Writing the Atom: Niels and Margrethe Bohr and the Construction of Quantum Theory

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A dissertation presented
by
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to
The Department of the History of Science

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
in the subject of
History of Science

Harvard University
Cambridge, Massachusetts

January 2016

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Abstract

This dissertation examines the material culture of quantum theoretical work from 1911 to 1927. It argues that the writing practices and the editorial rules Niels Bohr used in his own work and enforced at the Institute for Theoretical Physics in Copenhagen shaped the culture of theoretical physics and quantum theory itself. Each chapter uses oral histories, archived drafts, photographs, and personal and professional correspondence to reconstruct the writing practices of quantum physicists -- dictation, drawing, revising, and reading.

Examining the contributions made in writing and editing work foregrounds different historical actors and scientific sites than those privileged within previous histories of quantum theory. It reveals the importance of Margrethe Bohr, Niels Bohr's wife and amanuensis, and of his secretary Betty Schultz, to the creation of quantum theory. It also emphasizes that domestic spaces including the Bohr family home and summer vacation cottage were significant sites where scientific theories were disciplined.

Chapter 2 addresses Bohr's practice of using dialogue with a non-scientist to produce first drafts. These dictation/dialogue practices co-evolved with Bohr's ideals for scientific communication and underscored the value Bohr placed on communicating to non-experts. Bohr's dictation practices are particularly visible in how he defined the responsibilities of authors and readers in the creation of the meaning of a text.

Chapter 3 reconstructs Bohr's revision practices and the revision and editorial practices he enforced at his institute, especially those surrounding the 1913 article "On the Constitution of

Atoms." It uses theoretical tools from literary theorists, scholarly editors, and historians of science to argue for a new, cinematic way of reading scientific writing, which privileges neither the published version nor the original moment of discovery, situating the meaning of a work in the process not the final product.

By examining the reception of Bohr's theories, Chapter 4 reveals the importance of informal networks in the spread of quantum theory. Bohr's use of interleaved pages to record feedback on published work suggests a writing process that does not stop at publication. The completed work, instead of remaining a fixed text, immediately becomes basis of a new draft.

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Acknowledgements

Like the texts that it examines, this dissertation benefited from the work of many. In one of the parallels that often emerge between historians and their actors, the challenges I faced in writing this dissertation closely resemble the focus of my research. If, as scholars, we are people in a messy world with bodies that tire and turn against us, families that both enable our best thinking and crowd us at inconvenient moments, and daily obligations to our households and students, how do we still produce good work? And how does our work absorb and reflect the daily world in which it is created? This is a dissertation with a lot of world around it. It was written in libraries, but also in hospital rooms. It was work shopped in departmental working groups and on conference panels, but even more vitally on friends' porches and in drafts exchanged while I spent big chunks of my time in medicine infusion centers. It was, crucially, advised by Peter Galison, Ann Blair, and Adelheid Voskuhl, three scholars who, each in their own way, model how to bring rigorous teaching together with sustained human kindness.

Within his historical work and his advising, Peter Galison acknowledges that scholars are embodied people in a real and tangible world and that that world-- Einsten's clocks, Poincare's maps-- carries forward into the scholarship they produce. Rather than aiming to draw clean lines between intellectual work and daily life, Peter shows patience for and interest in the ways in which theory and practice are tangled in spaces that may not be labeled scientific or academic. While I am grateful to Peter for doing all the pivotal work of an advisor: offering well-timed comments and questions, putting the right books in my hand at the right moments, writing all the letters of recommendation that funded and enabled my research, I am most grateful for the steady voice he brought to navigating the world around the work. Peter was always the first to urge me to put my health before my research, promising in voicemails left while I was heading into surgery, that time and space for writing could be carved out later. My gratitude for this repeated wisdom, as I was learning how to maintain my health and my research at the same time, cannot be overstated.

Over meticulously prepared ginger tea served in chipped teacups in Heidi Voskhul's office, I learned so many things: How to prepare a lecture right up to the moment before class, and then deliver it as if you've known the material for years. How to indulge rigorous, extended historiographical exploration, but then emerge with your own voice and ideas. Many of the core questions for this dissertation can be traced back Heidi's "Technology and the Text" graduate seminar, which I took during both of our first semester at Harvard. Heidi has been a careful and conscientious reader, a source of exceptional and pragmatic advice, and a model for how to approach an academic career with humor, kindness, and realism. While an interest in how the material connects with the theoretical grew out of my work with Peter and Heidi, it is Ann Blair who taught me how to begin to answer these questions using tools from book history and material bibliography. I'm appreciative of Ann's attentive and encouraging reading, her eye for detail, and the rich world of book history that her advising and teaching opened up for me.

The entire Department of History of Science supported this dissertation in important ways. I wish to acknowledge in particular the crucial support of the faculty, especially Janet Browne, Alex Csiszar, and Katy Park, and the skilled, sometimes unrecognized administrative work of Allie Belser, Jillian Nyitray, Linda Schneider, and Deborah Valdovinos. I was lucky to overlap with many colleagues in Cambridge whose careful reading and rigorous questions sustained and shaped this project. For their friendship and intellectual engagement I am deeply grateful to Ellen Bales, Stephanie Dick, Emily Dolan, Emily Harrison, Christopher Phillips, Ana Vollmar, Alex Wellerstein, and Beth Yale. Early in my graduate career, the Technology and Imagination Working Group taught me about the special kind of thinking and writing that can happen when smart people engage sincerely with one another's work; there was a magic there that I am always trying to recapture in my courses and writing today. That little group was composed of people who, while now quite spread out, are still some of my favorite people to write and think with: Daniela Helbig, Dave Unger, Nasser Zakariya, Leo Marx, and Roz Williams. More recently the Dissertation Writing Group (DAWGs) formed a special community and support during the final years of writing. Many thanks to all the members of DAWGs for making such a successful workshop, especially He Bian, Margo Boenig-Lipstin, Yan Liu, Jenna Tonn, and Oriana Walker. Each year I looked forward to the insights from other historians of physics at Phunday. I am particularly grateful to Evan Hepler-Smith, Dave Kaiser, Michael Gordin, and Deborah Coen for their generous readings and thoughtful comments and questions. The Minda du Gunzburg Center for European Studies Dissertation Completion Fellowship and the Hiebert Graduate Research Fellowship provided me with essential and much appreciated time to research and write.

My own messy desk is covered with all the genres of writing that show the ways in which my dissertation relies on Jenna Tonn: our shared calendars, to-do lists, conference proposals, and line edits. Without these paper systems and the hundreds if not thousands of hours we spent sitting side by side in the History of Science Library or CES typing until 90-minute timers released us from the task at hand, my dissertation would never have been written. In our breaks between writing, Jenna made intellectual contributions to every part of this dissertation. I am grateful for the rare and steady thoughtfulness with which she approaches both her friendships and her work. My own writing practices improved immensely after Jenna Tonn, Amy Kohout, and I put together our "Science on Vacation" HSS panel and, learning from our historical actors that the best work gets done on vacation, began scheduling writing retreats that allowed us uninterrupted time to churn out pages. May we continue these retreats for many years to come.

I am thankful to my parents, Catherine Shields and Carl Stromsness, for their many forms of support. My mother's repeated insistence that "life is material," which I'm sure she thinks I've forgotten, can be clearly seen in this work. Judy Myers has shown me the kind of unconditional care that I always feel with me, especially when I am writing. Barbara Sternfeld and Bill Carroll first put the possibility of graduate school in my mind and showed a sustained and genuine interest in my writing and ideas. I wish Bill in particular were alive to read this and ask me pointed but pleased questions about it over dinner.

Meredith McGill and Andy Parker were there at the very start, and I'm grateful for the fine advice and nourishing meals they've shared with me all along the way. Together they model the most rigorous and generous approach to this profession.

Finally, I have been in graduate school for my entire marriage to Jamie Formato. He has patiently created the time and space for my work even when that meant doing bedtime with a toddler three weeks in a row. He has always valued this project, which has allowed me to see its value and return to it even when it was very difficult to do so. Our son Owen, now five, has lived with this dissertation his whole life. He has been mostly very patient with what he calls "Mommy's *long* book." Owen has given me very good reasons to finish, and I hope he has learned through watching this slow process the value of steadily working on something each day even when frustrations and challenges emerge. More vitally, Owen has given me the best reasons to pause, stop writing, and be in the world with him.

For Andy Parker and Nasser Zakariya,
my own ideal readers.

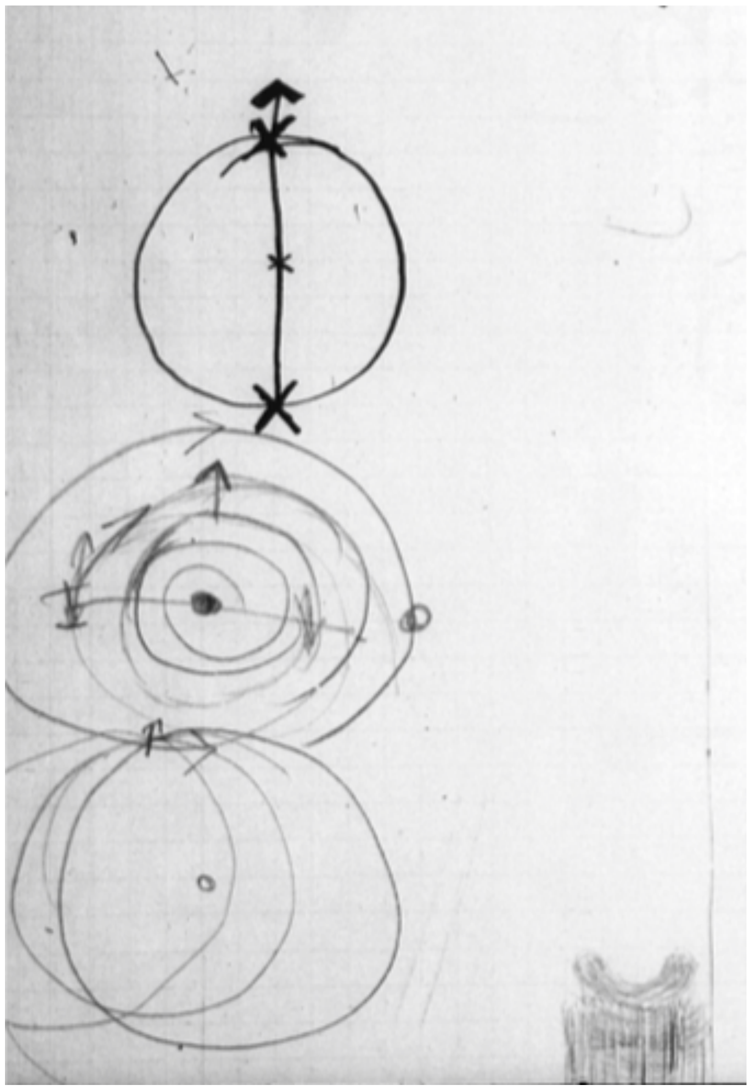

Figure 1: Niels Bohr's first Drawing of the Atom, 1912 (Bohr MSS No. 4, AHQP, Harvard University)

Introduction: A Writing Apparatus for Quantum Theory

Animating Niels Bohr's Writing Practices

The physicist Niels Bohr's personal correspondence and his public lectures often struck readers and listeners as spontaneous or unrehearsed. In fact, a painstaking, labor-intensive writing process is layered beneath all of Bohr's final drafts. Bohr's professional writing, his conference presentations, and his professional and personal correspondence were composed through a process involving dialogue, dictation, and extensive revision. Bohr went through tens to hundreds of drafts for all of his writing and enlisted the aid of an amanuensis throughout his lifetime.[1] Writing to his brother, Harald Bohr, about the process of writing his doctoral thesis, Niels Bohr explained, "I have succeeded in writing fourteen more or less divergent rough drafts."[2] Describing Bohr's editorial oversight at the Institute for Theoretical Physics in Copenhagen and his approach to his own writing and submissions to journals, his longtime secretary Betty Schultz described retyping drafts "many, many times" and complained about the inconvenience of Bohr's life-long habit of continuing to make changes to manuscripts even after he had declared them done: "When he saw now it was finished, and now it could be sent, Professor Bohr said, 'I must just take it home and sleep, and then we'll send it tomorrow morning.' The next morning we wrote it once more."[3]

Revisions were branching, as frequently discarded as they were kept, and changes were made through further discussion, through dictation that mirrored the generation of the first draft, and

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[1] P. J. Kennedy, Niels Henrik David Bohr, and A. P. French, *Niels Bohr : A Centenary Volume* (Cambridge, Mass.: Harvard University Press, 1985), 6.

[2] Harald Bohr cited in Abraham Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, (Oxford; New York: Clarendon Press; Oxford University Press, 1991).

[3] Interview of Betty Schultz on May 17, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4866.html.

through line edits and written notes. Describing Niels Bohr's preparations for his Como lecture, the physicist Oskar Klein recalls:

> Bohr began eagerly... in April, and then we went to Tisvilde... and Bohr dictated and the next day all he had dictated was discarded and we began anew. And so it went all summer and after a time Mrs. Bohr became unhappy... one time when I sat alone in the little room where we worked she came in crying... and then Bohr had to go to the Como meeting and then, under strong pressure by his brother Harald, he really tried to get an article written down.[4]

Betty Schultz, interviewed by the Archive for the History of Quantum Physics (AHQP) about the same set of drafts, had a similar recollection to Klein's:

> Forman: Oskar Klein spoke of the months before Bohr went to the Como conference in the fall of 1927. Do you remember that at all? He recalled that he had been working with Bohr on a paper that he was to deliver there, and then it was to be sent off as a letter to *Nature* and they were working and working and they weren't getting it done. They were rewriting and rewriting and working the whole summer, and then at the very end Bohr was supposed to leave at midnight on the train. And that very evening everybody, you and he and Bohr, were trying to get ready the final copy to send off to *Nature*.
>
> Schultz: That was always so when Professor Bohr should go away, then he worked the whole night and made the last things at the railway station on a bench.[5]

This exchange between Schultz and Forman highlights not only Bohr's extensive revision process but also the social nature of his writing practice. When Forman describes, "[a]nd that very evening everybody, you and he and Bohr, were trying to get ready the final copy to send off to *Nature,"* just for a moment, he frames the context of writing quantum theory almost as widely as it should be framed, with an "everybody" broader than just physicists involved, and in a place outside of official institutes, laboratories, and classrooms.

This dissertation tells the history of quantum theory as a history of writing. Across the chapters, I animate Niels Bohr's writing practices in order to understand how the work of quantum

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4 Oskar Klein interviewed by N. Rosenfeld 7 November, 1968 as cited by Pais, Abraham. *Niels Bohr's Times : In Physics, Philosophy, and Polity*. Edited by Anonymous. Oxford; New York: Clarendon Press; Oxford University Press, 1991, 311.

5 Interview of Betty Schultz on May 17, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4866.html.

theory— dictation, revision, publication— was done and by whom. Using close readings of Niels Bohr's drafts and the correspondence and oral histories of his wife, colleagues, and assistants, I explore how writing practices were negotiated by quantum theorists from 1911 to 1927 and argue that the physics produced during this period was contingent upon these practices. By troubling the processes through which quantum theory was written and re-written, edited, and read, I illuminate our understanding of the narrative structures and epistemic claims of the final printed texts that circulated more widely. Bohr's practice of dictating first drafts to non-scientist amanuenses, for example, creates a commitment in quantum theory to a specific kind of non-scientist reader and to a written language that would be accessible to that reader. An image chosen in an early draft of *On the Constitution of Atoms*, as a consequence of Bohr's drafting and revision practice, moves from analogy to a physical property of the atomic model.

Understanding writing as a core cultural practice of theoretical physics reveals the importance of new historical actors, including Bohr's wife and amanuensis, Margrethe Nørland Bohr, and his longtime secretary Betty Schultz, and new places of work including the Bohr family home and summer cottage. Much of the secondary literature on 20th-century theoretical physics imagines just one tightly knit community and social setting for the work.[6] The secondary literature leads us to the expectation that work done within the walls of Bohr's institute by people carrying the label of "physicists" would be most central to quantum theory. Following writing practices and administrative labor, however, produces a more varied picture of the social contexts for theoretical

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[6] Take for example David Kaiser's claim that Einstein and Bohr were operating within "essentially the same social context" or Mara Beller's *Quantum Dialogue and* Helge Kragh's *Quantum Generations* in which the same cast of historical actors Einstein, Bohr, Heisenberg, Dirac, rotate through and form the community important to the development of quantum theory with little critical attention paid to non-scientist actors. David Kaiser, "Bringing the Human Actors Back on Stage: The Personal Context of the Einstein-Bohr Debate," *The British Journal for the History of Science* 27, no. 2 (June 1, 1994): 129–52; Helge Kragh, *Quantum Generations : A History of Physics in the Twentieth Century* (Princeton, N.J.: Princeton University Press, 1999); Mara Beller, *Quantum Dialogue : The Making of a Revolution*, Science and Its Conceptual Foundations (Chicago, IL: University of Chicago Press, 1999).

physics. Attention to Bohr's writing practices reveals domestic spaces and family members as central to the production of quantum theory. Bohr's home in Copenhagen and his family's vacation cottage in Tsivilde function as significant sites of scientific work as well as cultures in which scientific ideas were disciplined.

Recollections of Niels Bohr's writing process share an emphasis on the intensity of the revision and dictation sessions and the sense that Bohr, and somehow Bohr alone, despite the others upon whom his process depended, was making changes all the way to the last moment: on the railway bench as he was departing, as Betty Schultz described in her response to Forman's leading question, or requesting changes from publishers as things were going to press, as Bohr's assistant Rozental described, or as Abraham Pais quips in his biography of Niels Bohr, "his favorite definition of a manuscript was: a document in which to make corrections."[7] The emotional intensity and cost is also visible in these accounts; Margrethe Bohr brought to tears when a summer holiday is overtaken by the work of dictation, drafting, and revision. Famously, Erwin Schrödinger became ill after Niels Bohr kept him up around the clock discussing quantum theory on Schrödinger's visit to Copenhagen. Bohr's stamina and the exhaustion of his amanuenses and junior colleagues is a recurring theme, which makes clear the materiality of Bohr's writing practices-- the embodied labor and long hours involved in dictation, drafting, and revising.

In the history of 20th-century physics, Bohr's writing habits most often have the status of an entertaining puzzle or peripheral anecdote, something that is always mentioned but rarely taken up as an object of analysis or explored as a working part of the apparatus of knowledge production. When secondary sources do talk about Bohr's writing at any length, they focus on Bohr's narratives about writing or his ideals for scientific communication as if they are one and the same with his

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7 Pais, Abraham. *Niels Bohr's Times : In Physics, Philosophy, and Polity.* Oxford; New York: Clarendon Press; Oxford University Press, 1991, 103.

practices[8] or they subordinate practice to theory, interpreting every writing practice— each note, drawing, change between drafts— as serving a purpose intended by Bohr's strategy for and philosophy of communication. The Bohr that emerges from the existing secondary literature is thus heavily indebted to the scholarly editors and textual critics of the 1960s who saw their role as trying to restore conceptual control to the author by divining and representing his or her true intentions. This mode of analysis persists in the literature of the history of science and particularly the history of quantum theory despite the intervening movements of structuralism, post-structuralism, new historicism, and deconstruction within literary theory and textual criticism and despite all of the attention to authorship and paper tools within history of science and science studies. The persistence of this reading of Bohr, and its insulation from new ways of reading and understanding writing practices, reveals the stubbornness of a particular vision of the scientific author as a disembodied genius whose labor is not particularly relevant to the ideas he produces. Even as paper tools have allowed us to see the products of scientific authorship (journal articles, books) as something that moves and influences scientific communities, the production of such tools and the compositional practices of individual authors have been mostly black-boxed.

In order to draw writing *practices* to the center and understand the work they do within Bohr's process, I draw on methods and analytical frameworks from book historians, scholarly editors, and historians of technology that privilege attention to material practice. Their work provides tools for distinguishing narratives about writing from writing practices and for understanding narrative and practice as distinct but mutually informing processes. Here, we begin to understand the historiographical process through which the iconic image of a calmly overseeing,

[8] For a number of reasons detailed at length below in "Coming to Terms for Writing Practices in the Theoretical Sciences" actors' accounts of their own practices tend to miss many important practices. Many writing practices and habits function as part of the tacit knowledge of this community, rarely spoken of or explicitly recognized as part of the construction of theory.

disembodied Bohr is produced and to recover another view of Bohr as a dialogical interlocutor. Within the history of science, a family of terms developed to talk about materials and writing cultures within the theoretical sciences— inscriptions, theoretical technologies and paper tools— provide vocabulary for the work done by writing practices in quantum theory. Bohr's fuller writing process in my own archaeological reconstruction emerges as more closely connected to these tools and allied with post-structuralist and deconstructive critiques.

Ideals for Writing

For Bohr the problem of knowledge was always closely tied to the problem of communication, both in practice and in theory. While not the primary focus of this dissertation, the story that Bohr told himself and others about the role writing played in scientific communication, his ideals for writing and their accompanying rules, are worth having in our minds before moving into the granular details of his practices. Each chapter of the dissertation in one way or another draws our attention to what happens when these ideals for communication come into contact with material and administrative practice. Bohr cared deeply about the communication of scientific ideas and his rules for writing, revision, and editing were strategic and meticulous, but his ideals did not hit the page in the way that he imagined or planned. As they were put into practice, his rules for writing acted in ways that were as unpredictable as they were important; at this often-messy intersection of ideals and practice, quantum theory was constructed.

A Geography of Clarity and Truth

Bohr was concerned throughout his life with understanding the relationship between scientific communication and scientific knowledge.[9] In Bohr's view, scientific knowledge only became

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[9] Several scholars and biographers have thoughtfully approached the origins of Bohr's attitudes toward scientific knowledge and epistemology by embedding Bohr's scientific work within the intellectual climate in which he was raised and educated. Most of these look to Bohr's childhood home and in particular to the

scientific knowledge when and if it could be communicated to a non-scientist. As Bohr explains this in *Atomic Physics and Human Knowledge* the communication of science to non-scientists "is a clear logical demand, since the very word 'experiment' refers to a situation where we can tell others what we have done and what we have learned."[10] How science was written and how it was read were therefore of central importance to Bohr, believing as he did that scientific theories were incomplete, were not in fact science, until they were understood by someone else. In Bohr's writing process and in the editorial rules he enforced at his institute, there was no such thing as "mere rhetoric." The formation and survival of quantum theory hinged on each word choice.

This did not mean that Bohr believed that scientific theories were constructed through the choices made in writing and rewriting. Scientific knowledge may have been incomplete until it was communicated and understood by others, but for Bohr this communication was also a necessary evil; every stage of the writing process involved progressive compromises, each of which sacrificed

impact of his father on his understanding of what made knowledge whole while others are interested in how Bohr's philosophical interest were shaped at the University of Copenhagen. Bohr's father, Christian Bohr, was a professor of physiology at the University of Copenhagen and a member of the Royal Danish Academy of Sciences. He hosted meeting with other members including the philosopher Hararld Høffding, the linguist Vllhem Thomsen and physicist C. Christiansen. Harald and Niels Bohr listened to their conversations, and, it is argued, absorbed a sense of interdisciplinary scientific knowledge from these figures. For more on Bohr's father's influence see Gerald Holton, "The Roots of Complementarity," *Daedalus* 99, no. 4 (1970): 1015–55. ; Richard Rhodes, *The Making of the Atomic Bomb*, (New York: Simon & Schuster, 1988); Ruth E. Moore, *Niels Bohr: The Man and the Scientist* (Hodder & Stoughton, 1967). Another line of thinking holds that Bohr developed his understanding of science and communication through philosophy courses with Harald Høffding once he became a student at the University of Copenhagen and through his involvement with a student philosophical discussion group called Ekliptika. For more detail on Høffding and Ekliptika's influence on Bohr see, especially, J. Faye's strong claim that Høffding is Bohr's intellectual father in *Niels Bohr: His Heritage and Legacy: An Anti-Realist View of Quantum Mechanics* (Kluwer Academic Pub, 1991). See also, Niels Henrik David Bohr and L. Rosenfeld, *Collected Works: Volume 6* (Amsterdam: North-Holland Pub. Co., 1972); Ruth E. Moore, *Niels Bohr: The Man and the Scientist* (Hodder & Stoughton, 1967); Finn Aaserud and J.L. Heilbron, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013). Note that Pais is dismissive of Høffding's influence on Bohr in *Niels Bohr's Times : In Physics, Philosophy, and Polity*, ed. Anonymous (Oxford; New York: Clarendon Press; Oxford University Press, 1991). Notably, none of these scholars look to the influence of Bohr's mother on his understanding of what made knowledge whole even though, as we shall see shortly, she wrote all of his work for him from the start of school at age seven through age 22, and he could do no work without her. See also Kuhn's Interview of Margrethe Bohr, AHQP.

