

History of Science Society Distinguished Lecture

Thomas S. Kuhn, Earwitness: Interviewing and the Making of a New History of Science

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Abstract: What are the narratives that guide the history of science? Certainly one of the more recent ones is “science in action,” the emphasis on practices and the imperative of studying science as performed. Perhaps not surprisingly, this story itself has a history; it starts in the early 1960s, when the project “Sources for History of Quantum Physics” was established. The main task of Thomas S. Kuhn, John L. Heilbron, and Paul Forman’s work, lasting three years, was to interview the old heroes of quantum mechanics and to archive the spoken word. While giving an account of the project’s history, this essay will focus on the process of interviewing and characterize its wider context. Not only does their approach offer us important insights into the shaping of the persona of the scientist; it also represents an important step toward the post-Kuhnian way of doing history of science.

When Niels Bohr died unexpectedly in Copenhagen in November 1962, it was not only his family that was devastated by the loss. The historians who had been working closely with the physicist to archive his life’s work in the form of letters, essays, and laboratory books were also utterly dismayed—in particular Thomas S. Kuhn, who, together with his colleagues John L. Heilbron and Paul Forman, had arrived from the United States several weeks earlier to interview Bohr and numerous other physicists throughout Europe. Just the night before this unexpected loss, Kuhn was able to conduct a detailed interview with the famous scientist, serving as his primary earwitness. Kuhn was in the midst of preparing for the next interview when news of Bohr’s

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death arrived, leaving him so despondent that he wondered, at least for a couple of days, whether it was even worth continuing with the project. Bohr's death occurred near the beginning of an extraordinarily ambitious undertaking, in which Kuhn intended to record the life stories and memories of physicists who had taken part in the great scientific revolution in quantum physics in the 1920s. It should be noted that at this point, between 1961 and 1964, Kuhn had yet to attain global fame as the author of *The Structure of Scientific Revolutions*. In what follows, I would like to tell the story of this project, which was called "Sources for History of Quantum Physics"—or simply SHQP—a project that nearly failed owing to Bohr's untimely death but then went on to shape the history of science in its recent form like no project before it, building an archive of interviews, original publications, laboratory journals, and letters as an answer to the central question: How does one analyze and tell the history of modern science?

THE HISTORY OF SCIENCE

How should science be remembered? How can knowledge be historicized? These questions are much older than the Kuhnian project. Since the establishment of the research university at the beginning of the nineteenth century, since the differentiation of modern disciplines and, with it, the development of the human sciences, these questions have proliferated, generating many different answers along the way. Above all, it was the ambitious, self-confident natural scientists around the middle of the nineteenth century who could feel in their day-to-day work the rapid transformation of their knowledge and technical foundations. Memorializing what had become obsolete was a way of championing all that had recently been achieved. The experience of progress was a powerful motivator for this yet-to-be-written history; the life of the great man was its genre. Like pearls on a string, the biographies of scientists were strung together, one after another—Kepler, Galileo, Newton, Humboldt, Darwin—as a celebration of the greatest examples in astronomy, physics, and biology. While their stories extended far back in time, their accomplishments were worthy of remembrance. The history of science—understood as the history of natural sciences well into the twenty-first century—was a permanent fixture in the grandiose speeches presented at academic anniversary celebrations, in the formal inaugurations of international conferences, and in the solemn obituaries written for famous colleagues. Around 1900, people began to consider how to professionalize this memory work. What archives should be established in order to make a history of physiology possible? Surely the papers of scientists, not just those of literary luminaries, deserved safekeeping and preservation as national heritage. Fragments of correspondence from late scientists were bought up at auctions; living scientists received requests for their handwritten materials; the first journals dedicated to the history of science, such as *Isis*, were established. These developments, propelled by private collectors and new institutions, were interrupted by World War I, but they did not fundamentally change course. It was only World War II and its aftermath that marked a true turning point. The natural sciences and medicine had contributed substantially to a twofold collapse of civilization. The development of the atomic bomb and the development of the National Socialists' instruments of human destruction had abruptly and fundamentally called into question the modern view of simultaneous progress in both science and morality.

In light of this new situation, the history of the modern natural sciences that was waiting to be told now had to be built on a completely new foundation. It had become impossible to frame questions regarding the newest and most decisive developments in the fields of physics, genetics, or physiology as a celebration. Completely different questions needed to be taken into consideration: How was this knowledge generated and what path led to its gruesome applications? How could this knowledge be cleansed of its political contamination? Was this even still possible? In the 1950s and 1960s, the history of science—or, rather, the few people at universities who understood themselves as historians of science—attempted once more to ask the question: How and

with what sources should science be remembered? This was the moment when memory itself came under scrutiny.

