

Introduction

The Impact of *The Structure of Scientific Revolutions*

K. Brad Wray

The influence of the *Structure of Scientific Revolutions* (SSR) has been remarkably wide-ranging. Thomas Kuhn, the author of the book, was honored by the History of Science Society, the Philosophy of Science Association, and the Society for the Social Studies of Science (see Buchwald and Smith 1997, 361), three very different academic societies.

Given SSR's wide-ranging influence, it is useful to review the impact of SSR, and the changing perceptions of its significance, one discipline at a time. Necessarily this survey is very selective. Nothing approaching a comprehensive literature review is possible for a book that has been cited more than 135,000 times. My focus here will be on book reviews of SSR, some written soon after the book was first published, and others written as much as fifty years after its publication, in response to the publication of the fourth edition of the book. I will also discuss articles that reflect on the impact of the book and eulogies or appreciations of Kuhn marking his death in 1996.¹

I.1 History of Science

Let us first consider the reception of SSR among historians of science, as Kuhn's professional identity was initially as a historian of science. I will rely heavily on the pages of *Isis*, the journal of the History of Science Society, to provide a window into how historians have responded to the book over the decades, though I will discuss a few reviews from other sources as well.

Mary Hesse wrote a very positive review of the book when it was first published (see Hesse 1963). Her first sentence says it all: "this is an important book" (p. 286). Her second sentence, though, hardly captures the spirit of today. It reads as follows: "it is the kind of book one closes with the feeling that once it has been said, all that has been said is *obvious*,

¹ *Structure* has been published in four editions, in the following years: 1962 (SSR-1), 1970 (SSR-2), 1996 (SSR-3), and 2012 (SSR-4).

because the author has assembled from various quarters *truisms* which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed” (ibid., emphases added). On the one hand, Hesse is correct to say that the book transformed our whole image of science. But many readers today would object to her claim that Kuhn has assembled various *truisms* and that what he says is *obvious*.

Hesse rightly recognized that Kuhn sought to replace the philosophical view of science associated with the positivists “with a view of science as a historical succession of *paradigms*” (ibid.).² Further, she also grasps his “method of argument”: to examine historical examples of scientific revolutions (ibid.). She praises Kuhn for his “deft explication of those tricky conceptual tools of the historians trade: ‘discovery,’ ‘priority,’ ‘anticipation,’ and many others” (p. 287).

Despite her praise of the book, she did not think it was a foregone conclusion that the book would be well received by historians of science. She ends the review noting that “the major question for historians of science ... is whether history bears the interpretation here put upon it” (ibid.). In a reflective turn, she claims that “the answer, as in the case of any paradigm shift, will be partly dependent on impressionistic and non-logical factors and will be subject to the kinds of resistance Kuhn finds to paradigm-change within the sciences” (ibid.). Hesse, though, claims that her “own impression is that Kuhn’s thesis is amply illustrated by recent historiography of science and will find easier acceptance among historians than among philosophers” (ibid.).³

Charles Gillispie reviewed SSR for the journal *Science*. He begins his review noting that “this is a very bold venture” (Gillispie 1962, 1251). Rightly, Gillispie recognizes that Kuhn “is not writing history of science proper. His essay is an argument about the nature of science, drawn in large part from its history but also, in certain essential elements, from considerations of psychology, sociology, philosophy, and physics” (ibid.). From Gillispie’s point of view, “Kuhn’s critique of the very

² Hesse ends the review suggesting that “Kuhn has at least outlined a new epistemological paradigm which promises to resolve some of the crises currently troubling empiricist philosophy of science” (Hesse 1963, 287).

³ The distinguished historian of science Marie Boas Hall reviewed the book for *The American Historical Review*. Though she describes it as a “closely reasoned monograph,” one hardly gets the impression from her review that she thought the book would have much of an impact. She suggests that Kuhn was probably influenced by Crane Brinton’s *Anatomy of Revolution* and by George Sarton (see Boas Hall 1963). Given the research I have conducted on the influences on Kuhn’s intellectual development, I doubt that either of these had much influence on Kuhn, especially not Sarton (see RSS 275 and 281–282; also Wray 2021a, especially chapter 2).

notion of scientific discovery may ... be the strongest part of his argument, and is certainly at the heart of it" (ibid.). Gillispie sees it as part of an attack on a wrong-headed theory of discovery, according to which "inventions of theory ... were found like hidden treasure or a misplaced hat ... wanting mainly to be revealed" (ibid.).

Though Gillispie expresses some minor concerns, he ends his review noting that "there can be only admiration for the erudition, the scholarship, the fidelity, and the seriousness that the enterprise reflects on every page" (p. 1253). Further, he remarks that "every historian ... will surely applaud one recurrent and fundamental emphasis, which is that the development of science must be set into the context of a Darwinian historiography and treated as a circumstantial evolution from primitive beginnings rather than the ever closer approach to the telos of a right and perfect science" (ibid.). Oddly, this dimension of Kuhn's view was not discussed much at all, at least not until the last decade or so (see Renzi 2009; Reydon and Hoyningen-Huene 2010; and Wray 2011b).