[10] Niels Bohr, *Atomic Physics and Human Knowledge.* (New York: Wiley, 1958), 72. Also published in Danish as Niels Bohr, *Atomfysik Og Menneskelig Erkendelse*, Second Edition edition (Schultz Forlag, 1957).

truth for clarity. Bohr approached the task of finding the right language for the communication of science with an evolving set of rules. Every word in a final draft had to be able to be translated into three languages (English, French and German) without a change in meaning.[11] Figurative language was methodically crossed out and replaced with literal language in later drafts. Mathematics and jargon were removed and concepts were required to be explained using what Bohr called "plain language."[12] Even when colleagues like Max Born and Werner Heisenberg worried that plain language would cause readers to rely on their own misleading intuition, Bohr persisted in his rules: "[W]e must recognize that above all that, even when the phenomena transcend the scope of classical physical theories, the account of the experimental arrangement and the recording of observations must be given in plain language, suitably supplemented by technical physical terminology." While these rules developed and changed over time, Bohr's way of neatly accounting for them remained steady. He had an unchanging geography of writing and thinking, truth and clarity in mind.

Eventually Bohr would explain his philosophy of language, the choice between truth and clarity forced on any scientific author by the writing process, through an analogy with the predicament encountered by any attempt to describe the inner workings of the atom. Clarity and truth—*Klarheit* and *Wahrheit*— were positioned as a complementary pair. Bohr's theory of complementarity (1927) acknowledged the way in which the speed of the electron and its position were locked in a complementary relationship; any experiment designed to investigate one brought the observer further from being able to realize the other, and yet both were needed to fully describe the electron. Bohr extended complementarity into a far-reaching philosophy, locating

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[11] S. Rozental, *Niels Bohr : Memoirs of a Working Relationship* (Copenhagen: Christian Ejlers, 1998).

[12] Interview of Niels Bohr by Thomas S. Kuhn, Leon Rosenfeld, Erik Rudinger, and Aage Petersen on October 31, 1962, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4517_1.html

complementary pairs within physics as well as other disciplines and situations. The complementary pair Bohr located in writing was clarity and truth.

Bohr's characterization of the writing processes as something that produces multiple subjects and objects while forcing the author to choose just one, long preceded Bohr's articulation of complementarity in 1927. Bohr's attitude toward language was rooted in part in a book that he received when he was a boy and gave as a gift to colleagues and students throughout his life, the Danish classic *The Adventures of a Danish Student* by Poul Martin Møller. Møller was a 19th-century Danish academic and mentor to Kierkegaard. *The Adventures of a Danish Student (En dansk Students Eventyr)* was an unfinished, humorous novel that followed the philosophical musings of a university student.[13] The portion Bohr often quoted in conversation and in lectures considered a student explaining to his teacher why he wrote and worked so slowly:

> I get to think about my own thought of the situation in which I find myself. I even think that I think of it, and divide myself into an infinite regressive sequence of "I's" who consider each other. I do not know at which "I" to stop as the actual, and in the moment I stop at one, there is indeed again an "I" which stops at it. I become confused and feel a dizziness as if I were looking down into a bottomless abyss, and my pondering results finally in a terrible headache.[14]

In Bohr's view truth lay in a wordless abyss. The first utterance, even the first contemplation of utterance, birthed a new consciousness that was already a certain distance from the truth. Clarity existed high above on the surface, many times removed from the essential, true (and wordless) idea that lay below.[15] Writing and speaking were the vehicles to move from the abyss to the surface. Or,

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13 There is consensus that Bohr read Møller at or before his first year of university, when, if he hadn't encountered it at home, he would have been introduced to it through Høffding. See Moore, *Niels Bohr,* (15-16) and Finn Aaserud, *Collected Works of Niels Bohr: Volume 11,* 122. Møller, Poul Martin. *En dansk Students Eventyr*. Schubotheske Forlag, 1896.

14 Translation to English in Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986) Pg. 58.

15 Through his father's friendship with Harald Høffding and through his studies at University of Copenhagen, Bohr would have been familiar with Neo-kantianism, Hegel, Kierkegaard, Møller, and William James, all

as Bohr explained to his children, words were "tools and their limitations had to be recognized."[16] Bohr felt that in writing he and his colleagues had a choice of place. In a play on Kierkegaard's assertion that the condition of human life is to have "70,000 fathoms of water beneath him,"[17] Bohr liked to say that our condition was worse, that we were instead "suspended over a bottomless pit, caught in our own words."[18] We can choose to be closer to truth or closer to clarity, but being near one meant sacrificing the other and moving between these two poles involved the use of tools that were flawed but necessary. Clarity on its own inspired distrust in Bohr. Thus, his frequent praise that someone spoke or wrote clearly was understood by his inner circle as a cutting criticism.[19]

The core limitation of language was in Bohr's mind its inability to express more than one layer of consciousness at a time. No one could possess truth and clarity at once, yet everyone needed to be committed to both for the communication required to make scientific theory count as scientific knowledge. Bohr viewed his rules for his own writing and his rules for the writing coming out of his institute as guidelines for navigating between the danger of being either too clear or too true because of the inevitable loss of the other virtue. Or, as he explained it in a letter to Einstein, in

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scholars and schools of thought who contributed a stable vocabulary for his meditations on what can and can not be thought or put into language.

16 Hans Bohr, "My Father," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (New York: Interscience, 1964), 327.

17 Søren Kierkegaard, *Stages on Life's Way : Studies by Various Persons*, Kierkegaard, Søren, 1813-1855. Works. English. 1978 . (Princeton, NJ: Princeton University Press, 1988), 444.

18 Bohr, "My Father," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, 328.

19 Another expression of this same idea was Bohr's maxim that truths that did not acknowledge the validity of a contradictory truth were merely trivialities. While truths that acknowledged that the opposite was also a truth were profound truths. For this kind of thinking, Bohr has been embraced by some deconstructionist theorists, most notably Arkady Plotnitsky, in his work C*omplementarity : Anti-Epistemology after Bohr and Derrida* (Durham: Duke University Press, 1994).

writing one had the task of navigating between Scylla and Charybdis.[20] Using languages that could be translated across three languages without a change in meaning was one strategy for grasping for terms that balanced clarity and truth effectively for a wide swath of readers. Literature, particularly *The Danish Student* and the Icelandic Sagas that Bohr memorized and recited to friends and colleagues, provided a model for how to produce writing that was neither too clear nor too true, successfully conveying complex and sometimes contradictory ideas to a reader.[21] Rhyming, punning, and humor became ways for Bohr to communicate one idea while suggesting contradictory or double meanings. As he explained it, "Some things are too serious to be spoken of except by ways of punning."[22] Irony and humor in particular were resources for Bohr to navigate along the line between clarity and truth by communicating subtlety and indirectly and therefore, in his mind, effectively but truthfully.

While Bohr was unique in the time and attention he put into questions of language and writing, many of his peer physicists shared his view that the development of new kinds of physics—quantum theory and relativity— forced the question of what language was right for the communication of science. The period covered by this dissertation, 1911-1927, stretches from Bohr's early work developing his model of the atom through to the Copenhagen Interpretation, a

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20 "It has of course long been known how intimately the difficulties of the quantum theory are connected with concept, or rather with the words, which are used in the ordinary description of nature, and which all originate in classical theories. These concepts just give us a choice between Scylla and Charybdis." Bohr to Einstein 1927, AHQP

21 According to his son, Hans Bohr, the writings that Bohr favored and recited in the evenings to his children and colleagues if they were present included Shakespeare, Dickens, Ibsen, Goethe, Schiller, Holberg, Kierkegaard, and, his most favorite book, *Adventures of a Danish Student* by Møller. Heilbron also briefly examines Icelandic Sagas as an inspiration for Bohr because of settings and characters filled with contradiction. Hans Bohr, "My Father," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (New York: Interscience, 1964), 325–39; Finn Aaserud and J. L. Heilbron, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013), 106.

22 Phrase attributed to Bohr in Niels Henrik David Bohr and L. Rosenfeld, *Collected Works: Volume 6* (Amsterdam: North-Holland Pub. Co., 1972).

unified quantum theory described between 1924-27 at Bohr's Institute for Physics in Copenhagen. During this period, theoretical physicists working primarily in Denmark, Germany and the United Kingdom were aware of themselves as creating a "new" physics. They understood that quantum theory broke sharply with the theory and language used to describe the physical world in the latter half of the 19^{th} century. Their desire to persuade their own scientific community and the broader public of this new form of physics made them acutely conscious of how to communicate both at the level of audience and on the level of language.

Bohr is both an example of the kind of attention quantum theorists brought to language and communication and a special case. The community as a whole had a sense of communication as critical to the success of quantum theory, but for Bohr it went further. He had a specific audience in mind and a dialogical understanding of language in which the educated, non-physicist reader had a right to understand the science, and if they did not, it was the failure of the scientist not the reader. Bohr's case also makes clear that the arrows between writing practices and scientific theory go both ways; while relativity and quantum theory raised questions about language and communication, communication and writing practices also put pressure on science, shaping scientific theory in traceable ways. Writing was a crucial tool in the construction of quantum theory.

Because Bohr actively promoted his rules for the right kind of language and terminology for scientific communication in his private and professional correspondence, in his popular books, and in his role as the director of the Institute for Theoretical Physics in Copenhagen, his colleagues were prompted to respond by articulating their own ideas for the ways quantum theory should be written and communicated. With Bohr's prompting, writing practices and ideals for writing that generally function as tacit knowledge among theoretical scientists, were more explicitly contested in this community. Though responses to Bohr's ideals and editorial rules took many forms, Bohr's colleagues seemed to agree that quantum theory posed a new sort of descriptive and rhetorical problem for physicists. Many of Bohr's colleagues including Werner Heisenberg, Max Born, Erwin Schrödinger and Albert Einstein came to their own ideas about the role of metaphor, plain language, and math as language. The German physicist Werner Heisenberg saw scientific language as something that needed its own vocabulary. He felt that the only language for physics was math. "The concepts of ordinary language are inaccurate and only vaguely defined. The concepts of the general laws must in natural science be defined with complete precision, and this can be achieved only by means of mathematical abstraction."[23] A similar kind of confusion of intuitions worried Max Born as well, so he advocated for the coining of entirely new words for quantum theory. For Born the purpose of an entirely new language would be to pull readers out of their old systems of thinking and into new and unfamiliar ones. Plain or ordinary language risked readers understanding new ideas such as quantum theory through old frameworks like classical theory, and for Born that constituted misunderstanding.[24] Leon Rosenfeld, one of Bohr's colleagues and assistants in Copenhagen, was also concerned about the multiple meanings of words producing ambiguity. Alongside Bohr he insisted that "[t]here [was] no point in trying to remove such ambiguities; we must rather recognize their existence and learn to live with them."[25]

As physicists working during the first three decades of the 20th century grappled for the right words for their new physics, they drew their ideals and rules for writing from models inside and outside of the sciences. Their own educational backgrounds, the culture arising out of written exams at Cambridge, the bourgeoning co-education movement in Denmark, correspondence with other physicists, philosophers and psychologists, Icelandic sagas, 19th century novels, and early

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[23] Werner Heisenberg, *Physics & Philosophy : The Revolution in Modern Science* (London, England: George Allen, 1959), 206.

[24] Max Born, *Physics in My Generation* (Oxford: Pergamon, 1956).

[25] L. Rosenfeld, "Niels Bohr's Contribution to Epistemology," *Physics Today* 16 (1963): 47.

20^{th}-century modernist movements in poetry and visual art all played roles in shaping the conventions for scientific writing and communication that were contested by this community. I argue that many of their ideals, especially ideals concerning legibility to ordinary readers, also had their origins in the writing practices themselves.[26]

These ideals for scientific communication were disciplined in ways and places that we might expect—physicists intervened in comments on each others' draft papers, they challenged each other about scientific terminology in academic conferences and in scientific journals[27]—but even more often the right way to write quantum theory was discussed in places that are less expected from the point of view of the existing historiography—on walking tours physicists took together, during winter and summer holidays, and over long evenings around the Bohr family dinner table with a chalkboard standing ready.[28] It is Bohr's writing practices that enable us to see these domestic spaces as sites of work, and it is those same writing practices that reveal interlocutors who do not carry the label of physicist or even scientist.

The Archive for the History of Quantum Physics & the Twinning of Bohr and Einstein

Toward the end of his life, when Bohr was interviewed by Thomas Kuhn for the *Archive for the History of Quantum Physics* (*AHQP*), Bohr was concerned but hopeful about the legacy of quantum theory and complementarity. Speaking to Kuhn, he compared complementarity to the Copernican system, initially rejected but then almost universally accepted: "But in the next generation, the

[26] See Ch. 2 for a discussion of how Bohr's dictation practices structured his idea of an ideal reader and his criteria for intelligibility of quantum theory.

[27] See, especially, discussion of revision and editing in Ch. 3.

[28] Throughout the dissertation I also follow the major themes, tropes, points of consensus and divergence in the ways that writing was talked about by this community from 1911-1927 and, much later in the 1960s and 70s, when the first major histories of quantum theory were written. I attend to the ways that these later discourses erase work, particularly the work of women, and place the site of scientific work in the Institute instead of the home.

school-children did not think it was so bad, and thereby a situation was created where it belonged to common knowledge or common preparation that one had to take that into account. I think it will be exactly the same with the complementary description. It may be it's already, but I do not know." Kuhn, tried to spare Bohr's feelings by answering, "I think not yet."[29] In fact, complementarity, central to Niels Bohr's own understanding of quantum theory, never had anything close to the effect on popular culture that relativity had.[30] Far from being embraced by classrooms full of school children, at the time of Bohr's death complementarity was being reduced from a robust theory to the more confined use of complementary pairs, and would be handled cautiously and briefly by Niels Bohr's biographers.

Eighteen days after his interview with Kuhn, Niels Bohr died at his home in Copenhagen, Denmark. In the wake of his death, there was a flurry of biographies and histories written celebrating his life and work. While these biographies exhibited less worry about complementarity and quantum theory as a rival to relativity than Bohr did in his later years[31], they were concerned with the related task of painting a picture in which Niels Bohr's influence on 20th Century Physics could be understood as equal to Albert Einstein's. At the time of Bohr's death, the *AHQP* was in the midst of conducting oral histories and photographing and collecting manuscripts and correspondence of the generation of physicists who articulated quantum theory. Efforts to forge, shape and edit Niels Bohr's legacy are visible throughout this primary source base, a source base which continues to be of fundamental importance to every scholarly and popular history of quantum

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[29] Interview of Niels Bohr by Thomas S. Kuhn, Leon Rosenfeld, Erik Rudinger, and Aage Petersen on October 31, 1962, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4517_1.html

[30] Complementarity receives a mere 3 mentions in the New York times throughout the 20th Century.

[31] See Chapter 4 on Reading and Reception for manifestations of Bohr's concern

theory. Both the *AHQP* and the secondary literature that uses the archive as a core source base perpetuate Bohr's work on quantum theory as solitary and disconnected from the world around it.

Einstein died seven years before Bohr in 1955 and his reputation as an iconic, lone genius and giant of 20th century physics was well defined at the time of Bohr's own death. The effort to hold Bohr equal to Einstein constrains the historiography of Bohr and quantum theory in stated and unstated ways, causing memoirists, historical actors, and historians themselves to emphasize parts of Bohr's life and career that conform to the existing conventions of Einstein's biography and to de-emphasize or leave out parts of his life that do not conform. Whether he is compared or contrasted with Einstein, Einstein is always there, and the conventions of the telling of Einstein's own story serve as a constant frame for Bohr's. Bohr's marriage, the collaborative nature of his work, and the portions of his career devoted to more philosophical topics such as complementarity are treated as obstacles to creating a legacy that can measure up to or be easily compared with Einstein's, and they are consequently excluded or marginalized in actors' and analysts' accounts of Bohr's work.

The editors of *Niels Bohr: A Centenary Volume* waste no time in positioning Bohr's legacy in relation to Einstein's. On the very first page of their introduction they explain that this is a "companion volume" to Einstein's Centenary Volume. "We became convinced," they explain, "that Bohr was the one physicist of this century, besides Einstein, whose life and work were so wide-ranging and important that a commemorative book about him might have a broad appeal." [32] The twinning of Bohr and Einstein actually takes place before the reader opens the book; the Bohr volume is the same size, shape and color, meant to sit together with Einstein's as a set. Writing a biography of Bohr six years later, Abraham Pais also describes his work as an effort to make sure that Bohr receives as much credit as Einstein, and a fear that thus far this has not been the case. He

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[32] P. J. Kennedy, Niels Henrik David Bohr, and A. P. French, *Niels Bohr : A Centenary Volume* (Cambridge, Mass.: Harvard University Press, 1985).

characterizes Bohr and Einstein as equally important figures in 20th-century physics with very different working styles. Bohr worked in groups while Einstein worked alone.[33] For Pais, there is something in the way that Bohr's contributions were connected to other people that prevented him from receiving as much credit and recognition as Einstein. He wonders whether the fact that Bohr worked so often with other people, created an institute, devoted time to teaching, mentoring, and editing, is part of what has deprived him of receiving his recognized place alongside Einstein.

This is in part the story of framing Bohr as Einstein's equal, but it is also the task of the genre of scientific biography more generally. The conventions in Niels Bohr's biographies, particularly the organizational choices, are motivated by conforming to the project of scientific biography. In their work hoping to refigure or re-imagine the potential of scientific biography, Theodore Porter and Mary Terrall both acknowledge that a fundamental task of scientific biography has been to recover the individual figure of the scientist. As Porter notices, the work of biography is often celebrated as humanizing the scientist, and this is accomplished through sequestering the science in separate chapters.[34] During the period in which the *AHQP* was formed and many of Bohr's biographies were written, three factors worked in tandem to contribute to their particular form and emphases and help to explain why writing practices and the people who participated in them are moved to the side in such a consistent way: a nostalgia for workbench physics during the beginnings of big science, an effort to ensure Niels Bohr's legacy by twinning his biography to Albert Einstein's, and a desire to conform to the conventions of scientific biography more broadly. At the heart of all three there is a move to extract and preserve the individual figure and individual

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33 Abraham Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, ed. Anonymous (Oxford; New York: Clarendon Press; Oxford University Press, 1991), 3.

34 Theodore M Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton, NJ: Princeton University Press, 2004), 314; Mary Terrall, "Biography as Cultural History of Science," *Isis* 97, no. 2 (June 1, 2006): 306–13.

agency. Together these genre conventions and motivations help to explain how and why certain kinds of collaboration are moved to the periphery of the secondary literature on Bohr.

I mine the biographical and secondary literature on Bohr and Quantum Theory for stories about writing, dictation, and reading, gathering these anecdotes in order to tell a larger story about writing ideals and practices in theoretical physics from 1911-1927.[35] At the same time, I examine the stakes of the existing secondary literature about Bohr in order to understand why and how it moves his collaborative writing practices into the margins, erasing work, particularly the work of women who wrote and took dictation for Bohr. Scientific genius and the conventions of scientific biography are central themes in the dissertation, emerging repeatedly as obstacles that have prevented the description and analysis of Bohr's writing practices.

Placing Writing

When the story told by the actors travels more or less whole into the secondary literature, what avenues do we have for telling a different sort of history, one that escapes retreading the same emphases and motivations that the actors and analysts share? Working in three different periods and contexts, Steven Shapin, Paula Gould, and Anne Secord suggest that a focus on place is one way to get around the standard, reigning narratives of how knowledge is produced in order to unearth unaccounted for actors and activities involved in the production and acceptance of knowledge.[36]

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[35] Ch 1 describes in greater depth the narratives about writing operating in the community of quantum theorists in the 1910s and 1920s.

[36] Shapin argues that place is a way of describing the social activities constraining knowledge production in 17th century England. Citing Shapin's approach, Gould sees place as a way to access and account for the experience of women in Cavendish laboratory in the 1870s and 1880s. Gould notes the difficulty of following women into the laboratory using conventional sources; they do not appear in financial records and have no official membership in the University. For Secord, place is a way around the dominant framework set out in the historiography in which botany was displaced from the pub to the field. S. Shapin, "The House of Experiment in Seventeenth-Century England," *Isis* 79, no. 3 (1988): 373–404; P. Gould, "Women and the Culture of University Physics in Late Nineteenth-Century Cambridge," *British Journal for the History of Science* 30

For all three, attention to place, to where science is actually practiced, is a way to recover the social activity of science and sidestep the framework of the dominant story set out in the historiography. While actors at the time and historians later have placed the activity of theoretical physics in institutions like the Bohr Institute for Theoretical Physics in Copenhagen and emphasized the important role those institutes play in the rise of modern physics, archived correspondence, drafts and oral histories indicate that much of the work these physicists were doing took place elsewhere including, especially, the Bohr family dining room and vacation cottage.

For understanding the material culture of writing practices during the development of quantum theory, a focus on place operates in two distinct but related ways. A better understanding of the places of quantum theory is one of the greatest rewards of carefully tracing writing practices as revealed by close reading of Bohr's drafts and correspondence. We learn from these drafts and letters that pivotal work in quantum theory was not worked out in institutional settings, as we might expect, but instead after dinner at the Bohr residence or by candlelight in the Bohr's vacation cottage after Icelandic Sagas were recited by Bohr and all the children were put to bed. We learn that these spaces— half-lit kitchens in Tisvilde, a neighbor's abandoned painting studio, a black board dragged into the dining room after the dishes were cleared— are integral work sites and spaces of quantum theory. At the same time, a focus on place is an important strategy for recovering more detail about these writing practices, the under-accounted for activity of quantum theory. Once we become aware of domestic spaces as sites of quantum theoretical work, a focus on sources that are explicitly about those places— accounts and correspondence of Bohr's children, for example— emerge as unexpectedly rich sources for describing the quantum theoretical work and writing practices and understanding the roles of new actors in those practices.

(1997): 127–49;A. Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire," *History of Science* 32, no. 97 (1994): 269–315.

Family, the home, and the communication of quantum theory

Where historians of physics have been concerned about placing scientists in their institutions, in their social, political and intellectual contexts, we have not generally been as concerned with putting them in their families. Or in how understanding their families enrich our understanding of scientific ideas and institutions. Bohr's childhood family of origin, his adult family home in Copenhagen, and his summer retreat in Tisvilde, provide a middle scale for understanding the development of quantum theory not as the product of one or of many. Along with other historians who have attended to family, Deborah Coen reminds us that "Between the laboratory revolution and the dawn of big science, scientific work was often carried out in the home, with relatives as collaborators." [37] She urges more historians to pay attention to family as a way to recover dimensions of scientific practice unacknowledged by accounts in which science is rendered as either the work of a lone genius or as a "vast ensemble production."[38] In *Vienna in the Age of Uncertainty* Coen uses the biography of a prominent Viennese family, the Exners, to trace how probability and statistical perspectives move from the social sciences into the natural and physical sciences in the early 20th Century. For Coen, Vienna and its nearby summer retreats cultivate a "culture in which the family sphere was not a retreat from rational thought and political engagement but constitutive of them."[39] In late 19th and early 20th century Vienna, family occupied an important space in between private and public life, and the ways that the actors themselves mapped public and private space afforded a special status to family.

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[37] For work on relatives as scientific collaborators see: Janet Browne, *Charles Darwin: The Power of Place* (Princeton: Princeton University Press, 2002); Mary Jo Nye, "Aristocratic Culture and the Pursuit of Science: The De Borglies in Modern France," *Isis* 88 (1997): 397-421.; Paul White, "Science at Home," in *Thomas Huxley: Making the "Man of Science"* (Cambridge: Cambridge University Press, 2002).

[38] Deborah R Coen, *Vienna in the Age of Uncertainty: Science, Liberalism, and Private Life* (Chicago: University of Chicago Press, 2007), 21.

[39] *Ibid*, 2.

