Introduction

Thomas Kuhn's *The Structure of Scientific Revolutions* is a remarkable book. It has sold over 1 million copies, a startling number for an academic title.[1] It has been translated into numerous languages, ensuring that its reach is nearly endless. People have wondered how such a book could have come to be written.

Some have suggested that the success of *Structure* was quite accidental (see Fuller 2000), or that any number of other contemporary books could have achieved the success that *Structure* achieved. But I believe that there is another story to tell that explains why *Structure* is the book that it is and had the impact it had.[2]

Kuhn had quite atypical experiences, even before the publication of *Structure*. George Reisch has described Kuhn's unusual early education at a number of alternative progressive private schools in the United States (see Reisch 2019; see also Kuhn 1997/2000, 256–259). Kuhn suggests that these schools "made a major contribution to [his] independence of mind" (Kuhn 1997/2000, 257). Kuhn's experiences when he attended Harvard were also atypical. Though he was studying physics, he was able to foster his interests in both science and the humanities by writing for *The Harvard Crimson*, the student newspaper (Kuhn 1997/2000, 264; see also 268).[3] Kuhn completed his

[1] A letter from Penelope Kaiserlian, of the University of Chicago Press, from 1986, indicates that by 1986 the Press had sold 12,761 hard copies of the book and 633,924 paperback copies (see Kaiserlian 1986). This was ten years before the publication of the third edition.

[2] Steve Fuller (2000) compares Kuhn to Chance Gardiner, the protagonist in Jerzy Kosiński's *Being There* (see Kosiński 1970). Chance's unreflective remarks are misinterpreted by others as being profound. Thus, people are led to think of Chance as a very sophisticated and insightful thinker rather than the simple person that he is. Fuller believes that we have, similarly, inadvertently projected a profoundness on to Kuhn that is wholly undeserved. Further, Fuller suggests that our misunderstanding of what Kuhn wrote has shifted our thinking about science in quite radical ways, and not for the better.

[3] Further, as an undergraduate, Kuhn was also a member of the Signet Society, a very selective and prestigious "intellectual discussion society" at Harvard (see Kuhn 1997/2000, 268). In fact, he was the president of the society in his final year as an undergraduate. Incidentally, James B. Conant, who I will say more about shortly, had also been on the editorial board of *The Harvard Crimson* when he was a student (see Bartlett 1983, 92–93).

bachelor's degree in three years, and he immediately went to work at the Radio Research Laboratory, working for the American war effort (see Kuhn 1997/2000, 268–269). This is hardly the typical path even at Harvard.

When Kuhn returned from his service in the military, he began work on a dissertation in physics with John Van Vleck, who was then the Chair of the Physics Department at Harvard (see Bleaney 1982, 638). Kuhn was already familiar with Van Vleck, as he was the "head . . . of the theory group" at the Radio Research Laboratory during the war (see Bleaney 1982, 628; 637; see also Kuhn 1997/2000, 268–269). Together with Van Vleck, Kuhn published "A Simplified Method of Computing the Cohesive Energies of Monovalent Metals," one of Kuhn's first publications. Van Vleck, in turn, was no ordinary scientist. He served as President of the American Physical Society in 1952–1953, and Vice President of the American Academy of Arts and Sciences in 1956–1957. And he would later go on to win the Nobel Prize for physics in 1977 (Bleaney 1982, 628).[4] Indeed, Van Vleck was not the only scientist of Nobel Prize caliber Kuhn encountered at Harvard. He also took courses in graduate school with Percy Bridgman and Julian Schwinger (see Kuhn 1997/2000, 267–268 and 274–275).[5]

In addition to working with a supervisor who was exceptional in his accomplishments, Kuhn also had peers who were exceptional. One of Kuhn's age peers was Philip W. Anderson. Anderson started his undergraduate education at Harvard in 1940, the same year Kuhn did, worked at the Radio Research Laboratory immediately after finishing his bachelor's degree, as Kuhn did, and returned to Harvard after the war to complete a Ph.D. under Van Vleck's direction, as Kuhn did (see Nobel 1977). Anderson won the Nobel Prize in Physics in 1977, along with Van Vleck

