Liste der englischen Originalzitate aus Anke te Heesen: Revolutionäre im Interview. Thomas Kuhn, Quantenphysik und Oral History, Kleine Kulturwissenschaftliche Bibliothek Band 92 im Verlag Klaus Wagenbach, Berlin 2022

Die im Folgenden angegeben Buch- und Aufsatztitel finden sich allesamt in der Literaturliste ab S. 218.

Endnote 2
»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 1961, S. 807.

Endnote 5
»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.« Kuhn 2012, 48.

Endnote 7
»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.« McLuhan 1997, S. 108.

Endnote 27
»The physicists in this country have become more and more interested in the history of atomic structure and quantum theory as it developed especially in the 1920's. They also became suddenly aware that the original sources of this information are rapidly disappearing. The untimely death of Pauli was a strong reminder that if one wants to recapture some of the authentic information it has to be done soon.« S. A. Goudsmit an P. Rosbaud, 7.6.1961, American Institute of Physics, Niels Bohr Library & Archives (im Folgenden AIP Archives), Samuel A. Goudsmit Papers, 1921–1979, Series IV, Subseries B, Box 28, Folder 45, S. 1.

Endnote 28
»the great men, the great struggles, the great ideas«, Kuhn et al. 1967, S. vi.

Endnote 29
»How urgent it is to proceed promptly [...] is emphasized by the death of Einstein in 1955, von Neumann in 1957, Pauli in 1959 [sic!] and Schroedinger in 1960 [sic!], and by the age of those who remain.« Antrag an die National Science Foundation (im Folgenden: NSF), 7.6.1961, AHQP/MPIWG, Mikrofilm 1419–08, Blatt 347ff., S. 2.

Endnote 30
»the urgency that animates our desire for rapid action. Many of the principals who took part in creating the new science, [...] are still alive, though well advanced in years. Others, who were here only yesterday, [...] have died, so that we have a sharp sense of time's winged chariot at our door.« H. Woolf an J. A. Wheeler, 12.9.1960, Anlage, American Philosophical Society (im Folgenden APS), John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 2.

Endnote 33
»revolution in scientific theory without parallel in the last three hundred years«, Kuhn at al.1967, S. v.

Endnote 34
»These features rank the revolution in theoretical physics as one of the greatest achievements of the human mind.« Ebd.

Endnote 36
»One might anticipate that developments so central to modern civilization and so much to the glory of the human spirit would by now have been fully documented and described. Few expectations could be further from the facts.« Ebd.

Endnote 44
»It is a semi-technical report which it is hoped men of science in this country can use to help their fellow citizens in reaching wise decisions. The people of the country must be informed if they are to discharge their responsibilities wisely.« Smyth 1945, S. 226.

Endnote 53
»It may save a little time for the committee if one of the members takes the liberty to list points which come to mind when any of us discusses the history with his colleagues. I feel a special obligation to make such a listing because of the hours which a few historians and physicists have given in my company to this question […].« J. A. Wheeler an G. Corner, K. K. Darrow, R. H. Shryock, G. E. Uhlenbeck, J. H. Van Vleck, 9.2.1961, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 1.

Endnote 58
»It seemed to me that there was never a full meeting of minds on the crucial question whether the Project should be headed by a theoretical physicist or by a historian«, K. K. Darrow an F. Seitz, 20.2.1961, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 1.

Endnote 60
»Leadership of the project should be an integral part of a professional career, not a digression or a mode of retirement. If possible the director should be a man who might himself exploit the materials assembled by the proposed project and who could train others to do so. Though the project was not itself to prepare a history of quantum physics, it was to maximize the chance that such histories be written.« Kuhn et al. 1967, S. viii.

Endnote 65
»Request for $202,996 for period 1 July 1961–1 July 1964 to record interviews with men central in the 1913–1938 quantum revolution and to retrieve personal documentary material on quantum theory and related scientific developments, 1898–1938.« Bewilligungsschreiben an E. Hutchisson vom 3.8.1961, AHQP/MPIWG, Mikrofilm 1419–08, Blatt 385.

Endnote 78
»And you know that this first paper of Bohr is really the fundament of the whole development of modern physics and the whole development of attempts to understand the structure of atoms and molecules and even of the nuclear in detail.« Churchill, Jack: The Physical Science Study Committee with the support of the National Science Foundation, Alfred P. Sloane Foundation, Ford Foundation presents as part of its course in Physics The Franck-Hertz Experiment. Physical Science Study Committee: Reed College, 1961, online: Archive.org (https://archive.org/details/FranckHertzExperiment).

Endnote 81
»to give access to his writings and thought«, Schilpp 1949, S. viii.

Endnote 87
»And yet, on looking into the history of science, one is overwhelmed by evidences that all too often there is no regular procedure, no logical system of discovery, no simple, continuous development.« Holton 1953, S. 90.

Endnote 88
»Throughout, a particular aim has been to show, by specific case studies of the growth of ideas of physicists from Kepler to Einstein and Bohr, in what respects the traditional views of the way the scientific mind works have to be changed and supplemented.« Holton 1973, S. 11.

Endnote 89
»The philosopher of science is not much interested in the thought process which lead to scientific discoveries … [sic!] that is, he is interested not in the context of discovery, but in the context of justification.« H. Reichenbach zit. n. ebd., S. 17.

Endnote 90
»Recent Past of Physics«, »science-in-the-making or private science«, »science-as-an-institution«, Holton 1961, S. 809.

Endnote 91
»straightforward autobiographical statements«, ebd., S. 808.

Endnote 96
»very influential contemporary distinction between the ›context of discovery‹ and ›the context of justification‹«; »For many years I took them to be about the nature of knowledge«; »actual situations in which knowledge is gained, accepted, and assimilated«, Kuhn 2012, S. 8/9.

Endnote 102
»which is so interestingly written that it is sure to be widely read, and thus to make a large contribution to spreading confusion and erroneous views about its subject.« Condon 1958, S. 1619.

Endnote 103
»hopelessly inaccurate«, Hewlett/Anderson 1962, S. 662.

Endnote 105
»The pressure from members of the committee [...] is tremendous.« T. S. Kuhn an G. Holton, 23.5.1961, APS, AHQP, Box 17, 530.1 AR. 2.5.

Endnote 107
»Reiche's *Quantum Theory* and Whittaker's *Theories of the Aether and Electricity*, vol. 2, proved particularly helpful.« Kuhn et al. 1967, S. 2.

