Sharpening Philosophical Concepts: Thomas Kuhn and the History of Quantum Physics

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Abstract

In 1962, Thomas Kuhn started work on a project titled Sources for History of Quantum Physics. For this project, Kuhn and his collaborators conducted interviews with quantum physicists, and they collected archival materials regarding these developments. As of yet, the SHQP-project has received little attention from Kuhn-scholars. I will discuss how Kuhn made use of these historical materials in his later work. My starting point will be a claim by Anke te Heesen, who argues that Kuhn's aim with the interviews was to empirically test Structure. I will argue that there is little evidence for this claim, and that Kuhn's primary aim was instead to collect as much historical resources as possible. These he then used, among other things, in his work on a lecture titled 'The Crisis of the Old Quantum Theory, 1922-1925'. I will then highlight how Kuhn, in his philosophical writings, made use of this historical study to further sharpen certain concepts he had introduced in Structure. This will also allow me to reply to a claim by Suman Seth, who has argued that Kuhn's crisis-concept does not fit quantum theory before 1925. I will argue, however, that Kuhn's work was very close to Seth's own position.

1 Introduction

1962 was a pivotal year for Thomas Kuhn. Not only did he publish *The Structure of Scientific Revolutions* (1962), he also enrolled in a new project,

titled *Sources For History of Quantum Physics* (*SHQP*). For this project, Kuhn and his collaborators collected archival materials about the history of quantum physics, and they conducted interviews with physicists who had participated in its development (including Niels Bohr, Werner Heisenberg, Max Born, Pascual Jordan and Paul Dirac). The *SHQP*-project was of major significance for both history of quantum physics and oral history of science,[1] as well as for Kuhn himself: as he pointed out, it provided him with much of the material for both his *Black-Body Theory* book (1978, xi) and his paper on Bohr's quantized atom model with John Heilbron (1969, 213).

Notwithstanding this significance, Kuhn-scholars have barely studied the *SHQP*-project. Quite a few of them do not mention it at all, including the *Stanford Encyclopedia of Philosophy*-entry on Kuhn (Bird, 2022).[2] Most authors that do mention it, only do so in passing.[3] And even work specifically focused on Kuhn and the history of science often contains very little discussion of the *SHQP*-project.[4] The only two real exceptions are K. Brad Wray (2021b, 121-135) and Juan Mayoral (2024, 350-368), who both offer relatively extensive discussions of the project within Kuhn's career.

Moreover, those Kuhn-scholars that have discussed Kuhn's work on the history of quantum physics have focused only on whether his philosophical work, in particular *Structure*, influenced his historical work.[5] The central topic of this paper, on the other hand, will be the other direction, i.e. the influence of Kuhn's historical work on his philosophical reflections. My focus will be, more specifically, on how Kuhn used materials collected by the *SHQP*-project to further develop the concepts he had introduced in *Structure*. My starting point will be a claim by Anke te Heesen, who until now

[1]For the history of quantum physics, see e.g. (Brush, 2000, 49; Van Dongen et al., 2009, 277; Badino, 2016, 329; Freire et al., 2022, 4). For oral history of science, see e.g. (Niethammer, 1972, 472, note 53; Weiner, 1988, 551; Doel, 2003, 349).

[2]It is not mentioned either by Paul Hoyningen-Huene (1993) or Wes Sharrock and Rupert Read (2002), nor can it be found in the volumes edited by Theodore Arabatzis and Vasso Kindi (2012), Alisa Bokulich and William Devlin (2015), Moti Mizrahi (2018) or K. Brad Wray (2021a).

[3]As is done, for example, by Thomas Nickles (2003, 10), James Marcum (2015, 17-18), and Lorraine Daston and Robert Richards (2016, 4).

[4]It is not mentioned, for example, by Bird (2015), Vasso Kindi (2005), Jan Golinski (2012) Stefano Gattei (2016), Pablo Melogno (2021) or Mauro Condé (2023).

[5]This is for example the case with *Black-Body Theory*, which has been discussed mainly regarding the question whether the book is in line with *Structure*. See (Timmins, 2019) and (Potters, 2022) for recent overviews of this discussion.

has developed the only extensive historical account of the *SHQP*-project (te Heesen, 2020, 2021, 2022). According to her, "[t]he interview project – this, at least, was the hope – could be a way of grounding Kuhn's theory of the dynamics of scientific knowledge in a new way" (2020, 89; 2022, 14). I will argue that there is little evidence for this claim that Kuhn's aim with the *SHQP*-project was to test *Structure*. Kuhn's primary goals seems rather to have been to collect as much historical resources as possible, which could then be used to develop historical narratives. These could then in turn be used to further fill in aspects of the sketch of scientific practice he had elaborated in *Structure*.

I will illustrate how Kuhn did this by means of a specific lecture that Kuhn gave multiple times after the *SHQP*-project, titled 'The Crisis of the Old Quantum Theory, 1922-1925'. This lecture, which was explicitly motivated by how he had conceptualized 'crisis' in *Structure*, offered a detailed historical discussion of what Kuhn (1970, 258) described as one of the clearest examples of a crisis. By means of a discussion of a few of Kuhn's philosophical writings from that period, I will then show how Kuhn used these historical materials to further develop his views not merely on crises, but equally well on values, incommensurability, and scientific communities. This will not only lead me to conclude that the *SHQP*-project and Kuhn's work on quantum physics deserve more attention, but also to develop a short reply to historian of physics Suman Seth (2007). He has argued that Kuhn, in his later work, reconceptualized his notion of crisis: no longer was it the state of mind of an individual scientist, but rather a state permeating a whole scientific community. The problem with this conceptualization, Seth argues, is that it does not fit the development of quantum physics before 1925, since a large part of the community did not have any urgent sense of a crisis at the time. I will argue, however, that Kuhn's views in fact were quite close to Seth's.[6]

[6]In what follows, I will often refer to materials that are part of the Thomas S. Kuhn papers, which are located in the MIT Distinctive Collections (MC-0240). For these materials, I will use the following reference: TSK, followed by the box and the folder number/title.

2 Kuhn and Sources for History of Quantum Physics

2.1 The Sources Project

The *SHQP*-project did not start with Kuhn. Near the end of the 1950s, several physicists and historians (including Edward Purcell, Gerald Holton, John Wheeler, John van Vleck, and Richard Shryock) decided that it was urgent to collect as much material as possible regarding the historical development of quantum mechanics, and in particular to collect memories from those who had participated in these developments, since quite a few of them had already passed away (Kuhn et al., 1967, vi). In 1961, a funding proposal was submitted to the National Science Foundation for a three-year project (1962-1964) that would collect manuscript materials and interview physicists who had participated in the development of quantum physics between 1900 and 1930.[7]

Kuhn was eventually chosen to lead the project, primarily because of his PhD in physics and his expertise as historian of science, having written a book on the Copernican Revolution (1957) and having contributed extensively to James Conant's Harvard Case Histories project.[8] Kuhm's collaborators were John Heilbron, Paul Forman, and Lini Allen. Kuhn and Heilbron prepared and conducted the interviews, and Heilbron was also responsible for surveying and contacting libraries regarding their archival materials. Forman's task was to arrange and catalog archival materials on microfilm, and to transcribe interview recordings. Lini Allen, finally, took care of all (international) administrative duties, and together with Forman she also transcribed interviews.

