Thomas Kuhn's Revolution: An Historical Philosophy of Science

JAMES A . MARCUM

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An Historical Philosophy of Science

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James A. Marcum

Preface

Thomas Samuel Kuhn (1922–1996), although trained a physicist at Harvard University, became an historian and a philosopher of science through the influence and support of Harvard's president James Bryant Conant. In 1962, Kuhn's renowned work, *The Structure of Scientific Revolutions*, was the last installment in Otto Neurath's *International Encyclopedia of Unified Science*. Kuhn's book helped to inaugurate and promote a revolution—the historiographic revolution in the history and philosophy of science—in the latter half of the twentieth century. The revolution provided a new image of science in which periods of stasis (normal science) are punctuated by sporadic upheavals (scientific revolutions). It not only influenced the history and philosophy of science, Kuhn preferred the term historical philosophy of science, but other disciplines as well, such as sociology, education, economics, religion, political science, and even science policy.

My first encounter with Kuhn was as a postdoctoral fellow at Massachusetts Institute of Technology in the early 1980s. A friend of mine lent me a copy of *Structure*, but upon a first reading I was not impressed with its image of science in terms of my experience as a scientist-in-training. I then learned that Kuhn was teaching a course on the nature of scientific knowledge, during the 1982 spring semester. I approached him about taking the course and he graciously permitted me access to it. It was then that I began to appreciate Kuhn's new image of science, one that was dynamic as opposed to the static image I had learned through my scientific training. From my experience in that course and from a continued relationship with Kuhn, I gradually switched from a career in the biomedical sciences to one in the history and philosophy of science. My personal recollection of Kuhn is of a man who cared deeply not only for the subject matter of his adopted discipline but also for his students and colleagues.

Since his death in 1996, the literature on Kuhn and his philosophy of science continues to escalate. Within the past decade or so, around a dozen book-length studies have appeared on Kuhn. These consist of general surveys of Kuhn's philosophy of science and of studies that focus on specific theses arising from Kuhn's work, especially the incommensurability thesis. Recently, Steve Fuller argues that Kuhn is not responsible for the historiographic revolution others claim he is, but rather that he was a bystander to more powerful events and personages. One of the purposes of the present book is to contribute to that discussion, by the examining of the development of Kuhn's historical philosophy of science and the impact it had not only on the history and philosophy of science but also on other disciplines.

The primary focus of the present book is to provide a reconstruction of the development of Kuhn's historical philosophy of science. To that end, I focus on Kuhn's philosophy and on questions surrounding it. Who is Kuhn? How does he arrive at *Structure*? What is *Structure*? Why does Kuhn revise *Structure*? What is Kuhn up to after *Structure*? What is Kuhn's legacy? What is Kuhn's stake in the historiographic revolution? What is Kuhn's impact on the history and philosophy of science? Why is Kuhn misunderstood? At the heart of the answers to these questions is the person of Kuhn himself, i.e. his personality, his pedagogical style, his institutional and social commitments, and the intellectual and social context in which he practiced his trade. In a developmental approach to Kuhn's ideas, I map in detail the unfolding of his ideas from work in the 1950s on physical theory in the Lowell lectures and on the Copernican revolution to work in the 1990s on the historical philosophy of science and the incommensurability thesis. Rather than present Kuhn's ideas as finished products, I strive to capture them in the process of their being formed. By following the development of Kuhn's ideas, I believe a more accurate representation of his ideas is possible.

To that end I examine, in the first chapter, the person of Kuhn and his career, including his family and schooling. In the next chapter, I explore the path to *Structure*, beginning with the 1951 Lowell lectures and concluding with the 1961 "Dogma" paper. In the third chapter, I outline *Structure*'s major themes, including the notions of paradigm and paradigm shift, normal and revolutionary science, and incommensurability. In the following chapter, I review the various criticisms leveled against Kuhn's book and especially the important 1965 London colloquium in which Kuhn and Karl Popper exchanged critiques of each other's philosophy. I also examine Kuhn's response to critics, in which he revises some of his ideas in *Structure*. In the fifth chapter, I discuss Kuhn's effort, from the early 1970s to end of his career, to refine and clarify the new image of science and to answer critics. In a final chapter I examine Kuhn's legacy, in terms of the impact his ideas, especially paradigm and paradigm shift, had on the history and philosophy of science, as well as on other disciplines such as sociology, economics, religion, natural science, and even science policy. In an epilogue, I evaluate briefly Kuhn's role in the historiographic revolution.

My thesis is that Kuhn was a major contributor to the historiographic revolution in the mid-twentieth century, which not only influenced how the history and the philosophy of science are done today, but the very understanding of science itself. The revolution's influence transcends the boundaries of the history and the philosophy of science communities to include other professional communities as well. Although the present book is primarily an introduction to the development of Kuhn's historical philosophy of science, it is also a sustained argument that establishes this thesis and strives to interpret and situate Kuhn within a larger academic framework than simply the history and philosophy of science.

James A. Marcum

PART I

The path to *Structure*

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Chapter 1

Who is Thomas Kuhn?

During the year that Moritz Schlick moved from Kiel to Vienna, thus inaugurating the Vienna circle, Thomas Samuel Kuhn was born in Cincinnati, Ohio, on 18 July 1922. He was the first of two children born to Samuel L. and Minette (née Stroock) Kuhn, with a brother Roger born several years later. His father was a native Cincinnatian and his mother a native New Yorker. The family, states Kuhn later, was "certified Jews. Non-practicing Jews. My mother's parents had been practitioners, not Orthodox practitioners. My father's parents had not."[1] When Kuhn was six months old, the family moved to New York. But other members of the Kuhn family, including a favorite aunt, Emma (née Kuhn) Fisher, Sam's younger sister, remained in Cincinnati. Aunt Emma was a source of inspiration for Kuhn. During World War II, she opened her home to a young German Jewish refugee. Kuhn inscribed a copy of *Structure* to her with these words: "For Emmy—who as Aunty Emma—helped me find what I was and liked."

Kuhn's father, Sam, was a hydraulic engineer, trained at Harvard University and at Massachusetts Institute of Technology prior to World War I. He entered the war, and served in the Army Corps of Engineers. According to Kuhn, these were the best years of his father's life. After leaving the armed services, Sam returned to Cincinnati to help his recently widowed mother Setty (née Swartz) Kuhn. His father's career after moving to New York, however, was a disappointment, as Kuhn later remembered: "he was never, I think, the sort of success he had expected to be and under the circumstances might have been."[2] But Kuhn admired his father and considered him one of the brightest people he knew, next to Conant.

Kuhn's mother, Minette, was a liberally educated person, who on occasion did professional editing. She came from an affluent family and her stepfather was a lawyer. Minette's biological father died from tuberculosis shortly after her birth. Although Kuhn thought of his mother as more of an intellectual than his father, in that she was well read, he considered her not as bright as his father. Kuhn recalls later that everyone claimed he took after his father and his brother after their mother. But he later recognized that the opposite was true. "I finally realized," recollected Kuhn, "that it was because theoretical physics was more nearly an intellectual activity and I was following my mother at this point, not my father."[3] Minette took an active interest in her son's career and read and discussed his books with him.

Kuhn's early education reflected the family's liberal progressiveness. In 1927, Kuhn began schooling as a Kindergartener at the progressive Lincoln School in Manhattan. "Progressive education," according to Kuhn, "was a movement which . . . emphasized subject matter less than it emphasized independence of mind, confidence in ability to use one's mind."[4] Kuhn was taught to think independently, but by his own admission there was little content to the thinking. Kuhn remembered that by the second grade, for instance, he was unable to read proficiently, to the consternation of his parents.

Beginning in the sixth grade his family moved to Croton-on-Hudson, a small town about 50 miles from Manhattan, and the adolescent Kuhn attended the progressive Hessian Hills School. According to Kuhn, the school was staffed by left-oriented radical teachers who taught the students pacifism. When he left the school after the ninth grade, Kuhn felt he was a bright and independent thinker. After spending an uninspired year at the preparatory school Solebury in Pennsylvania, Kuhn spent his last two years of high school at the Yale-preparatory Taft School in Watertown, Connecticut. He was even less enthusiastic for it, but felt that it gave him "more formal training."[5] Kuhn graduated third in his class of 105 students and was inducted into the National Honor Society. He also received the prestigious Rensselaer Alumni Association Medal.

Kuhn wrote a number of student essays on various topics, ranging from student strikes to tariffs. One essay, "Some things about E—," captures Kuhn's struggle to articulate the ineffable, a struggle that plagued him for the rest of his life. The essay is obviously about Aunt Emma. After describing certain ineffable features of his aunt, Kuhn concludes the essay writing, "she has other qualities I would like to express, but I can't seem to catch and untangle them. I wish I could!"[6] This essay must be contrasted with others on technological devices. For example, in essays on the telegraph relay switch and the ice box, Kuhn provided both accurate and modestly detailed descriptions and drawings, with little anxiety expressed over depicting them. Kuhn also exhibited interest in literature, with an essay on a minor character in Zweig's *The Case of Sergeant Grisha*.[7] In the essay, Kuhn reveals an early ability to place himself within a text and explore the development of its characters, an ability that would serve him well when he shifted from science to history.

The Harvard years

Undergraduate education

Kuhn recalled that during his grammar and high school years, he "had almost no friends. I was isolated . . . I was quite unhappy about it. I wasn't a member of the group and I wanted terribly to be a member of the group."[8] All of that changed for Kuhn when he matriculated to Harvard College in the fall of 1940, following in his father's and uncles' footsteps. At Harvard Kuhn was to acquire a better sense of himself socially, by participating in various organizations. During his first year at Harvard, Kuhn took a year-long course in philosophy. In the first semester he studied Plato and Aristotle, while in the second semester he studied Descartes, Spinoza, Hume, and Kant. Although he found them stimulating and challenging, it was Kant that was a "revelation," especially Kant's categories and the synthetic *a priori*. Kuhn later considered himself "a Kantian with movable categories."[9] He intended to take more philosophy but could not find the time. He did, however, attend several of George Sarton's lectures on the history of science, but found them "turgid and dull."[10]

Kuhn wrote several undergraduate essays that reveal his early interest in metaphysical issues. The first essay, "An analysis of causal complexity," was for a philosophy course taught by D. C. Williams. Kuhn wrote that:

> The essay represents an attempt to analyze the notion of cause so as to eliminate from it those elements which are irrelevant to a metaphysically reasonable formulation of scientific law and an effort to investigate the possible epistemological grounds of the remaining concept.[11]

Kuhn drew upon the work of Bertrand Russell and David Hume to complete the task. Williams found the essay "generally accurate and elegant" but felt that it needed "ripening."[12]

The second essay, "The metaphysical possibilities of physics," was for an English course taught by Mr Davis. Kuhn asked the question of whether physics is capable of discovering an exhaustive conception of the universe. To address this question, Kuhn proposed a two-step investigation. The first is to determine the nature of the data and whether it yields a finite amount of information about the universe. Obviously a finite amount of information would be conducive to comprehending it, rather than an infinite amount. The second step is to determine the relationship between concepts and data/information. That relationship is derivative: "They are generalizations made to fit the data."[13] This led Kuhn to the questions of "how are they derived and to what extent are they logically necessary?"[14] But he had no answers.

In the essay, Kuhn also addressed the question of how many concepts can be derived from data and information. In principle, Kuhn believed a limited number of concepts are possible. But they may not provide the necessary knowledge about the world, only that the world is knowable. The problem is picking out the right concept from all the concepts derivable to explain the data. But Kuhn felt confident that even if there are an infinite number of concepts derived from the data, physicists would eventually arrive at one to explain the universe even though there would always be some question concerning its veracity. "But if this investigation, correctly performed, yielded the possibility of but one concept," concluded Kuhn, "we would believe that science could in time arrive at a picture of the universe, and that that picture would be an image of the reality."[15]

During his first year at Harvard Kuhn was torn over majoring in either physics or mathematics. After seeking counsel from his father, he chose physics because of greater career opportunities. Interestingly, one of the attractions of mathematics and physics was their problem-solving traditions.[16] In the fall of Kuhn's sophomore year, the Japanese attacked Pearl Harbor and Kuhn expedited his undergraduate education by going to summer school. The physics department focused on predominantly electronics, and Kuhn followed suit. He did not have a course in relativity until graduate school.

During his sophomore year, Kuhn underwent another radical transformation. Although he was trained a pacifist, the atrocities perpetrated in Europe during World War II, especially by Hitler, horrified him. Kuhn experienced a crisis, since he was unable to defend reasonably pacifism. The outcome was that he became an interventionist, which was the position of many at Harvard, especially Conant its president. The episode left a lasting impact upon him. In a Harvard *Crimson* editorial, he supported Conant's effort to militarize the universities in the United States. The editorial, of course, came to the attention of the administration, and eventually Conant and Kuhn met—an experience Kuhn relished and never forgot. Their relationship was cordial, although Kuhn found Conant reticent.

Kuhn graduated from Harvard College with an S.B. (summa cum laude) in the spring of 1943 and was invited to present the Phi Beta Kappa address. In the speech, Kuhn began by affirming the importance of a liberal arts education. However, the issue he addressed was his generation's skepticism precipitated by the world wars. Although he did not resolve that skepticism, he turned to the humanities tradition in which

Harvard had indoctrinated him: “it is thus most of all from the sense of being part of a tradition that we can turn from the edge of nihilism with a positive faith, a faith which causes us to say, we will not depart from the way of life we have learned here.”[17] He concluded the address by quoting from a *Crimson* editorial, which identified the basis of this way of life as “Veritas.”

After graduation, Kuhn worked for the Radio Research Laboratory located in Harvard’s biology building. He conducted research on radar counter technology, under John van Vleck. The job procured for Kuhn a deferment from the draft. After a year, he requested a transfer to England and then to the continent, where he worked in association with the U.S. Office of Scientific Research and Development. The trip was Kuhn’s first abroad and he felt invigorated by the experience. For example, he was in France when de Gaulle entered Paris. However, Kuhn came to realize that he did not like radar work, which led him to reconsider whether he wanted to continue as a physicist. “I was beginning to get doubts,” Kuhn later recalled, “as to whether a career in physics was what I really wanted.”[18] But these doubts did not dampen his enthusiasm for or belief in science. During this time, Kuhn had the opportunity to read what he wanted; and he read in the philosophy of science, including authors such as Bertrand Russell, P. W. Bridgman, Rudolf Carnap, and Philipp Frank.

Graduate school

After V.E. day in 1945, Kuhn returned to Harvard. As the war started to abate with the dropping of the atomic bombs on Japan, Kuhn activated an earlier acceptance into graduate school and began studies in the physics department. However, Kuhn convinced the department to allow him to take philosophy courses during his first year. “I took two courses [relational logic and metaphysics],” remembered Kuhn, “and I realized that there was just a lot of philosophy I hadn’t been taught, and didn’t understand.”[19] Kuhn again chose the pragmatic course and focused on physics. While a graduate student, he was also a tutor in Kirkland House. In 1946, Kuhn passed the general examinations and received a master’s degree in physics. He then began dissertation research on theoretical solid-state physics, under the direction of van Vleck. The dissertation’s title was “The cohesive energy of monovalent metals as a function of their atomic quantum defects.” In 1949, Kuhn was awarded a doctorate in physics.

On 27 November 1948, Kuhn married Kathryn Muhs. She was born in Reading, Pennsylvania, in 1923, and graduated in 1944 from Vassar College. They had three children: Sarah (b. 1952), Elizabeth (b. 1954), and Nathaniel (b. 1958). Kuhn’s wife was instrumental in and supportive of his career, typing out his doctoral dissertation and encouraging his passion for scholarly work. In appreciation, Kuhn called her "his favorite epistemologist."[20] He also expressed his appreciation and gratitude to his family in *Structure* for their support and encouragement. Kuhn had a warm and caring relationship with his three children to whom he dedicated his last book with the inscription "For Sarah, Liza, and Nat, my teachers in discontinuity."[21]

In 1943, Conant assembled a committee to examine general education at Harvard. After the committee issued its report, *Objectives of a General Education in a Free Society*, a précis of it appeared in a September 1945 issue of the *Harvard Alumni Bulletin*, along with the opinions of 12 Harvard professors on it. Kuhn was selected to represent the student perspective. In an essay, "Subjective view," Kuhn acknowledged that the report points in the right direction because the increased scientific facts obtained within the last century cannot be taught through traditional means. But he concluded that the success of the general education reform depends on professors who are

> undoubtedly both scholars and teachers, but such men are rare. Far more numerous are the inspiring teachers whose scholarship lacks profundity and the profound scholars whose teaching lacks appeal. The University may have to increase the proportion of its staff in the first of these categories if the general education program is to realize the maximum of its great potential.[22]

One of the impetuses for revising Harvard's general education curriculum was Conant's and others' desire to educate the general public about science and its role in the modern world's prosperity. Moreover, politicians of American domestic policy realized that an educated populace concerning science would be more sympathetic for funding its research. Science was the new frontier for Americans, as Vannevar Bush so aptly articulated in *Science, the Endless Frontier*. "During the immediate postwar years," as Kuhn explained later, "there was much discussion of what every educated voter ought to know about science, and there were numerous experiments with special science courses for the non-scientist."[23] Conant's approach was through the history of science, which was unique among the approaches taken at other institutions.

Although Kuhn had high regard for science, especially physics, he was unfulfilled as a physicist and continually harbored doubts during graduate school about a career in physics. He had chosen both a dissertation topic and an advisor to expedite obtaining a degree or "walking papers."[24] But he was to find direction for his career through an invitation from Conant in 1947 to help prepare an historical case-based course on science for upper-level undergraduates. Conant had recently outlined his strategy to educate the American populace, using the history of science, in *On Understanding Science*. Kuhn accepted the invitation to be one of two assistants for Conant's course. As Kuhn recalled later:

> At our first meeting, Conant turned to me and said "I can't imagine a General Education course in science that doesn't have something about mechanics in it. But I'm a chemist, I can't imagine how to do that! You're a physicist, go find out!"[25]

And with that assignment, Kuhn undertook a project investigating the origins of seventeenth-century mechanics, a project that would transform his understanding of the nature of science.

That transformation came, as Kuhn recounted on a number of occasions, on a "memorable (and very hot summer) day" in 1947 as he struggled to understand Aristotle's idea of motion in *Physics*. John Horgan narrated the event from an interview with Kuhn accordingly: "Kuhn was pondering this mystery, staring out of the window of his dormitory room ('I can still see the vines and the shade two thirds of the way down'), when suddenly Aristotle 'made sense'."[26] The problem was that Kuhn tried to make sense of Aristotle's idea of motion using Newtonian assumptions and categories of motion. Once he realized that Aristotle had to be read using the assumptions and categories contemporary when *Physics* was written, suddenly Aristotle's idea of motion made sense.

From this experience, Kuhn formulated a hermeneutical method for the history of science, in terms of the following methodological 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 . . . when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning.[27]

Kuhn's insight on how to read a text from a previous scientific era was also to form the backbone of the incommensurability thesis. He concluded years later from the experience: "it was untranslatability rather than translatability that I increasingly saw in studying the history of science."[28]

Society of Fellows

After this experience, Kuhn realized that he wanted to be a philosopher of science by doing history of science. His interest was not strictly history of science but philosophy, for he felt that philosophy was the way to truth and truth was what he was after.[29] To achieve that goal, Kuhn asked Conant to sponsor him as a Junior Fellow in the Harvard Society of Fellows. The society was recently initiated to provide young and promising scholars freedom from teaching for three years to develop a scholarly program. The fellows meet once formally and several times informally a week to dine and discuss ideas. Kuhn's colleagues stimulated him professionally, especially a senior fellow by the name of Willard Quine. At the time, Quine was publishing his critique on the distinction between the analytic and the synthetic, which Kuhn found reassuring for his own thinking.

Kuhn began as a fellow in the fall of 1948, but had to finish his doctoral dissertation in physics first. However, once it was completed the fellowship provided him with the opportunity to retool as an historian of science. Kuhn took advantage of the opportunity and read widely over the next year and a half in the humanities and sciences. Just prior to his appointment as a fellow, Kuhn was also undergoing psychoanalysis. Although the analyst acted rudely and irresponsibly, Kuhn was able "to climb into others' heads" for historical research.[30]

For Kuhn, climbing inside another's head or understanding the context in which scientific research is performed was stimulated by the historical work of Alexandre Koyré. In 1947, Bernard Cohen recommended Koyré to Kuhn. Kuhn read Koyré's *Etudes Galiléennes* and "loved them. I mean," as he testified later, "this was showing me a way to do things."[31] Koyré's impact on the development of the history of science is well attested by Kuhn later. "Within a decade of their [*Etudes*] appearance," wrote Kuhn, "they and his subsequent work provided the models which historians of science increasingly aimed to emulate. More than any other scholar, Koyré was responsible for . . . the historiographical revolution."[32]

For Kuhn, Koyré represented a different kind of historian from those like Sarton. According to Kuhn, Sarton "was a Whig historian and he certainly saw science as the greatest human achievement and the model for everything else."[33] Kuhn's interest in history of science was not to produce a chronology of scientific discoveries and the people responsible for them but to reconstruct the process and practices by which scientific knowledge develops. Besides Koyré's intellectual history, Kuhn was also significantly influenced A. O. Lovejoy.

Through his reading and discussions with other fellows, Kuhn came into contact with other influential thinkers. While reading Robert Merton, for example, Kuhn noted a reference to Jean Piaget. He delved into Piaget's theory of children learning. Commenting on Piaget's work later, Kuhn claimed that "these children develop ideas just the way scientists do, except . . . [scientists] are being taught, they are being socialized, this is not spontaneous learning, but learning what it is that is already in place."[34] In another example, while Karl Popper was at Harvard giving the William James lectures he suggested Emile Meyerson's *Identity and Reality* to Kuhn. Kuhn, however, found Meyerson's treatment of the philosophy of science not as helpful as his treatment of science's history.

Through a footnote in Hans Reichenbach's *Experience and Prediction*, Kuhn came across another influential thinker—Ludwik Fleck. Although Kuhn claimed he did not obtain much from reading Fleck, he "certainly got a lot of important reinforcement. There was someone who was, in a number of respects, thinking about things the way I was, thinking about the historical material as I was."[35] But Kuhn did not find Fleck's notion of thought collective useful but rather he found it repulsive, although Fleck's work, along with a remark by another fellow Francis Sutton, helped Kuhn to appreciate the role of the "sociology of the scientific community."[36]

Kuhn was also influenced by other thinkers, which he read or came into contact with, during his fellowship years. Other fellows introduced him to the writings of the Gestalt psychologists, including Kurt Koffka, Wolfgang Köhler, and Max Wertheimer, and to Benjamin Whorf's work on language and worldviews. In the summer between his second and third years of the fellowship, he traveled to Europe. In England, he met Mary Hesse and Alistair Crombie, among others. In France, he met Gaston Bachelard, whose work he was somewhat familiar with. Unfortunately, the meeting was held in French to Kuhn's disappointment. He also came into contact with Hélène Metzger's work on the history of chemistry. Furthermore, he read Anneliese Maier's work in the history of science. These thinkers showed Kuhn "what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today."[37]

During the last year of Kuhn's fellowship, Conant stopped teaching the science general education course. Kuhn, along with a colleague Leonard Nash, a well-known and respected chemistry teacher at Harvard, took over the course. The student numbers plummeted and Kuhn found himself over preparing and anxious about his teaching, an anxiety that would plague him for the rest of his career and spill over into his writing. Students initially found Kuhn's lectures less than inspiring compared to

Nash's but over the next few years students came to appreciate the detail Kuhn put into lectures.

In 1950, the trustee of the Lowell Institute, Ralph Lowell, invited Kuhn to deliver the 1950–1951 Lowell lectures.[38] The institute was founded in 1836 by John Lowell, Jr. Although previous lecturers were well-known personages, such as Alfred North Whitehead, by the time Kuhn gave the lectures they were usually drawn from the Harvard Fellows. Kuhn agreed to deliver a series of eight lectures during March 1951, at the Boston Public Library in Copely Square. An advertisement, which appeared a month before the lectures in the Boston *Globe*, announced them accordingly: "What are the problems of scientific research today?????"[39] On the day the advertisement appeared, Kuhn wrote a letter to Lowell expressing dissatisfaction with it. Kuhn claimed that it "bears no relation to the announced title of either the series or the individual lectures."[40]

The misleading newspaper advertisement, which was the result of an overly ambitious copy writer, was eventually corrected, but a flyer advertising the lectures was also distributed at the same time and added: "In a world in which science's quest for physical theory has already had results that promise to change the course of history, the fate of mankind may depend upon solving the problems of research."[41] Upon learning about the flyer, Kuhn wrote another letter to Lowell complaining about its sensationalism: "it may help you to understand my dismay if I explain that the fascinating topic your copy writer has so clearly stated is one to which I believe no serious and reputable student of science would address himself."[42] In the lectures, however, Kuhn was well on his way to developing a theory of science distinct from the traditional view, but he realized that more historical research and philosophical reflection were needed before he could publish it.

Faculty

In the same year of the Lowell lectures, Kuhn was appointed an instructor and the following year assistant professor. In a 1951 letter to David Owen, chair of the General Education Committee, Kuhn outlined his research project and teaching interests. He first noted that he is soon to deliver the Lowell lectures on issues in scientific methodology, which he plans to publish as a book. "With this manifesto behind me," wrote Kuhn, "I should hope to turn my attention to certain of the more detailed research problems which proceed from this orientation toward methodological problems."[43] Specifically, he planned to write a book on revising the history of motion up to the time of Newton. According to Kuhn, his reading of that history "does not indicate that the ancients were bad scientists . . . but that the problem of motion was itself differently conceived in antiquity."[44] Besides his "semi-historical" research, Kuhn was also interested in a number of philosophical problems with the nature of science. "Those which most concern me at the moment," confided Kuhn, "arise from the necessity of retrieving, within the broader conception of science as an activity in which facts and theories continually interact."[45]

Kuhn's primary teaching duty was in the general education curriculum, where he continued to teach Natural Sciences 4 along with Nash. He eventually taught courses in the history of science. It was during this time that Kuhn developed one of his favorite undergraduate courses in the history of science, "The rise of scientific cosmology: Aristotle to Newton," which he taught for many years. In the course, Kuhn "started out by getting people to read Aristotelian texts and talk about what motion was like and what the so-called laws of motion were and why that was not the thing to call them."[46] Kuhn also utilized the course material for scholarly writing projects. For example, chapters of the Copernican revolution book were handed out in his classes. However, Kuhn found class preparation time-consuming and often detracted from his writing.

A part of Kuhn's motivation for developing a new image of science was the misconceptions of science held by the laity. He blamed this misunderstanding on introductory courses that stress the textbook image of the nature of science, as a static body of knowledge. After discussing this state of affairs with friends and with Conant, Kuhn attempted to provide students with a more accurate image of science. The key to that image, claimed Kuhn, is science's history, which displays the creative and dynamic nature of science.

Around 1952 Charles Morris invited Kuhn to write a monograph in the history of science for the *International Encyclopedia of Unified Science*. Kuhn was assigned to write a monograph at the suggestion of Bernard Cohen, after another was unable to do it.[47] The title of the monograph was *The Structure of Scientific Revolutions*. In 1953, Kuhn applied for a Guggenheim fellowship to supplement a half-year leave of absence granted to untenured faculty within the first five years. In the application, he proposed to finish a book on the Copernican revolution and to write a monograph on scientific revolutions for the *Encyclopedia*. He started the fellowship in 1954, but did not finish the Copernican book until several years later and the scientific revolutions monograph until almost a decade later.

In 1956, Kuhn was denied tenure at Harvard because the tenure committee felt his book on the Copernican revolution was too popular in its approach and analysis. The book—as Kuhn admitted in his Guggenheim fellowship application—represented a synthesis of current scholarship, although he claimed that the narrative was to some extent unique. Interestingly, Kuhn also acknowledged in the fellowship application that *Structure* too was a work of synthesis like the Copernican book. But he believed that the synthesis in *Structure* was more significant, since it drew on disciplines not normally considered by philosophers of science. He firmly believed that the image of science in *Structure* was a more accurate depiction of the process of science.

Mid-career

The Berkeley years

A friend of Kuhn, who was also a tutor at Harvard's Kirkland House, knew Steven Pepper, who was chair of the philosophy department at the University of California at Berkeley. Kuhn's friend told Pepper that Kuhn was looking for an academic position. Pepper's department was searching for someone to establish a program in the history and philosophy of science. Kuhn was eventually offered a position in the philosophy department and later asked if he also wanted an appointment in the history department. Kuhn accepted both positions and joined the faculty at Berkeley as an assistant professor in 1956. A. Hunter Dupree was also hired at the same time, to assist in establishing the program. Since courses could not be cross-listed, Kuhn taught courses in both departments. He taught a year-long survey course in the history of science, which he organized not chronologically but according to scientific practices. Kuhn also taught a course in philosophy, focusing on Aristotle to Newton, and a seminar on various topics depending on the strength and interests of the students.

Kuhn found in the philosophy department Stanley Cavell, a soul mate to replace Nash. Kuhn had meet Cavell earlier while they were both fellows at Harvard. Cavell was an ethicist and aestheticist, whom Kuhn found intellectually stimulating and with whom he could discuss issues in half-sentences. Cavell introduced Kuhn to Wittgenstein's notion of language games. Kuhn also developed a professional relationship with Paul Feyerabend. They often met to discuss ideas "in the now defunct *Café Old Europe* on Telegraph Avenue [Berkeley, CA] and greatly amused the other customers by their friendly vehemence."[48] Kuhn's book on the Copernican revolution appeared in 1957. In it, Kuhn expounded a narrative in which both astronomical and non-astronomical factors shaped the revolution.

In October 1956, while first arriving on the Berkeley campus, Kuhn presented a paper, "Role of measurement in development of science," at the university's Social Science Colloquium. The paper was revised during the spring of 1958 and delivered the following year at a conference on the "History of quantification in the sciences." The paper was eventually published in 1961, as "The function of measurement in modern physical science." Kuhn introduced the notion of normal scientific practice, albeit in terms of measurement. "The second section, Motives for Normal Measurement," according to Kuhn, "was a product of those revisions, and its second paragraph contains the first description of what I had, in its title, come very close to calling 'normal science'."[49] Although he was aware for several years that there are periods of scientific practice governed by tradition punctuating revolutions, their importance, in terms of a notion of normal science, eluted him. But once Kuhn had the insight into normal science by the end of the summer in 1959, the transition from "The function of measurement" to *Structure*'s chapter 4, "Normal science as puzzle solving," was straightforward.

In 1958, Kuhn was promoted to associate professor and granted tenure. Moreover, having completed his historical research, Kuhn was now ready to return to the philosophical issues that first attracted him to the history of science. He spent a year, beginning in the fall of 1958, as a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford, California. His intention was to write a draft of *Structure*, but he ran into a problem concerning the intervals between revolutions. The year Kuhn spent at the center, filled with social scientists, was critical for resolving the problem. What struck Kuhn about the relationships among these scientists was their inability to agree on the fundamental problems and practices of their discipline. Although natural scientists do not necessarily have the right answers to their questions, there is an agreement over fundamentals. This difference between natural and social scientists led Kuhn to the notion of paradigm. Although Michael Polanyi visited the center while Kuhn was in residence and gave a lecture on tacit knowledge, Kuhn claimed that his approach was not focused on propositional knowledge as Polanyi's approach was. Within a year and a half, beginning in the summer of 1959, Kuhn completed a draft of *Structure*.

The initial fruit of Kuhn's labor, however, was a paper entitled "The essential tension: tradition and innovation in scientific research," presented at the Third University of Utah Research Conference on the Identification of Creative Scientific Talent, held at the Peruvian Lodge in Alta, Utah, from 11 to 14 June 1959. The conference was part of a larger movement at the time to identify predictors of creativity, in order to expedite scientific discovery and advancement. It was the brainchild of Calvin Taylor and was sponsored by the National Science Foundation.

A steering committee invited participants from a variety of disciplines and occupations, who had contributed to the criteria, predicators, external conditions, and development of scientific creativity. Papers were presented in an informal manner, and committees were formed to explore different aspects of scientific creativity. Kuhn participated on a committee exploring its environmental conditions.

As Kuhn noted later, the importance of "The essential tension" paper was the introduction of the paradigm concept. "That concept," admitted Kuhn, "had come to me a few months before the paper was read, and by the time I employed it again in 1961 and 1962 its contents had expanded to global proportions, disguising my original intent."[50] Indeed, Kuhn's use of paradigm in the paper is more constrained than in *Structure* and reflects the traditional function in language pedagogy. Just as students learn a language by declining nouns and conjugating verbs, so students learn science by solving standard problems. Kuhn later claimed he used the term properly in this paper, as compared to *Structure*, for paradigm is limited to scientific consensus especially in terms of scientific models.[51] Kuhn now had both poles for his scientific epistemology: one of episodic change (innovation), the other of stasis (tradition). For him, this generates a tension in which scientists practice.

In July 1961, after completing a draft of *Structure*, Kuhn participated in a symposium, "The structure of scientific change." The symposium was held at the University of Oxford, UK, under the auspices of the Royal Society's International Union of the History and Philosophy of Science, and directed by Crombie. In introductory remarks, Crombie discussed the importance of and the problems associated with scientific change, especially in terms of internal and external factors, for both historians and philosophers of science. Kuhn delivered a paper, "The function of dogma in scientific research," which represented a revision of "The essential tension" paper and which contained material from the first third of the *Structure* draft. In the paper, Kuhn expanded the notion of paradigm comparable almost to that present in *Structure*. Rupert Hall and Polanyi commented on Kuhn's paper, followed by a general discussion of the paper involving Bentley Glass, Stephen Toulmin, and Edward Caldin. Kuhn provided closing remarks.

In 1962 *Structure* was published as the final monograph in the second volume of Neurath's *International Encyclopedia of Unified Science*. Charles Morris was instrumental in its publication and Carnap served as its editor. The book was well received initially, as evident from contemporary reviews, although many criticized the ambiguous formulation of paradigm. While the book reviews exposed Kuhn to a wider audience than the history and philosophy communities and he had people writing to him on a regular basis, participation in a 1965 international philosophy colloquium in London thrust him onto center stage in the historiographic revolution. Finally, it is fitting, as John Heilbron notes, that *Structure* was birthed at Berkeley, which was the one of the important centers of the academic revolution—as part of the Cambridge–Berkeley axis—during the 1960s.

In the early 1960s, Kuhn was invited by van Vleck to direct a project in collecting materials on the history of quantum mechanics. The impetus for the project was the "immortality" of its "heroes:" "With ever increasing frequency the physicist in his middle years has asked his colleagues, what can we do to capture the great dialogs and the great moments before they fade away?"[52] In August 1960, Dupree, Charles Kittel, Kuhn, John Wheeler, and Harry Wolff, met in Berkeley to discuss the project's organization. Wheeler next met with Richard Shryock and a joint committee of the American Physical Society and the American Philosophical Society on the History of Theoretical Physics in the Twentieth Century was formed to sponsor and develop the project. The project lasted three years, from 1 July 1961 to 31 June 1964, with the first and last years of the project conducted in Berkeley and the middle year in Europe. The National Science Foundation funded the project.

Kuhn's colleagues on the project were Heilbron, Paul Forman, and Lini Allen. Their duties were to interview physicists, who participated in the transition from classical physics to quantum physics during the early twentieth century, including Niels Bohr, and to collect and deposit their published articles, unpublished manuscripts, letters, notebooks, and autobiographical remembrances, at different locations. The project's staff also conducted around 175 interviews with physicists, from February 1962 to May 1964. Kuhn found the interviewing process frustrating because the interviewees either could not remember or thought the question was not pertinent to the quantum story. Kuhn admitted later that he had reservations about the type of information the project would generate about the discoveries.

> I knew as a historian that scientists' recollections of their own work is quite bad historically; that they see themselves as having worked towards the thing they eventually discovered, although when you look back you find they were looking for something entirely different.[53]

In the same year that *Structure* was published, Kuhn moved his family to Copenhagen, Denmark, where he directed the archival project. The collected material was deposited at the library of the American Philosophical

Society in Philadelphia and at the library of the University of California in Berkeley, with a less complete collection at the Bohr archive in Copenhagen. A catalog of the archival material was published in 1967, by the American Philosophical Society. Besides the catalog, Kuhn published two historical studies related to the project. The first was an article on the origins of the Bohr atom, which was coauthored with Heilbron. The second was a book on Planck's black-body theory and the origins of quantum discontinuity.

In 1960, The Johns Hopkins University offered Kuhn a position as full professor at a substantially higher salary. Although he found the offer attractive, he decided to remain at Berkeley since he was only there for a few years and found his colleagues stimulating. However, he used the offer to negotiate for expansion of the program. Berkeley's administration agreed to hire another faculty member. In 1961 Kuhn was made full professor, but only in the history department. Members of philosophy department voted to deny him promotion in their department, a denial that angered and hurt Kuhn tremendously. Years later in an interview, Kuhn confessed that the hurt "has never altogether gone away."[54] Eventually he took a position elsewhere.

The Princeton years

Princeton University extended to Kuhn an offer to join its faculty, while he was in Copenhagen. The university had recently inaugurated a history and philosophy of science program. The program's chair was Charles Gillispie and its staff included John Murdoch, Hilary Putnam, and Carl Hempel. Upon returning to the United States in 1963, Kuhn and his wife visited Princeton. They decided to accept the offer and Kuhn joined its faculty in 1964. He became the program's director in 1967 and the following year was appointed the Moses Taylor Pyne Professor of History. From 1972 to 1979 he was also a member of the Institute for Advanced Study. Kuhn felt that Princeton would provide him with more resources professionally. He developed a close relationship with Hempel and they taught jointly a philosophy of science course. "Tom's ideas have influenced my thinking in various ways," testified Hempel later, "and have certainly contributed to my shift from an antinaturalistic stance to a naturalistic one."[55]

In late 1964, Imre Lakatos invited Kuhn to participate in an International Colloquium in the Philosophy of Science, organized jointly by the British Society for the Philosophy of Science and the London School of Economics and Political Science. The organizing committee included W. C. Kneale as chair and Lakatos and Popper, among others. Originally there were four session to the colloquium, with Kuhn invited to participate in a session on "Criticism and the growth of mathematical and scientific knowledge." Lakatos wrote to Kuhn: "this section will revolve largely around your work, which provoked so much interest all over the world. This would be, as far as I know, the first occasion when your theses could be discussed by philosophers of science."[56] The initial plan was for Lakatos to deliver a critical paper on Popper's philosophy vis-à-vis Kuhn's philosophy of science. Kuhn was then to follow with critical remarks on Lakatos' paper. Kuhn accepted with the condition that Lakatos supply him with a draft of his paper by March of the following year.[57]

Lakatos did not provide the draft by March but instead wrote Kuhn in mid-June that he was pulling out of the conference and planned not to present another paper again until he completed an overdue book manuscript. Feyerabend, who was also by then to participate in the colloquium, refused to attend and proposed to send a paper to be read by proxy. However, the committee rejected Feyerabend's proposal and invited John Watkins in his stead. Moreover, the committee decided to invite Popper to present a paper so he was not to chair the session, rather Rupert Hall was to replace him. "I am sorry for all this news," wrote Lakatos, "some of which may be a disappointment for you, but I hope that after my book you will forgive me."[58] Kuhn was more than disappointed! He found the changes "shocking" and in a return letter resigned from the conference. In a postscript to Lakatos, Kuhn acknowledged that some of the changes were beyond Lakatos' control. "What does, however, upset and offend me deeply," wrote Kuhn, "is your behavior."[59] Kuhn felt that Lakatos should have had the courtesy to consult him about the changes.

