*

The operation of dynamic rationality as case-based reasoning is evident in Kuhn's research activities, that is, his historiographic investigations and, more particularly, his juxtaposition and further development of those investigations into historical developmental accounts. Through the examination of scientific activities, debates and texts, Kuhn's historiographic investigations revealed that revolutionary discoveries typically involved a fundamental reconstruction of the problems, facts, and theories of science. This reconstruction was revealed by the identification of an alternative (and incommensurable) primitive similarity relationship by which those activities, debates and texts were most clearly and most coherently explained. This relationship was not identified on the basis of established views of facts or theories, for it was some aspect of those established views that was in question. Admittedly, these activities typically are characterized (by Kuhn, among others,) as hermeneutic or interpretive, rather than case-based. In this respect, they may more appropriately be characterized as case studies or case study interpretations, that is, as investigations of a particular case.

The exercise of case-based reasoning, that is, reasoning on the basis of (multiple) case study examinations, is more evident in the development of Kuhn's research and in his historical developmental accounts. The historiographic cases that he investigated were selected on the basis of his interest in the philosophical implications of his Aristotle experience. Thus while he was focused on historiographic research, he began to identify the similarities, patterns and underlying structures evident in the historical scientific discoveries that he investigated. The study of measurement prompted his investigation of (generalized) "normal" scientific activities and, coupled with his underlying interests, expanded his view to the entirety of scientific practice. In conducting these various investigations, Kuhn relied upon his historical research, however, he expanded upon both his historical findings and his historiographic methods as needed to develop his (quasi-philosophical) accounts. He did not rely on established views of science, its patterns, or structures, but explicitly sought to highlight the limitations of those views and to propose a new image of the nature of science.

Dynamic Rationality as Case-Based Reasoning

The practice of case law provides a widely recognized exemplar of case-based reasoning. Edward H. Levi's book, *An Introduction to Legal Reasoning* (1949), examines the processes involved in the development of case law, as well as the interpretation of statutes and the Constitution. The considerations and distinctions outlined by Levi are strikingly similar to those outlined above for the exercise of dynamic rationality:

> . . . if the legal process is [wrongly] approached as though it were a method of applying general rules of law to diverse facts – in short, as though the doctrine of precedent meant that general rules, once properly determined, remained unchanged, and then were applied, albeit imperfectly, in later cases. . . . it would be disturbing to find that the rules change from case to case and are remade with each case. Yet this change in the rules is the indispensable dynamic quality of law. It occurs because the scope of a rule of law, and therefore its meaning, depends upon a determination of what facts will be considered similar to those present when the rules was first announced. The finding of similarity or difference is the key step in the legal process.
>
> The determination of similarity or difference is the function of each judge. (Ibid., 2)

What we must understand, then, are the processes and activities by which the "dynamic" practice of case law – the judge's determination of similarity and difference – may be properly conducted.

Levi's comments suggest that case law presents a developmental structure that is analogous to Kuhn's approach to the causal theory of reference. The "dynamic quality of law" is reflected in the possibility for changes in the general rules of law, even after these are "properly determined." The applicability of a law – its scope, and therefore, its meaning in a particular situation – thus depends on the similarity between the "facts" of the original case and the "facts" of the current situation. The judge must determine this similarity (or difference) in order to render judgment on the law. Yet in doing so, as we will see, he must first determine which among the many (possible) facts of each case are relevant to the general rule and thus subject to his consideration. In this respect, he must examine not only the facts of the various cases but also their relation to the general rule. In considering this relation, however, he also, implicitly, must (re-)consider the rule itself.

In outlining the processes involved in a judgment of case law, Levi emphasized that under the doctrine of dictum, the judge in a particular case is not bound by the statement of a rule of law by any prior judges. He may determine that facts judged to be important in the previous case are irrelevant or absent in his own case. Alternatively, he may identify new facts that draw the previous judgment into question. In this respect, Levi emphasized, each judgment of case law reflects "what the present judge, attempting to see the law as a fairly consistent whole, thinks should be the determining classification" for the rule of law (Ibid., 3). The rule of law thus emerges along with the classification of its relevant facts:

> Thus it cannot be said that the legal process is the application of known rules to diverse facts. Yet it is a system of rules; the rules are discovered in the process of determining similarity or difference. (Ibid.)

Levi acknowledged that the reasoning involved in such determinations is "imperfect" because it is not guided by a fixed prior rule determining the "common and ascertainable similarity" that is to be decisive. Considering whether reason operates at all, he noted that it "appears to be involved; the conclusion is arrived at through a process and was not immediately apparent" (Ibid.).

Following this rather tenuous characterization of the "rationality" of legal reasoning, Levi provided a more detailed account in terms of its function and its effect:[194]

> . . . it appears that the kind of reasoning involved in the legal process is one in which the classification changes as the classification is made. The rules change as the rules are made. More important, the rules arise out of a process which, while comparing fact situations, creates the rules and then applies them. (Ibid.)