It is worth contrasting these early reviews of the first edition of SSR with Joel Isaac's review of the fourth edition, published in 2012 to mark the fiftieth anniversary of the book. Isaac focuses mainly on the features that distinguish the fourth edition from earlier editions, specifically:

- (i) the fact that it has been newly typeset, which has the consequence of shifting some passages from one page in the first, second, and third edition, to a different page in the fourth edition;
- (ii) the new and expanded index; and
- (iii) the Introductory essay by Ian Hacking, a long-time sympathetic reader of Kuhn's SSR (see Isaac 2013).

The latter two features, Isaac notes, are "much more unambiguously goods" than the first (p. 658).

In praising the new index, Isaac notes that the older index, prepared for the third edition, was merely two pages long, and erroneously listed an entry for "Clarant" intended to guide readers to a brief mention of Alexis Claude Clairaut (ibid.). Oddly, Isaac does not mention that the index in the new edition includes an entry for "Foucault, Michel," which is intended to guide readers to the Foucault of "Foucault's pendulum," that is, Jean Bernard Léon Foucault (see SSR-4 211).

Isaac ends his review noting that "of course I shall make no attempt to review the book itself. I can do no better than repeat Hacking's opening comments: 'Great books are rare. This is one. Read it and you will see' (p. vii)" (p. 659). Despite the high praise, Isaac does note a few shortcomings of Kuhn's analysis of science. Specifically, he notes that "Kuhn's conception of science was indelibly shaped by his own training

as a physicist and by the ascendancy of physics among the sciences during the early years of the Cold War” (ibid.).⁴ He also notes that “Kuhn’s claims about theory change and experimentation do not obviously have purchase on the contemporary scientific world of biotechnology, information science, and computer simulation” (ibid.).

Peter Dear also provides some reflections on the fiftieth anniversary of the publication of SSR. He claims that “any historian of science who sits down to reread SSR will be struck by its almost archaic historiographical sensibilities” (Dear 2012, 426). In light of this assessment, it is not surprising that Dear claims that Kuhn “was never really a guide to historical research except by association” (p. 425).

Between these two dates, that is, 1962 and 2012, specifically, in 1982, the History of Science Society honored Kuhn with the Sarton Medal. The president of the Society, Frederic L. Holmes, provides some reflections on SSR and its impact on the history of science, though the prize was not awarded specifically for SSR but rather for Kuhn’s contributions to the profession as a whole.⁵

Holmes notes the wide appeal of the book. “Ever since [its publication in 1962], that book has remained the focal point for passionate debate among historians, scientists, social scientists, and even those in the arts and in political movements to whom the author had not imagined his ideas were relevant” (Holmes 1983, 247, in Hannaway et al. 1983). Writing in the early 1980s, Holmes reports that “the influence of the book shows no signs of having run its course” (ibid.).

Already, though, only twenty years after its publication, historians were more or less finished with the book. In fact, as Holmes notes, “historians of science have, curiously, been on the whole the most reluctant to enter into the discussions evoked by *The Structure of Scientific Revolutions*” (ibid.). Elaborating, Holmes explains that “some have admired it, others have tried to ignore it, still others have asserted that what Kuhn had to say was merely a codification of the common practices of historians of science” (ibid.). Holmes, though, insists that “whatever the personal reactions of individual members of our field may have been ... the history of science has not been the same since 1962” (ibid.).

Holmes then suggests that “all of us, whether we wished to or not, have had to locate ourselves with reference to Kuhn’s framework. Whenever we have described particular historical events, we have had to ask

⁴ A number of studies have explored the influence of the Cold War on Kuhn as he wrote SSR (see, especially, Fuller 2000b and Reisch 2019).

⁵ As John Heilbron explains, the Sarton Medal is “the Society’s highest award” (Heilbron 1998, 514).

ourselves whether they fell within some phase of his cycle of pre-paradigm, paradigm, normal, crisis, or revolutionary science” (ibid.). Whether this is how historians of science *felt* in the early 1980s, I cannot say, but one sees little evidence that between 1962 and 1982 historians of science located their own work with reference to Kuhn’s framework.

Incidentally, it is worth noting that in 1985 Paul Josephson reported on the influence of Kuhn’s SSR on Soviet historians of science (Josephson 1985). SSR was “translated into Russian in 1975” and “has been the subject of many articles in Soviet journals” (p. 76). Josephson attributes some of the appeal of the book to Soviet historians of science to the fact that “Kuhn’s postulated sequence of ‘normal science – anomalies – crisis/revolution – normal science’ ... fits the dialectical explanation of revolutions” (p. 551).

It is fitting to end this quick tour of the responses of historians of science to SSR by looking at remarks in Kuhn’s eulogy in *Isis*. Kuhn’s former student, John Heilbron, wrote the memorial notice. I will limit my analysis to Heilbron’s remarks on SSR. Heilbron describes SSR as an “enduring book” (Heilbron 1998, 505). As Heilbron explains, “it made ‘paradigm shift’ as common and misused a metaphor as ‘quantum leap’ and ‘critical mass.’ It achieved what few philosophical books have done. It simultaneously instructed a wide academic public and a specialist community” (ibid.). I think it is telling that Heilbron, who knew Kuhn well, and worked closely with him, describes the book as a philosophical book.