Just as Coen discovered that her actors' usage of terms pivotal to the development and transmission of uncertainty, terms like "subject" "normal" and "universal," were conditioned and disciplined through "experiences of domestic intimacy," this dissertation will explore how family is a site from which terms essential to Bohr's ideas about quantum theory and its communication drew their meaning.[40] Bohr and his colleagues drew terms like "universality" "harmony" and "common language" from models within scientific and literary culture, but these terms were molded and disciplined in domestic spaces where the bulk of their writing took place.

Along with Coen, other feminist scholars remind us that the demarcation of public and private space is always a political act. Coen understands these boundaries as malleable and contingent, and instead of drawing a priori lines between public and private, she shows how the historical actors themselves mapped these spaces.[41] Coen's intervention usefully contextualizes my own. Memoirs, biographies, and histories written in the latter half of the 20th century tell the history of the development of quantum theory in a way that inscribes clear lines between domestic and professional spaces, placing scientific activities in institutional spaces. I have, following Coen's example, strived to tell the history of quantum theory in a way that does not indulge or re-inscribe these existing lines, but rather follows the map the actors themselves used at the time to distinguish or merge professional and domestic spaces.

The Status of Writing in the Secondary Literature on the History of Quantum Theory

While writing has been marginalized or ignored in much of the secondary literature on Niels Bohr and Quantum Theory, two historians of physics have taken writing as the object of their inquiry. Gerald Holton and Mara Beller have both used Bohr's drafts as important sources in their works on the history of Quantum Theory. By examining the analogy between atoms and planetary systems as

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[40] Coen, *Vienna in the Age of Uncertainty*.

[41] Ibid., 3.

it moves across the drafts of Bohr's 1913 paper "On the Constitution of Atoms," chapter three of this dissertation in many ways takes up Gerald Holton's invitation to explore how "before a work has ripened, during its nascent period, traditional rhetorical elements such as conjecture and metaphor are at play."[42] Holton reads Bohr's drafts "orthogonally" as a "discourse among several actors whose interplay shape the publication."[43] In fact, when Holton refers to "analogy" or "metaphor" in an "unripened" work, he is talking about how a scientist navigates between many different kinds of thematic commitments, sometimes articulating one in terms of another. Mara Beller also looks to Bohr's drafts and revision as a way of recovering the lively debate and dialogue active around a variety of quantum theoretical breakthroughs. She is, as she puts it in her introduction, interested in tracking how "dialogic flux is flattened into a monological narrative" through the deployment of rhetorical strategies in later stages of revision.[44] Beller's argument as a whole is motivated by reasserting complexity and disagreements among physicists into the history of quantum theory, a history she fears has been artificially smoothed and made to seem inevitable by physicists and historians of physics alike. The smoothness and inevitability, Beller argues, has its roots in a rhetoric of orthodoxy in the final drafts of the Copenhagen Interpretation and the Como Lecture.

Like Beller and Holton before her, I am interested in how dialogue is manifested or rejected in the stages of revision, but I am working with a more expansive definition of dialogue. First, I am interested in dialogue among a larger group of historical actors. Beller defines dialogue as the exchange of ideas and disagreements among major and minor theoretical physicists as they are

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42 Gerald James Holton, *Science and Anti-Science* (Cambridge, Mass.: Harvard University Press, 1993), 76.

43 Ibid., 75.

44 Mara Beller, *Quantum Dialogue : The Making of a Revolution*, Science and Its Conceptual Foundations (Chicago, IL: University of Chicago Press, 1999), 2.

expressed in their archived correspondence, conference presentations, and published articles. Her intervention lies in drawing attention to the contributions and perspectives of minor physicists who have been overlooked. My intervention is the inclusion of other *non*-physicist historical actors who were called on repeatedly to contribute to the generation, revision and editing of drafts. While Beller is interested in recovering the roles of "lesser scientists" Margrethe Bohr and Betty Schultz and other assistants who took dictation make no appearance as active historical actors in her account.[45]

Second, the definition of dialogue operating throughout this dissertation includes not just the exchange of ideas between physicists in their completed correspondence, but also the oral dialogue that takes place in conversation and dictation sessions (see Chapter 2) as well as the written notes in the margins and line edits of drafts of letters and articles before they are sent to a recipient or to print (see Chapter 3). And, as will be elaborated in Ch. 2 on dictation and Ch. 3 on revision, the actual spoken dialogue, hard as it may be to recover, is an exceptionally important dialogue to consider. Ignoring actual dialogue obscures the work of the people crucial to the early stages of Bohr's writing, especially, and also throughout the revision process. I also argue in Ch. 3 that Bohr's specific writing practices, the line edits and changes he writes into his own drafts, produce a way for Bohr to be in dialogue with himself.

In my own approach to revision (see Ch. 3), I track how figurative language, prevalent in early drafts, is revised out of later drafts, and what the consequences of these revisions are. I examine how the conversion from figurative to literal shapes Bohr's model of the atom. In other words, I am interested in how the model of the atom is contingent on the rhetorical devices used across the drafts. While Beller's work makes it possible to see the texture of debate in a story of Quantum Theory that has more often been told heroically, smoothly, and inevitably, her interest in rhetorical strategies is confined to describing their use in later drafts to dismiss opposition or replace

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45 Ibid., 118.

vigorous disagreement with sense of agreement and orthodoxy.[46] Her work does not make visible the other kinds of labor accomplished through rhetorical devices and throughout the revision process.

Conflating Writing Practices and Discourses of Writing

When writing is attended to in the broader secondary literature on the history of physics, there is often a conflation between how historical actors report their writing practices with how they actually wrote. Historical treatments of Bohr that claim to be about his writing or work practices are more often closely and productively reading his theories of writing or his narratives of writing, but not the practices themselves.[47] The distinction between how one writes and the stories one tells about how one writes, what I will call writing practices and the discourse surrounding writing practices, is as crucial as it is hard to make. In the existing secondary literature "writing" slips in as synecdoche for theories of writing, narratives about writing, and the actual writing practices themselves. Even as I recognize how interrelated these ways of thinking about, doing, and reporting writing are, their relationship to one another can hardly be described without an effort first to understand the ways in which they are not the same. There are many reasons why Bohr's reporting of his writing practices is taken as the practice itself. Bohr was a figure that commanded a lot of trust, a father figure or mentor to the community of physicists developing quantum theory largely under his supervision. He was a half a generation older than most of his immediate colleagues.

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[46] Ibid., 243.

[47] For thoughtful, varied analysis of Bohr's theories of writing see: David Kaiser, "Bringing the Human Actors Back on Stage: The Personal Context of the Einstein-Bohr Debate," *The British Journal for the History of Science* 27, no. 2 (June 1, 1994): 129–52. John L. Heilbron and Thomas S. Kuhn, "The Genesis of the Bohr Atom," *Historical Studies in the Physical Sciences* 1 (1969): vi – 290. Arkady Plotnitsky, *Complementarity : Anti-Epistemology after Bohr and Derrida* (Durham: Duke University Press, 1994).

What we notice in David Kaiser's work comparing Bohr and Einstein's writing, for example, is a substitution of how Bohr and Einstein talked and wrote about writing for the writing practices themselves.[48] This slippage is not unique to Kaiser's article, but his argument is instructive for illustrating how tricky it can be to untangle writing practices from attitudes about writing or even writing practices as reported by the actors themselves. Kaiser argues that "Bohr primarily thought in words, Einstein in images." For Kaiser, Bohr's statement that "Every scientist... is constantly confronted with the problem of objective description of experience... Our basic tool is, of course, plain language"[49] is taken as an enthusiasm for language. But when we closely examine Bohr's drafts and correspondence along with his other writings about writing, we learn that Bohr's prescription for plain language is not born out of an enthusiasm for writing and language but rather from an abiding dread. Writing, for Bohr, was a difficult task and a necessary evil. As we will see in Ch. 3 on revision, his first drafts are mostly pictures and equations, not words. In the quote Kaiser cites and in other places, Bohr's writing practices are often in tension with his discourse about writing. In this dissertation I will at times examine the discourse that develops around writing practices, and the relationship this discourse has with the practices themselves, but my central priority will be on reconstructing the writing practices themselves.

To hold writing practices separate from discourses about writing, I draw from recent scholarship at the intersection of history of technology and history of the book. Matthew Kirschenbaum explores a similar conflation in *Mechanisms* between how computers work and the discourse about how computers work. The discourse about computers, which Kirschenbaum describes as being formed largely by science fiction writers in the 1980s and then picked up

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48 David Kaiser, "Bringing the Human Actors Back on Stage: The Personal Context of the Einstein-Bohr Debate," *The British Journal for the History of Science* 27, no. 2 (June 1, 1994): 129–52.

49 Ibid., 140.

uncritically by literary theorists in the 1990s, gives the impression that screens are the center of how a computer works and that data is ephemeral.[50] Kirschenbaum calls this a screen essentialist "medial ideology" that perpetuates assumptions that "electronic text is hopelessly ephemeral, that it is infinitely fungible…and that it is fluid or infinitely malleable."[51] Citing the impact that material bibliography has had on our understanding of printed sources, Kirschenbaum seeks to upend these assumptions by treating the computer as an artifact and telling a story centered on use: how computers are used, signs of use in the hard drive, in the work space.[52] This yields a history of computers centered on the hard drive and one in which data is viewed as permanent and not ephemeral.

I employ Kirschenbaum's methods and analytical tools throughout this dissertation to hold Bohr's writing practices separate from the narratives and discourses of writing developed by historical actors at the time and mimicked by scholars later. Just as Kirschenbaum is interested in describing the discourse about computers and locating themes and assumptions at work in them, I am interested in the discourse surrounding writing practices in quantum theory. In my engagements with secondary literature on the history of physics, I attend to the ways that these actors' discourses about writing are absorbed and repeated by the scholars who write about them. What are the shared stakes and assumptions of the historical actors and historians? Whose work is emphasized and whose work is lost? I am particularly interested here in exploring the discursive power to erase work, especially the work of women.

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[50] The theorists Kirschenbaum singles out here include George Landow, Michael Joyce, N. Katherine Hayles (42)

[51] Matthew G. Kirschenbaum, *Mechanisms : New Media and the Forensic Imagination* (Cambridge, Mass.: MIT Press, 2008), 50.

[52] Ibid., 17.

While Kirschenbaum's work is instructive to the task of separating the discourse surrounding writing from the writing practices themselves and describing these practices through a focus on artifacts and use, there are parts of the dissertation that diverge sharply from the priorities of his work in *Mechanisms*. Kirschenbaum tells a story where the material intervenes in and changes a dominant and, to his mind, false narrative about the history of computing. While I am certainly interested in the way that a history of quantum theory focused on writing practices might change our sense of how quantum theory was produced, where it was produced, and who authored it, I am also interested in exploring both how theories of communication and theories of textuality impact writing practices and writing practices impact theories of communication.

Coming to terms: Writing practices as Experimental Paper Tools

The proximal theoretical tools necessary for a new reading of Bohr's writing practices come from Coen who enables us to see the active role that domestic spaces and family play in disciplining scientific ideas, Kirschenbaum who clarifies the crucial distinction between discourses surrounding practices and the practices themselves, and finally from Ursula Klein's use of the term "paper tools." By coining a term that gives every line, label, scrap of paper, and diagram written by human or machines a name separate from the names scientists themselves use, a name that evokes the tools of the experimentalist, Klein gives theoretical work the texture of experimental work. With her new term, Klein seeks to identify the analogs to the instruments that historians of experimental science have found so useful for making practice visible in experimental science within theoretical, paper and pencil based sciences (or, in Klein's case, the paper and pence based labor of an experimental science like chemistry). Equipped with paper tools we ought to be able to approach theoretical scientific work not as a solitary activity but as skilled labor contingent on local cultural resources and to see the diagrams and labels that circulate in a community as that community's tools with all of the nuance that had been reserved for history and sociology of experimental sciences.(Unattached

Footnote)[53]A key dimension Klein's term for my own purposes, is the idea that a paper tool can react back on its user or creator limiting and enabling work in ways that are unanticpated by their creator. By viewing some elements of Bohr's writing practices as paper tools, we are better equipped to notice how dictation to a non-scientist amanuensis, reliance on drawing in early drafts, and the purging of figurative language over an exceptional number of drafts (among others), structure and determine his published writings in ways that are intended and unintended by Bohr.

In her work on Berzelian names for chemicals in the early 19th century, Klein grabs hold of Latour's "chains of inscription" to describe the process through which scientists "mobilize a plethora of material and conceptual resources" to make claims about invisible objects.[54] Klein's "paper tools" describe a specific kind of inscription near the end of the chain that is put to intended and unintended uses beyond the representation of invisible phenomena.[55] Berzelian formulas, for example, are a convention for labeling chemicals shared by European chemists in the early 19th century. Shortly after being used as labels, they began to be used in a way wholly unintended by their creators for representing, manipulating, and predicting chemical reactions on paper. The formulas then went on to contribute to a new way of classifying organic compounds, based not on their origins and properties but on their constituent parts, and this classification system privileged certain kinds of chemical reactions while making others difficult to represent. Paper tools such as Berzelian formulas, Klein argues, supplement laboratory tools as tools for constructing interpretive models

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[53] In Ch 3 on revision practices, I will describe how diagrams that dominated early drafts functioned as paper tools organizing Bohr's thinking across his pre-publication revisions. Instructive here is Klein's observation that paper to tools work in ways that are anticipated and unanticipated by their users.

[54] Ursula Klein, "Paper Tools in Experimental Cultures," *Studies in History and Philosophy of Science Part A* 32, no. 2 (2001): 265.

[55] Warwick's "theoretical technologies" provide important ground for both Klein's new term and her argument; without a sense of theoretical work as skilled, learned, and culturally contingent, Klein could not have articulated an active role for chemical labels and names, and a conception of theoretical work as disembodied, paperless and solitary would have persisted.

and, as she writes, “in unintended and unforeseen ways, the tools reacted back on the goals of their users and contributed to conceptual developments and a shift of scientific objects and practices.”[56]

Writing has sometimes been taken for granted by historians of science, particularly historians of theoretical sciences, as a space in which scientific theories are represented and dispersed and not as a culturally contingent set of practices and materials through which theory is produced and crafted. Klein's scholarship on "Paper tools" draws on a longer scholarship of work on "inscriptions, "literary technologies" and theoretical technologies," terms which have intervened in that passive conception of writing to offer historians of science ways to think about the work that writing, naming, and drawing do in the production of knowledge and the making of scientific theory.

Writing in 1979, Latour and Woolgar were two of the first to notice the amount of writing involved in scientific work and to examine writing as a part of how scientific facts are constructed. The fictional “observer” in *Laboratory Life* who has never before set foot in a laboratory is surprised by the amount of writing, both published and unpublished, in the laboratory: field notes, drafts of articles in preparation, letters between participants, memoranda, and data sheets. The “observer” is “confronted with a strange tribe who spend the greatest part of their day coding, marking, correcting, reading, and writing.”[57] As both Latour and Woolgar remind us, seeing writing takes work because writing practices are tacit knowledge, taken for granted by the actors themselves and therefore seldom announced as a part of their work. It therefore takes a concerted effort to shift one’s perspective in order to first see writing and and eventually bring it into focus as an object of analysis.

Beyond noticing the sheer volume of writing involved in scientific work, Latour and Woolgar draw our attention to how writing changes as it moves toward publication and the

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[56] Ibid.

[57] B. Latour and S. Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton Univ Pr, 1986), 49.

consequences of those changes. In Salk's laboratory, as writing moves up the publication chain from off-hand remarks to notes to draft statements, it sheds the modalities that connect it to the people and authors that created it. And by shedding their rhetorical connections to the social, these pieces of writing "achieve fact-like status."[58] Latour and Woolgar notice how after a certain point in the drafting process the social is brought up by scientists in writing and in speech only as a signal that something has gone wrong. When things are thought to have gone right references to the social construction of knowledge disappear.[59]

Though Bohr's writing is nominally single-authored, from the moment of dictation on through revision and editing, his drawings, diagrams and writing circulate in a way similar to the circulation of literary inscriptions in *Laboratory Life*. However, as I examine in greater detail in Ch. 3, in Bohr's writing the social is not reserved as a resource for signaling when something has gone wrong and the technical is not persevered as a resource for describing when things that have gone right. Rather, particularly in the early days of assembling and writing up quantum theory, the use of social modalities increases across as a set of drafts as a rhetorical strategy for embedding allies for the new theory within the published writing itself. In Ch. 3 I examine the historical contingencies at play in this particular use of social and track the changes across the drafts in a way similar to Latour and Woolgar's.

As a part of their account in *Laboratory Life*, Woolgar and Latour coin the term "literary inscription" to capture the unit of "something more basic than writing… traces, spots, points, histograms, recorded numbers, spectra, peaks" that circulate in the laboratory gaining and shedding

58 Ibid., 81–83.

59 Ibid., 22.

modalities and with them fact status.(Unattached Footnote)[60] Inscriptions have a long life in Latour's future writings in which scientific instruments will be re-figured as inscription devices, and, as one of the earliest attempts to name and theorize the work that writing does in the laboratory setting, inscriptions also inspire a small family of other terms for naming the kinds of writing scientists do including theoretical technologies and paper tools.

Building on work like Shapin and Schaffer's in the history and sociology of science that had made the practice-ladenness of experimental science visible,[61] Andrew Warwick coins the term "theoretical technology" to describe the pieces of a theoretical work, which are used to solve particular problems and which are taken for granted by members of a specific, local community.[62] As Warwick notes, "theoretical technology" is an adaptation of the material, literary, and social technologies employed by Shapin and Schaffer, and he hopes that the meaning of such technologies as "knowledge producing tools" will attach itself to his concept of "theoretical technologies" as well. According to Warwick the practice-ladenness of theory is particularly difficult to recognize and

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[60] Latour and Woolgar's "literary inscription" is taken from Jacques Derrida, *De la grammatologie*, Collection "Critique." (Paris: Éditions de Minuit, 1967) and François Dagognet, *Écriture et iconographie*, Problèmes et controverses (Paris, JVrin, 1973).

[61] Shapin and Schaffer coin "Literary technologie" to describe the ways that Boys used the page as a visual source that would evoke the experience of "virtual witnessing" in his reader. Though the use of the word "technology" to describe the material, social and literary practices and conventions is drawn from Carl Mitcham's work on techne, the purpose for the terms connects to the broader purpose of Shapin and Schaffer's project Literary technologies along with their siblings material and social technology are drawn from Mitcham's definition of techne as something that does physical work or something more closely associated with speech. Shapin and Schaffer citing Mitcham's Philosophy and History of Technology 172-175 on 21 of Leviathan}] make the craft and conventions of experiment visible they name these three new technologies.Steven Shapin, Simon Schaffer, and Thomas aeris English Hobbes, *Leviathan and the Air-Pump : Hobbes, Boyle, and the Experimental Life : Including a Translation of Thomas Hobbes, Dialogus Physicus de Natura Aeris by Simon Schaffer*, ed. Anonymous (Princeton, N.J.: Princeton University Press, 1989), 61. Literary technologies along with their siblings material and social technology are drawn from Mitcham's definition of techne as something that does physical work or something more closely associated with speech.

[62] Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905-1911: Part I: The Uses of Theory," in *Science and Society : The History of Modern Physical Science in the Twentieth Century*, ed. Michael D. Gordin, Peter Louis Galison, and David Kaiser (New York: Routledge, 2001), 633.

describe because practices are seldom referred to explicitly by historical actors. Because these theoretical technologies function as tacit knowledge, actors and scholars alike have been able to think of theory as untethered from day to day practice.[63] Two elements add a particular texture to the term Warwick introduces to help historians parse the writing that theoretical scientists do. First, Warwick's "theoretical technologies" are the technologies that are taken for granted. Second, these technologies are contingent on extremely local, cultural conditions. They can only be taken for granted by a community because all the members of that community learned them, most often this means they shared a very specific kind of training.

I share with Latour, Warwick, and, to some extent, Klein a sense of surprise at the sheer amount of writing my historical actors do. It was impossible not to have Latour's observer in my ear as I contemplated the boxes and reels of Bohr's correspondence and drafts leading to his 1913 article on the new model of the atom: "Who is this strange tribe who spend the greatest part of their day coding, marking, correcting, reading, and writing?"[64] spoke the observer as I scanned a mountain of microfilm. There is a simple but important first step made by all in defining their terms: making the writing their actors do visible. Without seeing 19th C chemical labels, the notebooks Cambridge students used to prepare for Tripos, or the Feynman diagrams in text books and their adaptations in class notes, one cannot begin to question what work these things are doing. For Latour and Woolgar making the many forms of writing in a laboratory visible takes the trick of seeing through the eyes of an uninitiated anthropological observer.

The force of arguments up until Klein had been to follow writing (inscriptions, material technologies, literary technologies) as a way of unlocking and revealing social conventions. Latour

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[63] Warwick develops "theoretical technologies" further into the term "cultures of theory" to refer to local cultures of working out problems and the highly specific and contingent use of black boards, notebooks, oral disputation and different moments set by pedagogical programs as Cambridge.

[64] Latour and Woolgar, *Laboratory Life*, 49.

examined inscriptions and modalities to understand how the social was used or rejected as a resource in fact-making. Shapin and Schaffer examined Boyle's rhetoric and literary technologies as a way of illustrating social conventions relating witnessing to successful knowledge claims. Warwick illustrated how mathematical physicists trained within the same pedagogical structures share a material culture. Klein is interested in the shared material culture of chemists in the 19th century as well, but she also traces a dialectic in which the material culture comes back to shape the experimental culture. She follows how the chemists' choice of paper tools guided the conceptual development of chemistry by making it easier to represent certain kinds of chemical reactions than others. Because Klein's "paper tools" encompass the social dimensions of writing practices and the ways that they can direct the content of scientific work, I have chosen to use "paper tools" as the central analytic for reading Bohr's practices and drafts.

While I am indebted to all of the scholarship above, I am also moving away from some of the earlier commitments of their work. Unlike the scientists in Latour and Woolgar's ethnography of a biological laboratory who are unaware of the role that writing plays in the production and communication of scientific knowledge, my historical actors are acutely aware of the importance of writing. Further, while Latour and Woolgar confine their study to the "tribes of science," following the conversations and writing of people carrying the label of scientist and placing the production of scientific knowledge within the walls of the laboratory, I follow the material practices of writing wherever they take place, revealing the integral labor non-scientists and importance of domestic spaces and institutions like family and marriage in the production of quantum theory.

The chapters are organized around writing and reading practices of Niels Bohr and the people and communities that supported and read him. After this introductory chapter, I move from dictation (Ch 2) to revision (Ch 3) and finally to reading and reception (Ch 4). Within each chapter, I reconstruct the writing practices themselves from oral histories, archived drafts, photographs, and personal and professional correspondence and explore how specific aspects of quantum theory and Bohr's other works were contingent upon each practice.

In the second chapter "Dictating the Atom: Margrethe Bohr and the Construction of an Ideal Reader" I reconstruct Bohr's practice of using dialogue with a non-scientist to produce first drafts. These dictation/dialogue practices co-evolved with Bohr's ideals for scientific communication. This pattern of dictation helps explain the value Bohr placed on communicating to non-experts and also became the way to practice that value; he guaranteed the intelligibility of his ideas to non-experts by having his ideal reader in the room with him and by speaking directly to her. Furthermore, the particular form of Bohr's dictation practices persists in the themes and syntax of his work. Echoes of his dictation practices are particularly present in the way that he defines the responsibilities of authors and readers in the creation of the meaning of a text.

The third chapter "Revising the Atom: Atomic Orbits and Metaphoric Haunting" reconstructs Bohr's revision and re-writing practices and the revision and editorial practices he enforced at his institute in Copenhagen. By bringing together two communities of scholars that often talk past one another, literary theorists, scholarly editors, and historians of science, I argue for a new, cinematic way of reading scientific writing, which privileges neither the published version nor the original moment of discovery, situating the meaning of a work in the process not the final product. By reading Bohr's crucial 1913 article and preceding manuscripts cinematically, we see both how changes made in revision reveal debates and thematic commitments alive in broader community of physicists and the consequences that the mechanics of a particular revision practices have on the finished work[65]

To study Bohr's revision practices, this chapter uses the full set of drafts from preliminary, handwritten notes to the printed published article of Niels Bohr's "On the Constitution of Atoms" published in *Philosophical Magazine* in the spring of 1913 as a case study. This set of drafts is a representative but contained example of Bohr's approach to revision. In many ways the drafts of "On the Constitution of Atoms" are typical: they showcase the stages of his writing practices and Margrethe's involvement in the writing, especially early in his career and their marriage. Across them we can track the kinds of revisions Bohr made at different stages of writing and some of the consequences of these changes. The roles of correspondence, dictation, face-to-face communication are representative here as are the form and organization of his notes. At the same time, as a case, the 1913 drafts are more vulnerable to analysis because the writing and revision process is less sprawling. There are tens of drafts instead of hundreds. There are fewer interlocutors and correspondents involved than in Bohr's later works produced after he had founded and was administering an institute full of other scientists.