Endnote 122
»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.« »Appeal«, Februar 1962, APS, AHQP, Box 22, 530.1 AR 2.5.

Endnote 123
»The project particularly desires information about: (1) letters bearing on the history of quantum physics, (2) manuscript materials, (3) records of meetings, (4) photographs and films, (5) 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.« Ebd.

Endnote 124
»Copies of such documents or accounts of such recollections will be gratefully received, but what is particularly requested at this time is *word of their existence and whereabouts*.« Ebd., (Herv. i. Orig.).

Endnote 133
»to get into the field with a tape-recorder«, T. S. Kuhn an G. Holton, 23.5.1961, APS, AHQP, Box 17, 530.1 AR. 2.5.

Endnote 138
»All this will help to avoid the usual quick published interview, which in some cases gets no facts but rather serves the interviewer for self-glorification.« G. Holton an C. Kittel, 23.12.1960, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 4.

Endnote 139
»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«, J. A. Wheeler an Ad Hoc Committee, 9.2.1961, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 3.

Endnote 145
»The personal element in interviewing may be carried to an extreme extent. The man who is interviewed may so far overshadow the importance of what he says that the report of the interview becomes almost a sketch of the man himself.« Grant Milnor Hyde in »Newspaper Reporting and Correspondence« von 1912; zit. n. Ruchatz 2014, S. 84.

Endnote 155
»to develop standards of evaluation creativity and ›personal effectiveness‹«, Cohen-Cole 2009, S. 241.

Endnote 160
»This study of scientists derives directly from an earlier study of mine, of artists. Up to that time, while there had been many studies of vocational aptitudes, or special skills related to particular vocations, there had been no clinical studies of vocational choice or performance in terms of life history or personality structure.« Roe 1953, S. 5.

Endnote 162
»These men rated the work of the 69 men on the list submitted to them and added four others. [...] Following this procedure, 30 men were selected. These included […] both theorists and experimentalists.« Ebd., S. 42.

Endnote 168
»a non-sterile science«, Lemov 2015, S. 192.

Endnote 169
»method of datacollecting«, »unmediated access to the inside feel of life«, ebd., S. 194.

Endnote 171
»either Shryock or Corner spoke of a technique for interviewing which has been developed by Allan Nevins formerly of Columbia, and it was suggested that whoever may direct the Project should aquaint himself with this.« K. K. Darrow an F. Seitz, 20.2.1961, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 2.

Endnote 172
»the interviewing of eye-witness participants in the events of the past for the purposes of historical reconstruction«, Grele 1996, S. 63.

Endnote 176
»which made a systematic attempt to obtain, from the lips and papers of living Americans who have led significant lives, a fuller record of their participation in the political, economic, and cultural life of the last sixty years«, Nevins 1938, S. iv.

Endnote 177
»For history, our understanding of times past, is more than facts and ideas; it is a matter of the spirit as well. And our comprehension of the spirit of bygone ages is given us not through physical remains, inscriptions, manuscripts, and books alone; it is given us in large part through the transmission of personalities.« Ebd., S. 117.

Endnote 181
»new technique«, »modern techniques of sound recording«, Bombard 1955, S. 124/125.

Endnote 182
»letter for letter«, vgl. dazu Lee 2004, S. 874–878.

Endnote 183
»Despite the torrent of newsprint, office memoranda, carbon copies in triplicate, and published memoirs [...], the inner workings of social institutions, major events, and 20th-century man himself often go undocumented. The telephone and airplane have made lengthy discursive letters an unnecessary luxury.« Gilb 1957, S. 335.

Endnote 184
»public opinion polling; it is intensive und personal [...]. At its best the interview reveals how the catastrophic and evolutionary events of our time have affected an individual life and how in turn that life has affected events.« Ebd., S. 338.

Endnote 188
»it's previously unrecorded information about what, in the first instance, a couple of the more important leaders of our time discussed between them […], and but for *us*, it wouldn't be preserved, it wouldn't exist.« [Starr in:] Dixon/Mink 1969, S. 15 (Herv. i. Orig.).

Endnote 190
»the black sheep among the contemporary historian's sources«, Seldon 1996, S. 353.

Endnote 196
»Sullivan's book on psychiatric interviewing might provide useful background for an interviewer«, »Notes of Oral Interviewing«, 15.9.1961, APS, AHQP, Box 22, 530.1 AR 2.5, S. 2.

Endnote 202
»I don't at all want to bind you to this program that we've laid out here. It did seem to us that it would make a good deal of sense to stay fairly chronological. Perhaps on this first morning it would be a good idea to talk particularly about your own early years in the field, the years of your education.« T. S. Kuhn, J. L. Heilbron, Interview mit Alfred Landé, 5.3.1962, AHQP/MPIWG, S. 1.

Endnote 206
»silent partner«, Woolfenden 1962, S. 3; »invariable companion«, T. S. Kuhn an W. Heisenberg, 14.11.1962, AMPG, Nachlass Werner Heisenberg, III. Abt., Rep. 93, Nr. 1561.

Endnote 207
»Before the first interview Professor Wentzel indicated great reluctance to have the discussions tape-recorded. Some of the things on his mind would, he insisted, have to be withheld if the recorder were used.« T. S. Kuhn, Interview mit Gregor Wentzel, 3., 4., 5. Februar 1964, AHQP/MPIWG, S. 1.

Endnote 209
«It has been suggested, too, that physicists have become hypersensitive as a result of the numerous inquisitions to which many of them have been subjected«, Roe 1953, S. 43.

Endnote 210
»the editorial aim became more nearly that of an accurate transcription of the interview.« T. S. Kuhn, J. L. Heilbron, Interview mit Alfred Landé, 5.3.1962, AHQP/MPIWG, o. S.

Endnote 212
»supporting documents«, «a new kind of historical document«, Benison 1967, S. ix-x.

Endnote 213
»The document which comes as a result of the Oral History process is a new document it is the product of two minds, the person being interviewed and the interviewer. Its significance depends not only on memory but the hard work and imagination of an historian who tries for the first time in history to put living memory in historical perspective. It is in effect the first interpretation made of a given series of events and interpretation without thought and research ain't worth a damn – unless you are interested in journalism.« S. Benison an T. S. Kuhn, 8.1.1962, APS, AHQP, Box 17, 530.1 AR 2.5.

Endnote 214
»traditional historical fashion«, H. Woolf an J. A. Wheeler, 12.9.1960, Anlage, APS, John A. Wheeler Papers, Series I, Box 26, B:W 564, S. 2.

