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*Thomas Kuhn and the Psychology of Scientific Revolutions**

David Kaiser, MIT

Introduction

Looking back in 1969, four years after a rousing session of the International Colloquium in the Philosophy of Science in London devoted to his work, Thomas Kuhn observed wryly, “I am tempted to posit the existence of two Thomas Kuhns. Kuhn₁ ... published in 1962 a book called *The Structure of Scientific Revolutions* Kuhn₂ is the author of another book with the same title.” Around the same time he noted that he had been “monitoring conversations” about the book, and “sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume.” He lamented to a correspondent that he had begun to “shudder as I discover what my ideas are taken to be.” Just as Roland Barthes declared “the death of the author,” Kuhn came to realize that his authorial intentions could not dictate how others would read and interpret his work.¹

Responding to the plasticity of readings inspired by *Structure*, Kuhn made an important change to the book, appending the lengthy 1969 postscript to the second edition. Finding even the postscript insufficient to discipline divergent readings, Kuhn composed a series of essays in which he labored to clarify his original intentions, critique particular construals of his work, and highlight areas in which his thinking had evolved since the early 1960s.²

In short, Kuhn experienced keenly that his famous *Structure* was no stable baton, passed among an expanding circle of readers; many ran off in their own directions.

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Behind the well-known essays and published responses to critics, an extensive correspondence survives in Kuhn's papers, stretching over the two decades after the original publication of *Structure*. The collection includes 170 distinct correspondents, with several of whom Kuhn shared multiple, lengthy exchanges. In his unpublished responses to readers, Kuhn crafted and auditioned versions of his later, well-known essays. The letters include anticipations or trial-runs of phrasings and disambiguations that appear in the 1969 postscript and in essays like "Second thoughts on paradigms," first published in 1974.³

Drawing upon Kuhn's extensive correspondence, we may chart the dialogic reception of *Structure*. The reception was "dialogic" in at least two senses. Most of Kuhn's responses had an informal tone, akin to an in-person conversation—a tone facilitated, perhaps, by their mode of composition: Kuhn composed most of his letters by speaking into a dictaphone and then having them transcribed and mailed by an assistant. More important, the reception was dialogic because Kuhn's responses reflected the interests of his interlocutors and the particular points they raised. Kuhn did not fashion his responses (or his published essays) in the abstract. Rather, he responded to specific questions and challenges from readers—readers who emphasized certain aspects of his work while ignoring or downplaying others.⁴

Though Kuhn corresponded about his book with scholars from a wide range of disciplines, across the natural sciences, social sciences, and humanities, the largest share came from psychology. Not surprisingly, few of the psychologists pressed Kuhn on such matters as social attributes of learning or institutional frameworks for apprenticeship in the sciences—topics that Kuhn had claimed throughout *Structure* were of crucial

importance in the natural sciences, but whose details he had left largely unanalyzed. Rather, most psychologists who wrote to Kuhn focused on matters of individual cognition, or what one correspondent jokingly called “intracranial determinism”: the factors within an individual scientist’s head that determined how he or she arrived at a particular conclusion.⁵

The psychologists were not responding at random to Kuhn’s book. Tracing Kuhn’s path to writing *Structure* from the late 1940s to the early 1960s highlights the importance of specific psychological traditions to Kuhn’s own evolving thought. Particularly important were the “genetic epistemology” of developmental psychologist Jean Piaget, the “New Look” experimental perception studies of midcentury, and Kuhn’s budding interest in psychoanalysis. Kuhn had become fascinated with certain strands of psychological research—bolstered by influential interactions with particular psychologists—as his thinking coalesced toward the first edition of *Structure*. Many of those strands were then amplified, even as they were edited or refined, in his correspondence with psychologists after the book had been published.

Following a brief description of how Kuhn’s *Structure of Scientific Revolutions* emerged to become one of the best-known books about science of the twentieth century, I turn to consider the book’s composition, focusing on the many legible traces left in the text of Kuhn’s own engagements with psychology and psychologists. I then consider Kuhn’s correspondence in the wake of the book, before concluding with some suggestions of how situating Kuhn’s *Structure* in its web of influences and interlocutors can help make sense of the changing assessments the book has received from historians of science.

Slow Emergence

Kuhn's *Structure of Scientific Revolutions* remains unparalleled among works in the history, philosophy, and sociology of science for having reached a mass audience. Cumulative sales exceed one million copies, and at least sixteen foreign-language translations have been published. To mark the fiftieth anniversary of the book's original publication, symposia were held during 2012 at the University of Athens, Boston University, the Max Planck Institute for the History of Science in Berlin, Princeton University, the University of Chicago, and jointly by MIT and Harvard, in addition to a combined plenary session of the History of Science Society and the Philosophy of Science Association at the annual meeting in San Diego. Several journals published special sections to assess the book's legacy in various domains of scholarship.⁶

One would have been hard-pressed to predict such an outpouring of interest when the book first appeared. The University of Chicago Press prepared an initial print run of three thousand copies (one-third clothbound and two-thirds paperback), to be released in early September 1962—an average print run for a scholarly monograph at the time. Neither the press nor its author anticipated widespread adoption of the book for classroom use, let alone that the book would become a broad-market bestseller. Kuhn explained to the Press that “it's not a text for any course but might well be assigned, if cheap enough, as collateral reading in phil. of science, etc.,” and that he had “no good ideas” about faculty or institutions that might be effective in promoting the book for classroom adoption. He asked the Press to send complimentary copies to his parents and

several relatives in addition to a small number of professors, several of them colleagues at Berkeley (where he was then teaching). A respectable 919 copies sold that first year, and 774 copies the next.⁷

Kuhn and his family were in Copenhagen when the book came out; he had taken a sabbatical from the University of California at Berkeley to head up the massive “Archive for the History of Quantum Physics” project. Mail that had been sent to him at Berkeley reached him in Copenhagen with delays that varied between weeks and months, so that by late 1962, Kuhn began to receive letters in response to *Structure*. One of the first responses to reach Kuhn came from Garret Hardin, a biologist at the University of California, Santa Barbara, who later introduced the notion of the “tragedy of the commons.” Hardin enthused to Kuhn that *Structure* was “the best thing that has ever been written” about how science works. Hardin shared a reprint of his own in which he had examined an episode in the history of life sciences, but cautioned Kuhn not to be taken aback by “how primitive my analysis of the historical aspects is.” After all, Hardin, conceded, his piece (which had appeared in 1960) had been “written in the year 2 B.K.”—before Kuhn. Kuhn was grateful, responding that he had felt “as though my neck were very far out” when publishing *Structure*, “and I have therefore been living with a nervous stomach during the months since the book first appeared.” Kuhn repeated that phrase—that he felt as if he had “stuck his neck very far out” in writing the book—in several other letters at the time.⁸

By the mid-1960s, Kuhn began to receive a steady flow of correspondence about the book. He heard from professors of history, economics, geography, psychology, physics, and English about their uses of *Structure* in their classroom teaching; the book

had earned its place as a textbook after all. A few correspondents even sent Kuhn their students' papers about the book.⁹ (For better or worse, the student papers are no longer extant in Kuhn's files; one can only imagine his reaction to receiving dozens of additional student papers to read.) Colleagues like Peter Fox, a historian at the California Institute of Technology, reported on local campus reactions to the book:

Dear Tom,

I must tell you how interesting, and useful, your 'Structure of Scientific Revolutions' has proved to be. An economist acquaintance at RAND told me about it, and I had the eighteen Freshmen in my European History survey section read it. ...

The reaction among the older physicists I know here is: 'I haven't read him, but he's all wrong.'

And I notice that in the syllabus of a new linguistics course here, the instructor has listed as item 4 under 'History of linguistic thought,' 'XXth century (on view of Kuhn's paradigms).'