Around the time of Kuhn's project, at the beginning of the 1960s, historians were not the only ones thinking about how to deal adequately with the past. Sociologists were too. Robert Merton reflected on the relationship to the past in a humorous, informed way: "A dwarf, who is perched on the shoulders of giants, sees farther than the giant himself" is an aphorism, one that was at the time attributed to Isaac Newton and whose history Merton attempted to tell. Merton had originally intended to use this quotation in his text to point out that scientists were clearly aware of the collaborative nature of their work.¹ But as he traced this phrase as it meandered through anecdotes, invectives, falsely adopted text passages, and whimsical speculations far earlier than Newton's time, it became clear to Merton how pretentious it had been to invoke this aphorism in ceremonial addresses. After all, it says more about the dwarf at the end of a preliminary scientific development than about the giant on whose shoulders he is perched. Those who repeated the quotation were ostensibly performing a gesture of humility; but at the same time, they were making it unmistakably clear that the continuous accumulation of achievements over time had elevated them to an intellectual superiority the world had not seen before. To stand on the shoulders of giants was to shower oneself in the experience of progress. Toward the end of his book, Merton cites one of the most recent invocations of the aphorism, by the physicist and historian of science Gerald Holton. Holton had argued during a talk he gave in 1961 that developments in physics—indeed, scientific progress in general—had moved so quickly in the twentieth century that the influential men were not only predecessors but, simultaneously, direct teachers and neighbors "in terms of time and tastes . . . : in the sciences, we are now uniquely privileged to sit side-by-side with the giants on whose shoulders we stand." Holton, who had been involved in advising the SHQP project and who had initiated a parallel endeavor for the history of physics in the United States, hence put a new spin on the image. Even if succeeding generations of scientists still stood *symbolically* on the shoulders of their predecessors, now, for the first time in history, they could also stand *physically* next to them. Merton considered Holton to be the last "principal witness"; he was also—as we will see in what follows—one of the most significant in terms of the history of science.² For Holton's attention to giants that were still living, rather than to events long past, meant that a subjective side of science came to the fore, a side that, up to that point, had been reserved for retrospective memoirs and polished autobiographies—namely, the memory of the scientist. While the "shoulders of giants" had previously consisted of discoveries and laws that had been put into writing, for the first time people considered it worthwhile to direct equal attention to the feelings or intuitions communicated verbally or in letters, to the dead ends and mistakes, to the scattered stories and unadorned facts. The giants could be questioned directly by historians. "Memory" no longer denoted the systematic commemoration of that which had been achieved in the past. Rather, it meant that the human, emotional side of scientific work (read: work in physics) now came into the historian's focus—and, with it, the multitudinous perspectives on a single event that together made up "real history." But—some historians of physics might object—the interviews of the SHQP have been consulted less frequently than the project's collection of letters and laboratory books. This is certainly true: as we will see, already in the early interviews there were doubts raised regarding the validity of the recollections and retellings of the events recounted in conversation. From the perspective of a historian of science today, however, these interviews are

¹ Robert K. Merton, *On the Shoulders of Giants: A Shandean Postscript* (New York: Free Press, 1965); and Merton, "The Normative Structure of Science," *Journal of Legal and Political Sociology*, 1942, 115:115–126, on p. 123 (original intention).

² Gerald Holton, "On the Recent Past of Physics," *American Journal of Physics*, 1961, 29:805–810, on p. 807. For Merton's reference to Holton see Merton, *On the Shoulders of Giants*, p. 266.

instructive insofar as they showcase the great hopes with which those involved in the project attempted to record detailed, minute steps of discovery, to treat failures and successes equally, and to chronicle “science in the making” retrospectively. The interviews also make clear how not only the memories of those whom we would today call “eyewitnesses” were brought forth, but also the direct experience of the historian as “earwitness.”

THOMAS S. KUHN AND THE PROJECT

Awareness of the proximity between giants and dwarves was the starting point for Kuhn as he began his work in 1961. By this point he had completed most of the manuscript for *The Structure of Scientific Revolutions* and was writing the introduction. A year later the book—now so famous for its description of perspective changes in the natural sciences, which Kuhn called “paradigm shifts”—was published. Kuhn resolutely argued against a continuous, additive notion of progress, where findings accumulate toward more knowledge. Instead, he made the case for sudden, revolutionary changes. According to this model, scientific development went through periods of what he called normal science, “the activity in which most scientists inevitably spend almost all their time” and during which “the scientific community knows what the world is like.” But then come “extraordinary episodes in which that shift of professional commitment occurs,” the famous “scientific revolutions.” “They are the tradition-shattering complements to the tradition-bound activity of normal science,” seldom “completed by a single man and never overnight.” New ideas emerge when an observed phenomenon no longer fits into the “normal framework” and new methods and concepts need to be developed in order to continue studying that phenomenon. These are phases of crisis: “The transition from the Newtonian to quantum mechanics evoked many debates about both the nature and the standards of physics, some of which still continue. There are people alive today who can remember the similar arguments engendered by Maxwell’s electromagnetic theory and by statistical mechanics.”³ Here it is crucial who builds on the ideas of whom, which ideas can coexist alongside others, or which require dismantling their antecedents.

