Fifty years of *Structure*

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The year 1962 saw the first publication of Thomas Kuhn's masterwork, *The Structure of Scientific Revolutions*. The present volume is one of a number of publications that were prepared in honour of the fiftieth anniversary of that event. The distinctive feature of this volume is the breadth of perspective brought to bear on the topic. Representatives of the different sub-disciplines of the history and philosophy of science are assembled in this volume. Perhaps understandably, the majority of the contributions are from philosophers.

The book opens with an introduction by the editors. After describing the background of the volume, William Devlin and Alisa Bokulich provide an overview of the central themes of *Structure*. They comment on the critical reception which the book initially met among philosophers. They then present a summary of the twelve chapters that follow.

The first of these is by Stephen Shapin. Shapin takes Kuhn's project to be naturalistic, to describe and explain rather than to celebrate or accuse. He suggests that "it was the new cultural and political place of science in the post-War decades that made the naturalism of *Structure* possible" (14). The political, economic and institutional conditions of science in the period after the Second World War were such that it might be approached in a matter-of-fact way without overwhelming need to justify its place in culture. For Shapin, the historical setting in which *Structure* emerged was one in which science was a powerful force not in need of defence. That is the "condition of possibility" for the "naturalistic sentiment" that Kuhn adopted.

In his contribution, Alexander Bird draws attention to historicist elements of Kuhn's view, and argues for an internalist reading of Kuhn. Bird distinguishes between a conservative and a determinist strand that may be found in Hegel's historicism. The former emphasizes history and tradition, while the latter sees laws and patterns underlying history. Kuhn embraces both elements with his focus on scientific traditions and his claim that scientific change conforms to a cyclical pattern of crisis, normal science, revolution, and return to normal science. Bird takes issue with sociologists of science who see Kuhn as

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providing the basis for an externalist approach to scientific change. As Bird notes, Kuhn's model of scientific change is primarily internalist: "A significantly externalist component in science would undermine the deterministic strand of Kuhn's historicism, and so is antithetical to Kuhn's philosophical aims for the history of science" (37).

Structure is a pivotal work in the history and philosophy of science. Alan Richardson uses the occasion of his contribution to reflect upon the relations between the history of science and the philosophy of science, remarking at one point on 'the underperformance of that late 1950s and 1960s intellectual formation titled "the history and philosophy of science"' (40). Kuhn himself thought that the history and the philosophy of science should be pursued as separate disciplines. Richardson suggests that Kuhn's reason for this was historiographical: "history of science as a practice engaged in by historians demands the formation of coherent and explanatory historical narratives and the practices involved in the creation of those narratives themselves demand answers to different sorts of questions than the default philosophical machinery would lead you to ask in the first place" (43). Richardson argues that it is less important to bring the history and philosophy of science into close contact than it is for historians and philosophers of science to separately articulate more carefully their "explanatory practices" (49).

According to the received view, Kuhn's historical approach marks a dramatic rejection of logical empiricism. In recent years, a revisionist view has emerged according to which the differences between Kuhn's approach and that of logical empiricists such as Rudolf Carnap are less extreme than had initially been thought. Though he grants that there are superficial similarities, in his chapter Jonathan Tsou argues that "the methodologies of Carnap and Kuhn are correctly regarded as two contrasting philosophical styles that mark a significant division between positivist and post-positivist philosophy of science" (52). Revisionists claimed that Kuhn and Carnap agree on incommensurability, the pragmatic basis of theory-choice and the existence of scientific revolutions. But, though there are similarities, Tsou argues that there are profound differences. Carnap's linguistic frameworks are not Kuhnian paradigms. Moreover, Carnap's logical analysis of scientific concepts differs methodologically from Kuhn's historical analysis of actual scientific practices.

One of the most influential objections to scientific realism is an appeal to the past failure of scientific theories known as the pessimistic induction: all past theories have been false, so it is likely that our current theories are also false. In her chapter, Sherrilyn Roush subjects the pessimistic induction to a rigorous critique, pointing out that it depends ultimately on questions of method. There are sufficient fine-grained differences in method throughout the sciences that past failures have no relevant implications with respect to probable failure on other occasions: "The horizontal inference from the past to the present is where the pessimist's stumbling block lies, in the question of whether our predecessors' unreliability is sufficiently relevant to our work to give us an induction to our own unreliability" (78). Roush's paper does not engage in detail with Kuhn. But it draws inspiration from Kuhn in the sense that it uses a historian's eye for detail to defeat the pessimistic induction and cast doubt on excessive discontinuity in paradigm-shift.