Often, they would also be assisted by temporary collaborators. One of them was Saul Benison, at the time research associate at the Columbia Center for Oral History (Kuhn et al., 1967, 2). He was consulted specifically with regards to how the interviews should be prepared and conducted. According to te Heesen (2020, 93), he "repeatedly stressed the necessity of extensive preparations for interviewing", an advice that was duly followed. The *SHQP*-interviews were indeed extensively prepared. For each scientist, Kuhn and Heilbron compiled an elaborate biography and bibliography, studied in detail

[7]NSF funding proposal, pages 3-4, dated June 7, 1961. TSK papers, Box 6, Folder 12.
[8]NSF Funding proposal, page 5, dated June 7, 1961. TSK Box 6, Folder 12.

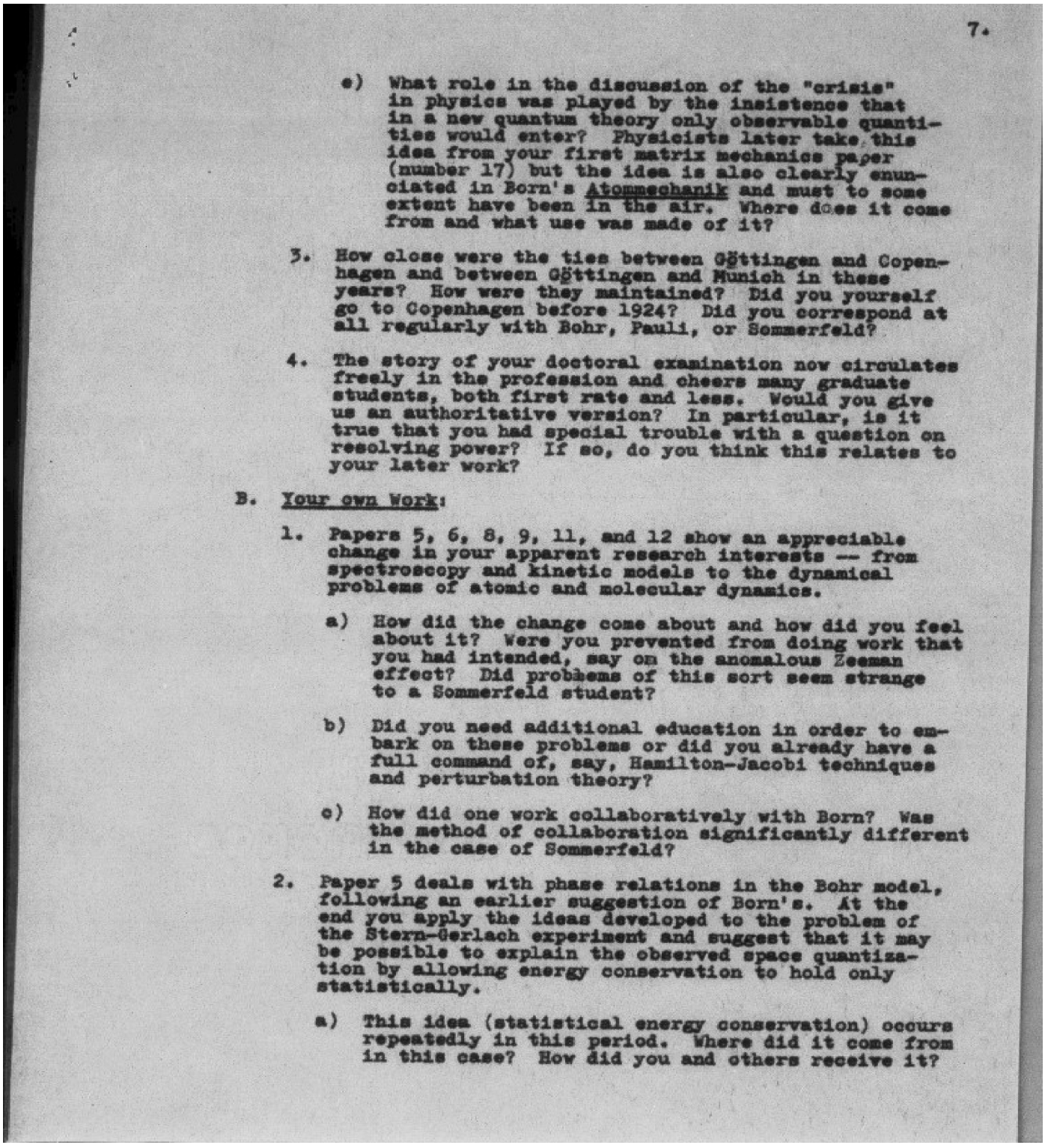
most of their published work, and prepared long lists of detailed questions. The focus of these preparations was primarily on the interviewee's education, their research and its scientific context (see an example below from the preparations for the Heisenberg interviews).

The interviewees were given these materials beforehand to prepare. During the interviews, these questions acted as starting point and as backup material for when the conversation would slow down. The interviews were recorded and transcribed afterwards, following a transcription-system developed during the project (te Heesen, 2022, 94-95). The collected archival materials and the interview transcriptions, as well as the preparatory materials, were then copied on microfilms, which were stored at different libraries in the US and Europe. The amount of material collected was enormeous: "[a]t the close of its operations in 1964 the project had conducted and transcribed some 200 interviews with about 100 informants, and had arranged for the microfilming of about 100.000 frames of material." (Heilbron, 1968, p. 98).

In later years, Kuhn often expressed disappointment with the interview-part of the *SHQP*-project. In a 1995 interview, for example, he stated that "[i]nterviewing was frustrating as hell!" (Baltas, Gavroglu & Kindi, 2000, 303). The main reason for this was that if the interviewees could remember anything at all, those memories were often colored and shaped by later events. Already in their first year-report, Kuhn and his collaborators pointed out this limitation: "we so very seldom get useful answers in response to questions on historical matters of technical substance[.] [...] [M]any of our subjects have simply erased from their memories almost all of the random groping for solutions that was an essential part of the development of quantum physics".[9] They repeated this claim both in the final *SHQP*-report and in later recollections (Kuhn et al., 1967, 4; Heilbron, 2011, 7; Baltas, Gavroglu & Kindi, 2000, 303; Sigurdsson, 2016, 26-27).

This does not mean, however, that Kuhn considered the interviews totally worthless. Several times, he and his collaborators highlighted how some interviews provided valuable biographical-social information. As they put it in the first year-report: "[t]he type of material in which our tapes are generally richest [...] is semi-personal and autobiographical. [...] Many of our subjects talk well and often movingly about their scientific education and mentors as well as about the institutions within which their own scientific

[9]First *SHQP* year-report, dated August 8, 1962, page 4. TSK Box 6, Folder 12.



7.

e) What role in the discussion of the "crisis" in physics was played by the insistence that in a new quantum theory only observable quantities would enter? Physicists later take this idea from your first matrix mechanics paper (number 17) but the idea is also clearly enunciated in Born's Atommechanik and must to some extent have been in the air. Where does it come from and what use was made of it?

3. How close were the ties between Göttingen and Copenhagen and between Göttingen and Munich in these years? How were they maintained? Did you yourself go to Copenhagen before 1924? Did you correspond at all regularly with Bohr, Pauli, or Sommerfeld?

4. The story of your doctoral examination now circulates freely in the profession and cheers many graduate students, both first rate and less. Would you give us an authoritative version? In particular, is it true that you had special trouble with a question on resolving power? If so, do you think this relates to your later work?

B. Your own Work:

1. Papers 5, 6, 8, 9, 11, and 12 show an appreciable change in your apparent research interests -- from spectroscopy and kinetic models to the dynamical problems of atomic and molecular dynamics.

a) How did the change come about and how did you feel about it? Were you prevented from doing work that you had intended, say on the anomalous Zeeman effect? Did problems of this sort seem strange to a Sommerfeld student?

b) Did you need additional education in order to embark on these problems or did you already have a full command of, say, Hamilton-Jacobi techniques and perturbation theory?

c) How did one work collaboratively with Born? Was the method of collaboration significantly different in the case of Sommerfeld?