If we return to our image of giants and dwarves, this would be the moment where the giants are invited to take a seat at the table, where they can be asked about their experiences. Kuhn’s famous book can be read as an indirect appeal for the collection of authentic accounts of a scientific revolution, a comparative inquiry into the countless moments in which science was conducted and transformed. To put it differently: The 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. This, I contend, was one of the reasons (besides the well-known rejection by his philosopher colleagues) why Kuhn interrupted his professorship at the University of California at Berkeley, just as his book was in the midst of being published, and instead devoted himself to the documentation of a recent scientific revolution. For this endeavor, novel at the time for the history of science, Kuhn developed a toolkit—a methodology for a new kind of historiography. Up to this point, the notion of using prepared questions and recording careful answers to trace the transformation of scientific understanding was nonexistent in the history of the natural sciences.

The project, which came to be known by the name “Sources for History of Quantum Physics” and which was funded by the National Science Foundation, can be seen as the first organized effort to secure and collect sources on the history of twentieth-century physics. The project’s initiators—above all John Wheeler, but also Kuhn’s doctoral advisor John van Vleck—and its executors were all trained in physics, worked as physicists, or had just begun work in the history of

³ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, with an introductory essay by Ian Hacking (Chicago: Univ. Chicago Press, 2012), pp. 5–6, 7, 48.

science. Kuhn, who led the project and conducted the majority of the interviews, considered himself a historian and philosopher of science. Deputy project director John L. Heilbron was primarily responsible for preparing the interviews. Paul Forman, finally, was to edit the interviews and prepare documents for their transfer to microfilm. It was up to this trio and their assistants, then, to document those years that were so significant for the field of physics: Which physicists were still living and could be interviewed? Which correspondence could be saved and made accessible for research? The core of the interview project was to make tangible the developmental processes of quantum physics: to ask about the models that were constructed, about the considerations that went into particular calculations, and about the obstacles that were encountered along the way.

The fact that three men were tasked with such a project was no coincidence. Physics, as well as the history of physics, was conducted overwhelmingly by men in the 1950s and 1960s; women appeared only at the margins. But along with Kuhn, Heilbron, and Forman, it is important also to highlight the role of Lini Allen, who accompanied the project for its duration as foreign language secretary and who held the organizational reins in her hand. Allen ultimately became a coauthor of the project's final publication, and it is a credit to the participants that they acknowledged her work on an academic level.⁴ Still, there is no way around the fact that we are talking mostly about men in this project. Both on the level of the project's organization and in physics in the time period the project examined, everything was centered on male physicists and their initiation rites. James Franck reported in one of the interviews that it was always the great men who stood at the center; they were the ones people attentively watched as they made their calculations and whose colloquia were always well attended.⁵

In fact, one must imagine the sites of physics at the beginning of the twentieth century as a map, drawn over the course of the physicists' extensive travels. While Ernest Rutherford researched in Manchester and Cambridge and Niels Bohr in Copenhagen, Albert Einstein had stationed himself during this period in Bern, Prague, Zurich, and Berlin. One would go to Munich to hear Arnold Sommerfeld speak and travel to Göttingen, where Bohr was giving a presentation; and—if possible—one's itinerary should also include a detour to see Max Planck in Berlin. To study physics was to embark on a grand tour, which led one to the centers of research, educated the novice, and imparted to him the spirit of physics research. Kuhn and his collaborators based their project on this map, designed to cultivate cosmopolitanism in the world of physics, and they followed its well-traveled paths, as if in a kind of reenactment. At the center of the project's itinerary in 1962 stood Copenhagen, where the team established headquarters in direct proximity to Niels Bohr. From Copenhagen, they then embarked on shorter journeys to reach the still-living physicists on the western side of the Iron Curtain—Eastern Europe had mostly to be passed over for obvious practical reasons. This stage was about going “into the field,” as Kuhn called it, where interviews would be conducted and documents collected.⁶ In this process, the distanced position of the researcher had to be reconciled with the demands of direct conversation and reverence for Nobel Prize winners. Contact with these dignitaries of physics was typically established through people who could vouch for the credibility of the three young traveling historians of physics—at

⁴ See Thomas S. Kuhn, John L. Heilbron, Paul Forman, and Lini Allen, *Sources for History of Quantum Physics: An Inventory and Report* (Philadelphia: American Philosophical Society, 1967).

