CHAPTER 1

What Did Aristotle Teach Kuhn?

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

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

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

https://doi.org/10.1017/9781009057882.002 Published online by Cambridge University Press

key ingredients would elude his grasp for years. So, my aim here is to understand how much Aristotle taught Kuhn, and how much Kuhn would need to learn before he could write *Structure*.

What Happened in the Summer of 1947?

Interestingly, Kuhn does not discuss the Aristotle experience in either *The Copernican Revolution* or *The Structure of Scientific Revolutions*, his first two books. Rather, the first sustained published discussion of the experience did not take place until 1977, in the Preface to his collection of papers, *The Essential Tension*. The discussion of the experience there runs for three pages. It is worth examining this account in detail.

We can begin with Kuhn's account of the facts. The experience happened in the summer of 1947 (see Kuhn 1977, xi–xii). He was preparing "a set of lectures on the origins of seventeenth-century mechanics" (Kuhn 1977, xi). These lectures were to be part of his contribution to the General Education Natural Science course he had been invited to work on with Conant. As Kuhn explains, in order to prepare his lectures on the origins of seventeenth-century mechanics, he felt he "needed first to discover what the predecessors of Galileo and Newton had known about the subject, and preliminary inquiries soon led [him] to the discussion of motion in Aristotle's *Physica* and to some later works descending from it" (Kuhn 1977, xi). Kuhn had initially thought that Galileo would have built on the work of Aristotle and contemporary Aristotelians. Scientific knowledge, he had assumed, was more or less cumulative in its growth.

But this is not what he found. Rather he was startled, largely as a consequence of what he unreflectively brought to his reading of the texts. As Kuhn explains, he "approached these texts knowing what Newtonian physics and mechanics were" (Kuhn 1977, xi). As a consequence of his own immersion in Newtonian physics, he reports that he approached the texts with the following two questions in mind: "(1) How much mechanics was known within the Aristotelian tradition, and (2) how much was left for seventeenth-century scientists to discover?" (Kuhn 1977, xi; numerals added). So he had approached the texts assuming that Aristotle's project was more or less the same as Galileo's and Newton's. Aristotle, Galileo and Newton were all physicists, after all, or so Kuhn had thought.

Initially, Kuhn was quite perplexed by the extent of Aristotle's ignorance of *mechanics*. As he explains, "the more I read, the more puzzled I became. Aristotle could, of course, have been wrong – I had no doubt that he was – but was it conceivable that his errors had been so blatant?" (Kuhn 1977, xi). Kuhn found it almost impossible to believe that Aristotle had been so misguided in his understanding of mechanics. The persistence of Aristotle's influence through the ages seemed irreconcilable with such a view.

Then the epiphany happened. Kuhn reports that "those perplexities *suddenly* vanished" (xi; emphasis added). Elaborating, Kuhn goes on to say that "I *all at once* perceived the connected rudiments of an alternative way of reading the texts with which I had been struggling" (Kuhn 1977, xi; emphasis added). His understanding of Aristotle and of Aristotle's writings was transformed. He felt that he could *now* understand the Aristotelian worldview. Kuhn's description of his transformative experience sounds much like the sort of shift one experiences when one sees the second image in a Gestalt figure, that is, the old lady as well as the young lady, or the duck as well as the rabbit, after a period of initially not seeing it.

What had changed? According to Kuhn, "for the first time [he] gave due weight to the fact that Aristotle's subject was *change-of-quality in general*, including both the fall of a stone and the growth of a child to adulthood" (Kuhn 1977, xi; emphasis added). He now recognized that Aristotle's *physics* was not principally a *science of mechanics*. Thus, he realized that Aristotle was not even engaged in the same enterprise as Galileo and Newton, even though we are accustomed to tracing a lineage from Aristotle to Galileo, and then to Newton.