After the problems were corrected, the colloquium went on as scheduled for 13 July 1965 at Bedford College in London. Kuhn presented a paper that was to appear in Paul Schlipp's Popper volume. Watkins then delivered a paper criticizing Kuhn, with Popper chairing the session. Popper also presented a paper criticizing Kuhn, as did several other members of the philosophy of science community, including Toulmin, L. Pearce Williams, and Margaret Masterman. Although Kuhn was vigorously critiqued during the colloquium, after it he began to acquire an international reputation. The papers from the colloquium, including additional papers by Lakatos and Feyerabend and with a response by Kuhn to critics, were published in 1970.

Kuhn's paradigm concept continued to be criticized, especially by historians and philosophers of science. Interestingly, especially to Kuhn, those outside the discipline of the history and philosophy of science were more receptive to *Structure*. As the 1960s came to a close, Kuhn's book was becoming increasingly popular, especially with student radicals who believed Kuhn liberated them from the tyranny of tradition. But Kuhn took to heart his critics and began to revise the notion of paradigm. In a lecture, "Paradigms and theories in scientific research," delivered at Swarthmore College in February 1967, Kuhn attempted to clarify paradigm by introducing the notion of "professional matrix."[60]

In March 1969, Kuhn attended a symposium, organized by Frederick Suppe and Alan Donagan, on the structure of scientific theories held at Urbana, Illinois. The aim of the symposium, as stated in its call, was to assemble "a number of the main proponents and critics of the traditional analysis, proponents of some of the more important alternative analyses, historians of science, and scientists to explore the question 'What is the structure of a scientific theory?'"[61] To that end, the organizers invited the luminaries in science and in the history and philosophy of science. Kuhn presented a paper, entitled "Second thoughts on paradigms," in which he clarified paradigm in terms of disciplinary matrix and exemplar. Suppe provided commentary, followed by a general discussion among Kuhn, Dudley Shapere, Sylvain Bromberger, Patrick Suppes, Putnam, and Peter Achinstein.

During this time, Kuhn also redressed the notion of paradigm in a second edition of *Structure*, which appeared in 1970. In its Japanese translation Kuhn added a postscript in which he addressed various criticisms, especially those of paradigm. The postscript was then added to the revised edition of *Structure*. An expanded version of *Structure* that was promised never appeared, although Kuhn made some effort in that direction.[62] "I came to realize," he admitted later, "that I didn't have anything more to say in the same general vein."[63]

In the late 1960s and the early 1970s, Kuhn addressed methodological issues in the history and philosophy of science. In the Isenberg lecture, "The relations between the history and the philosophy of science," presented at Michigan State University in March 1968, Kuhn contended that the history of science and the philosophy of science should remain separate enterprises. The journal of the American Academy of Arts and Sciences, *Dædalus*, sponsored a special issue in 1971 called "The historian and the world of the twentieth century." The issue contained papers delivered at meetings held in Princeton and Rome, funded by the Ford Foundation. In prefatory remarks, the editor commented on the transformation of historical scholarship since the 1920s and 1930s.

> The conceptual and methodological changes that have taken place within certain fields are given great importance; so, also, are the changes

> that have led historians to seek entirely new kinds of facts to answer wholly new kinds of questions. Many of the most important changes have come about through developments internal to the specific historical disciplines themselves; others reflect changes—intellectual, social, and political—taking place in society.[64]

Kuhn contributed, along with other prominent historians such as Arthur Schlesinger, to the special issue.

Stephen Rousseas, director of a Science, Technology and Society program at Vassar College, invited Kuhn to participate in a symposium focusing on *Structure*.[65] Rousseas wanted Kuhn to address the application of paradigm to disciplines like the social sciences. Kuhn's book was used in the program, with students and sociologists being supportive of it while philosophers and scientists, of a positivist bent, were not. In November 1974, Kuhn delivered a lecture, "Puzzles vs. problems in scientific development." In it, Kuhn acknowledged that the enthusiasts *Structure* engendered among sociologists are "part of the audience that seemed most easily able to find in it anything they pleased."[66]

Kuhn blamed this plasticity on himself because of a "bad mistake . . . I sometimes think it the only truly stupid one" made in *Structure*.[67] "I speak of the transition to maturity as the transition from the pre-paradigm to the post paradigm period, all of which now seems to me wrong."[68] In *Structure*, he claimed that during the pre-paradigmatic period, each school had a particular paradigm. "But if that's the case," reasoned Kuhn, "then the notion of paradigm, whatever its other virtues, is irrelevant to the transition from an underdeveloped to a developed or mature state."[69] Because of this mistake, he accepted partial responsibility for the inappropriate application of the paradigm notion by members of disciplines outside the physical sciences, especially sociologists, who used it to claim scientific status for their discipline. Kuhn was truly contrite for his mistake and endeavored, with moral fervor, to correct it.

The visit to Vassar was less than a success, at least from Kuhn's perspective. In a letter to the director, Kuhn wrote that "the trip to Vassar was for me a nightmare, unlike and far more severe than any I have encountered in a large number of similar trips to college campuses during the past ten or more years."[70] The nightmare was Kuhn's perception that people at Vassar thought he had no responsibility or obligation to correct the misuse and misunderstanding by others of his ideas in *Structure*. He characterized his host's attitude towards him as, "You have done your job; leave the rest to us; and don't rock the boat."[71] Kuhn considered this a moral issue and resented the attitude at Vassar. He thought it smacked of "anti-intellectualism."

The director responded to Kuhn, noting that the college had a long list of distinguished speakers who were gracious even though the discussions were often frank and intense. "Your letter," claimed Rousseas, "is an extraordinary document which reveals more about yourself than you may have intended."[72]

In November 1976, Kuhn delivered the Agnes A. and Constantine E. A. Foerster lecture, "Does knowledge 'grow'?", at the University of California, Berkeley. He shared the podium with Stefan Amsterdamski from the Institute of Science at the Polish Academy of Sciences in Warsaw, who also delivered a lecture earlier in the day, "Reflections on science and human rationality." Amsterdamski recently wrote a book, *Between Experience and Metaphysics: Philosophical Problems of the Evolution of Science*, which Kuhn read in preparation for the meeting. "You will know already," wrote Kuhn to Amsterdamski, "that I am in wholehearted agreement with most of your book."[73] But Kuhn was concerned over Amsterdamski's criticism that Kuhn did not distinguish adequately between revolutions in a specific scientific discipline and those in science as a whole; for Amsterdamski claimed that global revolutions rarely occur in science. In response, Kuhn acknowledged that he did not disagree with Amsterdamski but felt he missed an important point. "I doubt," wrote Kuhn, "that there are such things as global revolutions in the sciences excepting on occasions when the sciences are caught up in a transformation of thought that extends widely outside of the realm of the sciences altogether."[74]

During the 1970s, Kuhn also pursued a notion related to the incommensurability thesis, theory change. He discussed it in a Franklin J. Machette lecture, presented as a session of the Furman University Colloquium in Philosophy of Science, "The limits of reason in the science and the humanities," held in November 1973. Two other sessions also constituted the colloquium, in which Joseph Agassi and John Compton delivered papers. In the Machette lecture, "Objectivity, value judgment, and theory choice," Kuhn asserted that theory choice involves judgement based not only on objective criteria but also on subjective values. John F. Post from Vanderbilt served as commentator.

After the paper's publication, Hempel criticized Kuhn's approach to theory choice. The two philosophers began a discussion of the issue earlier when they were at Princeton and continued it at the tenth Chapel Hill Colloquium in Philosophy, in October 1976. At the colloquium Hempel delivered a paper "Scientific rationality and rational reconstruction" in a session titled the same. Kuhn followed with comments on Hempel's paper. The two philosophers had another go at theory choice at the eightieth annual meeting of the eastern division of the American Philosophical

Association, held in December 1983 at the Sheraton hotel in Boston, Massachusetts. The occasion was a symposium on Hempel's rationality of science. Kuhn and Wesley Salmon presented papers, to which Hempel responded.

In the early 1970s, Kuhn began a detailed historical analysis on Planck's development of the novel theory of black-body radiation, which was published in 1978 as *Black-Body Theory and the Quantum Discontinuity, 1894–1912*. The book was not especially well received either by historians and philosophers or by physicists. Historians and philosophers were disappointed because Kuhn did not explicitly frame the narrative in the terms of *Structure*. Interestingly, Kuhn acknowledged that he could not do history and philosophy simultaneously. He often focused on the historical narrative and only addressed the philosophical relevance later.[75] Physicists were critical of Kuhn's reconstruction of the science. Moreover, Kuhn ignored the social influences on the development of Planck's ideas, which annoyed the sociologists of science. However, he considered the Planck book "the best study in conceptual change I've done."[76] Kuhn responded to his critics in a journal article, "Revisiting Planck," which was later published as an "Afterword" in a revised edition of the book. The purpose of his response was to summarize the major technical points of the book and to address the relationship of *Black-Body Theory* to *Structure*.

In 1978, Kuhn was a fellow at the New York Institute for the Humanities. In September of that year, his marriage to Kathryn ended in divorce and, while she remained in Princeton, Kuhn decided to leave and soon moved to Cambridge, Massachusetts. "It," as Kuhn recalled later, "wasn't anything about Massachusetts Institute of Technology versus Princeton as such."[77] Moreover, Kuhn was to turn his attention away from history of science to philosophy of science. At MIT, Kuhn took a "linguistic turn" in his thinking, reflecting his new environment, which had a major impact on his subsequent work, especially on the incommensurability thesis.

Late career

In 1979, Kuhn was appointed a professor in MIT's Department of Linguistics and Philosophy, which is housed in wooden military barracks built during World War II. In 1983, he was appointed the Laurance S. Rockefeller Professor of Philosophy, the first to hold that position. And in 1982, Kuhn married Jehane Burns, whom he met at a dinner party in 1979.

During the 1980s and early 1990s, Kuhn was still engaged with issues associated with *Structure*, including scientific development, theory choice, and especially incommensurability. He wrote a number of papers on these issues, using a linguistic and taxonomic framework for incommensurability that reflected his new surroundings. In November 1980, he delivered a series of three lectures at the University of Notre Dame's Perspective lectures. The title of the lectures, "The nature of conceptual change," represented his return to the philosophical issues that emerged from *Structure*. In the first lecture, he revisited the nature of scientific revolutions. The lecture was revised and presented at the third annual conference of the Cognitive Science Society in 1981 and later published as "What are scientific revolutions?" In the next two lectures, he turned to a linguistic formulation for the incommensurability thesis by examining the linguistic elements of revolutionary change, on the one hand, and causal theory and necessary truth, on the other. Kuhn continued to develop the ideas of the last two Notre Dame lectures in a paper, "Commensurability, comparability, and communicability," which he delivered at the biennial meeting of the Philosophy of Science Association, held at Philadelphia in October 1982. Philip Kitcher and Mary Hesse provided commentary, to which Kuhn responded.

Achinstein invited Kuhn to deliver the 1983 Alvin and Fanny Blaustein Thalheimer lectures at The Johns Hopkins University's Department of Philosophy.[78] Full honorarium carried a stipulation that the lectures be published, with Thalheimer lectures noted in a subtitle. Kuhn was unable to accommodate the spring but agreed to give a series of four lectures in November 1984. In the series, "Scientific development and lexical change," Kuhn extended the ideas delivered in the Notre Dame lectures.[79] He proposed to cover material that was to appear in a forthcoming book, which he planned to publish with the University of Chicago Press. However, he was unable to publish it and never received the balance of the honorarium. The main argument of the lectures was that theories are embedded "inextricably" in a language or lexicon and that scientific knowledge is intimately tied to the structure of language as represented in a lexicon. Given this intimate relationship, change in a lexicon often results in an inability of the previous language user to translate an older theory into the current language. "Here and there," wrote Kuhn, "the two languages are incommensurable, and in the areas where they are, no full translations from one to the other are possible."[80]

Kuhn continued to pursue the linguistic turn toward an articulation of the incommensurability thesis in subsequent lectures and articles. At the sixty-fifth Nobel Symposium in August 1986, Kuhn presented a paper, "Possible worlds in history of science." Arthur Miller and Tore Frängsmyr provided commentaries, to which Kuhn responded. In the paper, Kuhn further developed the notion of a lexicon. He refined this notion in "Dubbing and redubbing: the vulnerability of rigid designation," which he delivered at the twentieth Chapel Hill Colloquium in Philosophy, held in October 1986. Arthur Fine provided commentary.

In the mid- to late 1980s, the Minnesota Center for Philosophy of Science sponsored an institute to address the question whether a new consensus, based on the work of Kuhn, Quine, Hanson, and others, was emerging in the philosophy of science to replace the older consensus of logical positivism. Kuhn participated in the institute with a revised version of the 1986 Chapel Hill paper. Kuhn's paper, along with others, was published as volume XIV in the Minnesota Studies in the Philosophy of Science. He addressed the incommensurability thesis again in the 1987 Shearman Memorial lectures, at University College, London. In the series of three lectures, "The presence of past science," he explored the regaining, portraying, and embodying of past science.[81]

In 1989, Kuhn submitted a grant proposal to the History and Philosophy of Science Program of the National Science Foundation. The title of the project was "Philosophy of scientific development." Kuhn proposed to complete a book, *Words and Worlds: An Evolutionary View of Scientific Development*, which he had been working on for the past decade. Much of the material was taken from the Perspective lectures, Thalheimer lectures, and Shearman lectures. Nine reviewers evaluated the grant application, with eight of the reviewers scoring it "excellent." For example, one reviewer claimed: "A new work by Kuhn, advancing and improving upon the arguments in his *Structure of Scientific Revolutions*, will be a significant and influential book."[82] Another reviewer had this to say: "Kuhn's *Structure* is a modern classic. Despite protests and criticisms and reservations, Kuhn's work has altered the conception of science not just for philosophers and historians and scientists, but for a whole generation of educated men and women."[83] The consensus of the reviewers was that Kuhn's grant should be funded and it was.[84]

From 1989 to 1990, Kuhn was president of the Philosophy of Science Association. In October 1990, he delivered the presidential address, "The road since Structure," at its biennial meeting, held at Minneapolis, Minnesota. In it, he discussed the various issues he was working on, especially incommensurability from an evolutionary perspective. He also noted that his remarks reflected the major themes of a book, *The Plurality of Worlds: An Evolutionary Theory of Scientific Discovery*, he was working on. Although large sections of it appeared in draft form, the book was not finished at the time of his death.

Besides philosophical issues, Kuhn also addressed issues concerning the practice and nature of history and its relationship to the philosophy of science. In 1980, Kuhn published a review, "The Halt and the blind:

philosophy and history of science," in which he again claimed that there is no discipline of history and philosophy of science. For philosophers are interested in truth and historians in what happened. But Kuhn acknowledged that the two disciplines may cross-pollinate each other. Kuhn also surveyed the history of science discipline in a 1985 keynote address, which he delivered at the seventeenth International Congress held at Berkeley. In it he discussed the rapid growth of the discipline and its shifts from ancient to modern histories and from intellectual to social histories. For Kuhn, the discipline he believed he had helped to spawn is an historical philosophy of science, which was the topic of a paper delivered in November 1991, as the Robert and Maurine Rothschild Distinguished lecture, in the Department of the History of Science at Harvard University. Kuhn retired from teaching in 1991 and became an emeritus professor.

During Kuhn's career he received numerous awards and accolades. He was the recipient of honorary degrees from around a dozen academic institutions, such as University of Chicago, Columbia University, University of Padua, and University of Notre Dame. He was elected a member of the National Academy of Science—the most prestigious society for U.S. scientists—and was an honorary life member of the New York Academy of Science and a corresponding fellow of the British Academy. He was president of the History of Science Society from 1968 to 1970 and was awarded its highest honor, the Sarton Medal, in 1982. Kuhn was also the recipient in 1977 of the Howard T. Behrman Award for distinguished achievement in the humanities and in 1983 of the celebrated John Desmond Bernal award.

In May 1990, a conference—or as Hempel called it, a "Kuhnfest"—was held in Kuhn's honor at the Massachusetts Institute of Technology, sponsored by the Sloan Foundation and organized by Paul Horwich and Judith Thomson. The conference speakers included Jed Buchwald, Nancy Cartwright, John Earman, Michael Friedman, Ian Hacking, Heilbron, Ernan McMullin, N. M. Swerdlow, and Norton Wise. The papers reflected Kuhn's impact on the history and philosophy of science. A special appearance was made by Hempel on the last day, followed by Kuhn's remarks on the conference papers. As he approached the podium after Hempel's remarks, before a standing-room-only audience, Kuhn was visibly moved by the outpouring of professional appreciation for his contributions to a discipline which he cherished and from its members whom he truly respected.

Kuhn died on 17 June 1996 in Cambridge, Massachusetts, after suffering for two years from cancer of the throat and bronchial tubes. He was an inveterate cigarette smoker. On one occasion that dependency did not serve him well professionally, when meeting Popper in Kuhn's Berkeley home in 1962. Heilbron narrated the event: "The guest was allergic to smoke and the host was addicted to cigarettes. Communication across worldviews, never an easy matter, failed altogether owing to coughing fits on the one side and tobacco fits on the other."[85]

Notes

1. Kuhn (2000), p. 266. Besides the published literature cited herein, an invaluable resource for Kuhn's early life is Karl Hufbauer's unpublished paper, "From student of physics to historian of science: T. S. Kuhn's education and early career (1940–1958)," presented at the conference on "The legacy of Thomas S. Kuhn," at the Dibner Institute for the History of Science and Technology in November 1997.
2. Kuhn (2000), p. 259.
3. *Ibid*, p. 260.
4. *Ibid*, p. 257.
5. *Ibid*, p. 258.
6. MIT MC240, box 1, folder 2, "Some things about E—," p. 2.
7. MIT MC240, box 1, folder 2, "Character Portrayal in *The Case of Sergeant Grisha*," p. 1.
8. Kuhn (2000), p. 261.
9. *Ibid*, p. 264.
10. *Ibid*, p. 275.
11. MIT MC240, box 1, folder 3, "An analysis of causal complexity," p. 1.
12. *Ibid*, p. 21.
13. MIT MC 240, box 1, folder 3, "The metaphysical possibilities of physics," p. 10.
14. *Ibid*.
15. *Ibid*, p. 11. Davis' remarks on the paper are: "This is an exciting idea and intelligently presented . . . The chief problem is in the nature of the conceptions whose possible number you want to calculate." *Ibid*, back of p. 11.
16. Kuhn (2000), p. 261.
17. MIT MC240, box 1, folder 3, "Phi Beta Kappa address," p. 2.
18. Kuhn (2000), p. 272.
19. *Ibid*, p. 273.
20. Kuhn (1977a), p. v.
21. Kuhn (1987a), p. v.
22. Kuhn (1945), 30.
23. Kuhn (1984a), 30.
24. Kuhn (2000), p. 274.
25. Sigurdsson (1990), 20.
26. Horgan (1991), 40.
27. Kuhn (1977a), p. xii.
28. Sigurdsson (1990), 20.

29. Kuhn (2000), pp. 277–8.
30. *Ibid*, p. 280. For discussion of Kuhn's method, see Andersen, J. (1999), S59–S61.
31. *Ibid*, p. 285.
32. Kuhn (1970a), 67.
33. Kuhn (2000), p. 282.
34. *Ibid*, p. 279.
35. *Ibid*, p. 283.
36. Kuhn (1964), p. ix.
37. *Ibid*, p. viii.
38. MIT MC240, box 3, folder 10, 8 March 1950 letter, Lowell to Kuhn.
39. *Ibid*, 17 February 1951, *Globe* clipping.
40. *Ibid*, 17 February 1951, letter, Kuhn to Lowell.
41. *Ibid*, flyer.
42. *Ibid*, 20 February 1951 letter, Kuhn to Lowell.
43. MIT MC240, box 3, folder 10, 6 January 1951 letter, Kuhn to David Owen, p. 4.
44. *Ibid*, p. 5.
45. *Ibid*.
46. Kuhn (2000), p. 288.
47. *Ibid*, p. 292.
48. Feyerabend (1970), p. 198. Kuhn gave Feyerabend a draft of *Structure* but Feyerabend "was terribly upset by this whole business of dogma, rigidity, which of course is exactly counter to what he believed himself." Kuhn (2000), p. 310. For Feyerabend's comments on the draft, see Hoyningen-Huene (1995).
49. Kuhn (1977a), p. xvii.
50. *Ibid*, p. xviii.
51. Kuhn (2000), p. 299.
52. Kuhn *et al* (1967), p. vi.
53. Sigurdsson (1990), p. 23.
54. Kuhn (2000), p. 302.
55. Hempel (1993), p. 8.
56. MIT MC 240, box 23, folder 9, 28 October 1964 letter, Lakatos to Kuhn, p. 1.
57. *Ibid*, 9 November 1964 letter, Kuhn to Lakatos.
58. *Ibid*, 18 June 1965 letter, Lakatos to Kuhn, p. 3.
59. *Ibid*, 23 June 1965 letter, Kuhn to Lakatos, p. 2.
60. MIT MC240, box 3, folder 14, "Paradigms and theories in scientific research."
61. Suppe (1977b), p. vii.
62. MIT MC240, box 4, folder 17, "Towards a second edition."
63. Wade (1977), 145.
64. Graubard (1971), v.
65. MIT MC240, box 5, folder 9, 31 May 1973 letter, Rousseas to Kuhn.
66. MIT MC240, box 5, folder 9, "Puzzles vs. problems in scientific development," pp. 2–3a.

67. *Ibid*, p. 3a.
68. *Ibid*, p. 6.
69. *Ibid*, p. 7.
70. *Ibid*, 9 December 1974 letter, Kuhn to Rousseas, p. 1.
71. *Ibid*, p. 2.
72. *Ibid*, 20 December 1974 letter, Rousseas to Kuhn, p. 2.
73. MIT MC240 box 5, folder 14, 29 June 1976 letter, Kuhn to Amsterdamski, p. 1.
74. MIT MC 240, box 5, folder 14, 29 June 1976 letter, Kuhn to Amsterdamski, p. 2.
75. Kuhn (2000), p. 314.
76. Sigurdsson (1990), 24.
77. Kuhn (2000), p. 319.
78. MIT MC240, box 23, folder 22, 2 March 1983 letter, Achinstein to Kuhn.
79. For reactions of those whose attended the lecture series, see Finkbeiner (1985).
80. MIT MC240, box 23, folder 21, "Thalheimer lectures", p. 3.
81. MIT MC240, box 23, folder 32, "Shearman lectures."
82. MIT MC 240, box 20, folder 13, HPST Panel Review, p. 2.
83. *Ibid*, p. 8.
84. One of the nine external reviewers scored Kuhn's proposal as "good." "I am not so nearly confident," wrote the reviewer, "as to the philosophical quality of what is likely to emerge." *Ibid*, p. 4. The reviewer's concern was the vagueness of Kuhn's proposal so that it could not be properly evaluated with respect to two points. First, "I do not see how the case for untranslatability based on the disjointness required for taxonomic schemes will be made." *Ibid.* The second point concerned bilingualism. If scientists can learn more than one theoretical language then "it is far from clear that *epistemic incommensurability*, which is what matters for the instrumentalism–realism controversy, obtains, for why cannot multi-linguals make at least *some* relevant comparisons." *Ibid.*
85. Heilbron (1998), 510.

Chapter 2

How does Kuhn arrive at *Structure*?

The development of Kuhn's new image of the nature of science began with the 1951 Lowell lectures. In these lectures, Kuhn outlined a dynamic conception of science in contrast to the static conception of traditional philosophy of science. The basic dialectic found in his later writings was present in theses lectures. In *The Copernican Revolution*, he continued to refine his historiographic approach to science. In three essays following *The Copernican Revolution*, Kuhn returned to the philosophical issues that animated his original interest in the history of science and continued to explore those issues in terms and concepts that presaged *Structure*.

The 1951 Lowell lectures: "The quest for physical theory"

Kuhn began the first lecture, "Introduction: textbook science and creative science," by citing the common (mis)perception, especially promoted by Karl Pearson, that the scientist is a "man in the highly starched, gleaming white coat who . . . abandons all prejudice so that he may proceed first to a dispassionate analysis of all the facts and then to the formulation of the immutable laws which govern them."[1] Kuhn's critique of the scientist's image and of the scientific method reflected Conant's earlier rejection of Pearson's distorted image of science and its methods. Conant asserted that Pearson failed "in analyzing the processes of science" and overemphasized the "applicability of what he considers the scientific method."[2]

"Now," declared Kuhn, "I think that this picture of the scientist, and the correlated description of the method by which the scientist reaches his conclusions, is altogether wrong."[3] Rather, Kuhn maintained that "prejudice and preconceptions are inextricably woven into the pattern of scientific research, and that any attempt to eliminate them would inevitably deprive this research of its fruitfulness."[4] Kuhn assured his audience that he, as a once practicing scientist, believed that science produces useful and cumulative knowledge of the world, but that traditional analysis of science distorts the process by which scientific knowledge develops.

> In the broadest and most fundamental sense, the objective of these lectures is then to provide a preliminary description of scientific activity and to discover the relationship of this, the activity of the working scientist, to the products of his profession, to science as a body of human knowledge.[5]

To that end, Kuhn distinguished between science as a dynamic activity or practice and science as a static body of knowledge. Moreover, he utilized the history of science to exhibit the process by which creative science advances, rather than focusing on the finished products of science as given in textbooks. It is this reliance on textbook science that misleads "the dominant empiricist methodological tradition."[6] Textbooks state the immutable scientific laws and marshall forth the experimental evidence to support the laws, thereby covering over the very creative process that leads to the laws in the first place.

> We assume that the structure of knowledge in the textbook, the structure which we give to scientific knowledge for its transmission and preservation, provides a substantial clue to the nature of the creative process by which we gained that knowledge. And it is from this assumption that I should like to dissent.[7]

Although he drew from studies in logic, language, and psychology to support his dissent, Kuhn drew mainly from the history of science. He rejected the traditional history of science in which laws are derived from facts, since it is based on a distorted image of science. Rather he proposed to utilize an alternative approach to the history of science.

In the next three lectures Kuhn presented this alternative historical approach to scientific methodology. In the second lecture, "The foundations of dynamics," Kuhn claimed that the traditional position in which Galileo rejected Aristotle's physics because of Galileo's experiments is a fallacy. Rather Aristotelianism, as an entire system, was rejected:

> we shall not understand the way in which his laws were overthrown unless we note how closely they were associated with a set of views about the cosmos which were rejected together with his laws during the latter portions of the middle ages. For Aristotle's laws, like all scientific laws, did not stand, or fall, alone.[8]

In other words, Galileo's evidence was necessary but not sufficient; rather, the whole Aristotelian system was under evaluation, which also included its logic. Change that logic and the evidence is now efficacious in dethroning the system. Although Galileo was trained in Aristotelian physics he represented a turning point, following a long tradition of logical and physical criticisms of it.[9]

Galileo's rejection, then, was not simply based on evidential rejection of Aristotle, but on a logical one.

> It was in the application of this new logical understanding [originating in Oxford with such people as Nicholas Oreame] and these new logical tools [graphic and algebraic] to the qualitative physics of the impetus school that Galileo made one of the principal contributions—a contribution whose ultimate outcome was of course to destroy totally the impetus school . . . What had changed was the scientific view of the phenomenon: motion had ceased to be a change between fixed endpoints and had become a quality of the moved body, a quality whose intensity was observed to increase throughout the motion.[10]

In the next two lectures, Kuhn applied the new approach to the history of science in the analysis of atomism: "The prevalence of atoms," in which he explored the differences between the Greek and modern notion of atomism, and of electricity, and "The principle of plenitude: subtle fluids and physical fluids," in which he examined the change from the notion of subtle to physical or ethereal or electrical fluids.[11]

In the final four lectures, Kuhn proposed an alternative image of science based on the new approach to the history of science. It was a creative image of science, in contrast to a textbook science of traditional analysis. In the fifth lecture, "Evidence and explanation," Kuhn replaced the initial terms of prejudice and preconception, used in the first lecture.

> In their normal usage, they imply an absence of intellectual activity and a regression from rationality. So that since I now wish to discuss the role of such elements in science, an activity which I take to be intellectual and rational in the extreme, it might be best to admit them to a more constructive function and to call these elements the points of view of the active scientist or the principles which orient his perceptions and judgments about the phenomenal world.[12]

Often these viewpoints are not just physical such as cosmological but also metaphorical.

These viewpoints are "conceptual frameworks," a phrase Kuhn derived from Conant's phrase "conceptual scheme" and presages Kuhn's later notion of paradigm.[13] These frameworks, insisted Kuhn, serve a comprehensive function.

> They suggest problems; they suggest the sorts of evidence relevant for the solutions of these problems; and they suggest the mode in which the answers to these problems must be cast. They are, if you will, predispositions to certain sorts of explanations . . . they are equally predispositions towards evidence, towards facts . . . This suggests that scientific research is inherently circular, that it does not proceed from experimental facts to theories, but that facts and theories are provided together, in a more of less inchoate form, by scientific orientations.[14]

Science progresses, then, "by a series of circular attempts to apply differing orientations or points of view to the natural world."[15] Kuhn's new image of science was dynamic, as opposed to a static image provided by traditional analysis. Here was Kuhn's revolution in nascent form.

In the next lecture, "Coherence and scientific vision," Kuhn drew from psychology to defend the advancement of science though scientists' predispositions. He discussed several examples of perceptual experiments from psychology (found later in *Structure*) and concluded that "the world of our perceptions is not uniquely determined by sensory stimuli but is a joint product of external stimulation and of an activity which we perform in organizing them."[16] It is the predispositions that allow us to negotiate the world and to learn from our experiences. Moreover, these predispositions allow us to see different things even though our stimuli are the same. They are, then, the means by which we compose an everyday "behavioral world," as Kuhn called it. He then drew from language studies and child cognitive development to support his position.

Just as we operate in a behavioral world, so do scientists. But this professional world (a precursor to Kuhn's notion of normal science in *Structure*) is different from the everyday world. First, it is complete in that it supplies everything scientists need to operate effectively and efficiently in the production of scientific knowledge. Moreover, it can also be changed due to an inadequacy, which is an "anomaly," in a behavioral world. This inadequacy may lead to a "crisis." Importantly, for Kuhn, "a crisis, by the recognition of an inadequacy in the older world, transforms experience as well as the mental category in terms of which we deal with experience."[17] Because of their importance in organizing the scientist's behavioral world, these predispositions or orientations cannot be dispensed with easily. Rather, change represents a foundational alteration in a scientist's behavioral world.

In the following lecture, "The role of formalism," Kuhn drew on logic and mathematics, especially in the physical sciences. He pointed out that "the application of formalism to a limited area of knowledge is a tool which by creating greater conceptual freedom aids in the resolution of crisis states in individual science. But the application of logical formalism can create such crises."[18] Logic systems are also important for deriving meaning and for managing and manipulating scientific knowledge. But scientific, as natural, language outstrips such formalization. Even if such formalization is possible, argued Kuhn,

> its results would be to freeze scientific attention upon just those aspects of nature which are embraced by contemporary science. It would provide a place in its meaning system for aspects of nature now considered technically relevant and no place for others. As a result it would not be a language adequate to embrace new conceptual developments in science.[19]

In other words, Kuhn turned the tables on an important tool for the traditional analysis of science. By revealing the limitations of logical analysis, he showed that logic is necessary but insufficient for justifying scientific knowledge. Logic, then, cannot guarantee the traditional, static view of science.

In the final lecture, "Cannons of constructive research," Kuhn continued the examination of logical analysis, especially in terms of language and meaning. His position was that language is a way of dissecting the scientific behavioral world in which scientists operate. But there is always ambiguity or overlap in the meaning of terms as that world is dissected. Certainly scientists attempt to increase the precision of their terms but not to the point that ambiguity is eliminated. For Kuhn, this had an important advantage.

> But in practice we do not achieve this complete formalization of our texts. And historically it appears extremely fortunate that we do not do so. We do leave vague meaning fringes on scientific terms, and our research is always conducted within the area determined by these vaguer fringes. It is in these areas alone that questions can arise as to established theories. The effect of full formalization is to make a theory impregnable except in so far as precise measurements may display deviations from the postulated laws. And this is not the most usual source of scientific advance . . . Thus, the vague and behaviorally determined meaning systems of natural language are one of the most important vehicles for what we have previously called scientific orientations. The area of stable

> meaning is an area of what we take to be certain knowledge. In this area no questions arise. The area outside our meaning system is an area which can be mediated neither by our language nor our perceptions. Prior to a shift of meaning systems no questions can arise here either. It is only in the area provided by meaning fringes that scientific questions can arise and that scientific exploration can occur.[20]

Kuhn claimed that scientific exploration may proceed in one of two ways. The first "may result in increasing the scope and precision of the existing meaning system."[21] He noted that this type of activity occurs during periods when a particular orientation or predisposition is operating. This is the constructive period in scientific development. The second way represents the destructive period of scientific development, in which the older meaning system is replaced by a newer one. This period is preceded by a crisis state, in which the older meaning system is no longer sufficient to guide research. Rather disputes over the meaning of terms arise, with eventual divergence over the meaning of those terms. Kuhn claimed that these crisis periods lead to scientific revolutions, which, in turn, "terminate with new precise criteria for scientific meanings and frequently with new central cores of meaning for natural languages."[22] Scientific revolutions, for Kuhn, are "simultaneously destructive and creative of scientific orientation, behavioral worlds, and meaning systems."[23]

Kuhn concluded the lectures by rehearsing the patterns of scientific activity he had explicated through historical case studies, language acquisition, and the psychology of perceptions, in order to distinguish between creative and textbook science.

> Our linguistic apparatus, our involuntary yet alterable organization of our perceptions provide us with our science in embryo. They are the vehicles of that inevitable predisposition to theories of a certain sort which, as we have noted again and again, govern our experiments and the conclusions which we draw from our experiments.[24]

But this creative process is what also grounds textbook science. "By increasing abstraction and increasing precision," claimed Kuhn, "we can create within the pre-existing patterns of language and perceptions a summary of our most certain knowledge we call science . . . which we embody in scientific texts."[25]

However, these perceptual and linguistic organizing structures were, for Kuhn, a deep problem surfacing over again during the lectures in that they are impermanent and mutable. "They arise from experience, they legislate for experience," contended Kuhn, "but they may prove inadequate to experience. When they do, they must be altered, and the process by which they are altered is destructive and constructive."[26] Without these organizing structures there is no science and with them only limited kinds of science. "So continuing progress in research," concluded Kuhn, "can be achieved only with successive linguistic and perceptual re-adaptations which radically and destructively alter the behavioral worlds of professional scientists."[27]

The Copernican revolution

Kuhn claimed he identified an important feature of the Copernican revolution, which previous scholars missed: its plurality. What Kuhn meant by plurality is that although Copernicus' *De Revolutionibus* "consists principally of mathematical formulas, tables, and diagrams, it could only be assimilated by men able to create a new physics, a new conception of space, and a new idea of man's relation to God."[28] Kuhn was interested in the new world(s) the revolution prompted. A methodological corollary to this insight was Kuhn's breach of institutional limits that separate the physical sciences from the humanities, which gave the appearance that his book is really two: "one dealing with science, the other with intellectual history."[29] "Scientific concepts are ideas," according to Kuhn, "and as such they are the subject of intellectual history."[30] It is the combination of the science and intellectual history that was Kuhn's methodological insight. Scientists have philosophical and even religious commitments, which are important for the development of scientific knowledge. This stance was anathema to traditional philosophers of science, who believed that such commitments played little if any role in the development of scientific knowledge.

The genesis of Kuhn's study of the Copernican revolution was the lectures he delivered in a science course for non-majors at Harvard. Kuhn's approach in the course was to situate the scientific information within an historical and a philosophical context. He defended this pedagogical method, claiming that students are better motivated to learn the material when they see the connections of science with culture at large. According to Kuhn, "the technical facts and theories that they learn function principally as paradigms rather than as intrinsically useful bits of information."[31] Although Kuhn used the term paradigm, he did not expand upon it. But the kernel of the idea was present and would bear historical and philosophical fruit in *Structure*. Moreover, Kuhn's concern with the Copernican revolution was not only pedagogical but also professional. "If we can discover the origins of some modern scientific concepts and the way in which they supplanted the concepts of an earlier age," stated Kuhn, "we are more likely to evaluate intelligently their chances for survival."[32]

Kuhn began his reconstruction of the Copernican revolution by establishing the genuine scientific character of ancient cosmological schemes, especially the two-sphere cosmology composed of an inner sphere for the earth and an outer sphere for the heavens. Importantly for Kuhn, astronomical "observations in themselves have no *direct* cosmological consequences . . . [rather] conceptual schemes [like the two-sphere cosmology] derived from these observations do depend upon the imagination of scientists. They are subjective through and through."[33] Conceptual schemes exhibit three important features. They are comprehensive in terms of scientific predictions, there is no final proof for them, and they are derived from other schemes. Finally, to be successful conceptual schemes must perform logical and psychological functions. The logical function is determined in explanatory terms, while the psychological function is determined in existential terms. Although the logical function of the two-sphere cosmology continued to be problematic, its psychological function afforded adherents "with a worldview, defining their place in the created world and giving physical meaning to their relation with the gods."[34] Such a position ran counter to that of the traditional view of science.

The major logical problem with the two-sphere cosmology was the movement and positions of the planets. In *Almagest*, according to Kuhn,

> it was Ptolemy who first put together a particular set of compounded circles to account, not merely for the motions of the sun and moon, but for the observed regularities and irregularities in the apparent motions of all the seven planets.[35]

The conceptual scheme Ptolemy developed in the second century guided research for the next millennia. But problems surfaced with the scheme and predecessors could only correct it so far with ad hoc modifications. As Kuhn contended, the Ptolemaic "system of compounded circles was an astounding achievement. *But it never quite worked.*"[36]

Kuhn asked at this point in the narrative why the Ptolemaic system, given its imperfection, was not overthrown sooner. The answer, for Kuhn, depended on a distinction between the logical and psychological dimensions of scientific revolutions. According to Kuhn there are logically different conceptual schemes that can organize and account for the data. The difference among these schemes is their predictive power. Consequently, if an observation is made that is not compatible with a prediction the scheme must be replaced. But Kuhn contended that "historically the process of revolution is never, and could not possibly be, so simple as the logical outline indicates."[37] There is also the psychological dimension to a revolution. Kuhn was overturning the logical view of science, which he found inadequate to account for scientific change in cultural terms.

Copernicus had to overcome not only the logical function of the Ptolemaic system but more importantly its psychological function. That later function was developed as far back as Aristotle, who wedded the two-sphere cosmology to a philosophical system. Through the Aristotelian notion of motion among the heavenly and earthly spheres, the inner sphere was connected and dependent on the outer sphere.

> In an era when man's need to understand and control his fate immeasurably transcended his physical and intellectual tools, this apparent celestial power was naturally extended to the other celestial wanderers. Particularly after Aristotle supplied a physical mechanism—the frictional drive—through which the heavenly bodies could produce terrestrial change, there was a plausible basis for the belief that an ability to predict the future configurations of the heavens would enable men to foretell the future of men and nations.[38]

That ability to presage future events was linked astronomically to astrology. Such an alliance according to Kuhn provided a formidable obstacle to change of any kind.