Endnote 217
»Its hallmark is talk!« Benison 1967, S. ix.

Endnote 220
In »the sciences, we are now uniquely privileged to sit side-by-side with the giants on whose shoulders we stand«, Holton 1961, S. 807.

Endnote 221
»There are people alive today who can remember« Kuhn 2012, S. 48.

Endnote 222
»Our very first interviews began last February, by accident of proximity, and we have been accumulating tape more quickly and more systematically ever since«, »Progress Report«, 26.4.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 2.

Endnote 223
»The preliminary discussions were with Professors Michael Polanyi (about Berlin in the early 1920's) and Maria Mayer [sic!] (recollections of Göttingen and of Professor Born). Our first scheduled and planned interviews were with Professor Alfred Landé who came to Berkeley for the purpose at the beginning of March.« Ebd., S. 2/3.

Endnote 225
»a young Danish physicist, Niels Bohr, came, and gave his first report on the frequency condition in the atomic model. This must have been in 1912.« T. S. Kuhn, J. L. Heilbron, Interview mit Alfred Landé, 5.3.1962, AHQP/MPIWG, S. 5/6.

Endnote 227

»TSK: Before it was published?
L: At the same time as it was published.
TSK: Do you think you heard it from him before you saw the paper?
L: Yes. You see, it was published in the Philosophical Magazine in English. The German at that time – I exaggerate a little – didn't believe anything before it was published in a reputable German journal. In fact, only a few people read English journals because in the gymnasium, English was not taught – only Greek, Latin, and French. Many people simply didn't understand English.
TSK: Did people in Germany by and large read French journals, or did they restrict themselves to a great extent to German?
L: Not too much besides German.
Anyway, I remember this first meeting of Bohr, which must have been after publication in English. He spoke rather poor German with his usual very soft voice, and in the front row were all the big wigs [sic!]. They shook their heads and said, «If it's not nonsense, at least it doesn't make sense.» I spoke with Max Born after the lecture, and he said to me, «All this is absolutely queer and incredible, but this Danish physicist looks so like an original genius that I cannot decline that there must be something to it. This was the attitude.

****

TSK: How had people felt about the Rutherford atom before the Bohr paper?
L: It was known, but only a short time after it was known, Bohr came already.
H: The Rutherford model wasn't given any particular priority?
L: No, no. I remember that at that time there was a little book on the structure of the atom completely in J. J. Thomson's line. There were pictures of corks with iron pieces in them floating on water. In the middle was a magnet. It showed how they arrange in fours and fives and sixes and so on, and even in various «shells». This was the main idea at that time. The only regret was that you could have these models only in two dimensions instead of attractions and repulsions in space.

****

[Recorder turned off]

TSK: You were just starting to say you were giving a seminar.
L: Yes. I had to give a seminar talk about my long calculations, fruitless calculations, on finding a gap in statistical mechanics which would allow selected energy levels. I gave this talk and wrote the whole blackboard full of formulas, and during that time it came to me, «This is all nonsense. I must believe Sommerfeld, who told me there is no solution this way.»
TSK: Was this when you were back in Munich?
L: Yes, before I became assistant of Hilbert. I went to Göttingen already a convinced quantum theorist …. By the way, I forgot this. In my first Göttingen period, before I even came to Sommerfeld, I wanted to begin already preparations for my thesis. I went to the laboratory of Professor Riecke, who was at that time the chief experimental physicist in Göttingen. Just like everybody else there, I was given some problem about cathode rays. The first job was to have a little wire in the empty space of the cathode ray. I put in an electrode or something. I pumped out the tube and every time next morning there was air in it again. I did this for about two or three weeks, and always dirt or air or something. I became so disgusted with experimental physics that I decided to become a theoretical physicist.
TSK: When, do you suppose, did the Bohr atom become generally accepted, or did it ever become generally accepted?
L: It never became generally accepted.

****

The older people, as always, simply couldn't follow the times. It was too complicated and too upsetting. It is the same with nuclear physics. I myself simply couldn't follow all the very difficult mathematics involved in nuclear physics. I also know of Max Born, who simply stopped when nuclear physics began. He said on several occasions that he understands nothing of it. Whereas the younger people find themselves in their veritable element. Quantum theory of radiation and all these things, for another generation are as the semi-classical theory of quantum mechanics was before.
TSK: I asked this question particularly because here is a new idea announced in 1913 and ten years later people including yourself are writing that it can't be done that way.
L: There were people who from the beginning said, «This is all nonsense, it is just a cheap excuse for not knowing what is going on.« Then others said that there must be something to it, and others after a rather short time just took it for the only truth, took it for granted. And this attitude lasted until 1926. Even Bohr himself spent quite a time going through the whole periodic system and explaining by orbital pictures how the qualities of the various atoms changed with one electron after the other built in ….
TSK: It's often said that Bohr himself felt well before 1926 that the model was wrong.
L: Yes, he was very dissatisfied with this model .... It was makeshift. I think he always had the idea that it was makeshift and something provisional. By the principle of correspondence he tried to make it a little bit more coherent with classical mechanics, and in fact the development of the new quantum mechanics came partially through the correspondence principle.« T. S. Kuhn, J. L. Heilbron, Interview mit Alfred Landé, 5.3.1962, AHQP/MPIWG, S. 6/7.

Endnote 228

»TSK: How did your own work with Born on crystallography, and the attempt to apply the Bohr atom to crystals start?
L: You know that Born was the first who proposed that the atoms might be space models, instead of all in one plane. I think Born stubbornly held to the idea of planetary systems, and then, since the planets are all in one plane, the idea that it might be in space simply didn't come to anybody. To talk about times a little bit before this, the problem of helium came directly after hydrogen. There you have two electrons and why should they not be in one plane ... Now this is of course the real history of physics. Sommerfeld found out that it would be a more stable system if you have one electron here and the other in an orthogonal plane. I think he wrote a paper on this, quoted by Born. I think I wrote some notes on this development on these papers which I sent you ...
Apparently Sommerfeld at least had the idea that there might be space models, but it didn't come to anything. Nothing could be proved, there was no experimental approach, until Max Born from his crystallographic point of view found out that the elasticity features of crystals cannot be explained by a plane system .... But again Born thought only of the symmetry elements in crystals – thought only of crystal structure. The symmetry had, therefore, to be cubic or octagonal. I think it was myself who really tried to set up a model in which electrons can run on a polyhedric model. I wrote several papers on «Electronen im Polyheder-Verband.» [sic!]
Born was more or less in sympathy with this, of course always with reservation. Everything was done with great reservations. I remember at that time in Berlin I was already in the Artillerie Prüfungs-Kommission. Born and I went together to Einstein's home, and Einstein also seemed quite interested in these space models. He only asked, «How can they remain stable?» «The slightest perturbation will upset the whole clockwork.» And the usual answer is that there are quantum conditions and they take care of that.