As a consequence of Kuhn's profound change in understanding, a number of other aspects of Aristotle's physics fell into place for him. First, Kuhn realized that mechanics was not a central part of Aristotle's concerns. For Aristotle "*the subject* that was to become mechanics was at best a still-not-quite-isolable special case" (Kuhn 1977, xi; emphasis mine). Generalizing from this case, Kuhn learned that scientific fields are not fixed by subject matter once and for all. In time, Kuhn would emphasize the importance of not using contemporary terms, like physics, for example, to refer to earlier scientific fields and practices from which these contemporary practices evolved (see Kuhn 1977, xv–xvi; see also Kuhn 1997/2000, 290 and 295). This, he thought, might aid historians in not projecting back on to earlier scientific practitioners interests and concerns that had not been part of theirs.[1] This underscores his conviction that theories in a field

[1] Indeed, Kuhn would later express regret that he had failed to take this precaution in *Structure* (see Kuhn 1997/2000, 290 and 295). Kuhn claims that this is a common defect of histories of science written by scientist-historians (see Kuhn 1971/1977, 149). In fact, he notes that "sometimes a specialty

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are not profitably conceived as successive attempts to get at the same underlying reality. That is, the history of a scientific field is not fruitfully told as a history of the convergence on a fixed reality.[2]

Second, Kuhn also realized that the Aristotelian ontology was fundamentally different from the Newtonian ontology.

> The *permanent ingredients* of Aristotle's universe, its ontologically primary and indestructible elements, were not material bodies but rather the qualities which, when imposed on some portion of omnipresent neutral matter, constituted an individual material body or substance. (Kuhn 1977, xii; emphasis mine)

Further, Kuhn claims that he realized that "position itself was ... a quality in Aristotle's physics" (xiii). And he also realized that "in a universe where qualities were primary, motion was necessarily a change-of-state rather than a state" (xii). By virtue of these ontological differences between Aristotle's theory and Newton's theory, he came to believe that Aristotle and Newton had lived and worked in different worlds. In John Heilbron's apt phrasing, Kuhn realized that "Aristotle had not been writing bad Newtonian physics but good Greek philosophy" (Heilbron 1998, 507). Further, as Paul Hoyningen-Huene notes, with this experience Kuhn "caught a glimpse of incommensurability," a notion that would come to play a significant role in *Structure* (see Hoyningen-Huene 2015, 194).

Third, Kuhn was now able to appreciate *the integrity of the Aristotelian worldview.* For example, Kuhn explains that "the exposure to Aristotle ... taught [him] the integrity [of Aristotle's] quadripartite analysis of causes" (Kuhn 1977, xiv). More generally, he learned that earlier theories had their own integrity, an integrity that can often only be appreciated if one recognizes that earlier scientists were not aiming to do what those that followed aimed to do. The differences between them and their successors are not signs of failure, but rather indicate different concerns. Indeed, here we have the basis for Kuhn's view that successive theories in a field are

which they traced from antiquity had not existed as a recognized subject of study until a generation before they wrote" (Kuhn 1971/1977, 149).

[2] According to Kuhn, the history of science presented in science textbooks is built on such an assumption. As Kuhn explains, "partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific" (Kuhn 1962/2012, 137). In the context of science textbooks, this distortion, Kuhn suggests, may serve an important function, perhaps motivating students (see Kuhn 1962/2012, 136–137 and 164–165).

fundamentally different and cannot be aptly described as a series of ever more accurate approximations converging on the truth.

With the perspective gained from this transformative experience, Kuhn realized that he had inadvertently approached Aristotle's texts with presuppositions that were an impediment to his understanding them and Aristotle's project in general. As Kuhn explains, "being posed in a Newtonian vocabulary, [the] questions [he asked] demanded answers in the same terms, and the answers then were very clear" (Kuhn 1977, xi). As Kuhn reports, "even at the apparently descriptive level, the Aristotelians had known little of mechanics; much of what they had had to say about it was simply wrong. No such tradition could have provided a foundation for the work of Galileo and his contemporaries" (Kuhn 1977, xi).

But after he discovered "a new way to read a set of texts," Kuhn reports that he "had few problems understanding why Aristotle had said what he did about motion or why his statements had been taken so seriously" (Kuhn 1977, xii). As Kuhn explains, though he "did not become an Aristotelian physicist as a result . . . [he] had to some extent learned to think like one" (xii). That is, Kuhn could now see the world as Aristotle had, and understand why the sorts of research problems that had engaged him and his followers seemed important. Indeed, this aspect of historical scholarship would continue to intrigue Kuhn (see Kuhn 1997/2000, 280).