Legal reasoning thus is the simultaneous classification of facts, creation of rules, and application of rules. Noting that this kind of reasoning "is open to the charge that it is classifying things as equal when they are somewhat different, justifying the classification by rules made up as the reasoning or classification proceeds" (Ibid., 4), Levi asserted that the legal process must operate in this way:

> Not only do new situations arise, but in addition people's wants change. The categories used in the legal process must be left ambiguous in order to permit the infusion of new ideas. (Ibid.)

These ideas, he continued, are the ideas of the community, reflected in the judgments of its case law and in the (interpreted) meanings of its statutes and its constitution.

Based on this functional characterization of legal reasoning, Levi proposed that the nature of law is both "certain, unchanging, and expressed in rules" and "uncertain, changing, and only a technique for deciding specific cases" (Ibid.). Rather than debating one or the other aspect of this duality as reflecting the "true" nature of law, he proposed that we pay close attention to the processes involved in making legal judgments, recognizing the law forum as "the most explicit demonstration of the mechanism required for a

[194] It is interesting to note that Levi's characterizations are made with respect to the function of legal reasoning, rather than assertions regarding its nature or authority. This emphasis on function is similar to Margaret Masterman's investigation of *Structure's* notion of paradigms (Masterman 1970).

moving classification system" (Ibid.). To function in this way, he noted, the law forum requires the presentation of competing examples and thus operates through reasoning by example.

Considering these processes, Levi noted that the ideas of the community are brought into the legal process through the presentation of competing examples, that is, through the presentation of legal cases. Furthermore, he insisted that it is this form of presentation (rather than the form of judgment) that ensures the fairness of the legal process:

> The ideas have their day in court, and they will have their day again. This is what makes the hearing fair, rather than any idea that the judge is completely impartial, for of course he cannot be completely so. (Ibid., 5)

Thus because the legal process operates through reasoning by example, both the ideas of the community and the distinctions of experts play a decisive role in the development of law. The ideas of the community enter the process as cases and the distinctions of experts shape the resulting determinations of law. Yet while the ideas may change and the adoption of ideas by the courts may be influenced by the power structure of society, Levi asserted that in both cases, reasoning by example will operate to change the idea (and the resulting legal concept) after it has been adopted.

Change, it would seem, is both an inevitable and an important attribute of the legal process. In emphasizing the ever-present possibility for change, however, Levi insisted that we recognize the unique character of legal reasoning and its operation within the legal process:

> The emphasis should be on the process. The contrast between logic and the actual legal method is a disservice to both. Legal reasoning has a logic of its own. Its structure fits it to give meaning to ambiguity and to test constantly whether the society has come to see new differences or similarities. (Ibid., 104)

Legal reasoning thus undergirds and makes possible the dynamism of the legal process. In doing so, it should not be regarded as a "contrast" to logic but as means by which ambiguities can be addressed and areas for doubt may be set forth. Legal reasoning is thus the means by which institution of law goes to work:

> This is the only kind of system which will work when people do not agree completely. The loyalty of the community is directed toward the institution in which it participates. The words change to receive the content which the community gives to them. The effort to find complete agreement before the institution goes to work is meaningless. It is to forget the very purpose for which the institution of legal reasoning has been fashioned. (Ibid.)