2. Paper 5 deals with phase relations in the Bohr model, following an earlier suggestion of Born's. At the end you apply the ideas developed to the problem of the Stern-Gerlach experiment and suggest that it may be possible to explain the observed space quantization by allowing energy conservation to hold only statistically.

a) This idea (statistical energy conservation) occurs repeatedly in this period. Where did it come from in this case? How did you and others receive it?

Figure 1: A microfilm scan of a page of the Heisenberg interview preparations. Heisenberg, Werner, Transcripts of oral history recordings: Heisenberg, Werner (Biography), AHQP M/f No. 1419-02_heisenberg-001
[alt=A scan from the preparations for the Heisenberg-interviews. It shows a number of very detailed, technical questions about Heisenberg's published papers, his education and his approach as a young scientist. One question explicitly refers to a crisis in physics, and asks whether there was much discussion during that crisis about how a new quantum theory should be restricted to observable quantities.]

work was done".[10] While Kuhn and his collaborators again highlighted this aspect of the interviews later on (Kuhn et al., 1967, 4-5; Baltas, Gavroglu & Kindi, 2000, 303), they also stressed that even with regards to these aspects, the interviews lacked detail and were less informative than the archival materials collected:

> One does often get from our tapes a vivid feeling of what it was like to be a physicist in the late teens and twenties, and this "sense of the milieu" is very hard to capture in other ways. But these biographical reports, because usually lacking in details, have come increasingly to retrace grounds already covered. And on substantive issues we continue to find that very few of our subjects can supply much significant information. Almost no one is able to tell us very much about the sources of new ideas and problems, the difficulties encountered in developing them, and their reception by the physics community. Because we have gotten some answers and have simultaneously been led to recognize unsuspected historical problems and because the area we have been investigating is of unique importance, our effort has surely been justified. But though our tapes and transcripts will be a useful supplement to the more usual sorts of historical record, they will not decisively affect the way in which history is henceforth done. Manuscripts and the published records will remain the first and the richest sources.[11]

2.2 No Explicit *Structure* in *SHQP*

As mentioned in the introduction, the only extensive historical study of the *SHQP*-project as of yet has been done by Anke te Heesen (2020; 2021; 2022). As a historian of the humanities, her focus is primarily on the project's place within the history of oral history of science: how it came about, how it was organized, how the interviews were prepared, conducted and transcribed, and how Kuhn and his collaborators navigated the project's societal context. While her focus is less on the project's position within history and philosophy of science, as one reviewer has pointed out (Walter, 2023), she does make a few claims about Kuhn's philosophical motivations. In particular, she

[10] First *SHQP* year-report, dated August 8, 1962, page 3. TSK Box 6, Folder 12.
[11] Second *SHQP* year-report, dated October 28, 1963, page 5. TSK Box 6, Folder 12.

argues in several places that "[t]he interview project – this, at least, was the hope – could be a way of grounding Kuhn's theory of the dynamics of scientific knowledge in a new way" (2020, 89; 2022, 14). As evidence for this claim, she points towards Kuhn's later disappointment with the *SHQP*-interviews. Kuhn was disappointed, according to her, because the *SHQP*-interviews did not offer the kind of historical material that could provide empirical confirmation of his philosophical views:

> Kuhn saw the core of *SHQP*, if not as a failure, then at least as extremely precarious. Interviews turned out to be a [...] less than valid historical method for him. The human factor that emerged here was not particularly useful for the resulting historiography, which should be based on the greatest possible objectivity. His high expectations of finding confirmation, perhaps even an empirical basis for his theoretical assumptions about scientific revolutions in these conversations had not been fulfilled. Before the interviews began, Kuhn had assumed that his scientific witnesses were credible and had precise memories, but he had to admit that they could offer neither. (te Heesen, 2022, 169-170; personal translation)

Te Heesen's claim that Kuhn was disappointed because he had hoped that his interviewees would provide credible, precise memories is mainly based on a 1991 interview with Kuhn (Sigurdsson, 2016). While Kuhn did indeed express disappointment there, he also stated that he already knew before the start of the project that scientists' own recollections were often unreliable. What disappointed him, rather, was that most interviewees just did not have many recollections at all:

> I knew as a historian of science that scientists' recollections of their own work is quite bad historically; that they see themselves as having worked towards the thing they eventually discovered, although when you look back you find that in fact they were looking for something entirely different. So I did not expect that the interviews would produce the sort of information about sources of discovery that the physicists on the committee expected. But I also knew that if you study the papers against the recollections of the scientists, you often find terribly important clues about the

> processes the scientists had gone through. [...] So that's what I thought would occur, and what surprised me, then, was the number of times I got simply "I don't remember [...] How would you expect me to remember something like that.". (Sigurdsson, 2016, 26)

This indicates that, contrary to what te Heesen has claimed, Kuhn hever had the hope to empirically ground or confirm *Structure* by means of the *SHQP*-interviews. Moreover, there are few to none explicit references to *Structure* in the *SHQP*-project that could provide evidence for such a specific philosophical motivation from Kuhn's side. *Structure* or Kuhn's philosophical views were not mentioned, for example, in the 1961 NSF-proposal in which Kuhn was put forward as the project's leader (except as a forthcoming book, without any specification about its content). What was rather primarily highlighted was that "Professor Kuhn is the only man who combines a thorough grounding and some research experience in quantum mechanics with an established position as a practicing historian of science".[12] Nor is there any explicit reference to Kuhn's philosophical views either in later *SHQP*-reports or in the interviews themselves. The only place where Kuhn explicitly mentioned it was in the Heisenberg-interview of February 27, 1963.[13] In response to a side-remark by Kuhn about his recent book, Heisenberg stated that he had read *Structure* with great interest, and he asked Kuhn about his views on theory change in physics. After going into a bit more detail, Kuhn soon fell back in his role as interviewer, stating that "You catch me in a position I'd rather not be in. [...] I would be reluctant at this point to get very deeply into this".[14] Nor, finally, did Kuhn ever explicitly refer to the interviews themselves in his later philosophical writings.

Moreover, later on in the same 1991-interview, Kuhn even stated explicitly that he did not expect any direct philosophical insights from the interviews: "I would say that the project had substantially no effect on my views in *Structure*. I never thought that *Structure* was more than a highly schematic sketch. I did not expect any direct lessons" (Sigurdsson, 2016,

[12]NSF Funding proposal, page 10, dated June 7, 1961. TSK papers, Box 6, Folder 12.

[13]The full transcript of this interview can be found here: `https://repository.aip.org/node/127987` (consulted on November 3 2025). The only discussion of this aspect of the Heisenberg-interviews until now has been by Alisa Bokulich (2006).

[14]Page 30 of the 9th *SHQP*-interview by Kuhn with Heisenberg, dated February 27, 1963.

27). If we take the 1991-interview as indicative of Kuhn's motivations in the 1960s, as te Heesen does, then this indicates that there is little evidence for te Heesen's claim that Kuhn aimed to empirically ground *Structure* in the interviews. This does raise the question how Kuhn, after the end of the *SHQP*-project, saw the relationship between his historical and his philosophical work. His characterization of *Structure* as a schematic sketch suggests that he saw it as something that needed to be filled out further by, among other things, detailed historical studies. In what follows, I will argue that this is indeed how Kuhn used his historical materials in the aftermath of the *SHQP*-project.