Kuhn describes the experience as his "own enlightenment." And most significantly, it led to his discovery of *the nature of scientific revolutions* (Kuhn 1977, xi). As Kuhn explains,

> what [his] reading of Aristotle seemed . . . to disclose was a global sort of change in a way men viewed nature and applied language to it, one that could not properly be described as constituted by additions to knowledge or by the mere piecemeal correction of mistakes. (Kuhn 1977, xiii)

This is the key insight Kuhn gained from the Aristotle experience, and this is why he claims the project that culminated in the publication of *Structure* began in 1947 (see Kuhn 1962/2012, xxxix). Scientific revolutions were central to Kuhn's new understanding of the growth of scientific knowledge.

Kuhn does draw some additional lessons from the experience, lessons concerning historiography and pedagogy. For example, Kuhn claims that, from this experience and similar ones with other classic texts in the history of science, he learned that "there are many ways to read a text, and the ones most accessible to a modern are often inappropriate when applied to the past" (Kuhn 1977, xii). Thus, he learned that the past is, at least initially, in

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some sense opaque to us, especially in those cases where we must, as historians of science, enter into a radically different theoretical framework and worldview.

But he also claims that the opacity of radical theoretical frameworks is not an insurmountable problem. He explains that "the plasticity of texts does not place all ways of reading on a par, for some of them (ultimately, one hopes, only one) possess a plausibility and coherence absent from others" (Kuhn 1977, xii). Thus, Kuhn did think that there were historical facts that we could in principle get at, or at least aim to get at. In no way was Kuhn led to some sort of unconstrained relativism about the history of science.

Finally, Kuhn gives students a heuristic to guide them in their endeavors to understand the scientific past, one that he learned from the Aristotle experience: "When reading the works of an important thinker, look first for the apparent absurdities in the text and then ask yourself how a sensible person could have written them" (Kuhn 1977, xii). These passages, he suggests, are the key to a more authentic reading of the texts. But Kuhn claims that "when those passages make sense, then you will find that more central passages, ones you previously thought you understood, have changed their meaning" (Kuhn 1977, xii). Thus, the process of making sense of theoretical frameworks and scientific practices from the past will change what we thought we understood. It is a hermeneutical process (see Kuhn 1977, xiii).

Kuhn discussed the Aristotle experience again in his 1987 paper "What Are Scientific Revolutions?" By this time, forty years had passed since it had taken place. The recounting of the story is quite similar to his account from 1977; not surprisingly, given that he could have reread his own earlier account as he set out to retell the story. But he does add some details. For example, see Kuhn's description of the circumstances of the experience: "I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out the window of my room – the visual image is one I still retain" (Kuhn 1987/2000, 16).[3] Next, he explains that "suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist

[3] Here Kuhn seems to employ a trope common in ethnography, recounting his experience in detail in order to give credibility to what he is about to tell us, just as the ethnographer gives a detailed description of what they see and experience as they arrive at the location where they will do their fieldwork.

indeed, but of a sort I'd never dreamed possible" (1987/2000, 16). The key here is his achieving a hitherto unanticipated new understanding.

In 1987, Kuhn makes clear some of the general lessons about scientific revolutions he drew from the experience, lessons that he had only alluded to in the earlier telling. Kuhn explains that "that sort of experience – the pieces suddenly sorting themselves out and coming together in a new way – is . . . [a] general characteristic of revolutionary change" (Kuhn 1987/2000, 17). With scientific revolutions, Kuhn notes, "the central change cannot be experienced piecemeal, one step at a time. Instead it involves some relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before" (Kuhn 1987/2000, 17). Kuhn thus draws attention to the *holistic* nature of the change from one theory to another. In this respect, his appeal in *Structure* to the duck/rabbit image is apt. One sees either a duck or a rabbit. There is no transitional figure open to our perception, something that is part duck and yet also part rabbit.