[4] It is interesting to note that Harriet Zuckerman discusses Van Vleck as an example of someone who occupies the forty-first chair (see Zuckerman 1977/1996, 158). The term "the 41st Chair" was coined by Robert K. Merton to describe the phenomenon that "a good number of scientists who have not received the [Nobel] prize and will not receive it have contributed as much to the advancement of science as some of the recipients, or more" (see Merton 1968/1973, 440). As Merton explains, "the phenomenon of the forty-first chair is an artefact of having a fixed number of places available at the summit of recognition" (Merton 1968/1973, 441). The term is derived from the situation created by the French Academy, where "only a cohort of forty could qualify as members" (see Merton 1968/1973, 441). Though Van Vleck won the Nobel Prize in Physics in 1977, Zuckerman's book was published that year, and it was obviously *written* without the knowledge that Van Vleck would win the prize in 1977. Zuckerman notes that Van Vleck, like other then-living occupants of the 41st chair, "[exhibits] the same pattern of early achievement, early recognition, and early institutional reward" (see Zuckerman 1977/1996, 158). For example, she notes that Van Vleck was awarded his doctorate when he was twenty-three years old and was "promoted to top academic rank while still in [his] twenties" (see Zuckerman 1977/1996, 158).

[5] Van Vleck had also taken courses with Bridgman (see Bleaney 1982, 632).

and Nevill Mott.[6] Kuhn was moving in elite circles, among people who were marked for success.

Kuhn worked with other exceptionally accomplished people during this time, most significantly, James B. Conant, the President of Harvard. The details of this relationship are outlined in Chapter 2, but I will highlight some of the key points here. When Kuhn was completing his Ph.D. in physics, Conant invited him to work as an assistant for him in the teaching of a General Education course in the history of science. This was part of an initiative of Conant's to ensure that the American elite were science-literate. It was a course designed for non-science majors. Conant's influence in America at this time was wide-ranging and profound. He had been an advisor on the Manhattan Project and played a significant role in the creation of the National Science Foundation. In fact, Conant was a widely known public figure, as he had appeared on the cover of *Time* magazine.[7] This involvement with Conant proved to be most significant, given the direction Kuhn's career subsequently took. In particular, Conant supported Kuhn's application to the Harvard Society of Fellows, which afforded Kuhn the opportunity to retrain as a historian of science upon completing his Ph.D. in physics (see Kuhn 1997/2000, 276; 278–279). Moreover, when Kuhn was writing *Structure*, he would draw extensively on material from the history of science course, specifically the cases from the history of chemistry that figured in the course. So Kuhn was, without a doubt, moving among some of the most powerful and accomplished researchers of his time.[8]

Although Kuhn was, like his peers, marked for success, his career was not without setbacks. He was denied tenure at Harvard in the mid-1950s, a significant psychological and professional blow. And he was denied a promotion to full professor in the Philosophy Department at UC Berkeley in the early 1960s, just before *Structure* was published.

Nonetheless, even through these challenging experiences, Kuhn's social capital as an academic continued to grow. The year *Structure* was published, he oversaw a project in Denmark sponsored by the American

[6] In a comprehensive study of American Nobel laureates, Harriet Zuckerman found that "more than half (forty-eight) of the ninety-two laureates who did their prize-winning research in the United States by 1972 had worked either as students, postdoctorates, or junior collaborators under older Nobel laureates" (see Zuckerman 1977/1996, 100).

[7] Conant would appear a total of four times on the cover of *Time* magazine, "'a rare record' for someone who had never held elected office" (Conant 2017, 478). The dates were: February 5, 1934, September 28, 1936, September 23, 1946 and finally September 14, 1959.

[8] This theme is stressed in Robert K. Merton's analysis of Kuhn's career in Merton's *The Sociology of Science: An Episodic Memoir* (see Merton 1977).

Physical Society, a project which involved constructing an archive of material related to the quantum revolution in physics in the early twentieth century. The goal of the project was to interview as many of the participants in the revolution as possible, before they died. This project put Kuhn in contact with many of the greatest physicists of the twentieth century. In fact, Kuhn interviewed Niels Bohr on a number of occasions, including just one week before his death.[9] Clearly, the fact that he was chosen to oversee such a project indicates that he was highly respected by physicists and historians of science, even before the publication of *Structure*.

As mentioned above, his interactions with Conant and involvement in the General Education science courses had a significant impact on Kuhn. The idea of writing a book about scientific revolutions first occurred to Kuhn in 1947, when he was preparing lectures for the course in the history of science. While trying to make sense of Aristotle's physics, he had a transformative experience. He came to realize that Aristotle was involved in a fundamentally different sort of enterprise than Galileo. And, rather than regarding Aristotle's worldview as mistaken, he saw that it provided a fundamentally different account of the world, that is, fundamentally different from either Galileo's or Newton's account. This experience also undermined his previous conviction that the growth of scientific knowledge is cumulative, with no significant setbacks.