Kuhn was aware that *Structure* could be "too nearly all things to all people" (Kuhn 1977, p. 293). This is nicely illustrated with the paper by Cyrus Mody. Kuhn's account of normal science, rather than of revolutionary scientific change, is what Mody takes to be of most value. Mody is a fan of empirical studies of quotidian scientific work undertaken by ethnographers of laboratory science. Mody does not explicitly deny a role to evidence or method. But he ascribes an important role to the "social cues" that influence scientific judgment. Science is done by humans in social contexts: "the lesson from Kuhn – perhaps his most important and robust lesson – is that the humanity and sociality of wild-type science are constitutive of the scientific enterprise as we know it" (95). Recognition of this fact leads to a focus on what scientists do in their day-to-day practice rather than questions about incommensurability or the progress and rationality of paradigm change.

In their chapter, Rogier De Langhe and Peter Rubbens propose an "agent-based modelling" approach to scientific theory-choice. Rather than restrict theory-choice to the choice between existing theories, they expand the topic to include the search for new theories. They distinguish between scientists who "exploit" an existing theory and scientists who "explore" a new theory. They interpret Kuhn's non-algorithmic values of theory-choice as heuristics which scientists employ under conditions of uncertainty. However, they note that Kuhn was unable to explain how "such values lead decentralized scientists to produce collectively successful science" (106). On the basis of their modelling, they suggest that: "in the absence of centralized control, with only limited information and using nothing but a simple heuristic, the interactions of scientists result in a robust pattern of intermittent theory exploitation and exploration with shifts between them occurring at the rational point in time" (113).

One of the most controversial claims of *Structure* is the claim that in scientific revolution a choice must be made between a paradigm and an incommensurable alternative candidate for paradigm. Incommensurability seemed to render paradigm-choice irrational and lead to relativism. Kuhn continued to develop and refine the notion of incommensurability throughout his career. In his chapter, James Marcum chronicles the development of Kuhn's notion of incommensurability from his original version of the idea in *Structure* to the latest refinements in unpublished lectures near the end of his career. In *Structure*, incommensurability involves difference of problem-agenda, conceptual change and variation in how scientists view the world. In an attempt to respond to critics, Kuhn next characterized incommensurability in terms of non-algorithmic values and communication breakdown which requires scientists to become translators. Later Kuhn argued that incommensurability involves localized inability to translate from a sub-set of the terms of one theory into a sub-set of terms of another theory. Ultimately, Kuhn came to present the idea in terms of variation in the taxonomic structure embedded in the "lexicons" of theories. At the same time as Kuhn continued to refine the notion of incommensurability, Marcum notes, he increasingly came to present his account of science in evolutionary terms, so that in the end he adopted an evolutionary rather than a historical approach to the philosophy of science.

What was perhaps more controversial than the claim of incommensurability was Kuhn's idealist-sounding claim that "when paradigms change, the world itself changes with

them" (2012, p. 111). An important attempt to make sense of this claim was Paul Hoyningen-Huene's neo-Kantian reading of Kuhn, on which the phenomenal world of scientists varies with paradigm though the world-in-itself remains the same (Hoyningen-Huene 1993, p. 36). Drawing on Hoyningen-Huene as well as Ron Giere's perspectivism, Michela Massimi takes the neo-Kantian approach in interesting new directions. Massimi defines a notion of "naturalized Kantian kinds": "Scientific kinds are groupings or clusters of empirical properties, which have proved survival-adaptive and have met our conditions of possibility of experience (but not via some constructive activity of our mind)" (147). The idea reflects Quine's insight that natural kinds are projectible and support inductive inference. At the same time, the idea has a Kantian flavour because our natural kind groupings reflect "epistemic constraints" that derive from us. The resulting position develops the neo-Kantian approach to Kuhn in a way that brings it in some respects closer to realism.

Not until the final pages of *Structure* does Kuhn say anything explicit about truth. On his account, there is no need to see science as converging on truth. The "developmental process" described in *Structure* is one on which science evolves "*from* primitive beginnings" rather than "*toward* anything" (2012, pp. 169-70). In the 'Postscript' Kuhn rejected the idea of a match between theory and "what is really there", noting that he can see no "coherent direction of ontological development" in the history of physics (2012, p. 205). In his own contribution to the volume, co-editor William Devlin subjects Kuhn's views about truth to scrutiny. Kuhn's grounds for rejecting correspondence between statement and mind-independent reality are epistemic, since he holds that we have no neutral way of determining whether a correspondence obtains. As Devlin notes, Kuhn's epistemic point fails to show that there may be no such correspondence. Devlin also notes that Kuhn combines his rejection of truth with the claim that science yields knowledge. Assuming that knowledge requires truth, this makes Kuhn's view inconsistent. But Devlin makes a plausible interpretative suggestion which avoids the inconsistency. If Kuhn is understood in the neo-Kantian manner proposed by Hoyningen-Huene, truth may be taken to be correspondence to facts that obtain in a phenomenal world. Devlin concludes with the thought that: "By eliminating the notion of correspondence to a mind-independent world (which was Kuhn's initial reason for rejecting the traditional correspondence theory), the *phenomenal-world correspondence* theory of truth is able to help resolve the tension within Kuhn's enterprise and not disrupt the central philosophical claims he argued for in *Structure* onward" (166).