As the article that first introduced Bohr's model of the atom, with an electron that moved in quantum jumps between electron orbits or stationary states, "On the Constitution of Atoms" is also an exceptionally important text in the history of 20th-century physics. It represents the work for which Bohr eventually won a Nobel prize in 1922. If an examination of revision practices can provide new insights into a model that is not peripheral or tangential to physics, but right at the center of what counts and is celebrated as scientific discovery, we should be persuaded of the usefulness of understanding writing practices as a part of scientific practice more broadly.

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65 Daniel Ferrer, *Logiques Du Brouillon: Modèles Pour Une Critique Génétique.* Poétique. Paris: Éditions du Seuil, 2011.

The fourth and final chapter "Reading the Atom: Reception as a Foundation for New Writing" examines readers' reception of quantum theory. By closely examining the routes through which Bohr's theories reached his readers, I reveal the importance of informal networks, correspondence and conferences in the spread of quantum theory in the second decade of the 20th century. I describe how Bohr's awareness of his own reception feeds back into his writing practices through administrative systems for tracking readers and their responses, records that form the basis for new writing. On another level, Bohr's awareness of his reception refines his understanding of who an ideal reader is and how they can and should read.

Figure 2: Niels and Margrethe Bohr riding George Gamow's motorcycle at their summer home in Tisvilde, a site of both work and play. Archive of the Niels Bohr Institute, Copenhagen, courtesy AIP Emilio Segre Visual Archives.

CH. 2: DICTATING THE ATOM: MARGRETHE BOHR AND THE MAKING OF AN IDEAL READER

I didn't work with him; I only worked as his typist — writing for him.
-Margrethe Bohr, interview with the Archive for the History of Quantum Physics, January 1963

After a preliminary rough copy the great work of further elaboration began. This always took a long time and the various rough copies were taken across to my mother, without whose approval it could not be completed.
-Hans Bohr, Margrethe Bohr and Niels Bohr's son, writing about his father[1]

Let us take for a moment these two descriptions of Margrethe Bohr's role in her husband, Niels Bohr's, writing and working life. In the first, Margrethe Bohr carefully describes herself as a "typist" closing off any space for her work and his to be characterized as collaborative. Later in the same interview she makes even clearer that her writing came after Niels Bohr's thinking was already complete, "He dictated whole papers, not notes. I think he had it fairly well prepared in his head."[2] Margrethe Bohr's description of the process of taking dictation for her husband forms a strong contrast with the descriptions offered by Niels Bohr's physicist peers, colleagues, and students of their own roles writing for him. While Bohr's colleagues and students walk a careful line, giving Bohr full credit for his own ideas while also preserving space for their role as a sounding board or other kind of contributor, Margrethe Bohr's description is, at first glance, both less complicated and more self-effacing.[3] The second account of Margrethe Bohr's writing work is from a short essay by Hans Bohr, Margrethe and Niels Bohr's son. He describes a writing process where Niels Bohr's thinking evolves over a "long time" and where Margrethe Bohr's opinion and "approval," not just

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[1] Stefan Rozental, *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (North-Holland, 1967), 333.

[2] Margrethe Bohr, Aage Bohr, and Leon Rosenfeld, interview by Thomas Kuhn, January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4514_1.html.

[3] Nearly every published recollection of Bohr contains some reference to his writing habits with each author offering a slightly different explanation of what it meant to write for/with Bohr. A few examples: Writing in Rozental's edited volume, Leon Rosenfeld describes his role as a "helper" offering "approbation or dissent." He also describes how Bohr liked to work with a "partner." Rozental, *Niels Bohr*, 119. Rozental describes the role as a "sounding board." Ibid., 23.

her typing skills, are valued and required at every stage. Hans Bohr's account of Margrethe Bohr's work is echoed in the recollections of colleagues like Oskar Klein, who spent a great deal of time with the Bohr family, and it is also corroborated by the archival drafts of Niels Bohr's writing, in which Margrethe's handwriting is visible throughout the writing process, often on final drafts.

There are many registers on which the particularities and longevity of Niels Bohr's writing practices reverberate through his own work and the work of others. Niels Bohr's dictation practices shaped and were shaped by the priority he placed on the use of ordinary or plain language. Margrethe and Hans Bohr's accounts, or the space between them, provoke the questions for this chapter. How and to whom did Niels Bohr dictate? Why did he choose to dictate to Margrethe Bohr? What were the implications of that choice for the quantum theory he produced?

After a summary of important conversations on dictation in the secondary literature, the first part of this chapter reconstructs Niels Bohr's dictation practices from the existing sources. I describe how and to whom Niels Bohr dictated and how his dictation practices changed over the course of his life and career. The second part of this chapter explores how Niels Bohr's dictation practices and his philosophy of scientific communication were co-constitutive. Niels Bohr's ideals for scientific communication, his ideas about whom scientific ideas ought to be accessible to, were the focus of writings throughout his life and determined the register in which he wrote even highly specialized papers. These ideals were shaped by his own experiences working with amanuenses, and, at the same time, his choices of amanuenses were shaped by these ideals. Niels Bohr's development of complementarity, conventionally described as a response to a purely physical dilemma, can be fruitfully understood as an idea rooted in his dictation practices. Finally, this chapter considers how Bohr's amanuenses are characterized in the secondary literature and biographical treatments of Niels Bohr. Even though dictation played a central role in the form and content of Niels Bohr's work there has been a move in the construction and maintenance of Bohr's legacy to move the people who took dictation for him to the margins. I will describe both the consequences of their marginalization and some of the ways the history of this period of quantum theory changes once we pull them back into the room.

Dictation, Collaboration, and Credit

We routinely refer to a single authorial mind, or personality, or consciousness to validate "meaning" or "authority" where others besides the nominal author have a share in the creation of a text, we usually ignore that share or call it corruption and try to get rid of it.
— Jack Stillinger *Multiple Authorship and the Myth of Solitary Genius* (vi)

Pinning down exactly what taking dictation involves is always difficult, and this is especially true in the case of Niels Bohr. Amanuenses, especially female amanuenses working for male authors, are sometimes imagined as unmediating tools, but they do mediate.[4] Dictation practices come to be defined by the individuals taking part in a given working relationship, but they are also subject to broader conventions. Dictation has been practiced since the very earliest forms of writing, and like other reading and writing practices is highly historically contingent.[5] An important contextual element for Niels Bohr's dictation practices was the rise in female secretarial labor, and the schools in which secretaries were trained. In Britain, female office workers increased eighty fold between

[4] See Pamela Thurschwell, *Literature, Technology and Magical Thinking, 1880-1920*, ed. Anonymous, Cambridge Studies in Nineteenth-Century Literature and Culture ; 32 (Cambridge ; New York: Cambridge University Press, 2001), 90.

[5] A helpful overview of ancient, early modern and modern dictation practices can be found in Guglielmo Cavallo, Roger Chartier, and Lydia G. Cochrane, *A History of Reading in the West*, Studies in Print Culture and the History of the Book (Amherst: University of Massachusetts Press, 2003). See also Paul Saenger, "Silent Reading: Its Impact On Late Medieval Script Society," *Viator* 13 (1982): 367–414 on the movement from dictation and oration to silent writing and reading in late medieval period.

1850 and 1914, growing from two to twenty percent of the total number of British clerical workers[6]. Similar increases occurred in Denmark, and the United States as well. While neither Bohr's first amanuensis, his mother, Ellen Bohr, nor his wife Margrethe Bohr attended secretarial school, Bohr's first hired secretary, Betty Schultz, had attended secretarial school and learned to take dictation in shorthand. Dictation was widespread among Bohr's colleagues in graduate school, and Margrethe Bohr initially took dictation for Bohr and his colleagues before working exclusively for her husband.[7]

Dictation has become a site for historians and literary theorists to sharpen their questions about authorship and credit. Scholars continue to grapple with how Foucault's understanding of the role of an author in determining a relationship or sense of homogeneity between texts is complicated by the practice of dictation.[8] While some have argued that dictation enhances an author's control over his or her text, others have argued the opposite, that using an amanuensis interrupts the relationship between author and text or introduces additional authors into the mix. For scholars who approach dictation with the latter perspective, there is a sense that using an amanuensis might redistribute credit or sully an original and authentic text. This kind of work is often motivated by a desire to establish the authenticity of a text and to understand who is the unambiguous author. At

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[6] Robert J. Griffin, *The Faces of Anonymity : Anonymous and Pseudonymous Publication from the Sixteenth to the Twentieth Century* (New York: Palgrave Macmillan, 2003), 2-3.

[7] "I am transcribing for Niels Erik and am now also beginning to transcribe for Niels—yes, he is very busy during this fortnight, an he and Harald are talking about it, for he would like to write it so that Harald can fully understand it, otherwise it will be too difficult to understand for others." "Saa skriver jeg af for N.E. og begynder nu ogsaa for Niels—ja, han har meget travlt i disse 14 Dage, og han og Harald snakker sammen om det, for han vil gerne skrive det saadan, at Harald helt kan forstaa det ellers bliver det altfor vaskeligt at forstaa for andre." MN to SN 15, January 1911, Niels Bohr Archive. Also cited in Finn Aaserud and J. L. Heilbron, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013).

[8] Robert J. Griffin, *The Faces of Anonymity : Anonymous and Pseudonymous Publication from the Sixteenth to the Twentieth Century* (New York: Palgrave Macmillan, 2003); Michel Foucault, "What Is an Author?," in *Language, Counter-Memory, Practice : Selected Essays and Interviews* (Ithaca, N.Y.: Cornell University Press, 1977), 240.

stake in Paul Bertagnolli's article about the Liszt-Raff collaboration, for example, is the "integrity" and "originality" of work that was produced using an amanuensis. Bertagnolli seeks to defend Liszt from accusations that his work is somehow not his own that begin in 1898 and stretch through to the present in biographical literature.[9]

Working within a smaller scholarship that sees dictation as something that enhances an author's control over their text Jennifer Douglas argues that women who used amanuenses during the 15th century had greater control over the content of their writing than women who did not.[10] Between these two poles, Harold Love understands dictation as one of many possible technologies of writing or reproduction. Love argues that an author's choice of a particular writing technology usually involves "a surrender of control over some aspects of a finished text." While he acknowledges that dictation blurs or changes an author's relationship to their work, he maintains that this same process happens with many other modes of textual production and reproduction.[11]

Douglas also notes that there is a gendered dimension to whether dictation is interpreted by historians as something that weakens or strengthens an author's claims to their work. In her view, scholars have raised questions about what the use of amanuenses means for women writers[12], but male authorship is rarely seen to be complicated, compromised or enriched by the use of an

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[9] Paul A. Bertagnolli, "Amanuensis or Author? The Liszt-Raff Collaboration Revisited," *19th-Century Music* 26, no. 1 (Summer 2002): 23–51.

[10] Jennifer Douglas, "'Kepe Wysly Youre Wrytyngys': Margaret Paston's Fifteenth-Century Letters," *Libraries & the Cultural Record* 44, no. 1 (2009): 36.

[11] Harold Love, *Attributing Authorship : An Introduction* (Cambridge, U.K. ;: Cambridge University Press, 2002), 34.

[12] See for example James Daybell's excellent argument about how male secretary's help women letter writers to strengthen their arguments in Early Modern England. J. Daybell, *Women Letter-Writers in Tudor England* (Oxford University Press, USA, 2006).

amanuensis.[13] So while women in the 15th century using male scribes are more vulnerable to historians raising questions about the extent of their control of the works that they write, these sorts of questions are rarely raised about male figures dictating to female secretaries.

The fact that dictation is often understood as a practice that dilutes, tarnishes or redistributes credit, has constrained engagement with Niels Bohr's dictation practices. Across historical actors' accounts, and the commemorative and scholarly literature that makes mention of Niels Bohr's dictation practices, there is a hesitation to spend too much time on these practices or engage with them too closely. Margrethe Bohr's self-effacement in the interview with Niels Bohr at the start of the chapter, Thomas Kuhn's framing of Margrethe Bohr's role as "typist", and Bohr's secretary, Betty Schultz's, resistance to describing her role as changing or growing in any way over time, are all examples of ways that substantive descriptions and engagement with Niels Bohr's dictation practices have been avoided.[14] In this evasive maneuvering around the collaborative nature of Niels Bohr's dictation and revision practices, I read a concern that his dialogical working and writing style will undermine his legacy by calling into question the credit he deserves for quantum theoretical discoveries and breakthroughs. An emphasis on solitary writing practices in the secondary literature is part of the more general concern to portray Niels Bohr as an equal to Albert Einstein. Einstein pre-deceased Bohr by seven years and the framing of Einstein's accomplishments as the efforts of a solitary genius applied pressure to historical actors, memoirists, and historians who were keen for

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[13] Douglas puts forward evidence that in the case of Margaret Paston, use of an amanuensis allowed them greater control over their correspondence than their own writing would have.

[14] Interview with Margrethe Bohr by Aage Bohr, Leon Rosenfeld and Thomas Kuhn, January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4514_1.html and Interview of Betty Schultz on May 17, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4866.html.

Bohr to be read as Einstein's equal, to de-emphasize the collaborative elements of his writing and working practices, including dictation.

In my approach to dictation I am not interested in using Niels Bohr's dictation as a way to redistribute credit or better understand whether his work was fully his own. Questions about who deserves credit tend to eclipse and undermine my own questions about how dictation was practiced and how artifacts of this practice echo through the writing that was produced. Attempts to draw lines between dictation and collaboration pigeon hole the working relationship in a way that become an obstacle to a fuller and more nuanced description I hope to achieve here.

As a way of stepping out of conversations about dictation, attribution, and credit, I rely on an understanding of authorship closely aligned with Jerome McGann's socialized concept of literary production and Jack Stillinger's "collaborative authorship." McGann argues that literary production is almost never autonomous but rather social and institutional.[15] Most texts assumed to have one author, including Niels Bohr's single-author published writing and letters bearing just his signature, are in fact co-authored. Building from McGann's socialized production of literary texts, Jack Stillinger defines authorship expansively in a way that includes "the nominal author and a friend, a spouse, a ghost, an agent, an editor, a translator, a publisher, a censor, a transcriber, a printer, or--what is more often the case-- several of these acting together or in succession."[16] Within this framing, all texts are collaborative in certain ways, and the idea of autonomous authorship, especially autonomous authorship as a criterion for or measure of credit, emerges as a myth. And, as Stillinger argues, the questions which have occupied scholars about the death or persistence of the author can be more productively framed as "How many authors are being banished from the text or

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[15] Jerome J. McGann, *A Critique of Modern Textual Criticism* (Chicago: University of Chicago Press, 1983), 100.

[16] Jack Stillinger, *Multiple Authorship and the Myth of Solitary Genius* (New York: Oxford University Press, 1991), v.

apotheosized in it?"[17] Translated for this project, Stillinger's question becomes an invitation to look for signs or traces of the Margrethe and other amanuenses being banished or preserved in Bohr's drafts and published writings.

Niels Bohr's Dictation Practices

I will certainly with the greatest pleasure be your pen, my dear boy.
— Ellen Bohr writing to her son Niels in 1910

Throughout his life Niels Bohr dictated whenever possible to a non-scientist amanuensis. Bohr dictated personal and professional correspondence, talks for conferences, and scientific papers and articles. During grade school and high school (over the protests of his father) he dictated to his mother, Ellen Adler Bohr, who felt that he could not work in any other way. His master's thesis was dictated to his mother and to his brother, Harald Bohr. Parents serving as amanuenses for their children was not common practice, and Ellen Bohr did not take dictation for her other son, Harald during this time period. After Niels became engaged to Margrethe Nørland in 1910, she immediately began taking dictation for him. It is evident from the correspondence between Niels Bohr, Margrethe Nørland and their families during their engagement that an integral part of their courtship and Margrethe's acceptance into the Bohr family involved a shared understanding of what her role would be writing for Niels, and the service she would perform in supporting Niels' intellectual life. Finn Aaserud reads this time in their courtship as the "tuning of Margrethe to be a sounding board to replace Niels' mother and anticipate his assistants."[18]

Shortly after Margrethe and Niels Bohr's engagement Margrethe became ill and Bohr's mother Ellen wrote to the pair urging Margrethe to postpone her move to Copenhagen, reassuring

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[17] Ibid.

[18] Finn Aaserud, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013), 17.

them both that Margrethe "need not be afraid that during this period you (Niels) will not make progress on your doctoral thesis, for I will certainly with the greatest pleasure be your pen, my dear boy."[19] Ellen Bohr's letter makes clear that from even before they were married, it was understood by Niels, Margrethe, and the Bohr family that Margrethe Nørland would take over writing for Bohr, a role that had been performed most consistently by Ellen Bohr up to 1910. Still convalescing from illness in 1910 in Copenhagen, Margrethe Nørland expressed to Ellen Bohr some ambivalence about ceasing her education and other possible professional goals to marry and write for Bohr. Margrethe was one year short of a degree in home economics and had been receiving high marks.[20] She evidently understood that a marriage to Niels Bohr would require her personal and professional devotion and hesitated while he was away completing his doctoral dissertation. Ellen Bohr sought to reassure her Margrethe that she was the perfect person to support her son's intellectual life. "Wisdom is not the amount of knowledge but the understanding of and love for the value of intellectual work, which is so rarely valued because so few know what it requires of strength, diligence and unselfish striving, but you and I know it, and Father and I have never been worried that you would not understand our dear boy."[21] In her communication with Margrethe Nørland, Ellen Bohr articulates the remarkable domestic supports required for intellectual life, the role that the institution of marriage plays in intellectual work.

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[19] Ellen Bohr to Niels Bohr August 24, 1910, Niels Bohr Archive."at bruy et lille Offer for hinandeus Skyd… Hun be høver ikke at vaere burge fur, at Du iden Tid ikke skal komme vidt med Din Doktora fhandling, for jeg skal nok med størst Glaedevaere din Pen min Dreng."

[20] Finn Aaserud, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013), 18.

[21] Ellen Bohr to Margrethe Nørland December 10, 1910 Niels Bohr Archive, as cited by Finn Aaserud, *Love, Literature, and the Quantum Atom : Niels Bohr's 1913 Trilogy Revisited*, First edition. (Oxford: Oxford University Press, 2013), 19.

Niels Bohr's own correspondence with Margrethe Nørland also emphasizes the writing she will be doing for him. Writing to her from the coastal retreat of Hornbaek where he went to "be alone with [his] electrons" and complete his doctoral dissertation he hopes, "next time I write something that I find difficult, then we will perhaps be married and then we will perhaps go out to Hornbaek or some other place together and write together. I have been thinking of so many wonderful things these days."[22] Bohr's fantasies about their marriage involve traveling to remote places and having Margrethe write for him. Notice that from the start here he does not characterize this work as dictation, but as "writing together." In other early letters he explains the value of her conversation and point of view as he writes his ideas.

Niels and Margrethe did marry in 1911, and shortly after, on their honeymoon in fact, she began the work of writing with him and the administrative work of keeping track of his drafts and correspondence. Margrethe wrote with Bohr the three major articles redefining the structure of the atom in 1912 and 1913.[23] Later in 1919, he hired Betty Schultz who served as his secretary at the Institute for Theoretical Physics in Copenhagen and took dictation for him until his death in 1962. Bohr also dictated to younger colleagues and assistants, but Margrethe Bohr and Betty Schultz took dictation for him most consistently over the longest period of time.

When asked how long Niels Bohr had been dictating and to whom, Margrethe Bohr described her role as similar to the role that his mother played before her and the one that Betty Schultz, Niels Bohr's secretary at the Institute took on after her.

> As long as he had any independent work which he was making himself. He dictated to [his mother] his whole doctor's dissertation. In any case, until I came into it, she always did it. And my mother-in-law told me that my father-in-law always said, "You mustn't help Niels

[22] Niels Bohr to Margrethe Nørland, May 23, 1911, Niels Bohr Archive.

[23] Margrethe Bohr was also heavily involved in the administrative work of promoting and sharing Niels Bohr's work. As will be discussed in greater detail in Ch. 4, Margrethe Bohr mailed out 300 copies of Bohr's dissertation to colleauges in Denmark and beyond.

> so much; you must let him learn to write himself." And then she said, "But it was no good because he could not." He could work in this way, and he could not any other way.[24]

Niels Bohr continued to work with Margrethe Bohr or Betty Schultz, even as he incorporated colleagues and students into his writing process. After she was hired, Betty Schultz stayed with Niels Bohr for the rest of his life.

Though I have been using the word "dictation" as an approximation for the way that first drafts were generated, descriptions of this process range from dictation to collaboration depending on who is doing the recounting. Dictation, in Bohr's case, is clearly not the same as straightforward recording and transcription of speech. In fact, recording machines became available during Bohr's career, but he repeatedly refused to use them, claiming that they did not provide the same service at all. As we will come to see, what he was looking for was not the simple recording of and transcription of his voice, but something else. Something more and less.

People who occupied this role described it as everything from a typist to a "sounding board" that responded to Bohr's ideas and asks questions[25] to being a "victim" or younger physicist who Bohr pulled aside and worked with on a particular problem.[26] The differences between these descriptions are significant, and the diverging accounts pose one major set of challenges to attempting a thick description of Bohr's dictation practices. Another set of challenges emerges from how little Margrethe Bohr and Betty Schultz write or speak about their work. The majority of Bohr's first drafts during this period are in the handwriting of his wife, Margrethe Bohr, or his longtime

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24 Interview of Margrethe Bohr by Thomas Kuhn, Aage Bohr and Leon Rosenfeld on January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4514_1.html.

25 S. Rozental, *Niels Bohr : Memoirs of a Working Relationship* (Copenhagen: Christian Ejlers, 1998), 23.

26 P. J. Kennedy, Niels Henrik David Bohr, and A. P. French, *Niels Bohr : A Centenary Volume* (Cambridge, Mass.: Harvard University Press, 1985).

secretary, Betty Schultz, but the availability of accounts of dictation in the form of oral histories, memoirs, and correspondence are the inverse of this; accounts of writing for Niels Bohr by his male colleagues are far more plentiful than descriptions offered by Margrethe Bohr or Betty Schultz. Toward the end of this chapter the divergence of accounts and the absence of Margrethe Bohr and Betty Schultz's descriptions of their own work will become a resource for thinking about how women's writing work is rendered in the existing primary and secondary literature and what the patterns and consequences of that rendering might be. To work around the absence of Margrethe Bohr and Betty Schultz's own accounts of taking dictation, oral histories by male colleagues and assistants, photographs, memoirs of the Bohr family children, photographs, and the dictated drafts themselves form the source base for reconstructing a sketch of what happened during dictation session.

Preceding the piles of drafts and the endless revisions there was a first draft, almost always generated through a dictation/dialogue process that involved Niels Bohr pacing, dictating, mumbling, and question asking around an amanuensis who sat in the room with him. Niels Bohr dictated to family members, to fellow scientists, to non-scientist secretaries, and to his wife. Though he dictated first drafts throughout his life, Margrethe Bohr describes the period during which he relied almost exclusively on dictation as beginning with his doctoral thesis in 1909 and ending in the 1930s, after which point he did some of his writing on his own.[27] The archives bear this out with almost no first drafts written in Niels Bohr's own hand from 1911-1930, though his handwriting is still present in line edits, extensive notes between drafts.

Paul Forman in his interview with Niels Bohr's longtime assistant Betty Schultz draws our attention to some of the particularities of these dictation sessions:

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[27] Interview of Margrethe Bohr by Thomas Kuhn, Aage Bohr and Leon Rosenfeld on January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4514_1.html.

Forman: And then there was the way he worked-- always having to speak to someone while he worked.

Schultz: Yes, but I couldn't compare it with anything else, because I had never worked for a scientist before.

Forman: But didn't it somehow seem strange that it all came out as he talked, and that otherwise---. I mean, didn't you imagine that he might spend his time sitting thinking in his office and then writing this up?

Schultz: Yes, it was something quite other than what I was accustomed to seeing. But he went round and round and that made it difficult to hear what he said.