****

TSK: Has anyone asked about stability conditions for coplanar models?
L: There is always this question in the air, but the quantum conditions took care of that.
This was my first personal contact with Einstein, and he of course made a great impression on me personally. From that time on I always thought of space models, and a little bit later I wrote my first paper on the helium atom in which calculations about mutual perturbation under quantum conditions were considered – I think for the first time. I tried to explain the two spectra of helium. I don't remember the

details. One was coplanar and one was in mutually inclined orbits, and at that time I studied one of the astronomical works on planetary perturbations very thoroughly. [End Side 1]« Ebd., S. 9/10.

Endnote 233

»I'm very much interested in this particular story you told us about worrying about the possibility of multiplicity in the Bohr levels themselves, [...]. And it is just exactly the thing we will never discover by reading the literature.« T. S. Kuhn, J. L. Heilbron, Interview mit Alfred Landé, 8.3.1962, AHQP/MPIWG, S. 33.

Endnote 234

»In the paper on the Quantenregel you speak of some change in the formulation being due to something that happened in the Munich seminar.« »something like that.«
»L: It was an issue, but one which could have been solved only after the first issue was clarified. You go step by step. The first step is what the reason for the difference between ortho- and para-helium, and these other questions are so difficult that you better shift them aside and don't think about them.
H: There is a suggestion in one of those papers that it is a nutation that gives the –
L: Well, whether this is just a kind of excuse, I don't know. This just doesn't mean much. I simply thought this is too difficult, that one has to go step by step.« Ebd., 6.3.1962, AHQP/MPIWG, S. 13.

Endnote 235

»Well, do you know when you saw Sommerfeld's 1920 paper – the one that does introduce the inner quantum number – and utilizes it to explain singlet, doublet, triplet strucure?« »No. I don't.« Ebd., S. 14.

Endnote 237

»TSK: Kronig speaks of a visit of both himself and Pauli to Tübingen in 1925, [...] at which there was apparently a good deal of talk.
L: There wasn't much talk because Tübingen at that time was a small university. There was one experimental physicist [...] Paschen. There was Back, his assistant. [...] And one theoretical physicist – this was myself – and this was all. I didn't have anyone to discuss things with.
TSK: Sam Goudsmit has very little of his own correspondence left, but he has been looking through it; and he has a card from you, dated January 1925, which says that Kronig and Pauli had just been to Tübingen and that there was much interesting talk. I just wonder whether there's anything left to be discovered about what may have gone on at Tübingen on that trip.
L: I remember a visit of Pauli to Tübingen which however came immediately at the time of the exclusion principle. Could this have been the same visit? Probably not .... I remember Pauli being in Tübingen. Apparently at that time he was already looking for confirmation of the exclusion principle. [...] And he stayed in my home. We had a party at night, and after the party Pauli worked on in the kitchen, and the next morning he told me about the exclusion principle. So I claimed that he discovered the exclusion principle in my kitchen! But there are seven other cities with seven other physicists who also make similar claims.
TSK: You didn't know when he started out on the films what he was looking for?
L: Well I did in [sic!] the same day, in the morning.
TSK: You don't remember Kronig's being there with Pauli?
L: I remember Kronig's being there, but I don't associate the two. But if there's a postal card, it must be .... So Pauli must have been at least two times in Tübingen apparently.« Ebd., 7.3.1962, AHQP/MPIWG, S. 24/25.

Endnote 239

»of original printed papers from 1918 on [...] which with their dates of publication give a quite complete picture of the development of quantum physics.« Landé, Alfred (Biography), AHQP/MPIWG, A. Landé an T. S. Kuhn, 30.1.1962, Blatt 20.

Endnote 240
»Dear Professor Kuhn, HEUREKA, at a quite unexpected place the whole correspondence of my Frankfurt and Tübingen days has turned up, about 150 items«, A. Landé an T. S. Kuhn, o. D., ebd., Blatt 21.

Endnote 243
»[he] proves by far the best informant the project has yet encountered.« »Progress Report«, 26.4.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 3.

Endnote 246
»Oh, and then there was this whole generation which came with it. All these youngsters, who had, with great facility, made these calculations because it was, so to say, a technique which was given you, and you didn't have to understand much. You just computed, and you did this and you did that and everything came out. Ehrenfest said ›Diese Klugscheisser.‹ [sic!] ›Always so clever they were! And nobody understood anything.‹ Which was partially true and partially wrong of course.« T. S. Kuhn, Interview mit George E. Uhlenbeck, 30.3.1962, AHQP/MPIWG, S. 1.

Endnote 247
»the atmosphere at the time«, ebd., 31.3.1962, AHQP/MPIWG, S. 21.

Endnote 249
»dream-like way of using mathematics and then getting something out«, ebd., 30.3.1962, AHQP/MPIWG, S. 6.

Endnote 250
»Pauli saw it right away«, »he had this feeling of depth«, ebd., S. 2, S. 5.

Endnote 251
»but he had such a feeling that that was the answer to many of these difficulties.« Ebd., 31.3.1962, AHQP/MPIWG, S. 14.

Endnote 252
»TSK: You told me before, in talking about the hydrogen paper, that in your discussions with Sam [Goudsmit, AtH.] you kept insisting it was nonsense to treat hydrogen one way, the alkalis another. As you learned the whole of spectroscopy and models with Sam, or from Sam after the return from Italy, were there other things which you or he or both of you felt to be wrong?
UHL: Well, that's so mysterious, you see. The thing with hydrogen was, of course, that you had the Sommerfeld theory. This was a difficult theory, but it was solved, so to say. One didn't think about it any more. Then there was this other one, where you had the vector model. This seemed to be completely different. Every other word was the vector model, of course, because that was the only thing at hand. And then you had to do such hocus-pocus, you see, changing the quantum numbers once in a while from j2 to j2–1/4. Nobody was satisfied with it, but that was, so to say, the spirit of the times. You had to guess it somehow. [...]
TSK: I take it that very often what precedes a basically new idea is a generally diffused sense that something's wrong. I've been trying to give more structure to this sense of something wrong, even though it isn't a particular thing being wrong that leads to a particular idea, but the general sense that things are not going as they should be.
UHL: There was, of course, this general feeling. First of all, it was a central problem just what the proper quantum theory should be. That was general; that everybody had.
TSK: Do you mean quantum theory for the atom, or does this sweep in the whole question of black-body radiation, specific heats?
UHL: For the atom, the structure of the atom. How, really, the correspondence principle should be sharpened, how one could get all these remarkable regularities in the spectra, that was the central problem

of ’24, ’24 for sure. How it had to be done, that of course maybe some of the great people, as Heisenberg, had some vague ideas, but we certainly didn’t know. And in such a case, you see, there were also, in a sense, no sharp contradictions. The situation was always really such that you had to take for granted certain things which somehow seemed strange.« Ebd., S. 19/20.