Heilbron summarizes the book’s effects:

the book comforted social scientists who wanted to assimilate their discipline to physics, Luddites who blamed social problems on scientists and engineers, and everyone who rejected authority. It repelled the philosophers of science at which it was aimed for the good reason that it undercut their belief that scientific knowledge advances by application of rational criteria to the products of observation and experiment. (ibid.)

Indeed, Heilbron captures well the wide range of people to whom the book spoke.

I.2 Philosophy of Science

The reactions of philosophers of science were somewhat hostile right from the beginning. Dudley Shapere’s review of SSR, published in *The Philosophical Review*, set the stage for the book’s reception among philosophers of science. Shapere begins the review noting that “this

important book is a sustained attack on the prevailing image of scientific change as a linear process of ever-increasing knowledge, and an attempt to make us see that process of change in a different and ... more enlightening way" (Shapere 1964, 383). Shapere rightly anticipates the book's place in the history of philosophy of science. He notes that Kuhn's "view, while original and richly suggestive, has much in common with some recent antipositivistic reactions among philosophers of science – most notably, Feyerabend, Hanson, and Toulmin" (ibid.). Thus, already, only two years after its publication, Kuhn's book is characterized as a typical contribution to what we now often refer to as "the historical school in philosophy of science."⁶

The more lasting impact of Shapere's review is his critique of Kuhn's use of the term "paradigm." In Shapere's assessment,

[Kuhn's] view is made to appear convincing only by inflating the definition of 'paradigm' until that term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of some central aspects of science. (p. 393)

As I have discussed in detail elsewhere, Kuhn would spend the next ten years working out what he meant by the term "paradigm" (see Wray 2011b, chapter 3). Ultimately, Kuhn restricted its application to the exemplars that scientists appeal to in their research to solve research problems in the normal course of conducting research.

Knowing that Kuhn intended to write an expanded version of the book, Shapere ends the review suggesting that "the difficulties that have been discussed here indicate clearly that the expanded version of this book which Kuhn contemplates will require not so much further historical evidence (p. xi) as ... more careful scrutiny of his tools of analysis" (Shapere 1964, 394).

In light of the critical nature of Shapere's assessment, it is quite surprising that the book went on to have the impact it had.

Indeed, one would have been left with the same impression if one had read Harry Stopes-Roe's review of SSR in the *British Journal for the Philosophy of Science*. Stopes-Roe remarks that "one's first impression is of enthusiasm and vitality. The author clearly feels himself to be opening up a new world of appreciation and understanding" (Stopes-Roe 1964, 158). Stopes-Roe continues:

⁶ On the historical school, see Kuhn's presidential address to the PSA, "The Road since *Structure*" (see RSS 91).

In the face of such force and charm, it seems mean to question the lasting value of the work; but it must be said that many of its features are already well established ...; and the author's enthusiasm leads him to over-state his novelties in a way that prejudices the appreciation of those things of value he has to say. (ibid.)

Stopes-Roe also registered a complaint about "the ubiquitous use of the odd word 'paradigm'" (p. 159). He even goes so far as to "suggest ... that if the reader wishes to bring out the real content of what Kuhn is saying, he may find it advantageous to try substituting 'basic theory' for every occurrence of 'paradigm' in the book" (Stopes-Roe 1964, 159).

This suggestion betrays the fact that Stopes-Roe has missed the importance of the paradigm concept for Kuhn's analysis. In many instances where Kuhn uses the term he is drawing attention to the reasoning by analogy that scientists engage in when solving research problems. Kepler's mathematical model of the orbit of Mars provided a template for modeling the orbits of other planets, the orbit of the Moon, and the orbits of other satellites, and even, ultimately, the paths of comets. Similarly, Planck saw similarities between Boltzmann's modeling of gasses and the black-body problem that led him to develop a hitherto unimagined solution to the latter. This is an aspect of paradigms that Margaret Masterman saw and appreciated, though scholars tend to cite Masterman's remarks about the many different ways that Kuhn used the term "paradigm" without acknowledging that she was extremely supportive of Kuhn's project (see Masterman 1970).

Alexander Bird wrote an essay review of the fiftieth anniversary edition of SSR. Bird, though, does not approach the task as Isaac did in his review in *Isis*, assessing the new features of the fourth edition. Instead, Bird consciously limits himself to assessing the content and impact of the first edition (see Bird 2012b, 860 n. 1).

Unlike the early reviews by philosophers of science, discussed above, Bird claims that "Kuhn's *The Structure of Scientific Revolutions* ([1962]) is in many ways an unusual and remarkable book" (p. 859). With the benefit of hindsight, Bird rightly notes, "it has a strong claim to be the most significant book in the philosophy of science in the twentieth century" (ibid.). Bird further describes it as "an original, wide-ranging, interdisciplinary, and bold book" (p. 878).