Petersen: Did he speak as softly then as later?

Schultz: Very softly and he spoke with his boots squishing all the time. And the streetcars disturbed. But nevertheless I wish we had him still here.[28]

From many of the accounts of dictation we learn that Niels Bohr spoke quietly and was hard to hear. Like Betty Schultz, Abraham Pais recalls that Niels Bohr's voice was as quiet in his private speaking as he was in his public speaking.[29] The layout of the room and Niels Bohr's movement within it is consistent across the descriptions. The room, whether it is Niels Bohr's home study, a neighbor's painting studio near the Bohr vacation home in Tisvilde, an office at his own Institute is staged the same: a central table, a chalk board, a person taking notes sitting in a chair, and Niels Bohr pacing in circles around the note taker.

Amanuenses or colleagues usually describe these sessions as generative. Niels Bohr had no notes prepared ahead of time, and the early part of the sessions were mumbled, often with repetition. Or in Forman's words: "[I]t all came out as he talked." Pais describes writing for Niels Bohr in Einstein's office at the Institute for Advanced Study

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28 Interview of Betty Schultz on May 17, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4866.html.

29 Abraham Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, ed. Anonymous (Oxford; New York: Clarendon Press; Oxford University Press, 1991), 11.

> Bohr wanted me to come down to his office and talk. We went there, and it should be explained that Bohr at that time used Einstein's own office in Fuld Hall. At the same time, Einstein himself used the adjoining small assistant's office; he had a dislike of the big one, which he did not use anyway. After we had entered, Bohr asked me to sit down and soon started to pace furiously around the oblong table in the center of the room. He then asked me if I could note down a few sentences as they emerged during his pacing. It should be explained that, at such sessions, Bohr never had a full sentence ready. He would often dwell on one word, coax it, implore it, to find the continuation. This could go on for several minutes.[30]

Pais description is exceptional only in its setting. Other descriptions of writing for and with Bohr describe the mumbling, the pacing, the repetition of a single word to generate a sentence.[31] Betty Schultz, who was among the people who wrote most consistently for Bohr over the longest period of time, describes in her oral history with *AHQP* how dictation usually took place after the standard work day was complete sometimes in Niels Bohr's office at the Institute but more often in his home study. After dictation was complete they would talk about other things: Schultz's belief in life after death and Bohr's denial of it. This was over tea (chamomile) and often sweets.

Most often invoked is his handwriting, which was notoriously illegible. According to Rozental, "NB did not like to do writing himself. Private letters were written by hand and everything else was dictated. He wrote slowly and for anyone not used to his handwriting, it was often difficult

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30 Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity,* 13.

31 Almost every oral history or memoir written about Bohr's dictation and lecturing habits comments on his soft voice. Mogens Andersen describes straining to hear Bohr's mumbling. Richard Courant describes a lecture of Bohr's, "It was a typical Bohr lecture such as we all have experienced so often, excitingly inspiring, though neither acoustically nor otherwise completely understandable." (302) Otto Frisch describes a scene very similar to Weiskopff's but set instead in a hired painters studio near Bohr's vacation home in Tisvilde: "With some writing paper and a pencil in front of me I was placed at a table around which Bohr wandered, alternately dictating in English and explaining in Danish, while I tried to get the English text on to paper. Sometimes there were long interruptions either for pondering over waht was to follow, or because Bohr had thought about something outside the theme which he had to tell me about. Thus, once, in connection with some criticism of Einstein's general relativity theory presented to him by Helg Holst, he worked out a detailed discussion of the so-called clock paradox such as it would occur on a straight line journey out into space and back again. Often, also, work was interrupted by short running trips or cycling to the shore together with the family for bathing." S. Rozental, *Niels Bohr : Memoirs of a Working Relationship* (Copenhagen: Christian Ejlers, 1998).

to decipher."[32] Rozental even asserts that besides "a few personal letters and short notes" there are no manuscripts in Niels Bohr's own hand. In his biography of Bohr, Pais recalls that although "Bohr devoted tremendous effort and care to the composition of his articles… to perform the physical act of writing, pen or chalk in hand, was almost alien to him. He preferred to dictate… On one of the few occasions that I actually did see him write himself, Bohr performed the most remarkable act of calligraphy I shall ever witness." Bohr went to the blackboard and wrote down an outline for the discussion. Pais remembers the word harmony as looking something like this:

Later in the discussion, "Bohr became dissatisfied with the use of harmony. He walked around restlessly. Then he stopped and his face lit up. 'Now I've got it. We must change harmony to uniformity.' So he picked up the chalk again, stood there looking for a moment at what he had written before, and then made the single change:

[32] Rozental, Niels Bohr: Memoirs of a Working Relationship, 34.

with one triumphant bang of chalk on the blackboard."[33] The illegibility of Niels Bohr's handwriting became a joke during his time at the Los Alamos Laboratory during WWII when it was unclear to his colleagues whether he was signing his true name to letters or signing "Uncle Nick" a nickname for the assumed name, Nicholas Baker, assigned to him during the war.

The collaboration and dialogue that preceded the first written drafts and was returned to between drafts is often interpreted in biographies and memoirs to be an extension of Bohr's mentoring or even fathering of colleagues and younger physicists. In these sessions he is remembered as providing guidance while also using conversation to "help clarify his own thinking." Pais writes, "And Bohr (contrasted here with Einstein), always in need of other physicists, especially young ones, to help him clarify his own thoughts, always generous in helping them clarify theirs, not so much a teacher of courses nor a supervisor of Ph.D.'s but forever giving inspiration and guidance to so many engaged in post-doctoral and senior research, father figure extraordinary."[34]

Finally, there is the explanation, particularly from Margrethe Bohr, that Niels Bohr could not work in any other way. "In his younger days he had so much in his head that just had to be put down, and he could concentrate while he dictated."[35] Margrethe recalls her mother-in-law, Niels Bohr's mother, being told by her father-in-law, "'You mustn't help Niels so much; you must let him learn to write himself." And then [her mother-in-law replying], 'But it was no good because he could not.' He could work in this way, and he could not any other way."[36]

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[33] Abraham Pais, *Niels Bohr's Times: In Physics, Philosophy, and Polity* (Oxford: Clarendon Press, 1991), 10.

[34] Ibid, 3.

[35] Interview of Margrethe Bohr by Thomas Kuhn on January 23, 1963, Niels Bohr Livrary & Archives, American Institute of Physics, College Park, MD, USA, http://www.aip.org/history/ohilist/4514_1.html.

[36] Ibid.

Though Niels Bohr's handwriting was indeed often illegible, even to himself, he was able to write in his own hand, and there are many examples of him doing so, especially before 1913 and after 1930. Bohr's dictation was not an accident of bad handwriting or an inevitable habit formed in childhood, but a choice for how to write that had broad consequences for the content and form of the physics he and his colleagues produced. His dictation practices influenced and were influenced by ideas about an ideal readership for quantum theory and negotiations about truth and descriptions of nature at the core of quantum theory.

Thematic Consequences of Dictation: Fantasies about Communication

Pamela Thurschwell shares my interest in describing how dictation practices come to be determined and imagined by both author and amanuensis. Thurschwell follows the relationship between author and amanuensis, or the author's imagination of this relationship, to see what kinds of traces it leaves in the writing itself. In *Literature Technology and Magical Thinking,* Thurschwell describes Henry James' working relationships with his amanuenses, and how his anxieties about communication with his secretaries manifest as themes in his stories. The descriptions of language and writing in his stories carry traces of the particular dictation relationship he had—or imagined he had—with his amanuenses.[37]

Working from Henry James' secretaries diaries and the stories produced through their dictation work, Thurschwell looks at how the particularities of James' attitudes toward these women and their work, and their own attitudes toward their work manifest as themes in James' stories. Thurschwell argues that Henry James stories move between two fantasies about communication. First, communication as telepathy, a direct, unmediated mind-to-mind transfer, relying on intimacy

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[37] Pamela Thurschwell, *Literature, Technology and Magical Thinking, 1880-1920*, ed. Anonymous, Cambridge Studies in Nineteenth-Century Literature and Culture ; 32 (Cambridge ; New York: Cambridge University Press, 2001), 91.

where words should not even be necessary. And, second, a conception of language as material objects, as "things to be counted."[38] She tracks how these same two fantasies about communication structure his relationships with two different secretaries/amanuenses, Mary Weld and Theodora Bosanquet, who have very different ways of thinking about and recording their work in their diaries and memoirs. How the relationship between the author and the amanuensis is imagined has great bearings on how language is imagined in James stories. James' descriptions of language and writing bear the stamps of his particular dictation relationship with his secretaries.[39] She goes on to think about some of the origins of these fantasies, naming the rise of telegraphy and the slippage between telegraphy and telepathy as two places James draws from for his understandings of communication. Niels Bohr's fantasies about communication, fantasies quite different from James', structure his writing practices and his writing practices, particularly his dictation practices, structure his fantasies about communication. There are a range of ways in which Bohr's understanding of the experience of dictation get taken up and reflected through his writings. Bohr's ideals for scientific communication were in constant dialogue with his dictation practices. To make his ideals for scientific communication visible I rely first and foremost on Bohr's published writings on authorship, reading and communication.[40] Beyond Bohr's published writings, I also look to books that Bohr gave as gifts throughout his life for clues about his understanding of the possibilities and limitations of scientific communication and use his frequent meditations on art and the relative

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38 *Ibid*, 93.

39 *Ibid*, 91.

40 A similar source base has been used by other scholars to make arguments about an author's motivations in their own writing process. Bertagnolli, for example, uses Listz's published views on authorship as one way of understanding his motivations in his own writing process. Paul A. Bertagnolli, "Amanuensis or Author? The Liszt-Raff Collaboration Revisited," *19th-Century Music* 26, no. 1 (Summer 2002): 23–51.

responsibilities of viewers and painters as an analogy to his understanding of the responsibilities of authors and readers.

Plain Language as a Foundation and Legacy of Dication

Scientific communication is a theme of many of Niels Bohr's talks and writings, but he takes it up most explicitly in the early 1930s with *Atomic Theory and the Description of Nature*, a collection of essays geared toward a popular, non-physicist audience. The introduction of the book reads as a defense of the use of plain language for the communication of quantum theory. Plain language is, for Niels Bohr, the language of ordinary experience, of the non-scientist. The challenge Bohr encountered was a perceived fit between classical, 19th century physics and plain language and a fundamental misfit between quantum theory and plan language. Or as he wrote in *Atomic Theory and the Description of Nature*, plain language was based on idealizations that did not hold true on the tiny quantum scale. For Niels Bohr then, the challenge became to use a medium, plain language, that was constantly suggestive of and formed by another experience to explain phenomena that were outside of that experience. In spite of these difficulties, he remained committed to plain language: "In appraising this situation," he wrote, "however, we must not forget that, in spite of their limitation, we can by no means dispense with those forms of perception which colour our whole language and in terms of which all experience must ultimately be expressed." Later he repeated again that the use of plain language was vital, "Indeed, the recognition of the limitation of our forms of perception by no means implies that we can dispense with our customary ideas or their direct verbal expressions when reducing our sense impressions to order."[41]

The women who took on the role of taking dictation for Niels Bohr were outsiders to the world of theoretical physics that Bohr created at Copenhagen. They did not have advanced degrees

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[41] Niels Henrik David Bohr, *Atomic Theory and the Description of Nature* (New York, Cambridge, Eng.,: The Macmillan Company; The University Press, 1934), 5.

in physics or any other related field. This was an intentional move on Bohr's part, meant to help satisfy his desire to express science in plain language accessible to ordinary readers.[42] Betty Schultz describes her outsider status as a specific criteria, in fact the *only* criteria, for her hire, "I had heard that he wanted a secretary and I prepared the whole way what I was able to do. I took shorthand and knew a little English, and such things, but when I came there, he didn't ask for anything except whether I had been interested in science. And I said, 'No I do not know what it is,' and then I was engaged.[43]" When Margrethe Bohr needed to step away from the time intensive role of taking dictation for Bohr in order to parent five sons, Bohr's single criteria in replacing her was to find someone who was also an outsider to physics, a way of insuring that his ideas would continue to be formed and communicated in ways that would be intelligible to non-scientists.

The role of Margrethe Bohr and Betty Schultz in securing the intelligibility of Quantum Theory to non-scientists is made perhaps even more powerfully by Niels Bohr's repeated insistence that his colleagues re-write something so that Margrethe Bohr would be able understand. Margrethe Bohr, sometimes the real Margrethe Bohr who was handed a draft or called into the room, and sometimes an imagined version of Margrethe Bohr, came to stand in as the criteria of clarity and intelligibility not just for Niels Bohr but for all the writing produced while at the Institute. She was the reader that they were all meant to imagine. When Niels and Margrethe Bohr's youngest son Hans describes Margrethe Bohr's role in his father's work, he describes her as a reader and editor: "After a preliminary rough copy the great work of further elaboration began. This always took a

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[42] See Chapter 1 "A Geography of Clarity and Truth" for a discussion of the priority Niels Bohr placed on plain language and the importance of non-scientists in the creation of science.

[43] Interview of Betty Schultz by Paul Forman and Aage Petersen on 17 May 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

long time and the various rough copies were taken across to my mother, without whose approval it could not be completed."[44]

Niels Bohr's ideas about what readers he wanted to reach guided his dictation practices, especially his choice of amanuenses. His choice of Margrethe Bohr and then Betty Schultz was motivated by a desire to make sure that his work would be intelligible to non-physicist readers. With Margrethe Bohr or Betty Schultz writing, Niels Bohr had in the room with him someone who functioned as a useful stand-in for the non-scientist audience to whom he hoped his writing might be accessible. Yes, she wrote or typed for him, but she also provided a crucial intelligibility test. If Margrethe Bohr could not understand it, the goal of plain language had not been achieved.

Niels Bohr's ideas about which readers Quantum Theory should be able to reach shaped how and to whom he dictated, and, at the same time, the practice of dictation carried forward into the style and themes of his writing. It is more straightforward to argue that because of a philosophical belief that science ought to be accessible to a certain kind of reader, Niels Bohr sought out non-scientist amanuenses. And less intuitive to understand that Niels Bohr's lifelong use of an amanuensis had a role in forging and strengthening this commitment to plain language and his ideas about scientific communication more generally.

Bohr's understanding of plain language and the priority placed on its continued use in spite of all the ways that he himself found it to be a misfit for quantum theory is rooted in Bohr's own dictation practices. Returning again to the explanation of plain language he offers his reader in *Atomic Theory and the Description of Nature*, we can hear echoes of dictation sessions in his description of how much responsibility a reader has in determining the meaning of a text. "Indeed, the recognition of the limitation of our forms of perception by no means implies that we can dispense

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[44] Stefan Rozental, *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (North-Holland, 1967), 333.

with our customary ideas or their direct verbal expressions when reducing our sense impressions to order."[45] Here, he acknowledges that the reader has the ability and responsibility of shaping the meaning of the words with their own "sense experiences" from outside of quantum theory. This view of a reader and an author arriving at a shared meaning is a legacy of sitting in a room with Betty Schultz or Margrethe Bohr, pacing, dictating and question asking while he double checks his understanding of what he is saying against theirs. While Bohr acknowledges that his reader may well bring in meanings and associations that did not intend or anticipate, the greater fear is that if he does not use the language he has in common with them, there would be no chance for the text to have any meaning at all. While other physicists advocated abandoning language all together or inventing new terms, Bohr insisted on the using plain language, the language of Margrethe Bohr and Betty Schultz.[46]

Bohr's attitudes toward writing are often read as purely hostile, as a sort of fear and loathing of a necessary evil. His sentences, following every term with layers which contextualize and then further contextualize it, can be interpreted as efforts to triangulate a precise meaning in the face of a reader determined to take things the wrong way and a language designed to mislead.[47] His rules for

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[45] Bohr, *Atomic Theory and the Description of Nature.*

[46] Bohr was very involved in the translations of his work into other languages. In supervising this process and preparing for revisions for translation, he sought to choose words in late drafts that could be translated across three languages (French, German and English) without a change in meaning. These terms were, he felt, less vulnerable to the sense experience of the reader than words or terms that held. See S. Rozental, *Niels Bohr: Memoirs of a Working Relationship* (Copenhagen: Christian Ejlers, 1998) for more details on Bohr's translation practices.

[47] Take, for example, the first sentence passage from Niels Bohr's letter to Einstein dated April 13, 1927 in which concepts is clarified as "words" and then further clairified as words which have their "origin in classical theories." "It has of course long been recognized how intimately the difficulties of quantum theory are connected with the concepts, or rather with the words that are used in the customary description of nature, and which all have their origin in the classical theories. These concepts leave us only with the choice between Charybdis and Scylla, according to whether we direct our attention towards the continuous or the discontinuous aspects of the description. Yes, at the same time we feel that it is the hopes, conditioned by our own customs, that are here leading us into temptation, inasmuch as it has hiterto been possible to keep

writing have been interpreted as an effort to control—to whatever extent possible—the distortions he felt were inherent to the process of writing. Attending more closely to details of his dictation practices and looking at his attitudes about writing as a product of this peculiar form of dictation helps us understand his understanding of written and spoken language differently. Because of the way Bohr wrote with an amanuensis just outside the edges of his tight knit theoretical physics community, he developed an understanding of writing where meaning is the shared responsibility of the author and the reader, the speaker and the listener. Bohr found this tricky and worrisome, but also, vital.

Problems of Representation in Art and Science

Outside of his writing practices and his writing about scientific communication, Bohr's engagement with visual art provides a clue that he took some pleasure and not just pain in relinquishing agency to ordinary readers. When asked in her oral history for the *AHQP* whether Bohr ever spoke to her about art, Betty Schultz, rather reserved throughout the interview, offering one word answers most of the time, launched into monologue about Bohr's taste in modern art. During her time at the Institute, Bohr had collected some paintings by the modern artist Mogens Andersen and hung them in Betty's office. "I think these things of Mogens Andersen which Professor Bohr liked to put up were dreadful… They had put them in my office, and Prof. Bohr said to somebody else that they had to move them because Mrs. Schultz doesn't like to look at them."[48] The interviewer, Petersen, who had been at the Institute at the time when this occurred chimed in, "You said they looked like a truck tied to a factory chimney, or something. And then Bohr said—do you remember that—that

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ourselves swimming among the realities, as long as we are prepared to sacrifice any accustomed wish. This very circumstance that the limitations of our concpts coincide so closely with the limitations in our possibilities of observation, permits us - as Heisenberg emphasizes - to avoid contradictions."Niels Henrik David Bohr and L. Rosenfeld, *Collected Works. General Editor: L. Rosenfeld*, (Amsterdam: North-Holland Pub. Co., 1972), Volume 6.

[48] Interview of Betty Schultz by Paul Forman and Aage Petersen on 17 May 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

the difference between Greek art and modern art is that in Greek times all the work was supposed to be done by the artist, but in modern times all the work is left to the spectator."

Bohr collected Mogens Andersens paintings, but they also had a long friendship. He first met Andersen as a friend of his eldest son Christian Bohr when Andersen visited the Bohr family on holiday in Tisvilde in 1934. That same summer, Christian Bohr would drown in a boating accident, and Mogens Andersen would remain close to the Bohr family, forming a longterm connection between Niels Bohr and a new generation of Danish abstract artists and painters. Bohr's interest in painting preceded his friendship with Andersen. Indeed, at their very first meeting in the woods at Tisvilde, Bohr paused from felling trees (a frequent summer diversion) and asked Andersen questions that revealed knowledge of modern art. "What do you young people think of Scharff, Girsing, Isakson and Weie?" asked Bohr listing the painters responsible for bringing modernism to Danish painting during the first third of the 20th-century.[49] William Scharff, was the Bohrs' neighbor at Tisvilde and one of the artists responsible for bringing cubism to Denmark. In the summers at Tisvilde, Bohr frequently worked in one of Scharff's studios where his children sometimes took art classes from Scharff. The Bohrs and the Scharffs spent many evenings together along with their other artist neighbors who included the painter Julius Paulsen and the musicians Dagmar and Victor Bendix.[50] Modern art and Cubism's influence on Bohr was not a matter of Bohr being generally aware of changes in painting that occurred in his lifetime by viewing key pieces or reading key works (though he did both of these things, frequently using Sundays to take Margrethe Bohr and his children to galleries and reading theoretical works by modern painters including *De Cubisme*), but

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[49] Karl Isakson and Edvard Weie were members of the Bornholm School of Painting, which brought together classical modernist painting to evoke the unique landscape of the Danish island of Bornholm. William Scharf was also committed to using modernist techniques, especially Cubism to depict the Danish countryside near his home is Tisvilde.

[50] William Scharff, "Memories of Tisvilde," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, ed. Stefan Rozental (New York: Interscience, 1964), 315–20.

rather a matter of daily work practice and conversation. Each summer from 1918 on he worked, wrote, dictated and conversed in the studio of one of the first Danish Cubist painter, and spent breaks conversing with Andersen and other artists about the intersections of art and quantum theory.

Both Mogens Andersen and William Scharff describe Bohr's sustained interest in modern art in terms that are similar to Betty Schultz's understanding (though with considerable less contempt for the paintings themselves). Bohr was interested first and foremost in what modern painting, in particular Cubist paintings asked of the viewer, causing them to pause, detach themselves from expectations of what a painting does or should do, and take an active role in understanding what was represented. He also found Cubism's ability to represent several viewpoints at once in a single painting compelling. When given a budget by the Carlsberg Foundation to furnish his study at the Carlsberg house in 1932, Bohr chose *La Femme au Cheval* a painting by the French cubist Jean Metzinger. As Mogens Andersen remembers, "I have often seen Niels Bohr, full of life, explain his view and give his interpretation of that picture. In his eyes there was the pleasure of giving form to thoughts to an audience unable at first to see anything in the painting. They came with a pre-conceived idea of what art should be. More often than not with definite demands for the external appearance of a picture, demands dictated by the diluted naturalism of the 19th century as it was expressed in Danish art of the eighties and nineties… (He) was engrossed with the ambiguity of the motif, or rather of the elements of the painting— face and limbs depicted simultaneously at several angles." The viewers of a Cubist painting like *La Femme au Cheval* had a task analogous to the challenge readers of the new quantum theory faced. They had to take an active role in piecing together for themselves the meaning of a painting that depicted contradictory and sometimes irreconcilable points of view and details.

Dictation Practices and Biography

Following Niels Bohr's death in 1962 there was a flurry of efforts both popular and scholarly to describe his role in 20th-century physics. In 1913 Niels Bohr had published "On the Constitution of Atoms" in *Philosophical Magazine*. In this article he laid out the model of the atom we are still familiar with from high school chemistry classes: a massive nucleus surrounded by tiny electrons moving in "stationary elliptical orbits."[51] This contribution made Niels Bohr a central figure in the budding new field of quantum theory and ultimately earned him a Nobel Prize in 1922. Since his death, Bohr has been the subject of more than twenty book-length biographies and memoirs in English and Danish.[52] These efforts to tell Niels Bohr's story and frame his legacy differ from one another in many ways; they seek to reach different audiences and emphasize different aspects of Bohr's career.

Despite their differences, there are two narrative moves made across these volumes with remarkable consistency. The first is a move to separate the way Niels Bohr worked with Margrethe Bohr from the ways that he worked with colleagues trained in physics or mathematics, characterizing his work with Margrethe Bohr as dictation and his work with colleagues as conversation, dialogue or collaboration. The second is a move to hold Bohr's work in physics as separate from his contributions to and interest in philosophy. These lines, dictation/dialogue and philosophy/physics, are drawn quite forcefully across the 1960s-1990s as Bohr's legacy is being constructed and maintained.

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[51] Niels Bohr, *On the Constitution of Atoms and Molecules; Papers of 1913 Reprinted from the Philosophical Magazine* (Copenhagen: Munksgaard, 1963), 13.

[52] See, among others, Ray Spangenburg and Diane Moser. *Niels Bohr: Gentle Genius of Denmark*, 1995, Faye, J. *Niels Bohr: His Heritage and Legacy: An Anti-realist View of Quantum Mechanics*, 1991, Stefan Rozental, *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, 1967, Abraham Pais, *Niels Bohr's Times: In Physics, Philosophy, and Polity*, 1991, P. J. Kennedy, Niels Henrik David Bohr, and A. P. French. *Niels Bohr: a Centenary Volume*, 1985, Rozental, S. *Niels Bohr: Memoirs of a Working Relationship*,1998, Ruth E. Moore, *Niels Bohr: The Man and the Scientist*, 1967.