⁵ Interview of Thomas S. Kuhn with James Franck (and M. Mayer, H. Sponer), 9 July 1962, Niels Bohr Library and Archives, American Institute of Physics, College Park, Maryland, USA (hereafter AIP), <https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4609-1> (accessed Aug. 2019).

⁶ T. S. Kuhn to Gerald Holton, 23 May 1961, Archive for the History of Quantum Physics (hereafter AHQP), Box 17–530.1 AR 2.5, American Philosophical Society, Philadelphia, Pennsylvania, USA (hereafter APS).

the time, Kuhn was thirty-nine, Heilbron twenty-seven, and Forman twenty-four years old. Established physicists on the project's scientific advisory board acted as further guarantors for the historians by providing letters of recommendation for them.⁷ The travelers on this most famous grand tour for the history of physics had made it their goal to interview and document the giants of the field. That these giants were also witnesses to and protagonists of a scientific revolution was particularly welcome to Kuhn.

In the spring of 1962 Kuhn was hoping to procure testimonials—or, as he called them, “authentic information”—by thoroughly interrogating individuals who had participated in the revolutions in physics at the outset of the twentieth century, those he referred to as “living sources.”⁸ In February 1962 the project sent out a request to 180 physicists, entitled “An Appeal for Letters, Manuscripts and Recollections.” Little was known, the appeal explained, about the development of theoretical physics; little “of the battles of minds in the decisive period has been described.” Such documents were valuable “for analyzing the scientific method in action.” “Only those involved know who influenced whom and how and why in the formulation of quantum physics.” Letters, manuscripts, meeting minutes, photographs, and films were to be collected, as well as “recollections of seminars where critical steps were discussed; of moments at which an important concept emerged; and of occasions when the outlook of one investigator was dramatically changed by another.” “It is already nearly too late to undertake a project with these aims. Its success will depend upon the active cooperation of the physics profession.”⁹ Even if no one knew yet exactly what memory-collecting interviews looked like, there was a consensus that they should *not* resemble journalistic interviews, which were held to be superficially conducted and most likely inaccurately rendered. John Wheeler had a strong opinion on this subject: “Bohr telling history of physics to a LIFE reporter—or Bohr telling physics to a physics graduate student, even a very good physics graduate student—is totally different from Bohr telling history to George Uhlenbeck.”¹⁰ Informed men should be the ones interviewing famous men.

Thus the mission consisted of *surveying* (literature), *compiling* (data), *comparing* (biographies and institutions), and *selecting and designing* (interviews).¹¹ The project collaborators wrote to libraries, sent off questionnaires, and drafted list after list: those who had already died; those who were still alive but should be consulted as soon as possible; and those still young enough that their future as interview partners seemed secure. Steeled by this intensive preparation, they traveled with their tape recorders to their interview partners and conducted hours upon hours of discussion, interrupted only by the occasional tea break and meal. Chiefly in the years 1962 and 1963, a grand total of 175 interviews with 100 individuals were conducted. The amount of transcription work was, accordingly, overwhelming, and Forman likely spent most of his time editing the raw transcripts and archiving the supplemental material. Heilbron prepared for the interviews by meticulously compiling biographical and institutional data. Both Kuhn and Heilbron established contact with the interview partners and conducted the actual interviews.

⁷ The members of the advisory board were John A. Wheeler, Richard H. Shryock, George W. Comer, Karl K. Darrow, S. A. Goudsmit, Gerald J. Holton, Henry Allen Moe, George E. Uhlenbeck, and John H. Williams.

⁸ Kuhn, *Structure of Scientific Revolutions* (cit. n. 3), p. 136 (“authentic information”); and “Replies to Priority List,” 16 Oct. 1961, Box 22–530.1 AR 2.5, AHQP, APS (“living sources”).

⁹ “An Appeal for Letters, Manuscripts and Recollections,” in “Description of Project,” Nov. 1961, Box 22–530.1 AR 2.5, AHQP, APS. See also, e.g., letter to members of the Joint Committee, in “Progress Report,” 26 Apr. 1962, p. 1, Box 22–530.1 AR 2.5, AHQP, APS (request to 180 physicists).

¹⁰ John Wheeler to Ad Hoc Committee, 9 Feb. 1961, p. 3, John Wheeler Papers (hereafter **Wheeler Papers**), Series I: Princeton Files, Sources for History of Quantum Physics, Box 26: B: W 564: “Aug. 1960–Apr. 1961,” APS.