But change began to take place, albeit slowly. From Aristotle to Ptolemy, a sharp distinction arose between the psychological aspects of cosmology and the mathematical precision of astronomy. By Ptolemy's time, astronomy was less concerned with data interpretation and more with their prediction. To some extent this aided Copernicus, in that whether the earth moved was determined by predictive power. But still the earth as center of the universe gave existential consolation to people. The strands of the Copernican revolution, then, included not only the astronomical but also the theological, economic, and social. For example, Kuhn explored the cosmological reliance of Dante's *Divine Comedy* on the Aristotelian-Ptolemaic cosmology, and concluded: "Moving the earth may necessitate moving God's Throne."[39] But criticism of ancient cosmology also originated with the Scholastic tradition, such as the impetus theory of motion that replaced Aristotle's theory and in the hands of Newton established the Copernican revolution. Other factors also paved the way for the Copernican revolution, including the Protestant reformation, navigation for oceanic voyages, calendar reform, and Renaissance humanism and neoplatonism.

Copernicus, according to Kuhn, was the immediate inheritor of Aristotelian-Ptolemaic cosmological tradition and, except for the position of the earth, was closer to that tradition than to modern astronomy: "the universe of the *De Revolutionibus* is classical in every respect that Copernicus can make seem compatible with the motion of the earth."[40] He had, as it were, one foot in the ancient tradition and the other headed for the modern tradition. Kuhn considered *De Revolutionibus* to be "a revolution-making rather than a revolutionary text."[41] Although the problem Copernicus addressed was the same for his predecessors, planetary motion, his solution was to revise the mathematical model for that motion by making the earth a planet that moves around the sun. Essentially, Copernicus maintained the Aristotelian-Ptolemaic universe but exchanged the earth for the sun. Although Copernicus had eliminated major epicycles he still used minor ones and the accuracy of planetary position was no better than Ptolemy's. Kuhn concluded that Copernicus did not really solve the problem of the planets.

Although Copernicus did not solve the problem of planetary motion, he "did convince a few of Copernicus' successors that sun-centered astronomy held the key to the problem."[42] The reason for this conviction was aesthetic, according to Kuhn: "Copernicus' arguments . . . appeal, if at all, not to the utilitarian sense of the practicing astronomer but to his aesthetic sense and to that alone."[43] It was these few, convinced by the "neatness and coherence" of *De Revolutionibus*, who completed the Copernican revolution. Importantly for Kuhn, that revolution did not occur overnight but by degrees. Kuhn defended this position, claiming the "extent of the innovation that any individual can produce is necessarily limited, for each individual must employ in his research the tools that he acquires from a traditional education, and he cannot in his own lifetime replace them all."[44]

Initially, according to Kuhn, there were only a few supporters of Copernicus' cosmology, including George Joachim Rheticus, Thomas Digges, and Michael Maestlin. Although the majority of astronomers accepted the mathematical harmonies of *De Revolutionibus*, after its publication in 1543, they rejected or ignored its cosmology. Tycho Brache, for example, although relying on Copernican harmonies to explain astronomical data, proposed a system in which the earth was still the universe's center. Essentially it was a compromise between ancient cosmology and Copernican mathematical astronomy. However, Brache recorded accurate and precise astronomical observations, which helped to compel others toward

Copernicanism. Johannes Kepler was one of the first formidable defenders of Copernicanism, who, Kuhn claimed, was "converted" as Maestlin's student and Kepler's "faith in it never wavered."[45] Although Kepler espoused Copernicanism he was not uncritical of it and extended its mathematical precision to solve the planetary problem. The final player Kuhn considered in the revolution was Galileo, whose "astronomical work contributed primarily to a mopping-up operation, conducted after the victory was in sight."[46] Moreover, Galileo's telescopic observations afforded not "proof" of but "propaganda" for Copernicanism.[47]

During the seventeenth century, according to Kuhn, Copernicanism gained acceptance with astronomers but there were a few who attempted to replicate the accuracy of Kepler's astronomical calculations but without his radical cosmology. However, at century's end consensus was achieved among astronomers. But Copernicanism still faced serious resistance from Christianity owing to, explained Kuhn, "a subconscious reluctance to assent in the destruction of a cosmology that for centuries had been the basis of everyday practical and spiritual life."[48] Religious resistance continued long after the seventeenth century; but, as Kuhn notes, "old conceptual schemes do fade away."[49] The Copernican revolution was completed with the Newtonian universe.

The Newtonian universe not only had an impact on astronomy but also on the other sciences and even non-sciences. For instance, Newton's universe changed the nature of God to that of a "clockmaker, the Being who had shaped the atomic parts, established the laws of their motion, set them at work, and then left them to run themselves."[50] For Kuhn, Newtonianism's impact on disciplines other than astronomy was an example of its "fruitfulness." Scientific progress, concluded Kuhn, is not the linear process, as defended by traditional philosophers of science, in which facts are stockpiled in a scientific warehouse. Rather, it is the repeated destruction and replacement of scientific theories. Finally, just as the Ptolemaic universe was replaced by a Copernican one so the Newtonian universe is currently being replaced by an Einsteinian one.

Reviews

Philip Weiner criticized Kuhn's notion of scientific progress, which he considered to be an important logical problem in the philosophy of science. Weiner argued that current scientific theories do not "destroy" previous theories, "if 'destroy' means eliminating them completely along with their confirmatory evidence," but rather they "correct" them by situating them in a larger explanatory context.[51] Moreover for Weiner, a logical continuity exists between the data of the previous theory and the more precise data and enlarged framework of the current theory; hence, a current theoretical "explanation is part of the cumulative growth of scientific explanations."[52] Although Weiner holds to a traditional notion of cumulative scientific progress, he appreciated Kuhn's historical turn for the philosophy of science.

> If the philosopher of science is more than a logical analyst by also considering the cultural implications of fundamental changes in scientific world-views, and the present reviewer agrees with the author about this broader scope of the history of scientific thought, then his remarks about the affiliations of Copernicus' astronomical views to neoplatonic, Aristotelian, and Newtonian patterns are both pertinent and suggestive.[53]

For historians of science, Kuhn's *The Copernican Revolution* was exemplary of contemporary historical scholarship that takes into consideration the cultural context of science. "No other book," stated Herbert Butterfield, "so enables us to see the intellectual hurdles that existed and to relive something of the process of actual scientific discovery."[54] Harry Woolf concurred with Butterfield's assessment and claimed that Kuhn's book was "a paradigm of synthesis and interpretation."[55] Others congratulated Kuhn for the sensitive narration of the Copernican revolution, especially the documentation of its gradual unfolding, "the result of hundreds of years of conjecture, observation, calculation, intuition, supposition, and to a certain extent emotion."[56] Although a number of historians found factual errors within the narrative and questioned Kuhn's historical and philosophical acumen, one reviewer wrote that "we can certainly expect great things from him in the future."[57]

The reception of *The Copernican Revolution* indicates Kuhn's acceptance into the philosophical and historical communities. His reconstruction of the revolution was considered for the most part scientifically accurate and methodologically appropriate. The integration of the science and the social was considered an advance over other histories that ignored these dimensions of the historical narrative. Although philosophers appreciated the historical dimension of Kuhn's study, they found its analysis insufficiently precise according to their standards. Overall, both the historical and philosophical communities expressed no major objections to the theory of science that animated Kuhn's narrative.

Significance of *The Copernican Revolution*

Kuhn's reconstruction of the Copernican revolution portrayed a radically different image of science than that by traditional philosophers of science. The justification of scientific knowledge was not simply a logical or an objective affair but included non-logical or subjective factors. "The triumph of Copernicanism was a gradual process," according to Kuhn, "and its rate varied greatly with social status, professional affiliation, and religious belief."[58] Moreover, he claimed that "the early Copernicans did not fully see where their work was leading."[59] In other words, scientific progress is not a clear-sighted linear process aimed directly at the truth. Rather, there are contingencies that can divert and forestall the march of science. Moreover, Copernicus' revolution changed the way the world is viewed not only by astronomers but also by non-astronomers as well. This world change, as later denoted by Kuhn, was the result of new sets of challenges, new techniques, and new hermeneutics for interpreting data.

Besides differing from traditional philosophers of science, Kuhn's view of science put him at odds with Whig historians of science. These historians underrated ancient cosmologies by degrading them to traditional myth or religious belief. Such a move was often a rhetorical ploy on the part of the victors to enhance the status of the current scientific theory. But for Kuhn, the

> older astronomical theories differed radically from the ones we now hold, but most of them received in their day the same resolute credence that we now give our own. Furthermore, they were believed for the same reasons: they provided plausible answers to questions that seemed important.[60]

Only by showing how Aristotelian-Ptolemaic geocentric astronomy was authentic science, could Kuhn argue for the radical transformation (a revolution) that Copernican heliocentric astronomy invoked or initiated.

Finally, as Heilbron recognized later, nascent in Kuhn's account of the Copernican revolution was the framework later articulated in *Structure*: the successful Aristotelian-Ptolemaic conceptual scheme (paradigm) that guided research for over a millennium but failed to account for certain irregularities concerning planetary movement (anomalies) was replaced —after an intense struggle (crisis)—by a new conceptual scheme that differed radically (incommensurably) from the previous one (paradigm shift or revolution).[61] Kuhn also asserted that Copernicus' theory was not accepted simply for its predictive ability, since it was not as accurate as the original conceptual scheme, but because of non-empirical factors, such as the simplicity of the Copernican system in which certain ad hoc modifications for accounting for the orbits of various planets were eliminated.

The emergence of *Structure*

Although *The Copernican Revolution* represented a significant advance in Kuhn's articulation of a revolutionary theory of science, there were several issues that had to be sorted out before he was ready to publish it. What was missing from Kuhn's reconstruction of the Copernican revolution was an understanding of how scientists function on a daily basis, when no impending revolution is looming. That understanding emerged gradually in three papers written from the mid-late 1950s to the early 1960s.

"The function of measurement in modern physical science"

Kuhn began the paper by quoting Lord Kelvin's dictum: "If you cannot measure, your knowledge is meager and unsatisfactory." He acknowledged that "physical science is so often seen as *the* paradigm of sound knowledge."[62] However, he believed that there is a problem with this view since the function and origin of the efficacy of measurement in physical science is mythic in origin. To approach this problem, Kuhn addressed from an historical perspective questions concerning the "actual function" and the "special efficacy" of measurement in physical science.

Part of the reason for Kuhn's concern over the function and efficacy of measurement in physical science was the textbook tradition, which he believed perpetuates a myth about measurement that is misleading. Kuhn compared the textbook presentation of measurement to a machine in which laws and theories along with "initial conditions" are fed into the machine's hopper at the top, a handle on the side is turned representing logical and mathematical operations, and exiting the machine's chute in the front are numerical predictions. These predictions are then compared to experimental measurements. The function of these measurements serves as a test of the theory, which is the confirmation function of measurement. Another function of measurement as presented in the textbook is exploration, in terms of generalizing laws and theories from measurements. This function, stated Kuhn, is like running the above machine in reverse, except the logical and mathematical operations are aided by intuition.

Kuhn claimed that the above functions are not why measurements are reported in textbooks; rather, measurements are there to give the reader an idea of what the professional community believes is a reasonable agreement between theoretical predictions and experimental observations. Reasonable agreement depends upon the approximate, not exact, agreement between theory and data and changes from one science to the next. Moreover, there is no external criterion for determining reasonableness, only "the mere fact that [measurements] appear, together with theory from which they are derived, in a professionally accepted text."[63] Kuhn cautioned people, especially those dependent on traditional philosophy of science, that "though texts may be the right place for philosophers to discover the logical structure of finished scientific theories, they are most likely to mislead than to help the unwary individual who asks about productive methods."[64]

For Kuhn, the actual function of normal measurement in physical science is found in journal articles, "which displays not finished and accepted theories, but theories in the process of development."[65] That function is neither detection of novel theories nor the confirmation of older ones. Discovery and exploratory measurements in physical science instead are rare. The reason is that changes in theories, which require discovery or confirmation, occur during revolutions, which are also quite rare. Once a revolution occurs, moreover, the new theory exhibits only a potential for ordering natural phenomena. To actualize that ordering is the function of measurement during what Kuhn called "the normal practice of science . . . [which consists of] a complex and consuming mopping-up operation that consolidates the ground made available by the most recent theoretical breakthrough and that provides essential preparation for the breakthrough to follow."[66] The function of normal measurement is to tighten the reasonable agreement between predictions of the new theory and experimental observations of the world.

The textbook tradition is also misleading in terms of normal measurement's effects. The tradition claimed that theories must conform to quantitative facts. "But in scientific practice, as seen through the journal literature," wrote Kuhn, "the scientist often seems rather to be struggling with the [quantitative] facts, trying to force them into conformity with a theory he does not doubt."[67] Such facts are not the "given" but the "expected" and the scientist's task is to obtain them. It is this obligation to obtain the expected quantitative fact that is often the stimulus for developing novel technology. Moreover, this obligation often bars the route from measurement to theory. "Numbers gathered without some knowledge of the regularity to be expected," asserted Kuhn, "almost never speak for themselves."[68] Rather, a well-developed theoretical system is required for meaningful measurement in physical science.

Besides the function of normal measurement in physical science, Kuhn also examined the function of extraordinary measurement. It is this latter type of measurement that exhibits the discovery and confirmatory functions. When normal scientific practice that results consistently in unexpected data or anomalies leads to an abnormal situation or crisis, extraordinary measurement occasionally aids discovery to resolve the situation. “To the extent that measurement and quantitative technique play an especially significant role in scientific discovery,” contended Kuhn, “they do so precisely because, by displaying serious anomaly, they tell scientists when and where to look for new quantitative phenomena.”[69]

Besides discovery, crises also lead to the invention of new theories. Again, extraordinary measurement plays a critical role in this process. Theory innovation in response to quantitative anomalies leads to decisive measures for judging a novel theory’s adequacy, whereas qualitative anomalies generally lead to ad hoc modifications of theories.

> In scientific practice the real confirmation questions always involve the comparison of two theories with each other and with the world, not the comparison of a single theory with the world. In these three-way comparisons, [extraordinary] measurement has a particular advantage.[70]

In other words, extraordinary measurement allows scientists to choose among competing theories.

In this paper, Kuhn moved closer toward a notion of normal science through an analysis of normal measurement, in contrast to extraordinary measurement, in physical science. Kuhn’s conception of science continued to distance him from traditional philosophers of science. But the notion of normal measurement was not as robust as required. Importantly, Kuhn was changing the agenda for philosophy of science from the justification of scientific theories as finished products in textbooks to the dynamic process by which theories are tested and assimilated into the professional literature. A robust notion of normal science was the revolutionary concept he needed to overturn the traditional view of science as a static body of knowledge.

“The essential tension”

With the introduction of normal and extraordinary measurements, the step toward the notions of normal and extraordinary sciences in Kuhn’s revolutionary view of science was immanent. Kuhn worked out those notions in this paper. He began by addressing the notion that creative thinking in science assumes a particular view of science, a view in which science advances through unbridled imagination and divergent thinking. Kuhn acknowledged that such thinking is responsible for some scientific progress, but he proposed that convergent thinking is also an important means of such progress.

> 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.[71]

While revolutions, which depend on divergent thinking, are an obvious means for scientific progress, Kuhn insisted that few scientists consciously design revolutionary experiments. Rather, almost all scientist engage in "normal research," which is "a highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession."[72] But the occasional scientist may eventually break with the tradition of normal research and replace it with a new tradition. Science, as a profession, is both traditional and iconoclastic and is at times practiced in a space created by this tension.

Next Kuhn introduced the term paradigm, while discussing the pedagogical advantages of convergent thinking, especially as exhibited in science textbooks. Whereas textbooks in other disciplines include the methodological and conceptual conflicts prevalent within the discipline, science textbooks

> exhibit concrete problem-solutions that the profession has come to accept as paradigms, and they then ask the students, either with a pencil and paper or in the laboratory, to solve for himself problems very closely related both in method and substance to those through which the text or the accompanying lecture has led him.[73]

Science education is then the transmission of a tradition that guides the activities of practitioners, which was very different from the progressive education of Kuhn's youth. In science education students are taught not to evaluate the tradition, whereas in progressive education students are encouraged to engage and evaluate it. Kuhn's early education certainly positioned him to see the stark difference between these two pedagogical methods.

Progress within normal research projects represents attempts to bring theory and observation into closer agreement and to extend a theory's scope to new phenomena. "Under normal conditions the research scientist is not an innovator but a solver of puzzles," observed Kuhn, "and the puzzles upon which he concentrates are just those which he believes can be both stated and solved within the existing scientific tradition."[74] Given the convergent and tradition-bound nature of science education and of scientific practice, how can normal research be a means for the generation of revolutionary knowledge and technology? According to Kuhn, a mature science provides the background that allows practitioners to identify nontrivial problems or anomalies with a paradigm: "In 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."[75] In other words, without mature science there can be no revolution.

In conclusion, Kuhn came full circle to the "essential tension" in scientific research: "the productive scientist must be a traditionalist who enjoys playing intricate games by preestablished rules in order to be a successful innovator who discovers new rules and new pieces with which to play them."[76] The challenge is "to understand how these two superficially discordant modes of problem solving can be reconciled both within the individual and within the group."[77] But Kuhn cautioned his audience that by focusing on the conditions for divergent thinking the conditions for convergent thinking may be ignored to our peril in terms of scientific progress *via* normal research.

The 1961 "The structure of scientific change" symposium

Kuhn's "The function of dogma in scientific research"

Kuhn appealed to the audience's common view of science as an objective and open-minded enterprise. Although this is the ideal, the reality is that often scientists already know what to expect from their investigations into nature. If the expected is not forthcoming, then scientists must struggle to find conformity between what is expected and what is observed. "Strongly held convictions that are prior to research," claimed Kuhn, "often seem to be a precondition for success in the sciences."[78] These convictions represent the "dogmatism of a mature science," as encoded in textbooks. This dogmatism, which is so critical for the normal practice and advance of science, defines the problems for the profession and the criteria for their solution. Although a community's commitment to a dogma is essential for participation in the community, the dogma also serves as "an immensely sensitive detector of the trouble spots from which significant innovations of fact and theory are almost inevitably educed."[79]

Kuhn next introduced an expanded notion of paradigm, which is associated with scientific practice in general rather than simply with a model for research, as in "The essential tension." Paradigm is enlarged to include not only a community's previous scientific achievements but also its theoretical concepts, the experimental techniques and protocols, and even the natural entities. In short, it is the community's body of beliefs or foundations. Paradigms are also open-ended in terms of problems to be solved. Moreover, they are exclusive in their nature, in that there is only one paradigm per mature science. Finally, they are not permanent fixtures of the scientific landscape, eventually paradigms are replaced by others. Importantly, for Kuhn, when a paradigm is replaced by another, the two paradigms are radically different.

Having done paradigmatic spade work, Kuhn then discussed the notion of normal scientific research. Kuhn's main thesis was "that scientists, given a paradigm, strive with all their might and skill to bring it into closer and closer agreement with nature."[80] The process of matching paradigm and nature also includes extending and applying the paradigm to new parts of nature. This does not necessarily mean discovering the unknown as it does the known. In other words, a scientist engaged in normal, paradigmatic research is a puzzle solver much like a chess player.

> The paradigm he has acquired through prior training provides him with the rules of the game, describes the pieces with which it must be played, and indicates the nature of the required outcome. His task is to manipulate those pieces within the rules in such a way that the required outcome is produced.[81]

Thus, it is no surprise that scientists are committed to their paradigms and normally resist changing them. Paradigms provide the maps needed to investigate nature; without them there would be little scientific progress.

But paradigms are imperfect maps and eventually fail to guide their users over the natural terrain. Breakdowns in paradigms, or anomalies, are inevitable and lead to unanticipated discoveries. "After a first paradigm has been achieved," claimed Kuhn, "a breakdown in the rules of the pre-established game is the usual prelude to significant scientific innovation."[82] But first breakdown leads to a crisis, in which the research community realizes that the accumulated anomalies indicate a serious problem with the paradigm. Scientists then begin to question their discipline's foundation and to experiment outside the paradigm's aegis. "Only under circumstances like these, I suggest," wrote Kuhn, "is a fundamental innovation in scientific theory both invented and accepted."[83] He concluded with a clearer articulation of tension between dogma and innovation, than previously: "Scientists are *trained* to operate as puzzle-solvers from established rules, but they are also *taught* to regard themselves as explorers and inventors who know no rules except those dictated by nature itself."[84]

Discussion

Although Rupert Hall was sympathetic to Kuhn's notion of paradigm, he proposed that "intellectual framework" captures better what Kuhn is striving for. Moreover, Hall suggested that scientists' resistance to innovation is more than the result of their conservatism, it also involves false elements carrying over from the old to new paradigm that cannot be addressed because of limited information available to resolve them. He then discussed the paradox between dogma and innovation that Kuhn's view of science engenders. After rehearsing Kuhn's two solutions for the paradox, progress within an existing paradigm or paradigm replacement, Hall claimed Kuhn admitted that paradigms are rarely complete and that scientists are seldom committed completely to them. For Hall this was a significant concession.

> Once we admit that the paradigm or intellectual framework is somewhat less than monolithic, and allow that in most periods it has been a somewhat crazy, rambling structure, then we may begin to wonder whether reluctance to change it, beyond narrow limits, is not as much due to lack of human capacity as to weight, or inertia, of scientific dogma.[85]

For Hall, Kuhn's notion of dogma in science is inconsequential for significant scientific progress and Kuhn's defense of it is simply "an apology for weakness." Kuhn agreed with Hall that scientists resist innovation because of insufficient information. But according to the traditional view of science, under such conditions scientists should not make any conclusions—even to resist innovation. Commitment to a paradigm is not "an apology for weakness" but part of the scientist's "tool kit" for practicing science.

Stephen Toulmin took issue with Kuhn's apparent paradox between dogma and innovation. He argued that Kuhn's notion of dogma is unnecessary for scientific practice. Toulmin noticed that there is an inherent ambiguity in Kuhn's use of preconceived ideas and proposed a distinction to clarify their use. "One may have preconceived ideas," wrote Toulmin, "in the sense of holding prejudged (prejudiced) beliefs; or alternatively in the sense of employing preformed concepts."[86] Only in the former sense is science dogmatic in terms of being subjective and close-minded. But in the latter sense, science is both objective and open-minded and is dogmatic. He also took issue with the scope of Kuhn's use of paradigm. Although he had no concern with a narrow use of the term, e.g. basic concepts, Toulmin found the expanded scope may lead to prejudice, such as unquestioned authority. Kuhn disagreed with Toulmin's distinction between "prejudged" and "preformed" concepts, since both facets of concepts must necessarily be present concomitantly to guide future research. As for the confusion over the narrow and broad senses of paradigm, Kuhn argued that the narrow sense is insufficient to account for theory assessment and that the broader conception is not the result of prejudice but of genuine conflict over data interpretation.

In contrast to Kuhn, Bentley Glass believed that contemporary scientists discuss the validity of their basic assumptions and that the rapid growth in scientific knowledge would marginalize the role of paradigms in contemporary science. But what concerned Glass most were the implications of Kuhn's view of science for pedagogy.

> I am appalled to think that, if Mr. Kuhn is right, we should go back to teaching paradigms and dogmas, not as merely temporary expedients to aid us more clearly to visualize the nature of our scientific problems, but rather of the regular, approved method of scientific advance.[87]

Kuhn, from personal experience, disagreed with Glass that scientists discuss their basic assumptions. Moreover, Kuhn emphatically denied that his analysis of scientific dogma is a prescription for teaching science. He wholeheartedly supported contemporary educational reform.

Polanyi endorsed Kuhn's position with respect to a scientist's commitment to a paradigm, since he articulated previously a similar position. "A commitment to a paradigm," claimed Polanyi, "has thus a function hardly distinguishable from that which I have ascribed to a heuristic vision, to a scientific belief, or to a scientific conviction."[88] Moreover, he agreed with Kuhn that scientists must use caution when confronting anomalous evidence so as not to waste their time. What Polanyi found lacking in Kuhn's account, however, was how to demarcate between anomalous evidence that requires attention and that which does not. "Is there any rule," inquired Polanyi, "for distinguishing between the two?"[89] Of course, the answer is no. "We have some useful maxims to guide us," wrote Polanyi, "but the choice of the maxim to be applied and the discretion left open in applying it still leaves responsibility for his own conclusion to the individual worker engaged in research."[90] He claimed that such a position "tears open and leaves open the main questions concerning the nature of scientific method and the foundations of scientific knowledge."[91] Polanyi concludes that he "can accept the excellent paper by Mr. Kuhn only as a fragment of an intended revision of the theory of scientific knowledge."[92] Kuhn responded to Polyani's question concerning rules for distinguishing between critical and non-critical anomalous data by noting that the burden is not on the individual scientist but on the community. He also agreed that the dogma paper is but a fragment of the solution to problems associated with the traditional view of science. The complete solution was soon to appear in *Structure*.

Notes

1. MIT MC240, box 3, folder 11, "Introduction: textbook science and creative science," p. 3.
2. Conant (1947), p. 112.
3. MIT MC240, box 3, folder 11, "Introduction: textbook science and creative science," p. 3.
4. *Ibid*, p. 4.
5. *Ibid*, p. 5.
6. *Ibid*, p. 9.
7. *Ibid*, pp. 12–13.
8. MIT MC240, box 3, folder 11, "The foundations of dynamics," p. 9.
9. *Ibid*, pp. 11–13.
10. *Ibid*, pp. 21–23.
11. The lecture on atomism was revised as Kuhn (1951).
12. MIT MC240, box 3, folder 11, "Evidence and explanation," p. 3.
13. *Ibid*, p. 13. Conant (1947), pp. 18–19.
14. MIT MC240, box 3, folder 11, "Evidence and explanation," pp. 15–16.
15. *Ibid*, p. 17.
16. MIT MC240, box 3, folder 11, "Coherence and scientific vision," at VI-4-2.
17. *Ibid*, at VI-8-3.
18. MIT MC240, box 3, folder 11, "The role of formalism," at VII-7-2.
19. *Ibid*, at VII-7-1.
20. MIT MC240, box 3, folder 11, "Cannons of constructive research," at VIII-6-2-4.
21. *Ibid*, at VIII-6-4.
22. *Ibid*, at VIII-6-5.
23. *Ibid*. The constructive/destructive dialectic represents Kuhn's first pass at what would eventually become the normal/ revolutionary dialectic.
24. *Ibid*, at VIII-6-6.
25. *Ibid*.
26. *Ibid*, at VIII-7-1.
27. *Ibid*.
28. Kuhn (1957), p. vii.

29. *Ibid*, p. viii.
30. *Ibid*, p. viii.
31. *Ibid*, p. ix.
32. *Ibid*, p. 4.
33. *Ibid*, p. 26.
34. *Ibid*, p. 38.
35. *Ibid*, p. 72.
36. *Ibid*, p. 73.
37. *Ibid*, p. 76.
38. *Ibid*, p. 93.
39. *Ibid*, p. 114.
40. *Ibid*, p. 155.
41. *Ibid*, p. 135.
42. *Ibid*, p. 172.
43. *Ibid*, p. 181.
44. *Ibid*, p. 183.
45. *Ibid*, p. 209.
46. *Ibid*, p. 220.
47. *Ibid*, p. 224.
48. *Ibid*, p. 226.
49. *Ibid*, p. 227.
50. *Ibid*, p. 263.
51. Weiner (1958), 298.
52. *Ibid*, p. 298.
53. *Ibid*, p. 299.
54. Butterfield (1958), 656.
55. Woolf (1958), 367.
56. Hellman (1957), 218.
57. *Ibid*, 220.
58. Kuhn (1957), p. 227.
59. *Ibid*, pp. 227–8.
60. *Ibid*, p. 3.
61. Heilbron (1998), 508. For additional analyses of Kuhn's *The Copernican Revolution*, see Swerdlow (2004) and Westman (1994).
62. Kuhn (1961), 161.
63. *Ibid*, 166.
64. *Ibid*, 167.
65. *Ibid*, 162.
66. *Ibid*, 168.
67. *Ibid*, 171.
68. *Ibid*, 175.
69. *Ibid*, 180.
70. *Ibid*, 184.
71. Kuhn (1959), p. 162.

72. *Ibid*, p. 163.
73. *Ibid*, p. 165.
74. *Ibid*, p. 170.
75. *Ibid*, p. 171.
76. *Ibid*, p. 172.
77. *Ibid*.
78. Kuhn (1963), p. 348.
79. *Ibid*, p. 349.
80. *Ibid*, p. 369.
81. *Ibid*, p. 362.
82. *Ibid*, p. 365.
83. *Ibid*, p. 367.
84. *Ibid*, p. 368.
85. Hall and Polanyi (1963), p. 373.
86. Glass *et al* (1963), p. 383.
87. *Ibid*, p. 382.
88. Hall and Polanyi (1963), p. 375.
89. *Ibid*, p. 380.
90. *Ibid*.
91. *Ibid*, p. 379.
92. *Ibid*, p. 380.

PART II

Structure and its bumpy path

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Chapter 3

What is *The Structure of Scientific Revolutions*?

Structure was not a single publishing event in 1962 but covered the years from 1962 to 1970. After its original publication, Kuhn was occupied for the rest of the sixties addressing criticisms directed to the ideas contained in it, especially paradigm. During this time, he continued to develop and refine his new image of science. The end point was a second edition of *Structure* that appeared in 1970. The text of the revised edition, however, remained essentially unaltered and only a "Postscript" was added in which Kuhn addressed his critics.

A new historiography

As Aristotle's opening sentence to *Metaphysics* captures its essence, so Kuhn's opening sentence to *Structure* captures its essence: "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."[1] What Kuhn proposed to accomplish in *Structure* was a new image or theory of science, especially in terms of science's process rather than its product. This image, claimed Kuhn, differs radically from the traditional one of science. That difference hinged on a shift from a logical analysis and an explanation of scientific knowledge as a finished product to an historical or natural description and explanation of the scientific practices and processes by which scientific knowledge is produced by a community of practitioners. In short, it was a shift from the subject (the product) to the verb (to produce).

According to the traditional view, science is a repository of accumulated facts, discovered by individuals at specific periods in history. One of the central tasks of the historian, given this traditional view of science, is to answer questions about who discovered what, where, and when. Even though the task seems straightforward, many historians found it difficult and doubted whether these are the right kinds of questions to ask concerning science's historical record. "The result of all these difficulties and doubts is a historiographic revolution in the study of science," observed

Kuhn, "though one that is still in its early stages."[2] This revolution changed the sorts of questions historians ask by revising the underlying assumptions concerning the approach to reading the historical record. Rather than reading history backwards and imposing current ideas and values on the past, the texts and documents are read within their historical period, thereby maintaining their integrity.

The historiographic revolution in the study of science's record had implications for how science is viewed and understood philosophically. The goal of *Structure*, declared Kuhn, was to cash out those implications. Kuhn reassured the reader's concerns that this project was not doomed to failure. Rather he contended that the application of principles, such as the distinction between the context of discovery and the context of justification from the traditional view of science to historical analysis

> 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.[3]

Kuhn was confident that the new historiography avoided the vicious circle foisted upon the traditional analysis of science and thereby produced a more apt depiction of science.

The structure of *Structure*

The first edition of *Structure* contained 13 chapters. *Structure*'s overall organization is as follows. In the Preface, Kuhn details the origins of the book from an autobiographical perspective. The first chapter contains an *apologia* for the role of history in reshaping the traditional view of science. The remaining 12 chapters may be arranged into three sections: the transition from pre-paradigm science to normal science, which covers chapters 2 through 5; the transition from normal science to extraordinary science, which covers chapters 6 through 8; and the transition from extraordinary science to new normal science, which covers chapters 9 through 13. As noted previously a "Postscript," in which Kuhn responds to critics, was added to the second edition.[4]

The structure of *Structure* may be illustrated schematically, as follows: pre-paradigm science → normal science → extraordinary science → new normal science. The step from pre-paradigm science to normal science involves the convergence of community consensus around a single paradigm, where there was no prior consensus. This is the pattern for immature science as it enters into the mature science pattern. The step from normal science to extraordinary science includes the community's recognition that the reigning paradigm is unable to account for accumulating anomalies. A crisis ensues within the community and from it extraordinary science emerges, in which community members search for a resolution to its paradigm problems. This is the step at which a scientific revolution occurs. Once a community selects a new paradigm, the old one is discarded and another period of new normal science follows. The revolution or paradigm shift is now complete and the whole cycle from normal science to new normal science through revolution is free to occur again. According to Kuhn, "the successive transition from one paradigm to another via revolution is the usual developmental pattern of mature science."[5] Finally, this schematic represents the dynamism involved in the process of science, as Kuhn envisioned it.[6]

From pre-paradigm science to normal science

Paradigms

The notion of paradigm loomed large in Kuhn's new image of science. He defined paradigm not only in terms of the community's concrete achievements but also in terms of its "accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation."[7] A paradigm is certainly not just a set of rules or an algorithm by which science is practiced blindly. In fact, there is no easy way to abstract a paradigm's essence or to define its features exhaustively. Rather, a paradigm is a concrete instance of a significant scientific accomplishment, such as Newtonian mechanics, which the professional community can easily recognize or identify but cannot fully interpret or explain. But Kuhn maintained: "Lack of standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research."[8] A paradigm defines the family resemblance, *à la* Wittgenstein, of problems and procedures for solving problems that are part of a single research tradition.

Although rules are at times needed for guiding research, they do not precede paradigms. "Paradigms," asserted Kuhn, "may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them."[9] Importantly, Kuhn was not claiming that rules are unnecessary for guiding research but rather that they are not always sufficient, either pedagogically or professionally. Kuhn compared paradigms to Polanyi's notion of "tacit knowledge," in which knowledge production depends on the investigator's acquisition of skills that cannot be reduced to methodological rules.

The nature of paradigms influences their transmission, especially pedagogically. Students are not taught paradigms in the abstract but always in application to solved problems.

> As the student proceeds from his freshman course to and through his doctoral dissertation, the problems assigned to him become more complex and less completely precedented. But they continue to be closely modeled on previous achievements as are the problems that normally occupy him during his subsequent independent scientific career.[10]

A paradigm is akin to an accepted pattern or model of activity, "particularly of grammatical models of the right way to do things."[11] But, whereas the grammatical paradigm involves replication of a pattern, such as verb conjugation or noun declension, the scientific paradigm "is rarely an object of replication. Instead," continued Kuhn, "like an accepted judicial decision in the common law, it is an object for further articulation and specification under new and more stringent conditions."[12]

A paradigm allows scientists to ignore concerns over the discipline's fundamentals and to concentrate on solving the problems at hand. Importantly, paradigms not only guide scientists in terms of identifying problems that are soluble but they also prevent scientists from tackling problems that are insoluble. Kuhn compared paradigms to maps that guide and direct the community's investigations. But more importantly "paradigms provide scientists not only with a map but also with some of the directions essential for map-making."[13] Only when a paradigm guides the community's activities is scientific advancement and progress possible.

Pre-paradigm science

"History suggests," claimed Kuhn, "that the road to a firm research consensus is extremely arduous."[14] That road begins for a scientific discipline, with the identification of a natural phenomenon that is then investigated experimentally and explained theoretically. But each member of that nascent discipline is at cross purposes with each other; for each member often represents a school working from different foundations. Scientists, operating under these conditions, share no theoretical concepts, experimental techniques, or phenomenal entities. Rather, each school is in competition for monetary and social resources and for the allegiance of the professional guild. An outcome of this lack of consensus is that all facts seem equally relevant to the problem at hand and fact gathering itself is often a random activity. There is then a proliferation of facts and hence little progress in solving problems under these conditions, because of the competition among the various schools. The overall result of this situation, insisted Kuhn, appears to be "something less than science."[15]

Kuhn called this state of affairs pre-paradigm (or immature) science. In other words, there is no single paradigm that defines the discipline and dictates its practices. Pre-paradigm science is non-directed and flexible, offering a community of practitioners little guidance. Kuhn illustrated this pre-paradigm pattern with physical optics prior to Newton.

> Being able to take no common body of belief for granted, each writer on physical optics felt forced to build his field anew from its foundations. In doing so, his choice of supporting observation and experiment was relatively free, for there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. Under these circumstances, the dialogue of the resulting books was often directed as much to the members of other schools as it was to nature.[16]

The benefits of a single paradigm are critical for scientific practice and the advance of science. "When the individual scientist," asserted Kuhn, "can take a paradigm for granted, he need no longer, in his major works, attempt to build his field anew, starting from first principles and justifying the use of each concept."[17] The transition from pre-paradigm science to normal science is a one time deal, after which mature science cycles from normal science to a new normal science through revolution or paradigm shift. Lastly, the acquisition of a paradigm is Kuhn's demarcation principle: "it is hard to find another criterion that so clearly proclaims a field a science."[18]

Normal science

To achieve the status of a science, a discipline must reach consensus with respect to a single paradigm. That transition is realized when, during the competition involved in pre-paradigm science, one school makes a stunning achievement that catches the professional community's attention. The achievement must exhibit two characteristics to affect the transition. First, the "achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity."[19] Second, "it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to solve."[20] "To be accepted as a paradigm," claimed Kuhn, "a theory must seem better than its competitors, but it need not, and in fact never does, explain all the facts with which it can be confronted."[21] By the term better he meant that the candidate for paradigm status does a far more effective and efficient job in determining the problems worth solving. The candidate paradigm, then, elicits the community's confidence that the problems are solvable with precision and in detail. "Paradigms gain their status," explained Kuhn, "because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute."[22] The community's confidence in a paradigm is based on the "conversion" of its members, who are now committed to the paradigm.[23]

Once consensus is achieved, Kuhn claimed that scientists are now in the position to commence with the practice of normal science, which is "research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice."[24] The prerequisites of normal science include a commitment to a shared paradigm that defines the rules and standards by which science is practiced. Whereas pre-paradigm science is non-directed and flexible, normal or paradigm science is highly directed and rigid. Because of that directedness and rigidity, normal scientists are able to make the strides they do, because

> those restrictions born from confidence in the paradigm, turn out to be essential to the development of science. By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable.[25]

The activity of practitioners engaged in normal science is paradigm articulation or extension to new areas. When a new paradigm is established, it solves only a few critical problems that faced the community. But it does offer the promise for solving many more problems.

> Normal science is the actualization of that promise, an actualization achieved 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 paradigms predictions, and by further articulation of the paradigm itself.[26]

Much of normal science involves "mopping-up" activities, in which nature is forced into a conceptually rigid framework—the paradigm. Rather than being dull and routine, however, such activity according to Kuhn is exciting and rewarding and requires practitioners who are creative and resourceful.

Normal scientists are not out to make new discoveries or to invent new theories, outside the paradigm's aegis. Rather, they are involved in using the paradigm to understand nature more precisely and in greater detail. From the experimental end of this task, normal scientists go to great pains to increase the precision and reliability of their measurements and facts. They are also involved in closing the gap between observations and theoretical predictions and attempt to resolve ambiguities left over from the paradigm's initial adoption. They also strive to extend the scope of the paradigm by including phenomena not heretofore investigated. Much of this activity requires exploratory investigation, in which novel discoveries can be made but not unanticipated vis-à-vis the paradigm. To solve these experimental problems often requires considerable technological ingenuity and innovation on the part of normal scientists.

Besides the experimental problems, there are also the theoretical problems of normal science, which obviously mirror the types of experimental problems. Normal scientists conduct theoretical analyses to enhance the match between theoretical predictions and experimental observations, especially in terms of increasing the paradigm's precision and scope. "The need for work of this sort," observed Kuhn, "arises from the immense difficulties often encountered in developing points of contact between theory and nature."[27] Again, just as experimental ingenuity is required, so theoretical ingenuity is needed to address these problems successfully. Importantly, Kuhn rejected the distinction between the rational and the empirical since "the problems of paradigm articulation are simultaneously theoretical and experimental."[28]

Kuhn addressed an important motivational question concerning the normal scientist: "if the aim of normal science is not major substantive novelties—if failure to come near the anticipated result is usually failure as a scientist—then why are these problems undertaken at all?"[29] Although paradigm articulation—enhancing a paradigm's precision and scope—is an important part of the answer, it cannot account for the scientist's enthusiasm for the seemingly routine tasks of mopping up after a revolution. That enthusiasm, claimed Kuhn, is a result not of the anticipated result but of the path by which it is attained. The real excitement in normal science is the way in which a paradigm is articulated.