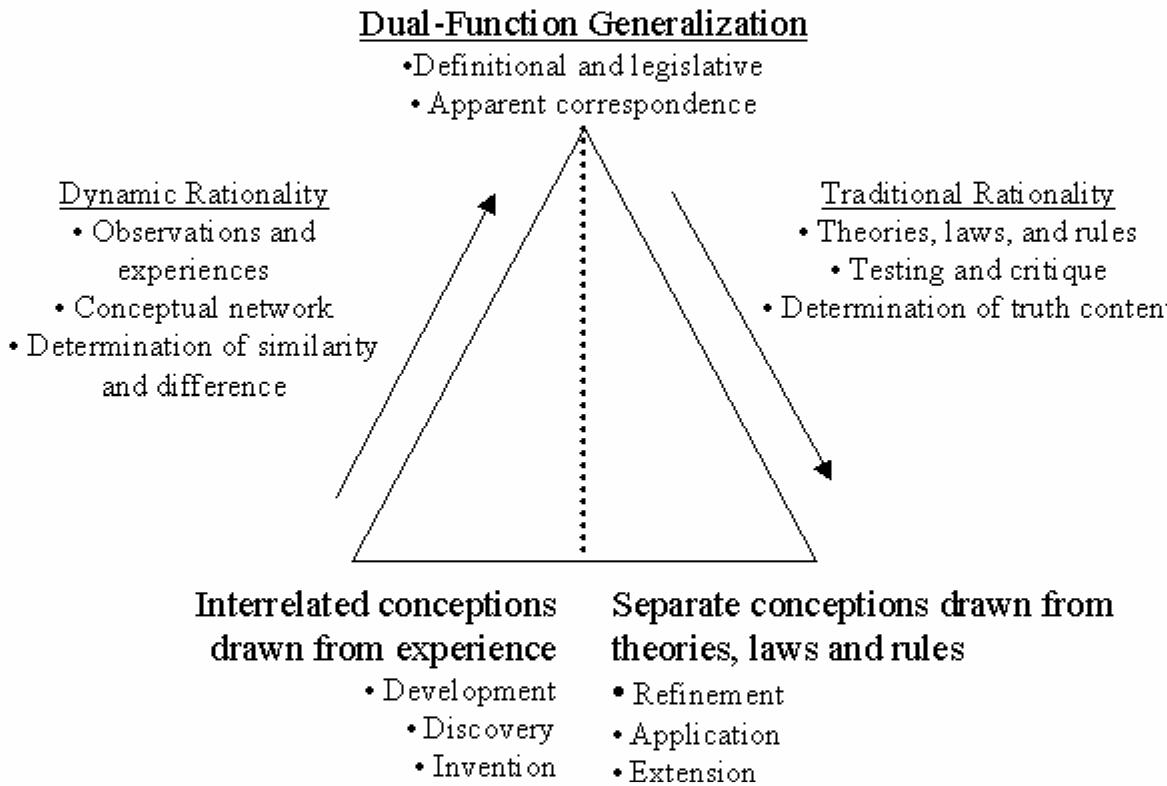
The "rationality" of legal reasoning, while imperfect from the perspective of traditional conceptions, is thus not only appropriate to but also constitutive of the legal process, the legal system, and the law itself. Rather than emphasizing its "imperfections" or debating its true nature, we must recognize that it is, itself, a changing, dynamic process that is deeply interrelated with the ideas, cases, and previous laws to which it is applied and through which it is developed.

The Authority of Dynamic Rationality

Returning to our consideration of the development of science, in what respect is the dynamic rationality of the conduct and investigation of science "rational," even in the imperfect, interrelated sense outlined by Levi? Considering the interrelation of legal reasoning with the cases and prior laws to which it is applied (and by means of which it determines the laws for the current case), we look to the interrelation of dynamic rationality with a particular set of exemplars and their associated theories. As discussed previously, this is the interrelation of conceptions of nature with conceptions of science in order to make determinations of scientific knowledge. In its most developed form, this interrelation takes the form of a dual-function generalization that is both definitional and legislative.

To the extent that the authority of legal reasoning lies in the process of legal reasoning, the authority of dynamic rationality in science will lie in the conduct of science, and the authority of investigations of science will lie in the conduct of those investigations. The operation of dynamic rationality is illustrated in **Figure 6**, below, as the development, discovery or invention of interrelated conceptions drawn from experience. The exercise of dynamic rationality over time will, if successful, lead to the achievement of a dual-function generalization and the establishment of apparent correspondence (the nature of which is still to be examined). This (apparent) correspondence, in turn, provides an epistemological basis for theories, laws, and rules. Once achieved, it provides the basis for the exercise of traditional rationality, that is, for refinement, application and extension of analytically separate conceptions that are drawn from those various generalizations.

Figure 6: The Role of Dynamic Rationality in the Development of Scientific Knowledge



In considering the "apparent" correspondence that is established through the achievement of a dual-function generalization, it is important to note that this correspondence cannot simply be asserted or even negotiated. Rather, it must be achieved by establishing a relationship between observations and experiences and the conceptual network of kind terms within a taxonomy of disciplines. To be considered "science," that is, knowledge of nature, nature must play a role in its establishment and determination.

The way in which this occurs has long been a point of difficulty in proposing such views; however, Kuhn's notion of dual-function generalizations provides much needed clarification as to how direct (apparent) correspondence might be established. Specifically, establishing correspondence requires the development of a (new) dual-function generalization, or the adjustment of a previously established one. Although the former is clearly more difficult than the latter, both involve determinations of similarity and difference, albeit at different levels of complexity.

To the extent that a dual-function generalization can be achieved, a correspondence (of some sort) is thereby established in the interrelation of the dual-function generalization with its terms and exemplars.

This correspondence, while not necessarily "direct," is inherent in the interrelation. The degree of interrelation will depend upon the precision with which the constraints of experience and the constraints of logic may be determined. More precise determination of these constraints – achieved through examination of a greater number of exemplars and more careful determination of their relation within the conceptual network – will yield a more exact, that is, seemingly more "direct," correspondence.