These narrative moves have shared underpinnings, and Margrethe Bohr is a site for both the drawing and destabilization of these lines between dictation/dialogue and physics/philosophy. The task of separating Niels Bohr's philosophy out from physics becomes much easier when Margrethe Bohr's work taking dictation for Bohr is characterized as different or separate in kind from the writing work performed by his colleagues. Similarly, as I have described earlier in this chapter, bringing Margrethe Bohr back into the room and thinking about her writing for Bohr as a part of his scientific work casts in a new light the ways in which his philosophical and physical work were linked at the level of practice.

Dictation/Collaboration

When Thomas Kuhn, Leon Rosenfeld, and Niels Bohr's son Aage Bohr sat down with Margrethe Bohr to take her oral history for the Archive for the History of Quantum Physics in January of 1963, they devoted many of their questions to her role taking dictation for her husband. Niels Bohr had died just the previous year having given his final interviews to the same project. Both the interviewers and Margrethe Bohr would have had his legacy on their minds. Kuhn began by asking Margrethe Bohr about the fall of 1912 during which Niels Bohr produced three important papers describing the Bohr model of the atom for the very first time. Kuhn's questions construct careful boundaries between the kind of work that Margrethe Bohr did and the kind of work that Niels Bohr's trained physicist, male colleagues did. Although Margrethe Bohr moves between a range of terms to describe her work for and with her husband, Kuhn never strays from using the word "dictation" to describe their work together, and over the course of the interview her terms for her work come to mirror Kuhns'. Margrethe says for example in reference to the period after her wedding when the papers on the new atomic model were drafted, "On the other hand, when the conversation turns to colleagues at the Niels Bohr Institute who wrote for Bohr, Kuhn accesses a new vocabulary to describe their work. Margrethe Bohr answers Kuhn's first question about

Kramers, a technically trained associate of Bohr's, by describing the work he did using the same terms that she had used to describe her own work with Bohr. It is Kuhn's next question that invites Margrethe Bohr to distinguish her work from that of Kramers' and other "technically trained collaborators": "Did you yourself, get the impression that having a technically trained collaborator, as Kramers was, made a great deal of difference in the way Professor Bohr was able to work?" And, Margrethe Bohr, obligingly, answers, "I suppose it must have gone better with Kramers."[53]

The lines drawn in this interview between Margrethe Bohr the "typist" and Kramers the "collaborator," between dictation and dialogue, echo with uncanny fidelity across the narratives constructed about Niels Bohr in memoirs, biographies and oral histories written in the decades following his death. This is in part a story of influence; these interviews with Margrethe Bohr are used extensively as sources in nearly all of the Bohr biographies and memoirs discussed here, so the way that she speaks about herself with Kuhn becomes the way that she is spoken about in both academic and popular treatments of Niels Bohr. But even when scholars and biographers are not working directly from this set of interviews, they often make moves that run parallel to Kuhn's.

There is a range of ways this move is made, all with the result of holding Margrethe Bohr to the side of Niels Bohr's professional work. In the introduction to *Niels Bohr: His Life and Work as Seen by Friends and Colleagues*, Rozental sets Margrethe Bohr aside in one swift, deeply complimentary paragraph:

> It must of course remain outside the scope of this present account to describe the happiness Niels Bohr found in his marriage with Margrethe Nørlund. What these two came to mean to each other cannot be put into words and those occasional references to their married life, which lasted over 50 years, given by several people in some of the following pages, can but give a very slight ideas of its unique character.[54]

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53 Interview with Bohr, Margrethe by Aage Bohr, Leon Rosenfeld and Thomas Kuhn, January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4514_1.html

54 Rozental, *Niels Bohr*, 37.

For Rozental, it is self-evident that Margrethe Bohr belongs outside the scope of this collection of memoirs. In fact, he suggests that the nature of their relationship would be impossible to put into words. Abraham Pais' biography of Niels Bohr aims to treat their marriage in more detail but continues to hold Margrethe Bohr separate from Niels Bohr's professional life through the organization of the chapters. Pais' chapters alternate between personal and professional topics; chapters covering Niels Bohr's "Boyhood" and his "Student Days" are placed between chapters on the state of physics in 1903, the structure of the atom, and quantum mechanics.[55] Although Margrethe Bohr is included in some detail, her story is told in the biographical sections and is never meant to intersect with the portions that treat Niels Bohr's professional and intellectual life.

It is important that even as we continue to retrace the lines drawn in these narratives, we begin to notice how—regardless of intentions-- the anecdotes contained within these accounts do not always observe the same boundaries that their authors describe. Even in a biography like Pais' where there is a clear architecture that separates the professional from the personal, there are plenty of moments where the two worlds bleed into one another. Pais worries about the impact his own friendship and affection for Bohr will have on his telling of Bohr's story, but rather than attempting to confine this friendship to the personal portions of the book, as he does with Bohr's marriage and family life, he gives himself permission to indulge his affection for Bohr through all the chapters of the book on the grounds that "history is subjective."[56] And while Rozental does not see space for anything beyond "occasional mentions" of Margrethe Bohr in his collection of memoirs, it is within

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[55] Abraham Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, ed. Anonymous (Oxford; New York: Clarendon Press; Oxford University Press, 1991).

[56] Ibid., 4.

this same volume that we find Hans Bohr grappling quite seriously with Margrethe Bohr's role in Niels Bohr's writing process.

The settings and work places that appear in these biographies also blur the carefully constructed boundaries between work and family laid out by the biographers. In these accounts and others, the most intense work spaces often end up being domestic spaces as well. As Pais puts it, "Blackboards were never far from where [Bohr] dwelt" (10) and dinners at the Bohrs' home were often hybrid occasions with conversation moving back and forth between physics, politics and other topics.[57] From the institutes founding in 1919 until 1932 when they moved to the Carlsberg Foundation house, the Bohr family lived in a flat within the institute. Margrethe Bohr remembers that Bohr had collaborators over every evening and worked well into the night for all but the last two years of his life[58]. Hans Bohr recalls that frequent collaborators were viewed as part of the family "Uncle Heisenberg" "Uncle Klein" and that as frequently as physicists were in their home, the children were in the work spaces of the institute. All six boys used the institutes workshop under the instruction of the foreman, for example.[59] According to Rozental, the place of work for Bohr and his colleagues in the late nineteen-teens was as likely to be Bohr's home where Mrs. Bohr received Bohr's colleagues with great hospitality at his room at the Technical University at Sølvtorvet.[60] And many accounts describe Bohr family holidays as the site for work, discussion and

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[57] Ibid., 5–6, 10.

[58] Interview with Margrethe Bohr, *AHQP*.

[59] Hans Bohr, "My Father," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (New York: Interscience, 1964), 325–39.

[60] Rozental, *Niels Bohr*, 76.

breakthroughs in quantum theory.[61] While the framing of these stories about Bohr hold Bohr's marriage and family life to be separate from his work life, the stories themselves constantly blend domestic and professional spaces.

Philosophy/Physics

While there can be a softness to the ways in which Margrethe Bohr is set to the side of Niels Bohr's work, the line between physics and philosophy is drawn more fiercely. Pais is the most vigilant in his sealing of Bohr's work off from philosophy. Once again the structure of his chapters does some of this work for him; all discussion of Bohr's philosophy is cordoned off into the last chapter titled "Bohr and Philosophy." For Pais, philosophy refers to any cross-pollination between Bohr's philosophical education and quantum theory and also to complementarity once Bohr began to apply it to fields other than physics. Key for Pais is the distinction between complementarity as a component of quantum theory, which he would like to see rescued and brought back into physics education and text books, and complementarity as a philosophy applied to other disciplines, which he sees as a sort of hobby Bohr took up after he had completed his most important contributions to physics.[62] In Pais' telling, everything in the long history of science—from Descartes and Newton on up through Planck-- is described as relevant to Bohr's biography; this is the world of ideas into which Bohr fit like a missing piece. At the same time, Pais rejects as "far fetched" the idea that something as immediate to Bohr as his undergraduate philosophy courses had any influence on his

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[61] In Rozental, see Klein on family vacations with the Bohrs (89), Hans Bohr writing about how the Bohr children viewed their father's colleagues as "uncles" (335). Pais also recollects working vacations with the Bohr family (5-6).

[62] Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, 16.

thinking.[63] Whenever he mentions philosophy, he issues a reminder that philosophy was separate from physics in Bohr's life: "In a technical sense, no philosopher ever influenced Bohr."[64]

As with Margrethe Bohr, even those who give more attention to Niels Bohr's philosophical pursuits and influences, have ways of holding them apart from what they consider his major contributions to physics. In Heisenberg's telling, Bohr was "primarily a philosopher and not a physicist" meaning that he placed priority on an intuitive understanding of the physical reality rather than formal, mathematical derivation.[65] This philosophical priority is something that, in Heisenberg's telling, began to hold Bohr's scientific work back. "I noticed that mathematical clarity had in itself no virtue for Bohr. He feared that the formal mathematical structure would obscure the physical core of the problem, and in any case, he was convinced that a complete physical explanation should absolutely precede the mathematical formulation. I was perhaps, already at that point, more prepared than Bohr to leave the models and take the step over to mathematical abstraction."[66] Writing in *Physics Today,* Leon Rosenfeld does not rank the importance of Bohr's philosophical and scientific pursuits, but he does insist that they have separate origins. "Bohr's first preoccupation with philosophical problems did not arise from his physical investigations but from general epistemological considerations about the function of language as a means of communicating experience... How to avoid ambiguity... that was the problem that worried Bohr."[67] For Oskar Klein, Bohr's focus on complementarity represented a turning away from the core problems tackled by quantum theory and quantum mechanics. "While Bohr was busy elaborating the viewpoint of

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63 Ibid., 311.

64 Ibid., 99.

65 Werner Heisenberg, "Quantum Theory and Its Interpretation," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (North-Holland, 1967), 98.

66 Heisenberg, "Quantum Theory and Its Interpretation."

67 L. Rosenfeld, "Niels Bohr's Contribution to Epistemology," *Physics Today* 16 (1963): 47.

complementarity, new progress was steadily forthcoming regarding the quantum-theoretical formalism and its application to various physical problems which had hitherto resisted theoretical explanation."

Conclusion

In Niels Bohr's dictation practices in particular, we how ideals for communication reinforce for Bohr the need to write using a particular dictation/dialogue practice and, at the same time, how the practice of dictation and dialogue is itself a foundation for those ideals. Other scholars have pointed to the influence that factors outside of Bohr's daily work practices had on the development of his theory of scientific communication. Novels and poetry, readings in philosophy, a childhood upbringing seated around a table of interdisciplinary scientific discussion are all clearly models for how and to whom Bohr seeks to communicate. But Bohr's dictation practices, the practice of producing a text in conversation with someone else who wrote for him, pre-dates these other influences. Before he encountered cubism, before he married Margrethe, before he read *Adventures of a Danish Student* or Kierkegaard, Bohr dictated. The most persistent, daily influence on Bohr's philosophy of communication was his own communication practice. It is primarily from this daily practice Bohr developed his value on plain language, on the creation of meaning through dialogue between author and reader.

Of all Bohr's writing practices, his dictation practices receive the most aggressive sidelining in the secondary and biographical literature. Red herring explanations abound from the actors and the analysts: Bohr could not write, he had difficulties with composition, he was "too sensitive for easy composition" (Heilbron 109 LLAP) all of which view dictation as an artifact of Bohr's peculiar genius and not as a practice with specific legacies for the construction of quantum theory and theories of scientific communication. Several factors come together to motivate the movement of this writing labor (and Bohr's amanuenses with it) to the edges of the history of quantum theory.

First and foremost, historians and colleagues of Bohr's are concerned with crafting and preserving Bohr's legacy as an individual genius, an equal to Einstein whose own work practices were far less dialogic. Secondly, but just as importantly, many historians of science writing during the Cold War used dictation as part of their own writing practices. Dictation, as a part of their own writing practice was invisible, not considered a mediated part of the production of a text and theory. It would be difficult for these same actors to understand dictation as playing a core role of any consequence in the construction of quantum theory.

Chapter 3: Revising the Atom: Atomic Orbits and Metaphoric Haunting

During the winter of 1926 and 1927, Niels Bohr and Werner Heisenberg worked together in Copenhagen to find a way to pull the developments in atomic theory together into one unified, concise description. Frustrated by their lack of progress, and still in the midst of resolving disagreements, Bohr left Copenhagen for a ski trip in Norway in February 1927. Heisenberg and Bohr worked closely together in Copenhagen, but often required time apart to make steps forward in their work. Bohr used his time away in Norway to begin to formulate Complementarity, the idea that certain truths exist as pairs such that only one can be true at a time. And immediately following Bohr's departure, Heisenberg wrote to Wolfgang Pauli from Copenhagen with a preliminary sketch of the thought experiment that would later become his Uncertainty Principle. In late March, when Bohr's return from Norway was imminent, Heisenberg submitted his uncertainty paper, ""Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik" to *Zeitschrift fur Physik* for publication.[1]

When Bohr returned to Copenhagen in early April and learned of Heisenberg's already submitted paper, he read through a copy and suggested numerous revisions. Heisenberg refused to recall the paper from press in order to revise it substantially. Instead, he added a postscript or "note added in proof" citing Bohr's criticisms and alluding to Bohr's own formulation of quantum theory, which held the complementarity of waves and corpuscles of light as a more central tenet.[2] Though

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[1] Werner Heisenberg, "Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik." *Zeitschrift für Physik* 43, no. 3–4 (March 1, 1927): 172–98. doi:10.1007/BF01397280.

[2] See English translation of postscript in Niels Bohr, *Collected Works: V. 6. Foundations of Quantum physics1 (1926-1932)*, ed. Jørgen Kalckar, vol. 6 (Amsterdam: North-Holland Pub-2008, 1972), 20: "Note added in proof. After conclusion of the present work, recent investigations by Bohr have led to points of view that permit an essential deepening and refinement of the analysis of the quantum mechanical relationships, attempted in this work. In this connection Bohr has drawn my attention to the fact that in some of the

Bohr was outwardly supportive of Heisenberg's uncertainty theory, promoting it carefully to Einstein in a letter in April of 1927, he was disappointed that Heisenberg had not incorporated his revisions into the body of the article.[3]

Many of Niels Bohr's colleagues, family, and staff recount how he went through an exceptional number of drafts for personal and professional correspondence and for the drafting of scientific papers.[4] The archives corroborate the anecdotes about Niels Bohr's drafting and revision practices. The unique dialogical nature of his sessions with amanuenses included other voices at the inception of the paper, and other voices had the opportunity to enter into the text during his long revision process. Various handwritings layer the margins and interrupt the typing of each draft. The exchange between Bohr and Heisenberg surrounding the uncertainty principle provokes questions about what exactly revision meant for Bohr. If a generous postscript describing corrections and Bohr's point of view did not satisfy Bohr's request for revision, what would? How did he use revision in his own writing process? What did he ask of others? And what were the consequences of these revision practices for his own writings and the writing he supervised?

Like dictation (see Chapter 2) Bohr's revision practices were lifelong practices. He revised extensively from childhood through late adulthood. When Bohr was asked to write the introduction to a volume on Danish culture up to 1942, for example, "Friends were invited to read what had been written and their comments led to changes but also resulted in expansions and additional

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discussions in this work I had overlooked essential points. First and foremost, the uncertainty in the observation does not exclusively depend upon the appearance of discontinuities, but is directly related to the requirement of doing justice at the same time to the different experimental facts which find expression in the corpuscle theory on the one hand and the wave theory on the other..."

[3] For more detailed explanation of Bohr's critique of the way that Heisenberg's uncertainty treated photons and electrons as point particles see)Mara Beller, "The Birth of Bohr's Complementarity: The Context and the Dialogues," *Studies in History and Philosophy of Science* 23 (1992), 172.

[4] Interview of Margrethe Bohr by Thomas S. Kuhn on January 23, 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4517_1.html

paragraphs. In the end the article filled nine printed pages and was first ready for printing after seven proof readings; the last was so comprehensive that it would have been easier to print a completely new text."[5] At certain stages in his drafting process, Bohr took suggestions and criticisms into the heart of his paper; he only submitted papers to publishers that he felt had already withstood the careful critical reading of many others.

One way to begin to map out Bohr's writing practices is to look at his desk through the eyes of his longtime assistant, Stefan Rozental. As Rozental described it, Bohr's office at the Institute for Theoretical Physics in the 1920s and 30s was filled with a large table, the entire surface of which was covered with papers piled according to their degree of completion.[6] Numerous drafts navigating complex editorial rules covered every inch of the desk. Sifting through these papers in the early 1920s, we can perform a kind of an archeological reconstruction of Niels Bohr's writing practices. The smallest pile held drafts marked, "Taboo." These papers were ready to be sent to the publisher only to be called back again when Bohr made a new word choice or final correction. Then there were piles containing typed drafts layered in handwriting belonging to Niels Bohr's wife, Margrethe Bohr, his secretary, Betty Schultz, his colleagues, and sometimes himself. The earliest notes were in yet another pile, densely populated with drawings and equations with only a few hard to decipher words dotting the page like punctuation.

Bohr's request that Heisenberg substantially revise his uncertainty paper was not unusual. His revision practices and expectations for revision were not confined to his own writing. In fact, Bohr presided over all the papers being written at the Institute for Theoretical Physics (later renamed the Niels Bohr Institute), and nothing was sent to the publisher before he read and

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5 Stefan Rozental, *Niels Bohr : Memoirs of a Working Relationship* (Copenhagen: Christian Ejlers, 1998), 39.

6 Ibid., 33.

discussed it with the author.[7] His requirements that others working at his institute revise repeatedly and substantially were predictable enough that Heisenberg, having worked with Bohr for some time and anticipating his imminent return, hurried to get his article out for publication as an attempt to navigate around the substantial line by line, word by word revision that Bohr normally required. Bohr was not just director, but editor in chief of publications turned out by physicists working at his institute in Copenhagen.

Bohr had several specific editorial rules for the revision of his own work and for the work of his colleagues. To name just a few: He restricted the use of mathematics, especially in late drafts. He required that every word in a final draft be able to survive translation into three languages (French, German and English) without any changes in meaning. These conscious, programmatic rules were implemented into revisions across the drafts. But these rules for revision do not fully encompass Bohr's revision practices; Bohr's drafts reveal other revision practices that are less directly linked to his stated editorial rules. These include the use of drawing and figurative language in early drafts, the purging of figurative language, his use of proofs interleaved with blank pages to make substantial edits, and the reliance on punning and wordplay in final drafts.

"It is dangerous to deny any proposition involving such an exceedingly ambiguous adverb"

Listed as they are above, Bohr's editorial rules and the principles that he put in place to drive revision, can begin to seem like odd and obsessive quirks having little to do with the development and application of quantum theory. Indeed, this is often how they are portrayed in the secondary literature. But even a quick glance at the reception of quantum theory reveals that Niels Bohr was by no means the only one approaching the application of quantum theory as a problem of language. Proponents and critics of quantum theory alike who were trying to understand, translate, and use the

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[7] Ibid., 21.

new physics of the 1910s and 20s framed their difficulties with the theories as stemming from good and bad word choices or misunderstandings about the definition of terms.

Writing in 1926 in *The London, Edinburg and Dublin Philosophical Magazine and Journal of Science,* the Cambridge physicists and philosopher of science Norman Campbell raised many questions about Bohr's model of the atom and about Heisenberg's Uncertainty Principle.[8] Though Campbell's article is published in a scientific journal and does not announce itself as being about writing, it reads as a set of instructions for the reading and writing of quantum theory. The argument of his article entitled "Time and Chance" was structured around a set of quarrels with word choices made in Bohr and Heisenberg's papers. He criticized Heisenberg for using the adverb "principally" to modify "observable." "Principally," in his reading, was far too elastic, and, even given its elasticity, still did not make "observation" correct for describing the frequency of radiation emitted by individual atoms. Concerning Heisenberg's "principiell beobachtbar" he wrote, "It is dangerous to deny any proposition involving such an exceedingly ambiguous adverb; but it is by no means obvious that if a distinction is to be made between quantities that are observed and those that are not, the frequency of radiation falls into the former class."[9] Campbell wanted quantum theory written in a way that made clear that it was a description of statistical aggregates and trends and *not* a description of a physical reality. Again, he suggested that this can be accomplished by carefully defining terms for a reader. "Time," he suggested, should be defined as a statistical phenomenon, especially since we are accustomed to thinking of it as something real and stable. "Temporal conceptions are so deeply embedded in all our thoughts and language that, if we decided to abandon them entirely in

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8 As Mara Beller notes in "The Birth of Bohr's Complementarity," Campbell had a longstanding interest in Bohr's work. He wrote an early review of Bohr's proposed planetary atomic structure in 1913 arguing that no reality could be ascribed to Bohr's model. "The Birth of Complementarity" takes on the role that some historians including Beller think Campbell has in pushing Bohr toward an articulation of Complementarity.

9 Norman R Campbell, "Time and Chance," *The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science* 7, no. 1 (1926): 1106–17. 1107.

discussing atomic phenomena, we should find that we could not even state adequately the problems we desire to solve."[10]

It is clear from the ways that Norman Campbell levels his criticisms against quantum theory and uncertainty that language played a core role in navigating quantum theoretical problems for more than just Niels Bohr. Campbell's article is five solid pages of defining words, distancing words from past uses, distancing words from corresponding physical reality, attaching them instead to statistics, and agreeing that, despite their profound and misleading dangers, words must be used instead of mathematical expressions. The revisions he suggested were revisions at the level of language. Bohr's habit of recalling whole articles to change a single word makes more sense in the context of readers like Campbell whose arguments against quantum theory rest on an adverb. His attentiveness to questions about language and quantum theory are shared by a community that brings similar levels of scrutiny to the written communication of quantum theory.

The Atom in 1912

Bohr began his work on the atomic model while working in Ernest Rutherford's laboratory at Manchester in 1912. At that time, the two most recent models of the atom were suggested by J.J. Thomson and Rutherford. Thomson's model was the first model to incorporate electrons or small negatively charged particles. His "plum pudding" picture of the atom was an even smear of positive charge dappled with thousands of tiny electrons, circulating through the positive "pudding" like little plums. Unless all the components were completely still, the atom could not be stable, but when still, it was exceedingly stable.

Thomson's model ran into trouble beyond its instability when the electrons were in motion with the experimental results published by Hans Geiger in 1908. Geiger had been studying the

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[10] Ibid., 1110.

scattering of alpha particles by thin gold foils at Rutherford's laboratory. He observed that some of the alpha particles scattered at angles up to 90 degrees. Abraham Pais likens this to a truck being deflected by a Volkswagen.[11] These results are also frequently described in physics and chemistry courses as the equivalent of a sheet of taut tissue paper deflecting a bullet. The scattering of alpha particles was so unexpected because the sheet of gold foil that the alpha particles were being scattered through was made of gold atoms. The accepted model of the electron at the time, Thomson's, was very soft and would never have deflected the alpha particles at such an extreme angle. Geiger's results suggested that there was in fact something very hard inside the structure of the atom.

Rutherford proposed a model that would explain Geiger's results. His atom had a tiny hard center that balanced the charge of electrons that surrounded it. In order to explain how the alpha particles had been reflected, almost all of the mass of the atom had to be concentrated in this tiny center. In Rutherford's own words, we are looking at a "type of atom which consists of a central electric charge concentrated at a point and surrounded by a uniform spherical distribution of opposite electricity equal in amount."[12] Most of the rest of the space that the atom took up, was empty. If an alpha particle in Geiger's set up encountered the empty space of the atom, it would breeze straight through. But occasionally one of the alpha particles would collide with the nucleus instead and be deflected backward.

Three months after Rutherford created this nuclear atom, Bohr came to work in his laboratory. It was Rutherford's atom that Bohr hoped to expand on and explain. And it was to

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[11] Abraham Pais, *Niels Bohr's Times : In Physics, Philosophy, and Polity*, (Oxford; New York: Clarendon Press; Oxford University Press, 1991),123.

[12] Sandro Petruccioli, *Atoms, Metaphors and Paradoxes* (Cambridge: Cambridge University Press, 1993), 42.

Rutherford that he sent several drafts of the Bohr atom, titled "On the Constitution of Atoms and Molecules." A paper of the same name was published in 1913 in "Philosophical Magazine."