> Bringing a normal science problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself to be an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on.[30]

Normal science then is puzzle-solving activity, and its practitioners are puzzle solvers and not paradigm testers. By puzzle, Kuhn meant the "special category of problems that serve to test ingenuity or skill in solution."[31] The paradigm's power over the community of practitioners is that it can transform seemingly insoluble problems into puzzles that can then be solved by the practitioner's ingenuity and skill. Besides the assured solution, Kuhn's notion of puzzle also involved the "rules that limit both the nature of the acceptable solution and the steps by which they are to be obtained."[32] Kuhn used rule in a broad sense of the term to indicate "established viewpoint" or "preconception." Rules are often laws or theories, but they can also originate from instrumental preferences. Besides these rules of the game, as it were, others may be obtained from metaphysical commitments, which inform the community as to the types of natural entities, and methodological commitments, which inform the community as to kinds of laws and explanations. Although rules are often necessary for normal scientific research, they are not always necessary. Normal science can proceed in the absence of such rules.

From normal science to extraordinary science

Anomaly

Although scientists engaged in normal science do not intentionally attempt to make unexpected discoveries, such discoveries do occur. Their paradigms are imperfect and rifts in the match between paradigm and nature are inevitable: "to be admirably successful is never, for a scientific theory, to be completely successful."[33] For Kuhn, discoveries not only occur in terms of new facts but there are also inventions in terms of novel theories. Both discovery of new facts and invention of novel theories begin with anomalies, "with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science."[34] Anomalies, then, are violations of paradigm expectations during the practice of normal science and can lead to unexpected discoveries. It must be noted that the detection of anomalies can only occur due to the background provided by a paradigm.

For Kuhn, unexpected discovery is a complex process that includes the intertwining of both new facts and novel theories. Facts and theories go hand-in-hand, for such a discovery cannot be made by simple inspection: "discovering a new sort of phenomenon is necessarily a complex event, one which involves recognition both *that* something is and *what* it is."[35] Because a discovery depends upon the intertwining of observation and theory, the discovery process then takes time for the novel to be conceptually integrated with what is known. Moreover, that process is complicated by the fact that novelties are often resisted because of prior expectations. Because of allegiance to a paradigm, scientists are loath to abandon it simply because of an anomaly or even several anomalies. In other words, anomalies are not considered counter-instances and they certainly do not falsify a paradigm.

Crisis

Just as anomalies are critical for the discovery of new facts and phenomena, so they are essential for the invention of novel theories. Although facts and theories are intertwined, the emergence of novel theories is the result of a crisis, "a period of pronounced professional insecurity."[36] The insecurity is the result of the paradigm's breakdown or inability to provide a solution to a puzzle or solutions to several puzzles. The community then begins to harbor questions about the ability of the paradigm to guide research, which has a profound impact upon the community. The chief characteristic of a crisis is the proliferation of theories. As members of a community in crisis attempt to resolve the anomalies, they offer more and varied theories to solve the problems. Interestingly, the problems that are responsible for the anomalous data are not necessarily new problems that arose after consensus but may have been present all along. This helps to explain why the anomalies lead to a period of crisis in the first place. The paradigm promised resolution of the problems but was unable to fulfill its promise. The overall effect is a return to a situation very similar to pre-paradigm science.

The proliferation of theories during crisis has an important philosophical implication for the underdetermination thesis. According to this thesis, there are always many potential theories that can be proposed to account for a set of data. In other words, the data cannot be used to determine which theory to choose. Kuhn agreed, in part, with this thesis but he limited it to periods of crisis and pre-paradigm science. Underdetermination is not a problem for those scientists engaged in normal scientific practice, because the community has reached consensus concerning its theoretical conceptions. It is this consensus that allows the community to make outstanding advances. To do otherwise, according to Kuhn, would be counterproductive and inefficient. "As in manufacture," observed Kuhn, "so in science—retooling is an extravagance to be reserved for the occasion that demands it."[37] Later, he admitted that he found the weak form of underdetermination thesis defensible but not the strong form, which states that "even with *all possible evidence*, the theories would still be underdetermined."[38]

Closure of a crisis occurs in one of three possible ways, according to Kuhn. First, on occasion the paradigm is sufficiently robust to resolve the anomaly and to restore normal science practice. Second, the anomaly is not resolved even by the most radical method. Under these circumstances, the community tables the anomaly until future investigation and analysis. Third, the crisis is resolved with the replacement of the old paradigm by a new one but only after a period of extraordinary science.

Extraordinary science

Kuhn stressed that the initial response of a community in crisis is not to abandon its paradigm. Rather, its members make every effort to salvage it using ad hoc modifications until the anomalies can be resolved, either theoretically or experimentally. The reason for this strong allegiance to a paradigm, claimed Kuhn, is that a community must first have an alternative candidate to take the original paradigm's place. For science, at least normal science, is possible only with a paradigm and to reject a paradigm without a substitute is to reject science itself, which reflects poorly on the community and not on the paradigm. Moreover, paradigms are not simply rejected because of a rift in the paradigm–nature fit. Kuhn insisted that he is not saying that nature does not play a role in paradigm decision during a crisis, but "that decision involves the comparison of both paradigms with nature *and* with each other."[39] His position was not relativistic outside the paradigm box.

Kuhn's aim here was to reject a naive Popperian falsificationism. "No process yet disclosed by the historical study of scientific development," asserted Kuhn, "resembles at all the methodological stereotype of falsification by direct comparison with nature."[40] And Kuhn went on to claim that philosophers who subscribe to this theory of science will behave like scientists when confronted with anomalous facts.

> By themselves [falsificationists] cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by an anomaly. They will devise

> numerous and *ad hoc* modifications of their theory in order to eliminate any apparent conflict.[41]

His point here was not to downgrade science but to demonstrate the accuracy of his analysis of professional communities, either scientific or not, when confronted by falsifying evidence. In fact, Kuhn reversed the tables and contended that counter-instances are essential for the practice of vibrant normal science. If there are no counter-instances scientific development comes to a halt for the discipline, which may become an engineering tool. Counter-instances or anomalies are the puzzles of normal science.

But if anomalies are the puzzles of normal science, how then can they lead to a crisis? Also, why would a community explore anomalies when they may portend disaster for the paradigm? Kuhn claimed that there is probably no general answer to these questions, but there are reasons for explaining the transition of a puzzle into a crisis-producing anomaly. First, a recalcitrant puzzle may raise questions about the discipline's foundations. Second, an unsolved puzzle "without apparent fundamental importance may evoke a crisis if the applications that it inhibits have a particular practical importance," such as calendar design in the Copernican revolution.[42] Finally, the sheer length of time a community struggles with a puzzle may suffice to transform it into an anomaly. "When, for these reasons and others like them," concluded Kuhn, "an anomaly comes to seem more than just another puzzle of normal science, the transition to crisis and to extraordinary science has begun."[43]

The transition from normal science through crisis to extraordinary science involves two key events. First, the paradigm's boundaries become blurred when faced with recalcitrant anomalies; and, second, its rules are relaxed, leading to a proliferation of theories and ultimately to the emergence of a new paradigm. Often the relaxing of the rules allows the practitioners to see exactly where the problem is and how to go about solving it. This state of affairs has a tremendous impact upon the community's practitioners, similar to that during pre-paradigm science. An extraordinary scientist, according to Kuhn, is a person

> searching at random, trying experiments just to see what will happen, looking for an effect whose nature he cannot quite guess. Simultaneously, since no experiment can be conceived without some sort of theory, the scientist in crisis will constantly try to generate speculative theories that, if successful, may disclose the road to a new paradigm and, if unsuccessful, can be surrendered with relative ease.[44]

The reason for this erratic behavior is that scientists are trained under a paradigm to be puzzle solvers not paradigm testers. In other words, scientists are not trained to do extraordinary science and must learn as they go. For Kuhn, this type of behavior is more open to psychological than logical analysis. Moreover, during periods of extraordinary science practitioners may even examine the philosophical foundations of their discipline. To that end, they analyze their assumptions, in order to loosen the old paradigm's grip on the community and to suggest alternative approaches to the generation of a new paradigm.

Although the process of extraordinary science is convoluted and complex, a replacement paradigm may "emerge all at once, sometimes in the middle of the night, in the mind of the man deeply immersed in crisis."[45] Often the source of that inspiration is rooted in the practice of extraordinary science itself, in terms of the interconnections among various anomalies. Finally, whereas normal science is a cumulative process, adding one paradigm achievement to the next, extraordinary science is not; rather, "it is a reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications."[46] Quoting Herbert Butterfield, Kuhn claimed that the scientist who experiences a change in paradigms is like a person "picking up the other end of the stick."[47] That other end of the stick represents a scientific revolution.

From extraordinary science to new normal science

Scientific revolutions

The transition from extraordinary science to a new normal science is through scientific revolution. According to Kuhn, scientific revolutions are "non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one."[48] They can come in two sizes: major revolutions such as the shift from geocentric universe to heliocentric universe or minor revolutions such as the discovery of X-rays or oxygen. But whether big or small, all revolutions have the same structure: generation of a crisis through irresolvable anomalies and establishment of a new paradigm that resolves the crisis-producing anomalies.

Metaphorically, scientific revolutions are comparable to political revolutions. Just as a segment of a country's populace believes that the ruling government is unable to solve the pressing social and political problems, so a segment of a scientific community's practitioners believes that the ruling paradigm is unable to solve the crisis-producing anomalies. In both cases action must be taken to resolve the problem(s). But because of the extreme positions taken by the participants in a revolution, opposing camps become galvanized in their positions and communication between them breaks down. And just as political recourse fails, so does scientific recourse. Kuhn described the situation accordingly:

> Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force.[49]

Whereas for political revolutions force may be physical, for scientific revolutions it is circular in which supporters of a particular paradigm use that paradigm to defend it.

> The man who premises a paradigm when arguing in its defense can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. That exhibit can be immensely persuasive, often compellingly so. Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistic compelling for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that.[50]

The ultimate source for the establishment of a new paradigm during a crisis period is community consensus, i.e. when enough community members are persuaded by the techniques of the argument and not simply by empirical evidence or logical analysis. Moreover, to accept the new paradigm, a community practitioner must be convinced that there is no chance for the old paradigm ever to solve the anomalies.

Why persuasion loomed large in Kuhn's scientific revolutions was that the new paradigm solves the anomalies the old paradigm could not. Thus, the two paradigms are radically different from each other, often with little overlap between them. Kuhn's position was in counter-distinction to the logical positivists and their followers, who "restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about the some of the same natural phenomena."[51] For Kuhn, the new theory can only be accepted if the community considers the old theory wrong. Kuhn defended his position against criticism that an older theory is simply a special case of a newer theory, under specific conditions. The problem with this criticism, stated Kuhn, is that

> to save theories in this way, their range of application must be restricted to those phenomena and to that precision of observation with which the experimental evidence in hand already deals . . . such limitation prohibits the scientist from claiming to speak "scientifically" about any phenomenon not already observed.[52]

In other words, logical positivist cut off any further scientific development since anomalies would be methodologically prohibited. Moreover, the new theory resolves the anomalies that the old theory cannot but which it gave rise to.

The radical difference between the old and new paradigms, such that the old cannot be derived from the new, is the basis for the incommensurability thesis. The origin of the thesis, according to Kuhn, dates to his high-school days. He was given a two-volume calculus book, which laid out a proof for the irrationality of the square root of two. Kuhn took away from this early experience a meaning of incommensurability that he used later as a metaphor in terms of the fundamental incompatibility between two competing paradigms. In essence, there is no common measure or standard for the two paradigms. This is evident, claimed Kuhn, when looking at the meaning of theoretical terms. Although the terms from an older paradigm can be compared to those of a newer one, the older terms must be transformed vis-à-vis the newer ones. But there is a serious problem with restating the old paradigm in transformed terms. The revised paradigm may have some utility, for example pedagogically, but it could not be used to guide the community's research. The older paradigm is like a fossil; it reminds the community of its history but it can no longer direct its future.

An interesting feature of scientific revolutions, according to Kuhn, is their invisibility. What he meant by this is that in the process of writing textbooks, popular scientific essays, and even the philosophy of science, the path to the current paradigm is sanitized to make the current paradigm appear as if it was in some sense born mature. Disguising the paradigm's history is a product of a view of scientific knowledge, which sees it as complete and its accumulation as linear. This disguising serves the winner of the crisis by establishing its authority, especially as a pedagogical aid for indoctrinating students into the community of practitioners. But as Kuhn labored to demonstrate the growth of scientific knowledge is not the result of piecemeal changes to theories over time to fit the facts; rather, it is the result of the emergence of theories and facts together "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."[53]

Another important effect of a revolution, which is related to a paradigm shift, is a shift in the community's view of science: "the reception of a new paradigm often necessitates a redefinition of the corresponding science."[54] The change in the image of science should be no surprise, since the prevailing paradigm defines the nature of science. Change that paradigm and science itself changes, or at least how it is practiced. In other words, the shift in science's image is a result of a change in the community's standards for what constitutes its problems and its problems' solutions. Finally, revolutions transform scientists from practitioners of normal science, who are puzzle solvers, to practitioners of extraordinary science, who are paradigm testers. Besides transforming science, revolutions also transform the world that scientists investigate.

Changes of world view

One of the major impacts of a scientific revolution is a change to the world in which scientists practice their trade:

> paradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world.[55]

Kuhn's "world changes" thesis, as it has become known, is certainly one of his most radical and controversial ideas, besides the associated incommensurability thesis. The issue here is how far ontologically does the change go, or is it simply an epistemological ploy to reinforce the comprehensive effects of scientific revolutions. In other words, does the world really change or simply the world view, i.e. one's perspective of the world? For Kuhn, the answer relied not on a logical or even a philosophical but rather a psychological analysis of the change.

Kuhn analyzed the changes in world view by analogizing it to a gestalt switch. "What were ducks in the scientist's world before the revolution," noted Kuhn, "are rabbits afterwards."[56] Although the gestalt analogy is suggestive, it is limited to only perceptual changes and says little about function of previous experience in such transformations. Previous experience is important because it influences what a scientist sees when making an observation. Moreover, with a gestalt switch the person can stand above or outside of it acknowledging with certainty that now a duck is seen or now a rabbit is seen. Such an independent perspective, which eventually is an authoritarian stance, is not available to the community of practitioners; there is no answer sheet, as it were. "The scientist," claimed Kuhn, "can have no recourse above and beyond what he sees with his eyes and instruments."[57] Because the community's access to the world is limited to what is observed, any change in what is observed has important consequences for the nature of what is observed, i.e. the change has ontological significance.

Thus, for Kuhn, the change brought about by a revolution is more than simply seeing or observing a different world; it also involves living in a different world. The perceptual transformation is more than a reinterpretation of the data. "What occurs during a scientific revolution," asserted Kuhn, "is not fully reducible to a reinterpretation of individual and stable data."[58] The reason is that the data themselves are not really stable but also change during a paradigm shift. Data interpretation is a function of normal science, while data transformation is a function of extraordinary science. That transformation is often a result of intuitions that "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."[59]

A paradigm determines not only what laboratory protocols are practiced but also what observations are going to be made. Change the paradigm and not only are the laboratory protocols different but so are the observations. Hence, an observation is not so much "given" as it is "the collected with difficulty."[60] Moreover, besides a change in data, revolutions change the relationships among the data.

> 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 one to another.[61]

This change in the world also has important epistemological implications for Kuhn. Although traditional western philosophy has searched for three centuries for stable and theory-neutral data or observations to justify theories, that search has been in vain. Kuhn desired to cling to this position but lamented that "it no longer functions effectively, and the attempts to make it do so through the introduction of a neutral language of observations seem to me hopeless."[62] All sensory experience is through a paradigm of some sort, even articulations of that experience. Hence, no one can step outside the paradigm to make an observation; it is simply impossible given the limits of human physiology. "It is, however, only after experience has been determined," claimed Kuhn, "that the search for an operational definition or a pure observation-language can begin."[63] But, he cautioned, only with the recognition that sensory experience is fundamentally paradigm-determined.

Resolution of revolutions

The establishment of a new paradigm resolves a scientific revolution and issues forth a new period of normal science. With its establishment, Kuhn's new image of a mature science came full circle. Only after a period of intense competition among rival paradigms, does the community choose a new paradigm and scientists are transformed from paradigm testers to puzzle solvers. The resolution of a scientific revolution is not a straightforward process that depends only upon reason or evidence. "The competition between paradigms," contended Kuhn, "is not the sort of battle that can be resolved by proofs."[64] Part of the problem is that proponents of competing paradigms cannot agree on the relevant evidence or proof or even on the relevant anomalies needed to be solved, since their paradigms are incommensurable.

Another factor that leads to difficulties in resolving scientific revolutions is that communication among members in crisis is only partial. This is the result of the new paradigm's theoretical terms and concepts and laboratory protocols being initially borrowed from the old paradigm. Although they share the same vocabulary and technology, the new paradigm gives new meaning and uses to them. Remember that members of each competing paradigm live in a different world from their competitors. The net result is that members of competing paradigms talk past one another. Moreover, the change in paradigms is not a gradual process in which different parts of the paradigm are changed piecemeal; rather, the change must be as a whole and suddenly. Convincing scientists to make such a wholesale transformation takes time.

How then does one segment of the community convince another to switch paradigms? For members who worked for decades under the old paradigm, they may never accept the new paradigm. The resistance of these mature members to the new paradigm "is not a violation of scientific standards but an index to the nature of scientific research itself . . . the assurance that the older paradigm will ultimately solve all its problems, that nature can be shoved into the box the paradigm provides."[65] Rather, it is often the younger members who accept the new paradigm through something like "a conversion experience that cannot be forced."[66] The conversion is based rather on faith, especially in the potential of the new paradigm to solve future problems.

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing that the older paradigm has failed with a few.[67]

By invoking the terms conversion and faith, Kuhn was not implying that arguments and reason are unimportant in a paradigm shift. Indeed, the most common reason for accepting a new paradigm is that it solves the anomalies the old paradigm could not. However, Kuhn pointed out that the "claim to have solved the crisis-provoking problems is . . . rarely sufficient by itself."[68] Aesthetic or subjective factors also play an important role in a paradigm shift, since the new paradigm solves only a few but critical anomalies. These factors weigh heavily initially in the shift by reassuring the community's members that the new paradigm represents their discipline's future. But Kuhn denied he was suggesting that "new paradigms triumph ultimately through some mystical aesthetic. On the contrary, very few men desert a tradition for these reasons alone."[69]

From the resolution of revolutions, Kuhn made several important philosophical points concerning the principles of verification and falsification. As Kuhn acknowledged, philosophers do not search for absolute verification anymore, since no theory can be exhaustively tested; rather, they calculate the probability of a theory's verification. According to probabilistic verification, all imaginable theories must be compared with one another vis-à-vis the available data. The problem in terms of Kuhn's new image of science is that theories are tested with respect to a given paradigm, and such a restriction precludes access to all imaginable theories. Moreover, Kuhn rejected falsifying instances because no paradigm resolves all the problems facing a community. Under these conditions, no paradigm would ever be accepted. For Kuhn, the process of verification and falsification must include the vagueness that accompanies theory–fact fit. "There is," stated Kuhn, "no more precise answer to the question whether or how well an individual theory fits the facts."[70]

Progress through revolutions

First Kuhn discussed the progress in normal science, to contrast it with progress through revolutions. "In its normal state," observed Kuhn, "a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigm defines. Furthermore, the result of solving these problems must inevitably be progress."[71] For normal science, progress is cumulative in that the solutions to puzzles form a repository of information and knowledge about the world. This progress is the result of the direction a paradigm gives to the community's practitioners. Importantly, the progress achieved through normal science in terms of the information and knowledge is used to educate the next generation of scientists and to manipulate the world for human welfare. Scientific revolutions are going to change all that.

What, then, does the community of practitioners gain, vis-à-vis the progress of normal science, by going through a revolution or paradigm shift? Has it made any kind of progress in its rejection of a previous paradigm and the fruit that paradigm has borne? Of course, the victors of the revolution are going to claim that progress is made after the revolution. To do otherwise would be to admit that they were wrong all along. Rather the framers of the new normal science are going to do all they can to ensure that their winning paradigm is seen as pushing forward a better understanding of the world. The progress achieved through a revolution is two-fold, according to Kuhn. The first is the successful solution of anomalies that a previous paradigm could not solve. The second is the promise to solve additional problems or puzzles that arise from these anomalies: "a community of scientific specialists will do all it can to ensure the continuing growth of the assembled data that it can treat with precision and detail."[72] Although progress involves the solution of these newer problems, it also consists of maintaining "a relatively large part of the concrete problem solving ability that has accrued to science through its predecessors."[73] However, argued Kuhn, revolutionary progress is not cumulative but non-cumulative.

But has the community gotten closer to the truth, i.e. Popper's notion of verisimilitude, by going through a revolution? According to Kuhn the answer is no. "We may . . . have to relinquish the notion, explicit or implicit, that changes of paradigms carry scientists and those who learn from them closer and closer to the truth."[74] For Kuhn, the progress of science is not a directed activity toward some goal like the truth. Rather, scientific progress is a

> developmental process . . . a process of evolution *from* primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything.[75]

Just as selection operates in the emergence of new species, so it functions in the emergence of new theories. And, just as a species is adapted to its environment so a theory is adapted to the world. Kuhn had no answer to the question why this should be other than the world and the community that studies it exhibit "special characteristics."[76] What these characteristics are, Kuhn did not know, but he concluded that the new image of science he had proposed would, like a new paradigm after a scientific revolution, resolve these problems. He invited the next generation of philosophers of science to join him in a new philosophy of science incommensurate with its predecessor.

Notes

1. Kuhn (1964), p. 1. The first two sentences of the 1960 *Structure* draft read:
    > The study of history has not been a usual source for the West's conception of science, and it might usefully become one. Viewed as a repository for more than anecdote or chronology, history could provide a decisive transformation in the image of science by which we are now possessed. (MIT MC 240, box 4, folder 5, "Draft, 1958–60," p. 1)
2. Kuhn (1964), p. 3.
3. *Ibid*, 9.
4. A two-page index was added to a third edition, published in 1996.
5. Kuhn (1964), p. 12.
6. For the standard philosophical reconstruction of *Structure*, see Hoyningen-Huene (1993).
7. Kuhn (1964), p. 10.
8. *Ibid*, p. 44.
9. *Ibid*, p. 46.
10. *Ibid*, p. 47.
11. Kuhn (2000), p. 298.
12. Kuhn (1964), p. 23.
13. *Ibid*, p. 109.
14. *Ibid*, p. 15.
15. *Ibid*, p. 13.
16. *Ibid*.
17. *Ibid*, pp. 19–20.
18. *Ibid*, p. 22.
19. *Ibid*, p. 10.

20. *Ibid.*
21. *Ibid*, pp. 17–18.
22. *Ibid*, p. 23.
23. *Ibid*, pp. 18–19.
24. *Ibid*, p. 10.
25. *Ibid*, p. 24.
26. *Ibid.*
27. *Ibid*, p. 30.
28. *Ibid*, p. 33.
29. *Ibid*, p. 36.
30. *Ibid.*
31. *Ibid.*
32. *Ibid*, p. 38.
33. *Ibid*, p. 68.
34. *Ibid*, p. 53.
35. *Ibid*, p. 55.
36. *Ibid*, pp. 67–8.
37. *Ibid*, p. 76.
38. Sigurdsson (1990), 22.
39. Kuhn (1964), p. 77.
40. *Ibid.*
41. *Ibid*, p. 78.
42. *Ibid*, p. 82.
43. *Ibid.*
44. *Ibid*, p. 87.
45. *Ibid*, p. 90.
46. *Ibid*, p. 85.
47. *Ibid.*
48. *Ibid*, p. 92.
49. *Ibid*, p. 93.
50. *Ibid*, p. 94.
51. *Ibid*, p. 98.
52. *Ibid*, p. 100.
53. *Ibid*, p. 141.
54. *Ibid*, p. 103.
55. *Ibid*, p. 111.
56. *Ibid.*
57. *Ibid*, p. 114.
58. *Ibid*, p. 121.
59. *Ibid*, p. 123.
60. *Ibid*, p. 126.
61. *Ibid*, p. 150.
62. *Ibid*, p. 126.
63. *Ibid*, p. 129.

64. *Ibid*, p. 148.
65. *Ibid*, pp. 151–2.
66. *Ibid*, p. 151.
67. *Ibid*, p. 158.
68. *Ibid*, p. 154.
69. *Ibid*, p. 158.
70. *Ibid*, p. 147.
71. *Ibid*, p. 166.
72. *Ibid*, pp. 169–70.
73. *Ibid*, p. 169.
74. *Ibid*, p. 170.
75. *Ibid*, pp. 170–1.
76. *Ibid*, p. 173.

Chapter 4

Why does Kuhn revise *Structure*?

Reactions to *Structure*

The reaction to *Structure* was at first congenial but within a few years turned critical, especially that of philosophers. For example, Dudley Shapere's review of *Structure* was especially critical of Kuhn's new image of science. He followed the review a few years later with another critical evaluation, in a paper delivered at the Pittsburgh philosophy of science colloquium series.[1] Kuhn's philosophy of science also became the focus of other critical reviews. For example, Israel Scheffler criticized Kuhn's philosophy of science as subverting scientific objectivity.[2] However, Kuhn's severest criticism came from a 1965 philosophy of science colloquium held in London, with Popper as chair. Kuhn gave a paper comparing his and Popper's views of the growth of scientific knowledge, which was then followed by critical papers delivered by Popper and others. The chief criticisms focused on the notions of paradigm and normal science. Kuhn responded to his London critics and to others in an unpublished 1967 Swarthmore lecture, in a published 1969 Urbana paper, and in a postscript to the second edition of *Structure*.

From Kuhn's recollection, he felt that the reviews of *Structure* were good.[3] His chief concern was the tag of irrationalism. "I was not saying, however," stated Kuhn later, "that there aren't good reasons in scientific proofs, good but never conclusive reasons."[4] He was also concerned with the charge of relativism, at least a pernicious kind. He felt that the charge was inaccurate. He proposed that science does not progress toward a predetermined goal but, like evolutionary change, one theory replaces another with a better fit between theory and nature vis-à-vis competitors. Kuhn believed that his use of the Darwinian metaphor was the correct framework for discussing science's progress. But he felt no one took that metaphor seriously.

Reviews

In 1962 and 1963, several dozen reviews of *Structure* appeared in a variety of professional journals and were, for the most part, favorable to Kuhn's new image of science. One of the first reviewers was his former chair at Princeton, Charles Gillispie. Gillispie acknowledged that Kuhn wrote not a typical history of science text but one that offers a new image of science drawn from historical, philosophical, psychological, sociological, and scientific sources. Moreover, rather than being a traditional philosophy of science text "in the usual Anglo-American sense of a study of logical problems found in scientific proceedings or systems . . . [it is] a sketch for a genetic philosophy of science."[5] Although Gillispie was sympathetic to Kuhn's new image of science, he charged Kuhn with circular definitions of terms like paradigm and normal science, i.e. paradigms determine normal science and normal science determines paradigms. The concern over circularity in *Structure* was shared by several reviewers. However, Gillispie commended Kuhn for situating scientific development in a Darwinian historiographic framework.

Many reviewers focused on segments of *Structure* that overlapped with their own discipline—a focus not to be unexpected. For example, sociologists recognized *Structure* as a contribution to the sociology of knowledge, which Kuhn claimed for the book itself. Bernard Barber lauded Kuhn's attempt to present a "sociology of scientific discovery," but branded Kuhn's attempt as being "quasi-sociological" because the "sociological analysis of the process of scientific discovery was not as theoretically explicit as we might wish it, nor does it include some sociological factors that would improve his analysis by enlarging it."[6] In another example, Edwin Boring evaluated the historical development of psychology in terms of paradigm shifts, lamenting, however: "Psychology has had as yet no big revolution, and perhaps that is why it seems to have had no Great Men."[7]

Many of the reviewers were not extensively critical in their analysis of Kuhn's new image of science. However, Derek de Solla Price presaged that "we must expect a considerable polemic to develop from this classic."[8] That polemic was on the immediate horizon. Beginning in 1964, critical reviews of *Structure* began to appear and, more importantly, several full-length reviews appeared in professional philosophy and history journals. The most influential review among philosophers was Shapere's poignant 1964 review. Shapere conceded that Kuhn convincingly demonstrated the problems associated with a philosophy of science based on a development-by-accumulation history of science. He situated Kuhn with other antipositivist philosophers of science, including Toulmin, Hanson, and Feyerabend.

However, he was concerned over certain issues, especially relativism, which arose from Kuhn's notions of paradigm and incommensurability.

Shapere detailed a number of problems with Kuhn's paradigm notion. First, the notion was too imprecise: "anything that allows science to accomplish anything can be part of (or somehow involved in) a paradigm."[9] Second, Shapere was concerned about the tail (paradigm) wagging the dog (Kuhn's analysis of science), in that Kuhn's enthusiasm for paradigm was "too strongly and confidently held to have been extracted from a mere investigation of how things *have* happened."[10] Shapere was also perplexed over the fact that although scientists cannot articulate paradigms satisfactorily, historians of science can recognize them by direct inspection of the historical record. In contrast to Kuhn, Shapere then argued that the distinction between "paradigms and different articulations of a paradigm, and between scientific revolutions and normal science, is at best a matter of degree, as is commitment to a paradigm."[11] Moreover, Shapere noted that the reasons Kuhn gave for the existence of paradigms, such as the inability to identify accurately methodological rules,

> do not compel us to adopt a mystique regarding a single paradigm which guides procedures, any more than our inability to give a single, simple definition of "game" means that we must have a unitary inexpressible idea from which all our diverse uses of "game" are abstracted.[12]

Finally, Shapere expressed concern that the expansive nature of paradigm may obscure significant divergence among scientific practices.

Shapere then discussed what he considers a "deeper" problem with Kuhn's notion of paradigm, the change in meaning of terms during paradigm shift. Shapere was bothered by Kuhn's argument that a fundamental change of a term's meaning, such as the term "mass," occurs after a scientific revolution (from Newton to Einstein).

> The real trouble with such arguments arises with regard to the cash difference between the saying, in such cases, that the "meaning" has changed, as opposed to saying that the "meaning" has remained the same though the "application" has changed.[13]

Shapere believed Kuhn failed to make this subtle distinction. The problem led to Shapere's critique of incommensurability. If two competing paradigms are incommensurable,

> if they disagree as to what the facts are, and even as to the real problems to be faced and the standards which a successful theory must meet—then what are the two paradigms disagreeing about? And why does one win?[14]

Shapere believed Kuhn had no ready answer for these questions. Moreover, he argued that Kuhn's incommensurability thesis reduces scientific progress to mere change, which raises the issue of how paradigms can be compared in the first place.

The upshot of Shapere's critique of both the paradigm and incommensurability notions was the charge that Kuhn's new image of science is relativistic. "For Kuhn," claimed Shapere, "has already told us that the decision of a scientific group to adopt a new paradigm is not based on good reason; on the contrary, what counts as a good reason is determined by the decision."[15] Shapere acknowledged that the appearance of this type of relativism in philosophy of science was only a matter of time, given the direction of current historiography, and warned philosophers of science to cast a jaundice eye toward it, "until historians of science achieve a more balanced approach to their subject—neither too positivistic nor too relativistic."[16]

Not only was *Structure* reviewed for professionals in the academic literature but it was also reviewed for the public in the popular literature. For the popular scientific literature, Kuhn's book received a particularly harsh review in *Scientific American.* The anonymous reviewer claimed that Kuhn's central thesis was common knowledge and that Kuhn distorted this thesis with his "relativism." The reviewer also criticized Kuhn's use of paradigm, as did many reviewers, and claimed that the effects of incommensurability "are at best wild exaggerations."[17] The review concluded with the statement that *Structure* was "much ado about very little."[18] Kuhn never quite forgot the treatment he received in the pages of this magazine.

Structure was also reviewed in *The Nation,* along with another dozen books on various topics related to science. The reviewer Philip Siekevitz focused on the community structure of Kuhn's new image of science, especially a structure that provides for open discussion and debate of issues germane to scientific advance. Siekevitz linked Kuhn's analysis of the scientific community's paradigmatic structure with Gerard Piel's analysis of democracy and science in *Science in the Cause of Man*: "just as science is based on the 'paradigm' subject to change, so is our democracy based on the Common Law, subject to constant reinterpretation."[19] For Siekevitz, the issue was how to motivate the public to read the popular science genre in order to be better-informed citizens in a world increasingly dependent on science.

Letters

After the publication of *Structure*, Kuhn received over his career hundreds of letters. The authors were mostly supportive of the paradigm notion and solicited assistance from Kuhn in applying the notion to their particular project or discipline. One of the earlier requests, for example, was from R. A. McConnell, who wrote Kuhn to request help with a book proposal on parapsychology.[20] According to McConnell, "Kuhn's discussion creates a framework in which there is a place for parapsychology as a characteristically scientific undertaking."[21] Moreover, McConnell recognized the importance of *Structure*: "The book represents a new perspective of science that may lead to a revolution in the historiography of science and that may in time deeply alter the education of scientists and the administration of scientific research."[22] In addition he sent Kuhn an abstract of *Structure* he had written and requested that Kuhn read it for faithfulness and accuracy to Kuhn's ideas. Kuhn responded with a detailed three-page letter encouraging McConnell with the book project and assuring him that the abstract of *Structure* was faithful and accurate.[23]

But at times Kuhn could not help the person seeking assistance. However, he still responded eventually. For example, in a letter, Ralph Anspach, then an assistant professor in economics at San Francisco State College, wrote Kuhn beseeching assistance with "an article on the methodology of economics in which I will draw from your analysis in the hope of bringing my profession up to date."[24] Kuhn wrote in return: "I am not going to be able to be very helpful even now. Almost none of the material that has appeared since my book was published is likely to be of any real use to you."[25]

Kuhn, however, did receive letters that were critical of *Structure*, especially in connection with the notion of paradigm. For example, Mendel Sachs, from the Department of Physics and Astronomy at the State University of New York, Buffalo, wrote Kuhn disagreeing with the role of paradigm in science. Defining paradigm as a "bandwagon," Sachs declared: "From my own experience, as a theoretical physicist, I am quite convinced that these bandwagons have been stumbling blocks to progress. They provide crutches for the individual scientists to lean on—instead of thinking for themselves!"[26] He went on to argue for an image of scientific practice similar to Feyerabend's. "I think," asserted Sachs, "that the healthiest thing for science itself is the approach of anarchy in research."[27]

Kuhn often responded to critics by trying to help them see through the differences between their criticisms and what Kuhn was trying to say. For example, in his reply to Sachs, Kuhn wrote: "how does one tell a 'bandwagon' effect from a decision made by large numbers of individuals to turn their attention to what they individually believe to be a promising new area of research?"[28] Kuhn continued to receive correspondence concerning *Structure*, and its revised edition, throughout his career and patiently addressed it as best he could. The sheer volume of this correspondence alone witnesses to the importance and impact his classic had, not only on the history and the philosophy of science but on many other disciplines as well.

1965 London colloquium: "Criticism and the growth of knowledge"

Kuhn's "Logic of discovery or psychology of research?"

The very title of Kuhn's paper invites comparison between Popper and Kuhn. Although Kuhn admitted there are similarities between him and Popper, there are fundamental differences between them analogous to a gestalt switch in which two people view the same picture but one sees one thing, the other another. "Sir Karl and I do appeal to the same data; to an uncommon extent we are seeing the same lines on the paper," but, argued Kuhn, "the figures which emerge from them are not [the same]."[29] The arduous task for Kuhn was to help Popper see what he sees when Kuhn looks at the historical record of scientific development. "How am I," queried Kuhn, "to show him what it would be like to wear my spectacles when he has already learned to look at everything I can point to through his own?"[30] Kuhn attempted to assist Popper in the gestalt switch by identifying Popperian locutions that Kuhn found unsuitable.

The first Popperian locution Kuhn tackles was "theory testing," i.e. scientists propose theories that are then tested experimentally. Kuhn argued that testing during normal science is not conducted to evaluate the correctness of a theory so much as to determine the normal scientist's ingenuity as puzzle solver. For Kuhn, Popper's theory testing is not normal but extraordinary scientific activity. "I suggest then," proposed Kuhn, "that Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary part."[31] Moreover, the demarcation between science and nonscience is not the potential refutability of theories. For example, astrology, according to Kuhn, is not a science not because it is not refutable but because it has no puzzles to solve. "To rely on testing as the mark of science," concluded Kuhn, "is to miss what scientists mostly do."[32]

The next Popperian locution Kuhn examines was "learning from our mistakes," i.e. the common method of trial and error or as Popper popularly referred to as "conjecture and refutation." Again, Kuhn claimed that the mistakes made by scientists are generally those made during the practice of normal science and involve the breaking of a paradigmatic rule. But the mistakes Popper refers to, according to Kuhn, are "out-of-date scientific theories" and so learning from mistakes "occurs when a scientific community rejects one of these theories and replaces it with another."[33] Again, Popper, as Kuhn sees it, has conflated normal and revolutionary science. "Like the term 'testing', 'mistake' has been borrowed," claimed Kuhn, "from normal science, where its use is reasonably clear, and applied to revolutionary episodes, where its application is problematic."[34]

Kuhn then proceeded to examine the most important Popperian locution, "falsification" or "refutation," i.e. when a theory fails a test it must be rejected. Kuhn acknowledged that Popper is not a naive falsificationist, in that a single observation does not necessarily falsify a theory because the observation can be questioned or the theory modified. However, he contended that Popper may "legitimately be treated as one," because the methodological bite of falsification is "conclusive disproof."[35] Kuhn's concern with Popper's notion of falsification was that logical analysis of theory testing cannot account completely for the development of scientific knowledge: "though logic is a powerful and ultimately an essential tool of scientific enquiry, one can have sound knowledge in forms to which logic can scarcely be applied."[36] For Kuhn, the notion of paradigm served to include the non-logical elements critical for the development of such knowledge.

Kuhn concluded with a discussion of questions concerning theory choice and scientific progress. He had no ready answers for them, but, just as a new paradigm in science offers promise for solving the anomalies an older paradigm could not, he believed that—in contradistinction to Popper's philosophy of science that has failed to answer them—he can "see the directions in which answers to them must be sought."[37] For example, the direction for answering questions about theory choice is to determine the values, such as simplicity and precision, which play a role in the process. "Knowing what scientists value," wrote Kuhn, "we may hope to understand what problems they will undertake and what choices they will make in particular circumstances of conflict."[38]

Finally, Kuhn invited Popper to join him in the quest for the directions to provide answers to the above questions. Kuhn believed that, although Popper appears to be interested strictly in the logical generation and to reject the individual's psychological role in the growth of knowledge, Popper does "inculcate moral imperatives in the membership of the scientific group."[39]

Critiques

Watkins rejected Kuhn's normal science as an accurate conception of science. He contrasted the two different communities of practitioners, with the Kuhnian community reflecting how scientists act as a "closed society, the intermittently shaken by collective nervous breakdowns followed by restored mental unison" and with the Popperian community reflecting how scientists should (and do) act as an "open society in which no theory . . . is ever sacred."[40] Watkin's strategy was to turn the table on Kuhn and to assist him to see through Popperian glasses that normal science is no science at all.