Endnote 254

»TSK: Had you felt before that things had to break loose?
UHL: Not really, because again I was just not up to it, you see. People who feel that there is something going on must have been people who really completely knew how the situation was. I would think those would be people like Heisenberg and Pauli and Bohr, especially, and perhaps Dirac. I didn’t know that Dirac had this feeling. But people like me – we knew that it was very confused. In discussion with Sam, I was very scathing about all this unmechanischen Zwang, which I didn’t understand what it all was, you see. But I wouldn’t have dared to say that! That was what fools said! If you didn’t understand what you were told, clearly there must be something to it. But that – in the summer during the spin period – that it was very confused was very clear. And that, therefore, something in the future had to be verified, that was of course also clear. But that this would imply a revolutionary change in the foundations – that for me was far too specific. I think only a few people had that, and those were the people who finally did it. All the others thought it’s all very difficult, and maybe you just go on with what there is. The usual people only think the next step.« Ebd., 5.4.1962, AHQP/MPIWG, S. 16.

Endnote 258

»Now you asked me actually about Bohr, was it not?« TSK: »Well, I asked you about Bohr and about this time in Copenhagen. And you have certainly been responding wonderfully.« T. S. Kuhn, M. Mayer, Interview mit James Franck und Hertha Sponer, 12.7.1962, AHQP/MPIWG, S. 12.

Endnote 259

»Let me take you back if I may, sir.« Ebd., S. 8.

Endnote 260

»I can tell you one story that shows Nernst’s attitude and his wish to win in discussions, and to impose his will if possible.« T. S. Kuhn, M. Mayer, Interview mit James Franck, 11.7.1962, AHQP/MPIWG, S. 7.

Endnote 263

»You told that story somewhat differently yesterday.« »Yes, what did I say?« T. S. Kuhn, M. Mayer, Interview mit James Franck und Hertha Sponer, 13.7.1962, AHQP/MPIWG, S. 8.

Endnote 264

»TSK: And you did the next year. That is the famous experiment with the successive peaks. And you report 4.9, and report it as an ionization potential. Which makes terribly good sense. I think I should say at this point – you may not remember this – that as late as this summary paper in 1916, you are still reporting that as an ionization potential. And there – and so far I know it’s the first time in your work that this occurs. You mention a suggestion of Bohr’s, that perhaps there is photoelectric emission from the electrodes. And you say no; the peaks are too sharp. It must be ionization. So as far as I know in your published work, it isn’t really until 1919 that you side with Bohr.
F: When I think that during that time, 1916–1919 we had no chance whatsoever to do anything. We were both soldiers. Hertz with his serious wound –...
TSK: The reason I raise this is that you speak of reconciling your 4.9 volt measurement with a 10.3 volt ionization potential. But I think that problem must come only later, after 1916.
F: We did, in other words, not know at that time Bohr’s theory at all. And therefore we didn’t know what line we would expect, and if we would have a line. I was only astonished to see in this paper right now the idea that there could be some excitation which was different from ionization was mentioned. In spite of that fact we have not really applied it to the mercury, and could not really apply it very well, because we

had no idea how at a high pressure, with so many inelastic collisions at 4.9, an electron would get enough energy if it were higher. So we said there must be something – a connection between that – and we don't understand it.« T. S. Kuhn, M. Mayer, Interview mit James Franck, 10.7.1962, AHQP/MPIWG, S. 5/6.

Endnote 265

»F: [...] But in all that what we have discussed, it was not discussed and not mentioned how great Bohr's influence was upon all of us. And if you don't mind I would like to say a few words about it. I say that we didn't know of Bohr's paper. But we learned it when we were in the hospital. I learned it. Hertz was too ill. When I was paralyzed, but otherwise quite all right, I read literature, and I found Bohr's paper ...
That was 1916. As soon as the war broke out, we had become soldiers. So there was no possibility any more to do something.
TSK: But you think you had not even heard of the Bohr atom, of Bohr's idea, when you went into the army?
F: I must say that I do not know that. I know that we have not heard of Bohr's paper when we wrote our article. Whether I had in the mean time [sic!] heard about it, I don't know. [...]« Ebd., S. 8.

Endnote 266

»But anyway, it [sein Aufsatz von 1916] shows such an ignorance of the whole theory of Bohr, that I am astonished how that came about. If this was before Stern – let me see. Now, it might be before that time, before I met Stern. I really don't know. If you have occasion to see Stern, ask him when we met in Laôn [sic!]. He might remember. Because I was so much on one front and then on the other front, I don't know anymore when that was. But Stern might know, and since it is really of importance in this connection, I would like to know myself.« Ebd., S. 11.

Endnote 267

»Franck, however, was surprised to discover that they were still describing the ionization energy of mercury vapor as 4.9 volts in 1916 and shocked that they had not even then recognized the relation between their experiments and the Bohr atom. Even when convinced that the now classical interpretation of the experiments had not been published for a full five years after the experiments themselves were performed, he was entirely unable to say how that reinterpretation had come about or what, excepting the pressures of war, had delayed it so long. On none of the central questions about this major episode in the development of quantum physics was he able to help us at all.« »First Year-End Report«, 8.8.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 4.

Endnote 268

»As James Franck said himself in a late stage of our discussions, it is unpleasant to remember mistakes and false starts and for the scientist such memories have no function once the ›right answers‹ have been discovered.« Ebd.

Endnote 269

»in which they sought, from the start, for the answer to a question which they learned to ask only at the end of their research. In that linearized account, the answer to the question was relatively directly forthcoming.« Ebd.

Endnote 271

»F: Do you have questions? TSK: No.« T. S. Kuhn, M. Mayer, Interview mit James Franck und Hertha Sponer, 14.7.1962, AHQP/MPIWG, S. 18.

Endnote 273

»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.« »First Year-End Report«, 8.8.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 3.