Perhaps one of the most noteworthy parts of Bird's review is his final remark on exemplars. According to Bird, "the exemplar idea is ripe for renewed investigation and development with the tools of current psychology and cognitive science, in a climate that is more receptive than that which Kuhn himself faced" (Bird 2012b, 880). So the concept that caused Kuhn so much grief in the initial years after its publication turns out to be the concept that seems most relevant to contemporary philosophy of science.

Upon Kuhn's death, Jed Buchwald and George Smith wrote a memorial notice for *Philosophy of Science*. Though it provides a useful overview of Kuhn's whole career, in both philosophy of science and history of science, I will focus narrowly on their remarks on SSR. They note that "in remarkably few words, Kuhn advanced the argument that the development of science cannot be understood simply as a process in which more accurate conceptions gradually replace less accurate ones under the impetus of experiment" (Buchwald and Smith 1997, 365).

Buchwald and Smith rightly note that "Kuhn's claims provoked strong resistance, particularly within the philosophic community" (p. 368). Elaborating, they note that "many felt, and continue to feel, that SSR did not fit well with claims to rationality and objectivity ... for scientific knowledge" (ibid.). And "others saw SSR as advancing theses about science that seemed to be paradoxical" (ibid.). Kuhn's remarks about "world changes" were often singled out as especially problematic.

Both Buchwald and Smith knew Kuhn, and knew him for a long time. Consequently, they felt many of his critics were uncharitable. In their words, "the picture of how science develops that Kuhn had formed came not out of philosophical reasoning, but from personal encounters with episodes in the history of science. The seemingly conflicting positions that his critics accused him of trying to maintain were merely artifacts of the way he communicated this picture" (ibid.).

Interestingly, and importantly, Buchwald and Smith also rightly note that "opposition to SSR did not prevent its impact on the philosophy of science. The 'problem of conceptual change' – i.e., the problem of incorporating something akin to Kuhn's conceptual readjustments into an account of the cumulative growth of scientific knowledge – took center stage in the wake of SSR" (ibid.). Consequently, Buchwald and Smith note, "philosophers of science began to look more closely and in much greater detail at the historical development of science, and they became increasingly attentive to the complexities of scientific practice" (ibid.).

Indeed, in highlighting normal science, Kuhn gave birth to the philosophy of science in practice, a development and movement in philosophy of science that generally eschews the more traditional focus on the logic of science, the traditional focus of the logical positivists and their heirs. Rather, those working in this new tradition are more inclined to examine laboratory practices, developments in techniques and instruments, and their impact on the advance of scientific knowledge than the logical relations between data and theory.

And as I have argued in detail elsewhere, the focus on the problem of conceptual change has had a profound impact on the realism/antirealism

debates since the mid-1970s (see Wray 2021a, chapter 10). No longer are these debates concerned with understanding the meaning of theoretical propositions or whether our theoretical vocabulary in science is reducible to and expressible in observation terms. Rather, central to the contemporary debates is a concern for understanding how, or if, we can reconcile radical theory change with a central tenet of scientific realism, that our theoretical knowledge is increasing with the development of science. If successive theories are incommensurable, as Kuhn suggests, it is challenging to understand how to ground the traditional realist assumption of convergence on the truth.

Buchwald and Smith provide a useful analysis of Kuhn's later work, especially the work that was meant to clarify and develop the general theory of science presented in SSR. On their reading, incommensurability figured importantly (see Buchwald and Smith 1997, 375).

David Hull wrote a brief commentary for the journal *Nature*, reflecting on Kuhn's career after his death. Hull notes that "professional philosophers of science were put off by Kuhn's views, especially his principle of incommensurability" (Hull 1996, 204). As Hull explains, "Kuhn was deeply frustrated by the philosophical responses to his views – so much so that he claimed that, among all the readers of his work, philosophers were uniquely unable to understand him" (ibid.).

Hull also claims that philosophers of science failed to appreciate Kuhn's philosophy of science. Indicative of this "is the fact that many younger, less influential philosophers ... were elected president of the Philosophy of Science Association before Kuhn was elected in 1988" (ibid.). Further, Hull suspects that Kuhn will have a more lasting impact. In Hull's words, "I suspect that a hundred years from now, Kuhn will be one of the few philosophers of science who will be looked back upon as having radically changed our understanding of science" (ibid.).

Also following Kuhn's death, Richard Rorty wrote a short reflective piece on Kuhn's impact. What is particularly interesting about Rorty's perspective is that he draws attention to the wide-ranging significance of SSR. Unlike many philosophers, Rorty welcomed the appropriation of Kuhn's work throughout the academic world. Rorty begins by noting that "the death ... of Thomas S. Kuhn, the most influential philosopher to write in English since the Second World War, produced many long, respectful obituaries. Most of these obituaries referred to him as a historian of science rather than as a philosopher" (Rorty 1997/1999, 175). Rorty then remarks that

if I had written an obituary, I should have made a point of calling Kuhn a great philosopher, for two reasons. First, I think that 'philosopher' is the most

appropriate description for someone who remaps culture – who suggests a new and promising way to think about the relation among various large areas of human activity.... My second reason for calling Kuhn a great philosopher is resentment over the fact that Kuhn was constantly being treated, by my fellow professors of philosophy, as at best a second-rate citizen of the philosophical community. (ibid.)