Watkins defended Popper's notion of critical science by distinguishing between a sociological and a methodological analysis of science. Kuhn's mistake, asserted Watkins, was to discount the revolutionary advances because they are rare and to promote normal science because it is common.

> From a sociological point of view, it may be quite in order to discount something on the ground that it is rare. But from the methodological point of view, something rare in science—a path-breaking new idea or a crucial experiment between two major theories—may be far more important than something going on all the time.[41]

Kuhn's notion of normal science, with its plodding and meticulous attention to detail, claimed Watkins, is applicable to any number of disciplines including Biblical exegesis and astrology.

Watkins' final blow to normal science was that it cannot be responsible for the emergence of revolutionary science. Inspection of the historical record of scientific development reveals that new theories emerge not all at once but over a lengthy period in response to continuous, critical challenges to a theory. Watkins credited Kuhn's misconception to the comparison of the emergence of new theories to gestalt switches. Scientists are not prisoners of a theory but free to challenge it at any time. If science is likened to a religion, then "heretical thinking must have been going on for a long time before paradigm-change can occur . . . [which] means that the scientific community is not, after all, a closed society whose chief characteristic is 'the abandonment of critical discourse'."[42]

Popper continued the charge against normal science. First, however, Popper admitted that he did not appreciate the distinction between normal science and revolutionary science and the problems the distinction raised for his analysis of science. He thanked Kuhn for "opening my eyes to a host of problems which previously I had not seen quite clearly."[43]

But Popper was not converted to Kuhn's new image of science but rather galvanized in his old image. For Popper, the normal scientist is someone who is trained inadequately, "in a dogmatic spirit . . . he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*."[44] Thus, rather than seeing normal science as normative or even descriptive for scientific practice, Popper saw it as a danger and threat as to how science is or should be practiced.

Popper next claimed that normal science is not supported by the history of science. Popper argued that Kuhn's new image of science fits astronomy best but not other sciences such as biology since Darwin and Pasteur. Most sciences do not have a single paradigm that determines scientific activity in a manner akin to normal science. Rather, for Popper science is critical in that "it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary."[45] But he is not adverse to dogmatism. "If we give in to criticism to easily," claims Popper, "we shall never find out where the real power of the theory lies."[46] Popper's dogmatism, however, differed from Kuhn's, in that it is part of the bold conjecture process and not normal science.

Finally, Popper asserted that the notion of normal science is predicated upon a relativistic logic that views rational discourse only within a framework whose foundations cannot be examined critically. Popper, however, insisted that we can critically examine these foundations.

> I do admit that at any moment we are prisoners caught in the framework of our theories . . . But we are prisoners in a sense: we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one.[47]

Popper's main concern with Kuhn's relativism was that it harbored irrationalism, in that scientists cannot rationally decide which framework to adopt.

Toulmin argued that Kuhn's current rendition of revolutionary breaks between paradigms appears not as radical as before in *Structure*. Revolutions are now demoted from macro to micro events. And with that demotion, they can now be viewed more like units of variation upon which selection acts.

> Suppose we stop thinking of Kuhn's small scale "micro-revolutions" as units of effective *change* in scientific theory, and treat them instead as units of *variation*. We will then be faced with a picture of science in which the theories currently accepted at each stage serve as starting-points for

> a large number of suggested variants; but in which only a small fraction of these variants in fact survive and become established within the ideas passed on to the next generation.[48]

Toulmin concluded that his approach may yield a mechanism for explaining revolutions rather than just labeling them.

Lakatos explored the differences or gestalt switch between Popper and Kuhn: science as continual critical assessment (duck) or science as paradigm commitment punctuated by paradigm shift (rabbit). One approach is rational, the other irrational.

> For Popper, scientific change is rational or at least rationally reconstructed and falls within the realm of the *logic of discovery*. For Kuhn scientific change—from one "paradigm" to another—is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (social) *psychology of discovery*.[49]

Lakatos believed that Kuhn's view of revolutionary change leads to religious conversion and is appalled that Kuhn resorts to "mob psychology" to charge Popper with naive falsification. Lakatos aimed to rescue Popper from Kuhn's charge by explicating a sophisticated version of falsification, which Lakatos then proceeded to develop in terms of a view of science he called "scientific research programmes," and in so doing justify rational progress or the role of criticism in the growth of knowledge.

Feyerabend claimed that Kuhn's normal science is fit only for dogmatic and narrow minded specialists. He was horrified by what he considered to be the logical outcome of Kuhn's new image of science: the way to scientific status for a discipline "is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm."[50] Such an image of science and its function, declared Feyerabend, cannot be supported historically; rather, "a science that tries to develop our ideas and that uses rational means for the elimination of even the most fundamental conjectures must use a principle of tenacity together with a principle of proliferation."[51] By the principle of tenacity Feyerabend meant "not just to follow one's inclinations, but to develop them further, to raise them, with the help of criticism . . . to a higher level of articulation."[52] By the principle of proliferation he meant "there is no need to suppress even the most outlandish product of the human brain."[53] For Feyerabend, this view of science trumped Kuhn's normal science.

Kuhn's imprecise use of paradigm was one of the chief complaints leveled by numerous critics of *Structure*. In a creative and sympathetic analysis of Kuhn's sense of paradigm, Margaret Masterman identified 21 senses of it. Her motivation for undertaking the analysis was that

> actual scientists are now, increasingly reading Kuhn instead of Popper: to such an extent, indeed, that, in new scientific fields particularly, "paradigm" and not "hypothesis" is now the "O.K. word". It is thus scientifically urgent, as well as philosophically important, to try to find out what a Kuhnian paradigm is.[54]

After identifying the different senses in which Kuhn uses paradigm in *Structure*, she grouped them into three categories. The first is the metaphysical paradigm or "metaparadigm," which provides the theoretical basis of scientific practice and includes a set of beliefs, a map, a standard, a metaphysical speculation or notion of an entity, an organizing principle that shapes perception, or a way of determining large areas of reality. The second category is the sociological paradigm, which directs the behavior of scientific communities and their members and includes a universally accepted achievement, a set of political institutions, or a device in common law. The final category is the artifact or construct paradigm, which is involved in concrete puzzle solutions and includes a textbook or classic work in the discipline, a source of tools for conducting experimental investigation, a machine-tool factory, or a gestalt figure that can be seen in two ways. Masterman concluded her analysis inviting others to join in articulating further Kuhn's notion of paradigm, for "if we retreat from all further consideration of Kuhn's 'new image' of science, we run the risk of totally disconnecting the new-style realistic history of science from its old-style philosophy: a disaster."[55]

Kuhn's "Reflections on my critics"

Kuhn began with an interesting rhetorical ploy often used in conflict. He conflated his identity by positing two Kuhns. The first Kuhn was the author of *Structure*, which was discussed by Masterman and the first Kuhn. The second Kuhn was the author of a book with the same title, which was discussed by Popper, Watkins, Toulmin, Feyerabend, and Lakatos. One might call these two the incommensurable Kuhns, a product of "partial or incomplete communication—the talking-through-each-other that regularly characterizes discourse between participants in incommensurable points of view."[56] Kuhn believed that he and his critics have talked past one another on three different sets of issues: methodology, normal science, and paradigm shift.

With respect to methodology, Kuhn observed that critics see his method as historical or social psychology and descriptive while their own is logical and normative. Kuhn claimed that this is a misperception, since all the participants in the colloquium engaged historical case studies and the behavior of scientists both individually and collectively. Moreover, the divergence between descriptive and normative is indistinct. With respect to the historical dimension of his method, Kuhn wrote:

> I am no less concerned with rational reconstruction, with the discovery of essentials, than are philosophers of science. My objective, too, is an understanding of science, of the reasons for its special efficacy, of the cognitive status of its theories. But unlike most philosophers of science, I began as an historian of science, examining closely the facts of scientific life.[57]

Kuhn's defense of the social psychology dimension of his method relied on the insufficiency of rules to dictate human behavior. For theory choice, for instance, Kuhn reaffirmed that community "behavior will be affected decisively by the shared commitments, but individual choice will be a function also of personality, education, and the prior pattern of professional research."[58] Finally, for the descriptive-normative distinction, Kuhn argued that his new image of science has normative implications for the practice of science:

> scientists behave in the following ways; those modes of behavior have (here theory enters) the following essential functions; in the absence of an alternate mode that would serve similar functions, scientists should behave essentially as they do if their concern is to improve scientific knowledge.[59]

Kuhn next took up the defense of normal science. He believed that critics' denial of normal science and classifying it uninteresting compared to revolutionary or critical science were unusual ploys. As for the nonexistence of normal science, Kuhn claimed that revolutionary science demands it. "By their nature," insisted Kuhn, "revolutions cannot be the whole of science: something different must necessarily go on in between."[60] The notion of revolution itself dictates against all science being revolutionary all the time. Normal science, with its period of stasis in which theories do not proliferate and scientists do not criticize their foundations, provides the scientific backdrop for revolutions to occur and to be recognized.

The problem of recognizing normal science or of distinguishing between normal and revolutionary science requires an appropriate understanding of the scientific community. By knowing what a community deems valuable, then the question of whether an historical period of scientific research is revolutionary or normal can be answered. "The gist of the problem," claimed Kuhn, "is that to answer the question 'normal or revolutionary?' one must first ask, 'for whom?'"[61] Moreover, normal science is palpable from history, asserted Kuhn, even from the case studies critics used to deny its existence. Finally, for the coinage of science normal science is a necessary obverse to the revolutionary converse in that it provides the stasis required for detailed scientific progress.

Kuhn then considered critics' charge that his position concerning theory choice or paradigm shift depends on irrationalism, relativism, and mob rule. Kuhn categorically denied the charge.

> 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 courts of appeal is neither to defend mob rule nor to suggest that scientists could have decided to accept any theory at all.[62]

In his defense of the charge of irrationalism, Kuhn wrote:

> What I am denying is neither the existence of good reasons nor that those reasons are of the sort usually described. I am, however, insisting that such reasons constitute values to be used in making choices rather than rules of choice.[63]

Kuhn also contended that his evolutionary notion of scientific development is not relativistic; for in the selection of one theory over another: "One scientific theory is not as good as another for doing what scientists normally do."[64] However, in relation to truth Kuhn admitted that his position is relativistic. He agreed that a newer theory is "better" than an older one "as a tool for the practice of normal science," but Kuhn denied that the newer theory captures the truth of reality.[65] Finally, in terms of the charge of mob rule, Kuhn appealed to the fact that in terms of mob psychology there is generally a rejection of community values; but if the scientific community rejects its values, "then science is already past saving."[66]

Finally, Kuhn turned to the notion of incommensurability and to the nature of paradigms. To address critics, he framed the discussion of incommensurability in terms of translation: just as a translator cannot provide a literal translation of a text from one language to another, so scientists cannot compare competing theories point-for-point because there is no theory neutral language by which to compare them. There is always some information lost in translation, which is critical for full communication. The sources of incommensurability, then, are two. First, for terms shared between two incommensurable paradigms, meaning changes in profound ways during a paradigm shift. Second, "languages cut up the world in different ways, and we have no access to a neutral sub-linguistic means of reporting."[67] In other words, there is no adequate translational manual by which to translate or transpose an older theory to a new one because such manuals are predicated on specific theories that interpret the world differently.

As for the nature of paradigms, Kuhn accepted partial responsibility for the confusion surrounding their use in *Structure*. He credited Masterman for skillfully demonstrating their various uses. In a later interview, Kuhn acknowledged that she was on target in terms of the core meaning of paradigms: "she's got it right! . . . a paradigm is what you use when the theory isn't there."[68] In a 1966 letter, Kuhn wrote to Masterman:

> I could not be more delighted by your piece for Imre's volume. It seems to me even clearer, more cogent, and occasionally deeper than the original, and you know that I liked that. I feel sure it will be effective with at least some of the people who have yet to be touched by the central tenets of our position, and I do not know what could possibly be asked.[69]

Although she proposed three categories for the uses of paradigms, Kuhn eventually divided them into two categories: disciplinary matrix and exemplars.

Structure revisited

1967 Swarthmore lecture: "Paradigms and theories in scientific research"

Kuhn acknowledged the problems associated with *Structure*, particularly concerning the notion of paradigm and the distinction between normal and revolutionary science. To address the problems Kuhn analyzed the scientific community, in terms of their commitments. Determination of a revolutionary development, asserted Kuhn, depends on the apposite community in which the change is judged revolutionary. He proceeded to discuss various methods for identifying and classifying scientific communities: “my reason for doing so is the conviction that it is those groups—both in their individuality and in their interrelationships—which must be studied if we’re to understand the nature of scientific advance.”[70]

The question arose as to what defines the common practice of these communities. Kuhn addressed the question by admitting that the answer given in *Structure*, i.e. paradigm, was too expansive. Now he wanted to narrow the scope of that term. “For the cluster of commitments which make possible a group’s research,” stated Kuhn, “I need some phrase like the group’s professional Weltanschauung, or its ideology, or its special matrix of beliefs and values.”[71] The phrase he settled on was “professional matrix.”

The professional matrix is made up of several elements. Natural laws are the first and most obvious element, which are “the heart of the formal component of the professional matrix.”[72] Laws only change during a major revolution because of their stability, although “the way they are fitted to nature by particular communities may change more often.”[73] Another element is the community’s “collective metaphysics.” “Roughly speaking,” wrote Kuhn, “this consists of the entities and powers which appear in or are used to explain the laws.”[74] A revolution depends on whether the metaphysical commitment is an essential component of the community’s matrix. An important element is the instrumental. “The techniques by which we choose to observe and measure objects of our environment,” explained Kuhn, “carry with them disguised commitments or expectations about what is and is not in the universe and about the way these things behave.”[75]

Although there are other elements, which Kuhn did not list, he concluded the discussion of “the one that plays a major role in the attachment of laws to nature, and it is this part of the matrix for which I originally introduced the term paradigm in [*Structure*].”[76] Traditionally that connection was mediated by correspondence rules. But, insisted Kuhn, these rules are insufficient for the task. Often scientists do not have adequate definitions for the different symbolic expressions of a law to cover every concrete problem.

How then can a particular problem be solved or the law connected to a particular part of nature? Since the correspondence rules are inadequate it appears that there is something missing in what scientists know about the world, in order to explain it with their abstract symbols. But Kuhn believed that the problem is not with the way scientists practice their trade but with the traditional view of science. “What is it then,” asked Kuhn, “which supplies the element we feel to be missing?”[77] The answer is pedagogical in nature. “It’s in the doing problems,” noted

Kuhn, “that a student acquires what substitutes for definitions and rules of application.”[78]

What is learned through this process of problem-solving is the similarity relationship between the solved problem and the unsolved one. Kuhn used an illustration of a child’s lesson in ornithology to clarify the process. The child and parent are walking through a park and the parent attempts to teach the child the differences among swans, geese, and pigeons. The parent points to the different classes of birds and then challenges the child to do likewise, reinforcing correct identifications and correcting mistakes. Through this process of ostension and of reinforcing and correcting, the child learns to discriminate among the three classes of birds.

> According to Kuhn, the consequences of this process are two-fold. First, when one learns in this way from examples, one clearly does learn something about what terms like “swan,” “goose,” etc. mean . . . But clearly that’s not all one’s learned. In learning meaning one’s also learned a good deal about what the world does and does not contain . . . Second, this mode of learning allows quite naturally room for what I’ve previously called both normal and revolutionary change.[79]

Paradigms clarified

Kuhn admitted that *Structure*’s popularity was due to its “excessive plasticity,” i.e. “people read the book with their own agenda and find in it what they want.”[80] Kuhn acknowledged that the many meanings and uses of paradigm in *Structure* were responsible for the plasticity and agreed with critics that clarification was warranted. He further developed the Swarthmore lecture to that end by categorizing paradigms into disciplinary matrix and exemplar and by discussing the philosophical implications of the more important of the two. Importantly, Kuhn disavowed that paradigm acquisition mystically transforms a discipline into a science.

Scientific communities

To clarify the notion of paradigm, Kuhn discussed the nature of scientific communities; for the nature of paradigm is intimately connected with the nature of scientific communities. Citing recent sociological work on those communities, Kuhn noted that their members are joined “by common elements in their education and apprenticeship, [and] they see themselves and are seen by others as the men responsible for the pursuit of a set of shared goals.”[81] Scientific communities vary in size, often defined by their subject matter, with the smallest and more specialized communities representing the basic taxonomic units. Practitioners may often belong to more than one unit. From these units, communities expand to include the largest unit: all natural scientists. Kuhn insisted that scientific communities are "the producers and validators of scientific knowledge."[82]

Disciplinary matrix

From the analysis of scientific community, Kuhn asked: "What do its members share that accounts for the relative fullness of their professional communication and their relative unanimity of their professional judgments?"[83] The answer, obviously, is a paradigm or a set of them. Paradigms govern the shared community life and not the subject matter. In other words, paradigms are more than a theory, which is too limited for Kuhn's purposes. They represent the milieu of the professional practice, or as Kuhn called it: the "disciplinary matrix."[84] "'Disciplinary' because it is the common possession of the practitioners of a professional discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification."[85] Kuhn acknowledged that there are many different constituents of disciplinary matrix used in *Structure*, but he focused on the following: symbolic generalizations, models, values, and exemplars.

As symbolic generalizations, paradigms are the formal components of a disciplinary matrix. For example, "in physics," wrote Kuhn, "generalizations are often found already in symbolic form: $f = ma$, $I = V/R$."[86] These generalizations allow the community's practitioners to use mathematics and logic to solve their puzzles and are indicators of their command of nature. But the question arises how to connect these symbolic generalizations to nature. Correspondence rules, in terms of basic statements, are incapable, claimed Kuhn; rather, he proposed "that an acquired ability to see resemblances between apparently disparate problems plays in science a significant part of the role usually attributed to correspondence rules."[87] This ability is acquired through education and apprenticeship and grounds normal science practice. Thus, detailed progress is only possible during times of normal science in which scientists are free to pursue technical puzzles about nature–theory fit rather than arguing about metaphysical principles or which instrumentation to use or how to interpret the data.

As models, paradigms are the community's "preferred analogies."[88] Models are also part of the metaphysical dimension of scientific practice. This dimension includes beliefs, such as in models as heuristic devices for guiding research or as ontological formulae for carving up the world. Models within a metaphysical context also provide the community with permissible metaphors. "By doing so," claimed Kuhn, "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".[89]

As values, paradigms function as the glue to hold the community together. They help to identify a crisis, to select a new paradigm, to judge a theory as a whole, to evaluate a theoretical prediction, and to determine science's social usefulness. Values include accuracy, simplicity, consistency, plausibility, among others. Except for accuracy, judgements of these values vary from person to person. Moreover, Kuhn stated:

> though values are widely shared by scientists and though commitment to them is both deep and constitutive of science, the application of values is sometimes considerably affected by the features of individual personality and biography that differentiate the members of the group.[90]

He realized that his position on values caused critics, such as Shapere and Scheffler, to charge him with subjectivity and irrationality. Kuhn addressed this charge, arguing that value judgements in any discipline are critical determinants of community behavior regardless of individual appropriation and that individual appropriation of values serves important functions in the community such as distributing risks. Far from being subjective and irrational, Kuhn insisted that values assure science's success by affording a certain amount of plasticity to its practice.

Exemplars

Exemplars are "concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic."[91] Kuhn differentiated their utility for various segments of the scientific community. For undergraduates, exemplars are the standard puzzles at the end of textbook chapters, on examinations, and in laboratory manuals. For graduate students and practicing community members, exemplars also include the solved puzzles in the professional literature. For Kuhn scientific knowledge is not localized simply within theories and rules, so that students simply apply them to solving problems. Rather it is localized within exemplars, so that students must learn the puzzle solutions shared by the community. "In the absence of such exemplars," claimed Kuhn, "the laws and theories [students have] previously learned would have little empirical content."[92] In other words, through exemplars students learn the vocabulary and concepts along with natural phenomena. The two go hand-in-hand.

The basis for an exemplar to function in puzzle-solving is the scientist's ability to see the similarity between a previously solved puzzle and a currently unsolved one. However, the scientist's ability to see the similarity is not due fundamentally, stressed Kuhn, to a set of procedural rules that dictate the solution; rather, the "basic criterion is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria [such as procedural rules] by which that same identification of similarity might have been made."[93] The principle of perception of similarity can be applied directly to the solution of a new puzzle vis-à-vis the old one.

Kuhn used the same illustration of a child's ornithology lesson, as he introduced in the Swarthmore lecture, to clarify the principle of perception of similarity, except ducks replaced pigeons. He also incorporated neurophysiology to explicate the pedagogical experience. The basis for the experience, wrote Kuhn, is that "part of the neural mechanism by which [the child] processes visual stimuli has been reprogrammed."[94] Importantly, the child learns the lesson without the aid of correspondence rules but rather with a "primitive perception of similarity and difference" and the knowledge learned "can thereafter be embedded, not in generalizations or rules, but in the similarity relationship itself."[95] Thus, shared examples and not necessarily rules are responsible for processing either stimuli or data. "Shared examples," concluded Kuhn, "can serve cognitive functions commonly attributed to shared rules. When they do, knowledge develops differently from the way it does when governed by rules."[96]

Further criticism and clarification

1969 Urbana conference

Suppe criticized Kuhn's clarification of paradigm in terms of disciplinary matrix and exemplars. The problem, claimed Suppe, is that the bird-learning example Kuhn offered to illustrate exemplar acquisition through resemblance relationships is a "disanalogy." Exemplars are more complex than simple ostensive definitions and require the ability to translate between experimental and theoretical languages, in order to connect symbolic generalizations to nature. Suppe also criticized Kuhn's acquisition of resemblance relationship as a substitute for the traditional view's correspondence rules. The problem, according to Suppe, is that the latter rules also function to define partially terms of a symbolic generalization. Although Kuhn's exemplars provide a similar service implicitly, Suppe contended that the difference in a term's meaning cannot be ascribed to resemblance relations. Other factors, such as unstated physical assumptions, are also required to provide the conceptual content of a term.

As for disciplinary matrix, Suppe claimed that Kuhn's rendering of it "is insufficiently precise and invites the sort of undesirable plastic employment that the paradigm experienced."[97] Rather than define disciplinary matrix in terms of the activities and concepts of a discipline generally, Suppe proposed to define it in terms of the specialized groups of practitioners, i.e. "the more esoteric state-of-the-art experiments in the recent journals, and more important, the private unpublished reports and communications which are circulated among the small groups of scientists who communicate and collaborate with each other about their current research."[98] Only at this level then can a group's disciplinary matrix be specified more precisely.

Suppe was also troubled by Kuhn's definition of theory as "a collection of symbolic generalizations with specific meanings attached to its constituent terms."[99] What bothered him was that any difference in a term's meaning implies a different theory. Since Suppe assumed that each member of a disciplinary group would have a faintly different resemblance-relation experience and thereby use a slightly different disciplinary matrix, the end result would be a proliferation of theories. Moreover, for Suppe no community resembles a single individual. He concluded that "Kuhn's claim that members of a disciplinary group share a common disciplinary matrix thus seems ultimately indefensible."[100] According to Suppe, the solution was to drop all mention of paradigm, disciplinary matrix, and exemplar, since they overpopulate the world of philosophical entities and obscure Kuhn's important insights in the nature of science.

Kuhn insisted that he was not overpopulating the world of philosophical entities by introducing terms such as exemplar. Rather he was identifying important features of theories overlooked by the traditional view. "Surely philosophers have been aware of [concrete problem solutions'] existence," pleaded Kuhn, "in which case my grouping them under the rubric 'exemplars' cannot have added a new entity to the discourse about science."[101] Kuhn corrected Suppe's misunderstanding of Kuhn's notion of theory. "A theory consists," asserted Kuhn, "among other things, of verbal and symbolic generalizations *together with* examples of their function and use."[102] Moreover, Kuhn claimed that Suppe's fears about theory proliferation are unfounded, since it is not the consequence of learning through exemplars but of language learning in general.

Kuhn also insisted that the ornithology illustration for resemblance relationships is not too simplistic for scientific laws. He saw no reason in principle why the illustration cannot be used to support his position.

He contended that more than words are learned during the process. What is learned also includes, for example, "what the world contains and about how the newly named entities behave."[103] Kuhn finally addressed Suppe's charge of the disanalogy between the ornithology illustration and exemplar acquisition. He claimed that the formulation of Suppe's critique in terms of translation is defective. What Kuhn wanted to resist in Suppe's move to translation was the syntactical analysis of science. Rather Kuhn was concerned about semantic analysis of science. Moreover, translation is a far richer activity for Kuhn and involves diagrammatic illustrations and especially laboratory demonstrations. Finally, Kuhn conceded that resemblance relationships do supply implicitly definitions of symbolizations but is unclear as to what "meaning" and "partial definition" is.

Sylvain Bromberger argued that Kuhn's account of exemplar acquisition did not in principle rule out a role for correspondence rules. Rather Bromberger proposed that to explain the ability of the child to classify birds is "to assume not that the exemplar is common, but rather that the effect of the exemplar on that sort of organism is the formation and internalization of some sort of a rule which is then applied to other cases."[104] He felt empirical investigation was needed to examine this proposal. Kuhn's response was equivocal. One the one hand he agreed that there are rules that govern the processing of neural stimuli under unconscious control, but on the other hand he believed that correspondence rules are insufficient to account for exemplar acquisition.

Patrick Suppes questioned Kuhn's reliance on disciplines such as psychology to do philosophy of science. "As I read your paper," claimed Suppes, "it seems to me that you want to suggest that the philosophy of science is really to become the psychology of science."[105] And, he wanted to resist that move. Kuhn defended his use of psychology to do philosophy of science. He believed that other disciplines, including psychology and history, allowed him to address issues in the philosophy of science from an empirical perspective. Even though Kuhn was professionally an historian, he claimed that epistemological issues motivated his historical interests.

> I really want to know what sort of thing knowledge is, what it is all about, and why it is that it works the way it does. Now in order to do that, it seems to me the right move (I am glad somebody else said philosophy is an empirical enterprise) is to look around and try to see what is going on and what it is that people who have knowledge have got.[106]

Through this means Kuhn believed he could contribute to a better epistemology.

Hilary Putnam claimed that Kuhn's exemplars play two roles. The first is obviously that students learn to solve problems correctly. The second is that students learn to use what Putnam called "auxiliary statements" to solve problems. These statements, acting to define "boundary conditions," serve to connect a theory to the natural world rather than correspondence rules. Kuhn responded that Putnam is simply putting new wine in old wine skins. "At the moment," confessed Kuhn, "I cannot see that substituting auxiliary statements for correspondence rules is going to have any bearing on the problem I have been trying to raise."[107] What Kuhn found problematic is Putnam's attempt "to find linguistic forms to make explicit what is, in fact, tacitly embodied in the language-nature fit."[108]

Shapere inquired as to whether similarity relationships are discovered. If so, then he insisted that Kuhn's position is no different than the traditional position of pure observation. He also inquired as to whether the similarity relationships or exemplars are important "because the community picked them out, or does the community pick them out because there is some good reason to pick them out."[109] The distinction was critical according to Shapere, for the former lapses back into the relativism of *Structure* while the latter vitiates the explanatory power of the sociological dimension of exemplars. Kuhn responded to the first inquiry by noting that there is no direct access to stimuli as "given" but only "to a data world that the community has already divided in a certain way."[110] To the second inquiry, Kuhn pointed out that his image of science is evolutionary. "In this sense," wrote Kuhn, "scientific development is a unidirectional and irreversible process, and that is not a relativistic view."[111]

Reviews of the revised Kuhn

The second edition of *Structure*, along with Kuhn's London and Urbana papers, were reviewed in professional journals. The two most widely acclaimed reviews were by Alan Musgrave and by Shapere. Musgrave claimed that Kuhn shifts from his earlier position because of a theoretical analysis of the micro community's structure. First, micro revolutions can occur between macro revolutions, without a preceding crisis period. Second, normal science may constantly be in a state of crisis. Finally, normal science can be conducted in the presence of metaphysical controversy. But Kuhn's shift raised a question for Musgrave: "So what are the paradigms, consensus upon which remains a pre-requisite for normal research?"[112]

After reviewing Kuhn's attempts to clarify the notion of paradigm by introducing the notions of disciplinary matrix and exemplar, Musgrave answered the above question in terms of Kuhn's exemplars. But he distrusted the answer and criticized Kuhn's reliance on an analogy between students solving textbook problems and scientists conducting research. Musgrave rejected the analogy, since the answer to the textbook problem is known but there is no pre-known answer for the practicing scientist conducting original research. "It is always an open question," insisted Musgrave, "whether the working scientist can model a satisfactory solution to his problem on previously obtained exemplary solutions to other problems."[113] Musgrave also reviewed Kuhn's denials over the charge of irrationality and relativism ascribed to in the latter's earlier positions. But Musgrave admitted that he is "unconvinced" by Kuhn's arguments and was "disappointed" in Kuhn's response to critics. "I find," confessed Musgrave, "the new, more real Kuhn who emerges in [his responses] but a pale reflection of the old, revolutionary Kuhn."[114]

Shapere agreed that Kuhn distances himself from the radical parts of *Structure*'s first edition. Although he appreciated Kuhn's efforts to bring clarity to the paradigm concept through the distinction between disciplinary matrix and exemplars, he claimed that Kuhn failed to achieve the goal. "This distinction, however," observed Shapere, "is of little help to those who found the earlier concept of 'paradigm' obscure."[115] The problem, contended Shapere, was not that readers of the 1962 *Structure* did not recognize paradigm's primary function as concrete puzzle-solving but that Kuhn failed to provide adequate clarification on the relationship of paradigm *qua* exemplar to paradigm *qua* disciplinary matrix and on how paradigm *qua* exemplar guides scientific research.

Shapere also discussed Kuhn's relativism. He agreed that Kuhn's current evolutionary spin on theory change is not relativistic; "but it is a far cry from Kuhn's first-edition attack on the view of scientific change as a linear process of ever-increasing knowledge."[116] Although Shapere acknowledged that Kuhn claimed there are "good reasons" for persuading a group to choose a particular theory, he was appalled that Kuhn equates these reasons with values. Such a claim makes recourse to reason "gratuitous" and he concluded that Kuhn's position is "as relativistic, as antirational, as ever."[117] Finally, Shapere ended with a question reminiscent of the one he asked Kuhn at the Urbana conference: "Do scientists . . . proceed as they do because there are objective reasons for doing so, or do we call those procedures 'reasonable' merely because a certain group sanctions them?"[118] Shapere claimed that Kuhn's position is the latter, for the community of practitioners is the locus of rationality.

Notes

1. Shapere (1966).
2. See, Scheffler (1982), pp. 67–89. For discussion concerning Scheffler's critic of Kuhn, see Meiland (1974) and Siegel (1976).
3. Kuhn (2000), 307.
4. Sigurdsson (1990), 21.
5. Gillispie (1962), 1251.
6. Barber (1963), 298.
7. Boring (1963), 181.
8. de Solla Price (1963), 294A.
9. Shapere (1964), 385.
10. *Ibid*, 386.
11. *Ibid*, 388.
12. *Ibid*.
13. *Ibid*, 390.
14. *Ibid*, 391.
15. *Ibid*, 392.
16. *Ibid*, 393.
17. Anonymous (1964), 144.
18. *Ibid*.
19. Siekevitz (1964), 148.
20. MIT MC 240, box 4, folder 15, 4 April 1963 letter, McConnell to Kuhn.
21. MIT MC240, box 4, folder 15, Memorandum, 7 March 1963.
22. *Ibid*.
23. *Ibid*, 23 April 1963 letter, Kuhn to McConnell.
24. MIT MC240, box 4, folder 6, 18 April 1966 letter, Anspach to Kuhn.
25. *Ibid*, 9 May 1966 letter, Kuhn to Anspach.
26. MIT MC240, box 4, folder 15, 10 September 1970 letter, Sachs to Kuhn, p. 1.
27. *Ibid*, p. 3.
28. *Ibid*, Kuhn to Sachs, p. 1.
29. Kuhn (1970b), p. 3.
30. *Ibid*, p. 4.
31. *Ibid*, p. 6.
32. *Ibid*, p. 10.
33. *Ibid*, p. 11.
34. *Ibid*, p. 12.
35. *Ibid*, pp. 14–15.
36. *Ibid*, p. 16.
37. *Ibid*, p. 19.
38. *Ibid*, p. 21.
39. *Ibid*, p. 22.
40. Watkins (1970), p. 26.
41. *Ibid*, p. 32.
42. *Ibid*, p. 37.

43. Popper (1970), p. 52.
44. *Ibid*, p. 53.
45. *Ibid*, p. 55.
46. *Ibid*.
47. *Ibid*, p. 56.
48. Toulmin (1970), p. 46.
49. Lakatos (1970), p. 93.
50. Feyerabend (1970), p. 198.
51. *Ibid*, p. 210.
52. *Ibid*.
53. *Ibid*.
54. Masterman (1970), p. 60.
55. *Ibid*, p. 88.
56. Kuhn (1970c), p. 232.
57. *Ibid*, p. 236.
58. *Ibid*, p. 241.
59. *Ibid*, p. 237.
60. *Ibid*, p. 242.
61. *Ibid*, p. 252.
62. *Ibid*, p. 234.
63. *Ibid*, p. 262.
64. *Ibid*, p. 264.
65. *Ibid*.
66. *Ibid*, p. 263.
67. *Ibid*, p. 268.
68. Kuhn (2000), p. 300.
69. MIT MC240, box 11, folder 41, 1 June 1966 letter, Kuhn to Masterman.
70. MIT MC240, box 3, folder 14, "Paradigms and theories in scientific research," p. 9.
71. *Ibid*, p. 10.
72. *Ibid*, p. 12.
73. *Ibid*, p. 11.
74. *Ibid*, p. 13.
75. *Ibid*, p. 14.
76. *Ibid*, p. 15.
77. *Ibid*, p. 17.
78. *Ibid*.
79. *Ibid*, pp. 23–4.
80. Kuhn (1977b), p. 459.
81. *Ibid*, p. 461.
82. Kuhn (1970d), p. 178.
83. *Ibid*, p. 182.
84. In the Urbana lecture, which predates the "Postscript," Kuhn also refers to these paradigms as "global."
85. Kuhn (1977b), p. 463.

86. *Ibid*, p. 464.
87. *Ibid*, 471.
88. *Ibid*, 463.
89. Kuhn (1970d), p. 184.
90. *Ibid*, p. 185.
91. Kuhn (1977b), p. 463.
92. Kuhn (1970d), p. 188.
93. Kuhn (1977b), p. 472.
94. *Ibid*, p. 474.
95. *Ibid*, pp. 475–7.
96. *Ibid*, p. 482. Kuhn admitted that exemplars resemble Michael Polanyi's tacit knowledge, "which is learned by doing science rather than acquiring rules for doing it." Kuhn (1970d), p. 191. But he denied that such knowledge is subjective and irrational.
97. Suppe (1977c), p. 495.
98. *Ibid*, p. 496.
99. *Ibid*, pp. 496–7.
100. *Ibid*, p. 498.
101. Kuhn *et al* (1977), p. 501.
102. *Ibid*.
103. *Ibid*, p. 503.
104. *Ibid*, p. 510.
105. *Ibid*, p. 511.
106. *Ibid*, p. 512.
107. *Ibid*, p. 516.
108. *Ibid*.
109. *Ibid*, 507.
110. *Ibid*, 509.
111. *Ibid*, 508.
112. Musgrave (1971), 291.
113. *Ibid*, 293.
114. *Ibid*, 296.
115. Shapere (1971), 707.
116. *Ibid*, 708.
117. *Ibid*.
118. *Ibid*, 709.

PART III

The path following *Structure*

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Chapter 5

What is Kuhn up to after *Structure*?

Structure's success in attracting attention and inciting people's imagination, as well as its failure to be understood properly, imprisoned Kuhn for the remainder of his career in efforts to explain and clarify his original intent and meaning. The specter of *Structure* was always before him, even when he tried to put it behind him. In this chapter, Kuhn's scholarship after *Structure* is examined in three sections: historical studies, historiographic studies, and metahistorical or philosophical studies.

Historical studies

Kuhn conducted two major historical studies after the 1962 *Structure*. The first was on the origins of the Bohr atom, which was conducted in collaboration with Heilbron. The authors provided a revisionist narrative of Bohr's path to the quantized atom, beginning with his 1911 doctoral dissertation and concluding with his 1913 three-part paper on atomic structure. The intrigue of this historical study was that within a six-week period in mid-1912, Bohr went from little interest in models of the atom to producing a quantized model of J. J. Rutherford's atom and applying that model to several perplexing problems. The authors explored Bohr's sudden interest in atomic models. They proposed that his interest stemmed from "specific problems," which guided Bohr in terms of both his reading and research toward the potential of the atomic structure for solving these problems.[1] The solutions to those problems resulted from what Heilbron and Kuhn called a 1913 "February transformation" in Bohr's research. What initiated the transformation, claimed the authors, was that Bohr read a few months earlier J. W. Nicholson's papers on the application of Max Planck's constant to generate an atomic model. Although Nicholson's model was incorrect, it stimulated Bohr in the right direction. Then in February 1913, Bohr, in a conversation with H. R. Hansen, learned the last piece of the puzzle. After the transformation, Bohr completed the atomic model project within the year. "Like any revolutionary contribution to science," concluded Heilbron and Kuhn concerning Bohr's trilogy, "his 'Constitution of Atoms and

Molecules' provided a program for research as well as a concrete research achievement."[2]

The second historical study was on Planck's black-body radiation theory and the origins of quantum discontinuity. The transition from classical physics, in which particles pass through intermediate energy stages, to quantum physics, in which energy change is discontinuous, is traditionally attributed to Planck's 1900 and 1901 quantum papers. According to Kuhn, this traditional account was inaccurate and the transition was initially affected by Albert Einstein's and Paul Ehrenfest's independent 1906 quantum papers. Kuhn's realization of this inaccuracy was similar to the enlightenment he experienced when struggling with making sense of Aristotle's notion of mechanical motion. His initial epiphany occurred while reading Planck's 1895 paper on black-body radiation. Through that experience, he realized that Planck's 1900 and 1901 quantum papers are not the initiation of a new theory of quantum discontinuity, but rather they represent Planck's effort to derive the black-body distribution law based on classical statistical mechanics.

Kuhn's narrative for the origins of quantum discontinuity began with Planck's search in the late nineteenth century to understand black-body radiation in terms of the second law of thermodynamics. The result was Planck's 1896 paper on a radiation-damped resonator. Kuhn then examined Ludwig Boltzmann's statistical analysis of irreversibility, which had an impact on Planck's research beginning in 1898. Planck appropriated Boltzmann's analysis in the first stage of the black-body radiation theory's development. The second stage of its development began in 1900 and resulted in the derivation of the black-body distribution law. According to Kuhn, Planck introduced the constant h to account for the resonator's cell size and not for its energy level. Consequently, Planck was not thinking in terms of the quantization of energy levels. Kuhn concluded the analysis with an examination of how Planck and his contemporaries understood the revised black-body theory, which reached maturity in 1906. Up to 1906, Planck did not discuss the research on black-body radiation in terms of discontinuity.