But Kuhn's Aristotle epiphany was just the beginning of a very long process. As Kuhn notes, "it was fifteen years between the time these ideas *started* and the time [he] was finally able to write *Structure*" (see Kuhn 1997/2000, 292). Many other important insights that are central to *Structure* still

[9] Stanley Cavell provides another, more personal, sense of how remarkable Kuhn was:

> Kuhn was the product of two distinguished German Jewish families, accustomed to the best of everything in growing up and to being recognized for his intellectual accomplishments. When I told my uneducated father from Eastern Europe that not alone Kuhn but Kuhn's father had gone to Harvard, my father treated the news as something quite beyond comprehension. He repeated the words, as if searching for a history that could make them true. (Cavell 2010, 356)

Cavell and Kuhn were colleagues at UC Berkeley and continued to be friends long after both had moved back to the east coast, in the early 1960s. Kuhn had a great appreciation for Cavell. He notes that "the person [at Berkeley] who was *extraordinarily* important was Stanley Cavell. My interactions with him taught me a lot, encouraged me a lot, gave me certain ways of thinking about my problems, that were of a lot of importance" (see Kuhn 1997/2000, 297; emphasis in original). And in the Preface to *Structure*, in which Kuhn acknowledges his intellectual debts, he describes Cavell as "the only person with whom I have ever been able to explore my ideas in incomplete sentences. That mode of communication attests an understanding that has enabled him to point me the way through and around several major barriers encountered while preparing my first manuscript" (Kuhn 1962/2012, xlv–xlvi).

eluded Kuhn. Kuhn claims that his Lowell lectures, "The Quest for Physical Theory," given in 1951, were his first attempt to write *Structure* (see Kuhn 1997/2000, 289; Kuhn 1977, xvi). But, in the course of giving the lectures, he realized that he was not yet ready to write the book.[10] In fact, it was not until the mid-1950s that he began to really appreciate the role of normal science, the "periods governed by one or another traditional mode of practice . . . [that] necessarily [intervene] between revolutions" (see Kuhn 1977, xvii). Without the concept of normal science, he was in no position to write the book.

Throughout his career, Kuhn would underestimate the amount of time and work it would take to complete projects. Robert K. Merton notes that

> by the age of thirty-two, when [Kuhn] made his application [for a Guggenheim Fellowship], he had published few articles: principally, one with Van Vleck in physics . . . and the other, a historical piece on Boyle and structural chemistry in the seventeenth century which appeared in *Isis*. (Merton 1977, 91–92)

Kuhn's inability to estimate how long things would take, and the high standards that he held himself to, would repeatedly delay him in reaching his goals. As I have noted elsewhere, his lecture notes are marked up with critical remarks about how the lecture went, and what he would not do again next time (see Wray 2018b). In fact, when Kuhn died, he left an unfinished manuscript that he had been alluding to for decades.[11]

But the delays affecting the publication of *Structure* were not wholly detrimental. In fact, they allowed Kuhn to develop his ideas, and ensured that he did not publish the book prematurely. Some of the most influential concepts that figure in *Structure*, normal science and paradigm, for example, did not even enter Kuhn's mind until ten or so years after he first thought of writing a book about scientific revolutions. Kuhn had the good sense to wait until he had worked out his ideas. No doubt this is part of what explains the success of the book.

Structure was finally published in 1962. It was initially published as a volume in the *Encyclopedia of Unified Science*, a series that originated with the Vienna Circle positivists. As he was making the final revisions to

[10] In giving the lectures, Kuhn quickly recognized that he had not yet worked out his view adequately. As he explains, "the primary result of that venture was to convince me that I did not yet know either enough history or enough about my ideas to proceed toward publication" (Kuhn 1977a, xvi).

[11] Kuhn's final manuscript has a bit of a "pharaoh's curse" associated with it. Susan Abrams, the editor at University of Chicago Press with whom Kuhn was working before he died, died in 2003, and John Haugeland died in 2010. After Kuhn's own death, Haugeland was going to coedit the volume with James Conant, James B. Conant's grandson. It is still unpublished.

his manuscript, Kuhn expressed some concern to the publisher that his book would not get the attention it deserved, given the declining popularity of the *Encyclopedia* and its fading influence (see Kuhn 1997/2000, 300). In hindsight, we can say that the series was in its final days in the early 1960s. The particular volume that Kuhn was commissioned to write was intended to be devoted to the history of science. Its path to production was somewhat precarious. It seems that others had been invited to write the volume and had declined before Kuhn was invited to do so (see Kuhn 1997/2000, 291–292). Specifically, Kuhn notes that I. B. Cohen and Aldo Mieli had been asked to write the volume. When Cohen declined, he suggested Kuhn to the editors (see Kuhn 1997/2000, 292).