Drafts as Cross-sections of Critical Debates

As Mara Beller and Gerald Holton remind us, there are different contexts of reception and motivations at work during the stages of revision. And debates alive in the theoretical physics community inform Bohr's rhetorical choices.[13] Like Beller and Holton, I interpret the word choices, style, organization and citational choices at each stage of the drafting process as windows into debates alive in Bohr's community. Unlike Beller and Holton, I will use these drafts to understand a dialogue among a larger group of historical actors; while Beller defines dialogue as Bohr's correspondence with other physicists, I will be working with a definition of dialogue that encompasses conversations and correspondence with his wife, secretary, and other non-physicist interlocutors. I am interested in the ways dialogue is incorporated, but also in the ways that it is rejected. Omission and the rigidity of the drafts in late stages are as much the object of my analysis as moments where marginal comments are enthusiastically incorporated into a porous text.

Paper Tools in the Bohr Drafts

How does what I term the mechanics of the revision carry forward into and affect the final text? This brings me into territory not well-covered by debates among scholarly editors. While Latour, Warwick, Klein, and Kaiser describe how inscriptions, theoretical technologies, and paper tools operate in a social context circulating within groups of chemists and physicists, I will describe the

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13 Mara Beller, *Quantum Dialogue : The Making of a Revolution*, Science and Its Conceptual Foundations (Chicago, IL: University of Chicago Press, 1999) and G. Holton, "Quanta, Relativity, and Rhetoric," *Persuading Science: The Art of Scientific Rhetoric*, 1991. For an elaboration of Mara Beller's assertion that Bohr's writing can only be understood if one understands the underlying debates that motivate each sentence or as she writes, "Without realizing to whom each sentence… is directed, for what and against what it argues, it is indeed very difficult, if not impossible, to understand both the meaning of these sentences and the connection between them." (148) see *Quantum Dialogue* and "The Birth of Bohr's Complementarity."

meaningful work that theoretical and paper tools do within a more private, pre-publication context. While they look mostly at the circulation of published documents (or if not published than pieces of writing like labels that are meant for public consumption), I look at private, unpublished, drafts. I argue that we can examine and interpret writing not just to identify the culturally contingent skills and conventions shared by a group of practitioners, but also to identify an individual's practices and the ways that his practices make up and relate to beliefs about knowledge. And just as scholars have examined anticipated and unanticipated effects of a paper tool on the way that a group practices physics (Kaiser Feynman Diagrams) or influences the kinds of chemical reactions that become most prevalent to study (Klein), we can talk about the work that paper tools perform in the crafting of a particular theory even when that theory is nominally single authored.

Equipped with the terms that Latour, Warwick, and Klein have developed, Bohr's stacks of drafts can be parsed for clues about the conventions and skills shared by Bohr and his collaborators. But each diagram, drawing, equation, and use of figurative or literal language is also an instrument with particular legacies for the final, published theory which circulates more widely. The intensely social and iterative nature of Niels Bohr's drafting process makes it possible to think about paper tools and their role within the generation of an individual article with a nominally single author.

Throughout this chapter, I use the word "work" to encompass the entire compositional practice of a scientist from first utterances and drawings to published and republished articles. In introducing the term "work" I sustain the sense of work as corpus, a usage borrowed from literary theorists and scholarly editors for whom "work" refers to all of the versions of a text, and connect this usage to the critical literature of paper tools, in which scientific writing is understood as being comprised of paper tools and scientific writing process would therefore refer to the construction and usage of such tools. "Work" simultaneously evokes the sense that a published text is one of many versions that precede and follow it and a sense of the scientific writing as a process of crafting and producing tools. Holding together work as corpus and work as the crafting of tools also reveals something of the provisional nature of the final text because notions of finality are uncomfortable in the language of tools. We do not think of a tool being made or completed in the same way as a novel or a painting. A tool is made to be put into use. Rather than privileging the final draft as the vessel for authorial intention, the term "work" allows us to the see the final draft as one of many drafts and as a set of tools that are not done, but are nonetheless being handed off to others to be put into use. "Text" in this context refers to the arrangement of words on a page. I am bracketing the visual and mathematical as non-textual in order to draw attention to the effacing of the visual and mathematical that I am tracing across Bohr's drafts. [14]

Revision Patterns

A central concern of this dissertation as a whole and this chapter in particular is noticing how the material culture of Bohr's writing process crashes up against his neat intentions for what he wishes to accomplish through his writing. Bohr brought powerful beliefs about scientific communication to his writing, but this does not mean that these guided his writing practices or moved unproblematically to the page. Instead, Bohr's final, published theories are the knotty legacies of ideals and practices coming together in the material culture of his writing process, a material culture that involved drafting, revising, marginal commenting, sorting, piling and drawing. To better understand how the mechanics of revision carry forward into the published text I follow several interconnected revision patterns across the drafts: Bohr's changing use figurative and literal language, his references to other scientists and use of modalities that suggest the social nature of scientific knowledge, and Margrethe Bohr's administrative and editorial work.

[14] In the interplay between "text" and "work" as I use them here, there is a particular resonance with James McLaverty's understanding of "the text as the score of the work." James McLaverty, "The Concept of Authorial Intention in Textual Criticism," *The Library* s6–VI, no. 2 (1984), 127.

Even as early as 1911, Bohr held values about using plain language to communicate scientific ideas. Plain language for Bohr was bounded on the one side by figurative language, which he considered the language of poetry and on the other side by mathematics, which he considered to technical to communicate scientific ideas to educated non-scientists. Plain language was unambiguous and universal, free of references that could be understood only by one nationality or language community. Derived from those ideals were his intentional revision practices including the purging of figurative language, figures, and math, and the practice of translating final drafts into three languages removing all words and phrases that could not be translated without changes in meaning. These writing habits later became informal editorial rules at his institute in Copenhagen. After the completion of initial rough drafts that are dense with images and equations, Bohr systematically replaced figures, figurative language, and mathematics with literal expressions and prose descriptions. The heavy reliance on figures and mathematics in early drafts and the subsequent purging figures, figurative language and mathematics in later drafts in a movement from figurative to literal, has consequences for Bohr's eventual description of electron orbits. I will continually track what truth claims shed and hold onto as they move through the drafts.

Ursula Klein gets at the messiness of the intersection of theory and practice in her use of the term "paper tools," inscriptions with the initial purpose of naming or representing something unseen. Klein's paper tools do more than represent, as they circulate in a community they do work, but the work they do is not always under the control of or anticipated by historical actors. They can, as they circulate and get put into use, structure thinking, limit the set of problems a community of scientists will take on, and act against the initial purposes they were designed for. Through these drafts, I treat Bohr's concept of the electron orbit as a paper tool and examine how it structures Bohr's model of the atom while being itself structured by his drafting and revision practices. Initially a scrawled circle intended as a loose analogy, the orbit comes to be taken for granted as real, and I want to understand this is an artifact of Bohr's particular drafting practice. The sheer number of drafts and iterations impact the boundary between figurative and literal in the atomic model as it is articulated in the published article. That orbit's crossover from figurative model to physical reality relies on the mechanics of Bohr's revision practices.

Outside of Bohr's intentional revisions motivated by "plain language," I will be tracking patterns that have been important to the arguments of other historians of science who examine writing as a part of scientific practice. After Latour and Woolgar, I will be attentive here to the status of the social. Latour and Woolgar, notice in the late 20th century biological laboratory that they observe in *Laboratory Life* that as writing progresses from scraps to published article, phrases shed rhetorical connections to the social in order to achieve "fact like status." {FN Latour and Woolgar 81-83} In late drafts and completed work, the social is only evoked to signal that something has gone wrong. I observe in Bohr's drafts a very different use of rhetorical strategies for connecting and distancing his work from the social. In fact, his final, published article is dense in its references to the social connections of the theories. This suggests that the social is a different kind of resource for Bohr writing in 1912-13. I will, as I track the use of modalities in these drafts, offer some interpretations as to why connections to social context have an inverse trajectory than the chains of inscription Latour and Woolgar follow.

Lastly I follow the impact and legacies of Margrethe's dictation, administrative and editing work. As we move through the stages of drafts, we learn that Margrethe Bohr not only took dictation for her husband, she also organized and titled his notes, recorded the feedback he received on his doctoral thesis, and filed his correspondence. The broad question of how the context of generation and composition carry forward into the final text will be broken down in this chapter into several smaller ones: How do Margrethe's systems for organizing and titling notes carries forward into the organization of the article? How does the binding of notes, the systems for filing and recording correspondence and feedback, the actual piling of drafts on a desk shape the eventual published article that appeared in *Philosophical Magazine* in May of 1913.[15]

Pre-Draft Notes 1911/12

Niels Bohr's work on his 1913 paper begins, as so many articles begin, with notes he took in 1912. Several myths about Bohr's writing practices can be dismissed through examining just this one set of notes preceding the drafting of his papers on the model of the atom 1913. First, most of the pages, 39 of 45, are in Niels Bohr's handwriting (see figure 3). While not as legible as the pages written by Margrethe Bohr, Bohr's writing is mostly readable. Bohr's illegible handwriting has often been cited as his reason for using an amanuensis to generate first drafts and execute revisions, but his readable handwriting here and elsewhere in the drafts suggests that his handwriting alone does not explain his use of an amanuensis.[16] The factors influencing what and when he chooses to write (or draw) and what and when he chooses to have others write for him are far more complex than an inability to write or dysgraphia. At this pre-drafting stage of the process, Niels Bohr draws the atomic model, writes all the equations and calculations, drafts portions of responses to relevant correspondence, and Margrethe Bohr writes the synthetic titles summarizing each section.

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[15] Though this chapter is devoted to revision and the previous to dictation, it is important to remember that dictation and dialogue recurred throughout Niels Bohr's revision process. Dictation sessions were always held at the start of a drafting process, but they were frequently held again to plan and execute revisions and edits between versions. And the same people Niels Bohr relied on to take dictation and listen to a first draft of his ideas, including Margrethe Bohr, were called on again to review and react to later drafts.

[16] Rozental, Niels Bohr: Memoirs of a Working Relationship, 34,

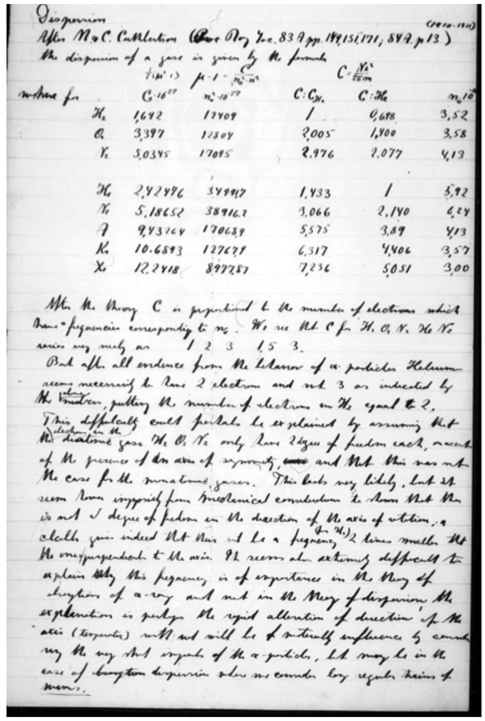


Figure 3: Example of legible handwriting in Niels Bohr's 1912 notes. "Dispersion OG Absorption Af Alfa-Straler." Bohr MSS No. 4 AHQP, Harvard University.

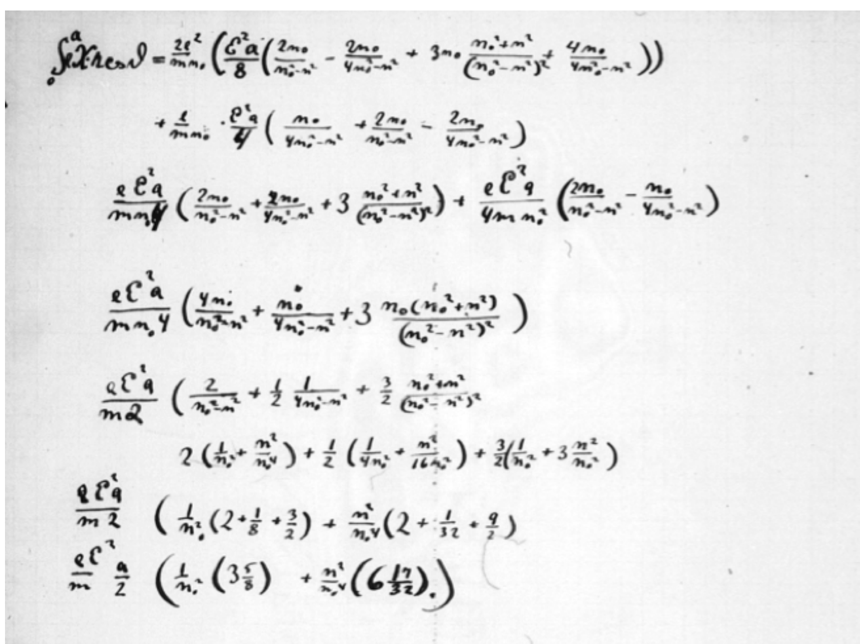
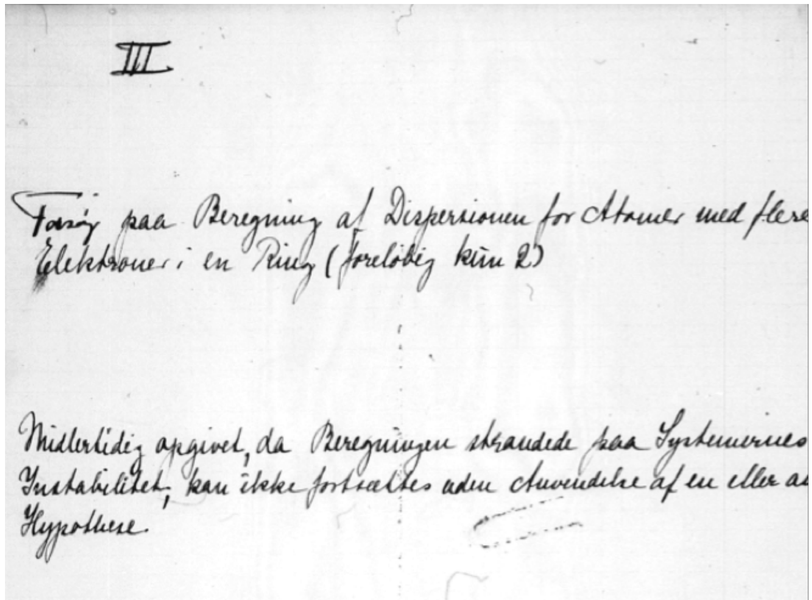


Figure 4: Example of Niels Bohr's 1912 draft notes dense with mathematical equations Dispersion OG Absorption Af Alfa-Straler, Bohr MSS No. 4, AHQP, Harvard University

Second, although Bohr was widely believed to be a less mathematically oriented thinker than many of his colleagues, these notes are dense with equations, tables calculating the dispersion of gases, and diagrams. That is, in working through his early ideas for an atomic structure that could explain alpha scattering and in his correspondence with others, Bohr's own independent work was often mathematical (See Figure 4). The assumption that Niels Bohr avoided math or grounded his thinking in other forms of reasoning seems to be taken from a reading of final, published materials, which are indeed light on equations. As we move through the drafts, we will see that this is the result of a purging of math, not an indication that Bohr was not fluent or interested in solving problems with mathematical tools.

These notes also enrich our understanding of Margrethe Bohr's role in Niels Bohr's work. The notes are organized and bound into booklets and given substantive, synthetic title pages, such as "Prev. On Calculations of Dispersion of atoms with several electrons in a ring."[17] These titles are in Danish in Margrethe Bohr's handwriting (See Figure 5 and 6), which suggests that she was responsible for this administrative and organizational work. While the notes are highly technical, the title pages are in prose, allowing us to see Margrethe Bohr not just as an amanuensis and final editor, but also as a someone with a key role in the intervening drafts: organizing, synthesizing, and binding things for later reference. The organization of preliminary notes are an important aspect of revision largely orchestrated by Margrethe Bohr in the early parts of Niels Bohr's career. As Bohr builds up his first drafts from these early notes, the sections Margarethe creates in her binding and titling hold together as sections in the early draft. In this way her organization of notes and draft correspondence has a lasting effect on the organizational structure of the article itself; the material culture of their work together has a legacy in the content and organization of his published work.

[17] "Foreg paa Beregning af Dispersion for Atome med fiere elektroner i en Ring" Dispersion OG Absorption Af Alfa-Straler, Bohr MSS No. 4, Archive for the History of Quantum



III

Forsøg paa Beregning af Dispersionen for Atomer med flere Elektroner i en Ring (foreløbig kun 2)

Midlertidig opgivet, da Beregningen strandede paa Systemernes Instabilitet, kan ikke fortsættes uden Anvendelse af en eller an

Hypothese

Figure 5: Margrethe Bohr's Title work providing titles for Niels Bohr's Notes "Dispersion of OG Absorption Af Alfa-Straler" (1912) AHQP, Harvard University.

In these notes from 1912, Bohr is grappling with the question of dispersion of gases and how both the dispersion of gasses and alpha particle scattering might be explained through the geometry of electron rings in the atom. Using drawings and calculations, he manipulates both the rings' orientations in relation to one another and in relation to the alpha particles coming their way. At this stage this stage his drawings of the atom are a point representing the nucleus surrounded by concentric circular rings, some populated with dots representing electron and others empty. Arrows between the rings indicate how the electron might move from one to the other. (See Figure 8)

Drawings in the Early Drafts

In early drafts Niels Bohr's pages were dominated by drawings. According to his personal assistant, Rozental, "He usually began to work with drawings, adding more and more details which were often not easy to grasp at first glance."[18] Some of these drawings were made on the blackboard while Bohr dictated to an amanuensis and were therefore not captured in the archival record. Others can still be found in the early drafts. As revision progressed Bohr converted drawings to prose and to equations, and very few drawings or diagrams survive into final drafts. But it is apparent from the surviving drafts that he sometimes had a hard time disentangling himself from drawing as a primary mode of representing his ideas. Several revisions in, sentences contain frequent interruptions by hand drawn pictures. Drawings of various models dapple the page like punctuation, sneaking in like semicolons without any explanation or description in the nearby prose. Only gradually does prose overtake the drawing and do diagrams become secondary supplemental illustrations of the ideas and models being described.

In Bohr's notes from Jean's lectures Autumn 1911, pictures and words hold equal footing on the page, often interrupting each other (See Figure 7).[19] The words are illegible and incomplete and the drawings swarm the page. In Bohr's "First Draft of Atomic Models" pictures of the rings of the atoms line the margins of the page each time the atomic ring is mentioned (see Figure 8).[20] Before Bohr finishes the sentence meant to describe the model of the atom, he gives up and reverts to drawing instead.

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[18] Rozental, *Niels Bohr: Memoirs of a Working Relationship*, 49.

[19] Niels Bohr, "Notes Lectures from Jeans" 1911, Bohr Manuscripts 1911-12 BMSS_3 Film A 603.5 (3), *Archive for the History of Quantum Physics*, Harvard University.

[20] Niels Bohr, "First Draft on Atomic Models," 1912, Bohr Manuscripts 1911-12 Film A 603.5 (2)*Archive for the History of Quantum Physics*.

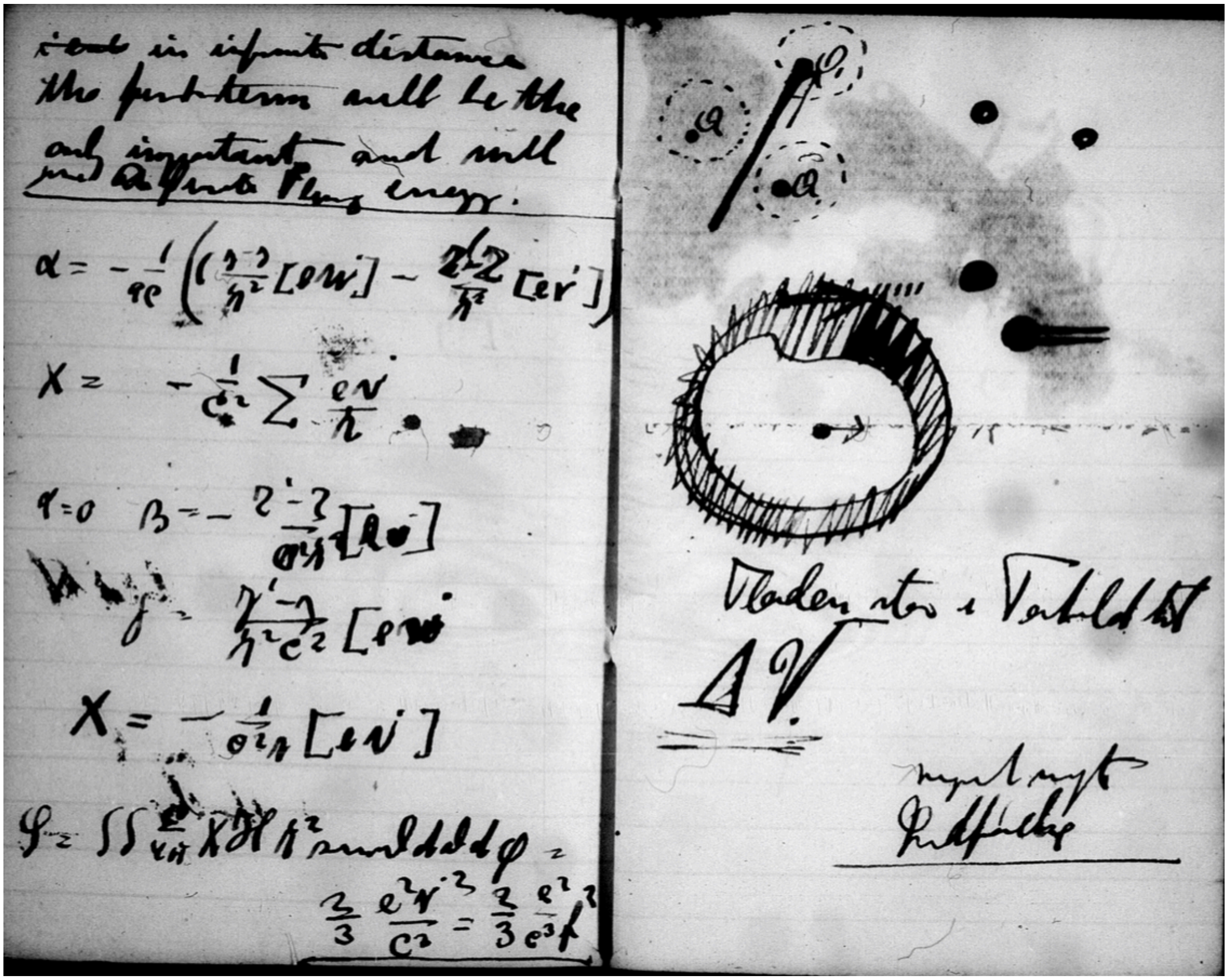
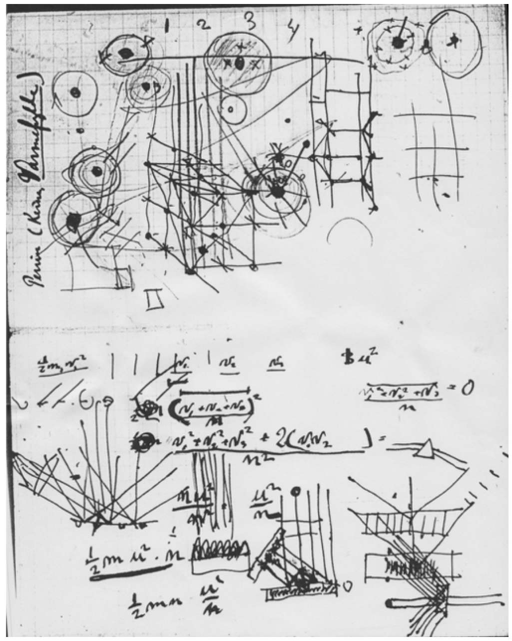
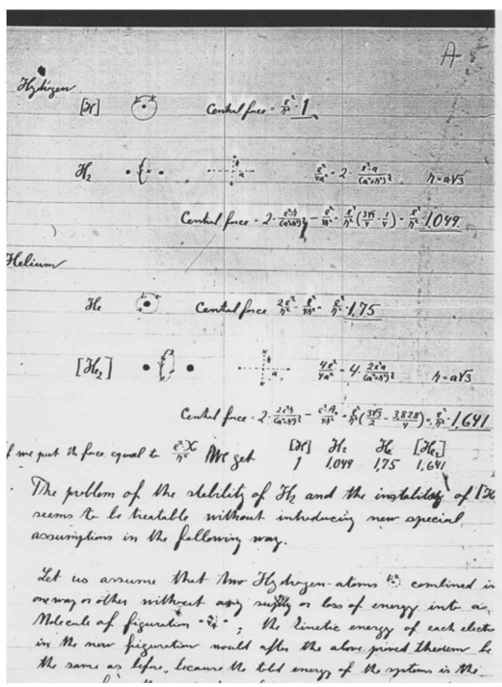


Figure 6: Examples of frequency of drawings in early drafts. "Notes from Jeans" BMSS 3_3, AHQP, Harvard



Figures 7: Midpoint drafts (fourth) of Bohr's 1913 paper on the structure of the atom.
BMSS 3_15 & 20, AHQP, Harvard University



Figures 8: Midpoint drafts (twentieth) of Bohr's 1913 paper on the structure of the atom.
BMSS 3_15 & 20, AHQP, Harvard University

The drawings and visual representations more broadly are generative for Bohr. Early revision practices involved better integration of his drawing with his text, such that the drawings first become supplemental to and finally unnecessary to the text. Figurative language replaces or integrates with a drawing that is no longer central, but rather complementary to the text. For example, in the page preceding a set of drawings of various atoms in the same 1912 draft, there is the description: "Energy of electrons in a ring (consistent of n electrons) circulating around a positive charge concentrated in a point."[21] What is noticeable here is that Bohr describes the shape of the electron formation as a ring, their movement as circulation, and the object that they circle around as a concentrated point. From this description a reader can begin to view the atom in his or her mind even without immediate access to the drawings.