¹¹ See Kuhn *et al.*, *Sources for History of Quantum Physics* (cit. n. 4), pp. 2–3.

In the summer of 1963 the group returned to Berkeley.¹² In the remaining year of the project's financing, the final interviews were conducted, the materials organized, and the various interviews with accompanying papers collated into an accessible microfilm archive, which was initially made available to libraries in Berkeley, Copenhagen, and Philadelphia for further consultation. The project ended in 1964, and three years later the group published a report that described the content and structure of the archive.¹³

THE INTERVIEW AND THE ORAL HISTORY

In the 1950s and 1960s, the interview enjoyed a clear and uncritical function (compared to what we are used to today): journalistic interviews served to substantiate reports in the media, and questioning for research purposes was part of the foundation of any ambitious social scientific study. Interviews were, in every respect, state of the art for intellectual life. What was the significance of orality, and what was the source of its relevance for journalism and research during this period? While the media historian Marshall McLuhan did not offer an answer at the time, his book *The Gutenberg Galaxy*, published in the same year as Kuhn's *Structure*, points to some possible ways to approach these questions. His central argument was that the establishment of book printing marked the definitive transition from an oral culture to a visual one, from the diction of the spoken word to that of the two-dimensional and visually structured printed word. From this transition, "typographic man" had been born. McLuhan's book attends to the acclimatization of the human sensory apparatus in connection to the printing press and paper. Yet the Gutenberg Galaxy, which had been developing and consolidating since the fifteenth century, was now itself, according to McLuhan, in the midst of dissolution: the homogeneous textual space of the individual page and the book as a whole was beginning to break apart.¹⁴

McLuhan refers here to a situation that was in full swing by 1962: with the introduction of radios, tape recordings, and, finally, televisions and computers—in short, electric and electronic media—typographic visibility was fading into the background, giving way to a new visual and oral culture. "And today in the electronic age we can understand why there should be a great diminishing of the special qualities of print culture, and a revival of oral and auditory values in verbal organization."¹⁵ McLuhan did not delve deeper into the kind of new relationship humans could have to orality. But an important indication for the accuracy of his diagnosis is the rise of the interview.

But which interview? Notes written by Wheeler during the conception of the SHQP project make it clear that he was interested in the literary magazine *Paris Review*, which at that time stood at the forefront of intellectual discussions with its new format of author interviews.¹⁶ Young critics questioned renowned authors on their work, with the goal of uncovering the genesis of their literary oeuvre. It was not the celebrity that stood in the foreground, but the person—not the polished formation of a book, but the "dark beginnings" of writing. The interview series would later be presented under the title "Writers at Work," and the idea was to give a candid, unvarnished account of the highs and lows of the creative process. It was not only John Wheeler, however, who was aware of interviews as a genre, but also John van Vleck: he had in fact participated

¹² "Second Year-End Report," 28 Oct. 1963, p. 1, Box 22–530.1 AR 2.5, AHQP, APS.

¹³ See Kuhn *et al.*, *Sources for History of Quantum Physics* (cit. n. 4).

¹⁴ Marshall McLuhan, *The Gutenberg Galaxy: The Making of Typographic Man* (London: Routledge & Kegan Paul, 1962), pp. 253–255.

¹⁵ *Ibid.*, p. 108.

¹⁶ Wheeler Papers, Series I: Princeton Files, Sources for History of Quantum Physics, Box 26: B: W 564: "Aug. 1960–Apr. 1961," APS.

in the study “The Making of a Scientist” conducted by the psychologist Anne Roe, who analyzed the relationship between one’s personality and one’s choice of profession. To this end, she conducted sixty-four biographical interviews with a group of scientists selected from the social sciences, biology, and physics and complemented these with several clinical studies, such as the Rorschach test.¹⁷