So you are appearing in all sorts of nooks and crannies. I hope your royalties reflect it.

The royalties did indeed begin to accumulate: by the late 1960s, sales had reached 40,000 copies per year.¹⁰

Not everyone was pleased with the book's repeated appearances on course syllabi. One horrified student at Fairhaven College, a small liberal arts college in Washington State, wrote to Yale physicist and historian of science Derek de Solla Price in 1969 to ask for advice. She had seen that Price had praised Kuhn's book in a review in *American Scientist*, so she inquired whether he thought the book was appropriate for undergraduates "with an extremely limited background in science": "Kuhn's book is being used as a text for six hundred students this year and several more will be using it next year unless we have some evidence that the book is too complex and discouraging [for] students with past histories of poor science courses." Sensing the student's distress, Price responded that he was "horrified that it [Kuhn's *Structure*] is used for liberal arts undergraduates with limited scientific background. Kuhn is a book for professionals, and

perhaps its greatest fault is that it has been much misused by non-professionals who find that ‘paradigm’ is a nice word to roll on the tongue.” In place of *Structure*, Price suggested that the students read his own recent book, *Science Since Babylon*. Other students wrote directly to Kuhn to express their dissatisfaction.¹¹ (See Fig. 1.)

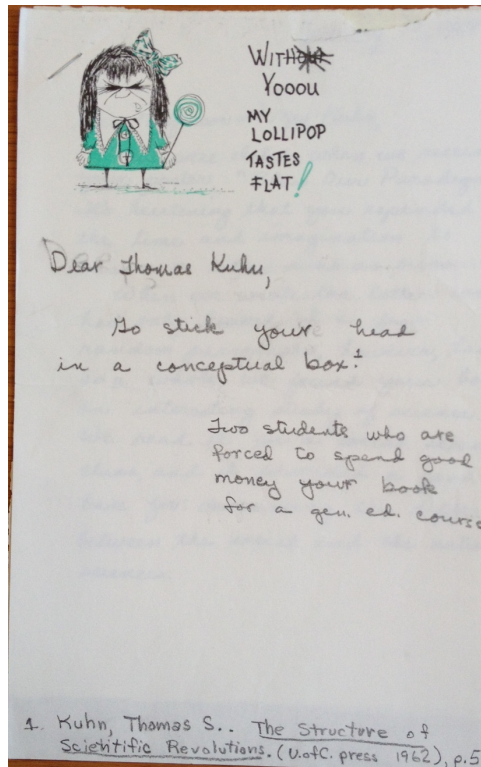


Figure 1. Unsigned letter to Thomas Kuhn, ca. January 1968, in TSK folder 4:14.

By the late 1960s, the increasing popularity of *Structure* had begun to take a toll. Declining a request in 1967 to lecture on his book at a small college in Wisconsin, Kuhn responded that he had “increasingly and painfully realized that I must drastically restrict my activities as a propagandist for my views if I am ever to get the time to write another book.” Early in 1970 he reported to another correspondent that he had “been getting perhaps a letter a week about my book ever since it appeared almost seven years ago.”

Many of those letters included manuscripts on which the senders hoped Kuhn would comment; Kuhn lamented the combined “intellectual” and “diplomatic” problems of coping with so many unsolicited materials. By the summer of 1973, he had resorted to a kind of form letter. Several letters went out that season with identical wording: “For better than five years I have been receiving two or three unsolicited manuscripts, sometimes of book length, every week. Though I regularly mean to write more than routine acknowledgments, I find that, in fact, very few of them do get read. Though I very much hoped that my *Structure* would be widely read, I never dreamed of the nature or magnitude of the problems which its success would create for me.” At the same time Kuhn was feeling inundated, his book made even sharper inroads into the academy. Between 1976 and 1983, *Structure* was the most-cited book in the *Arts & Humanities Citation Index*, cited more often than works by Freud, Wittgenstein, Chomsky, Derrida, or Foucault.¹²

Composition

Though *Structure* first appeared in 1962, Kuhn often recalled that he had begun thinking about the material fully fifteen years earlier. While pursuing his Ph.D. in theoretical physics at Harvard in the late 1940s—where he studied solid-state physics with Nobel laureate John Van Vleck—he also began teaching in Harvard’s then-new General Education program, working closely with Harvard’s President James B. Conant. Writing a few years later, Van Vleck noted how disappointed he was that Kuhn began spending so much time in General Education. “He would have stood up very well in

theoretical research in quantum mechanics, had he wished to make this his main pursuit,” Van Vleck observed.¹³

In 1947, while helping to prepare a new General Education course, “Natural Sciences 4,” Kuhn had what he later described as his “Aristotle experience.” He had been reading Aristotle’s descriptions of motion and, with a Newtonian framework in mind, he found Aristotle’s account “full of egregious errors, both of logic and of observation.” But he kept reading:

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

Kuhn often narrated this experience as akin to a Gestalt switch. It became the prototypical version of his famous description of revolutionary changes in science, “in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before.”¹⁴

Soon after his “Aristotle experience,” just as he was wrapping up his physics dissertation, Kuhn began a three-year fellowship in the prestigious Harvard Society of Fellows (1948-1951). His reading notes from that period survive. Alongside major works from the Western philosophical canon, he also began to immerse himself in writings by developmental psychologists Jean Piaget and Heinz Werner.¹⁵ Kuhn read in the opening chapter of Piaget’s *The Child’s Conception of Physical Causality* (1930), for example, that young children describe physical motion in distinctly Aristotelian terms, explaining

that a projectile continues to move after being thrown because “the air pushes it.” Piaget argued throughout his book that children pass through distinct stages in their reasoning about physical motion. The stages mirrored those of the history of science, from the ancient Greeks through more modern notions of impetus and inertia—a mental version of Ernst Haeckel’s famous dictum that ontogeny recapitulates phylogeny. In his conclusion Piaget posed the central question that would occupy Kuhn’s own wrestling with positivism and antipositivism: “If the child’s mind is active in the process of knowing, how is the collaboration effected between his thought and the data of the external world?”¹⁶

Piaget expanded upon this theme throughout *The Child’s Conception of Movement and Speed* (1946), another book that Kuhn read while in the Society of Fellows. Piaget agreed with phenomenologists and Gestaltists about the “radical interdependence” of knowing subjects’ preexisting ideas and how they ordered their sense impressions. “It is clear that the same empirical observation,” he wrote, “will give rise to quite a different construction according to the way it is understood”—an anticipation of what Kuhn, Paul Feyerabend, and others would later dub “theory-laden observations.” To Piaget, it was not enough to highlight the role of preexisting ideas. One had to consider how people’s “schematization” changed over time: “this understanding is transformed in terms of the mental stages already traversed.” Moreover, the act of imposing order on sense impressions, of seeing-through-a-schema, constitutes “a modification of reality by the subject.” Or, as Kuhn would later write, when Lavoisier began to see oxygen while Priestley saw dephlogisticated air, Lavoisier “worked in a different world.”¹⁷

Kuhn encountered Heinz Werner's writings around the same time. Werner, like Piaget a developmental psychologist, had developed an experimental program devoted to perception while director of the Psychology Department at the University of Hamburg in the late 1920s. A colleague of art historian Erwin Panofsky, Werner had also been closely associated with the Leipzig school of Gestalt psychologists before he was forced to flee Germany when the Nazis took power in 1933. Settling in the United States, he expanded an earlier study into a massive English-language tome, *Comparative Psychology of Mental Development*, which went through multiple editions in several languages. For the 1948 edition Harvard psychologist Gordon Allport added a foreword, praising the book for making German traditions of Gestalt psychology more accessible to American audiences. Allport praised Werner's book soon after he (Allport) had helped to establish the new Department of Social Relations at Harvard; perhaps the local ferment helped bring Werner's book to Kuhn's attention.¹⁸