After describing some of the details of Aristotle's physics, Kuhn notes that

> those remarks . . . should sufficiently illustrate the way in which Aristotelian physics cuts up and describes the phenomenal world. Also, and more important, they should indicate how the pieces of that description lock together to form an integral whole, one that had to be broken and reformed on the road to Newtonian mechanics. (Kuhn 1987/2000, 20)

Kuhn makes explicit the holistic nature of theories, and the implications this has for both (i) the way they are understood, and, ultimately, (ii) the way they are overthrown. To learn a theory one must learn a cluster of concepts together. And a scientific revolution, unlike normal scientific research, involves a radical reworking of the scientists' worldview. This is why scientific revolutions are disruptive and are experienced as such by the scientists involved.

Kuhn also makes some passing remarks about the Aristotle experience in the interview published in *The Road since Structure*; this interview was conducted in 1995, near the end of his life (see Kuhn 1997/2000, 275, 276, 278, 285 and 292–293). Some of these remarks add no details beyond what he had already reported earlier. He does, though, note how excited he was to be given the opportunity to work with Conant on the course, and his surprise at being asked to "go out and do a case study on history of mechanics for this course" (Kuhn 1997/2000, 275). Kuhn also suggests that it was after the first semester of teaching the course with Conant that he realized he wanted to change his career path. As he explains, "I wanted to teach myself enough history of science to establish myself there in order to do the philosophy," for ultimately, Kuhn's ambitions were philosophical (see Kuhn 1997/2000, 276). The intended audience for the book he wanted to write about scientific revolutions was philosophers of science (see Kuhn 1962/2012, xxxix–xl).

Kuhn wanted to write a book about scientific revolutions in order to show how key discoveries in science can only be made by working with a radically new theory, one that makes assumptions about the world that are fundamentally different from the assumptions made by the replaced theory. This was the key insight Kuhn had gained that summer. And this was an important catalyst for his idea for a book on science, the book that would ultimately become *The Structure of Scientific Revolutions*. In fact, Kuhn repeatedly remarked that he "had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience" (Kuhn 1997/2000, 292; see also Kuhn 1977, x; but also Kuhn 1962/2012, xxxix).

What Kuhn Still Had to Learn

So far, I have argued that the Aristotle experience had taught Kuhn about the *nature* of scientific revolutions. Importantly, Kuhn came to understand that different theories in a scientific field have their own integrity and that the succession of theories in a field cannot be appropriately described as steps that bring us ever closer toward a final true theory. Rather, earlier theories, when understood correctly, in *their* cultural context, provided the means for progressive scientific practices and traditions. Aristotle's theory served the interests of his contemporaries in ways similar to those in which Newton's theory served the research interests of his contemporaries. And each new theory in a scientific field is not aptly described as aiming at the same things as the theory it replaced. Rather, the *field* itself shifts and changes with each change of theory, making claims of progress *through* revolutionary changes of theory somewhat problematic (see Kuhn 1997/2000, 292). These were important insights for Kuhn, and they would play a central role in his theory of science, and thus figure importantly in *Structure*, when he was ready to write it. But Kuhn was still a long way off from being able to write *Structure* after the summer of 1947.[4] There are

[4] Galison notes that Kuhn had sketched the outline of a book in a notebook from 1949. Here the proposed title was "The Process of Physical Science" (see Galison 2016, 55–56). This was even before the Lowell lectures, which Kuhn described as his first attempt to write *Structure*.

three important things about scientific revolutions that he had not yet grasped, and that would delay the writing of *Structure*. In fact, fifteen years would elapse between the Aristotle experience and the publication of *Structure*.