Interviews in the late 1950s served both as publication material for print media and as a data foundation for social science studies in the broadest sense of the term. In both cases, orality guaranteed the authenticity of the description of the working process by virtue of its subjectivity. Whether Kuhn ever studied such printed interviews must remain a matter for speculation. What is clear, though, is that these interviews belonged to the same experiential space as the SHQP project and can therefore help us understand its development against the backdrop of “new orality.” Kuhn, in any case, was evidently interested in oral history, if only for a short period of time. It was very likely Richard Shryock, the chief librarian at the American Philosophical Society, who first directed Kuhn to oral history and to one of its most prominent representatives at the time, Saul Benison in New York. Benison had worked since 1953 as Research Associate at Columbia University’s Oral History Research Office. The office was established by the journalist Allan Nevins in 1948, and it used the biographical interview as a key method for historical research. Nevins retired as emeritus in 1958, and Kuhn found a similarly experienced oral historian in Benison, a student of Nevins’s. Benison was also a useful contact, given that his own biographical research focused on medical physicians and scientists. The two met and corresponded with one another. Benison repeatedly stressed the necessity of extensive preparations for questioning. He recommended using primary and secondary sources, not only to prepare the interviewer but also to refresh the memories of the interviewee. The “memoirs” that emerged from this process were the goal of oral history. Biography after biography had to be collected in this manner in order to work against the oblivion of time. In Benison’s account of the physician and virologist Tom Rivers, which he had worked on while advising Kuhn and which was published several years later, he gave a detailed description of his techniques. At the conclusion of each interview, Benison would prepare Rivers with the questions for their upcoming session as well as with written material “that might serve to refresh his memory.” Rivers, in turn, would give Benison references to relevant literature. During the interview itself, this material stood at the ready, so that each person could draw on it when asking or answering a question. In some cases, Rivers cited from the documents at hand or requested that the material later find its way into the memoir. When an interview on a specific subject was complete, Rivers received the transcript for corrections.¹⁸ Oral history, as it was understood in the early 1960s, was a constant piecing together of various materials into a tapestry of sources, whose fibers the historian had to weave together into an orderly form. The justification for this practice was not only that documents constantly went missing over time but, more important, that since the recent revolution in communication they were no longer even being generated. At the very moment when technology had made it possible to record on an altogether grander scale, actual records dwindled. Telephones and airplanes might encourage communication, but not on paper. This in fact was the reason for oral history’s central importance.¹⁹ And it was this argument, which would become commonplace in the coming years, that functioned as leverage to initiate and finance interviews with prominent individuals who had

¹⁷ Anne Roe, *The Making of a Scientist* (Westport, Conn.: Greenwood, 1953).

¹⁸ Saul Benison, “Oral History and Manuscript Collecting,” *Isis*, 1962, 53:113–117, esp. pp. 114–115; and Benison, *Tom Rivers: Reflections on a Life in Medicine and Science* (Cambridge, Mass.: MIT Press, 1967), pp. xii (quotation), xiii.

¹⁹ Benison, “Oral History and Manuscript Collecting,” pp. 113–114.

taken part in key developments in society and science: former U.S. presidents, company founders, and professors. Early oral history was hence not a story of small people, granting a “voice” to those who couldn’t otherwise express themselves. Rather, it was—initially—a history of individuals whose memories were considered absolutely worthy of safekeeping, given their perceived importance and the belief that their decisive thoughts and actions had not yet been adequately archived.²⁰ This kind of oral history was understood as a complement, as a process of balancing out the deficits in sources for a new kind of contemporary history.

So: What could have seemed more promising for the author of *Structure* than the no doubt labor intensive, yet worthwhile, effort to conserve the past through individual lives and thereby explore the watershed moments of scientific revolutions in greater depth?

And yet, if we look back today at the remaining papers and records of the project, they attest to a growing disappointment around the form of the interview. A progress report from April 1962 details some of the first interviews: “Together these interviews total about 30 hours of tape or about 500 double-spaced pages of draft transcript. All the tapes include at least some new and valuable historical information, though its average density is not, and probably never will be, extremely high.” “There is an immense variation in the useful memory of even the most willing subjects. As a result, it proves important to try out a number of people but to commit ourselves for much time in advance to none.”²¹ Where nothing can be expected, nothing should be invested. The interview that Kuhn and Uhlenbeck conducted with the physicist and chemist Peter Debye at the Rockefeller Institute on a May afternoon in 1962 offers a concentrated example of the sense of scant results that must have prevailed among the project’s organizers. In the following transcription of their conversation, Debye is being asked about his time in Germany, in particular about the time he spent with the theoretical physicist Arnold Sommerfeld in Munich. At the crux of the exchange is the question about when exactly the first intensive discussions regarding the theory of relativity took place.

UHL [Uhlenbeck]: Now the relativity theory of course not yet so far.

D [Debye]: No, no, no. So he [i.e., Sommerfeld] was very much interested in it, and accepted it right away, you see, but that was all, so to say.

TSK [Kuhn]: You didn’t have discussions then, that you remember, of relativity theory as early as the 1905 paper of Einstein?

D: Oh yes, when the paper came out we had discussions about it, but that was just to say, “Well this is all right.” There was nothing to it.

UHL: Nothing about the paradoxes and so forth?

D: No, no, well, the few paradoxes—we went over that very easily. This was not like the paradoxes later with Einstein and quantum theory.

UHL: No, no, but I mean some of the simultaneity paradoxes.

D: Yes, yes, well we accepted that rather easily. It was not hard to accept it. After all we had Lorentz’s transformation, and then it was just a question of philosophical interpretation of the Lorentz transformation. That was all.