Like Piaget, Werner emphasized various "structures" or levels of thinking through which individuals and entire societies pass. Even more than Piaget, Werner argued that development between stages was highly nonlinear and noncumulative: "each higher level is fundamentally an innovation, and cannot be gained merely by adding certain characteristics to those determining the preceding level." Rather than "unbroken continuity," Werner concluded, changes between levels are punctuated by "crises."¹⁹

Reading Piaget and Werner clearly left their mark on young Kuhn. He briefly mentioned encountering works by Piaget and the Gestalt psychologists in the preface to *Structure*, but other interactions suggest even stronger influences. His wife at the time, Kay Kuhn, recently recalled that Piaget visited the Berkeley area to deliver some lectures

in the late 1950s, while Kuhn was on the faculty there. As Kay remembers, Kuhn felt shy about approaching the famous psychologist even though he had been so inspired by his work. Kay commanded: if Kuhn didn't introduce himself to Piaget, then he shouldn't bother coming back home! Meanwhile, as late as 1965, Kuhn recommended the 1948 edition of Werner's *Comparative Psychology of Mental Development* to a correspondent who had asked for reading suggestions about the psychology of perception.²⁰

Kuhn's encounters with developmental psychology were rather bookish. He interacted more directly with acolytes of the "New Look" school of experimental psychology. Jerome Bruner and Leo Postman's work became especially important. Like Kuhn, both Bruner and Postman had completed their Ph.D.s at Harvard in the 1940s, and both were young professors in the new Department of Social Relations while Kuhn was in the Society of Fellows. In 1949 they published their paper, "On the perception of incongruity: A paradigm." More than the word "paradigm" impressed Kuhn; he would later devote several pages of *Structure* to summarizing their findings. Bruner and Postman measured how much time test subjects required to identify playing cards, and found that subjects took longer to identify cards that were intentionally "incongruous" (or, as Kuhn would later say, "anomalous"), such as a red five of clubs or a black ten of hearts. To Bruner and Postman, and to Kuhn, the study highlighted how much a person's pre-existing ideas or expectations could impact seemingly "direct" empirical observation.²¹

Following his three years in the Society of Fellows, Kuhn began teaching full time in the General Education program at Harvard. As early as 1951, he began to incorporate his various psychological encounters during his Society fellowship in his thinking about science. In his Lowell Lectures that year, he emphasized Gestalt-like

switches among conceptual orderings of the world and the roles that psychological “predispositions” play in scientists’ observations. Two years later he was invited to contribute the entry on “history of science” for the *International Encyclopedia of Unified Science*. The commission—likely an outgrowth of his participation in a discussion group on the “Unity of Science” organized by Harvard physicist and philosopher Philipp Frank—set Kuhn on the path to writing *Structure*.²²

In 1956 Kuhn left Harvard to take up a joint appointment in History and Philosophy at Berkeley. In addition to bringing him into close contact with philosophers Paul Feyerabend and Stanley Cavell, the move also solidified his connections with psychologists. Leo Postman, for example, had left Harvard for Berkeley in 1950, and it appears that he and Kuhn reconnected once Kuhn arrived. When describing Bruner and Postman’s famous experiment with the playing cards in *Structure*, Kuhn reported that “My colleague Postman tells me that, though knowing all about the apparatus and display in advance, he nevertheless found looking at the incongruous cards acutely uncomfortable.”²³ Kuhn spent the academic year 1958-59 at the nearby Center for Advanced Study in Behavioral Sciences at Stanford University. He later recalled the year as a pivotal one in his writing of *Structure*—an early draft of the book from that time survives in his papers—and while there he befriended James Jenkins, a cognitive psychologist and psycholinguist from the University of Minnesota who was also spending a sabbatical year at the Stanford center. A few years after their stint Jenkins reminisced with Kuhn about “the good old days” they had shared there; the two remained in touch for years afterward.²⁴

During that same time, Kuhn began to meet with psychiatrists and psychoanalysts in the San Francisco area. As he later reported to a psychiatrist acquaintance, Kuhn had “given one seminar to a part of the group, and have had numerous other opportunities to explore the extent and nature of their interest” in the themes that would eventually appear in *Structure*. “All of this has been good for my intellect and my morale.”²⁵ He had read Freud’s *Psychopathology of Everyday Life* as a young man and had also undergone psychoanalysis during the 1940s. Later in life he seemed uncertain whether psychoanalysis had much therapeutic power (at least for himself), but “it sure as hell is interesting.” In his discussions with psychiatrists and psychoanalysts, he drew a distinction between “psychoanalysis as therapy and psychoanalysis as science.” The latter, it seems, offered Kuhn one more resource with which to think about the active role played by an individual’s mind in construing the facts of experience: the “same” situation could be interpreted differently by individuals in distinct mental states, pointing (as much as the “New Look” perception studies did) to the priority of mental particulars over seemingly empirical evidence from the outside world.²⁶

With these psychological influences and relationships in mind, we may consider Kuhn’s rhetorical strategies throughout *Structure*. Though the book was commissioned as an exercise in the history of science, virtually every important philosophical claim in the book—antipositivism and theory-laden observations, radical ruptures between periods of normal science, incommensurability and the notion of “different worlds” for inhabitants of successive paradigms—is motivated by analogy to experimental psychology, rather than defended by close historical analysis of primary sources. In section 6 on anomalies and perception, for example, Kuhn writes of the Bruner-Postman experiment,

Either as a metaphor or because it reflects the nature of the mind, that psychological experiment provides a wonderfully simple and cogent schema for the process of scientific discovery. In science, as in the playing card experiment, novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation. Initially, only the anticipated and usual are experienced even under circumstances where anomaly is later to be observed.²⁷

In section 8 on scientists' responses to crises, Kuhn invokes "the switch of a gestalt, particularly because it is today so familiar," and hence can serve as "a useful prototype for what occurs in full-scale paradigm shift," even if the implications of such ruptures for understanding the practice of science "demand the competence of the psychologist even more than that of the historian."²⁸

To counter the positivists' long quest for a "pure observation-language," Kuhn writes in section 10: "modern psychological experimentation is rapidly proliferating phenomena with which that theory can scarcely deal." Experiments with the duck-rabbit gestalt image demonstrate that people with "the same retinal impressions can see different things," while experiments with inverting lenses demonstrate that people with "different retinal impressions can see the same thing." Indeed, Kuhn concludes, "psychology supplies a great deal of other evidence to the same effect"—evidence of the sort that filled Werner's 1948 book.²⁹ In that same section, Kuhn draws on the notion of schemas from developmental psychologists like Piaget and Werner to argue that

The child who transfers the word 'mama' from all humans to all females and then to his mother is not just learning what 'mama' means or who his mother is. Simultaneously he is learning some of the differences between males and females as well as something about the ways in which all but one female will behave toward him. His reactions, expectations, and beliefs—indeed, much of his perceived world—change accordingly.

The same process occurs, argues Kuhn, as scientists learn to apply concepts to make sense of the world holistically rather than “piecemeal or item by item.”³⁰

Residues from Kuhn’s encounters with specific psychological traditions thus run throughout *Structure*. Developmentalists’ focus on children’s learning processes, with their nonlinear, noncumulative stages, provided a framework with which to make sense of Kuhn’s own “Aristotle experience.” The demonstrations by “New Look” perception psychologists that predispositions affect individuals’ perception and cognition bolstered Kuhn’s antipositivist notions of theory-laden observations and the roles of anomalies. And his interest in psychoanalysis further focused his attention on individuals’ active construal of the givens of experience. Each of these traditions offered Kuhn important insights with which to challenge logical positivism.