First, in 1947 Kuhn had not yet grasped the role or function of scientific revolutions in the growth of scientific knowledge. He knew that scientific revolutions threatened the cumulative account of the growth of scientific knowledge. But, in *Structure* Kuhn is quite clear that revolutions play a significant and definite function in the growth of scientific knowledge. Because every scientific theory ultimately encounters anomalies that expose its limitations, revolutionary changes of theory are an integral part of the growth of scientific knowledge. As Lydia Patton astutely notes, "it is an axiom of Kuhn's account that no paradigm can deal with all the phenomena" (see Patton 2018, 116). Revolutionary changes of theory are scientists' means of normalizing the anomalies that a theory brings to light, but cannot adequately resolve or normalize. That is, revolutionary changes of theory are scientists' way of adapting once they have run up against the limits of the theory they have been working with. Melogno and Courtoisie express the point as follows: "scientific revolutions constitute a key mechanism for scientific progress" (Melogno and Courtoisie 2019, 27). In 1947, Kuhn had not yet realized this.

Second, Kuhn had not yet put scientific revolutions into the general schema that emerges in *Structure*, the cyclical pattern of change that he claims characterizes the growth of knowledge in the natural sciences. This is not surprising as revolutions are only part of the cyclical pattern. He still lacked the necessary ingredients to articulate what role revolutions play in science, even if he did recognize that such revolutions exist.

Third, by 1947, Kuhn was in no position to describe or account for the periods of science between revolutionary changes of theory. That is, he had not yet conceived of normal science, the tradition-bound research activities that occur between revolutions. Kuhn suggests that this piece of the puzzle did not fall into place until the late 1950s. In fact, he suggests that it was while he was working on a late draft of his paper "The Function of Measurement in Modern Physical Science" that he began to work out the nature of normal science (see Kuhn 1977, xvii). He explains that in a late revision of that paper he introduced a section titled "Motives for Normal Measurement," where he claims "the bulk of scientific practice is . . . a complex and consuming *mopping-up operation* that consolidates the ground made available by the most recent theoretical breakthroughs" (see Kuhn 1977, xvii; see also Kuhn 1997/2000, 295; emphasis added).[5]

Kuhn's paper on measurement is devoted to debunking the idea that measurements in science are driven by the desire to either test or confirm theories, a view that Kuhn felt was implied by scientific textbooks. Indeed, measurement often involves the extension of theory, a mopping up of sorts. This notion of mopping up would make its way into *Structure* (see Kuhn 1962/2012, 24); it was crucial, as it made sense of what happens between revolutions, when a theory is taken for granted and assumed to provide an accurate description of the world.

In fact, the elusiveness of normal science would prove to be a real barrier for Kuhn in his efforts to write *Structure*. Kuhn notes how challenging it was initially for him to write on normal science to work out his ideas, even as late as 1958 when he was at the Center for Advanced Study in the Behavioral Sciences (see Kuhn 1997/2000, 296). He was still stuck in a particular framework that was proving rather unfruitful. As he explains, he "was taking a relatively classical, received view approach to what a scientific theory was – . . . [attributing] all sorts of agreement about this and that, and the other thing, which would have appeared in the axiomatization either as axioms or as definitions" (Kuhn 1997/2000, 296). Thus, Kuhn was still under the spell of the Logical Positivists, to some extent. With the aid of the "paradigm concept," specifically, the notion that much of the consensus in scientific research communities is on exemplars or models, rather than theories, which are expressed explicitly in propositions, things fell into place (see Kuhn 1997/2000, 296). The successful conduct of normal science requires the aid of exemplars, as well as a theoretical framework. And this insight enabled Kuhn to abandon the conception of scientific theories with which he had been working, a conception that was strongly influenced by Logical Positivism.

The potent combination of (i) the paradigm concept and (ii) the notion of mopping up enabled Kuhn to resolve the issue to his satisfaction. He now understood what scientists were doing between scientific revolutions.[6] And this enabled him to clarify the function of scientific revolutions, setting them in the context of the cycle of change. None of this, though, emerged from the Aristotle experience in the summer of 1947. Indeed, as

5 We can be even more precise here, if Kuhn's memory is to be trusted. He claims to have completed the revised draft that integrated this section in the spring of 1958 (see Kuhn 1977, xvii).

6 The elusiveness of normal science, that is, the practice of science between revolutionary changes of theory, caught Karl Popper off guard as well. Indeed, though critical of the practice of normal science, Popper acknowledges that Kuhn drew his attention to the notion (see Popper 1970/1972).

late as 1958, Kuhn was still working this out. Thus, George Reisch is mistaken in claiming that "by 1951 . . . *Structure*'s philosophy of science was largely in place" (Reisch 2016, 18).[7] There was still much to work out even in 1951.