Endnote 274

»I have spoken once with Professor Stern, but he has to a very great extent become a recluse. He has retired a good deal from talking to people, and he has not really been willing to talk with us.« T. S. Kuhn, M. Mayer, Interview mit James Franck, 10.7.1962, AHQP/MPIWG, S. 11.

Endnote 275

»Stern was totally unwilling to have a recorder used. In part, he is somewhat sceptical about the project. Perhaps more important, there is now a tape of his on deposit in Zürich, and he rather regrets having made it. From remarks of his relayed by Minkowski, I have the impression that that tape contains a number of caustic remarks about some of his colleagues. Apparently, he does not wish to be burned again.«, »In any case, he doubts that compiling mountains of data is any way to go about it.« Alle Zitate aus T. S. Kuhn, Interview mit Otto Stern, 29.5.1962, AHQP/MPIWG, S. 1.

Endnote 277

»The interview with Professor Hedwig Kohn (department of physics, Duke University, Raleigh, North Carolina) was not very productive because there was very little time and because she did not like the idea of the tape recorder. Since her own connections with the development of quantum mechanics are slight, any consistent line of questioning was very difficult. Besides, she had very little conception of what we might want. She was prepared to show us photographs in her possession and to disclaim any real knowledge of the history. By the time we got to questions, there was not much left.« T. S. Kuhn, Interview mit Hedwig Kohn, 7.6.1962, AHQP/MPIWG, S. 1.

Endnote 278

»His ostensible objection was that he hadn't enough time; he could only give us half an hour at this busy period during the term, etc. Actually the difficulties were much deeper and involved our whole approach to prospective interviewees.« J. L. Heilbron, Interview mit Patrick M. S. Blackett, 17.12.1962, AHQP/MPIWG, S. 1.

Endnote 279

»It does now not seem likely that many people will be able to provide us with detailed information about the development of their own ideas and research. […] For what might be called the sociology of science they are, often, very rich indeed.« »Progress Report«, 26.4.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 3.

Endnote 280

»tacit knowledge«, »that is acquired through practice and that cannot be articulated explicitly«, Kuhn 2012, S. 44/45.

Endnote 281

»Also, he said, how good a place it was because there one can always consult people if one doesn't understand something – the people who are the originators of these ideas – and they can tell you exactly what they meant.« T. S. Kuhn, Interview mit Michael Polanyi, 15.2.1962, AHQP/MPIWG, S. 6.

Endnote 282

»The physicists really did in that respect make very marked progress under the influence of Planck, Bohr, and Einstein. Not so much Rutherford. But James Franck. A kind of nobility was established there. Courteous, firm, and in many ways quite different from what it had been in the previous generation.« Ebd., S. 12.

Endnote 290

»Copenhagen quarters were only a single concrete manifestation of the immense cordiality and helpfulness shown to us by Professor Bohr, his family, associates, and staff.« »Second Year-End Report«, 28.10.1963, APS, AHQP, Box 23, 530.1 AR 2.5, S. 1.

Endnote 294

»There is significant material to be gotten in this way [durch Interviews, AtH.] that will be lost entirely if we do not continue. But the failures of memory which we are encountering do, I believe, make it terribly important that we place more emphasis than we have yet done upon the location and preservation of contemporary manuscript sources – particularly correspondence and notebooks. Significant records of this sort are still in existence.« T. S. Kuhn an P. Rosbaud, 3.7.1962, AIP Archives, Samuel A. Goudsmit Papers, 1921–1979, Series IV, Subseries B, Box 28, Folder 45, S. 1.

Endnote 298

»The second point raised with such emphasis in Rutherford's letter brought me into a quite embarrassing situation. In fact, a few days before receiving his answer, I had sent Rutherford a considerably extended version of the earlier manuscript, the additions especially concerning the relation between emission and absorption spectra and the asymptotic correspondence with the classical physical theories. I therefore felt the only way to straighten matters was to go at once to Manchester and talk it all over with Rutherford himself.« Bohr 1961, S. 1092.

Endnote 302

»It is difficult to go into any of that now«, »I hope another time to tell you about it«, »Now I cannot speak about it today, you see, if we are really to make it proper«, T. S. Kuhn, L. Rosenfeld, E. Rüdinger, A. Petersen, Interview mit Niels Bohr, 31.10.1962, AHQP/MPIWG, S. 1 und S. 2 sowie Ebd., 1.11.1962, AHQP/MPIWG, S. 1.

Endnote 304

»I'm really getting more and more disturbed about it, [the interview, AtH.], and I don't know what we should say now.« »Already a great many things that surprise me and help me a great deal in thinking about these developments have come out, and I think that's the way it should be.« Ebd., 31.10.1962, AHQP/MPIWG, S. 8.

Endnote 305

»You see, the thing is that you expect too much.« Ebd., 7.11.1962, AHQP/MPIWG, S. 14.

Endnote 307

»That encouragement was largely due to the intense interest Professor Bohr was showing in our activities and to the amount of work which he was doing in preparation for our interviews with him. No other physicist has participated so fully in our activities. You will know, then, how severe a blow his unexpected death has been to us here. We have lost not only the man but also most of the oral records and reflections which he alone could have provided.« T. S. Kuhn an Joint Committee, 7.12.1962, APS, AHQP/MPIWG, Box 23, 530.1 AR 2.5, S. 1.

Endnote 308

»an inconceivably rich source for men working on the history of quantum physics«, »Although the loss of Professor Bohr continues to shock and disappoint us, I can assure the Committee that the project has not lost all reason for being.« Ebd., S. 2.

Endnote 309

»Having lost Bohr, Darwin [d.i. der Physiker Charles Galton Darwin, AtH.], and Rosbaud in quick succession, I begin to believe the project is a jinx.« T. S. Kuhn an S. A. Goudsmit, 15.2.1963, AHQP/MPIWG, Mikrofilm 1419–07, Blatt 814–815, S. 1.

Endnote 310

»[We] shall now be doing more work away from Copenhagen than had been anticipated when I last reported to the Joint Committee.« T. S. Kuhn an Joint Committee, 7.12.1962, APS, AHQP, Box 23, 530.1 AR 2.5, S. 2.

Endnote 312
»The sources grow thinner at a distressing rate«, »there are still a few thoroughly worthwhile ones«, T. S. Kuhn an Joint Committee, 4.2.1963, APS, AHQP, Box 23, 530.1 AR 2.5, S. 2.