Interlocutors

Among the earliest letters that Kuhn received after *Structure* was published came from yet another Harvard psychologist, Edwin G. Boring. A past president of the American Psychological Association, Boring had long been a champion of experimental psychology. In 1934 he had been instrumental in convincing Harvard’s president Conant to create a Department of Psychology separate from Philosophy, and he had spent much of the 1930s focused on experimental studies of perception. He was among the first researchers in the United States to focus on Gestalt images such as the famous cartoon by W. E. Hill entitled, “My wife and my mother-in-law,” on which Boring published an article in *The American Journal of Psychology* in 1930. (Boring called such images

“ambiguous images” rather than Gestalt images, and after his 1930 article they were sometimes referred to as “Boring images” in the United States.) Boring had also written extensively on the history of the field, including his landmark *History of Experimental Psychology*, first published in 1929 and expanded in 1950.³¹

Boring had retired in 1949, though he remained active on campus until his death in 1968. Kuhn and his wife Kay had known Boring’s assistant during Kuhn’s graduate-student days, and Kuhn sent a copy of *Structure* to Boring upon its release. Boring was immediately impressed, and he dashed off a letter to Kuhn in early November 1962. “Your book is brilliant and deserves to have an enormous effect,” Boring wrote. “You say very much that seems to me to be exceptionally wise, and you say it with clarity and charm. Whatever little bit I can do to get people to read you shall be done.”³²

Boring was as good as his word. In a flurry of letters over the next two months, Boring reported that he was revising his upcoming Presidential Address to the International Congress of Psychology to incorporate Kuhn’s notion of paradigms and the abrupt “all-or-none” transitions between them. He also wrote an unsolicited essay review of *Structure* for the journal *Contemporary Psychology*—a journal he had founded in 1956 but no longer edited—and sent an unsolicited endorsement to the publicity department of the University of Chicago Press, encouraging them to use it in marketing the book. As Boring wrote to Kuhn in late November 1962, “it seems to me probable that the publisher [of *Structure*] does not know that this is a book about psychology. I know that it is, and I can make it clear to psychologists that it is.” Kuhn replied immediately to thank Boring for being so supportive, not least because “Your interest and support will certainly gain

attention for my book among a group that I had hoped, but not altogether expected, to reach. At this moment, there is nothing that interests me more.”³³

Other readers shared Boring’s intuition that *Structure* was “a book about psychology.” Of the 91 correspondents who wrote to Kuhn about the book during the first five years after its publication, the largest share—nearly 20%—came from psychology. Even after members of other academic fields began to engage more substantively with the book, including philosophers and sociologists, psychologists still dominated Kuhn’s correspondence about the book, comprising nearly twice as large a portion as any other field.³⁴ (See Table 1.)

Field	Percentage of correspondents, 1962-1967 (N = 91)	Percentage of correspondents, 1962-1981 (N = 170)
Psychology	18.7	14.7
History of Science	12.1	7.6
Physics	7.7	6.5
History	6.6	4.1
Sociology	5.5	8.8
Economics	5.5	4.7
Biology	4.4	2.9
Theology	3.3	2.9
Philosophy	3.3	5.3
Philosophy of Science	3.3	5.9
Medicine	3.3	3.5
Chemistry	3.3	2.4
English	1.1	2.4
Linguistics	1.1	1.2
Art History	1.1	0.6
Mathematics	1.1	1.8
Political Science	0	2.9

Kuhn’s friend James Jenkins, the psycholinguist who had spent the 1958-59 academic year with Kuhn at Stanford’s Center for Advanced Study in the Behavioral Sciences, confirmed the trend. “I enjoyed your book immensely and intensely,” Jenkins wrote to Kuhn in the spring of 1965. “I stayed up all night reading it and even went out and *bought* a copy!” He was eager to let Kuhn know that other colleagues in his field had

also noticed the book. “I don’t know whether you know how much your book is being cited and is influencing current thought,” Jenkins continued, and he enclosed a recent review from the *Journal of Linguistics* that had quoted extensively from Kuhn’s *Structure* to herald a contemporary “paradigm shift” within the field.³⁵

Several other psychologists wrote to express similar sentiments. Many proclaimed that reading the book changed how they thought about their discipline, which they argued was in the midst of its own “revolution.” They read *Structure* as a template for what it is to be scientific and argued on that basis that psychology had at last become a legitimate science. A few told Kuhn that they were requiring their students to read the book, and more than one assigned students the task of writing a paper on an episode in the recent history of psychology using the framework articulated in *Structure*.³⁶ Influential psychologists like Donald Campbell and Abraham Maslow praised the book—as early as the winter of 1962-63 Campbell wrote that *Structure* had become “my favorite way of introducing students to a proper perspective on science,” while Maslow reported that his iconoclastic book, *The Psychology of Science* (1966), was “much influenced by your monograph—which I read with fascination and admiration and gratitude.”³⁷ In 1967 Kuhn was invited to deliver an address on “Logic and Psychology” at the annual meeting of the American Psychological Association; at the meeting, younger psychologists took the opportunity to tell Kuhn directly how much his book had influenced their thinking. Two years after Kuhn’s address at the APA, the president of the association featured Kuhn’s book in his Presidential Address.³⁸

Kuhn’s psychologist-interlocutors did more than just praise the book. Many pressed Kuhn on specific points. Some wondered why Kuhn had not made even more

explicit use of Piaget's work or terminology, since (to them) the projects were obviously so similar. As one psychologist explained to Kuhn, "You use assimilation to cover Piaget's assimilation and accommodation, but you probably do not feel this is a problem. According to Piaget, acquiring mastery of a new exemplar would be predominantly accommodation. Solving new problems by means of it would be primarily assimilation. Most problems are a bit of both, which is one way things evolve."³⁹

Like the Piaget enthusiasts, several psychologists read Kuhn's book as a treatise on how individual scientists think. Their focus on individual cognition steered their questions. Edwin Boring wondered whether Kuhn thought a paradigm resided in an individual's conscious or unconscious thinking, while a psychologist from Bennington College explained that in his understanding, paradigms refer to an individual researcher's "implicit assumptions, methodological biases and predispositions which lead to ways of thinking and operating." A psychiatrist wrote that Kuhn's *Structure* captured something essential about what it was like to work with psychiatric patients, since the therapist must "work hard with them to change the paradigms in which they construe their experience. In work with a satisfactory patient, one finds repeatedly that there is a succession of revolutions, each of which culminates in a new 'insight'"—an example at the individual level of what Kuhn suggested must occur across an entire scientific community. Like Boring, who was content to gesture that an individual's apprehension of a prevailing paradigm is "in large measure carried in the stream of the *Zeitgeist*," the psychiatrist saw no need for intervening levels of explanation—no social, institutional, or pedagogical mechanisms that might account for the synecdoche of individual and group.⁴⁰

Kuhn, too, continued to emphasize perception by individual knowers, rather than social structures or pedagogical institutions, when pressed by psychologists to clarify what he meant by “paradigm.” Boring had gently chided Kuhn that it was “hard for me to pin you down [on the meaning of ‘paradigm’] by going through your book,” and other psychologists had expressed similar frustrations, all before linguist Margaret Masterman identified twenty-one distinct meanings of the word throughout *Structure*. To Robert Watson, a psychologist at Northwestern University, Kuhn conceded late in 1964 that “I have let myself in for a good deal of misunderstanding by using the term ‘paradigm’ in too many different ways.”⁴¹