It is important to note, however, that even the achievement of highly exact correspondence will not necessarily reflect a correspondence to "truth," or "what is really out there." This is not to deny the possibility of a realist position – to propose that observations and experiences are embedded in knowledge is to affirm some sort of "reality" that may be known in some way. Yet knowledge of this reality is made possible only through the conceptual network that interrelates multiple observations and experiences into sets. As such, it cannot be considered to be independent or objective knowledge of reality.

To the extent that a conceptual network encompasses an ever-broader and ever-more complex set of observations and experiences, its determinations of similarity and difference will be ever more precise. If it includes only a few, relatively homogeneous observations and experiences, its determinations will be more broad and its expectations and capabilities more general. Yet the emergence of highly specialized determinations and ever-more-complex conceptual networks will be possible only to the extent that the ability to make such determinations and the conceptual network of previous determinations are passed down from one generation to the next.

In this respect, the conceptual network will be limited by the past observations, experiences, and determinations that constitute it. It will be exercised and developed over time only through its use by individuals, communities, and future generations. It may be changed in part or, (over time and step by step), in its entirety as compared with some previous time. Alternatively, it may simply be neglected, in which case it will not be passed along and will be accessible only by something akin to historiographic analysis of out-of-date texts.

The determinations of dynamic rationality thus involve not only the subjective observations and experiences of individuals but also the shared observations and experiences of various communities. The conceptual network employed by an individual will include personal observations and experiences as well as those acquired through formal and informal education. Importantly, an individual need not be a "member" of a community to acquire its conceptual determinations, but need only to be able to communicate with members of that community.

Similarly, we needn't specify that the various generations make their determinations "in the same world." As in the development of case law, any environmental changes will be incorporated into the network through the ongoing consideration of exemplars. To the extent that such changes cannot be incorporated (due to contradiction with other parts of the network, etc.), adjustments will be made until they no longer appear to be necessary – that is, until there appears to be a direct correspondence (both definitional and legislative) between observations and experiences, on the one hand, and the operative conceptual network, on the other hand.[195] In this respect, then, dynamic rationality is not the *result* of (apparent) correspondence. Rather, it is the *source*. In this possibility and this potential achievement – the creation and establishment of an interrelation among facts, theories, and other generalizations such that these then may be separately examined and extended – lies the authority of dynamic rationality.

Implications

Dynamic rationality is not necessarily an "imperfect" form of rationality but is something akin to the process of rationalizing. It is "conducted" by individuals, with the guidance and influence of the broader community. Yet even if a community mandates a particular law or generalization for its members, those individuals may – through the exercise of dynamic rationality – reject that mandate and, if need be, "leave" that community. Dynamic rationality thus involves "imperfect" and changeable determinations, which are made by individuals through the processes of investigation. It is the active and flexible "rationality" of interpretation or language learning, rather than the definitive "rationality" of translation. It is the "rationality" associated with learning from observation and experience (such that these are deeply interrelated), rather than logical deduction or even induction. Finally, it is the "rationality" associated with aesthetic judgments and matters of taste, rather than laws, rules, or first principles.

[195] As Kuhn outlined in *Structure*, the occurrence of an anomaly that may be resolved in one of three ways. It may be assimilated within the established paradigm (conceptual network); it may be deemed "inexplicable" yet of insufficient importance for further research; or it may prompt a revolutionary paradigm shift (i.e., change in the conceptual network).

Yet while the exercise of dynamic rationality remains an exercise or activity that is conducted by particular individuals with respect to particular situations at particular times, it is also the means by which assertions of a broader and more generalizable (even universalizable) scope are made possible. Through the processes of dynamic rationality, cases are grouped together, examined with respect to their similarities and differences, and related with theories or generalizations that both describe and explain them. The determinations made through the exercise of dynamic rationality will differ among individuals depending upon their observation, experience, education, and individual, subjective factors. Yet their use of a shared conceptual network – through their affiliations with various communities – will provide a common basis for evaluating and developing further their ability to exercise dynamic rationality. These will thus provide a common basis for refining the conceptual network within the context of which they make their various determinations.

To the extent that an individual is associated with various communities, her determinations will be both influenced and constrained by the boundaries of their respective conceptual networks. To make determinations that either contradict or lie outside of those boundaries is to risk communication break-down and, potentially, isolation from those communities. Such break-downs or isolation are always possible (and the individual may join others in choosing this path), yet they reflect the choice to disavow the shared commitments of the community, thereby "opting out" of their conceptual network and its associated language games. On the other hand, the use and extension (or interrelation) of those various networks by an individual re-introduces and re-establishes them within the community for further use and extension.

Section Three

The Prospects for Progress

To the extent that the historical developmental perspective entails a conception of dynamic rationality, we must consider the implications of its exercise over time. Thus in order to understand the implications of the historical developmental perspective, we first must understand the implications of historical development. That is, we must examine the operation of dynamic rationality and the implications of its determinations over time. Within the context of science, these determinations constitute the development of scientific knowledge, or what might be characterized as scientific progress. Yet in what respects can the exercise of dynamic rationality over time be said to reflect the development of "knowledge" or to result in "progress?"