Rorty's second point is interesting because he too was an outsider of sorts in mainstream American philosophy.

The first point is more substantive. And Rorty makes it clear exactly what Kuhn did in writing SSR. According to Rorty, "Kuhn's major contribution to remapping culture was to help us see that the natural scientists do not have a special access to reality or to truth. He helped dismantle the traditional hierarchy of the disciplines" (p. 176).

On a more personal note, Rorty explains, "Kuhn was one of my idols, because reading his *The Structure of Scientific Revolutions* (1962) had given me the sense of scales falling from my eyes" (p. 175). Many readers, no doubt, have had a similar experience with the book.

Despite the fact that philosophers of science were so displeased with the image of science represented in the book, it has become, without a doubt, a canonical text in the philosophy of science, and the history of the philosophy of science.

I.3 The Sociology of Science

Bernard Barber provides valuable insight into how the book was received by sociologists of science. Reading his review, though, one could easily get the impression that Barber had read an entirely different book from the book that Shapere and Stopes-Roe read and reviewed. Barber claims that "Kuhn's book is offered as an essay in the sociology of scientific discovery" (Barber 1963, 298). Barber notes that "Kuhn's subtle, rigorous analysis of the social process of scientific discovery is ... different from that presented in the reports of 'normal science' ... and especially in the textbooks of the reigning 'normal science'" (ibid.).

Whereas Stopes-Roe questioned the lasting value of SSR, Barber expresses unrestrained enthusiasm. In fact, Barber makes two prescient observations. First, noting that "Kuhn has limited himself to examples chiefly from the physical sciences," Barber suggests that the book "has obvious and important relevance to the social sciences" (ibid.). In fact, as I have argued elsewhere, social scientists have found the book a rich source for reflecting on their own fields (see Wray 2021a, chapter 5). Across the disciplines – economics, political science, sociology, and anthropology – social scientists reflected on (1) whether their own fields

had paradigms, (2) whether their own fields followed the developmental cycle Kuhn described, and (3) whether specific changes in their own fields can be accurately characterized as paradigm changes.

Second, Barber notes that Kuhn's analysis does not "include some sociological factors that would improve his analysis by enlarging it," specifically, a consideration of the effects of "external factors" (Barber 1963, 298). Barber does not fault Kuhn for this. Rather, he suggests that "when we are given so much ... we should not ask for more. We can instead, take it as a challenge to try and give it ourselves" (p. 299). Indeed, this is exactly what sociologists of science did. First, the Strong Programme in the Sociology of Scientific Knowledge emerged, displacing the Mertonians. Their studies began by blurring the boundary between internal and external factors. Then the sociology of science underwent a metamorphosis into science studies. Now, in science studies, it is regarded as quaint or antiquated to speak of external factors.

In 2012, the journal *Social Studies of Science* ran a series on the fiftieth anniversary of SSR, which coincided with the twenty-fifth anniversary of Bruno Latour's *Science in Action*. There is quite extensive disagreement about the value of SSR among sociologists of science. As well, there is significant disagreement among those who value the book about what Kuhn's most important insight was.

On the one hand, Michael Lynch argues that "Kuhn's book effected a revolution in history and philosophy of science, and set the stage for a 'paradigm shift' in the sociology of scientific knowledge during the 1970s which is widely regarded as a crucial turn in the establishment of STS" (Lynch 2012, 450). In this respect, he sees it as foundational to the development of the sociology of scientific knowledge and STS. But Lynch rightly notes that "given the half-century that has passed since its publication, it is not surprising that Kuhn's SSR is not often treated anymore as a source of novel insight into the history and social study of science. SSR is mentioned much more than it is used, but the uses and abuses of Kuhn's ideas in the past half-century have been legion" (p. 452).

Lynch notes that "Kuhn ... famously denounced the Edinburgh School's Strong Programme by linking it to 'deconstruction'" (p. 454). But Lynch astutely observes that "this remark seemed to be designed less as a characterization of the research done by the Science Studies Unit in Edinburgh, and more as an attempt to deter his critics in philosophy and history of science from linking his philosophy to relativism and subjectivism" (ibid.). Lynch also suggests that Kuhn benefited from the "mis-uses" of the ideas in SSR that he objected to (ibid.).

On the other hand, Harry Collins seems far less impressed by SSR. He suggests that many of the ideas in Kuhn's SSR "were largely already

in place,” specifically in the work of Ludwik Fleck, Ludwig Wittgenstein, and Peter Winch (see Collins 2012, 420 and 421). But Collins also suggests that “we would never have had the idea of incommensurability without Kuhn, and it is a vastly under-exploited idea” (p. 422).