Kuhn found an opportunity to try to clarify what he meant by “paradigm” as he prepared his remarks on “Logic and Psychology” for the American Psychological Association meeting in 1967. He sought to emphasize the importance of paradigms as exemplars, concrete problems or techniques that science students learn to manipulate in the course of their training. Kuhn argued that science students must learn to perceive similarities between a new problem and one that they had already learned to solve. Such “a perception of similarity” is “both logically and psychologically prior” to any explicit criteria or definitions that might have been adduced. With an audience of psychologists in mind, Kuhn invoked the distinction between “stimuli” and “sensations,” and the “vast amount of neural processing” within an individual’s head required to convert the former to the latter. Though such learning processes were irreducibly “neural,” Kuhn suggested, they were not entirely “innate”: “To an extent still unknown, the production of data from stimuli is a learned procedure.”⁴²

Rather than illustrate the point with examples from the history of science, Kuhn asked his audience at the APA meeting to imagine a child and parent walking around a zoo, as the child learned to distinguish ducks, geese, and swans—not by memorizing definitions or applying explicit criteria, but by repeated exposure to specific examples. The result: “During the afternoon, part of the neural mechanism by which he [the child] processes visual stimuli has been reprogrammed, and the data he receives from stimuli which would all earlier have evoked ‘bird’ have changed.” Here, Kuhn believed, was the essence of how paradigms—as-exemplars worked in science. Several years later, Kuhn’s extended example of ducks, geese, and swans from his APA lecture appeared as the last section of his essay, “Second thoughts on paradigms,” to illustrate his notion of exemplars.⁴³

Kuhn’s reliance upon categories like perception, stimuli, and sensations clearly resonated with his psychology readers. “Kuhn is wise in choosing perception for his paradigm of paradigms,” Boring had announced in his essay review for *Contemporary Psychology*, and privately he wondered why Kuhn had not included any Gestalt images in the book.⁴⁴ Yet others were less convinced. A graduate student in philosophy of science noted that Kuhn’s *Structure* “relies heavily on psychology—Gestalt switches, etc.,” and wondered whether the perception studies were invoked merely as analogy or as central to his argument. Kuhn replied that his use of Gestalt experiments was meant to be metaphorical, “because some of the disanalogies are very severe, especially one’s ability to switch back and forth between the two modes of seeing which the diagram allows.” But Kuhn averred that the psychological perception studies offered more than mere metaphor: “On the other hand, I do not think that it is merely an accident that

fundamental experiments about perception should also illuminate the nature of scientific knowledge and its changes. Doubtless, some of the same fundamental neural processes are involved in both.” Unlike the audience at the APA, the retreat to individual “neural processes” did not impress the philosophy student, who replied with cautions about overhasty reductionism in the light of the perennial mind-body problem.⁴⁵

Readers from other academic backgrounds responded to different themes within Kuhn’s book. He heard from several physicists, for example, some of whom he had known from his earlier studies but most of whom wrote to him, just as most psychologists did, having only encountered his book. None of the physicists asked Kuhn about individual cognition, whether in Piagetian terms or in the framework of neural processing. Instead, several pressed him to elaborate or clarify some of his more sociological suggestions. Mendel Sachs, theoretical physicist at SUNY Buffalo, for example, thought Kuhn’s book was primarily about “bandwagons” or fashions in science—that is, the tendency (unfortunate, in Sachs’s view) for whole communities of researchers to follow a dominant research trend rather than explore unpopular alternatives. Another physicist wondered if Kuhn was correct that few broadly shared value judgments within a given scientific community persist across scientific revolutions: didn’t the community of physicists agree on some measures of what counted as worthwhile research even as particulars of their research programs came and went? Still another physicist congratulated Kuhn for having highlighted so clearly the roles of competing “schools” within a developing scientific field. Of the physicists who wrote to Kuhn, in other words, most read *Structure* as a sociological treatment—and either applauded it or criticized it as such—but none pursued it as an exercise in psychology.⁴⁶

Still the letters from psychologists kept arriving. Soon after Kuhn's APA address he heard from Alfred Fuchs, chair of the Psychology Department at Bowdoin College. Fuchs noted that Kuhn had mentioned in his talk that he had at least two meanings of the word "paradigm" in mind, and Fuchs wondered whether Kuhn had any materials he could share on the topic. Kuhn did not—"I have as yet prepared nothing for publication on this subject"—and he proceeded to sketch for Fuchs the additional clusters of meanings he now wished to clarify:

One use of the term 'paradigm' in my book is to denote the entire matrix of belief and technique shared by the members of a given professional community. I should now prefer to refer to this as something like 'the professional matrix,' though the phrase does not please me very much. Elements in that matrix would now include such things as: accepted laws, definitions, and other general statements; relevant metaphysical commitments, preferred metaphors, and interpretations of abstract theories; and, as a third major element, paradigms. That use of 'paradigms' is the second one which emerges in the book, and, for me, it is the more important one. In this sense, paradigms are restricted entirely to particular concrete examples of the way in which members of the scientific community in question practice their trade. They are the vehicles of the apprenticeship and practice-problem-solving components of scientific education. In my talk at the A.P.A. they were replaced, for simplicity, by concrete exposure to swans, ducks, and geese.

Over the next year and a half, Kuhn would expand this outline into his famous 1969 postscript. Most of the phrasing stayed the same, though he edited "professional matrix" to "disciplinary matrix."⁴⁷

Kuhn, *Structure*, and the Social

Kuhn's 1969 postscript to *Structure*, with its description of a "disciplinary matrix," moved concertedly beyond a description of individual knowers, let alone neural processing within a given brain. The postscript serves as a useful reminder that Kuhn was not formally trained in psychology, nor were his academic interests limited to that field.

Far from it—he read eclectically from his early years, and part of the appeal of *Structure* was surely the fact that the book spoke so eloquently, and concisely, about a range of interesting ideas. Many scholars other than psychologists picked up *Structure* and pursued its ideas, sometimes (as in the case of certain sociologists of science) in directions that drove Kuhn to distraction; certainly not all readers found only lessons about individual cognition in the book.

Nevertheless, Kuhn actively sought out psychologists as an audience, even asking some practitioners (beyond Boring) to review the book in psychology journals to make sure it reached other psychologists' attention.⁴⁸ When psychologists did find the book, there was plenty of material to interest them, in an idiom that seemed familiar.

Kuhn's appropriation of insights from specific psychological traditions—developmentalists like Piaget and Werner, "New Look" experimentalists like Bruner and Postman, and psychoanalysis—provided him with impressive material with which to challenge the logical positivists: a neutral observation-language seemed impossible; identical retinal images could produce wildly different concepts and conclusions; pre-existing expectations (regarding the colors of suits on playing cards, for example) could affect the "neural processing" within an individual's head, as "stimuli" became "sensations." The Gestaltists' duck-rabbit flips, and the way a child learns to sort ducks from geese and swans, provided potent resources on which to build an antipositivist, noncumulative account of science.