Kuhn turned to the proximate causes of the discovery of quantum discontinuity, which was initiated by Einstein's and Ehrenfest's realization that the derivation of Planck's black-body law required restricting resonator energy to discrete multiples. Although published in 1906, the notion of discontinuity was not generally recognized, at least by German physicists, until Lorentz's 1908 lecture on the black-body radiation problem to an assemblage of mathematicians in Rome. At the time, both Einstein and Ehrenfest were relatively unknown and did not have

Lorentz's community stature. Kuhn next explored the events that led Lorentz to adopt discontinuity to resolve the black-body problem. By the end of 1910, most theoretical physicists followed Lorentz's lead.

The black-body radiation research program, however, offered little guidance for resolving discontinuities at the quantum level and was subsequently dropped. Its successor was a research program focusing on specific heat at low temperatures, which opened up new areas of research undertaken by an international community of practitioners. Kuhn concluded the study with an analysis of Planck's "second" black-body theory, first published in 1911, in which Planck used the notion of discontinuity to derive the second theory. Rather than the traditional position that represents the second theory as a regression on Planck's part to classical physics, Kuhn argued that it is the first time Planck incorporated into his theoretical work "a theory he never came quite to believe."[3]

In the black-body radiation and quantum discontinuity historical study, Kuhn did not use paradigm, normal science, anomaly, crisis, or incommensurability, which he championed in *Structure*. Critics, especially within the history and the philosophy of science communities, were disappointed. Commenting on the relationship between *Structure* and *Black-Body Theory*, Martin Klein, for example, lamented the missed opportunity of using the general theoretical framework from the former book to clarify and improve the understanding of a specific revolutionary change detailed in the latter book. Most reviewers commented on the absence. However, Peter Galison justified its absence accordingly:

> This is no accident, for as Kuhn often points out himself, he wears two hats, one as a historian of science and the other as a philosopher of science. In *The Quantum Discontinuity* he is writing as a historian of modern physics.[4]

Galison noted that the traditional view of black-body theory and quantum history that served as a backdrop for Kuhn's revisionist interpretation were Klein's and Hans Kangro's work. Klein responded by criticizing Kuhn's central thesis that Planck's black-body theory was fully classical.

> Planck did think that he had based his derivation on the accepted theories—classical physics, as we say now . . . The energy elements or quanta were an artifice but were nevertheless "the essential point" in introducing that new natural constant *h* which he prized so highly from the beginning . . . The quanta were there in his theory, nevertheless, and some of his readers did draw attention to them.[5]

Other scholars also questioned Kuhn's central thesis. For example, Abner Shimony praised Kuhn for his analysis of Planck's black-body theory, but questioned his interpretation surrounding Einstein's contribution to wave-particle duality.

Reviewers also raised philosophical issues in *Black-Body Theory*. Shimony, for example, commented on the issue of rationality in paradigm shift in contrast to an irrational conversion. But he tempered his comments writing: "In making a claim for the rationality of the intellectual processes of leading investigators into the blackbody problem I am no means asserting that there is a rigid algorithm for scientific inference."[6] Rather he acknowledged the role of "informal" approaches and strategies to scientific inference. Moreover, John Nicholas took the opportunity to criticize Kuhn's notion of normal science in terms of Planck's research on black-body radiation.

> It may well be that normal science is more innovative than Kuhn originally allowed, and we have to look more closely at the role of novel theory itself in transforming a mere puzzle into a crisis causing anomaly. If there was a crisis, Planck's theory was one of the causes, not one of the effects.[7]

Kuhn bemoaned the book's reception, even by its supporters, "as a misfit, a problem child, among my publications."[8] However, he did consider it his best historical study. Although Kuhn believed the evidence supported the revisionist account of the discontinuity story, he made the claim that the account "could be wrong. No single piece of available evidence demands it," continued Kuhn, "and evidence incompatible with it could yet to be discovered . . . As it stands now, however, evidence for the reinterpretation seems to me overwhelming."[9] He contended that the counter-evidence rests upon a misunderstanding of Planck's first derivation of the black-body distribution law. Planck distributed energy in a manner reflecting an average and not a maximization, which represented a short-cut in the derivation of the distribution law. It is taking this short-cut, claimed Kuhn, which misled Planck's readers.

Kuhn also explored the historiographic and philosophical issues raised in *Black-Body Theory* vis-à-vis *Structure*. The historiographic issue that *Black-Body Theory* addressed was the same first raised in *Structure*. In the latter book, he claimed that current historiography attempts to understand a previous period of scientific endeavor in terms of its contemporaries and not in terms of modern science. Kuhn's concern was more than historical accuracy; rather, he was interested in recapturing the thought processes that lead to a change in theory. Although *Structure* was Kuhn's articulation of this process for scientific change, the terminology in which he expressed the theory does not represent a rigid straightjacket for narrating history. For Kuhn, the terminology and vocabulary used in *Structure* are not products, such as metaphysical categories, in which an historical narrative must conform; rather, they have a different metaphysical function as presuppositions toward an historical narrative as process.

Kuhn's major historiographic lesson in *Black-Body Theory* was a methodological imperative: "Concerned to reconstruct past ideas, historians must approach the generation that held them as the anthropologist approaches an alien culture."[10] Failure to heed this imperative is to engage in Whig or ethnocentric history. The philosophical or epistemological lesson of *Black-Body Theory* was that scientific progress is not simply the march toward a truer understanding of natural phenomena; rather, it is an evolutionary process in which knowledge is selected under current pressures of argumentation and evidence. Kuhn provided a corollary to these historiographic and philosophic lessons: "Entry into a discoverer's culture often proves acutely uncomfortable, especially for scientists, and sophisticated resistance to such entry ordinarily begins within the discoverer's own retrospects and continues in perpetuity."[11]

This resistance often hinders reconstruction of the original discovery and begins with a distortion of the discoverer's and contemporaries' memories of the discovery event. "Not always but quite usually," observed Kuhn, "scientists will strenuously resist recognizing that their discoveries were the product of beliefs and theories incompatible with those to which the discoveries themselves gave rise."[12] The distorted version of the original discovery is often justified by invoking "confusion" on the part of the participants in the original discovery, i.e. the fuzzy vision of an embryonic discovery that the discoverer eventually intuits. The reason this "inauthentic" confusion is invoked to defend a distorted version of the discovery, according to Kuhn, is to mask the fact that the discoverer does not have an embryonic notion of the discovery and could not immediately solve the anomalies arising from research under the older theory.

But what is gained from distorting a discovery? Discoveries are the bricks used to build the edifice of science, and a scientist's reputation reflects the number of bricks personally contributed to that edifice. Revising (distorting) discoveries makes it easier to justify or credit the discoverer, even though, as Kuhn noted, there is often tremendous debate over the priority of a discovery. For Kuhn, the distortion damages both the image of science and the development of scientific knowledge. Under the distorted image, science is a cumulative, linear process that produces a continuous stockpile of static scientific knowledge. According to Kuhn, however, science should be viewed as a complex process by which knowledge emerges from the assimilation of anomalies into a new way of doing science.

Historiographic studies

The purpose of history of science, according to Kuhn, is not just getting the facts straight but providing philosophers of science with a more accurate image of science to practice their trade. Kuhn fervently believed that the new historiography of science would prevent philosophers from engaging in the excesses and distortions prevalent within traditional philosophy of science. He envisioned history of science informing philosophy of science as an historical philosophy of science rather than the history and philosophy of science, since the relationship was asymmetrical.

Prior to 1950 history of science was a discipline practiced mostly by eminent scientists, who generally wrote heroic biographies or sweeping overviews of the discipline, often for pedagogical purposes. This earlier history of science focused on, according to Kuhn, "the development of science as a quasi-mechanical march of the intellect, the successive surrender of nature's secrets to sound methods skillfully deployed."[13] Within the past generation historians of science, such as E. J. Dijsterhuis, Anneliese Maier, and Alexander Koyré, developed a history of science that was simply more than chronicling science's theoretical and technical achievements. An important factor in that development was the recognition of institutional and sociological factors in the practice of science. An important consequence of this historiographic revolution was the distinction between internal and external histories of science.

Internal history of science is concerned with the development of the theories and methods employed by practicing scientists. In other words, it studies the history of events, people, and ideas internal to science's advance. The historian as internalist attempts to climb inside the mind of scientists as they push forward the boundaries of their discipline. As Kuhn advised, "the historian should ask what his subject thought he had discovered and what he took the basis of that discovery to be."[14]

External history of science concentrates on the social and cultural factors that impinge on the practice of science. There are three types of external history. The first is the analysis of institutional history, especially of scientific societies and organizations. The second is intellectual history, which examines the impact of scientific ideas on the development of western thought. For Kuhn, this type of history represented a need to close the gap between histories of ideas and science. The third is most recent and involves the development of science within a well-defined geographic location. An example of this type is the development of science in seventeenth-century England.

For Kuhn, the distinction between internal and external histories of science mapped onto his pattern of scientific development. External or cultural and social factors are important during the science's initial establishment; however, once established, the problems addressed by practitioners of a mature science are no longer influenced by those factors "but by an internal challenge to increase the scope and precision of the fit between existing theory and nature."[15] Although external factors do not affect a mature science's problems, they do have an impact on other aspects of its development, such as the timing of new technologies. Importantly for Kuhn, the internal and external approaches to the history of science were not necessarily mutually exclusive but complementary.

According to Kuhn, the major impact of the new historiography was a clearer picture of science itself. Although the new history had little relevance for the actual practice of science, it would have an indirect impact on science education, administration, and policy. The new history of science would also influence the philosophy of science by providing a new image of science distinct from the traditional image, resulting in a different set of problems for future philosophers to resolve. Another discipline that would benefit from the new history of science was sociology, which can "learn from history something about the shape of the enterprise they investigate."[16]

Besides interest over historiographic issues for the historians of science, Kuhn was also concerned over these issues for philosophers of science.[17] His concern was deeply personal.

> History might . . . be relevant to the philosopher of science and perhaps also to the epistemologist in ways that transcend its classical role as a source of examples for previously occupied positions. It might, that is, prove to be a particularly consequential source of problems and of insights.[18]

Although Kuhn considered himself a practitioner of both the history and the philosophy of science, he believed that there is no separate discipline as the history and philosophy of science for a very practical reason. Crassly put, the goal for history is the particular while for philosophy the universal. Kuhn compared the differences between the two disciplines to a duck–rabbit gestalt switch. In other words, the two disciplines are so fundamentally different in terms of their goals for the analysis of science, that the resulting images of science are incommensurable. Moreover, to see the other discipline's image requires a conversion. For Kuhn, then, the history and the philosophy of science cannot be practiced at the same time but only alternatively, and then with difficulty.

"Given the deep and consequential differences between the two enterprises," Kuhn asked, "what can they have to say to each other?"[19] His answer had two parts. The first was the need of historians of science for philosophy as an analytic tool. Kuhn quickly clarified this part of the answer by denying that historians of science need to be conversant with contemporary philosophy of science, since the ideas of the latter discipline, "as the field is currently practiced in the English-speaking world, include little that seems to me relevant to the historian."[20] According to Kuhn, contemporary philosophy of science provided a distorted image of science that might mislead historians. The second part was the need of philosophers of science for history of science. "I deeply believe," stated Kuhn, "that much writing on philosophy of science would be improved if history played a larger background role in its interpretation."[21] Importantly, the history of science Kuhn had in mind was intellectual, and not social, history.

How then can the history of science be of use to philosophers of science? The answer is by providing an accurate image of science. Rejecting the "covering law model" for historical explanation because it reduces historians to social scientists, Kuhn advocated an image based on an ordering of historical facts into a narrative analogous to the one he proposed for puzzle-solving under the aegis of a paradigm in the physical sciences.

> 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."[22]

Historians of science, as they narrate change in science, provide an image of science that reflects the process by which scientific information develops, rather than the static image provided by traditional philosophers of science in which scientific knowledge is simply a product of logic. Kuhn insisted that the history of science and the philosophy of science remain distinct disciplines, so that historians of science can provide an image of science to correct the distortion produced by philosophers of science.

Besides traditional philosophy of science, the social history of science, according to Kuhn, also provides a distorted image of science: "the social/institutional history of science has shown that more than the interplay of observation and reason is important to an understanding of the shape and direction of scientific advance."[23] For social historians, scientific knowledge is "constructed." Although Kuhn was sympathetic to this type of history, he believed it left some very important questions unanswered, such as "What are the materials out of which these constructions are made?," or, more importantly from Kuhn's view, "What is the relation between older constructions and their newer replacements, the relation that makes the latter seem much more powerful than the constructions they replace?"[24] Moreover, he found its stronger form as unsatisfactory as he did the intellectual history of science. Kuhn believed that the divergence between these two approaches to the history of science creates a "gap," which he challenged historians of science to fill.

Besides social historians of science, Kuhn also blamed sociologists of science for distorting the image of science. Although Kuhn acknowledged that factors such as interests, power, authority, among others, are important in the production of scientific knowledge, their predominant use by sociologists eclipse other factors such as nature itself. The key to rectifying the distortion introduced by sociologists is to shift from a rationality of belief, i.e. the reasons people hold specific beliefs, to a rationality of change of beliefs, i.e. the reasons people change their beliefs. This shift introduced three differences that can be harnessed to correct the distortion. First, from a rationality of change the reasons for the shift in positions can be based on neutral observations but only for a specific community, at a given time. The next difference is that the change occurs in small incremental steps, although the overall change may appear large. The final difference is that change in beliefs is based upon a comparison of the beliefs with each other. According to Kuhn, truth as correspondence is not required to account for change. The end product of this shift is the same as the sociologists'—"facts are not prior to conclusions drawn from them and those conclusions cannot claim truth—but the route is different . . . Nothing along that route," claimed Kuhn, "has suggested replacing evidence and reason by power and interest."[25] Science for Kuhn is not a monolithic enterprise.

> Rather it should be seen as a complex but unsystematic structure of distinct specialties or species, each responsible for a different domain of phenomena, and each dedicated to changing current beliefs about their domain in ways that increase accuracy and other standard criteria.[26]

The proper means for articulating that image of science is through an historical philosophy of science.

Metahistorical studies

Besides the historiographic issues, Kuhn was particularly concerned with the metahistorical or philosophical issues derived from his historical research. The chief concerns during his career were scientific development and the related issues of theory choice and incommensurability, which he addressed through an historical philosophy of science. Importantly for Kuhn, both theory choice and incommensurability are intimately related. The former could not be reduced to an algorithm of objective rules but requires subjective values because of the latter. Although Kuhn delivered several lectures and published several articles on these concerns, his ultimate goal was a book that would extend the discussion began in *Structure*—a goal not realized in his lifetime.

Scientific development

Kuhn explored scientific development using three different approaches. The first was in terms of problem- versus puzzle-solving. According to Kuhn, "problems are vexing questions, often urgent ones, to which one scarcely knows what an answer would look like, much less how to go about finding one."[27] Problems are of two kinds. First is the intellectual problem, which vexes not only scientists but also philosophers. Such problems include the nature of consciousness, life, or matter. The second kind is more social than intellectual in nature, including environmental issues and world peace. "Nothing," wrote Kuhn, "about the existence of a problem guarantees that there even is a solution."[28] Problem-solving is often generally pragmatic, i.e. "try it and see if the problem disappears."[29] According to Kuhn, problem-solving is the hallmark of an underdeveloped science.

Puzzles, on the other hand, occupy the attention of scientists involved in a mature science. Although they have guaranteed solutions, the methods for solving puzzles are not. The scientists, then, who solve them, demonstrate their ingenuity and are rewarded by the community. Puzzle solutions are also immediately evident to the community; no one debates whether they are correct, because of "a shared body of rules."[30] There are two types of puzzles. The first is the "exemplary" puzzle, in which a solution is actually known, and is used to train students. The other puzzle type occupies practicing scientists, in which the solution is only potentially known. Thus, the goal of puzzle-solving is not novelty but "to clarify existing theory, show how to extend it into areas where no one has made it work before, attempt to reconcile it with theories applied to other areas."[31]

With this distinction in mind, Kuhn now envisioned the transition of a scientific discipline from an underdeveloped state to a developed one as "the transition from a problem-solving to a puzzle-solving tradition."[32] But the question arises: "how does the transition from problem solving to puzzle solving get made?"[33] The answer that many took from *Structure* was, adopt a paradigm. Kuhn now found this answer to be "wrong" in that paradigms are not unique only to the sciences. But does articulating the question in terms of puzzle-solving help? Apparently not! "And to the person who persists, who wants to know what it would take to turn a given field to puzzle solving, I'm going finally to have to say," admitted Kuhn, "that I haven't a clue."[34] The only solace Kuhn offered is that the person has a "PROBLEM!" His advice was to "try thing one [sic] after another, live with each and see how it works out, see if the problem goes away."[35]

Kuhn's second approach to scientific development was in terms of the "growth" of knowledge. He proposed an alternative answer to the traditional one that knowledge grows by a piecemeal accumulation of facts. To shed light on the alternative answer, Kuhn offered a different reconstruction of science. First, the central ideas of a science "lock together, lending each other mutual support."[36] Second, a set of the central ideas that interlock forms the "core" of a particular science. "Roughly speaking," stated Kuhn, "the core of a theory is the group of its parts that can't be removed or changed individually without creating havoc in a large part of the surrounding territory."[37] Besides the core, there is also a "periphery" in which "the elements in it are only constrained, and not determined by the core."[38] The periphery represents an "area" where problems associated with a research tradition can be investigated "without doing violence to the core."[39]

Kuhn drew parallels between the current reconstruction of science and the earlier one in *Structure*. Obviously, the transition in cores from one research tradition to another is a scientific revolution. Moreover, the core represents the paradigm that defines a particular research tradition. Finally, the periphery is identified with normal science. Kuhn then confessed that the current reconstruction puts him

> back where I started some years ago, with one possible exception. I'm not sure whether it should be described as a novelty or a source of clarification, but this way of putting my points does indicate, far better than

> my old one, what I take the source of a paradigm's authority to be. Why people seem to get so locked into them.[40]

A paradigm's authority resides not in the community's "conservatism," or "habit," or in "a special authoritarian Establishment," or even the paradigm's predictive power. Rather, its authority is the interlocking or coherence of its core's ideas. "To change a component of the core," claimed Kuhn, "one must change many others at the same time, produce a new and different core."[41] The core then provides the means by which to practice science and to change it involves a traumatic event that is naturally resisted by practitioners.

Is this change in the core, growth of knowledge? To answer the question, Kuhn examined the standard account of knowledge as justified, true belief. "My difficulty with the standard doctrine," admitted Kuhn, "has been that it's ultimately unilluminating or too little illuminating, with respect to the difference between the circumstances under which one may properly make a belief and a knowledge claim."[42] What is problematic is the amount or nature of the evidence needed to distinguish between knowledge and belief. And this, of course, raises the issue of truth. "But since we can't tell . . . whether the object of a knowledge claim is in fact true," contended Kuhn, "we're left as puzzled as ever about the nature of the circumstances under which we may appropriately claim knowledge."[43]

To address the question, Kuhn relied on John Austin's distinction between "Why do you believe?" and "How do you know?" While the former question is answered in terms of the evidence, "knowing that," the latter is answered with respect to one's credentials, "knowing how." But Kuhn ultimately equivocated on the question of the growth of knowledge. "If by 'knowledge' we mean knowing how," claimed Kuhn, then the question can be answered affirmatively.[44] "On the other hand," insisted Kuhn "if we mean by 'knowledge' the more usual 'knowledge that' . . . then I think the answer must be that it does not grow."[45] He admitted that such knowledge changes; however, he saw "no evidence at all of growth or even of some asymptotic approach to a final state."[46]

Kuhn's final approach to scientific development was through the analysis of three scientific revolutions: the shift from Aristotelian to Newtonian physics, Volta's discovery of the electric cell, and Planck's black-body radiation research and quantum discontinuity. From these examples, Kuhn derived three characteristics of scientific revolutions. The first is holistic. According to Kuhn, a scientific revolution's "central change cannot be experienced piecemeal . . . 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."[47] In other words, scientific revolutions are all-or-none events.

The second feature is the way referents change after a revolution. According to Kuhn, "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."[48] He introduced the notion of "taxonomy" to explicate the redistribution of objects after a revolution. "What characterizes revolutions is, thus, change in several of the taxonomic categories prerequisite to scientific descriptions and generalizations."[49] And these categories for a particular tradition represent a whole, which is rooted in language; for it is through language that terms are assigned to categories. "Language is a coinage with two faces," analogized Kuhn, "one looking outward to the world, the other inward to the world's reflection in the referential structure of the language."[50]

The final characteristic of scientific revolutions involves "a central change of model, metaphor, or analogy—a change in one's sense of what is similar to what, and of what is different."[51] Through similarity relationships students and members of a community learn the meanings of words and taxonomic categories of objects, which populate the world. Thus, two types of knowledge are learned: that of words and that of the world. Kuhn concluded:

> If I am right, the central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to language itself and that is thus prior to anything quite describable as description or generalization, scientific or everyday . . . Violation or distortion of a previously unproblematic scientific language is the touchstone for revolutionary change.[52]

Theory choice

According to tradition, the objective features of a good scientific theory include accuracy, consistency, scope, simplicity, and fecundity. However, these features when used individually as criteria for theory choice, argued Kuhn, are imprecise, in that scientists "may legitimately differ about their application to concrete cases."[53] Moreover, these criteria may conflict with one another. Although necessary for theory choice they are, for Kuhn, insufficient. "One can explain," he claimed, "as the historian characteristically does, why particular men made particular choices at particular times. But for that purpose one must go beyond the list of shared criteria to characteristics of the individuals who make the choice."[54] These characteristics of the individual scientists include personal experiences or biography and personality or psychological traits. In other words, not only does theory choice rely on a theory's objective features but also on individual scientists' subjective characteristics.

Why have traditional philosophers of science ignored or neglected subjective factors in theory choice? Part of the answer is that they confined the subjective to the context of discovery, while restricting the objective to the context of justification. Kuhn insisted that this distinction does not fit observations of actual scientific practice. It is artificial, reflecting science pedagogy. "In science teaching," explained Kuhn, "theories are presented together with exemplary applications, and those applications may be viewed as evidence."[55] But actual scientific practice reveals that textbook presentations of theory choice are stylized to convince students who rely on the authority of their instructors. What else can students do? Textbook science discloses only the product of science, not its process. For Kuhn, since subjective factors are present at the discovery phase of science, they must also be present at the justification phase.

"What the tradition sees as eliminable imperfections in its rules of choice," asserted Kuhn, "I take to be in part responses to the essential nature of science."[56] For Kuhn this meant that the objective criteria function as values, which do not dictate theory choice but rather influence it. Values help to explain scientists' behavior, claimed Kuhn, which for the traditional philosopher of science may at times appear irrational. Most importantly, values account for the disagreement over theories and help to distribute risk during debates over theories. Kuhn's position had important consequences for the philosophy of science:

> theory choice, too, can be explained only in part by a theory which attributes the same properties to all the scientists who must do the choosing. Essential aspects of the process generally known as verification will be understood only by recourse to the features with respect to which men may differ while still remaining scientists. The tradition takes it for granted that such features are vital to the process of discovery, which it at once and for that reason rules out of philosophical bounds. That they may have significant function also in the central problem of justifying theory choice is what philosophers of science have to date categorically denied.[57]

Kuhn maintained that critics misinterpreted his position on theory choice as subjective. For them, the term denotes a matter of taste that is not rationally discussable. But his use of the term does involve the discussable with respect to standards. Moreover, Kuhn denied that facts are theory independent and that there is a strictly rational choice to be made. Rather, he contended:

> that communication between proponents of different theories is inevitably partial, that what each takes to be facts depends in part on the theory he espouses, and that an individual's transfer of allegiance from theory to theory is often better described as conversion than as choice.[58]

In other words, scientists do not choose a theory based on objective criteria alone but are converted based on subjective values as well.

Hempel criticized Kuhn's approach to theory choice, arguing that Kuhn's values function "as *justifying* in a near-trivial way the choosing of theories."[59] Kuhn took exception to Hempel's evaluation of "near-trivial" and defended the role of subjective values in theory choice. To that end, he relied on two characteristics of language. The first is "local linguistic holism." This is the notion that terms are not learned in isolation from each other but in clusters. The second is that some terms are necessary in a fundamental sense, which Kuhn failed to specify analytically, and that any change in their meaning changes the meaning of other contingent terms.

Kuhn employed an analogy in which he demonstrated how the term "science," for example, is determined. He noted that in terms of local linguistic holism, "one recognizes a group's activity as scientific (or artistic, or medical) in part by its resemblance to other fields in the same cluster and in part by its difference from the activities belonging to other disciplinary clusters."[60] For the term "science," or for any linguistic term, then, there is a referent with certain properties that determines its usage,

> 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 vocabulary of disciplinary characteristics permit its identification as, say, science; for that vocabulary alone can locate the activity close to other disciplines other than science. That position, in turn, is a necessary property of all referents of the modern term "science".[61]

Kuhn made a similar argument for the role of values in theory choice. Just as a person can demarcate one discipline a science and another not, so a scientist can decide between two scientific theories. But is this position irrational? No, claimed Kuhn, because the two terms, rational and justification, are part of the same cluster of terms. Thus to speak of rational justification is to engage in redundancy. He needed to meet only one of the terms in order to meet both. Kuhn's concern was not for justifying learning from experience but for explaining "the viability of the whole language game."[62]

Although Hempel found Kuhn's defense illuminating, he still had a major reservation. "Kuhn's construal presupposes" he claimed, "the availability of a widely shared language-cum-theory about science—a dubious assumption, considering the conflicting conceptions of science in vogue today."[63] Thus, the issue for Hempel, as for a scientist attempting to choose between two competing theories, was which language counts? Hempel admitted that although critical reasoning cannot yield unconditional justification, it still is required.

Kuhn took another approach to theory choice by illustrating the change the new historiography brings to the epistemological question for philosophers of science. That change for Kuhn was a developmental process for the generation of scientific knowledge. The epistemological question for traditional philosophers was, "Why should one believe a given body of knowledge claims?"[64] But now historically enlightened philosophers ask, "Why should one shift from one body of knowledge claims to another?"[65] The answer to the latter question does not involve evaluative criteria as absolutes, when comparing a theory to the empirical evidence. Rather, such criteria function in the comparison of one theory with another, in terms of such evidence.

Finally, Kuhn discussed theory choice with respect to the incommensurability thesis. The question he entertained is what type of communication is possible among community members holding competing theories. "Clearly," answered Kuhn, "communication could not be full."[66] This raised a second, and more important, question for Kuhn and his critics: "Can good reasons for preferring one theory to another nevertheless be transmitted across the lexical divide?"[67] The answer would be straightforward if the divide was complete, but it is not. Rather, there is considerable overlap or homology among the lexicons of competing theories. For Kuhn, this situation meant that ultimately reasonable evaluation of the empirical evidence is not compelling for theory choice and, of course, raised the charge of irrationality, which he denied.

Incommensurability thesis

Kuhn claimed that there are two common misconceptions of his version of the incommensurability thesis. The first is that if two incommensurable theories cannot be stated in a single language, then how can they be compared so as to choose between them? The second is that if an older theory cannot be translated into modern expression, how then can anyone talk about it meaningfully?

Kuhn addressed the first misconception by distinguishing between the literal sense of incommensurability as "no common measure" and his sense as "no common language." "The claim that two theories are incommensurable," according to Kuhn, "is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue of loss."[68] Most theoretical terms are "homophonic" and can have the same meaning in two competing theories. Only a handful of terms are incommensurable or untranslatable. Kuhn considered this a more modest version of the incommensurability thesis, calling it "local incommensurability" and claimed it was originally his intention. Although there may be no common language to compare terms that change their meaning during a scientific revolution, there is a partially common language composed of the invariant terms that do permit some semblance of comparison. Thus, the first criticism fails. But, and this was Kuhn's main point, there is a residue that is still unaccounted for even with the use of the partially common language.[69]

As for the second misconception, Kuhn claimed that critics conflate the difference between translation and interpretation. The conflation is understandable since translation often involves interpretation. Translation for Kuhn is the process by which a "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."[70] Interpretation, however, involves attempts to make sense of a statement or to make it intelligible. Incommensurability, then, does not mean that a theoretical term cannot be interpreted, i.e. cannot be made intelligible; rather, it means that the term cannot be translated, i.e. there is no equivalent for the term in the competing theoretical language. In other words, in order for the theoretical term to have meaning the scientist must go "native" in its use.

Kuhn also took issue with Quine's translation manual with respect to the context in which a term in one language is substituted for a term in another language.[71] First, a term may be ambiguous vis-à-vis context, i.e. a term may have more than one meaning and the context is required to determine it. Second, a term in one language is conceptually disparate from a term in another language, i.e. a term in one language is not equivalent to any other term in another language. In other words, translation is not possible because the intention or sense of a term in one language cannot be captured by a term in another language. Thus, translation for Kuhn required more than simply identifying a term's referent; it required also identifying its intention. In fact, a term's intention is the basis for its referring since it is responsible for structuring the world. A translation for Kuhn then must preserve not only a term's referent but also its intention or sense. Quine's translation manual fails, then, because it considers only the ambiguity of terms and not their intention.

Kuhn introduced the notion of a lexicon and its attendant taxonomy to capture both a term's reference and intention or sense. In the lexicon, there are referring terms that are interrelated to other referring terms (the holistic principle). Now the lexicon's structure of interrelated terms resembles the world's structure in terms of its taxonomic categories. The lexicon then can be used to describe and explain the world in terms of this taxonomy. And members of a community or of different communities must share the same lexicon, if they are to communicate with one another: "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."[72] Moreover, claimed Kuhn, if full translation is to be possible, the two languages must share a similar structure for their respective lexicons. Incommensurability, then, reflects lexicons that have different taxonomic structures by which the world is carved up and articulated differently.

Entrance into a scientific community requires acquisition of its lexicon. For the lexicon not only specifies terms but also provides the taxonomic groups that carve up the world. "To acquire a lexicon is thus to acquire a taxonomy," wrote Kuhn, "a knowledge of the sorts of objects and situations that do and do not populate the corresponding world."[73] Moreover, acquisition of the lexicon occurs through a holistic process, i.e. "both referring expressions and the features that permit the identification of their referents must ordinarily be learned, not one-by-one, but in interrelated, mutually dependent steps."[74]

Since the lexicon provides an entry into a professional community, "it is thus constitutive of the beliefs about which the community members may agree and disagree."[75] Moreover, since terms in the lexicon are interconnected to each other, changes in one part of the lexicon reverberate throughout the entire lexicon. According to Kuhn, what changes for the group is not experience, but the learned prerequisites of experience, the conceptual vocabulary or lexicon through which group members exchange experience and recognize their experience as the same. To say that the lexicon is constitutive of beliefs about the world is to say that it is constitutive of what may be experienced in it.[76]

Importantly, the lexicon, for Kuhn, does not determine the community's belief but rather it "is constitutive of what beliefs there can be discourse about, or evidence for, and that some key steps in the development of knowledge require changes in that constitutive foundation."[77]

Entries within a lexicon occupy a "feature space within which the referents of different terms cluster into distinction regions."[78] According to Kuhn, there is a specific distance among each lexical entry, such that those within a common taxonomic group are closest to each other than those within different groups. Kuhn called this distance a "similarity/difference metric." This metric allows comparison between lexical entries and

> provides the lexicon with a structure determined by the relative distances between the nodes at which the referents of terms cluster and to which the names of those referents are attached. Think of the structure, if you will, as a multi-dimensional lattice of nodal points, each labeled by a referring term and all interconnected by lines of different but determinate length.[79]

Corresponding with each feature of a particular lexical space, Kuhn claimed there is a "verbalizable generalization," which points in two directions. The first is into the lexicon itself and forms part of lexicon's structure. The second is into the world, occupied by the entities to which the terms refer. "Until one has assimilated a considerable number of such generalizations, articulated or not," contended Kuhn, "one cannot use the corresponding part of language."[80] Besides the constitutive elements within a lexicon, there are also contingent elements. And there is no "decision procedure" to demarcate between them. Moreover, unobservable entities also occupy feature spaces within a lexicon.

Kuhn also addressed a problem that involves communication among communities, who hold incommensurable theories, or across the historical divide. Kuhn noted that once "a community's lexicon has changed, some of the community's previously constituted beliefs can no longer even be described."[81] But this does not deter members from reconstructing their past in the current lexicon's vocabulary. Such reconstruction obviously plays an important function in the community. But the issue is that, given the incommensurable nature of theories, assessments of true and false or right and wrong are unwarranted, for which critics charged Kuhn with a relativist position—a position he is less inclined to deny.

The charge stemmed from the fact that Kuhn advocated no privileged position from which to evaluate a theory. Rather, evaluations must be made within the context of a particular lexicon. "Evaluations that can justify truth values," claimed Kuhn, "must always presuppose the lexicon with which the statements to be evaluated were framed."[82] Thus, evaluations are relative to the relevant lexicon. But Kuhn founded the charge of relativism trivial. "Perhaps that position is relativistic," admitted Kuhn, "but, if so, what has been lost?"[83] The answer, according to Kuhn, was not much: "Neither Descartes nor anyone else ever succeeded in wiping the slate clean, building up knowledge item by item from sure foundations. There are no such foundations."[84]

Kuhn acknowledged that his position on the relativity of truth and objectivity, with regards to the community's lexicon, left him no option but to take literally "my repeated locution that the world changes with the lexicon."[85] But is this an idealist position? Kuhn admitted that it appears to be, but he claimed that it is an idealism like none other. On the one hand, the world is composed of the community's lexicon, but on the other hand, "it is a world with sufficient solidity to confute those who would bend it to their individual interests or their individual worlds."[86] "Perhaps it is an idealist's world nonetheless," confessed Kuhn, "but it feels very real to me."[87]

Word and Worlds: An Evolutionary View of Scientific Development[88]

Kuhn's goal was to address the philosophical issues left over from *Structure*, especially the incommensurability thesis.

> 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.[89]

For Kuhn the incommensurability thesis was required to defend rationality from the postmodern developments. Kuhn's proposed book is divided into three parts, with three chapters in each.

In the first part, Kuhn framed the problem of incommensurability. In the first chapter, "Scientific knowledge as historical product," he presented an evolutionary view of scientific development—as the book's subtitle portends. For an evolutionary epistemology, claimed Kuhn, "scientific development must be seen as a process driven from behind, not pulled from ahead—as evolution from, rather than evolution towards."[90] This evolutionary metaphor has significant implications for foundationalism and the correspondence theory of truth, notions that Kuhn felt were no longer relevant to contemporary philosophy of science. Without an "Archimedean platform" to guide theory assessment, Kuhn proposed that:

> the only procedures available for evaluating a proposed change in current scientific knowledge require comparison of the body of knowledge which existed before the change with the mostly identical body of knowledge which would replace it if the change were accepted.[91]

In the next chapter, "Breaking into the past," Kuhn examined the problems associated with examining past historical studies in science. Based on the three case studies presented in "What are scientific revolutions?," Kuhn stated that anomalies in older scientific texts can be understood only through an "interpretative ethnographic or hermeneutical reading."[92] A vital component of the interpretive reading is "the discovery that some sets of interrelated words in the texts under scrutiny once functioned in ways systematically different from the ways in which they later came to be used."[93] He had now laid the spade work for examining the incommensurability thesis.

In chapter 3, "Taxonomy and incommensurability," Kuhn discussed changes in word meanings as changes in a taxonomy embedded in a lexicon. The result of these changes is an untranslatable gap between two incommensurable theories. "The way to close the resulting gap," asserted Kuhn, "is language learning, a process which terminates not in universal translatability but in bilingualism."[94] He retreated from the inclusively of the earlier language metaphor that grounded the thesis' articulation. Kuhn proposed a more chastened version of the thesis that is limited to "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."[95] Kind terms are also non-overlapping with respect to referents. Incommensurability, then, is located to these non-overlapping kind terms of separate lexical taxonomies. For a change in the meaning of a kind term results in a restructuring of a lexical taxonomy. Thus, for communication to occur between different communities they must share the same lexicon.

Kuhn continued to explore the nature of a community's lexicon in the second part, "The lexicon and its cognitive content." That nature is explicated in terms of a theory of taxonomic categories. These categories are grouped as "contrast sets" and there is no overlap of categories within the same contrast set, which Kuhn called the "no-overlap principle." Moreover, the properties of the categories are reflected in the properties of their names: "Names of categories are identifiable as taxonomic terms by lexical characteristics."[96] A term's meaning then is a function of its taxonomic category. "It follows," declared Kuhn, "that no lexicon may be enriched by adding a term that shares referents with another term in the same contrast set."[97] Hence, this restriction was the origin of untranslatability.

In chapter 4, "Substances, sortals, and the no-overlap principle," Kuhn discussed the nature of substances in terms of their sortal predicates. This move allowed Kuhn to introduce plasticity into the use of the lexicon: "No two people need to use the same sets of differentiating features in picking out individuals, but they must use differentia that pick out the same individuals."[98] Moreover, the differentiating set is not strictly conventional but relies on the world to which the "differentia" connect. In the next chapter, Kuhn extended the lexicon to artifacts, abstractions, and theoretical entities.

And in the last chapter of this part, he specified the means by which community members acquire a lexicon and "the nature and status of the knowledge of nature that possession of a lexicon necessarily provides."[99] According to Kuhn, there are five ways by which members acquire a lexicon. First, they must already possess a vocabulary about physical entities and forces. Next, definitions play little, if any role, in learning new terms; rather, those terms are acquired through ostensive examples, especially through problem-solving and laboratory demonstrations. "The learning that results from such a process," explained Kuhn, "is not, however, about words alone but equally about the world in which they function."[100] Third, a single example is inadequate to learn the meaning of a term; rather, multiple examples are required. Fourth, acquisition of a new term within a statement also requires acquisition of other new terms within that statement. Lastly, students can acquire the terms of a lexicon through different routes.

In the concluding part, "Reconstructing the world," Kuhn discussed what occurs during a change in the lexicon and the implications for scientific progress. In chapter 7, he examined the means by which lexicons change and the repercussions this has for communication among groups with different lexicons. Moreover, he explored the role of arguments in lexical change. In the following chapter, Kuhn identified the type of progress achieved with changes in lexicons. He maintained that progress is not the type that aims at a specific goal: “Phrases like ‘cutting nature closer to its joints’ cannot be fitted to the evolutionary character of scientific development. But a more instrumental concept of progress fits scientific development well.”[101]

In the final chapter, “What’s in a real world?,” Kuhn addressed the relationship between a lexicon and reality. “Insofar as the structure of the world can be experienced and the experience communicated,” wrote Kuhn, “it is constrained by the structure of lexicon of the community which inhabits it.”[102] Although the structure of the lexicon dictates the world’s structure, Kuhn denied that his position depends on a mind-dependent world, which is constructed. Rather, claimed Kuhn, the world “is entirely solid; not in the least respectful of an observer’s wishes and desires; quite capable of providing decisive evidence against an invented hypotheses which fail to match its behavior.”[103]

But Kuhn was not concerned with absolute truth but with adaptability. The world is, as it were, a niche and science helps a community adapt to it. Kuhn described his approach to science and scientific development as “post-Darwinian Kantianism.” The lexical taxonomies are similar to Kant’s categories in that both make experience possible and intelligible. However, the lexical categories can change. And supporting change in lexical taxonomies is “something permanent, fixed, and stable. But, like Kant’s *Ding-an-sich*, it is ineffable, undescribable, undiscussible.”[104] Kuhn broached the issues of relativism and realism not in traditional terms of truth and objectivity but rather in terms of “stateability.” They can be neither true nor false, for their stateability is relative to the historical community. Thus, according to Kuhn, statements from incommensurable theories that cannot be translated are ultimately “ineffable”—much like the ineffable Aunt E—.