What About the Historians Kuhn Was Reading?

It is worth briefly examining the influence that the various historians whom Kuhn was reading had on his thinking in this early period. In particular, one might be led to hypothesize that it was their influences that led him to the correct reading of Aristotle, the one that led Kuhn to recognize that science progresses through revolutionary changes of theory.

In the Preface to *Structure*, for example, Kuhn identifies a group of historians who were especially influential on his thinking. Specifically, he identifies a group of French historians: Alexandre Koyré, Emile Meyerson, Hélène Metzger and Anneliese Maier (see Kuhn 1962/2012, xl). He also mentions Arthur Lovejoy: specifically, his *Great Chain of Being* (see Kuhn 1962/2012, xl). In the Preface, Kuhn claims that "their works . . . have been second only to primary source materials in shaping my conception of what the history of scientific ideas can be" (Kuhn 1962/2012, xl). He also mentions Ludwik Fleck in the Preface, specifically Fleck's "almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*" (see Kuhn 1962/2012, xli). Is it possible that these historians of science drew Kuhn's attention to the significance of scientific revolutions?

I do not think so. In fact, there is good reason to believe that these historians were not the source of Kuhn's ideas about scientific revolutions, that is, the ideas that set him on course to writing *Structure*. After all, Kuhn notes that he only encountered the work of Meyerson, Metzger, Maier and Fleck when he was a junior fellow at the Society of Fellows at Harvard. That would mean that he only encountered them in or after 1949. That is two years after the Aristotle experience and Kuhn's experience working with Conant on the General Education Natural Science course. Indeed,

7 Reisch proceeds to list the various things that were "in place" for Kuhn by 1951. They include the following: "[1] experience underdetermines theory, [2] theory and observation were dependent and 'intermingled,' [3] theories were understood as holistic sets of ideas or conceptual schemes, and [4] the scientific mind was unaware that it operates within only one possible system of ideas . . . that in *Structure* would lead Kuhn to characterize scientific revolutions as 'invisible' to most scientists" (Reisch 2016, 18; numerals added). Oddly, shortly afterwards, Reisch claims that "what was missing at this early stage . . . was *Kuhn's theory of paradigms*" (see Reisch 2016, 18; emphasis added). This seems irreconcilable with Reisch's claims that "by 1951 . . . *Structure*'s philosophy of science was largely in place" (Reisch 2016, 18).

Kuhn states explicitly that it was only in 1950 that he was made aware of Meyerson's work. In a discussion with Karl Popper, when Popper was giving the William James Lectures at Harvard, Popper drew Meyerson's work to Kuhn's attention (see Kuhn 1997/2000, 286–287).[8]

It is interesting to see how Kuhn describes the influence of Fleck's book. Kuhn claims that Fleck's "essay ... anticipates many of my own ideas" (Kuhn 1962/2012, xli). But, elaborating, Kuhn notes that "Fleck's work made me realize that those ideas might require to be set in *the sociology of the scientific community*" (Kuhn 1962/2012, xli; emphasis added). So, if we are to take Kuhn's account as accurate, it was the sociological dimensions in Fleck's work, not the historical dimensions, that left their mark on Kuhn. Kuhn, though, was uncomfortable with the specific details of Fleck's "sociology." In particular, Kuhn claims that he "never felt at all comfortable ... with [Fleck's] 'thought collective.' It was clear it was a group, since it was a collective, but [Fleck's] model ... was the mind and the individual" (Kuhn 1997/2000, 283). That is, Fleck was ascribing properties that we associate with individuals to scientific collectives in a manner that struck Kuhn as implausible.