Kuhn thought of science as a group activity undertaken by a community of scientists. For that reason, *Structure* has often been read as a founding work in the sociology of science by practitioners of that discipline. In his chapter, Brad Wray examines the connection between Kuhn's approach to science and that of sociologists of science such as the Edinburgh Strong Programme. Rather than sociology of science, Wray takes Kuhn as offering a "social epistemology of science" (167). Though Kuhn thinks of science as a social activity, Wray points out that *Structure* has only one citation of a sociologist. Wray takes Kuhn as having been sympathetic to Merton but increasingly unsympathetic to the Strong Programme. Kuhn was critical of the Strong Programme with respect to their view that the values of science are subject to variation, as well as their emphasis on socio-economic at the expense of cognitive

interests and the way that they downplay the role of nature in the resolution of scientific disputes. Still, Kuhn thought that social factors play an important epistemological role in science. Wray points to the role of crisis in breaking down consensus and enabling the scientific community to explore alternatives to the existing paradigm as one aspect of this. A second aspect is one which emerged increasingly in Kuhn's later work, namely, the way in which specialization may resolve a crisis by replacing a crisis-stricken paradigm with new specialities that divide the previous field of research into separate areas.

In the final chapter of the book, Paul Hoyningen-Huene describes two phases in the ongoing development of Kuhn's thought. The first relates to changes that Kuhn introduced in the manuscript of *Structure* in light of feedback which he received on an earlier draft which Hoyningen-Huene refers to as *Proto-Structure*. Hoyningen-Huene calls attention to two chapters relating to normal science in *Structure*. In Chapter IV, 'Normal Science as Puzzle-Solving', Kuhn explains that normal science does not strive for novelty. It is devoted to puzzle-solving which is governed by rules of puzzle-solving adequacy. In Chapter V, 'The Priority of Paradigms', Kuhn goes on to say that though rules derive from paradigms, there is a sense in which paradigms may provide guidance even in the absence of rules. The discussion of this issue in *Structure* differs from the discussion in *Proto-Structure* precisely with respect to the latter issue. The idea that paradigms are prior to rules and may function in their absence is missing from the earlier draft. Hoyningen-Huene suggests that this was a late addition to *Structure* due to the Wittgensteinian influence of Stanley Cavell, one of the philosophers who provided comments on the draft. The second phase in the development of Kuhn's thought to which Hoyningen-Huene draws attention is material found in the manuscript entitled *The Plurality of Worlds* on which Kuhn was working at the time of his death. In that still unpublished work, Kuhn was continuing to work out his ideas about incommensurability, taxonomic structure and kind terms. It is clear from the summary that Hoyningen-Huene gives of this material that it will be of great interest to Kuhn scholars when it eventually does appear. On this last point, I cannot resist adding: *if* it ever appears!

All in all, this book represents some of the best of contemporary Kuhn scholarship. For the most part, the authors are well-informed about Kuhn and his work, as well as by the critical literature that has continued to grow in the years following Kuhn's death in 1996. A number of the papers draw on the texts of unpublished lectures that Kuhn presented toward the end of his career. Several of the authors have made good use of materials that are available in the Kuhn archives at MIT. All of the papers focus on different aspects of Kuhn's work and bring different perspectives to bear on these different aspects. At the same time, there are non-redundant overlaps between a number of papers which bring further strength and focus to the volume. As a collection, the volume represents a very fine body of work indeed.

On the topic of overlaps, I wish by way of conclusion to point to two common themes shared by a number of papers in the volume. The first relates to Kuhn's naturalism. Though a number of the authors comment on naturalistic elements in Kuhn, the notion of naturalism is not always employed in the same way. For Shapin, Kuhn is a naturalist in the sense that he writes about science in a matter-of-fact way without praise or blame, or any felt need to

legitimate the place of science in culture. By contrast, others such as Massimi tend to write about naturalism in a way closer to that of Quinean or post-Quinean naturalized epistemology. The second theme relates to the sociology of science. Though Mody shows how Kuhn's treatment of normal science may inspire sociological interest in the routine activities of workaday science, Bird and Wray argue that Kuhn's approach to science is an internalist one that is out of keeping with the externalist approach favoured by much sociology of science.

References

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