Andrew Pickering also provides a more positive assessment of SSR. On his assessment, “Kuhn assembled provocative lines of contemporary thought in history, philosophy and psychology into a coherent, seductive and unforgettable, neo-Hegelian vision of what science is and how it changes” (Pickering 2012, 467). But, for Pickering, the most fertile notion in SSR was “[Kuhn’s] theme of ‘different worlds’” (ibid.). On Pickering’s reading, “different worlds ... can be produced, not by anything linguistic, but by different ‘material grips’ on nature. Grab hold of nature this way, with this material set-up, and you get this world; and another world can ... be produced by a different set-up” (p. 468).

I.4 Social Scientists

Sociologists of science were not the only social scientists to read SSR. In fact, social scientists were some of the most enthusiastic early readers of SSR (see Wray 2021a, chapter 5). Consequently, it is not surprising that they also reviewed the book when it was initially published, and then reflected on its influence when Kuhn died.

The anthropologist, Marshall Sahlins reviewed the book for *Scientific American*. Sahlins raises the same concern about the paradigm concept that Shapere did. In Sahlins’ words,

the term ‘paradigm,’ which carries the burden of [Kuhn’s] argument, is so loosely defined that it covers not only well-articulated theories and vague conceptual schemes but also single scientific laws and specialized instrumental techniques. As a result almost any discovery or innovation can be considered the beginning of a new period of normal science or the occasion for a scientific revolution. (Sahlins 1964, 144)

Sahlins also raises the concern that “it is difficult to see how, in view of Kuhn’s claim that a paradigm determines the way scientists perceive their subject matter and thereby completely controls their interpretations of observed data, paradigm-based expectations can ever be defeated or periods of crisis ever arise” (ibid.). The idea here is that paradigms have such a firm grip on scientists that it becomes inexplicable how any scientist could possibly see outside a paradigm, and thus bring about a paradigm change. Though Kuhn makes ample remarks that address this concern, and thus makes clear why a paradigm will inevitably fail and be replaced by a new paradigm, this reading of Kuhn became quite widespread among philosophers of science.

Interestingly, Larry Laudan would later raise the opposite concern about Kuhn's account of science. As Laudan explains, "Kuhn's model correctly predicts that dissensus should be a common feature of scientific life. What it cannot explain so readily, if at all, is how ... scientific disagreements are ever brought to closure" (Laudan 1984, 16). In fact, Laudan claims that "Kuhn's analysis has several features built into it which seem to foreclose any possibility of accounting for the emergence of consensus formation" (p. 17).

Sahlins ends his review remarking that "the book succeeds in presenting sound but familiar reflections on the nature of science; it is also much ado about very little" (Sahlins 1964, 144). What is striking about Sahlins' assessment is that he completely fails to anticipate the impact the book would have generally, but especially among social scientists. Where Sahlins sees "much ado about very little," other social scientists, including leaders in a number of fields, saw a set of concepts and a general framework that proved to be a rich source for reflections on key methodological and epistemological issues in their own disciplines (see Wray 2021a, chapter 5). Most significantly, many social scientists thought about the natural sciences in quite a different way after reading SSR. They no longer thought of them as shielded from the impact of social factors external to science.

The anthropologist Clifford Geertz provides another interesting perspective, writing one year after Kuhn's death and from the perspective of someone who knew Kuhn, worked with him, and liked him. Kuhn was affiliated with the Institute for Advanced Studies at Princeton between 1972 and 1979, which overlapped with Geertz's time at the Institute (see Wray 2021a, 8).⁷ On Geertz's assessment, "*Structure* was the right text at the right time" (Geertz 1997/2000, 161). Interestingly, Geertz claims that "what remains of Kuhn's legacy, what enrages his most intransigent opponents and befuddles his most uncritical followers, is his passionate insistence that the history of science is the history of the growth and replacement of self-recruiting, normatively defined, variously directed, and often *sharply competitive scientific communities*" (p. 163; emphasis added). In this way, Geertz argues, "*Structure* opened the door to the eruption of the sociology of knowledge into the study of those sciences about as wide as it could be opened" (p. 164). Before the publication of SSR, sociological studies of science were confined narrowly to "external" factors, "concerned with the social effects of science, the institutional

⁷ The historian William Sewell, Jr., recounts his involvement in a seminar in 1975–1976 led by Geertz at the Institute for Advanced Studies, in which Kuhn participated (see Sewell 2005, 42–43).

norms which govern it, or the social origin of scientists” (p. 162). The new sociological studies, in contrast, were wholly unconstrained. The Strong Programme sociologists of science, for example, were committed to finding the social causes of both false and *true* beliefs. Geertz notes that “this particular genie, once out of the bottle, can’t be stuffed back in” (p. 164).

On the overall impact of the book, Geertz summarizes things quite well: “he had prayed for rain and got a flood” (p. 165). The astounding impact of SSR continues to perplex people.