Kuhn's worries about publishing the book as a volume in the *Encyclopedia* were wholly unfounded. *Structure* was quickly regarded as a book well worth reading, not only by historians and philosophers of science, but also by many other academics and educated laypeople. Among Kuhn's collected papers, lectures and manuscripts in the archives at the Massachusetts Institute of Technology are countless letters. Many are from influential economists, psychologists and other academics. There is even a set of letters discussing a possible meeting between Kuhn and Newt Gingrich.[12] Many people were very excited by Kuhn's book.

In the 1970s, Merton noted that "in the first dozen years since its publication, [*Structure*] has given rise to *a library* of appreciative applications and diversely critical commentary" (see Merton 1977, 106; emphasis added). And since the 1970s this library has continued to expand. In fact, Kuhn was not only inundated with letters expressing positive responses to the book, but also manuscripts inspired by it. In 1973, in response to one fan who sent along a manuscript, Kuhn remarked that

> for better than five years I have been receiving two or three unsolicited manuscripts, sometimes book length, every week . . . 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. (Kuhn 1973)

Indeed, the many unsolicited manuscripts Kuhn received would not be the most significant problem that the book created for him. The most significant challenges he faced were the criticisms, many of them based on misunderstandings of *Structure*.

[12] See Thomas S. Kuhn Archives. MC240. Box 5: Folder 29, Congressional Clearinghouse.

My aim in this book is twofold. First, I aim to reconstruct the writing of *Structure*, clarifying the intellectual influences on Kuhn as he wrote the book. The existing studies of Kuhn have tended to focus on the influence of Kuhn's social milieu, understood in the broadest terms, with special attention given to the culture of Cold War America. Though these studies are often insightful, they fail to take adequate account of the intellectual influences on Kuhn as he wrote *Structure*. Second, I will trace the impact of *Structure*, with particular attention to its influence on the sociology of science, the history of science and the philosophy of science.

I will also discuss its broader influence, especially in the social sciences. In fact, Kuhn's influence in the social sciences is most interesting. Nowhere is the broad appeal of the concepts he developed more profound than in those fields. Social scientists found both the concepts and the general conception of science Kuhn developed highly fertile. The publication of *Structure* initiated a period of extensive reflection among social scientists on (i) the nature of the social sciences, (ii) their relationship to the natural sciences and (iii) the capacities of the social sciences to produce knowledge. This is quite ironic, as Kuhn claims to have discovered the paradigm concept while working among social scientists at the Center for Advanced Study in the Behavioral Sciences, at Stanford University. Kuhn describes how he realized that what the social sciences lacked, and what characterizes the natural sciences, are paradigms – fundamental research achievements that play an essential role in creating a consensus in a field and that make the sort of progress that we associate with the natural sciences possible. Kuhn also ignited a revolution of sorts in the sociology of science, one with which he was never fully comfortable.

I will also examine Kuhn's difficult relationship with the history of science. Though he spent many years working in history departments or involved in history of science programs, the impact he had on the history of science was comparatively insignificant: that is, when compared to the impact he had on the sociology of science and the philosophy of science. And recent assessments of his work by historians are not particularly flattering. Finally, I will also examine the impact Kuhn has had on the philosophy of science. On the one hand, Kuhn's legacy has been a set of problems that are a consequence of a particular reading of *Structure*, one that settled into place by around 1970. This has left us with a Kuhnian position of sorts that is widely deemed to be deeply problematic, as it threatens the integrity of science and scientific knowledge. On the other hand, his notion of revolutionary theory change left a lasting impact on debates and developments in contemporary philosophy of science, most

notably in the realism/anti-realism debates. A central problem in these debates is what I call the problem of theory change, a problem that takes its form from Kuhn's *Structure*.

I will begin with a discussion of the Aristotle experience, the experience that set Kuhn on a new path away from a career in physics and toward a career in the history of science.

I have included information from Kuhn's curriculum vitae, prepared for an NSF application from the late 1980s, as a useful guide through the course of Kuhn's career.