3 Historically Investigating the Crisis of the Old Quantum Theory

3.1 Crisis in the Interviews

One concept that Kuhn seems to have been eager to investigate further through his studies of the history of quantum physics was the notion of 'crisis'. A possible motivation for this could be that the concept was not only central to *Structure*, but equally well a term used extensively by physicists and mathematicians in the 1920s, as Forman (1971) later showed in an article that relied extensively on materials collected by the *SHQP*-project.[15] Both in the interview preparations and in the interviews themselves one can find the term being used.[16] On most of these occassions, Kuhn did not specify how he understood 'crisis' in this context: he was primarily interested either in locating what he believed to be a crisis-moment – situating it, more or less, between 1922 and 1925, and primarily in Copenhagen and Göttingen – and in discerning some of its sources. The most extensive discussion of it is to be found in the interview with Edwin Kemble:

[15]For further examples beyond those provided by Forman, see (Seth, 2007, 45).

[16]It appears, for example, in the preparations for the Heisenberg-interviews (the first question on the image above), as well as in those for the interviews with, among others, Franco Rasetti and Enrico Persico, Nevill Mott and G.E. Uhlenbeck (TSK Box 15, Folder 'Fermi', and Box 16, folders 'Mott' and ''Uhlenbeck'). The term is also used in the interviews with Felix Bloch (May 14, 1964), Max Born (October 17, 1962), Paul Dirac (May 7, 1963), Heisenberg (February 11, 1963), Edwin Kemble (October 2, 1963) and Alfred Landé (March 6, 1962).

> **Kuhn:** I get the impression from some of the things you said that there was among American physicists in the middle 20's, 1923-1926, very little of that sense of acute crisis in quantum mechanics that was very prevalent at both Göttingen and Copenhagen. I think people at Munich were not so convinced, but still aware that other people felt this way. Dirac felt this way, although I'm not at all clear what Fowler did.
> **Kemble:** There was almost nobody to pick up a crisis of that kind and do anything with it in this country.
> **Kuhn:** Regardless of whether there was anybody to do anything with it, the question as to whether people really felt that there was something fundamentally astray –. I don't mean just being aware that there were a lot of unsolved problems that nobody had learned how to manipulate yet. People had pretty well decided that these problems were just not going to come out by any sort of better, higher approximations or something of the sort – that the problems go right smack to the root of the matter.[17]

Kuhn's characterization of a crisis in quantum physics here seems to be quite close to how he had conceptualized it in *Structure*, namely as "a period of pronounced professional insecurity [...] generated by the persistent failure of the puzzles of normal science to come out as they should" (Kuhn, 1962, p. 67). In what follows, I will show that Kuhn was indeed interested in further filling out, by means of historical studies, how one could understand crisis as a philosophical concept.

3.2 The Crisis-lecture

Soon after the *SHQP*-project ended, Kuhn started giving lectures titled 'The Crisis of the Old Quantum Theory, 1922-1925'. He gave these lectures on at least four occassions: on November 23, 1965, at the New York Section of the History of Science Society;[18] on April 21, 1966, at the Annual Meeting

[17]Page 21 of the *SHQP*-interview with Edwin Kemble by Thomas Kuhn and John H. Van Vleck, October 2, 1963, Harvard University, Cambridge, United States. The transcript of this interview can be consulted at `https://repository.aip.org/node/128028` (consulted on November 11, 2025).

[18]TSK Box 3, Folder 14.

of the American Philosophical Society in Philadelphia;[19] on April 17, 1970, as the Lovejoy Lecture at Johns Hopkins University in Baltimore;[20] and on November 5, 1980, as a HUSC Research Lecture at Harvard University, which apparently was recorded (Massimi, 2005, p. 78). Kuhn started his 1966 American Philosophical Society lecture as follows:

> In a recent study of scientific development, I risked the generalization that revolutionary transformations of scientific theory are ordinarily preceded by crises, these being induced by the breakdown of previously successful problem-solving techniques. 'Risked' may not, however, be quite the right word, for the most likely shortcoming of a generalization like this one is triviality rather than error. At every stage in the evolution of a scientific speciality, some problems, later resolved by a new theory, are attacked without success. If the claim that crises prepare the way for scientific revolutions means only that novel theories always solve some previously intractable problems, then both the claim and the concept of crisis are empty. A few critics have indeed suggested that this is the case. That challenge can be met only by detailed historical investigation.[21]

The issue for Kuhn was the following. Looking back on a scientific revolution, he argued, one could always find certain problems that the old theory could not solve, as well as scientists who, already before the revolution, had pointed out their existence. Not all of these problems, however, would in the end give rise to a revolution. As long as it was not possible to specify which kinds of problems specifically lead to revolutions, his crisis-claim would remain trivial, saying nothing more than that before revolutions, known unsolved problems exist. His goal was therefore to investigate historically "whether their existence, their nature, the process by which they get recognized, and the response to them can illuminate the timing and the nature of scientific advance".[22]

[19] TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[20] TSK Box 3, Folder 15.

[21] Crisis-lecture, page 1, dated April 21, 1966. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[22] Crisis-lecture, dated November 23, 1965, page 2. TSK Box 3, Folder 14.

To address this issue, Kuhn turned to the development of quantum physics before 1925-1926, the years when the field was radically altered with the appearance of matrix and wave mechanics. The motivation for this was that in a very general sense, quantum physics had been in a constant crisis ever since its first appearance in 1900-1905. The general problem was that the theory combined classical elements with quantum constraints, but that it did not offer any insight into how quantized concepts were related to their classical counterparts. This rendered the theory "[t]horoughly unreasonable[, and h]ard to visualize",[23] and it led to quite a few conceptual paradoxes and unclarities, which had been pointed out quite often, in particular by Einstein and Schrödinger. Phrased in this way, the field clearly displayed what Kuhn had described as a trivial sense of crisis: there were certain issues that the theory could not solve, and these problems had been pointed out repeatedly by certain people. In this sense, the crisis-concept was not very useful, since "a crisis that's coextensive with the life of the theory that gives rise to it [...] is not a useful element in historical analysis" (or as he added in handwriting, 'crisis in perpetuity is no crisis at all').[24]

According to Kuhn, however, there was much more that we could learn from the development of quantum theory before 1925. His starting point was the observation that, at the time, there were really three different attitudes within the physics community concerning the relationship between classical mechanics and the quantum. First, there was the large majority of physicists. They had heard of these issues, but "[a]ssumed the problems would be resolved, probably classically, by someone else and went on with their own work meanwhile". This group consisted at least until 1920 of "almost all physicists in United States, France, and England, [...] and most of the older physicists in Germany".[25]

The two other groups, on the other hand, were very concerned with the problems that plagued quantum theory. The distinction between them, Kuhn claimed, primarily concerned how they dealt with these problems. One group, consisting mainly of Einstein and Schrödinger, was convinced that it was first necessary to clear up the fundamental conceptual paradoxes. On the basis of a citation-analysis of their work,[26] Kuhn showed that in this way, they did not contribute very much to the theory's development before 1924:

[23]Crisis-lecture dated November 23, 1965, page 6. TSK Box 3, Folder 14.
[24]Crisis-lecture, dated November 23, 1965, page 3. TKS Box 3, Folder 14.
[25]Crisis-lecture, dated November 23, 1965, pages 6-7. TSK Box 3, Folder 14.
[26]Crisis-lecture, dated November 23, 1965, page 10. TSK Box 3, Folder 14.