Notes

1. Heilbron and Kuhn (1969), 212.
2. *Ibid*, 283.
3. Kuhn (1987a), p. 254.
4. Galison (1981), 72.
5. Klein *et al* (1979), 432.
6. *Ibid*, 437.
7. Nicholas (1982), 297.
8. Kuhn (1984b), 231.
9. *Ibid*, 233.
10. *Ibid*, 246.
11. *Ibid*.

12. *Ibid*, 248.
13. Kuhn (1968), p. 75.
14. *Ibid*, p. 77.
15. *Ibid*, p. 81.
16. *Ibid*, p. 82.
17. Kuhn was also concerned with the future of the newly developed history of science vis-à-vis traditional history. Although historians recognized the importance of science in the development of western thought, claims Kuhn, they often ignored or resisted its importance, with "damaging [effects], both to their own work and to the development of the history of science." Kuhn (1971), 272. Kuhn's goal was to rectify this problem by discussing the recent revolution in the historiography of science and advances in the sociology of science. Although separatist attitudes and stances of historians and of historians of science often produced hostility between members of the two disciplines, Kuhn was optimistic that rapprochement was possible.
18. Kuhn (1977c), p. 4.
19. *Ibid*, p. 10.
20. *Ibid*, p. 11.
21. *Ibid*, p. 12.
22. *Ibid*, pp. 17–18.
23. Kuhn (1986), 33.
24. *Ibid*.
25. Kuhn (1992), pp. 14–15.
26. *Ibid*, pp. 18–19.
27. MIT MC240, box 5, folder 9, "Puzzles vs. problems in scientific development," p. 8.
28. *Ibid*, p. 10.
29. *Ibid*.
30. *Ibid*, p. 13.
31. *Ibid*, p. 15.
32. *Ibid*, p. 8.
33. *Ibid*, p. 19.
34. *Ibid*, pp. 19–20.
35. *Ibid*, p. 20.
36. MIT MC240, box 5, folder 14, "Does knowledge 'grow'?," p. 9.
37. *Ibid*.
38. *Ibid*, p. 10.
39. *Ibid*. The similarity here with Lakatos' notion of a "research programme" is inescapable.
40. *Ibid*, pp. 10–11.
41. *Ibid*, p. 11.
42. *Ibid*, p. 13.
43. *Ibid*.
44. *Ibid*, p. 19.
45. *Ibid*.

46. *Ibid.*
47. Kuhn (1987b), p. 9.
48. *Ibid*, p. 19.
49. *Ibid*, p. 20.
50. *Ibid.*
51. *Ibid.* For further comments on metaphor, see Kuhn (1979).
52. *Ibid*, p. 21.
53. Kuhn (1977d), p. 322.
54. *Ibid*, p. 324.
55. *Ibid*, p. 327.
56. *Ibid*, p. 330.
57. *Ibid*, p. 334.
58. *Ibid*, p. 338.
59. Hempel (1983a), p. 91. In comments on Kuhn's Machette lecture, John Post took issue with Kuhn's assertion that the community's collective judgement of appropriately trained practitioners should prevail in the absence of criteria for theory choice. Post contended that the training of scientists, even with the rigors of scientific education, does not warrant that the best decision is made. "At most," wrote Post, "it would ensure that there is a greater likelihood of this occurring than in some other sorts of groups." MIT MC240, box 5, folder 6, "Comments on Prof. Kuhn's Objectivity, value judgment, and theory choice."
60. Kuhn (1983a), 567.
61. *Ibid.*
62. *Ibid*, p. 570.
63. Hempel (1983b), 571.
64. MIT MC240, box 23, folder 21, "Scientific development and lexical change," p. 12.
65. *Ibid.*
66. *Ibid*, p. 98.
67. *Ibid.*
68. Kuhn (1983b), 670.
69. Moreover, Kuhn was reticent to restrict incommensurability to too small of a locality. "It is simply implausible," asserted Kuhn, "that some terms should change meaning when transferred to a new theory without infecting the terms transferred with them." *Ibid*, 671.
70. *Ibid*, 672.
71. Kuhn also took issue with Kitcher's context-dependent phrases for referring terms. According to Kuhn, some terms of older theories do not refer vis-à-vis a newer theory, and these non-referent terms are not eliminable from a translation. They are irreducible and require incorporation into a translation, if the meaning of the older theory is to be preserved. Thus, incommensurability cannot be avoided simply by equating translation with referent determination or context-dependent phrases. Kitcher defended his strategy of context-dependent interpretation of texts by arguing that a

context-sensitive interpretation can provide a point of entry into a textual passage by specifying the referents of its terms. But Kuhn retorted that Kitcher is "badly mistaken" concerning the incorporation of alien terms into a language.

72. Kuhn (1983b), 683.
73. MIT MC240, box 23, folder 21, "Scientific development and lexical change," p. 55.
74. *Ibid*, p. 57.
75. *Ibid*, p. 60.
76. *Ibid*, p. 62.
77. *Ibid*, p. 63.
78. *Ibid*, p. 67.
79. *Ibid*, p. 91.
80. *Ibid*, p. 69.
81. *Ibid*, p. 84.
82. *Ibid*, p. 110.
83. *Ibid*, p. 111.
84. *Ibid*.
85. *Ibid*, p. 120.
86. *Ibid*, p. 123.
87. Arthur Fine contended that Kuhn's relativism leads to a contructivist position in contrast to a realist one.

> The changes brought about by a scientific revolution may seem to call us to settle the following question: does the revolution amount to different beliefs about essentially the same things, or, are we now really talking about significantly different things. The realist wants to settle the question in the first way and Kuhn, I believe, goes the second way. (MIT MC240, box 23, folder 26, Fine's handwritten comments, p. 5)

For Fine, drawing upon the relationship of Newton and Einstein:

> the alternatives here—change of belief *vs* change of reference—are not differentiated by the material we can extract from the history of science. They are, if you like, contaminants in an analysis of science that is overly philosophical. Fortunately the alternatives are easy to avoid, and things are better if you do. (*Ibid*, p. 17)

88. As noted earlier, this was the title Kuhn proposed in the 1989 NSF grant proposal, which differs from the one proposed in his 1990 PSA presidential address. The following reconstruction of Kuhn's book is taken from the "Dubbing and redubbing" paper, the NSF grant proposal, and the PSA presidential address.
89. Kuhn (1991), 3.
90. *Ibid*, 7.
91. MIT MC240, box 20, folder 13, "NSF application," p. 4.
92. *Ibid*.
93. *Ibid*, p. 5.
94. *Ibid*.

95. Kuhn (1991), 4.
96. MIT MC240, box 20, folder 13, “NSF application,” p. 5.
97. *Ibid.*
98. *Ibid.*
99. *Ibid*, p. 6.
100. Kuhn (1990), p. 302.
101. MIT MC240, box 20, folder 13, “NSF application,” p. 6.
102. Kuhn (1991), 10.
103. *Ibid.*
104. *Ibid*, 12.

Chapter 6

What is Kuhn's legacy?

It is safe to say that there is not an academic discipline not influenced by Kuhn's paradigm. "Like a virus," observed Horgan, "the word had spread beyond the history and philosophy of science and infected the intellectual community at large, where it came to mean virtually any dominant idea."[1] Kuhn recognized that part of the reason other disciplines appropriated his ideas, especially paradigm and paradigm shift, was that it provided them with a means to claim a status comparable to that for the natural sciences. In this chapter, the legacy of Kuhn's impact on selected disciplines is briefly explored.[2]

History and philosophy of science

Kuhn's legacy to the history and philosophy of science is nonpareil. The incommensurability thesis alone still elicits considerable attention.[3] In the introduction to the "Kuhnfest" papers, Paul Horwich paid homage to Kuhn's impact on the discipline. "Kuhn's radical views," acknowledged Horwich, "have been the focus of much debate not only by philosophers, historians, and sociologists of science but also by large numbers of practicing scientists. Nevertheless, many questions remain unsettled regarding their precise nature and validity."[4] The purpose of the "Kuhnfest" was not only to honor Kuhn's legacy to the history and philosophy of science but also to draw attention to remaining unsettled questions. The papers from the conference, then, are used to represent Kuhn's legacy to the discipline.[5]

History of science

Heilbron examined an historical study first investigated by his graduate mentor Kuhn.[6] He extended Kuhn's analysis with several important "lessons," concerning the transition from qualitative (classical) to quantitative (experimental or mathematized) physics in the late eighteenth to early nineteenth centuries. The lessons included the wide-open nature of mathematics, the conservative nature of mathematicians regarding the fundamentals of their discipline, and the "mathematizing" of physics. Heilbron also paid tribute to Kuhn, praising him for his

> passionate interest in the work of his graduate students. Eager to know how our research results would fit his general ideas, he gave us to understand that we were engaged in an intellectual adventure of great moment. Some of us think we still are.[7]

N. M. Swerdlow discussed Regiomontanus' 1464 inaugural oration to a series of lectures on astronomy. Swerdlow was motivated by Kuhn's analysis of the scientific revolution, in which physics was transformed from a classical form in which mathematics was less concerned with quantifying natural phenomena to a more modern form in which mathematics is used to manipulate the quantification of nature. Swerdlow concluded, in terms that echo Kuhn's analysis of Copernicus:

> What we have in the oration, in the prospectus, and indeed in Regiomontanus' very technical works, is something that belongs to its own time, the Renaissance, with values and virtues of its own that cannot be understood if we regard it only as an early part of the scientific revolution.[8]

Utilizing nineteenth-century electromagnetism, Jed Buchwald commented on translation among incommensurable theories. According to Buchwald, translation occurs infrequently among scientists; rather, scientists often expropriate selected segments of previous theories as an untranslatable core. He warned that such expropriation over time may lead to nonsense. Buchwald also acknowledged the problems associated with Kuhn's relativism; however, he concluded that scientists embroiled in controversy each have independent reasons for holding their respective positions and "that relativism, of a kind, where the initial generation and diffusion of a novel scheme is concerned simply cannot be avoided in many cases."[9] But once consensus is achieved a scientist is considered "irrational" to resist the newly established codification.

Norton Wise explored the role of technologies in mediating the transformation of local knowledge into knowledge networks. Using a case from Enlightenment France, Wise demonstrated how the calorimeter specifically mediated between different subcultures, such as chemistry and physical astronomy. The knowledge network technologies that arose eventually stabilized as the technologies became transparent, giving the impression of a direct connection between theories and reality. Wise noted that there are two historiographic poles for the transformation of Enlightenment science: Lovejoy's and Gillispie's history of ideas and Kuhn's and Heilbron's history of measurement. He found both wanting. First, the measurement tradition fails to realize that "measurements are cultural expressions. Their significance must be understood in cultural terms."[10] And, the history of ideas tradition fails to consider what makes a culture function.

Philosophy of science

John Earman compared Carnap's and Kuhn's versions of relativism, and puzzled over Kuhn's approach to community consensus. The puzzle was how consensus is possible when shared values are differentially applied by individual community members. According to Earman,

> major scientific revolutions . . . needn't be seen as forcing a choice between incommensurable linguistic/conceptual systems, since it is often possible to fit the possibilities into a larger scheme that makes the theories commensurable to the extent that confirmation questions can be posed in terms of an observation base that is neutral enough for assessing the relative confirmation of the theories.[11]

In homage to Kuhn, Michael Friedman acknowledged that:

> Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) forever changed our appreciation of the philosophical importance of the history of science. Reacting against what he perceived as the naively empiricist, formalist, and ahistorical conception of science articulated by the logical positivists, Kuhn presented an alternative conception of science in flux.[12]

The current position in the philosophy of science is that the alternative conception of science has superseded the traditional conception. By exploring the relationship between the history of science and the development of modern philosophy, Friedman came to the conclusion, however, that "the currently popular diagnosis of the failure of logical positivism (a diagnosis due largely to the work of Kuhn and his followers) is fundamentally misleading."[13]

Ernan McMullin took Kuhn to task for defending a form of rationality but not realism, not that the two go together. According to McMullin, Kuhn's reliance on values for theory choice could have easily led him to a realist's position. Rehearsing the events of the Copernican revolution, McMullin concluded that "Copernicus and those who followed him believed that they had good arguments for the reality of the earth's motion around the sun."[14] McMullin's conclusion was based on the use of "super-empirical" values, which carry "special epistemic weight" in theory choice. McMullin acknowledged that Kuhn's legacy is a divided one, in that Kuhn tried "to maintain the rational character of theory choice in science while denying the epistemic character of the theory chosen . . . Thirty years later, *The Structure of Scientific Revolutions* still leaves us with an agenda."[15]

Nancy Cartwright invoked Lessing's fable theory to explicate the relationship between the abstract and the concrete. Just as a moral is fitted out by a fable, so the abstract is fitted out by the concrete. "Precisely this idea of fitting out shows us why," wrote Cartwright, "Kuhn argues students need to practice problems."[16] By solving exemplary problems, students gain insight into the fit between the abstract and the concrete. Cartwright utilized the notion of model to instantiate that relationship: "Models make the abstract concepts of physics more concrete. They also help to connect theory with the real world."[17] Models accomplish this task through influencing the design of experiments, which affects the form of natural laws. The drawback is that the laws only apply to ideal conditions, with no guarantee how the law operates outside the laboratory.

Hacking acknowledged that Kuhn's notion of world changes presents a philosophical quandary: although the natural world does not physically change after a scientific revolution, scientists work in a different world. Hacking dubbed it the new-world problem and proposed a nominalistic solution, in which there are different stances or platforms rather than different worlds. Hacking identified the unchanging world with individuals and the changing world (stance or platform) with scientific kinds, instead of natural-term kinds. He concluded with a challenge:

> It will prove to be a rich field of enquiry, for future philosophers of experimental science, to study how the introduction of a kind of instrument alters the world in which the experimenter works not by having a new pile of physical stuff held together with string and sealing wax but by having an instrument of a new kind, with which certain types of intentional behavior become possible.[18]

Hempel recounted his relationship with Kuhn, beginning in 1963, as colleague and eventually as friend. He acknowledged that compared to the logical analysis of scientific methodology he practiced for most of his career: "Kuhn's approach to the methodology of science was of a radically different kind."[19] That radical approach to science was naturalistic in which Kuhn examined not only the rational but also the psychological and sociological dimensions of science. He closed by reassuring Kuhn:

> Whatever position your colleagues may take, Tom, I am sure that they all feel a large debt of gratitude to you for your provocative and illuminating ideas, and all of us in the audience await with keen interest your thoughts.[20]

Kuhn's "Afterwords"

Kuhn acknowledged Hempel's work on his own, but

> what I primarily owe to him is not from the realm of ideas. Rather it is the experience of working with a philosopher who cares more about arriving at the truth than winning arguments. I love him most, that is, for the noble uses to which he puts a distinguished mind.[21]

Kuhn was particularly grateful to Swerdlow for enlightening historians as to the problems associated with such phrases as "medieval physics and chemistry." However, he took issue with Heilbron's suggestion that such phrases be marked in some fashion, such as italicizing, in order to indicate their function in the narrative. "The danger," warned Kuhn, "in using the names of contemporary scientific fields when discussing past scientific development is the same as that of applying modern scientific terminology when describing past belief."[22] Kuhn enumerated the similarities between Buchwald's paper and his current book project. The most important was between Buchwald's notion of the unarticulated core and his notion of the lexicon. Kuhn commented on Wise's story about network nodes, which hold "between practices in the various scientific fields as well as between them and the larger culture."[23] He announced that Wise's thesis had "converted" him to the importance of cultural factors in scientific development; but he maintained that there are key parts to Wise's story that are absent, such as to what is a rationalist scientific culture and the relationship between it and its individual members.

Kuhn admitted that his knowledge of logical positivism early in his career was "decidedly sketchy" and that he was ignorant of the post-*Aufbau* Carnap. Moreover, when he received Carnap's letter about *Structure,* "I interpreted it," claimed Kuhn, "as mere politeness, not as an indication that he and I might usefully talk."[24] Moreover, the similarity Earman finds between Kuhn and Carnap, when examined more closely, reveals a deep divide between them. While Carnap is concerned about the pragmatic implications of a notion of translation between different theories, Kuhn pressed the notion into the service of a developmental view of science. Kuhn appreciated Friedman's essay and looked forward to the complete story.

Kuhn sympathized with McMullin's concern over a possible realist interpretation of Kuhn's efforts, in order to maintain the rationality or epistemic character of scientific values. But he claimed that Friedman's interpretation of Kant's *a priori* as a relativized notion provided him with a way to keep an instrumental position. For Kuhn, in contrast to Cartwright, the transmission of the lexicon from one generation to the next through concrete exemplars is critical for understanding scientific development. "What is acquired in this process is, of course," wrote Kuhn, "the kind-concepts of a culture or subculture. But what comes with them, inseparably, is the world in which members of the culture live."[25] Kuhn discussed Hacking's proposed nominalist solution to the new-world change problem and rejected it, since "there are real individuals out there, and we divide them into kinds at will."[26] Kuhn also rejected Hacking's scientific-kind terminology, since it is too restrictive.

Kuhn closed with comments on scientific revolutions. Although he still envisioned them as discontinuities on a background of normal science, revolutions now represent scientific progress through "an increase in the number of scientific specialties required for the continued acquisition of scientific knowledge."[27] For specialization is akin to speciation: "What permits the closer and closer match between a specialized practice and its world is much the same as what permits the closer and closer adaptation of a species to its biological niche."[28] But Kuhn's position again raised the specters of rationality and realism. Is this process governed rationally? Does "closer" refer to the real? To these questions Kuhn answered puzzle-solving, which provides greater understanding of the world. But there is so much more than the rational and the real, concluded Kuhn, there is also the political and social interests that must be brought to bear on puzzle-solving.

Natural sciences

Natural scientists used Kuhn's notion of paradigms to reconstruct the history of a discipline. Reconstruction of the Darwinian revolution along Kuhnian lines is used to illustrate this function. Scientists also used Kuhn's paradigms to establish their discipline as a science. The founding of molecular biology is used to illustrate this function.

John Greene was one of the first to use Kuhn's notion of paradigm to reconstruct the development of Darwin's evolutionary theory. He began by describing the first paradigm in natural history, the Linnaean classificatory system, which represents natural history in static terms. Although anomalies to the system emerged, they were ignored or evaded. Later, Buffon and Lamarck proposed competing paradigms not necessarily in response to the anomalies, but these proposals eventually resulted in a crisis that lead to replacement of the static Linnaean paradigm with the Darwinian evolutionary theory.

But the use of Kuhn's paradigm notion was not completely adequate. For example, the Darwinian revolution represented not only a break with the past natural history paradigm but also a continuation of it. "The Kuhnian paradigm of paradigms," concluded Greene, "can be made to fit certain aspects of the development of natural history from Ray to Darwin, but its adequacy as a conceptual model for the development seems doubtful."[29] "On the whole the paradigm doesn't work very well," confessed Greene years later, for reconstructing the Darwinian revolution.[30] Moreover, Greene claimed that although he used Kuhn's terminology throughout the essay, that usage did not represent approval of Kuhn's historiography. Although Greene found Kuhn's historiography inadequate, he believed it is the best available and better than nothing at all. "Those who question the validity of Kuhn's model," asserted Greene, "should feel themselves challenged to provide alternative interpretations to the genesis of revolutions in science."[31]

Ernst Mayr utilized Kuhn's notion of paradigm to evaluate the reason why the Darwinian revolution was so slow in being accepted. In a characteristically Kuhnian move, Mayr examined the resistance to Darwin's ideas. "It was not the lack of supporting facts, then, that prevented the acceptance of the theory of evolution," noted Mayr, "but rather the power of the opposing ideas."[32] In Kuhnian terms, scientific evidence was not sufficient to induce theory change; rather, non-rational factors were also important. Mayr then rehearsed the various ideologies scientists and non-scientist marshaled in resisting Darwinian evolution. He insisted that often specialists were blinded by the dogma with which they were indoctrinated, in compliance with Kuhn's analysis of why scientists resist a new paradigm.

Mayr also noted two differences between the Darwinian revolution and other scientific revolutions, especially those in the physical sciences. First, the Darwinian revolution required not only the replacement of one paradigm by another but also the rejection of six other paradigms. Second, it had a profound ethical and religious impact on society far more than other scientific revolutions. Mayr then drew two conclusions from his analysis concerning scientific revolutions. First, they occur over long periods of time in which parts of the older paradigm are replaced piecemeal by parts of the newer paradigm. Second, the combination, and not merely the addition, of the various elements of a revolution dictate the revolution's content and form.

In conclusion, Mayr claimed that "the Darwinian revolution does not conform to the simple model of the scientific revolution, as described, for instance, by T. S. Kuhn."[33] He argued that the Darwinian revolution was a complex event that took 250 years, with its major elements appearing at various times. However, he did concede that Darwin's notion of evolution was based on the incommensurable shift from perfection to undirected change. "The result," wrote Mayr, "was an entirely different concept of evolution. Instead of endorsing the eighteenth-century concept of a drive toward perfection, Darwin merely postulated change."[34]

In a pivotal 1970 article, Eugene Hess explored the origins of molecular biology. He claimed molecular biology is a legitimate scientific discipline, both by reviewing its recent rise to the status of a science and by seeking a philosophical justification for it. That justification came in the form of Kuhn's notion of paradigm.

> The molecular approach to biology has provided, nevertheless, a unifying paradigm to guide an active and productive group of researchers, and, as Kuhn has argued ably, it is hard to find another criterion which so clearly proclaims a field of science.[35]

He then proceeded to map the emergence of the molecular biology paradigm, which includes important ideas and assumptions as well as techniques and instrumentation. Hess concluded with a formulation of that paradigm: "biological structures are organized on a molecular basis."[36]

Once in place the molecular biology paradigm was seen by molecular biologists to inaugurate a period of fruitful discoveries, which had an immediate impact across the board for the biomedical and evolutionary sciences. Michel Morange compared this productive period to normal science.

> Once researchers had deciphered the genetic code and described regulatory mechanisms in micro-organisms, molecular biology entered what the historian of science Thomas Kuhn has called a period of "normal science." Research no longer involved testing global models but "puzzle-solving" within the framework of existing theories.[37]

Indeed, the molecular biology paradigm is being applied successfully to puzzles ranging from evolution and taxonomy to gene therapies. The potential of the molecular biology paradigm seems to have no limits. As Marshall Nirenberg wrote, "the revolution in molecular genetics has created tremendous opportunities to do research that surely will lead to fundamental advances in knowledge of normal and pathological processes."[38]

Sociology

Kuhn's notion of paradigm was greeted enthusiastically by sociologists, who utilized the notion to assess the development of their discipline *qua* science. For example, Robert Friedrichs proposed that sociology is composed of first- and second-order paradigms. Clashes between paradigms over the subject matter of sociology, such as between the system paradigm and the conflict paradigm, are a function of first-order paradigms, in which the sociologist is envisioned as the "scientific agent." "The paradigms that order a sociologist's conception of his subject matter . . . ," explained Friedrichs, "may themselves be a reflection, or function, of a more fundamental image: the paradigm *in terms of which he sees himself.*"[39] That paradigmatic image may be either as priest or prophet, and it is that image which determines whether one is committed to either the system or conflict paradigm.

George Ritzer also applied Kuhn's paradigm notion to sociology and concluded that the discipline is a "multiple paradigm science," i.e. "there are several paradigms vying for hegemony within the field as a whole."[40] He identified three sociological paradigms: social facts paradigm, social definition paradigm, and social behavior paradigm. Proponents of the social facts paradigm are concerned with the development of abstract theory, such as the functionalist, system, or conflict theory, through empirical or statistical methods. Proponents of the social definition paradigm reject abstract theory and are concerned with specific skills that allow them to observe accurately social phenomena and to discredit social myths. Action theory, symbolic interactionism, and phenomenological sociology constitute this paradigm. The social behavior paradigm straddles the other two paradigms. Its proponents engage in abstract theory, such as behavioral or exchange theory, through experimental means and, in turn, use these theories to improve society.

In a review of sociologists' use of paradigms, Douglas Eckberg and Lester Hill claimed that their efforts obfuscate the paradigmatic status of sociology. "The results of these attempts," insisted Eckberg and Hill, "have been far from satisfactory. In fact, there are almost as many views of the paradigmatic status of sociology as there are sociologists attempting such analyses."[41] They proposed that the confusion over sociology's status is a result of the misuse or misunderstanding of Kuhn's notion of paradigm. For example, they claimed that Ritzer's misunderstands paradigm as disciplinary matrix, whereas Kuhn intended it as exemplars. The important question, then, was whether sociology is paradigmatic in terms of exemplars. According to Eckberg and Hill, the answer is no. They qualify their analysis claiming that exemplars may be possible someday for sociology, but they must be localized to an area of research within a sociological subdiscipline and attract practitioners who utilize them to solve puzzles.

In response to Eckberg and Hill, Ritzer claimed that defining paradigm as disciplinary matrix than as exemplar serves his purposes better for explicating the structure of sociology. He outlined this structure in terms of the intersection of the macroscopic/microscopic and objective/subjective continua, which yield four levels of social reality. Upon these four levels he mapped the three earlier paradigms along with a fourth integrated sociological paradigm.[42] "In conclusion," wrote Ritzer, "I would argue that being a Kuhnian purist leads one to focus on exemplars in sociology but that Kuhn's concept of a disciplinary matrix is a more useful tool for understanding the metatheoretical status of sociology."[43] Hill and Eckberg responded to Ritzer, reclaiming that, as Kuhn himself insists, the central idea of paradigm is the notion of exemplar. "And finally," they concluded, "what does it mean to be interested in the 'paradigmatic status of sociology' if the paradigm concept is borrowed from Kuhn, but Kuhn's major arguments/implications are rejected?"[44]

Besides general sociology, Kuhn's philosophy of science was instrumental in terms of a new school of sociology of science called the sociology of scientific knowledge or SSK.[45] As these sociologists acknowledged, they are not interested in praising the natural sciences but want to turn them onto themselves. Thus, the agenda of SSK scholars was to shake the very foundations of these sciences and to question their privileged position in society, in terms of both their access to and pronouncements on the natural world. From their analyses of the natural sciences, SSK scholars concluded that scientific knowledge is not discovered but constructed, created, or manufactured. Kuhn was not sympathetic to SKK, especially the strong program, and believed that notions like truth and knowledge must be defended from its "excesses."[46]

An outgrowth of SSK was the science wars.[47] Scientists and their sympathizers eventually responded to SSK. Paul Gross and Norman Levitt, for example, provided a highly publicized—and rather sensational—response to SSK's cultural constructivism. From their critique of SSK's constructivism, they concluded: "this point of view rigorously applied leaves no ground whatsoever for distinguishing reliable knowledge from superstition."[48] They located part of the blame for constructivism with a distorted interpretation of Kuhn's philosophy of science.[49] SSK scholars responded to Gross and Levitt and others. For example, Andrew Ross decried the caricature of the cultural and social studies of science. Ross defended the postmodern position(s), declaring that the

> class-soaked pronouncements about the return of the Dark Ages among the ill-educated masses are intended to reinforce the myth of scientists as a beleaguered and isolated minority of truth-seekers, armed only with objective reasoning and standing firm against a tide of superstitions.[50]

Economics

Economists used Kuhn's philosophy of science to examine their discipline's history and methodology. For example, Donald Gordon proposed that economics' fundamental paradigm is Adam Smith's "postulate of the maximizing individual in a relatively free market."[51] He claimed that Smith's paradigm is still viable after two centuries and that economics has yet to undergo a major revolution, although it has had "major, if unsuccessful, rebellions."[52] According to Gordon, "economic theory is very much like a normal science."[53] A. W. Coats asserted that "the theory of economic equilibrium via the market mechanism" is economics' principal paradigm.[54] In contrast to Gordon, Coats insisted that economics underwent a Keynesian revolution in the 1930s, which exhibited "many of the characteristics associated with Kuhn's 'scientific revolutions'."[55] However, he noted that "it is now clear that the Keynesian paradigm was not 'incompatible' with its predecessor."[56]

The Keynesian revolution was the predominant example for economists of a Kuhnian revolution. For example, Ron Stanfield purported that the economic theory articulated by Keynes represents a Kuhnian-like revolution in economics. In essence, the Keynesian revolution resulted in a change of the types of puzzles economists tackled, which ultimately led to a change in their worldview. He went on to list other features of the Keynesian revolution that supported the conclusion that the Keynesian revolution was Kuhnian in nature. "Keynesian normal science," for example, ". . . was sufficiently open-ended to allow substantial articulation."[57]

Michel de Vroey provided a Kuhnian analysis of the transition from classical to neoclassical economics. "In the last years," he acknowledged, "studies applying the Kuhnian framework to economic science have flourished, and no discussion about the history of economic theories takes place without at least a reference to Kuhn."[58] According to de Vroey, the classical and neoclassical economics are "coherent and specific" paradigms, and the transition from classical to neoclassical economics represents "a scientific revolution a la Kuhn rather than as a scientific advance in a Popperian way through a process of criticism and falsification of existing laws or assumptions."[59] He also proposed a cause for the transition. "The new paradigm," wrote de Vroey, "was especially attractive because it looked as scientific as the natural sciences theories."[60]

Alfred Eichner and Jan Kergel claimed that the post-Keynesian theory, as developed by Keynes' Cantabridgian associates and a younger generation of economists, represented "in Thomas Kuhn's sense . . . a new paradigm."[61] Although they acknowledged that there is no single neoclassical theory by which to compare the post-Keynesian theory, the neoclassical theories do share sufficient common features to allow comparison with the post-Keynesian paradigm.

> As Kuhn's work brings out, it is difficult to choose between alternative paradigms—especially when the newer one is still in an inchoate state—even if there is agreement that the purpose of a theory is to explain the empirically observable world. When there are two alternative paradigms, each designed to serve a quite different purpose, the task of choosing between them is further complicated.[62]

Economists used Kuhn during the 1970s, but

> the application of Kuhn's arguments to the history of economic thought has encountered serious difficulties, difficulties which can be linked both to the ambiguities of the Kuhnian definition of a "paradigm" and to its origins in the history of the natural science. So much so that the characteristics of a truly Kuhnian revolution in the history of economic thought have only been identified, and then not without controversy, in the Keynesian revolution.[63]

Kuhn was eclipsed by another philosopher of science, Lakatos. As Deborah Redman recognized, "the Kuhnian wave of the seventies is being swallowed up by the Lakatosian program."[64] Mark Blaug led the charge to replace Kuhn with Lakatos. According to Blaug, "the term 'paradigm' ought to be banished from economic literature."[65] He argued that even the best representative of a Kuhnian revolution in economics, the Keynesian revolution, is better viewed as a Lakatosian "scientific research programme".

Although economists attempted to utilize Kuhn's scientific methodology for explicating economic methodology, success was minimal. For example, Benjamin Ward applied six tests derived from Kuhn to address the issue of whether economists behave as normal scientists. Although economists passed the tests, Ward cautioned that the only reasonable conclusion is simply that "there are striking similarities between the ways in which economists behave in their professional life and the behavior of natural scientists."[66] However, other economists were less than enthusiastic about Kuhn's methodology. For example, Redman insisted that economists are not "normal" in a Kuhnian sense, since "there is no paradigm . . . that is unquestioned by all economists."[67] She maintained that consensus cannot be forced but must be attained through an attitude she calls "scientific rationalism," which involves "tolerance, honesty, commitment to the advance of science above personal advance and to the freedom to exercise criticism, a willingness to listen and learn from others."[68]

Moreover, as David Hausman noted, Kuhn's methodology is difficult in its application to economics since it is "evasive on questions of theory appraisal, which still interests most of those writing on economic methodology."[69] Theory appraisal is an important issue for economists, and Kuhn, for example, offers no criteria to account for the role of anomalies in paradigm shifts. Bruce Caldwell also claimed that Kuhn's methodology may disappoint economists, "who would prefer that methodology offer a rigorous, objective, prescriptive framework."[70] He proposed that a Popperian critical rationalism provides a more satisfactory approach to economic methodology, while others claimed that Lakatos offers a more balanced position between Popperian and Kuhnian extremes. Although economists were critical of Kuhn's methodology, they did adopt "Kuhn's account of actual scientific practice as differing significantly from the austere strictures of positivism."[71]

Political science and science policy

In a presidential address delivered to the 1965 American Political Science Association annual meeting, David Truman stated: "In thinking about the contemporary development of political science, I find particularly suggestive the notion of the paradigm, which is one of the two key concepts in Thomas S. Kuhn's *The Structure of Scientific Revolutions*."[72] He then suggested that "something loosely analogous to a paradigm characterized American political science for at least the half-century running sometime in the 1880s into the 1920s."[73] He gave no specific name to this "paradigm," other than the consensus of a "political system." But after the World War II it dissolved, leading to a crisis. He concluded with a challenge to his colleagues to forge a new paradigm.

The discussion over political science's theoretical development continued, with Kuhn's paradigm concept often fueling it. In a poem, Inis Claude, Jr. probed a recruit's scientific acumen, closing with a nod towards Kuhn's impact on political science's methodology: "Are you adept at research design—/Brother, can you paradigm?"[74] Using the last line of Claude's poem as a title for an article, Jack Walker addressed the debate over the discipline's theoretical development. "There may not be any ruling paradigm to shape their efforts," observed Walker, "but political scientists still have firm ideas about what ought to be studied and what should be ignored."[75] From a survey of articles published from the past decade in political science journals, he discovered that more than half of the articles published were on "the health and well being of democratic political institutions."[76] Walker concluded that political science does have a ruling paradigm, as long as paradigm is defined in terms of social values and commitments.

In a review of Kuhn's impact on political inquiry, Jerome Stephens noted that "most of the political scientists who have used Kuhn's ideas have been more interested in using Kuhn's authority to dub the formulations they accept as a paradigm—and the formulations of others as non-paradigms."[77] Although Stevens rejected Kuhn's original criteria for paradigm assessment for political inquiry, he did accept Kuhn's revised criteria, the use of values rather than rules, as pertinent for political inquiry. But he encouraged political scientists to avoid the fashions in the philosophy of science and to develop criteria specific for solutions to their own problems.

Political scientists' use of Kuhn became more critical as the 1970s progressed and as the discussion over methodology intensified. For example, Philip Beardsley contended that political science is multiparadigmatic rather than uniparadigmatic. But in response to criticism by Michael Kirn, Beardsley abandoned the term paradigm in favor of "general frameworks of ideas."[78] Richard Ashcroft criticized political scientists for engaging in abstract methodological discussions, especially in terms of Kuhn's paradigms. According to Ashcroft, paradigms "divorced from an empirically-grounded perspective of social-historical change are of little value for understanding the political conflict amongst groups within any specific society."[79]

Several political scientists attempted to reconstruct their discipline's history along Kuhnian lines. For example, Andrew Janos narrated the history of political science using Kuhn's paradigm and paradigm shift, since they "seem to be eminently applicable to the experience of the social sciences, and within them, to the experience of political inquiry."[80] However, James Farr claimed that the shifts in political theory are often the result of factors external to the community.

> Kuhn's sketch of scientific change—in general and, especially, in the particular case studies he provides—depends upon developments internal to the scientific community. In the end, then, we should look beyond paradigms for a narrative—and especially for a political narrative—to tell the history of political science.[81]

However, political scientists continued to invoke Kuhn, such as in the rational choice debate.[82]

Kuhn's philosophy of science also had a profound impact on science policy and often, according to scientists, with disastrous outcomes.[83] During the 1990s, the American scientific community experienced a drastic drop in research funding as the U.S. government faced financial crisis.[84] What particularly irritated scientists was their loss of prestige in the public's eye and, more importantly, of access to the government's coffers. No longer was science funded without question. Moreover, science and its practitioners were under siege from antiscience groups, who claimed that science had no privileged access to truth and therefore must compete as other disciplines for a share of the funding pie. Many scientists felt that Kuhn's philosophy of science paved the way for science's low priority at the funding trough.

One of the more devastating effects of Kuhn's philosophy on science policy, according to some scientists, was the superconducting super collider's demise in the early 1990s. In response to its demise, Steven Weinberg, who was intimately involved in the super collider project, took issue with Kuhn's philosophy of science and its impact on science's image as the means for discovering truth. Weinberg criticized Kuhn's agnostic position toward scientific progress. In contrast to Kuhn's agnosticism, Weinberg advocated a retrograde view of scientific progress. In other words, as the history of science unfolds the progress of scientific advance is now clearly visible. Specifically for Weinberg, the super collider was the next step to a unified theory of gravitation. "This is what we," declared Weinberg, "are working for and what we spend the taxpayers' money for. And when we have discovered this theory, it will be part of a true description of reality."[85] Implicit in Weinberg's critique is the charge that if Kuhn had not tarnished science's image as a means toward truth, the super collider would be smashing atoms under the Texas prairie.

Psychology

Psychologists realized early on the importance of Kuhn's philosophy of science for their discipline. In a 1966 review of *Structure*, for example, Robert Watson commented on the value of Kuhn's structure of scientific development for the history of psychology. "The present reviewer," wrote Watson, "is now working on an account of the history of psychology in which one of the guide lines is the conviction that it is a pre-paradigmatic science."[86]

Psychologists also used Kuhn's normal/revolutionary dialectic to reconstruct the history of psychology, in order to establish the scientific status of their discipline. For example, David Palermo claimed that psychology had already experienced two scientific revolutions. The first was the shift from the introspectionist paradigm to the behaviorist paradigm, particularly in American experimental psychology. Although the behaviorist paradigm reached its height in the 1940s and 1950s, anomalies appeared and a crisis ensued, with challengers competing to overthrow behaviorism. Palermo explored the various challengers and proposed that Chomsky's psycholinguistics is "the new paradigm which may replace behaviourism."[87] Neil Warren took issue with this reconstruction of psychology's history, insisting that it is

> false to conclude that behaviourism triumphed, except in a narrowly parochial point of view. It is in the imputation of parochialism that my emphasis—and my argument against Palermo—lies. For behaviourism became a dominant framework (as Palermo admits) only in the United States of America.[88]

Moreover, L. B. Briskman argued that American behaviorism "ought to be thought of as a research programme in degeneration rather than a paradigm in crisis."[89]

American psychologists continued to invoke Kuhn for reconstructing psychology's history. For example, Irving Kirsch claimed that mentalism was psychology's first, but unrecognized, paradigm. Also, Allan Buss, who found Warren's and Briskman's critique of Palermo "unconvincing," contended that psychology underwent two major revolutions, from structuralism to behaviorism and from behaviorism to cognitive psychology, along with two peripheral revolutions, the psychoanalytic revolution and the humanistic revolution.

By the mid–1980s, psychologists utilized Kuhn for more than reconstructing their history. In a citation analysis of the psychological journal literature from 1969 to 1983, S. R. Coleman and Rebecca Salamon demonstrated that psychologists use Kuhn in a selective and superficial way as a "rhetorical device . . . to magnify the significance of the author's findings, conclusions, or reflections."[90] Gerald Peterson came to a similar conclusion in a separate analysis of the psychological literature. "Psychologists," wrote Peterson, "have shown great flexibility in their use of Kuhn's ideas . . . The result has not been the elucidation of fundamental issues, or fruitful exchange, but further debate over who has the truer paradigm."[91]

In a review of Kuhn's philosophy of science, William O'Donohue encouraged psychologists to take a more critical position concerning its application. He recommended other philosophies of science that might better serve psychologists' ends. O'Donohue concluded that "if normal meta-science has been dominated by the Kuhnian paradigm, I suggest a revolution in which other philosophical paradigms are considered."[92] Some psychologists have advocated other philosophies of science. For example, Barry Gholson and Peter Barker promoted Lakatos' notion of research programmes and Larry Laudan's notion of research traditions. They suggested that these alternative models of science better account for historical episodes in the history of psychology, such as for the psychology of learning in which there is an ongoing debate between the conditioning and cognitive research programs.