"Progress" within the Historical Developmental Perspective

Kuhn proposed that the historical perspective (i.e., what we have re-characterized as the historical developmental perspective) supports an image of progress "from behind." Based on the insights provided by his historiographic and historical developmental investigations of scientific practice, he rejected the possibility for a fixed Archimedean platform that can justify scientific activity, knowledge or belief. More specifically, he rejected the possibility for a mind-independent, objective truth, upon which scientific knowledge might be based and toward which scientific investigations might be directed. Instead, he outlined an historical developmental image of science as investigations that are drawn from and, in turn, effect changes in a series of historically situated platforms. Yet if we are to understand science in this way, on what basis can its development – the change from one historically situated platform to another – be deemed "progress?"

The first principles of Kuhn's historical developmental perspective provide a clue: to the extent that the perspective involves comparative evaluation of proposed changes in an (historically situated) platform – that is, the conceptual network – "progress from behind" reflects progress in the shift from one platform to the next. To the extent that "progress" is achieved, this occurs through a series of particular choices to make particular changes. That is, it is achieved through the operation and determinations of dynamic rationality over time. What we must understand, then, is how the determinations of dynamic rationality are related over time and how (i.e., with respect to what) they can, collectively, be said to represent progress.

Returning to Edward Levi's account of the development of case law, we might ask how the law can be said to progress, and to what extent it, too, reflects progress "from behind." As a moving classification system, "progress" in case law occurs through changes in its classification of cases. The determination of these changes will be made (implicitly) through the pursuit of cases by members of the community and (explicitly) by the rules of law laid down by judges. Case law thus changes when particular cases and particular judges determine that it should change. The "progress" of case law thus does not reflect incremental movement toward an external, abstract principle (although such principles may be applied when deciding to pursue a case or when making a judgment). Rather, the "progress" of case law reflects the operation of the processes and the determinations that support the pursuit and resolution of particular cases.

Although this account supports Kuhn's characterization, it remains incomplete. How can the determination of any particular case – or even a series of particular cases – be said to reflect progress in the entire domain of case law? How, that is, can a particular determination be taken as "progress" for the whole? The answer lies in the structure and processes of case law, that is, in Levi's conception of the practice of case law as reasoning by example. As in Kuhn's alternative approach to the causal theory of reference, the changing nature of case law renders it best understood through the cases that constitute it at a particular point in time, just as historiography suggested that the "science" of a particular time is best understood by studying the activities and the exemplars of that time.[196] Given the changing nature of both law and science, a change in the determination of cases or exemplars will reflect a change in the conduct of the field and, thus, a change in the field itself.

In considering these assertions, we seem to have returned once again an image of science (and, we might add, case law) that undermines authority and is subject to charges of irrationalism, relativism and mob psychology. Even if we avoid accusations of irrationality on the grounds that legal (or scientific) reasoning is a form of reasoning, however imperfect, we still must confront the absence of guiding criteria for judgment (and thus the prospect of relativism). We must also confront the possibility of mob

[196] It is important to note, however, that these assertions reflect a characterization of law and science as activities, or practices. This implication will be examined in detail subsequently.

psychology on the part of either the litigants (i.e., those who present the cases – in science, the anomalies) or the judges (i.e., those who would decide the cases, or "explain" the anomalies). Without clear rules for determining either the cases that are examined or the judgments that are made, how can processes alone ensure that the decisions that are made will, in fact, represent "progress?" In short, if science and law are to be understood as practices or activities, rather than as (logically justified and thus authoritative) rules or laws, how are we to justify the activities of science? What can be the authority of scientific practice?

Scientific Progress and the Authority of Scientific Practice

The assertions made above with regard to the structure of science or case law mislead in their simplicity and thus in their implication that the change in a particular case will, simply by definition, reflect progress for the field. For the practice of case law extends beyond prior cases to the individuals and institutions that (also) constitute the legal system and its processes. Similarly, the practice of science extends beyond particular theories to the individuals and institutions that are involved in or influence the activities of science.

While the change from a particular case or exemplar will effect a change within the broader field of case law or science, that change will extend beyond the case or exemplar to the individuals and institutions engaged in the field. Specifically, the change will affect *how* those individuals and institutions are engaged within their field. By affecting their (future) activities and influences, it will affect the future practice (and determinations) of their field. How, then, are we to understand these individuals and institutions? What are the processes (and determinations) whereby changes in particular cases or exemplars affect their activities and influence within a field? How do these processes and determinations support scientific progress and provide the authority of scientific practice?