> Either like Schrödinger they do almost nothing in the new field and express considerable discontent with it[, or] like Einstein they direct their attention exclusively to the paradoxes at the base of the field and do work that, though ultimately quite important after the field has been developed, is for a very long time almost entirely ignored.[27]

The third group included, among others, Planck, Bohr, Born, Sommerfeld, and Ehrenfest. They were "thoroughly aware of the basic conflicts within the theories they work with, [...] [b]ut rather than trying to eliminate the conflicts, they [tried] to learn how to live with them and go on doing physics".[28] They did this by extending quantum theory as much as possible. In this they were very successful: they formulated a complete explanation of the structure of the periodic table; they accounted for many spectral and X-ray phenomena; and they developed very detailed and precise quantized models of the atom.[29]

These successes convinced the members of the third group that the theory would eventually also be able to solve long unsolved puzzles. Around 1922, however, this conviction started to change, as a consequence of at least three specific problems. The first concerned the question how to model the helium-atom. This had been an open question ever since Bohr (1913) had successfully developed a quantized atomic model for hydrogen. The general belief was that the helium-case could not be much more difficult, since besides hydrogen it was the simplest atom. While no attempt since then had been quite successful, it was believed for a long time that more complex theoretical models would do the trick. This conviction started to change, according to Kuhn, with John van Vleck's 'The Dilemma of the Helium Atom' (1922). He showed that none of the available models were in agreement with experimental results, and suggested that this "could be resolved only by some radical modification of the conventional quantum theory of atomic structure".[30] Soon, van Vleck was joined by Hendrik Kramers (1923) and Heisenberg and Born (1923), who equally well argued that the problem necessitated giving up central aspects of the quantum theory. In this way "[t]he

[27] Crisis-lecture, dated November 23, 1965, page 9. TSK Box 3, Folder 14.

[28] Crisis-lecture, dated November 23, 1965, page 7. TSK Box 3, Folder 14.

[29] Crisis-lecture, dated November 23, 1965, page 9. TSK Box 3, Folder 14.

[30] Crisis-lecture, dated April 21, 1966, page 4. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

helium dilemma [...] alerted the profession to the possibility of failure in regions where success had previously seemed only a matter of time".[31]

While the work by van Vleck, Kramers, Heisenberg and Born indicated how fundamental the issue was, it did not suggest any solution. This was rather done by work on two other unresolved problems: the anomalous Zeeman effect, i.e. the fact that spectral lines emitted by certain atoms split in very irregular ways when a magnetic field is applied; and the dispersion problem, which concerns the interaction of atoms with light. As with the helium-problem, certain physicists became convinced over time that these problems could only be resolved by abandoning fundamental principles, such as the conservation of momentum or of energy.[32] In contrast to the helium-issue, however, work on these problems did indicate ways to overcome them. Especially the dispersion problem proved very useful: because it had been so extensively developed in classical physics, it provided clear hints about how to reconceptualize then existing quantized atomic model. This soon led first to the Bohr-Kramers-Slater theory, which radically reconceptualized Bohr's atomic model in terms of virtual oscillators, and then to the formulation of matrix mechanics by Heisenberg, Born and Jordan.[33] With this, the crisis was at its end, according to Kuhn. He argued for this by means of a quantitative study of the number of people publishing on these problems (for an example, see the figure for the helium-problem below). This analysis showed that there was a sudden peak in research on helium in 1922-1923, following Van Vleck's dilemma-paper. This peak soon diminished when it became clear that this problem in itself could not provide a solution. Attention then shifted towards the anomalous Zeeman effect and dispersion, and this research really gained momentum when the Bohr-Kramers-Slater theory showed that progress was perhaps possible. Once matrix mechanics had been developed, interest in the helium-problem then returned, to see whether the new framework could provide an answer to it. Such quantitative analyses could provide very valuable insight into moments of crisis, according to Kuhn:

[31]Crisis-lecture, dated April 21, 1966, page 4. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[32]Crisis-lecture, dated April 21, 1966, page 8. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[33]Crisis-lecture, dated April 21, 1966, page 8. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

> If the crisis of the old quantum theory did change the profession's sense of the problem-structure of its field, then that change should show in a variety of ways, some of them less vulnerable to the bias of the historian than those I have so far employed. One of these would be the distribution of the community's research effort, and I have therefore attempted to count, for each year between 1918 and 1926, the total number of men publishing on the three problems principally discussed in my text: helium, the anomalous Zeeman effect, and dispersion.[34]

This study of the old quantum theory, Kuhn concluded, could teach us several things about crises. First, it suggested that such crises could be traced in a historical-sociological way, by counting for example the number of people publishing on specific topics throughout a certain period. Second, it showed that a crisis could consist of different stages: in the case of the old quantum theory, it consisted of "three problems that changed first [the scientists'] attitude [of confidence], then the structure of research, and finally the quantum theory itself".[35] Third, it indicated that different groups of scientists within the same community could have differing attitudes to a crisis. Fourth, it showed how crises could be very creative moments: by "necessitating and licensing a new range for permissible conceptual experimentation", and by "[bringing] to the surface essential conceptual ingredients [from the old theory] which [are] incorporated into the new", they give rise to new situations which are such that "the combinatorial possibilities available to the creative imagination are often very different [...] from what they were before".[36] And finally, it convinced him that crises do not arise from, nor get resolved by, focusing on fundamental, conceptual issues, as Einstein and Schrödinger had done for a certain time. As Kuhn summarized it in his 1965 lecture:

> - It is often possible, particularly for someone a bit removed from practice in the field, to identify fundamental problems within the theories of a scientific specialty.

[34] Crisis-lecture, dated April 21, 1966, page 8. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[35] Crisis-lecture, dated April 21, 1966, page 2. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.