Late in life, during a lengthy and fascinating interview with three historians and philosophers of science, Kuhn said that his goal as an historian had always been "to get inside a person's head"—always in the singular, and always the head rather than, say,

scientists' benchtop apparatus, social groups, or institutional settings.⁴⁹ When he defended his position against Popper's in his essay on "Logic of discovery or psychology of research," Kuhn wrote that Popper dismissed "the psychological drives of individuals" as irrelevant, yet, Kuhn insisted, a proper understanding of scientific practice "must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced"—the types of social and institutional trappings that he would later invoke under the banner of "disciplinary matrix." Though he could label them and gesture toward their importance, Kuhn conceded that he would go no further. At that point, he wrote, "my sense that I control my subject matter ends."⁵⁰

Kuhn's road toward *Structure*, and his fruitful discussions with psychologists after the book appeared, thus help to make sense of later historians' somewhat ambivalent relationship with the book. Several historians, like myself, have found inspiration in Kuhn's frequent mentions of the formative role of scientists' training or the centrality of textbooks, only to search in vain throughout his writings for any detailed historical analysis of such critical features of scientific life. Instead, the year after Kuhn's "Second thoughts on paradigms" appeared in print, a different model for such studies became available. Michel Foucault's *Discipline and Punish* offered, in its own way, what Kuhn had called for in his critique of Popper: a potent analysis of the institutions through which various "value systems," "ideologies," and practices are "transmitted and enforced." Combining insights from Foucault as well as Kuhn, a new generation of historians of science has sought to make sense of science in this altogether more fleshy idiom: beyond the heads of individual knowers, attuned to the culturally specific, politically situated

infrastructures in which knowledge has been produced, mesmerized by the ever-shifting entanglement of ideas and institutions.⁵¹

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¹Thomas Kuhn, “Reflections on my critics,” in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970), 231-278, on 231 (“I am tempted”); Kuhn, “Second thoughts on paradigms,” in *The Structure of Scientific Theories*, ed. Frederick Suppe (Urbana: University of Illinois Press, 1974), 459-482, on 459 (“monitoring conversations”), reprinted in Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 293-319; and Thomas Kuhn to Charles Christenson, 17 August 1973 (“shudder as I discover”), in Thomas S. Kuhn Papers, Institute Archives, Massachusetts Institute of Technology, collection MC240 (hereafter cited as *TSK*), folder 4:8. Cf. Roland Barthes, “The death of the author,” in Barthes, *Image, Music, Text*, trans. Stephen Heath (New York: Hill and Wang, 1977), 142-148. Barthes’s essay originally appeared in 1967. As noted in the preface to *Criticism and the Growth of Knowledge*, the London symposium was held in July 1965 and Kuhn completed his “Reflections” essay in 1969. Likewise, although “Second thoughts” was first published in 1974, Kuhn had completed the text in 1969. See, e.g., Michael Barkun to Thomas Kuhn, 17 September 1969, in *TSK*, folder 4:6.

²Kuhn had written the 1969 postscript as an introduction to the Japanese translation of the first edition, but realized it could be helpful for English readers, too: Kuhn, “Postscript–1969,” in Kuhn, *The Structure of Scientific Revolutions*, 4th ed. (Chicago: University of Chicago Press, 2012 [1962]), 173n1. On Kuhn’s other attempts to clarify his original intentions, see esp. Kuhn, *Essential Tension*; Thomas Kuhn, “Afterwords,” in *World Changes: Thomas Kuhn and the Nature of Science*, ed. Paul Horwich (Cambridge: MIT Press, 1993), 311-341; and Thomas Kuhn, *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview*, ed. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000).

³The relevant correspondence may be found in *TSK*, box 4, folders 6-16. These folders contain correspondence specifically in response to *Structure*, organized alphabetically by correspondent’s last name. Even in cases in which Kuhn corresponded with particular people about other topics, the correspondence related to *Structure* appears in *TSK* folders 4:6-16. For example, most of Kuhn’s correspondence with his longtime friend, the physicist H. Pierre Noyes, is in *TSK* folder 11:51, except for their letters about *Structure*, which are in *TSK* folder 4:13.

⁴On Kuhn’s use of a dictaphone to compose his letters see, e.g., Thomas Kuhn to Carl Swanson, 21 January 1963, in *TSK* 4:15; and Kuhn to Dennis A. Rohatyn, 6 January 1969, in *TSK* folder 4:14. On dialogic renderings of the meanings of a text, see Mikhail Bakhtin, *The Dialogic Imagination*, ed. Michael Holquist, trans. Caryl Emerson and Michael Holquist (Austin, TX: University of Texas Press, 1981).

⁵Edwin G. Boring to Thomas Kuhn, 7 December 1962, in *TSK* 4:7 (“intracranial determinism”).

⁶On sales and translations, see Lawrence Van Gelder, “Thomas Kuhn, 73: Devised science paradigm,” *New York Times* (19 June 1996): B7. On journal sections, see *Modern Intellectual History* 9, no. 1 (April 2012): 73-147; *Social Studies of Science* 42, no. 3 (June 2012): 415-480; and *Historical Studies in the Natural Sciences* 42, no. 5 (November 2012): 476-580.

⁷On initial print run, see “Book estimate and release” form, dated 25 February [?] 1962, available at http://chss.uchicago.edu/Materials_from_Kuhn_book_files.pdf (last accessed 6 February 2013). Kuhn’s author questionnaire for the University of Chicago Press, dated 25 March 1962, is available at the same site. Sales figures for 1962-64 from Ian Hacking, “Introductory essay,” in Thomas Kuhn, *The Structure of Scientific Revolutions*, 4th ed. (Chicago: University of Chicago Press, 2012 [1962]), vii-xxxvii, on xxxvii. Unless otherwise noted, all quotations from *Structure* will be from the 4th edition.

⁸Garrett Hardin to Thomas Kuhn, 14 December 1962, and Kuhn to Hardin, 21 January 1963, both in *TSK* 4:10. Kuhn repeated the phrase about his sticking his “neck very far out” in Kuhn to Carl Swanson, 21 January 1963, in *TSK* 4:15; Kuhn to Donald Campbell, 22 October 1963, in *TSK* 4:8; and Kuhn to George J. Stigler, 24 October 1963, in *TSK* 4:15. Kuhn’s correspondence throughout 1963 makes repeated reference to the delays in receiving his mail while overseas. The reprint Hardin had enclosed was Garrett Hardin, “The competitive exclusion principle,” *Science* 131 (29 April 1960): 1292-1297. See also Hardin, “The tragedy of the commons,” *Science* 162 (13 December 1968): 1243-1248.

⁹Among the faculty who reported to Kuhn on their use of his book in their teaching, see also Francis G. Haber (history department, University of Florida at Gainesville) to Kuhn, 22 December 1964, in *TSK* folder 4:10; J. Bruce Brackenridge (physics department, Lawrence University, Wisconsin) to Kuhn, 23 May 1967, in *TSK* folder 4:7; Robert A. Lufburrow (physics department, St. Lawrence University) to Kuhn, 9 June 1967, in *TSK* folder 4:11; John J. Beer (history department, University of Delaware at Newark) to Kuhn, 8 February 1968, in *TSK* folder 4:7; Ronald Abler (geography department, Pennsylvania State University) to Kuhn, 10 April 1968, in *TSK* 4:6; Thomas C. Cadwallader (psychology department, Indiana State University) to Kuhn, 19 August 1970, in *TSK* folder 4:8; James G. Blight (psychology department, Grand Valley State Colleges, Michigan) to Kuhn, 5 February 1979, in *TSK* folder 4:7; George Goodin (English department, Southern Illinois University at Carbondale) to Kuhn, 16 November 1979, in *TSK* folder 4:9. Abler and Blight mentioned in their letters that they had sent their students’ papers to Kuhn.

¹⁰Peter Fox to Thomas Kuhn, 9 January [no year printed, mid-1960s], in *TSK* folder 4:9. On annual sales, see Dorothy B. Ebersole (Kuhn’s assistant) to James J. Jenkins, 11 September 1975, in *TSK* folder 4:10. On blurred boundaries at the time between textbooks, scholarly monographs, and popular books, see David Kaiser, “A tale of two textbooks: Experiments in genre,” *Isis* 103 (2012): 126-138.

¹¹Diania Jackson to Mack Printing Company (Easton, Pennsylvania, publisher of *American Scientist*), 14 October 1969 (“Kuhn’s book is being used”), included as enclosure in Derek J. de Solla Price to Diania Jackson (Fairhaven College), 3 November 1969 (“horrid”), copies of which are in *TSK* folder 4:13.