In his Foreword to the English translation of Fleck's *Genesis and Development of a Scientific Fact*, Kuhn explains his chief problem with Fleck's analysis in more detail. In Kuhn's words:

> what troubles me is [that] ... the notion [of a thought collective is] intrinsically misleading and a source of recurrent tensions in Fleck's text. Put briefly, a thought collective seems to function as an individual mind writ large because many people possess it (or are possessed by it). To explain its apparent legislative authority, Fleck ... repeatedly resorts to terms borrowed from discourse about individuals. (Kuhn 1979, x)

When Kuhn developed his own "sociology of science" he assumed that scientific research communities, the sorts of groups that work with a theory and undergo revolutionary changes of theory, were composed of many individuals, each different from the others in subtle but important ways. These individual differences would play a crucial role in Kuhn's account of scientific change. The groups, as far as Kuhn was concerned, were like biological species, where individual differences really matter. They play a crucial role in understanding how the community as a whole responds to

[8] Kuhn claims that he "didn't like the philosophy at all" in Meyerson's *Identity and Reality*, "but, boy, did [he] like the sorts of things [Meyerson] saw in historical material ... [Meyerson] was getting it right in ways that were different from the ways that history of science was being written" (Kuhn 1997/2000, 287).

a new challenge, be it a change in the environment, in the biological case, or an anomaly, in the scientific case.

Koyré is a more complicated case. Kuhn did read Koyré's work as he prepared the material on the history of mechanics for the course with Conant. In particular, Kuhn read *Études Galiléennes* (see Kuhn 1990/2016, 21). And he often encouraged students to read Koyré's work. Indeed, John Schuster, who was a Ph.D. student in history at Princeton when Kuhn taught there, recalls that "when Kuhn addressed each year's crop of new history of science graduate students, he would make a point of bringing in his well-worn, pre–World War II copy of Koyré's *Études galiléennes* . . . He would intone, 'Nobody is leaving here until they have read all of this'" (Schuster 2018, 395, fn. 7). Koyré's work was thus really important for Kuhn. But nothing like the picture of science that we find in *Structure* can be found in Koyré's work. The type of influence Koyré could have had on Kuhn would have been methodological, specifically an internalist approach to the history of science. In fact, in a paper on the relationship between the history of science and the philosophy of science, Kuhn reports that "from [Lovejoy and Koyré] my colleagues and I learned to recognize the structure and coherence of idea systems other than our own" (Kuhn 1976/1977a, 11).

Consequently, I think that it is more likely that it was Kuhn's encounter with Aristotle's work that drew his attention to scientific revolutions and the complications they raise for a cumulative account of scientific progress.

My aim in this chapter has been to understand what Kuhn learned from the Aristotle experience he had in the summer of 1947, when he was preparing a set of lectures on the history of early modern mechanics as part of his contribution to the General Education Natural Science course that he was working on with Conant. Though the experience left Kuhn with an acute awareness of the existence of scientific revolutions, having had to cross a revolutionary divide to make sense of Aristotle's physics, he still lacked a clear sense of what role these episodes played in the *development* of scientific knowledge. That would come later, but only after he had developed an understanding of normal science and its relationship to revolutionary changes of theory. So, on the one hand, the importance of the experience should not be underestimated. It was the catalyst for writing *Structure*. It showed him that many of the assumptions he had held about science were mistaken; most importantly, the assumption that the growth of scientific knowledge is strictly cumulative. On the other hand, in a certain sense, the Aristotle experience was just a catalyst, for it would take Kuhn another fifteen years before he could articulate the significance of scientific revolutions in the development of scientific knowledge. Much more work would be required before he could do this.

Finally, as noted above, Kuhn also drew some methodological lessons from the Aristotle experience. These pertain to the practices of conducting research in the history of science. These lessons were, no doubt, important to Kuhn as a practicing historian, and in the seminar room, teaching the history of science. But they are quite tangential to the philosophical view that he was developing, initiated by the Aristotle experience. Historians of science are far less concerned with scientific revolutions than are philosophers of science (see, for example, Shapin 1996). They are, though, concerned with gaining the skills to cross a cultural (perhaps revolutionary) divide in their efforts to make sense of the work of earlier scientists and natural philosophers. Not surprisingly, given the profound impact that reading Aristotle had on his own development as a historian, Kuhn would often have students read Aristotle in his courses (see Kuhn 1997/2000, 288).