[36] Crisis-lecture, dated April 17, 1970, page 27. TSK Box 3, Folder 15

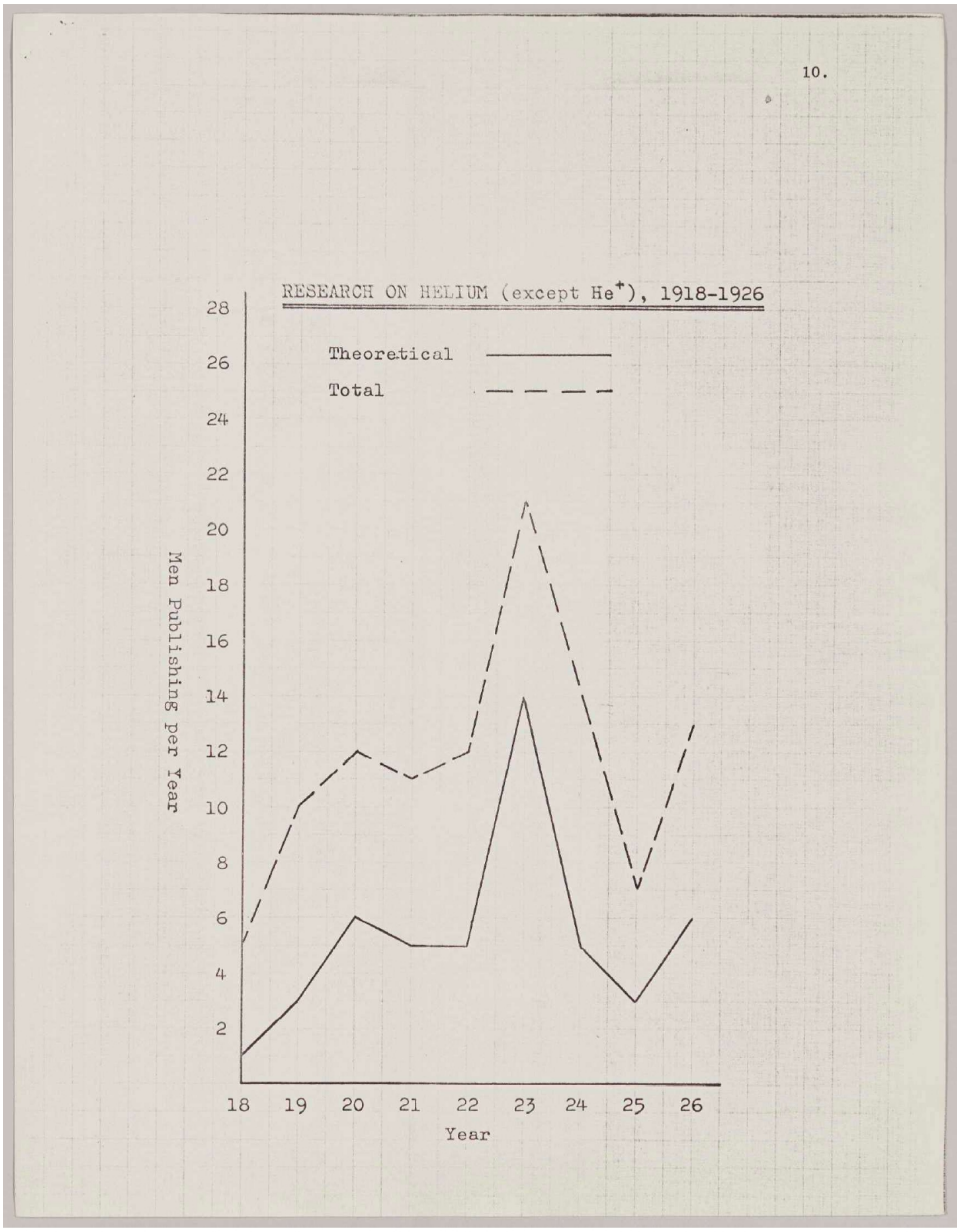


Figure 2: Crisis-lecture, dated April 21, 1966 page 10. TSK Box 5, Folder 'Professional Affiliations and Meetings: American Philosophical Society Meeting, 1966 April'.
[alt=A graph from Kuhn's 1966 Crisis-lecture. It depicts the number of theoretical papers and the total number of publications on the Helium-atom between 1918 and 1926. We see a steady increase in publications between 1918 and 1923. In 1924-1925, the number of publications significantly drops, after which it starts to increase again.]

  - Newton's gravitational theory presented them; so did Maxwell's theory; and so, I think, does quantum mechanical theory today.

- But there is generally no way for a scientist to get hold of them, to work on them. Even if he's concerned with them, believes them real, they usually prove uninformative, unsuggestive of the next direction to be taken in his field.
- That doesn't make them less fundamental, but it may help explain why scientists are so often unsympathetic with the people who raise them and why they insist (on no logical basis) that they lie outside of science, belong to the philosophers.
- They do, of course, quite regularly get solved, but never, I think, by a frontal assault. Instead, they remain until difficulties in a more concrete and more professional area make them relevant [unreadable].[37]

Kuhn continued to work on the crisis-paper, at least until 1972.[38] It was announced several times as forthcoming (Heilbron & Kuhn, 1969, 283, footnote 162; Forman, 1971, 61, footnote 141), and there was, at a certain moment, a concrete plan to turn it into a chapter of a book to be written together with Heilbron and Forman, as part of a series on understudied episodes of the history of science.[39] The paper was, however, never finished. This was, in part, due to the fact that Kuhn did not find the time to completely work out the central argument.[40] At the same time, Kuhn's study of the history of quantum physics brought him to focus on other topics: he started working on the development of Bohr's (1913) atomic model, which led to his (1969) paper with Heilbron; and he started working on Max Planck's black-body radiation papers, which eventually led to his *Black-Body Theory* book (1978). Moreover, at the same time Forman (1968; 1970) had started publishing several

[37]Crisis-lecture, dated November 23, 1965, page 10. TSK Box 3, Folder 14.

[38]Letter from Thomas Kuhn to John Heilbron, dated February 9, 1972. TSK Box 21, Folder 40.

[39]Correspondence between Thomas Kuhn and John Heilbron, dated May, 21-27 1969. TSK Box 21, Folder 40.

[40]Letter from Thomas Kuhn to David Bloor, dated July 14, 1981. TSK Box 10, Folder 2.

papers on the same episode, and Kuhn was unsure about how much overlap would be acceptable.[41]

4 Philosophically Investigating Scientific Crises

4.1 Kuhn Reflects on his Critics

That Kuhn's crisis-lecture never resulted in a published paper does not mean that these historical studies were for nothing. If we look at Kuhn's philosophical writings in the years after the lecture, we see how he used elements of it to further develop some of the concepts he had first introducted in *Structure*. The first occasion where Kuhn referred to his work on the crisis of the quantum theory was at a colloquium organized by Imre Lakatos in July 1965 (eventually published as Lakatos & Musgrave, 1970). The colloquium had started with a paper by Kuhn, followed by several critical reflections, to which Kuhn then in turn replied. In this final reply, Kuhn made use of his historical work to respond to Lakatos.

Lakatos had discussed quantum theory pre-1925 in his lecture titled 'Falsification and the Methodology of Research Programs' (1970). He used it there as an example of how he saw the development of scientific research programs. Such a program, according to Lakatos (1970, 133), tries to protect its hard core, i.e. its central tenets, by surrounding it with a protective belt, which consists of auxiliary hypotheses that address problems threatening the core. A research program is progressive as long as these auxiliary hypotheses lead to new insights beyond the problems addressed. It becomes degenerative when the auxiliary hypotheses introduced are purely ad hoc, and do not lead to any new insights. What Kuhn took issue with was Lakatos' characterization of the quantum theory around 1925 as a degenerative research program. Lakatos had described it as follows:

> The programme lagged behind the discovery of 'facts'. Undigested anomalies swamped the field. With ever more sterile inconsistencies and ever more *ad hoc* hypotheses, the degenerating phase of the research programme had set in: it started – to use one of Popper's favourite phrases – 'to lose its empirical character'. [...] A rival research programme soon appeared: wave

[41] Letter from Thomas Kuhn to John Heilbron, dated March 30, 1970. TSK Box 21, Folder 40.

> mechanics. Not only did the new programme, even in its first version (de Broglie, 1924), explain Planck's and Bohr's quantum conditions; it also led to an exciting new fact, to the Davisson-Germer experiment. In its later, ever more sophisticated versions it offered solutions to problems which had been completely out of the reach of Bohr's research programme, and explained the *ad hoc* later theories of Bohr's research programme by theories satisfying high methodological standards. (Lakatos, 1970, 154)

In his reply, Kuhn (1970, 256-259) argued that the Bohr programme around 1925 was anything but degenerative. What his historical studies showed, rather, was that "[t]o those who were experiencing the crisis, two of the three problems which had provoked it proved immensely informative: dispersion and the anomalous Zeeman effect" (Kuhn, 1970, 258). It was the work on these specific problems that, around 1925, gave rise to matrix mechanics, and in this way one can see how this case was a "clear, detailed, and cogent example of the creative functions of normal science and crisis" (Kuhn, 1970, 258). The same could be said, Kuhn continued, about the development of wave mechanics. On Lakatos' account, this "crisis-resolving innovation [appeared] like a magician pulling a rabbit from a hat" (Kuhn, 1970, 258). For Kuhn, on the other hand, these developments were part of a response to a different crisis: "[t]he wave equation was not a response to the crisis which began in 1922 but to the one which dates from Planck's work in 1900 and on which most physicists had turned their backs after 1911. If Einstein had not tenaciously refused to set aside his deep dissatisfaction with the fundamental inconsistencies of the old quantum theory [...] the wave equation would not have emerged when and as it did" (Kuhn, 1970, 258-259).