TSK: Was Sommerfeld pretty well convinced already that there was no need for a mechanical aether or anything of this sort?

²⁰ See the instructive article by Lutz Niethammer, who gives an early account of the Kuhnian project: Lutz Niethammer, “Oral History in USA: Zur Entwicklung und Problematik diachroner Befragungen,” *Archiv für Sozialgeschichte*, 1978, 18:457–501. For an overview of early oral history see Rebecca Sharpless, “The History of Oral History,” in *Handbook of Oral History*, ed. Thomas L. Charlton, Lois E. Myers, and Sharpless (Lanham, Md.: Altamira, 2006), pp. 19–32.

²¹ “Progress Report,” 26 Apr. 1962, p. 3, Box 22–530.1 AR 2.5, AHQP, APS.

D: Oh yes, oh yes, right from the beginning. We didn't bother about that. I had enough of that in this course of Wöllner, you know. No no, that was done approximately like as if the equations came from heaven, you see.²²

The metaphor of equations simply falling from the sky hardly captured the more precise descriptions that Kuhn and Uhlenbeck were really after. There was no account of a moment of confusion, no memory of long conversations that might have been able to convey the arguments leveled for or against the equation at the time. Instead, it was a narrative of insights and findings that appeared consecutively, one after the other. By the spring of 1962 the interviewers had spoken for a total of 4 hours and 30 minutes with Debye, and this is what Kuhn had to say about the interview: "Professor Debye's remarks on the reception of the Bohr atom are quite explicitly a reconstruction of how things must have been or ought to have been. As such, they are entirely lacking in the sort of circumstantial detail which would make them either useful or entirely credible." In the "First Year-End Report" he therefore quoted from the Debye interview to draw attention, in a nutshell, to the problem of missing "circumstantial details": no watershed moments at a kitchen table, no sudden realizations in the course of a hike, and no descriptions of the hard nights of work that were rewarded with new insights by dawn. Even if Kuhn repeatedly emphasized in the report that there were a handful of good interviews, later that same year he made it clear that he no longer found this sort of work to be a worthwhile pursuit. In a 1963 letter to George Uhlenbeck, Kuhn wrote: "To put first the point about which I am most nearly certain, I would not recommend that the Committee plan to continue this project on its present scale." The "Second Year-End Report" made the same point: "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."²³

Retrospectively, in his own biographical interview, Kuhn went a step further, commenting: "Interviewing was frustrating as hell."²⁴ And this was not just because Niels Bohr died too soon.

CONCLUSION

So how do we reconcile the great impact of the project I outlined in my opening paragraphs and its failure, as described in the frustrated self-assessments of its protagonist? First, it should be noted, Kuhn's comments were directed at the anecdote-filled interviews, only a few of which he actually considered successful. His exasperation did not extend to the parallel project of collecting letters, notes, preprints, and publications from the interview partners. This material—such is the tenor of much of the critical reception directly following the project—makes up the actual accomplishment of the three-year endeavor, even if most of the time was invested in the interviews and their transcription.

The collected documents were copied and hence could be compiled into a concentrated archive of quantum physics; they offered a kind of data tapestry, so to speak—albeit one much

²² "First Year-End Report," 8 Aug. 1962, Attachment III, p. 2, Box 22–530.1 AR 2.5, AHQP, APS.

²³ "Progress Report," 26 Apr. 1962, Attachment II, p. 1, Box 22–530.1 AR 2.5, AHQP, APS (time spent with Debye); "First Year-End Report," 8 Aug. 1962, p. 3, Box 22–530.1 AR 2.5, AHQP, APS; "Letter to Geo. E. Uhlenbeck," 18 Oct. 1963, p. 1, Box 22–530.1 AR 2.5, AHQP, APS; and "Second Year-End Report," 28 Oct. 1963, p. 5, Box 22–530.1 AR 2.5, AHQP, APS.

²⁴ Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi, "A Discussion with Thomas S. Kuhn," in Thomas S. Kuhn, *The Road since "Structure": Philosophical Essays, 1970–1993, with an Autobiographical Interview*, ed. James Conant and John Haugeland (Chicago: Univ. Chicago Press, 2000), pp. 254–323, on p. 303.