¹²Thomas Kuhn to J. Bruce Brackenridge, 26 May 1967, in *TSK* folder 4:7 (“increasingly and painfully realized”); Kuhn to Marvin Barsky, 2 March 1970, in *TSK* folder 4:6 (“getting perhaps a letter a week”); and Kuhn to Terry R. Kandall, 20 June 1973, in *TSK* folder 4:8. Kuhn sent a letter with identical wording to George Dalton on 16 August 1973, in *TSK* folder 4:8. On citations, see Eugene Garfield, “A different sort of great-books list: The 50 twentieth-century works most cited in the *Arts & Humanities Citation Index*, 1976-1983,” *Current Contents* 16 (20 April 1987): 3-7.

¹³John H. Van Vleck to Saul G. Cohen, 2 July 1953, in Harvard University Archives, Department of Physics collection, Box 3, folder “1953-54 Recommendations (A-L),” collection number UAV 691.10. My brief account of Kuhn’s composition of *Structure* owes much to Joel Isaac, *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (Cambridge: Harvard University Press, 2012), chap. 6, as well as to Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi, “A discussion with Thomas S. Kuhn,” reprinted in Kuhn, *The Road Since Structure*, 255-323.

¹⁴Thomas Kuhn, “What are scientific revolutions?,” reprinted in Kuhn, *Road Since Structure*, 13-32, on 16-17 (“I was sitting”; “some part of the flux”). Kuhn’s essay was originally published in *The Probabilistic Revolution*, ed. Lorenz Kruger, Lorraine Daston, and Michael Heidelberger (Cambridge: MIT Press, 1987), vol. 1, 7-20. Kuhn highlighted the “Aristotle experience” in Baltas, Gavroglu, and Kindi, “A discussion with Thomas S. Kuhn,” 275-276. See also Kuhn, *Structure of Scientific Revolutions*, xxxix; and George Reisch, “Aristotle in the Cold War: On the origins of Thomas Kuhn’s *Structure of Scientific Revolutions*,” paper presented at the University of Chicago, 30 November 2012.

¹⁵Isaac, *Working Knowledge*, 215-217.

¹⁶Jean Piaget, *The Child’s Conception of Physical Causality*, trans. Marjorie Gagain (London: Routledge and Kegan Paul, 1930), 24 (“air pushes it”), 238 (“If the child’s mind”); see also 20-23, 117-118, 237-240. On Piaget’s work and influence, see Fernando Vidal, *Piaget Before Piaget* (Cambridge: Harvard University Press, 1994).

¹⁷Jean Piaget, *The Child’s Conception of Movement and Speed*, trans. G. E. T. Holloway and M. J. MacKenzie (New York: Basic Books, 1970 [1946]), 29-30, 32; Kuhn, *Structure*, 118.

¹⁸Herman Witkin, “Heinz Werner, 1890-1964,” *Child Development* 36 (1965): 306-328, on 309-310; Mitchell Ash, *Gestalt Psychology in German Culture, 1890-1967: Holism and the Quest for Objectivity* (New York: Cambridge University Press, 1995), 317; Gordon Allport, “Foreword,” in Heinz Werner, *Comparative Psychology of Mental Development*, 2nd ed. (New York: Follett, 1948 [1940]), ix-xii. The first German edition of *Einführung in die Entwicklungspsychologie* appeared in 1926. On Allport and the founding of the Department of Social Relations, see Isaac, *Working Knowledge*, 175-176, 180-181.

¹⁹Werner, *Comparative Psychology*, quotations on 22; see also 5, 15-17.

²⁰Kuhn, *Structure*, xl-xli; Kay Kuhn, personal communication at the Boston University symposium on the anniversary of *Structure*, March 2012; Thomas Kuhn to Richard Gordon, 4 May 1965, in *TSK* 4:9.

²¹Jerome Bruner and Leo Postman, “On the perception of incongruity: A paradigm,” *Journal of Personality* 18 (1949): 206-223; Kuhn, *Structure*, 62-64. See also

Jerome Bruner, *In Search of Mind: Essays in Autobiography* (New York: Harper and Row, 1983), 67-71, 85-86. On Bruner and the emergence of “cognitive psychology” at the time, see also Jamie Cohen-Cole, “Instituting the science of mind: Intellectual economies and disciplinary exchange at Harvard’s Center for Cognitive Studies,” *British Journal for the History of Science* 40 (2007): 567-597; Cohen-Cole, “The creative American: Cold War salons, social science, and the cure for modern society,” *Isis* 100 (2009): 219-262; and Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature* (Chicago: University of Chicago Press, 2014), chaps. 6-7.

²²Isaac, *Working Knowledge*, 219-221. On Frank and the “Unity of Science” movement, see also Gerald Holton, “From the Vienna Circle to Harvard Square: The Americanization of a European World Conception,” in *Scientific Philosophy: Origins and Developments*, ed. Friedrich Stadler (Boston: Kluwer, 1993), 47-73; and Peter Galison, “The Americanization of unity,” *Daedalus* 127 (1998): 45-71.

²³Kuhn, *Structure*, 64n13; Baltas, Gavroglu, and Kindi, “A discussion with Thomas S. Kuhn,” 296-298, 300; and K. Brad Wray, “Kuhn and the discovery of paradigms,” *Philosophy of the Social Sciences* 41 (2011): 380-397, on 384.

²⁴James Jenkins to Thomas Kuhn, n.d. (ca. April 1965) (“good old days”), and Jenkins to Kuhn, 9 September 1975, in *TSK* 4:10. The early draft of *Structure*, dated 1958-60, may be found in *TSK* 4:5.

²⁵Thomas Kuhn to Harley Shands, 12 March 1963, in *TSK* 4:15.

²⁶Baltas, Gavroglu, and Kindi, “A discussion with Thomas S. Kuhn,” 280 (“sure as hell is interesting”); Thomas Kuhn to Maxwell Gitelson, 18 December 1964, in *TSK* 4:9 (“psychoanalysis as therapy”). On Kuhn’s early experiences with psychoanalysis, see also Jensine Andresen, “Crisis and Kuhn,” *Isis* 90 (1999): S43-S67; and John Forrester, “On Kuhn’s case: Psychoanalysis and the paradigm,” *Critical Inquiry* 33 (2007): 782-819, esp. 784-789. My thanks to Brad Wray for bringing Forrester’s paper to my attention.

²⁷Kuhn, *Structure*, 64.

²⁸Kuhn, *Structure*, 85-86.

²⁹Kuhn, *Structure*, 126.

³⁰Kuhn, *Structure*, 128.

³¹Edwin G. Boring, “A new ambiguous figure,” *American Journal of Psychology* 42 (1930): 444-445; Boring, *A History of Experimental Psychology*, 2nd ed. (New York: Appleton, 1950 [1929]); Boring, *Sensation and Perception in the History of Experimental Psychology* (New York: Appleton, 1942); S. S. Stevens, “Edwin Garrigues Boring,” *Biographical Memoirs of the National Academy of Sciences* (1973): 41-76; and Isaac, *Working Knowledge*, 100-102.

³²Edwin G. Boring to Thomas Kuhn, 9 November 1962, in *TSK* 4:7. On the Kuhns’ relationship with Boring’s secretary, Edith Annin, see Kuhn to Boring, 29 November 1962, and Annin’s postscript added to Boring to Kuhn, 7 December 1962, in *TSK* 4:7.

³³Boring to Kuhn, 26 November 1962 (including a draft of Boring’s upcoming Presidential Address); Kuhn to Boring, 29 November 1962, in *TSK* 4:7. Boring sent his endorsement to the Publicity Department of the University of Chicago Press (with a copy to Kuhn) on 26 November 1962, and sent a draft of his review for *Contemporary*

Psychology to Kuhn on 4 December 1962. In all, Boring and Kuhn exchanged ten letters between 9 November and 18 December 1962, all in *TSK* 4:7.