In this case as well, however, Einstein's stubborness only gave rise to a crises when he and others were able to connect his concerns to a particular concrete problem, namely how to reconcile Planck's law with the Compton effect. As such, for Kuhn both the Bohr- and the Einstein-programme illustrated how crises played a productive and creative role in scientific change. It was only by working on these seemingly unresolvable problems – dispersion and the anomalous Zeeman effect for the Bohr-tradition, the Compton effect for Einstein – that scientists could obtain information that would, eventually, give rise to theories that could resolve these problems. As such, even in their final phase, these research programs were not degenerative, and solutions to such crises were definitely not to be expected from outside of them, like a

rabbit out of a magician's hat. Rather, both periods of normal science and of crisis have a clearly creative function within scientific practice, a point that Kuhn also made explicitly in his crisis-lecture (see page 16).

4.2 Postscript to *Structure*

Kuhn again referred to his historical work on the crisis of the quantum theory in the postscript of the second edition of *Structure* (1969, third edition 1996 used here). Building on a paper given at the 1965 Colloquium by Margaret Masterman (1970), Kuhn there pointed out that the concept 'paradigm' had two quite different uses in *Structure*: a global use, which he would henceforth call a 'disciplinary matrix'; and a more concrete, local use, which he would denote as an 'exemplar'. A disciplinary matrix, on this view, consisted of different elements shared among its adherents: symbolic generalizations, metaphysical commitments, models, etc. One of the items of such a disciplinary matrix could also be values. One significant aspect of values, according to Kuhn, was that scientists could very well share them while differing about when and how to apply them:

> To a greater extent than other sorts of components of the disciplinary matrix, values may be shared by men who differ in their application. Judgments of accuracy are relatively, though not entirely, stable from one time to another and from one member to another in a particular group. But judgments of simplicity, consistency, plausibility, and so on often vary greatly from individual to individual. What was for Einstein an insupportable inconsistency in the old quantum theory, one that rendered the pursuit of normal science impossible, was for Bohr and others a difficulty that could be expected to work itself out by normal means. (Kuhn, 1996, 185)

Here, Einstein's and Bohr's reactions to the old quantum theory's fundamental inconsistencies thus served Kuhn to further spell out the role of values in science. He used it, more specifically, to account for how scientists working in the same disciplinary matrix can differ profoundly on whether a specific issue constitutes a crisis, and consequently on how to proceed further. This led him, among other things, to the claim that disciplinary matrices in some way guide scientists, but that those matrices should not be seen as collections

of rules that scientists are obliged to follow. They should rather be seen as a collection of shared values, which can be interpreted and applied in different ways. As he put it:

> The points at which values must be applied are invariably those at which risks must be taken. Most anomalies are resolved by normal means; most proposals for new theories prove to be wrong. If all members of a community responded to each anomaly as a source of crisis or embraced each new theory advanced by a colleague, science would cease. If, on the other hand, no one reacted to anomalies or to brand-new revolutions in high-risk ways, there would be few or no revolutions. In matters like these the resort to shared values rather than to shared rules governing individual choice may be the community's way of distributing risk and assuring the long-term success of its enterprise. (Kuhn, 1996, 186)

In this way, Kuhn relied on his historical work to further elaborate several ideas he had originally introduced in *Structure*. In particular, he used it to elaborate in more detail how scientists can differ significantly over whether a specific problem constitutes a crisis: depending on how they interpret and apply specific values, some see a specific problem as fundamental, while others see it as a mere conceptual issue that does not hinder progress. In this way, he further elaborated his *Structure*-suggestions that interpretive differences are possible among adherents of the same paradigm, and that paradigms should not be understood in terms of rule-following (Kuhn, 1962, 44). These insights were then in turn further developed in a 1973-lecture titled 'Objectivity, Value Judgment, and Theory Choice', which was eventually published in *The Essential Tension* (Kuhn, 1977, 320-339).

4.3 Replying to Bohm and Second Thoughts

The final instance of Kuhn's use of the crisis-lecture to be discussed here is at a 1969 symposium organized by Frederick Suppe (eventually published as Suppe, 1974). There, Kuhn not only gave a lecture titled 'Second Thoughts on Paradigms' (1974a), in which he further elaborated his ideas concerning disciplinary matrices and exemplars. He also replied to a lecture by David Bohm titled 'Science as Perception-Communication' (1974).

In his lecture, Bohm mainly criticized Kuhn's incommensurability-concept. The problem was, for Bohm, that the concept still suggested that theories could be comparable or similar, even if only in principle. This was problematic because, on his view, theories were not comparable at all: "each theory is itself a whole[; ...] all the terms in such a theory can have their meanings and their criteria of factuality and truth only in the total context of that theory" (Bohm, 1974, 375-376). Kuhn's 'incommensurability', Bohm argued, wrongly emphasized that scientists should still strive to retain aspects of previous theories. This would only lead to confusion, since such striving would lead scientists to ignore significant conceptual differences. This was what had happened in quantum physics, according to Bohm. There, physicists had not really addressed the conceptual issues underlying the different theories in the field – concerning, for example, what constitutes a measurement –, and this had over time led to significant conceptual confusion and different groups that no longer knew how to communicate:

> [B]ecause the experimental conditions are not described explicitly and nevertheless play a key role tacitly, the way in which they are taken into account will depend on largely subliminal and fortuitous choices, which will in general be somewhat different for each person who uses the theory. Therefore, different investigators, beginning in this way with different premises that are not fully relevant to each other, cannot properly communicate. This is actually the situation in the field, which contains a number of sharply separated schools who do not seem to be able to even decide in what ways they agree and in what ways they disagree. (Bohm, 1974, 386)

In his (1974b) reply, Kuhn agreed with Bohm that there were issues with the incommensurability-concept. He disagreed, however, with his specific criticism. Bohm's emphasis on the incomparability of theories, Kuhn argued, turned science into a sort of private activity: each scientist would have their own theory, and rather than trying to communicate with each other, they would constantly highlight how their account differed from other theories. This was both implausible and undesirable. A philosophical analysis should rather start from the fact that "some sort of communication goes on, and we must learn to understand it by making something out of phrases like 'partial communication,' or 'preservation of reference for certain terms, although some of the referential apparatus has itself changed'." (Kuhn, 1974b,

409). This could be seen, for example, in the history of quantum physics, where scientists were still able to communicate and get work done, regardless of their interpretive differences:

> [O]ne of the things I discovered when talking to physicists about the development of quantum mechanics is that even people who studied with Heisenberg, the author of the uncertainty principle, at Leipzig from 1928 on never heard of these problems [concerning the concept of measurement] unless they visited Copenhagen and were exposed to Bohr. They simply did quantum mechanics in the new vocabulary, without any concern for the fact that there were problems that could be raised by people who were bothered about the consistency of the vocabulary, and so on. [...] The situation Bohm describes does exist. But I think it has nothing to do, either historically or, at this time, logically, with the problems of interpretation of the Schrödinger equation. Although I may stand to be corrected, it is my impression that one of the greatest difficulties faced by people who are concerned to revise the Bohr interpretation is that none of the problems that emerge for them makes any contact whatsoever with the technical problems that physics has faced in recent years, and that has created a profound crisis for the profession. (Kuhn, 1974b, 411)

Kuhn's reply to Bohm relied on his work on the history of quantum physics in two ways. First, Kuhn compared Bohm's claim that there were pressing issues regarding the concept of measurement in quantum theory to Einstein's and Schrödinger's position before 1925. Here as well, Kuhn argued, crisis and revolution would not emerge through such conceptual criticisms, but rather through work on technical problems. And just as Schrödinger and Einstein had been seen as too philosophical and detached from the community (see the quote on page 18), here as well Bohm and others who raised conceptual questions about the standard interpretation of the Schrödinger equation were seen as not working on real physical problems. For Bohm to contribute to scientific change, Kuhn argued, it was first of all necessary to connect the issues he raised to concrete physical puzzles, just as Einstein and Schrödinger had eventually done.