smaller than the group had originally planned. The SHQP, moreover, represents the first time that the interview and the historian as interviewer had taken hold within the gradually professionalizing field of the history of science. A new kind of source thus entered the archive, and a new self-conception of the historian emerged from the process. Interviews were much more complex than the group had initially assumed. At the time of the project, the interview vacillated between at least three different functions. First, interviews were perceived as *records of original tones, or unique acoustic events*, which gave an authentic impression of a time or of a person. The 1950s concept of “contemporary witness”—in German, “*Zeitzeuge*”—was based on exactly this promise of authenticity. Aside from offering the occasion for narration, Kuhn, as questioner, receded into the background to give way to the individual questioned, such as Peter Debeye. Second, interviews could also be understood to have an explanatory function, as reductions of the most complex subject matter down to the spoken word; they were conversations, in which the interviewer’s questions solicited solutions from the interviewee. After all, the interviewers were sitting down with Nobel Prize winners and hence were entering into a contemporary variation on the disciple-master dialogue. And, finally, there were interviews that followed an objective known only to the questioner and in which the individual questioned produced material that was subject to subsequent analysis. The interviews served, in this case, for the *collection of information*—information that would make sense only in comparison with other data at a later point in time. Recording authenticity, explaining issues, and collecting information: with each conversational goal, the relationship between the interviewer and interviewee shifted. The three functions were constantly intermixed. Sometimes the questioners were by far the more knowledgeable parties, while at other times the hierarchies distinguishing Nobel Prize winners and project managers could be felt much more clearly; in other cases, the persistent questioner was confronted with the exasperation of a source who simply could no longer remember. Above all, one thing becomes clear. These interviews did not only produce a new source, one that recorded memory. They also called forth the experience of the questioner, as much as they summoned the memories of the questioned. Kuhn’s comments on going “into the field” and on the “hell” of interviewing marked a moment of conscious involvement, a convergence of (physics) research and the people who did it with an intensified awareness of “science as practice.” “Science as practice” refers here not only to the past events but equally to Kuhn’s own experience of fieldwork. To put it succinctly: the project made visible a historiography that was oriented toward practice.

What does all this mean for the historian today? And what does this mean for the history of science? Allow me to conclude with two observations that may at first seem banal but that I would like to summarize into two concomitant final appeals.

Interviews have a history. We have on our hands a genre whose various formats and narratives must be studied in their specific historical contexts. Each interview deserves to undergo a meticulous excavation of the context from which it emerges, in order to elucidate the specific procedures it uses to establish truth. Robert Merton and Marshall McLuhan, the *Paris Review* and oral history, make up a kind of resonance chamber in which Kuhn and SHQP must be understood. The history of the interview once again makes clear how crucial media history is—in this case, the recording of sound and its transcription with the help of tape recorders and microfilm. The history of science—and this my first appeal—cannot be done without media history, and the history of the interview can offer an array of central insights to support this position.

Interviews have (almost) no theory. Rather, interviews orient themselves alongside examples of best practice. Lines of questioning can follow certain guidelines, but they are always linked to practical experience—back then and today. Therefore, it is all the more important to look into the actual practice of conducting interviews, since only then is it possible to understand the objectives an interview aimed to fulfill. From this perspective, the history of SHQP is also relevant for today’s interview practice. Lutz Niethammer, the German doyen of oral history, remarked as

early as 1978 that Kuhn's project "was very carefully prepared."²⁵ And in fact it is possible to find among the writings of the project leaders suggestions on how to take an assertive position of data frugality, as well as inspiration on how to resist a particular medium's promise of vast storage capacities.²⁶ The genesis of the project and its attention to best practice furthermore show that the ostensible antagonism between "big picture" and "micro" history, between "longue durée" and "short term," is not necessary: the project—without having actively reflected on these matters—unifies both and shows us in its constant fluctuation what is lost and what is gained in each direction. Making this dynamic clear—and this is my second appeal—should be the task of a history of the history of science, which we should aim to represent in terms of the larger resonance chambers in which it unfolds.

These have been the ingredients of my story: the past history of a science, into which the present burst through; an ambitious man at the beginning of his career who found interviews to be deeply frustrating but who spent countless hours conducting them; and the spoken word as a new source that held great promise for historical research over the course of the media developments in the 1960s—but that, at the same time, problematized the role of the historian and his own experiences. These are the tapestry threads that stood at the beginning of a new history of science.

²⁵ Niethammer, "Oral History in USA" (cit. n. 20), p. 472.

²⁶ In numerous interview projects like those of Charles Weiner, Frederic Holmes, Susan Lindee, and Soraya de Chadarevian, this issue was reflected on and carefully addressed. For a balanced description of the historiographic instrument of the interview in the history of science see, e.g., Soraya de Chadarevian, "Using Interviews to Write the History of Science," in *The Historiography of Contemporary Science and Technology*, ed. Thomas Söderquist (Amsterdam: Harwood Academic, 1997), pp. 51–70; and Ronald E. Doel, "Oral History of American Science: A Forty-Year Review," *History of Science*, 2003, 41:349–378.