1.5 Natural Scientists

The physicist David Bohm reviewed SSR in *The Philosophical Quarterly*. Bohm claims that “this book is without a doubt one of the most interesting and significant contributions that has been made in recent years in the field of the history and philosophy of science” (Bohm 1964, 377). Bohm was especially struck by its relevance to working scientists. And, though he acknowledges the wide application of the ‘paradigm’ concept, he does not see any difficulties with it (p. 378). Rather, he ends his review suggesting that “it may be worthwhile for scientists to try to become aware of the role that paradigms actually play in the life of scientific research in order that they shall be able more easily to realize the need for a change of Gestalt, when a particular field of study has been characterized by general confusion for some time” (p. 379). Thus, Bohm had no doubt that there are paradigms in science, as Kuhn describes. I do not think Kuhn imagined the book aiding scientists in the way that Bohm suggests. In fact, Kuhn even suggests that scientists’ ignorance of the history of their disciplines may serve an important functional role.

It is striking that a physicist of Bohm’s caliber would appreciate the book, given that many philosophers of science would come to regard the book as representing science in a degrading manner (see, e.g., Scheffler 1967; Popper 1970; and, later, Fuller 2004). Many philosophical critics were concerned that Kuhn made scientists look irrational, or at least nonrational. Clearly, what Kuhn was degrading was the philosophers’ image of science, not science itself. Kuhn took the success of science for granted. His aim was to explain how it could be so successful, but to do so in a manner that was consistent with the historical record. Many of Kuhn’s readers, though, missed this.

Reflecting on SSR two years after Kuhn’s death, the distinguished physicist Steven Weinberg gave a mixed assessment. On the one hand, Weinberg was confident that something like paradigms characterize normal science. As Weinberg explains, “there is more to a scientific

consensus than just a set of explicit theories” (Weinberg 1998, 48). Indeed, reflecting on his own initial reactions to the book, Weinberg claims that he was especially impressed by Kuhn’s “treatment of normal science.” Specifically, Weinberg claims that “Kuhn showed that a period of normal science is not a time of stagnation, but an essential phase of scientific progress” (pp. 50–51). On the other hand, Weinberg found Kuhn’s characterization of scientific revolutions quite unsettling, and described it as “seriously misleading” (p. 51). Like many philosophical readers, Weinberg thought that “Kuhn made the shift from one paradigm to another seem more like a religious conversion than an exercise of reason” (p. 48).

Weinberg’s appraisal illustrates that every reader can find something in the book that speaks to their experiences or preconceptions. No doubt, that is why the book has sold so well, and continues to influence how we think about science and scientific knowledge.

I.6 The Birth of Kuhn Scholarship

As we have seen above, right from the beginning, many scholars across the disciplines drew on or criticized Kuhn’s SSR, but a new phenomenon emerged in the late 1980s. What emerged was a new subfield in the history of philosophy of science: Kuhn studies. These are proper scholarly studies of Kuhn’s views, with the aim of understanding the development of his views and assessing the influences on, and subsequent impact of, his work. Much of this research is less concerned with ensuring that philosophers know what is and is not true about Kuhn’s theory of science and more concerned with ensuring that philosophers know the facts about what Kuhn *really* believed, the influences on him, and the development of Kuhn’s theory of science.

The pioneering study of this sort is, without a doubt, Paul Hoyningen-Huene’s *Reconstructing Scientific Revolutions: Thomas S. Kuhn’s Philosophy of Science*. It was first published in German in 1989, but it was quickly translated and published in English in 1993. James Marcum describes it as “the first full length analysis of Kuhn’s philosophy of science” (Marcum 2015, 183).

The book was partly written while Hoyningen-Huene was a visiting scholar at the Massachusetts Institute of Technology in the 1984–1985 academic year (see Hoyningen-Huene 1989/1993, xvii). It was published with a very supportive endorsement from Kuhn: “No one, myself included, speaks with as much authority about the nature and development of my ideas” (Kuhn in Hoyningen-Huene 1989/1993, xi). The central conceptual innovations developed and applied by Hoyningen-

Huene were the distinction between genetically subjected-sided and genetically object-sided aspects of perception and cognition more generally,⁸ and the distinction between phenomenal worlds and the world-in-itself. In invoking these distinctions, Hoyningen-Huene was able to clarify many of the apparently paradoxical aspects of Kuhn's view, aspects that had led to many uncharitable readings of SSR.

Any new book in this subfield must come to terms, to some extent, with the interpretation advanced by Hoyningen-Huene. And a perusal of many of the influential books in Kuhn studies that have been published since illustrate this clearly. In fact, "Hoyningen-Huene" is typically an entry in the book indexes (see, e.g., Bird 2000b; Sharrock and Read 2002; Nickles 2003a; Andersen et al. 2006; Wray 2011b; Kindi and Arabatzis 2012; Marcum 2015; Mayoral 2017; Mizrahi 2018).⁹

Alexander Bird has raised doubts about Hoyningen-Huene's idealist, Kantian reading of SSR. In fact, Bird makes the provocative suggestion that "Kuhn was himself influenced by Hoyningen-Huene's" reading (Bird 2012b, 869). More precisely, Bird claims that Hoyningen-Huene gave Kuhn's "earlier thought a ... philosophical sophistication that it did not really have" (Bird 2012b, 869). Whether or not one agrees with Hoyningen-Huene's reading of SSR, it is undeniable that he continues to make important and insightful contributions to Kuhn scholarship.