In case law, the individuals who are involved in the practice of law include all members of the legal "community:" those who bring the cases (in our system, those who are governed by the case law) and those who adjudicate them. In science, the relevant individuals include all scientific practitioners. In both fields, there are individuals who are not directly involved in the practice of the field but, nonetheless, may influence it in some way. This includes individuals who are interested in or investigate the field (i.e., philosophers and historians of law or science, as well as reporters and other "popular" commentators)[197] as well as those who may regulate or fund it.[198]

The institutions involved in and influencing the practice of law include not only official legal institutions, such as the law courts and their associated products, resources and methods (laws, statutes, cases, decisions, etc.) but also educational institutions (law schools, textbooks, case methods of teaching, professional training, etc.), and even political, regulatory, and other sociological institutions (e.g., Bar associations, codes of conduct, etc.). Similarly, in science, one must recognize the influence of not only research institutes and laboratories, along with their associated products, resources (i.e., instruments or facilities) and methods, as well as educational, political, regulatory and other sociological institutions.[199]

To the extent that a particular change can be said to reflect progress with respect to its broader domain, this will occur through the influence of that change upon the individuals and institutions that (also) constitute that domain. With the change, individuals will have to adjust some of their previous conceptions and thus the basis of some of their future decisions. Similarly, a wide range of supporting institutions may be affected, including education, documentation, instrumentation, etc. Each of these points of change reflects a decision or determination, and each of those determinations provides the basis for considering further possible points of change.

Given the example-based nature of case law and of science, a (seemingly) particular change thus may have wide-ranging effects. The change will be assimilated into the larger processes and structures that support activity within the field, thus becoming established as an integral part of the field and its activities. In this way, however, the "change" itself will become subject to change, and its influence on individuals

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[197] It is not clear to what extent philosophers or historians of science are involved in or even influence the practice of science. Kuhn noted that philosophy of science is notably different from other areas of philosophy in that many scholars do not have detailed knowledge of the field. Margaret Masterman's comments suggest that *Structure* was unique in that it appealed to scientific practitioners, in contrast to the "aetherial" works of the philosophy of science. Furthermore, it is not yet clear what standards to establish for asserting the influence of those who would investigate the practice of science upon those who practice it.

[198] The regulation or funding of law or science can arguably be characterized as a reflection of regulatory or financial aspects of scientific institutions. This highlights the "looseness" of the distinction between individuals and institutions, in that individuals often act through institutions, just as institutions may be influenced by individuals. This relationship and interdependency is highly relevant to our investigation but must remain an issue for further exploration.

[199] This wide-ranging conception of "science" and "law," which is based upon consideration of their activities and practices, is reminiscent of Kuhn's early attempts to clarify the notion of paradigms with respect to the scientific community of specialists and the disciplinary matrix (*SSR-PS* 1969/1970A).

and institutions within the field will emerge only gradually. Over time, some or all aspects of its laws may be overturned. Over time, some or all aspects of its theories may be changed or rejected. Over time, some individuals will abandon the field, while others will enter it. The cycle of change thus will continue – gradually, and over time.

The influence of a change thus extends beyond its immediate impact, that is, beyond its particular case and even beyond its initial influence on individuals and institutions within the field. The full implications of a particular change may not be recognized when its initial determination is made. In fact, its full implications may not be evident until they are highlighted by one or more (later) cases or anomalies. Yet because a change will be incorporated within the field, its implications and influences will – over time – become deeply interrelated with the implications and influences of both previous and subsequent changes. In this respect, it will be difficult, if not impossible, to distinguish (or to evaluate) the full implications of a particular case. Instead, it would seem that one can only compare the "historically situated platform" of case law or science at one particular time with that of another time. Furthermore, one must understand this "platform" to include not only the laws or theories but also the individuals and institutions that establish, interpret, apply and support them.

In this respect, the progress of science and the authority of scientific practice are not reflected in a particular determination to change one or another aspect of scientific law or theory. Rather, the progress and authority of scientific practice (and thus, the progress and authority of "science") lie in the way that the laws, individuals, and institutions of science are interrelated as the conduct of scientific activity and the determination of scientific knowledge over time. It is this interrelation – rather than any particular determination or any singular, historically situated set of determinations – that provides the basis for the ongoing "practice" of "science."

This conception of science – as scientific practice involving changes in and determinations of its interrelated laws, individuals and institutions over time – provides an interesting basis from which to reconsider the charges of irrationality, relativism and mob psychology. Specifically, the charges seem to reflect an interrelation that parallels the one we have proposed for Kuhn's image of the nature of science. Charges of irrationality reject the assertion that "scientific" practice can be conducted without the authority of established laws, rules, or theories. Charges of relativism reject the claim that the choice between alternative ways of practicing science is influenced by individual or subjective factors. Finally, charges of mob psychology suggest that even the determinations of a specialist scientific community may be wrong-headed if they are made solely on the basis of the authority of that community.