Endnote 313
»Let me conclude with one word of encouraging news, an unfortunate rarity in these communications. Since I last wrote you, I have had one extremely fruitful interview with Werner Heisenberg. Next week I return to Munich for a prolonged series of discussions with him about which I am now quite optimistic.« Ebd.

Endnote 315
»Yes, well, I have studied your book already and it gave me great pleasure to see the way how you use the term paradigm. This whole comparison which you do between revolutions in science and revolutions in politics is a very interesting parallel. Certainly, one learns a lot from it. Yes, the necessity is to break away those things which seem to be obvious and which actually are the basis on which you stand. One always, in such a situation, is forced to cut the branch on which one is sitting. That can't be helped, because after all, one never can rest. There is no solid bottom.« T. S. Kuhn, Interview mit Werner Heisenberg, 27.2.1963, AHQP/MPIWG, S. 20/21.

Endnote 316
»the nature of science«, Gillispie 2006, S. 343.

Endnote 317
»When was oxygen discovered?« Kuhn 2012, S. 1–9, das Zitat S. 2.

Endnote 318
»History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. [...] This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself.« Ebd., S. 1.

Endnote 320
»the marks on paper that were first seen as a bird are now seen as an antelope, or vice versa.« Ebd., S. 85.

Endnote 321
»the switch of gestalt«, »a useful elementary prototype for what occurs in full-scale paradigm shift« Ebd.

Endnote 322
»unfortunately, the questions to which it leads demand the competence of the psychologist even more than that of the historian.« Ebd., S. 86.

Endnote 324
»reasonable men«, ebd., S. 157.

Endnote 325
»In the sciences, […] if perceptual switches accompany paradigm changes, we may not expect scientists to attest to these changes directly.« Ebd., S. 115.

Endnote 326
»›It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built.‹« Ebd., S. 83/84.

Endnote 327
»›At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics.‹ That testimony is particularly impressive if contrasted with Pauli's words less than five months later: ›Heisenberg's type of mechanics has again given me hope and joy in life. To be sure it does not supply the solution to the riddle, but I believe it is again possible to march forward‹.« »Such explicit recognitions of breakdown are extremely rare«, ebd., S. 84.

Endnote 328
»How do scientists proceed when aware only that something has gone fundamentally wrong at a level with which their training has not equipped them to deal?« Ebd., S. 86.

Endnote 332
»H: [...] I just see here a question: «Who were the leading teachers among the professors?» Now leading, in this case, means [...]«, T. S. Kuhn, Interview mit Werner Heisenberg, 7.2.1963, AHQP/MPIWG, S. 5.

Endnote 333
»I probably ought not now take you back to Göttingen. We must go back.« Ebd., 13.2.1963, AHQP/MPIWG, S. 20.

Endnote 334
»Now I'd like to stay away for a minute from the question of resonance, for I think we rush too fast if we get to that also already. I still wonder if there's more you can say about the way people reacted to all of this?« Ebd., 22.2.1963, AHQP/MPIWG, S. 24.

Endnote 335
»This is a terribly important description that you've been giving.« Ebd., 13.2.1963, AHQP/MPIWG, S. 15.

Endnote 336
»TSK: That's terribly interesting and helpful.« Ebd., 15.2.1963, AHQP/MPIWG, S. 2.

Endnote 337
»I wanted to see whether we could go just a little further because we dashed off on other very, very fruitful problems last time.« Ebd., S. 3.

Endnote 338
»But that's a particularly interesting passage in the paper for various reasons.« Ebd., 11.2.1963, AHQP/MPIWG, S. 10.

Endnote 339
»No, you don't. That's interesting though, because as a matter of fact, Fermi's own first report on it is submitted in February, '26.« Ebd., 27.2.1963, AHQP/MPIWG, S. 13.

Endnote 341
»You know that this stability of laminar flow has a very funny history? Didn't I tell you? Well, Sommerfeld had written [...]«, ebd., 7.2.1963, AHQP/MPIWG, S. 8.

Endnote 342
»I will tell you the story about the integral at the marble table in the cafeteria. That is a nice story. There were small tables in the Hofgarten [...].« Ebd., S. 17.

Endnote 343
»That's quite a fine story.« Ebd.

Endnote 344
»He has this kind of attitude to see whether the thing is right.« Ebd., S. 15.

Endnote 345
»You know it was this standard situation in an undeveloped field of physics where you feel that you have gotten hold of some parts of reality, only you cannot rationalize it to the end. You cannot really get a perfectly clear picture.« Ebd., 11.2.1963, AHQP/MPIWG, S. 16.

Endnote 346
»I want to ask a very general sort of question about this year and to see whether it brings any reactions.« »There's a larger variety of approaches than there usually is.« »Now, after a period in which people have known that things were not right, there is a basic change produced. Maybe now things are right and, if so, this ought to change all sorts of things – one's feeling about the field, the sorts of problems that one picks, the one way one talks to other people about them, the way one feels about heresy.« Ebd., 25.2.1963, AHQP/MPIWG, S. 3/4.

Endnote 347
»H: […] I might just tell this picture. It isn't too poetical. When you do some mountaineering, mountain climbing, you sometimes find yourself in the following situation: you want to climb some kind of peak but there is fog everywhere. Well, you have your map or some other indication where you probably have to go and still you are completely lost in the fog. You don't know whether you have got the right direction or whether you get somewhere entirely on the wrong track and so on. Then the weather becomes lighter, and all of a sudden you see quite vaguely in the fog, just a few minute things from which you say, «Oh, this is the rock I want.« In the very moment that you have seen that, then the whole picture changes completely because although you still don't know exactly the way that you want to go, although you still don't know whether you will make the rock, nevertheless, for a moment you say, «Oh, that's the thing, and now I know where I am; I have to go closer to that and I will certainly see it. When I have seen it, then I will certainly find the way to go,» and so on. I think this change has just taken place in this year. Up to that time, everybody was still prepared that things would turn out entirely different. But from the very moment, say, when this matrix mechanics appeared and especially when the Schrödinger paper appeared then everybody felt, «Well, now we are on the right track. There it is ahead and we have only to go in this direction, then we will certainly find everything we need.» [Absatz] Of course, we also realized that we had not found it, so there may be surprises on the way. These surprises one must always reckon with. Still, it's an enormous difference when you have once seen the whole picture. The whole picture means when you have seen how things can possibly be connected at large. I think it makes such an enormous difference, at least from my own impression, whether I only see details, or whether I see the picture.« Ebd., S. 4.