Recently, Erin Driver-Linn invoked Kuhn's philosophy of science to address a continuing crisis in psychology. According to Driver-Linn, psychologists are caught between the Scylla of the rationalist's natural scientific worldview and the Charybdis of the relativist's social scientific worldview, especially as it has been played out in the contemporary science wars. "Kuhn's philosophy of science," wrote Driver-Linn, "is an appealing one to marshal—it is popular and catchy, and it strikes a balance in the war."[93] That balance is reflected in Kuhn's adherence to

> maps, or models, or theories, or results [that] can be empirically based, while acknowledging the subjectivity inherent in psychological inquiry. Results can fix the world in ways that are discernibly good or better than those of the past, without trying to make the shaky claim that psychological science is progressing toward perfect correspondence with a verifiable and objective reality.[94]

However, the real appeal of Kuhn for psychologists, according to Driver-Linn, was not a "middling position" towards truth, but a "psychologized model" of progress, especially in terms of gestalt switches and Piaget's genetic epistemology. But, she claimed, Kuhn's model of progress has misled psychologists. "These psychologists," argued Driver-Linn, "see Kuhn's statements about changes in the sciences as informing how they think about the development of individuals."[95] But, equating community and individual behavior confounds psychologists' sense of progress. Moreover, Kuhn's conflation of description and prescription also adds to psychologists' confusion, especially a model of scientific progress based on developmental stages. For Driver-Linn, psychologists' reliance on Kuhn's philosophy of science is problematic, because "in general, psychologists seem more concerned with what signifies comparative progress than with generating or maintaining a vision of where the field is going."[96]

Driver-Linn's article generated considerable criticism. For example, Christopher Green argued that psychologists are drawn to Kuhn, not because of his middling truth or psychologized progress, but because of his image as a radical. "Kuhn's youth and his apparent radicalism," claimed Green, "fit with the values of the 1960s and appealed to the psychologists of the 1970s."[97] In response, Driver-Linn proposed that a better metaphor for psychology's travails vis-à-vis scientific status is not political or even psychological but biological. "Maybe the 'speciation' of psychology has yielded," wrote Driver-Linn, "along with thriving and extinct subspecialities and notable mutations, some unfortunate vestigial attributes."[98] Thus, she concluded, psychologists should look not to outside disciplines such as philosophy of science but to their own.

Science education

"Kuhn's impact on the educational research and theory," wrote an editor, "has been immense."[99] However, it was not immediate. Although *Structure* was reviewed in a 1963 issue of *The Science Teacher* by Morris Shamos, science educators basically ignored or were unfamiliar with Kuhn's new image of science. Shamos, for example, discussed the importance of Kuhn's book for the history and philosophy of science but not for science education. Moreover, at a 1968 annual meeting of the National Association for Research in Science Teaching held in Chicago, *Structure* was mentioned only once by a participant in a session on philosophy of science and science teaching and then in terms of the complexities surrounding the discovery of oxygen. Moreover, John Robinson, who presented a paper—"Philosophy of science: implications for teacher education"—at the Chicago meeting and whose doctoral dissertation was recently published, *The Nature of Science and Science Teaching*, did not mention Kuhn, or even Popper. Many of the science educators relied on traditional philosophy of science to inform their image of science.

At the start of the 1970s, Kuhn's new image of science began to receive recognition from science educators. For example, Yehuda Elkana discussed the rise of the new philosophy of science and its potential impact for science educators. Although he recognized Popper as its chief architect, he also recognized the role of a younger generation of philosophers of science who were engaged in developing the new philosophy of science, especially from a psychological perspective. Although Elkana utilized Kuhn's normal-revolutionary science dichotomy for science education, he proposed that science teaching should be "concerned only with normal science."[100] By the end of the decade, Kuhn's philosophy of science had eclipsed Popper's philosophy of science in terms of science education.

But the application of Kuhn's philosophy of science to science pedagogy was not accepted uncritically. Harvey Siegel, for example, argued that Kuhn's normal or paradigmatic science actively distorts the history of science. Moreover, he claimed that Kuhn "argues that the science educator, in order to effectively inculcate that paradigm, should systematically distort the history of science."[101] Siegel addressed two problems with Kuhn's pedagogy. First, he declared that it is "a rather pessimistic view of the student's critical capabilities."[102] Siegel claimed that students would have a greater appreciation for current scientific paradigms, if taught an undistorted history of science. Second, he found Kuhn's pedagogy morally "repugnant." "Students," objected Siegel, "are not objects with which we can, as science educators, do as we wish—they are persons, and deserve the respect of their personhood that we demand for ourselves."[103] To correct these problems, Siegel proposed an alternate pedagogical program based on an undistorted view of the history of science.[104]

But despite these criticisms, by the 1980s Kuhn's new image of science became the standard within science pedagogy. For example, Isaac Abimola, in discussing the relevance of the "new" philosophy of science for science education, used Kuhn as the source for many of its characteristics. He concluded that this new philosophy may "provide the necessary guidance to upgrade science education and research."[105] Paul Wagner also utilized Kuhn's philosophy of science to address science pedagogy. He agreed with Kuhn that the "essence" of scientific activity is puzzle-solving. "Consequently," wrote Wagner, "science education ought to equip science students with the skills necessary for puzzle solving in specific scientific domains."[106] He went on to outline three goals for science curriculum based on Kuhn's philosophy. Briefly, students should be taught the particular vocabulary, behavior pattern, and critical spirit associated with a scientific paradigm.

Derek Hodson proposed a three-stage scientific curriculum constructed along Kuhnian lines. The first stage is "pre-paradigmatic science education," in which students are taught the vocabulary and concepts of a particular scientific domain. The next stage is "within-paradigm science education." "The major goals at this stage," wrote Hodson, "would focus on learning the substantive structure of science and on acquiring and practicing the skills and procedures of normal science."[107] The final stage is "revolutionary science education." During this stage, students are taught "the creation of new theoretical ideas and investigation of the ways in which choices are made by the scientific community between rival theories."[108]

Kuhn continued to be discussed among science educators in the 1990s, with a more balanced use of Kuhn's philosophy of science emerging by the end of the decade. For example, Juli Eflin, Stuart Glennan and George Reisch encouraged science educators to expose students to Kuhn's philosophy of science, especially paradigm competition and the role of commitments and values in science. However, they cautioned that "students should be made aware that some interpretations of Kuhn's views are extreme and not persuasive (such as the popular claim of radical incommensurability between paradigms)."[109] Finally, Michael Matthews discussed the lessons learned from Kuhn's impact on science education, especially in terms of constructivism. The chief lesson, according to Matthews, was that "the science education community should be more effectively engaged with ongoing debates and analyses in the history and philosophy of science."[110]

Religion

Ian Barbour was one of the first theologians to apply Kuhn's philosophy of science to religion. Barbour discussed commitment to religious paradigms, stressing the importance of the religious community's traditions and exemplars. Both traditions and exemplars are important for defining the community and for initiating members into it. For religious communities, observed Barbour, they often revolve around a specific individual. Moreover, rather than rules for choosing among religious paradigms there are criteria of religious communities that include trust and loyalty, which often engender a deeper commitment to doctrines than criteria of scientific communities that engender commitment to theories. However, such subjective factors do not preclude critical analysis and reflection on one's religious faith. In other words, Barbour recognized a reciprocal relation between commitment and reflection for religious communities defined by a paradigm. "Commitment alone without enquiry," insisted Barbour, "tends to become fanaticism or narrow dogmatism; reflection alone without commitment tends to become trivial speculation unrelated to real life."[111]

Others also used Kuhn's philosophy of science to address religious issues. For example, Henry Veatch claimed that Kuhn and other contemporary philosophers of science provide a novel means by which to conduct apologetics, especially under the yoke of Popper's philosophy of science. According to Popper, religious statements fall on the wrong side of the demarcation divide. But Veatch invoked Kuhn to question Popper's demarcation principle and to turn the tables on Popper:

> it would seem to be scientific truth that can claim to be no more than a truth about appearances, whereas the very logic of theological truth, when rightly understood, can in all propriety claim to be a factual truth and a truth about the way things really are.[112]

For Veatch religious doctrines are necessarily true, while scientific theories are merely invented.

Cordell Strug identified two problems with theologians' reliance on Kuhn. First, theologians misrepresent his philosophy and thereby cause inadvertent damage to their discipline. For example, Veatch's reliance on Kuhn to turn the tables on Popper misrepresents Kuhn's view of science as irrational. But Strug contended that Kuhn has "a broader understanding of scientific rationality."[113] The second problem is that Kuhn's philosophy may "contain elements, unnoticed in the original environment, which will question the possibility of theology."[114] But Strug did concede that Kuhn's philosophy is useful for understanding the traditional and historical dimension of a discipline and thereby may be helpful in reconstructing religious history.

The Roman Catholic theologian Hans Küng from Tübingen used Kuhn's paradigm concept to address the question of consensus in modern Christian theology.[115] Küng claimed that theology exhibits "normal" practice guided by paradigm, its paradigm can break down leading to a crisis period, a theological paradigm is replaced only when a better one is available, acceptance of a new theological paradigm depends also on extra-rational factors and thereby resembles a conversion, and a new paradigm, if successful, becomes the new tradition. Küng also reconstructed Church history in terms of six paradigms: the early Christian apocalyptic paradigm, Patristic-Hellenistic paradigm, Mediaeval-Roman Catholic paradigm, Reformational-Protestant paradigm, Modern-Enlightenment paradigm, and the emerging Contemporary paradigm.

Küng believed that theology is currently experiencing a crisis, precipitated by several contemporary issues such as the end of western cultural hegemony, the ambiguity of science in terms of its creative and destructive capacities, and especially "*an undermining of Christianity's dominance* as the 'one true', 'absolute' religion."[116] Küng's concern was for a more conscious attempt by theologians to birth a new paradigm, "a post-Enlightenment or post-modern paradigm."[117] But he was not advocating a monolithic paradigm. "Our aim is not a rigid canon of unchangeable truths," claimed Küng, "but a historically changing canon of fundamental conditions which have to be fulfilled if theology is to take its contemporary character seriously."[118] To that end, he specified four conditions or dimensions of the new paradigm, including biblical, historical, ecumenical, and political.[119]

In the 1989–1991 Gifford lectures, Barbour extended his earlier discussion of Kuhn's notion of paradigm for reconstructing religious experience. "As in the scientific case," claimed Barbour, "a religious tradition transmits a broad set of metaphysical and methodological assumptions that we can call a paradigm."[120] Theologians, both professional and lay who operate within a given paradigm, are working within "normal religion," analogous to "normal science." Barbour identified three key features of normal religion: paradigm-dependence of religious experience, resistance of religious paradigms to falsification, and no rules or algorithm for choice among competing religious paradigms. Finally, he noted that theologians from different traditions do not

> seem to have a loyalty to an overarching and universal religious community, with shared criteria and values comparable to those shared by all scientists. In a global age, could such wider loyalties be encouraged, without undermining the distinctiveness of each religious tradition?[121]

Fine art

Fine art historians used Kuhn's notion of paradigm to discuss changing periods in fine art, especially the shift between modernism and postmodernism. However, Caroline Jones cautioned that "Kuhn's notion of the paradigm is not a neutral idea that can be applied unproblematically to characterize knowledge-production in the artworld, especially during the crisis period of transition from modernism to postmodernism in the sixties and seventies."[122] For Jones, Kuhn's notion is part of the crisis itself and must be "historicized" before its use by art critics and historians can be justified. E. M. Hafner was one of the first to use Kuhn's philosophy to analyze art history, in a paper delivered at a three day conference in May 1967 at Ann Arbor, Michigan. Kuhn provided commentary.

Hafner explored the relationship between the abstract worlds of modern art and science. He noted the recent trend in fine art history towards abandoning representational art: “The artist seems—and often claims—to have left the real world behind him in a search for the meaning of his own complex subconsciousness.”[123] Hafner likened abstraction in modern art to that found in modern science. Scientists too avoid phenomenal nature through their technical language, in which the scientific world is accessible only to the trained expert.

The obvious question about reality arises from these abstract movements. “It is evidently not a fixed substratum of perception toward which we probe with ever sharper tools,” answered Hafner, “but a shifting set of concepts dependent on the depth of our perception and the character of our probes.”[124] He invoked Kuhn’s analysis of scientific revolutions to justify his answer.

> Whether we speak of science or art, we recognize the essential role played by revolution. Every epoch is marked by its current paradigms: coherent traditions of observation and interpretation which set the stage for normal activity. But every epoch ends in revolution, after which the old paradigms give way to the new. Whenever this happens, it is fair to say that the world itself has changed.[125]

Modern art and science have much in common, including “aesthetic values” for science and “quantitative structure” for art.

Based on this commonality between modern art and science, Hafner queried: “Do the graphic images themselves, emerging from laboratory and studio, betray their scientific or artistic origins?”[126] Although the answer for traditional artistic and scientific works is yes, contemporary works can only be distinguished by the trained eye. For example, at a 1958 art exhibit in Basel, Camille Graser’s constructivist painting was similar in composition to a picture of aspartic acid crystals. The close association between modern artistic and scientific expressions of the world was not accidental but required an explanation.

For an explanation, Hafner turned to the artistic and scientific images themselves. From them he discovered symmetry between the impact of modern art and science on each other. “I am drawn strongly to the conclusion,” wrote Hafner, “that the abstract forms of modern art are largely a result of sympathetic vibration with concurrent trends toward abstraction in science.”[127] In other words, given science’s position in contemporary society changes in its conception of the world are bound to influence modern art. But modern art helps to visualize abstract images in order to understand them, as well as providing "clues to a new scientific reality."[128]

Kuhn agreed with what he considered Hafner's main thesis: it is often difficult to distinguish artist from scientist. But Kuhn found the thesis "disquieting."[129] He believed that the similarity between modern art and science is not intrinsic but the result of faulty analytic methods. "Close analysis," claimed Kuhn, "must again be enabled to display the obvious: that science and art are very different enterprises or at least have become so during the last century-and-a-half."[130] He took exception to three parallels between modern art and science Hafner draws on to support his thesis.

The first parallel is the resemblance of artistic and scientific products. Kuhn observed that Hafner relied on a limited number of samples to support his thesis, such as photomicrographs of chemical substances. The second parallel is artistic and scientific activity involved in creating products. Artistic activity leads to an end-product, while most scientific activity, such as a photomicrograph, is a by-product. The art work is the goal of creative activity, while much scientific activity is a tool or means to an end. To transfer a by-product from the laboratory to a product for the gallery, claimed Kuhn, is to transpose means for an end. The final parallel is public reaction. Kuhn contended that for art the public may reject one school for another, while for science it rejects it as a whole.

In concluding remarks, Kuhn recognized that there is a general developmental pattern common to art and science—periods of practice governed by tradition that are punctuated by periods of rapid change. However, he believed that there are significant differences between them in terms of the finer details of their development. It is at this level that Kuhn observed the most fundamental difference between art and science, the role of innovation. For artists, innovation is standard, while for the scientist it is exceptional. According to Kuhn, "artists do seek new things and new ways to express them. They do make innovation a primary value."[131] For scientists, innovation is required only during extraordinary times when the reigning paradigm no longer serves to solve certain puzzles.[132]

Notes

1. Horgan (1991), 49.
2. One of the first tributes to Kuhn's legacy was the 1980 collection of essays, *Paradigms and Revolutions: Applications and Appraisals of Thomas Kuhn's Philosophy of Science*, edited by Gary Gutting.
3. See, e.g., Favretti *et al* (1999) and Hoyningen-Huene and Sankey (2001).

4. Horwich (1993b), p. 1.
5. Space prohibits discussion of the numerous papers in the literature spawned by Kuhn's historical philosophy of science.
6. See Kuhn (1976).
7. Heilbron (1993), p. 112.
8. Swerdlow (1993), p. 166.
9. Buchwald (1993), p. 196.
10. Wise (1993), p. 248.
11. Earman (1993), p. 24.
12. Friedman (1993), p. 37.
13. *Ibid*, p. 54.
14. McMullin (1993), p. 74.
15. *Ibid*, pp. 75–6.
16. Cartwright (1993), p. 270.
17. *Ibid*.
18. Hacking (1993), p. 307.
19. Hempel (1993), p. 7.
20. *Ibid*, p. 8.
21. Kuhn (1993), p. 313.
22. *Ibid*, p. 321.
23. *Ibid*, p. 326.
24. *Ibid*.
25. *Ibid*, pp. 333–4.
26. *Ibid*, p. 315.
27. *Ibid*.
28. *Ibid*, p. 337.
29. Greene (1971), p. 23.
30. Wade (1977), 145.
31. Greene (1971), p. 23.
32. Mayr (1972), 982.
33. *Ibid*, 988.
34. *Ibid*, 987.
35. Hess (1970), 664.
36. *Ibid*, 668.
37. Morange (1998), p. 167.
38. Nirenberg (2000), 615.
39. Friedrichs (1970), p. 56.
40. Ritzer (1975), p. 12.
41. Eckberg and Hill (1979), 925.
42. For a fuller discussion of the integrated sociological paradigm, see Ritzer (1981a).
43. Ritzer (1981b), 247.
44. Hill and Eckberg (1981), 251.
45. Barnes (1982).
46. Kuhn (1991), 3–4. See also Kuhn (2000), pp. 316–17.

47. For analysis of the science wars by philosophers of science, see Hacking (1999) and Kukla (2000).
48. Gross and Levitt (1998), p. 45.
49. For discussion of Kuhn's role in the origins of the science wars, see Sardar (2000).
50. Ross (1996), p. 9.
51. Gordon (1965), 123.
52. *Ibid*, 124.
53. *Ibid*, 126.
54. Coats (1969), 292.
55. *Ibid*, 293.
56. *Ibid*.
57. Stanfield (1974), 105.
58. de Vroey (1975), 415.
59. *Ibid*, 429.
60. *Ibid*, 435.
61. Eichner and Kergel (1975), 1293.
62. *Ibid*, 1319.
63. Screpanti and Zamagni (1993), p. 5.
64. Redman (1991), p. 142.
65. Blaug (1975), 399.
66. Ward (1972), p. 13.
67. Redman (1991), 151.
68. *Ibid*, 172.
69. Hausman (1989), 124.
70. Caldwell (1994), p. 230.
71. Dow (1997), 77.
72. Truman (1965), 865.
73. *Ibid*, 866.
74. Claude (1970), 47.
75. Walker (1972), 419.
76. *Ibid*, 420.
77. Stephens (1973), 468.
78. Beardsley (1975), 328.
79. Ashcroft (1975), 15.
80. Janos (1986), p. 3.
81. Farr (1988), 1186.
82. See Green and Shapiro (1994) and Friedman (1996).
83. See Beesley (2003).
84. See Park (1996).
85. Weinberg (1998), 52.
86. Watson (1966), 276.
87. Palermo (1971), 151.
88. Warren (1971), 409.
89. Briskman (1972), 94.

90. Coleman and Salamon (1983), 436.
91. Peterson (1981), 15.
92. O'Donohue (1993), 285.
93. Driver-Linn (2003), 271.
94. *Ibid.*
95. *Ibid*, 273.
96. *Ibid*, 276.
97. Green (2004), 271.
98. Driver-Linn (2004), 274.
99. Anonymous (2000), 2.
100. Elkana (1970), 30.
101. Siegel (1979), 111.
102. *Ibid*, 113.
103. *Ibid.*
104. Siegel later examined Kuhn's epistemological relativism vis-à-vis dogmatism in science pedagogy. "It is ironic," quipped Siegel, "that Kuhn defends relativism in epistemology but dogmatism in education." Siegel (1985), 102. Siegel proposed a pluralistic approach to science education, in which students are exposed "to a variety of philosophical methods and theoretical formulations." *Ibid*, 103.
105. Abimola (1983), 190.
106. Wagner (1983), 605.
107. Hodson (1988), 33.
108. *Ibid.*
109. Eflin *et al* (1999), 114.
110. Matthews (2004), 112. Kuhn's impact on science education was also the topic of a special edition of a 2000 issue of *Science & Education.*
111. Barbour (1974), p. 136.
112. Veatch (1977), 48.
113. Strug (1984), 271.
114. *Ibid*, 269.
115. Küng, along with another Catholic theologian David Tracy, organized an ecumenical symposium at the University of Tübingen in May 1983 to explore the role of paradigm in theological consensus. Kuhn, along with Toulmin, was invited to participate in the symposium but Kuhn was unable to attend because of prior commitments. MIT MC240, box 11, folder 36, 9 March 1983, letter from Kuhn to Küng.
116. Küng (1989a), p. 446.
117. *Ibid*, p. 442.
118. Küng (1989b), p. 218.
119. In response to Küng's new paradigm for theology, Toulmin cautioned theologians to remain "paradigmless." Toulmin (1989), p. 237. Erich von Dietze claimed that Küng's reliance on paradigm change ignores the problems associated with incommensurability. "If different religions or theologies are (or

contain) different paradigms," argued von Dietze, "then Küng must explain how he has the ability to transcend incommensurability and how he is able to make comparisons and hold dialogue." (von Dietze (1998), 72)

120. Barbour (1997), p. 127.
121. *Ibid*, p. 132.
122. Jones (2000), 522.
123. Hafner (1969), 387.
124. *Ibid*, 388.
125. *Ibid*, 389.
126. *Ibid*, 390.
127. *Ibid*, 395–6.
128. *Ibid*, 397.
129. Kuhn (1969), 403.
130. *Ibid*, 404.
131. *Ibid*, 411.
132. Robert Root-Bernstein (1984) insisted that both art and science are creative endeavors and examined the various practices that make them up, in order to address the differences between Kuhn and Hafner.

Epilogue

Kuhn still attracts, even after his death in 1996, considerable attention from the science studies community, especially from the history and philosophy of science community.[1] Most members of that community acknowledge Kuhn's contribution to the historiographic revolution and his impact on the discipline. "When Kuhn died in 1996," testifies Howard Sankey, "he left the field of history and philosophy of science a different field from the one he entered."[2] However, others question Kuhn's contribution to the revolution and some contend that his impact on the history and philosophy of science was not for the better. I briefly address several questions concerning Kuhn's role in the historiographic revolution and his impact on the history and philosophy of science.

What is Kuhn's stake in the historiographic revolution?

In his 1990 presidential address to the Philosophy of Science Association, Kuhn reminisced about his participation in the historiographic revolution in the history and philosophy of science.

> That's a transition for which I get far more credit, and also more blame, than I have coming to me. I was, if you will, present at the creation, and it wasn't very crowded. But others were present too: Paul Feyerabend and Russ Hanson, in particular, as well as Mary Hesse, Michael Polanyi, Stephen Toulmin, and a few more besides. Whatever a Zeitgeist is, we provided a striking illustration of its role in intellectual affairs.[3]

Kuhn's role in the historiographic revolution is a contested issue among scholars, with some claiming he played the major role in it and with others depicting him as a pawn of larger social forces and personages. Most—and possibly Kuhn himself, as evident from the above quotation—believe he falls somewhere in between these two extremes.

In a recent sociohistorical analysis of *Structure*, Fuller argues that Kuhn was simply Conant's foot soldier: "Kuhn appears as a 'normal scientist' in the Cold War political paradigm constructed by James Bryant Conant."[4] Moreover, he claims that Kuhn's philosophy of science was not revolutionary but conservative and reactionary. According to Fuller, *Structure* was not the "cause" of the historiographic revolution but simply its "symptom." Fuller's revisionist account touched off a flurry of responses.[5] Hanne Andersen, for example, identifies a "serious defect" with it: "one searches in vain for anything beyond the most standard of Kuhn's publications, let alone unpublished manuscripts, notes or correspondence."[6] In other words, Fuller's account errors in presenting a myopic account of *Structure* based mostly on external factors. A "fuller" account should also include the internal factors that led to its development.[7] Only then can Kuhn's role in the historiographic revolution and impact on the history and philosophy of science be properly assessed. Fuller responds initially to Andersen stating that: "After having consulted the Harvard archives, interviewed Kuhn myself, and read his final substantial interview, I concluded that the content of Kuhn's unpublished papers at MIT would not contradict the main points of my interpretation."[8]

After examining the MIT archival material, especially focusing on *Structure*'s development and reception, I contend that Kuhn played an important role in the historiographic revolution and had a significant impact on the history and philosophy of science. And, I would conclude that Kuhn's image of science is incommensurable with the traditional view of science. The internal account I develop above leads to and supports that conclusion. I have also endeavored to incorporate the intellectual context in which Kuhn's work is situated. Analysis of Kuhn's relationship to Popper, Polanyi, Lakatos, Feyerabend, Toulmin, Shapere, and others, in terms of the ideas debated among them, provides a balanced assessment of Kuhn's role in the revolution. For as Kuhn himself testifies, he was not the only participant to initiate or shape it.

Alexander Bird provides an intriguing assessment of Kuhn's role in the historiographic revolution.[9] Just as Kuhn argued that Copernicus is closer to Ptolemy than to modern astronomy, so Bird argues that Kuhn is closer to the traditional view of science than to the postmodern view. Kuhn too helps to initiate a revolution by providing a different conception of science from that of traditional historians and philosophers of science, just as Copernicus helped to initiate a revolution by providing a different conception of the universe from that of Ptolemy. Today, just as we view the earth traveling around the sun so we view science as an institution like other institutions.

The postmodern interpretation of Kuhn's philosophy of science sees scientists as no more privileged than theologians or artists, in terms of their access to truth. Kuhn certainly resisted this interpretation of his views but this is the direction they have led. To expect Kuhn to understand or accept this direction, postmodernists argue, is to expect too much from him or any individual locked into a particular worldview. Just as Kuhn insisted that Copernicus is restricted by his education so Kuhn is restricted by his. So maybe Kuhn is the last of the traditionalists; but, he provided the direction, i.e. an historical philosophy of science, which must be used to resolve problems, such as the growth of knowledge, traditional philosophy of science could not.

What is Kuhn's impact on the history and philosophy of science?

Fuller's more radical claim is not that Kuhn was Conant's foot soldier but that "the impact of *The Structure of Scientific Revolutions* has been largely, though not entirely, for the worse."[10] According to Fuller, Kuhn's legacy is the "Kuhnification" of disciplinary horizons. The result is a pathological condition he calls "paradigmitis," in which a discipline to legitimate itself conforms to the paradigmatic structure of Kuhn's image of science.[11] The problem with the condition is that members of the affected disciplines lose their critical edge and become politically impotent.

For the philosophy of science paradigmitis produced a class of "underlaborers," who work under a "master narrative." For twentieth-century American science pedagogy and policy, Conant developed a master narrative, while Kuhn, according to Fuller, developed a "*servant narrative*."[12] And it is Kuhn's narrative under which contemporary philosophers of science labor, whether they admit—or know—it. These philosophical underlaborers—or underunderlaborers—in the Kuhnified vineyard, claims Fuller, have surrendered their "prescriptive, legislative, and critical attitudes that had marked philosophy's traditional relationship to the natural sciences."[13] Scientists too are duped into an acritical attitude, especially in terms of normal science practice.

But does Kuhn's philosophy of science dull the critical attitude of philosophers? As we saw above, throughout his career Kuhn's philosophy of science came under severe criticism from philosophers. He certainly contributed to the naturalization of the discipline, but he was one voice among many. The story of contemporary philosophy of science is more complex than Kuhn's role in its naturalization or that Kuhn dominated the discipline.[14] Moreover, as Bird argues convincingly, Kuhn reverts to an apriorism late in his career. In effect, it could be argued, as Bird does, that Kuhn never completely overcame traditional philosophy of science. Had he remained committed to the new paradigm he helped to establish, Kuhn may have had an even larger impact on contemporary philosophy of science. As Bird concludes,

> precisely because philosophy of science has become more naturalized and more open to cognitive science and artificial intelligence, the time has come to reappraise those naturalistic elements of Kuhn's thought that he himself abandoned. In particular Kuhn's account of the function of paradigms-as-exemplars and the psychological nature of a scientific revolution and a psychological rather than linguistic notion of incommensurability are all ripe for development with the tools of cognitive science and allied disciplines that were unavailable to Kuhn at the time he wrote *The Structure of Scientific Revolutions.*[15]

Or, does Kuhn's philosophy of science lull scientists into an acritical attitude with respect to the practice of science? As we have seen, for Kuhn normal science or puzzle-solving certainly entails a critical attitude. It cannot be otherwise. Criticism under the normal practice of science, however, is not always directed toward the foundations of science—although sometimes it is.[16] Where the critical attitude is compromised, according to Kuhn, is during paradigm shift or revolution. Although it is certainly a part of the process for deciding the fate of a reigning paradigm, it is sometimes not sufficient. Under such conditions, the traditional objective criteria for choosing a paradigm may at times function as subjective values. Thus, the critical attitude does not vanish in Kuhn's account of science; it is there in normal science and it is also there, although sometimes not determinatively, in revolutionary science.[17]

So, are we worse off after Kuhn? Most people would probably not want to return to the days when traditional science and its philosophy of science dominated almost everyone, even with its "critical" attitude.[18] Although some members of academic disciplines misapplied Kuhn's philosophy of science, this does not mean that we are worse off after him. In fact, Kuhn helped others, who were marginalized under the traditional view of science, to voice their views and concerns about science. For example, the feminist movement in science embraced Kuhn's philosophy of science to address gender issues in science. "Kuhn's *Structure,*" claims Helen Longino, "offered a vocabulary for articulating the complex critique of science and its ideology that feminist scientists sought to develop."[19] Of course, no system is without problems but to claim that we are worse off after Kuhn and his cohorts in the historiographic revolution is simply unfounded by the evidence.

The real question, however, is what do we do with Kuhn's fairly accurate description of the physical sciences? Are we to make it normative for all the other natural and social sciences? There are certainly problems, as we have seen above, in applying Kuhn's *Structure* to the sciences and disciplines outside the physical sciences. But does it provide at least a starting point? And finally, there are the larger social and political questions concerning science, which so justly concern Fuller. Kuhn himself was incapable by personal constitution to act socially or politically on his new image of science.[20] But those competent to address these questions must do so.

Why is Kuhn misunderstood?

Kuhn was pained by those who misunderstood him, especially with those for whom he desperately wanted to communicate—philosophers. Kuhn later recounts how he

> simply stopped reading the things about me, from philosophers in particular. Because I got too angry. I knew I couldn't answer, but I got too angry trying to read them and I would throw them across the room . . . It was too painful.[21]

Although Kuhn was disappointed over being misunderstood, he never regretted the importance of *Structure* for those who understood and appreciated it. But why was Kuhn misunderstood so often?

David Bloor claims that "Kuhn stood outside the dominant cultural 'paradigm' of individualism and rationalism."[22] But he realizes that this can only be part of the answer, for if sufficient then incommensurability is equivalent to incommunicability—an equivalence Bloor denies. For Bloor, an additional reason for the misunderstanding is voluntaristic in the sense that some did not want to communicate or understand.[23] But why should this be, queries Bloor. His answer is:

> When it comes to defining the nature of science and assessing the virtues of rival models (rather than doing actual scientific work) the academic community often seems more concerned with getting a desirable answer than with getting a factually adequate one.[24]

In other words, deeply held values win out over the facts.

Bloor identifies a part—and a very big part—of the problem for why Kuhn is often misunderstood. Kuhn's understanding of rationality is also incommensurate with the dominant Enlightenment understanding of science. As Ronald Giere argues, Kuhn is traditionally viewed as replacing the “old logical paradigm” of science with a “new historical paradigm.”[25] But Giere goes on to claim that Kuhn is not the founder—let alone a member—of this new school of historically oriented philosophers, which includes Lakatos, Laudan, McMullin, Shapere, and Toulmin. These philosophers, according to Giere, “appealed to a historical notion of rational progress rather than to a logical notion of rational inference. It was never a part of Kuhn’s project to show science to be globally rational in either of these ways.”[26]

Giere is correct in that Kuhn did not attempt to demonstrate that science is globally rational, if Giere means a rationality that transcends time and place. Rather Kuhn is more interested in showing that science is historically or locally rational. The historical for Kuhn is not simply using case studies of past scientific activity and practice, in order to score philosophical points. Rather, Kuhn’s use of history is to demonstrate the local nature of rationality and scientific knowledge. In other words, the generation of scientific knowledge cannot be wrested from its historical (local) context; it is situated in a particular time and location. And if we are to understand the science of a particular time and location, according to Kuhn, we must climb inside the heads of its practitioners. This sympathetic read is driven by the oddities of the text, given our modern perspective and requires an understanding of the paradigm (disciplinary matrix and exemplars combined) that was used by its contemporary—not current—advocates. Kuhn’s method is not irrational, he claims, since logic and reason are certainly required to understand a text. However, he also wants to include an hermeneutic contextualism in its reading.

Scientific knowledge, then, is not universal or absolute, which can be justified only by a global rationality. In other words, there is no distinction for Kuhn between the logic of discovery and the logic of justification. Discovery is not an algorithmic progress (contra Hanson) or a psychological process (contra Popper) and justification is not a logical process, separate from each other. Rather they are part of the seamless process by which scientists practice their trade, within a specific culture and historical period. Moreover, there is no logic of pursuit (contra Laudan), since this is just an ad hoc adjustment to a doomed analytic analysis that fails to capture the complexity and richness of scientific practice. The generation and development of scientific knowledge, according to Kuhn, depends on a specific set of practices and ideas (paradigms), which are unique to a specific place and to a particular time, so that when change occurs (paradigm shift) the new paradigm often has points of contact with the old paradigm that become untranslatable (incommensurable lexicons) because the context of the old paradigm becomes forgotten or suppressed.

Kuhn illustrates the above position with his experience in attempting to understand Aristotle's notion of motion vis-à-vis Newtonian conception of motion. I never fully appreciated Kuhn's experience until I went through a similar one. While reading William Howell's article on thrombin, I became aware that his use of the term for the clotting factor was at odds with its modern use. For Howell, thrombin was a colloid not an enzyme. Once I realized the difference, Howell's theory of blood coagulation and the experiments he conducted to substantiate it made sense.[27] Part of the reason that Kuhn is misunderstood may be because some members of a discipline have not had a similar "conversion" experience.

Kuhn's disappointment with others who misapplied his ideas was that they were unwilling, as Bloor argues, or unable to climb inside Kuhn's head. As we have seen a number of scholars from various disciplines have appropriated Kuhn not on his terms but on their own. But is this unfortunate? For Kuhn, yes, since we all want to be understood and judged fairly. But for those who have misappropriated Kuhn, no. Why? Although a parsimonious reading of the text, whether data or otherwise, is to be valued, however, pushing the boundaries of a text or discipline is often required for progress or development. Those who have seemingly misappropriated Kuhn have advanced their own disciplines, at least from their perspective.

What can be concluded about Kuhn's revolution?

According to Giere, "Kuhn's real legacy for North American philosophy of science is that he shamed post-war philosophers of science into dealing with *real science*, rather than the trivial logical surrogates for real science."[28] In other words, what makes Kuhn revolutionary is that he punched holes in the perceived pretensions and arrogance of some analytic philosophers and their façade of precision and accuracy, as they attempted to mimic the physical scientists for establishing the certainly of their epistemic claims. Our knowledge systems are often at best houses of cards that collapse upon examination of their metaphysical and empirical foundations. Moreover, precision and accuracy are sometimes an illusion and dangerous to the creative impulses necessary to drive progress. Normal science and its advances are impressive but insufficient to harness nature completely. We live in a world that outstrips our abilities to understand it ultimately. The best we can do is to move from one paradigmatic situation to the next. There may indeed be mind-independent real events, but more important than there being these events is what they mean to those participating in them. Objective facts simply are insufficient to account for the full status of reality. How can one axiomatize the ineffable Aunt E—?

Kuhn elicits a wide range of responses. To some he is a savior who dethroned physical science and its hegemony. To others he is a deceiver for parading a false image of science. The issues he raised concerning reason, truth, progress, etc. have unsettled many. In a real and important sense we live in different world from the one prior to Kuhn. But the issue is, as Fuller frets about, whether it is a better, or as Popper would say a roomier, world. Or have we been corrupted? As Kuhn confesses in a final interview, he was an avid reader of mystery novels. He did not give the reason why, but it might be the lure of solving the mystery or puzzle. Although family members chided him about the diversion, they eventually became hooked on reading them through his influence. From the anecdote, Kuhn ended with this final word: "I'm a corruptor of the mind!"[29]

Notes

1. A sample of major works and collections on Kuhn include Andersen, H. (2001a), Bird (2000), Nickles (2003a), Sharrock and Reid (2002), von Dietze (2001), and special issues of *Configurations* (vol. 6, no. 1, 1998) and *Perspectives on Science* (vol. 9, no. 4, 2001).
2. Sankey (2002), 823. Mary Hesse situated Kuhn in the transformation of the philosophy of science accordingly:

   > When considering how radical Kuhn's thesis was in 1962, one has to remember what the problem situation then was. It was characterized by the deductivist account of science of Carnap, Nagel, Hegel, Braithwaite, and others. This was not a positivist account, but it did retain an important feature of logical positivism, namely a reliance on deductive logic, and its necessary presupposition of a scientific language that is ideally univocal and hence fit to carry deductive entailments. This was the presupposition that was called into question by Kuhn's and Feyerabend's discovery of "meaning-change" or "incommensurability." Relative to the received account of science this discovery was very radical indeed. (Hesse (1983), 704)
3. Kuhn (1991), 3.
4. Fuller (2000), p. 5.
5. A special issue of *Social Epistemology* (vol. 17, nos. 2&3, 2003) was devoted to a discussion of Fuller's revisionist account of *Structure*. Fuller's response appeared in a subsequent issue (vol. 18, no. 1, 2004).
6. Andersen, H. (2001b), 260.
7. Kenneth Caneva makes a similar critique: Fuller's account of *Structure* does not include "the substance of Kuhn's work as a necessary part of any story that would explain its impact." Caneva (2003), 135.

8. Fuller (2001), 570. After visiting the MIT archives, Fuller provides this account: “I take a perverse pleasure in admitting that nothing I have read there causes me to revise my original evaluation—only to deepen it.” Fuller (2004), p. 4.
9. Bird (2000), pp. 2–3.
10. Fuller (2000), p. xvi.
11. *Ibid*, p. 318.
12. *Ibid*, p. 31.
13. *Ibid*, p. 36.
14. For example, John Preston (2003) argues that Putnam was much more influential in setting the agenda for contemporary philosophy of science.
15. Bird (2004), 14.
16. For example, a controversy is currently playing itself out over the foundations of cancer research. See Marcum (2005).
17. Petri Ylikoski argues that the “right way to get critical discourse back into the sciences is to take naturalism more seriously, not turning back to Popper and general philosophy of science.” Ylikoski (2003), 323.
18. For a lively discussion of the problems associated with traditional science’s hegemony, see Feyerabend (1975).
19. Longino (2003), 262.
20. As Thomas Nickles claims: “That sort of thing did not suit Kuhn’s personality at all, and he assured us that . . . he would not have been good at it.” Nickles (2003b), 253. Of course, Nickles recognizes that there is more than one way of being socially and politically active.
21. Kuhn (2000), p. 315.
22. Bloor (1997), 501.
23. Interestingly, Fuller recognizes a similar explanation. See Fuller (2000), p. 5.
24. Bloor (1997), 501.
25. Giere (1997), 496.
26. *Ibid*, 497.
27. Marcum (1996).
28. Giere (1997), 497.
29. Kuhn (2000), p. 323.

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Note: "MIT MC 240" refers to the Thomas S. Kuhn papers, at the Institute Archives and Special Collections, MIT Libraries, Cambridge, MA.

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