In each of these charges, then, we see a presumed "failure" in some aspect of our broader conception of science, that is, in its laws, its individuals, or its institutions. Each of these concerns is serious and presents a challenge to the conduct of "science," as it is traditionally conceived. Yet if we understand science according to our broader view of the interrelation of its multiple aspects through its practice over time, these concerns can be somewhat mitigated. Tendencies toward irrational laws may be offset or addressed by a commitment to rationality by individuals or (implicitly) institutionalized structures. Tendencies toward relativistic judgments by individuals may be countered by reliance on established laws and the determinations inherent in established institutions. Finally, tendencies toward mob psychology may be challenged by contradictions with respect to established laws or by the challenges presented by individuals. In short, the ongoing conduct of science as "scientific practice" serves to reinforce, to refine, to readjust, and, occasionally, to revolutionize the laws, individuals, and institutions of science.

Our proposed image of the nature of science thus addresses these charges as the interrelation of laws, individuals and institutions expressed through determinations that are made over time. For it is only over time – and with recourse to dynamic rationality – that irrationality, relativism or mob psychology can be avoided, or, more precisely, identified and addressed. Furthermore, it is these determinations and this application of dynamic rationality that represent the ongoing activities and the continued operation of "science." In this we have, then, something akin to a "near-trivial" justification for the practice and the authority of science.

Philosophical Implications

Given this preliminary view of the nature, authority and operation of Kuhn's historical developmental perspective and its dynamic form of rationality, how are we to understand its philosophical implications or, for that matter, its implications for philosophy? More precisely, how are we to understand the relationships that exist among the various perspectives that constitute the "taxonomy of disciplines" engaged with the conduct and the investigation of scientific practice? If we are to support a "near-trivial" justification for the exercise of an "imperfect" form of rationality, how are we to understand the more authoritative justifications that are provided by logic and the exercise of traditional forms of rationality? How are these various perspectives and their associated views of rationality to be considered in relation to a sociological view of science?

While our investigations suggest that the traditional philosophical and sociological perspectives of science are limited with respect to the conduct of scientific activity over time, the historical developmental perspective seems to provide the basis not only for a broader view of both science but also for the taxonomy of disciplines within which it is, itself, understood. Just as the historical developmental perspective highlights the early stages and the transitions that occur in the development of a science, so it reveals the early stages and the changes in investigative activities that occur in the development of knowledge. While a philosophical perspective is based upon a more established "authority" and a sociological perspective provides greater insight into the nature and operation of the underlying community, an historical developmental perspective provides the basis by which changes in the nature and the operation of these may be revealed, examined, and understood.

In this respect, the historical developmental perspective provides valuable insights regarding the "crisis of authority" that has engaged sociologists, philosophers, and others. By revealing the interrelation of laws, individuals and institutions, it affirms the role of logic, psychology, sociology, etc. in influencing the development of knowledge over time. Yet by examining these various elements in relation to one another and with respect to the changes of each over time, it highlights both the determinations and the constraints that each imposes within the developmental process.

The development of knowledge thus may be viewed from a number of different (yet related) perspectives. Within an historical perspective, knowledge will be investigated with respect to the entities and puzzle-solutions that are operative during a particular historical period. Within a traditional philosophical perspective, it will be examined with respect to the most current conceptions of entities and puzzle-solutions. Within a sociological perspective, it will be examined with respect to the various communities, affiliations, and members that are engaged with its development, establishment, and refinement. Finally, within an historical developmental perspective, knowledge will be considered with respect to its change and development over time, through its laws, individuals, and institutions. Each of these represents a valuable and important way of understanding knowledge.

In order to gain a comprehensive view of the "nature" of knowledge, it seems that all of these perspectives are needed. Thus in addressing issues of cognitive authority, we must either specify the perspective from which we are examining the issue (in which case, the conclusions will be limited to that perspective), or we must find a way to consider multiple perspectives. If we are to conduct such a complex examination, we must recognize the different interrelations of assumptions and approaches that are inherent in each of the different perspectives. We must find a way both to identify and to evaluate the distinctive points of difference among the various views and approaches. While it still requires further development, an historical developmental perspective might provide further insight here.

A Reconsideration of Legacy

Kuhn's work and the interpretive debates surrounding it can be understood as a search for first principles in the face of challenges to established views of authority, knowledge, and truth. The varied interpretations and evaluations of *Structure* were made on the basis of the views and approaches of particular fields yet were all conducted within the broader context of the crisis of authority. Sociologists recognized a broader role for sociological concerns and responded by rejecting traditional conceptions of authority as based upon logic and the conception of an objective, mind-independent truth. Philosophers viewed the challenge somewhat differently, rejecting the proposals of sociologists in favor of adjustments to the methods and the bases for asserting epistemic authority. Kuhn, like the sociologists, perceived the limitations of traditional philosophical views and responded by attempting to broaden conceptions of knowledge beyond the determinations of logic and method. In doing so, he echoed the concerns of sociologists, yet he differed in his proposed response. While rejecting traditional views of the basis for cognitive authority, he did not intend to undermine that authority *in toto*.