Second, here as well Kuhn highlighted how scientists belonging to the same community could differ significantly on interpretive issues. This point

Kuhn had emphasized as well in his postscript to the second edition of *Structure*, where he had phrased it in terms of values (see page 21). Here, Kuhn further elaborated this point by emphasizing how in Leipzig in 1928, quantum physicists were able to communicate up to a point regardless of their interpretive differences. As such, he replied to Bohm, even though there were indeed different schools within the physics community, it still was the case that communication was possible: "[t]here are schools within the physics community and you can pretty well pick out their membership at any given time: individuals communicate relatively fully within the schools but with much more difficulty across school lines" (Kuhn, 1974b, 412). In his 'Second Thoughts on Paradigms' (1974a), Kuhn then elaborated this point about fluency of communication in more detail by means of his 'disciplinary matrices' and 'exemplar'-concepts, where he illustrated it by means of the role played by the Schrödinger equation in different fields within quantum physics (Kuhn, 1974a, 471, footnote 17). As such, we see how, in his contributions to the 1969-symposium as well, Kuhn relied on his work on the history of quantum physics to further elaborate the paradigm-concept he had introduced in *Structure*.

5 Concluding Remarks

In his 1968 lecture 'The Relations between the History and Philosophy of Science', which was eventually published in *The Essential Tension* (1977, 3-20), Kuhn argued that he saw history and philosophy of science as two separate disciplines that could not be practiced at the same time. The primary reason for this was that their methods and aims were very different: while historians of science try to account for singular events by situating them in a historical narrative, philosophers of science aim for universal generalizations by means of more abstract arguments. For this reason, Kuhn thought it better to keep them separate, since "[t]o train a student simultaneously in both would risk depriving him of any discipline at all" (1977, 5). Such disciplinary division should not, however, prevent active discource between the two fields, according to Kuhn, and philosophers of science in particular could benefit from being more attentive to the work of historians (1977, 12). In particular, history of science could provide philosophers not only with data about specific sciences, it could also bring them to look differently at the philosophical concepts they use to describe scientific practice (Kuhn, 1977, 20).

My discussion of Kuhn's crisis-lecture shows that Kuhn himself relied on the output of the *SHQP*-project in this way. He first used it to develop a historical study on quantum theory before 1925-1926. In his philosophical writings, he then relied on this historical work to further elaborate several philosophical concepts: not merely 'crisis' itself, but equally well 'values', 'paradigm', and 'incommensurability'. He used his work on the crisis-lecture more specifically to argue, contra Lakatos, that crises are productive and creative moments in scientific practice; to elaborate how scientists can differ profoundly about what constitutes a crisis because they are working with a different value system; and to argue, against Bohm, that regardless of interpretive differences communication between scientists is still possible.

This study of Kuhn's work on quantum history has also shown that, contrary to what te Heesen (2020, 89; 2022, 14) has argued, Kuhn's work on the *SHQP*-project was not primarily aimed at empirically grounding or confirming *Structure*. According to te Heesen, Kuhn was disappointed with the interviews because the interviewees could not provide the credible and precise memories he had been hoping for. I have argued, however, that Kuhn already knew before the project started that interviewees could provide no such thing, and that he was disappointed rather because the interviews offered very little material in general: most scientists just did not remember much. Because of this, in his historical work after the *SHQP*-project Kuhn primarily relied not on the interviews, but rather on published materials, manuscripts and archival resources, as was the case with his crisis-lecture.

In his crisis-lecture, as we have seen, Kuhn developed several claims about how this specific crisis emerged and ended. One of these claims was that a sense of crisis is not necessarily shared equal among all members of a scientific community. Thus, while Einstein, Bohr, Schrödinger and Born all were convinced that quantum theory was in some kind of crisis around 1922, they differed among each others about what actually constituted this crisis. That Kuhn recognized this variability in how urgent a crisis appears to an individual scientist now allows me to address a specific claim by historian of physics Suman Seth. He has pointed out how Kuhn, in the postscript to the second edition of *Structure*, had significantly changed his philosophical conceptualization of 'crisis': there, "it [had] become a collective comprehension, whereas it had once been an individual experience" (Seth, 2007, 26). The problem with this, according to Seth, is that this new conceptualization of crisis does not fit quantum physics before 1925. As Seth puts it:

> [N]ot all members of the theoretical-physics community recognized the events as part of a crisis. In particular, one of the two great centres for the study of the quantum, Sommerfeld's Munich school, failed to perceive a crisis at all. Nowhere is this more obvious [...] than in Kuhn's own interviews with people who had been participants in the 'revolution' [...]. In spite of leading questions about the existence of a crisis in 1923 and 1924, many of those Kuhn interviewed failed to remember such a sense. (Seth, 2007, 29)

The central issue here, according to Seth, is not primarily that Kuhn used the term 'crisis' in his historical studies of quantum physics before 1925, but rather that his conceptualization of such a crisis prevented him from seeing that a significant part of the community did not perceive any crisis at all. Kuhn's conceptualization of crisis and its relation to community membership prevented him, more specifically, from paying attention to significant differences within a scientific community: "Kuhn's analysis breaks down at what might be called the 'mesoscopic' level that distinguishes between small groups within a community" (Seth, 2007, 49).

The discussion of Kuhn's crisis-lecture and his subsequent use of these materials for philosophical reflection has shown that Kuhn's views were in fact very close to Seth's. Already in the interviews, as the fragment from the Kemble-interview (page 11) shows, Kuhn seems to have been aware that there was a more outspoken sense of crisis in Copenhagen and Göttingen than in Munich or in other places. In the crisis-lecture itself as well, Kuhn explicitly pointed out that different groups within a community could have a very different attitude with regards to e.g. the relationship between classical and quantized mechanical concepts (see page 13). And in his later philosophical work, Kuhn made use of his historical studies to further elaborate, among other things, his views on how different value systems can lead to interpretive differences within one community (see page 21). In particular, Kuhn there elaborated a view according to which it was actually beneficial for a scientific community to harbour such differences, since as he put it, " [i]f all members of a community responded to each anomaly as a source of crisis or embraced each new theory advanced by a colleague, science would cease. If, on the other hand, no one reacted to anomalies or to brand-new revolutions in high-risk ways, there would be few or no revolutions" (Kuhn, 1996, 186, quote discussed on page 22). This indicates that Kuhn, in his

later philosophical work, was explicitly searching for ways to further analyse scientific communities in a way that was very close to Seth's, and that Kuhn did this, in part, on the basis of his historical studies of quantum physics.

In this way, we come to see how Kuhn, in the 1960s and 1970s, made use of his historical work to fill up what he and others perceived as gaps in the schematic sketch offered by *Structure*. I have argued here that he did this by means of the historical work done for his crisis-lecture, in line with how he characterized the relationship between his historical and his philosophical practice both in his 1968-lecture and in his 1991-interview. This suggests that there could be more close alignments between Kuhn's philosophical reflections and his historical work on, for example, the Copernican Revolution, Max Planck and black-body radiation, or Bohr and his atomic model (all these topics are referred to, for example, in his *Road Since Structure* Kuhn, 2000). Whether this is so, is as of yet an open question.

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