Endnote 348
»H: Well, it's also nice that this change takes place all of a sudden and still only by very slight changes. In outer appearance, there is very little change from the one to the next stage. But still, these changes are absolutely essential because all of a sudden you see that the whole thing may be connected like that. As soon as you see that, then, of course, you can go in this direction and don't just flounder around.« Ebd., S. 5.

Endnote 350
»But you don't remember in your own experience or with anybody else, a point at which it suddenly became clear that this had to be the right idea?« Ebd., 27.2.1963, AHQP/MPIWG, S. 9.

Endnote 351
»they simply see it«, Kuhn 2012, S. 85.

Endnote 352
»[to] see, more and more clearly« »it looked right«, T. S. Kuhn, Interview mit Werner Heisenberg, 13.2.1963, AHQP/MPIWG, S. 15, 19.

Endnote 354
»And as soon as a paper got warmer, so to say, one had the impression that it was more satisfying.« »Which of these papers did you think were warm?« Ebd., 19.2.1963, AHQP/MPIWG, S. 20.

Endnote 355
»We've come close to this several times and I did not want to let it get away from us.« Ebd., 28.2.1963, AHQP/MPIWG, S. 31.

Endnote 356
»I'm delighted.« »[Kuhn gives Heisenberg an outline showing that Kuhn had noticed exactly this point in reading Heisenberg's paper.]« Ebd., 13.2.1963, AHQP/MPIWG, S. 13.

Endnote 357
»incidental and an incalculable by-product of my having you tell about Landé« »Now, I'm delighted to have that. It's a very important thing for me to know. It might equally well have not come out of my asking you about that. What sort of questions should I be asking you in order to make sure that we don't miss things? [Great laughter].« Ebd., 27.2.1963, AHQP/MPIWG, S. 3.

Endnote 358
»I hate to miss the chance to collect one thing like the story you told me the other day when I'm afraid the machine was not on about the bet over the Klein-Nishina formula.« Ebd., 28.2.1963, AHQP/MPIWG, S. 18.

Endnote 359
»this remains one of the most difficult things for me to get some concrete episodes that will help to pin it down.« Ebd., S. 11.

Endnote 360
»I'd love it if one could make it more concrete.« Ebd., S. 16.

Endnote 361
»The only danger in my telling the story too often is that I improve the story too much. But I think I will try to do it as historically as possible.« Ebd., 11.2.1963, AHQP/MPIWG, S. 2.

Endnote 362
»If I recall correctly«, ebd., S. 9.

Endnote 363
»As you read it, if you can keep in mind that what I would most hope for is not so much a contemporary explanation of what is in it, but anything that it may bring back to your mind of things that went on while you were writing it, of problems that bothered you, of things that you tried to do but couldn't do, of the points that seemed stronger and the points that seemed weaker at the time.« »It's a very different way of reading a paper from the way you would sit down to read a contemporary paper. I know with my own students I have a terrible time, if they have had training in the sciences, making them read a paper as a historian, instead of reading it as a scientist. One has to put oneself in a very different frame of mind in order to get the sorts of points that one's then most concerned with.« Ebd., S. 16.

Endnote 366
»to climb into other people's heads«, «a phrase I used then and now«, Baltas et al. 2000, S. 280.

Endnote 369
»I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out the window of my room – the visual image is one

I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible.« Kuhn 2000, S. 16.

Endnote 370

»When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning.« Kuhn 1977, S. xii.

Endnote 372

»between opening up and clamping down«, Carson 2010, S. 414.

Endnote 377

»The recordings of my conversations with you will be among the most valuable records that this project has collected. [...] I had a thoroughly good time throughout my stay in Munich, and I cannot say nearly so much for all of the trips that I make for this project.« T. S. Kuhn an W. Heisenberg, 13.3.1963, AMPG, Nachlass Werner Heisenberg, III. Abt., Rep. 93, Nr. 1561, S. 1.

Endnote 379

»the staff's return, shortly after Labor Day, to Berkeley«, »Second Year-End Report«, 28.10.1963, APS, AHQP, Box 23, 530.1 AR 2.5, S. 1.

Endnote 380

»In particular, we returned from Europe with a list of about twenty-two men whom it would be desirable for the project to interview.« Ebd., S. 8.

Endnote 381

»There are many other similar disappointments.« Ebd., S. 6.

Endnote 384

»biographical interviews of the sort undertaken by the Columbia Oral History project«, T. S. Kuhn an G. E. Uhlenbeck, 18.10.1963, APS, AHQP, Box 23, 530.1 AR 2.5, S. 1.

Endnote 385

»interviewing as a historical research method«, »not decisively affect the way in which history is henceforth done«, »Second Year-End Report«, 28.10.1963, APS, AHQP, Box 23, 530.1 AR 2.5, S. 5.

Endnote 387

»This volume catalogs materials on the history of quantum physics and related developments in theoretical physics. Many of these source materials now stand ready for use in depository libraries at Berkeley, Copenhagen, and Philadelphia.« Kuhn et al. 1967, S. v.

Endnote 388

»Never in the history of science has so effective an effort been made to record decisive moments in the evolution of new ideas while key participants are still alive.« Ebd.

Endnote 390

»[...] we aim at a more general account, directed particularly to those who may undertake such work in the future, of the project's interviewing policy, procedures, and experience.« Ebd., S. 3/4.

Endnote 391

»stock of evidence for the use of other researchers«, [Brooks in:] Dixon/Mink 1969, S. 6.

Endnote 395
»There is nothing particular to tell about it [SHQP, AtH.], except one thing: that project has probably had some real influence.«, Baltas et al. 2000, S. 303.

Endnote 397
»The following pages offer a reconstruction of a momentous episode in the history of science: Niels Bohr's journey from his doctoral thesis of 1911 to the composition, some two years later, of his famous three-part paper, ›On the Constitution of Atoms and Molecules‹.« Heilbron/Kuhn 1969, S. 211/212.

Endnote 398
»comprehensive and plausible«, ebd., S. 212.

Endnote 399
»route to a conceptual innovation«, »interlocking of conceptual and social factors«, Forman 1970, S. 153–158.

Endnote 406
»Interviewing was frustrating as hell!« Baltas et al. 2000, S. 303.

Endnote 409
»undertaking without precedent«, Hiebert 1967.

Endnote 410
»The significance of letters is obvious to most science historians today, but it was apparently not so then«, Cassidy 2011, S. 265.

Endnote 411
»experimental model for historians, to build upon, and perhaps to improve for other fields«, Hiebert 1967, S. 625.