Another early influential book in Kuhn studies was Steve Fuller's *Thomas Kuhn: A Philosophical History for Our Times* (Fuller 2000b). In marked contrast to Hoyningen-Huene's book, which is aptly characterized as an "internalist" analysis, as it traces the development of Kuhn's ideas, Fuller's is decidedly an "externalist" analysis. Fuller argues that Kuhn's book is the product of Cold War American culture, produced under the influence of James B. Conant, who was himself involved in the Manhattan Project, and rationalizes a particular relationship between science and society, one that shields science from public scrutiny. On Fuller's reading, Kuhn's self-policing scientific specialty communities are answerable only to themselves, and are, at least in their own minds, not affected by broader societal concerns.

These two books, representing the two poles of research in Kuhn studies, speak to quite different audiences. Hoyningen-Huene is concerned with the development of Kuhn's thought, specifically, with the intellectual sources that influenced him as he wrote SSR. Fuller, on the other hand, is concerned with showing how Kuhn's view is merely

⁸ "Genetically" here refers to their origins.

⁹ With respect to collections, I only list those to which Hoyningen-Huene did not contribute a piece.

reflecting recent developments in the relationship between science and society, developments that were a consequence of the First World War.

Another major development in Kuhn studies is the use of the archival materials at the Massachusetts Institute of Technology, deposited there by Kuhn upon his retirement. Gradually, some of this material has been published thanks to the efforts of George Reisch and Pablo Melogno†, among others. For example, readers can now access Kuhn's Lowell Lectures, *The Quest for Physical Theory*, which Kuhn describes as his first attempt to write SSR (see QPT). It is now commonplace for those working in Kuhn studies to draw on the material in the archives (TSK Archives–MC240).

And no serious Kuhn scholar can ignore the candid and insightful interview that Kuhn gave in October 1995, the year before he died. The interview was conducted by Aristides Baltas, Kostas Gavroglu, and Vasso Kindi, while Kuhn was in Greece, on the occasion of his being awarded an honorary doctorate from the department of philosophy and history of science, from the National and Kapodistrian University of Athens (see RSS 253–323). Kuhn remarked that he rarely agreed to give interviews (see RSS 321). But in Athens, Kuhn speaks frankly about his upbringing and education, his career successes and disappointments, as well as his legacy and the impact of SSR, as he saw it in 1995. Indeed, during the interview, Kuhn claims, “Look, I’m saying some things that I’m glad to think will be around somewhere” (RSS 321).

The contributions to this volume build on the vast body of literature on Kuhn's philosophy written over the past three decades, as well the material in the Kuhn Archives and the interview. Some of the contributors to the volume have already made significant contributions to our understanding of Kuhn, while others provide new perspectives on Kuhn, the development of his ideas, and the value of the Kuhnian account of science. Together the essays in this volume constitute a rich contribution to our understanding of Kuhn, SSR, and science.

The essays are organized into four thematic parts. Part I contains essays that address influences on Kuhn as he wrote SSR. Richard Creath revisits the topic of the relationship between Rudolf Carnap's philosophy of science and Kuhn's. And Jamie Shaw examines the influence of the post–Second World War science policy debates on Kuhn, given that Kuhn was a protégé of James B. Conant, a key player in these debates. Part II contains essays on normal science and science education. Alisa Bokulich and Federica Bocchi examine Kuhn's so-called Fifth Law of Thermodynamics, which states that “no experiment gives quite the expected numerical results” (see ET 184). Pablo Melogno† examines the role of normal science in Kuhn's cycle of scientific change. And

Alexandra Bradner and Hasok Chang both examine aspects of science education, in the light of Kuhn's remarks on science education and science textbooks. Part III is concerned with the concepts of incommensurability, scientific progress, and revolutions. Alex Levine examines Kuhn's appeal to the notion of translation in explaining the relationship between one theory and its successor. Haixin Dang examines the social dimensions of group belief change during scientific revolutions. Chris Haufe examines Kuhn's appeal to fruitfulness as a value affecting theory choice. K. Brad Wray examines Kuhn's appeal to George Orwell's *1984* as a means to illuminate the nature of theory change. And Hanne Andersen raises concerns about the applicability of Kuhn's account of scientific change to contemporary science, given the dramatic ways in which the practices of science have changed since 1962. The essays in Part IV are concerned with the impact of SSR on the philosophy of science, the sociology of science, and the history of science. Jonathan Tsou examines Kuhn's ambiguous legacy in philosophy of science. Markus Seidel examines the appropriation of Kuhn's views by the Strong Programme in the Sociology of Scientific Knowledge. And Vasso Kindi provides a comprehensive overview and assessment of Kuhn's impact in the history of science.