Introduction

The Impact of *The Structure of Scientific Revolutions*

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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 nonlogical 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 ascendency 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