Vita of Thomas S. Kuhn

(Source: TSK Archives, Box 20: Folder 12, NSF Research Reports)

Education

S.B. (summa cum laude), Physics, Harvard University, 1943
A.M., 1946
Ph.D., 1949

Positions Held

With radio research laboratory. Am-British Lab., OSRD, 1943–1945
Junior Fellow, Harvard Society of Fellows, 1948–51
Harvard University, 1951–56
  Assistant Professor, General Education and History of Science, 1952–56
University of California, Berkeley, 1956–1964
  Professor, History of Science, 1961–64
Princeton University, 1964–79
  M. Taylor Pine Professor of the History of Science, 1968–1979
Member, Institute for Advanced Studies, 1972–79
Fellow, New York Institute for the Humanities, 1978–79
Massachusetts Institute of Technology, 1979–
  Professor, Philosophy and History of Science, 1979–83
  Laurance S. Rockefeller Professor of Philosophy, 1983–

Honors and Fellowships

Guggenheim Fellow, 1954–55
Fellow, Center for Advanced Study in the Behavioral Sciences, 1958–59

PART I

The Groundwork for Structure*: Harvard 1947 to 1955*

I want to begin by exploring the origins of *Structure*, with special attention to the years Kuhn spent at Harvard after he returned from his time in the military. In Chapter 1, I examine Kuhn's Aristotle experience; that is, the experience he had when he read Aristotle's writings in physics, as he prepared some lectures as part of his contribution to the General Education science course he was working on with President James B. Conant. This is where it all began. That is, this is where Kuhn first decided that he would write a book on scientific revolutions. In Chapter 2, I provide a systematic analysis of James B. Conant's influence on Kuhn. It lays to rest a number of widely circulating claims about the inspiration for Kuhn's ideas. In Chapter 3, I provide an analysis of the influence of the history of chemistry on Kuhn's thinking. This chapter makes clear that Kuhn was concerned narrowly with the natural sciences, a point he insisted on repeatedly, despite the very broad appeal of the book. But it also draws attention to the fact that the histories of physics and astronomy were less significant in shaping Kuhn's views. Finally, in Chapter 4, I analyze the influence of the Logical Positivists on Kuhn's thinking during this period. Despite their significant influence in philosophy of science in America during the 1940s and 1950s, Kuhn does not engage directly with them in any significant way in *Structure*, and some have suggested that he had an outdated understanding of their views. I show that Kuhn engaged with the Logical Positivists more explicitly in the Lowell Lectures, his first attempt to write *Structure*. I also argue that insofar as he had a distorted picture of Logical Positivism, it was a consequence of W. V. Quine's influence.

CHAPTER 1

What Did Aristotle Teach Kuhn?

Thomas Kuhn referred to his now-famous Aristotle experience on a number of occasions (see Kuhn 1977, xi–xii; 1987/2000, 15–20; Kuhn 1997/2000). And it is now commonplace for commentators of Kuhn's philosophy of science and history of science to discuss this incident, even if only in passing (see, for example, Bird 2000, 27; Fuller 2000, ch. 4, § 4; Andersen 2001, 2; Grandy 2003, 248; Nickles 2003, 144; Zammito 2004, 64; Hoyningen-Huene 2015, 194; Marcum 2015, 9–10; Kaiser 2016, 77; Reisch 2016, 13–17 and 24–26; Sankey 2018a, 82–83; Reisch 2019, 65–66 and 153–154; Burman 2020, 133–134, fn. 1). Indeed, so profound was the experience alleged to have been that it is not uncommon for it to be referred to as his Aristotle epiphany (see, for example, Reisch 2016, 16; and Heilbron 1998, 507).

My aim in this chapter is to examine the impact that this experience had on Kuhn's thinking, especially as he was writing *The Structure of Scientific Revolutions*. In many respects, this experience counts as one of the most profound influences on Kuhn as he wrote *Structure*. It rivals both (i) his experience working with James B. Conant on the General Education science courses at Harvard, and (ii) the year he spent at the Center for Advanced Study in the Behavioral Sciences, where he discovered the importance of paradigms for *natural* scientists, and their absence in the social sciences (see Kuhn 1962/2012, xlii). As we will see, the Aristotle experience was the source of Kuhn's initial discovery of scientific revolutions, that is, those disruptive changes in science that undermine the strictly cumulative account of scientific progress that he reacted against in *Structure* (see Kuhn 1977, xiii). That experience thus marks the beginning of his long journey toward writing *The Structure of Scientific Revolutions*.

I will also identify key parts of Kuhn's project that were not yet within his grasp in 1947, when he had the Aristotle experience. I thus explain why Kuhn was in no position to complete a book like *Structure* then. Indeed,