³⁴*Structure* was published in September 1962 and correspondence in response to the book began several weeks later, hence the period labeled “1962-67” includes five years and two months. Most correspondents wrote to Kuhn on letterhead stationary, which clearly identified their institutional and departmental affiliations. For all but a handful of the remaining letters, unambiguous identifications could be made from the contents of the letters or titles listed below signatures. No identifications were possible for a modest fraction of correspondents (2.2% between 1962-67, 6.5% between 1962-81). In addition to the fields listed in Table 1, other groups include students (4.4% between 1962-67, 5.9% between 1962-81); university administrators (3.3% between 1962-67, 2.4% between 1962-81); and lawyers (1.1% between 1962-67, 1.2% between 1962-81). In Table 1, I have distinguished historians of science from general historians. During the early period (1962-67), many of the letters that Kuhn received from historians of science were brief acknowledgments from close colleagues of receipt of the book rather than substantive letters, and hence quite different in character from the letters Kuhn received from general historians. See, e.g., Harry Woolf to Thomas Kuhn, 11 December 1962, in *TSK* 4:16; Charles Gillispie to Thomas Kuhn, 21 January 1963, in *TSK* 4:9; Alexandre Koyré to Thomas Kuhn, 9 August 1963, in *TSK* 4:11; Melvin Kranzberg to Thomas Kuhn, 19 July 1965, in *TSK* 4:11; Derek J. de Solla Price to Thomas Kuhn, 16 December 1965, in *TSK* 4:13.

³⁵Jenkins to Kuhn, n.d. (ca. April 1965), in *TSK* 4:10. Jenkins enclosed a photostat of James P. Thorne’s review of Paul Postal’s *Constituent Structure: A Study of Contemporary Models of Syntactic Description*, which had appeared in *Journal of Linguistics* 1 (April 1965): 73-76.

³⁶Kenneth Hammond to Thomas Kuhn, 15 February 1963, in *TSK* 4:10; David Smith to Kuhn, 15 June 1964, in *TSK* 4:15; Robert Lissitz to Kuhn, 28 September 1964, in *TSK* 4:11; Stewart Perry to Kuhn, 2 December 1964, in *TSK* 4:13; Alfred Fuchs to Kuhn, 15 September 1967, in *TSK* 4:9; Robert Ravich to Kuhn, 17 September 1967, in *TSK* 4:14; Danny Moates to Kuhn, 2 January 1969, in *TSK* 4:12; Wayne Lazar to Kuhn, 7 March 1969, in *TSK* 4:11; Walter Weimer to Kuhn, 30 October 1969, in *TSK* 4:16; Jacobo Valero to Kuhn, 17 March 1970, in *TSK* 4:16; and Thomas Cadwallader to Kuhn, 19 August 1970, in *TSK* 4:8.

³⁷Donald Campbell to Thomas Kuhn, n.d. (ca. January 1963), in *TSK* 4:8; Abraham Maslow to Kuhn, n.d. (ca. January 1967), in *TSK* 4:12. See Abraham Maslow, *The Psychology of Science: A Reconnaissance* (New York: Harper and Row, 1966).

³⁸On Kuhn’s address at the APA, see Alfred Fuchs to Thomas Kuhn, 15 September 1967, in *TSK* 4:9; and Thomas Cadwallader to Kuhn, 19 August 1970, in *TSK* 4:8. See also George Miller, “Psychology as a means of promoting human welfare,” *American Psychologist* 24 (1969): 1063-1075, esp. 1066, 1070, which reproduces Miller’s September 1969 Presidential Address to the American Psychological Association.

³⁹Jane Loevinger to Thomas Kuhn, 21 April 1969, in *TSK* 4:11; see also Harley Shands to Kuhn, 30 January 1963, in *TSK* 4:15.

⁴⁰Edwin Boring to Thomas Kuhn, 7 December 1962, in *TSK* 4:7; Louis Carini to Kuhn, 30 June 1963, in *TSK* 4:8; Harley Shands to Kuhn, 15 March 1963, in *TSK* 4:15.

⁴¹Edwin Boring to Thomas Kuhn, 7 December 1962, in *TSK* 4:7; Thomas Kuhn to Robert Watson, 27 October 1964, in *TSK* 4:16. See also Louis Carini to Kuhn, 30 June 1963, in *TSK* 4:8. Cf. Margaret Masterman, “The nature of a paradigm,” in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970), 59-89. Masterman first delivered her paper in July 1965 at the International Colloquium in the Philosophy of Science in London.

⁴²Quotations from Kuhn, “Second thoughts on paradigms,” 308-309. It appears that Kuhn’s talk at the 1967 APA meeting is no longer extant in his papers, but from the descriptions of the talk in correspondence from the time, it seems clear that much of that presentation was included in his essay on “Second thoughts,” the first complete draft of which dates from ca. 1969, even though it was not published until 1974. For discussion of the APA lecture, see Alfred Fuchs to Thomas Kuhn, 15 September 1967, and Kuhn to Fuchs, 25 September 1967, in *TSK* 4:9. On references to drafts of “Second thoughts,” see also Jane Loevinger to Thomas Kuhn, 21 April 1969, in *TSK*, 4:11; Michael Barkun to Thomas Kuhn, 17 September 1969, in *TSK* 4:6; Kuhn to Bruce Kuklick, 2 March 1970, in *TSK*, 4:11. Kuhn cited the essay as “in press” in Kuhn, “Postscript—1969,” 174n3. On the evolution of Kuhn’s meanings of “paradigm,” see also Wray, “Kuhn and the discovery of paradigms.”

⁴³Kuhn, “Second thoughts on paradigms,” 309-10.

⁴⁴Boring, draft essay review of *Structure*, enclosed in Boring to Kuhn, 4 December 1962, in *TSK* 4:7 (“Kuhn is wise”); Boring to Kuhn, 26 November 1962, in *TSK* 4:7.

⁴⁵George Ballester to Thomas Kuhn, 26 May 1972 (“relies heavily on psychology”); Kuhn to Ballester, 9 June 1972 (“disanalogies are very severe,” “On the other hand”); Ballester to Kuhn, 7 July 1972, all in *TSK* 4:6.

⁴⁶Mendel Sachs to Thomas Kuhn, 10 September 1970, in *TSK* 4:15; T. E. Phipps to Kuhn, 25 June 1963, in *TSK* 4:13; G. Jona-Lasinio to Kuhn, 23 July 1973, in *TSK* 4:10. See also Leonard Loeb to Kuhn, 27 February 1963 and 15 April 1963, in *TSK* 4:11.

⁴⁷Thomas Kuhn to Alfred Fuchs, 25 September 1967, in *TSK* 4:9; Kuhn, “Postscript—1969.” One of the best explications of Kuhn’s two main clusters of meanings of “paradigm” remains Joseph Rouse, “Science as practice: Two readings of Thomas Kuhn,” in Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca: Cornell University Press, 1987), 26-40.

⁴⁸See, e.g., Kuhn to Harley Shands, 12 March 1963, in *TSK* 4:15.

⁴⁹Baltas, Gavroglu, and Kindi, “A discussion with Thomas S. Kuhn,” 276, 280.

⁵⁰Thomas Kuhn, “Logic of discovery or psychology of research?,” in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970), 1-23, on 21. See also Forrester, “On Kuhn’s case,” 789-790, 798-799.

⁵¹Michel Foucault, *Discipline and Punish*, trans. Alan Sheridan (New York: Pantheon, 1977 [1975]). See also, e.g., Andrew Warwick and David Kaiser, “Kuhn, Foucault, and the power of pedagogy,” in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge: MIT Press,

2005), 393-409; Cyrus Mody and David Kaiser, “Scientific training and the creation of scientific knowledge,” in *Handbook of Science and Technology Studies*, rev ed. (Cambridge: MIT Press, 2007), 377-402.