The legacy of Thomas Kuhn should be understood as a search for first principles on the basis of an historical developmental perspective. From his exclamation, "I just want to know what Truth is!", to his Aristotle experience, development of the theory of *Structure* and examination of its philosophical implications, Kuhn dedicated his research to understanding the basis for (scientific) knowledge and how it develops. His research methods and his theory changed over time, with various aspects (and their relationships) abandoned, refined, added, or extended. In this respect, Kuhn's work was highly developmental – a fact that is consistent with the revolutionary nature of its conclusions and implications. In the same vein, the work was highly controversial, widely misinterpreted, and frequently dismissed. Again, this is entirely consistent with the expansive scope and ambitious objectives of Kuhn's project. More importantly, it is to be expected, based on his theory.

To a great extent, the interpretive debates surrounding *Structure* may be combined with the work itself, Kuhn's subsequent development of its philosophical implications, his identification of the first principles of the historical developmental perspective, and this investigation to present a new image of the nature of science, in particular, and the nature of knowledge, more generally. The theory presented *Structure* was not fully developed; however, it is not clear that even a more comprehensive account would have been sufficiently compelling to effect the change that Kuhn sought. In this respect, the misinterpretations of *Structure* not only support the theory presented in the work but also provide a clear exemplar of the limitations of traditional approaches. On the other hand, even with (and, to some extent, because of) those misinterpretations, the interpretive debates prompted and implicitly directed much of Kuhn's subsequent investigations, highlighting the ambiguities, points of distinction, and limitations of the theory presented in *Structure* and indicating the areas in which it required further development.

To the extent that Kuhn's work reflects the introduction of "a new order of things" (p. viii) both his theory and his experience suggest that revolutionary changes of ideas must be accompanied by changes in fundamental assumptions and approaches, accompanied by strongly argued challenges. It is only through the reconstruction of this deep interrelation that a new order can be developed and established, yet such dramatic, far-reaching, and tumultuous changes cannot be taken lightly. While such efforts are, as Machiavelli suggested, "difficult to take in hand," "perilous to conduct" and "uncertain in outcome" (p. viii), this difficulty, peril, and uncertainty are necessary parts of the developmental process.

In reconsidering *Structure* and the interpretive debates surrounding it, this investigation suggests that they represent interrelated yet still incomplete efforts to address the challenges raised by the crisis of authority. While the crisis requires much further investigation and clearer accounts must still be developed, this broader view suggests areas in which that investigation and those accounts might be most effectively conducted and developed. In particular, it highlights the possibilities provided by the historical developmental perspective in revealing and considering the various determinations and constraints to be provided by a consideration of activities, investigations, and exemplars.

In reconsidering the legacy of Thomas Kuhn, we must reject Fuller's suggestion that Kuhn was a "culturopath," unaware of the underlying issues and tensions of his times. To the contrary, it appears that Kuhn was, perhaps, a man just ahead of his times. Intent in his quest to "know what Truth is," he was willing to question established views while continuing both to ask the question and to insist upon its importance and relevance. Although Kuhn's reach seems to have exceeded his grasp, we must embrace the leadership, initiative, and creative genius that have allowed us to reconsider the challenges, questions, and possible alternatives that he investigated.

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Ph.D. in Philosophy. One of 40 national recipients of the Jacob K. Javits Fellowship in the Humanities, a four year fellowship with tuition and annual stipend.

1989 – 1991 **Harvard University Graduate School of Business Administration** **Boston, MA**
Master of Business Administration with emphasis on strategy and competition.

1982 – 1986 **Vanderbilt University** **Nashville, TN**
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Academic Appointments

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Research Associate in the Office of Research Integrity **University Park, PA**
Evaluated proposals for federal regulation of research involving human participants.

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Research Associate to Professor Michael E. Porter **Boston, MA**
Conducted research and analysis for Professor Michael E. Porter in the fields of strategy, competition and economic development. Published case studies for graduate courses.

Private Sector Experience

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Directed Consulting Services engagements for a $120 million internet services firm. Developed business and marketing strategies for General Motors and Delta Airlines.

1996 – 1997 **Salzinger & Company** **Vienna, VA**
Developed a business strategy and $50 million offering for a division of America Online.

1995 – 1996 **Boston Ventures Management, Inc.** **Boston, MA**
Evaluated investments for a private equity firm with $500 million in its fourth fund.

1993 – 1995 **PepsiCo, Inc.**
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Analyzed financial and operating performance and evaluated investment decisions for the senior executives of Fortune 500 companies in the insurance, paper, and travel industries.

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