In a late, typewritten translation of Bohr's "Electron Theory of Metals II" Bohr's integration between word and drawing is even more seamless. "If on the contrary we follow the electrons, which during the traveling of their paths, come in contact with the wall, it will be seen that these, as it appears from the figure, under the influence of the magnetic field will get a kind of creeping motion along the wall."[22] Bohr refers to the figure, but he also takes up the task of describing the motion of the electrons with language. His characterization of the electron's creeping along the wall is a personification of the movement of the electron. It helps us to see the electron without the aid of a drawing. Language works independent of drawings in late drafts like this one. The picture helps the reader, but it is not needed to understand what Bohr is saying.

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21 Ibid.

22 Niels Bohr, "On the Electron Theory of Metals Ii," in *Archive for the History of Quantum Physics* (1911), Pg. 167-8.

Rutherford's feedback on Bohr's final draft

Having produced a penultimate draft of his paper, Bohr mailed it off to his advisor and mentor, Rutherford, who was also the editor of the editor of *Philosophical Magazine* in which Bohr planned to publish his articles. Rutherford's feedback, especially about the length of the paper, was substantial.

> I think in your endeavour to be clear you have a tendency to make your papers much too long, and a tendency to repeat your statements in different parts of the paper. I think that your paper really ought to be cut down, and I think this could be done without sacrificing anything to clearness. I do not know if you appreciate the fact that long papers have a way of frightening readers, who feel that they have not time to dip into them. ... I will go over your paper very carefully and let you know what I think about the details. I shall be quite pleased to send it to Phil. Mag. but I would be happier if its volume could be cut down to a fair amount. In any case I will make any corrections in English that are necessary. ... I shall be very pleased to see your later papers, but please take to heart my advice, and try to make them as brief as possible consistent with clearness. ... P.S. I suppose you have no objection to my using my judgment to cut out any matter I may consider unnecessary in your paper? Please reply.[23]

Having received Rutherford's reply and his urging to cut a substantial portion of the paper and his suggestion that Rutherford himself could pare it down, Bohr almost immediately embarked on a journey from Copenhagen to Manchester to meet with Rutherford in person. As he describes in his Rutherford Memorial Lecture noting the "embarrassing" nature of the situation:

> I therefore felt the only way to strengthen matters was to get at once to Manchester and talk it all over with Rutherford himself. Although Rutherford was as busy as ever, he showed an almost angelic patience with me, and after discussions through several long evenings, during which he declared he had never thought I should prove so obstinate, he consented to leave all the old and new points in the final paper. Surely, both style and language were essentially improved by Rutherford's help and advice, and I have often had occasion to think how right he was in objecting to the rather complicated presentation and especially to the many repetitions caused by references to previous literature.[24]

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[23] Letter to Bohr, March 20, 1913, reproduced in "The Rutherford Memorial Lecture," *The Philosophical Writings of Niels Bohr.* 3, p. 41.

[24] Ibid., 42.

Notice that although Bohr ends with kind words about Rutherford's contributions and the wisdom of his suggestions, we learn from the preceding sentence that Bohr changed essentially nothing in response to Rutherford's critique. Rutherford eventually consented, "to leave all the old and new points in the final paper." The archived drafts confirm Bohr's memory of the impact of Rutherford's editorial suggestions. The paper submitted to and published by *Philosophical Magazine is* almost identical to the draft that Bohr sent to Rutherford in March of 1913.

Metaphoric Haunting in the Bohr Atom

Niels Bohr's published paper begins with an acknowledgement of Rutherford's interest and support, "I wish here to express my thanks to Prof. Rutherford for his kind and encouraging interest in this work." Read against Rutherford's critical feedback, this is a reminder that acknowledgements often belie a contentious back and forth between authors and readers. Bohr's refusal to make any of Rutherford's suggested changes even when Rutherford was simultaneously his mentor and his editor, complicates the idea that Bohr's work was dialogic. Like dictation, dialogue was not always Bohr's mode of working, and here and in late drafts of other papers, the dialogue consisted of Bohr refusing at length every edit offered to him by readers, colleagues, and editors. The end of Bohr's writing process was not porous and dialogic but impervious and rigidly inflexible.

At the start of his article "On the Constitution of Atoms" published in Philosophical Magazine in 1913, Bohr offers a neat and very clear summary of his mentor, Rutherford's, atomic model and shows how it accounts for the scattering of alpha-rays.

> In order to explain the results of experiments on scattering of alpha rays by matter Prof. Rutherford has given a theory of the structure of atoms. According to this theory, the atoms consist of a positively charged nucleus surrounded by a system of electrons kept together by attractive forces from the nucleus; the total negative charge of the electrons is equal to the positive charge of the nucleus. Further, the nucleus is assumed to be the seat of the essential part of the mass of the atom, and to have linear dimensions exceedingly small compared with the linear dimensions of the whole atom. The number of electrons in an atom is deduced to be approximately equal to half the atomic weight. Great interest is to be attributed to this atom-model; for, as Rutherford has shown, the assumption of the existence

> of nuclei, as those in question, seems to be necessary in order to account for the results of the experiments on large angle scattering of the a rays.[25]

While Rutherford's atomic model was compelling to Bohr, it raised the question of stability, a problem avoided by previous models such as J.J. Thomson's exceptionally stable plum pudding model. The paper, as Bohr himself explains, is an effort to solve the problems of stability in Rutherford's model of the atom by applying Planck's constant to the binding of electrons to the nucleus. Planck's constant (or the elementary quantum of action) had been used exclusively to explain the behavior of light up until Bohr's application of it to atomic structure. Though Bohr writes that, "The result of the discussion of these questions (specific heats, photoelectric effect) seems to be a general acknowledgment of the inadequacy of the classical electrodynamics in describing the behavior of systems of atomic size," the citation accompanying Bohr's description of consensus around abandoning classical dynamics for atomic sized systems cites just one source: "See f. inst., ' Théorie du ravonnement et les quanta.' Rapports de la réunion à Bruxelles, Nov. 1911. Paris, 1912." This lonely source points toward the fact that there was no such "general acknowledgement." The application of quantum of action to matter was novel. Thirty years later Bohr would still be in search for a general acknowledgement of quantum theory's validity from his colleagues and the public.[26]

Bohr then proceeds with an argument that is theoretical.[27] He sets up the simplest atomic system, a hydrogen atom, with an electron with a negligible mass closely orbiting a massive, positively charged nucleus. He describes a stable elliptical orbit and shows how, if it observes the

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[25] Niels Henrik David Bohr, *On the Constitution of Atoms and Molecules; Papers of 1913 Reprinted from the Philosophical Magazine* (Copenhagen, New York, W. A. Benjamin,: Munksgaard;, 1963).

[26] Niels Bohr, *Atomic Theory and the Description of Nature* (New York: The Macmillan Company, 1934).

[27] By 1911 Bohr was fully a theoretical practitioner. Some of his earlier work had relied on experiment, but here the experiments referred to were all performed by others.

laws of classical dynamics, a stability problem will emerge: the electrons will collapse into the nucleus, or, as Bohr puts it, "approach the nucleus describing orbits of smaller and smaller dimensions, and with greater and greater frequency."[28] An atom with this (Rutherford's) structure would be unstable when atoms are in fact extraordinarily stable, and the energy radiated out by the electron falling to the center would exceed observed energy levels. Working from Planck and Einstein's theories, which suggested the discontinuous emission of light, Bohr proposed a stable version of Rutherford's atom in which electrons could move between the stationary orbits in discontinuous quantum jumps. While Bohr offered a classical explanation for the stationary states themselves, he did not attempt to provide a classical explanation for the movement of electrons between stationary states.

The paper is divided in its scope and tone. It starts broad and bold in its claims to a new consensus about the application of Planck's constant ("the quantum of action") to theories of matter. But at its close the model of the atom that Bohr proposes becomes "preliminary," "hypothetical" merely a "representation" that holds together several different experimental findings. Certainly we can see here the ambitions Bohr has for quantum theory even from its inception balanced by his awareness that it will be met with skepticism. Humility, gesturing repeatedly toward the provisional nature of this theory, can be read as part of a persuasive strategy. The building up of references and connecting his formulas to a variety of experimental findings is another strategy he employs.

Though the footnote early in the paper supporting Bohr's claim to a consensus among physicists is thin, later in the paper there is a density of citations. He supports his model by showing the ways in which it is consistent with observed linear dimension, optical frequencies and ionization-

[28] Niels Henrik David Bohr, *On the Constitution of Atoms and Molecules; Papers of 1913 Reprinted from the Philosophical Magazine* (Copenhagen, New York, W. A. Benjamin,: Munksgaard;, 1963).

potentials. While Einstein, Haas, Stark, Sommerfeld are gestured at, Bohr's most sustained engagement is with Nicholson's observations of the solar corona. Bohr repeatedly references Nicholson's explanation that the homogenous frequency of solar corona radiation can be explained by the vibration of rings of electrons. Nicholson's theory assumes that the ratio between the energy of the system and the frequency of rotation of the ring is equal to an integer multiple of Planck's constant. At the end of his article, Bohr returns again to the significance of the agreement between the observed and calculated values for the constant:

> While, there obviously can be no question of a mechanical foundation of the calculations given in this paper, it is, however possible to give a very simple interpretation of the result of the calculation on p. 5 by help of symbols taken from the mechanics… Denoting the angular momentum of the electron round the nucleus by M, we have immediately for a circular orbit pM=T/w where w is the frequency of revolution and T the kinetic energy of the electron; for a circular orbit we further have T=W.[29]

What captures my interest here is the status of different elements of Bohr's model. Not only does the whole model seem to move from established theory to preliminary ideas and back again, but different elements of the model are treated with differing levels of certainty and physical reality. Some parts of the atom are presented as tools or mere translations to make ideas legible while other things are literal and real.[30] Fundamental in the published paper are analogies between mechanical states, mechanical renderings of the system by which Bohr means that the system can be accounted for "by ordinary mechanics" (classical physics). The movements of the electron can be explained by analogy to classical systems, but the status of mechanics here is translation. Bohr translates his

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29 Ibid.

30 Moving out from claims like this one, generations of scholars would go on to discuss the question of whether he is out to describe a physical reality or just set up a system of calculations that represent experimental findings without giving us purchase on an underlying description of nature. See especially, John Honner and Niels Henrik David Bohr, *The Description of Nature : Niels Bohr and the Philosophy of Quantum Physics*, ed. Anonymous (Oxford Oxfordshire; New York: Clarendon Press; Oxford University Press, 1987). Arkady Plotnitsky, *Complementarity : Anti-Epistemology after Bohr and Derrida* (Durham: Duke University Press, 1994). Mara Beller, *Quantum Dialogue : The Making of a Revolution*, Science and Its Conceptual Foundations (Chicago, IL: University of Chicago Press, 1999).

findings into concepts from classical mechanics for readers, but this is a matter of providing a "very simple interpretation," of using "symbols taken for the mechanics," or "analogy to mechanical states" and classical electrodynamics.[31] The mechanical explanations are useful but they are not real.

The orbit, on the other hand, has a constant, physical reality for Bohr. It is not an analogy or representation: "If we therefore assume that the orbit of the electron in the stationary states is circular, the result of the calculation on p. 5 can be expressed by the simple condition : that the angular momentum of the electron round the nucleus in a stationary state of the system is equal to an entire multiple of a universal value, independent of the charge on the nucleus." The shape of the orbit emerges here as an assumption, its exact shape can be shifted to create a working mechanical model, but the existence of an electron orbit is taken for granted a physical reality. The consistent reality of the orbit is striking in a paper in which so many other parts of the atom are guarded on all sides with the language suggesting that they are provisional analogies.

Beller is among several scholars also interested in figuring out when and how Bohr is making claims about the actual workings of the atom and when he is making less strong claims. {Cite Folse} From Beller we get additional vocabulary to describe the status of different elements of the atom: Some "heuristically useful," others "theoretically adequate," and still others have the status of giving an accurate picture of reality.[32] In her interpretation, Bohr ascribes the strongest physical reality to elements around which he has the strongest position in a live debate with colleagues. So, for example, Bohr doubles down on the physical reality of de Broglie wave-packets when they are crucial to defending his stationary states. (FN Beller) I suggest a different mechanism through which certain elements take on a physical reality for Bohr, one tied closely to his writing practices: Images often repeated in the drafting process move from the merely heuristically useful and

[31] Niels Bohr, *Atomic Theory and the Description of Nature* (New York: The Macmillan Company, 1934).

[32] Mara Beller notices this same pattern in Bohr's Como lecture where a preference for wave ontology hinges on the fruitfulness of the wave for the theory and not description of nature.

representational to the real for Bohr as they are repeated through his unique and extensive drafting process. Through repetition in dialogue and in drafts they lose modifiers that mark them as figurative and representational. They become familiar, indispensable, and real.

Niels Bohr's writing practices hold part of the explanation for the uneven strength and ambition of his claims and for the varying status of different elements of his model, ranging from the analogy to description of reality. These variations in the published version of Bohr's paper are legacies of the manuscripts and correspondence that underlay it. The elements most often repeated in Bohr's drafts begin to have their reality taken for granted in the final, published version. There from the start, the atomic orbits move through the drafts— from circles scrawled in the very first drafts, orbits have been so often repeated that they slip from figurative model, a working analogy to a real, literal orbit, midway through the writing process.

Figurative and Literal Language in The Bohr Atom

Bohr's 1913 model of the atom is a useful case study to track the movement from figurative to literal language across his revision process and the consequences for the model described in the eventual published work. The Bohr atom first appeared in his writing as a picture, an actual figure. A dot of a nucleus is surrounded by line circles and arrows in the margins of his paper. At this point he refers to it as a "ring."[33] In his Nobel speech in 1922, Bohr thinks back on his first models of the atom and uses an analogy between an atom and the solar system to describe his thinking at that time:

In this picture (of the constitution of the atom) we at once see a striking resemblance to a planetary system, such as we have in our own solar system. Just as the simplicity of laws that govern the motions of the solar system is intimately connected with the circumstance that the dimensions of

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[33] Niels Bohr, "First Draft on Atomic Models," 1912, Bohr Manuscripts 1911-12 Film A 603.5 (2)*Archive for the History of Quantum Physics*.

the moving bodies are small in relation to the orbits, so the corresponding relations in atomic structure provide us with an explanation of an essential feature of natural phenomena in so far as these depend on the properties of the elements.[34]

He remembers the neat harmony between the workings of the atom and the workings of the solar system, a harmony that he was struck with "at once." In his first draft "On the Constitution of Atoms and Molecules," the electrons are described as being positioned in rings or stationary states. Soon he adopts the word orbit to describe the electron around the nucleus. This description joins the two systems in the analogy that he mentions in the Nobel speech. He is working within the framework of the initial analogy he makes between the atom's structure and the solar system's structure. His understanding of the structure of the atom is happening through the structure of the solar system.

The laws of classical electrodynamics predict that the atom modeled after the solar system collapses almost instantaneously. These laws and their implications are something Bohr is aware of from the start of his drafting process. As a charged particle, the electron circling the nucleus would radiate and lose energy, spiraling into the nucleus. Thus Bohr was faced with a choice between the planetary model and the classical electrodynamics. He chose the planetary model, stating that the predicted collapses of the atom showed the "inapplicability" and "inadequacy" of "classical electrodynamics" in atomic theory.[35] Even though Bohr stood by much of the shape of the planetary model, the realization that an atom that worked exactly like a planetary system would collapse caused him to loosen his grip on the analogy between the two systems that had helped him generate the model. As he recalls in his 1922 speech, "As soon as we try to trace a more intimate connection between the properties of the elements and atomic structure, we encounter profound difficulties, in

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[34] J Rud Nielsen, ed., *Niels Bohr's Collected Works*, vol. 4 (Amsterdam: North Holland, 1977) Pg. 475.

[35] Ibid., vol. 2 pg. 61-2.

that essential differences between an atom and a planetary system show themselves here in spite of the analogy we have mentioned."[36]

The break with the analogy constitutes the beginning of a literal counter movement in Bohr's writing on the atom. He attempted to describe the atom on completely new terms. Instead of relying on classical electrodynamics he worked with an assumption that Planck had developed in Berlin to explain his experiments with Black Body radiation. Planck determined that heat energy was released in quanta, or little packets of energy, instead of a smooth stream. Bohr incorporated this into his model by suggesting that electrons jumped between discrete fixed orbits that were specific distances from the nucleus.

Metaphoric Haunting

The discarded figure haunts Bohr's writing even after his move toward the literal and concrete. His use of the word "orbit" is one of the first signs that he is generating his understanding of the atom through his understanding of the solar system. Long after he has abandoned that comparison, he still talks about the electron's orbit. The ghosts of the dead metaphor are the circular and elliptical orbits. Why are there literal orbits in an atom that is barely even figuratively a planetary system? Bohr attends meticulously to the differences between the two systems, and when he mentions the analogy, he is careful to mention it as an analogy and list its differences. Why does he not extend this same caution to the term "orbit"? And what does this loyalty to a literal orbit do to his final model of the atom?

How does the orbit change from being a figurative tool that causes Bohr to glance back and forth between the atom and the solar system to being a literal property of the atom that holds on even when the metaphor that it is attached to has been abandoned? Let us think for a moment

[36] Ibid., vol. 4 Pg. 76-77.

about how the figurative becomes the literal in a broader scheme. Donald Davidson gives us the examples of the mouth of a bottle and the mouth of a river. Initially both of these terms were metaphorical. "The application made the hearer notice a likeness between animal and bottle openings. Once one has the present use for the word, with literal applications to bottles, there is nothing left of notice. There is no similarity to seek because it consists simply in being referred to by the same word."[37] When and why does that transformation happen?

Scholars like Davidson working on the blurry line between figurative and literal suggest that the mouth of the bottle becomes the literal mouth of a bottle has to do with the way that we learn the term. The first person who makes the comparison is attentive to the likeness between two separate concepts. The first people he tells may also engage in the act of comparing two things. But people who do not yet have a term for the opening at the top of the bottle may accept this as simple vocabulary. In time, repetition of the term makes "mouth" belong as much to bottle as it does to animals, so we no longer jog back and forth between two separate ideas when we say or hear the term. Bohr's treatment of the orbit of the electron follows the same pattern. Through significant repetition in drafts and notes and letters the fact that the term orbit has been borrowed from another system gets forgotten.

Metaphors take root in the scientific community through repetition of their communication. The act of teaching and explaining canonizes metaphors and sometimes changes them into literal terms. David Kaiser in his work on the dispersion of Feynman diagrams has observed that there are many examples of representational schemes that were introduced as mnemonic devices but gain for the next set of practitioners, and "added sense of realism."[38]

[37] Ibid. Pg. 35.

[38] Kaiser, Drawing things apart, 368.

Bohr's theory of the atom is characterized by an attachment to the "orbit" through all stages of the draft. We can see metaphor taking a more central role in Bohr's thinking in the development of the word orbit through his drafts. At first Bohr's use of the word "orbit" is borrowed in order to explain a kind of motion in the electron in terms of the planetary model. At the point which the planetary metaphor is loosened into a cautious analogy, Bohr has already repeated the metaphor of the electron orbit in so many drafts and conversations that he no longer treats it as borrowed. Through a composition writing process that involves so much repetition in dialogue and writing, the figurative orbit becomes literal. It acts as an essential part of his model and a foundation for the ad hoc physics he creates in order to make his model possible.

Toward a New Reading of Scientific Drafts

Reading Bohr's drafts requires engaging with three communities of scholars that often talk past one another: scholarly editors interested in how to treat drafts in edited editions, literary critics articulating how the meaning of a text resides in the process through which it was composed, and historians and sociologists of science focused on representation. Collectively these scholars draw our attention to revision and allow for a reinterpretation of Bohr's drafts. Instead of seeing drafts as unfolding toward an ultimate and fixed intention, embodied by the published article or as progressive attempts to uncover and describe and object that exists outside of the text, we can view drafts as the place where the theory, in this case the Bohr atom, is assembled. My reading of Bohr's work privileges the meaning of the text that emerges from the process rather than the meaning that emerges with a close reading of just the final draft/outcome.

This approach allows me to avoid a teleological interpretation of the drafts in which all the compositional materials along the way are viewed as leading up to a final draft that represents true authorial intention as they have sometimes been interpreted by others. Instead, borrowing from textual critics in conversation with literary scholars, I treat each draft as having equal value for understanding the meaning of the work and each revision as contingent on dialogic and material contexts and on the labor of several people. The task of this chapter is to describe the revision process, the changes, their contexts, and to identify the people responsible for the revisions and the impact of their edits and comments. I would like, as Ferrer suggests in *Logiques du brouillon* to view the article and preceding manuscripts cinematically, to see the drafts not as still photographs but as a film, where the change, the motion between drafts is our object of our critical analysis and the source of meaning. By seeing the article and preceding manuscripts cinematically, we see both how changes made in revision reveal debates and thematic commitments alive in broader community of physicists and the consequences that the mechanics of a particular revision practices have on the finished work.[39]

Textual critics and scholarly editors have engaged in conversations about the drafts and compositional material that precede a published work for the past 40 years. They have vigorously debated the relationship between writing processes and products and the role that earlier versions ought to play in the production of authoritative scholarly editions. Though their central concern, how to produce a definitive scholarly edition of a text, is not my own interest here, the questions they have engaged in pursuit of this goal are instructive to my work with Bohr's drafts, providing me with methodological and theoretical tools. They debate whether a published version necessarily represents the culmination of an author's intent. They try to distinguish between different types of revisions and categorize. They provide ways to theorize what had been taken for granted as "authorial intent." All of these are welcome and needed moves in the history of quantum theory,

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[39] Daniel Ferrer, *Logiques Du Brouillon: Modèles Pour Une Critique Génétique*, Poétique (Paris: Éditions du Seuil, 2011).

where the approach to Bohr's drafts thus far has been to view final, published versions as the manifestation of his intention and preceding drafts as less important stepping stones.[40]

It was assumed from the publication of Walter Greg's "The Rationale of Copy-Text" in 1950 until the 1970s that the final, published version of a text ought to be privileged by scholarly editors because it represented the form of the work that the author "wished the public to have."[41] This concept of preserving and editing for authorial intention and privileging a final version went largely unquestioned until the 1970s when scholars seeking to emphasize the social and collaborative aspects of textual production began to suggest ways de-emphasize the singular nature of authorial intention by taking earlier versions of a text and revision more seriously. Michael Hancher and Thomas Tanselle tried to give nuance to authorial intention by breaking it apart into three kinds of authorial intention: programmatic, active and final (Hancher) and by classifying revision as horizontal and vertical (Tanselle). As McLaverty points out in "The Concept of Authorial Intention," though both Hancher and Tanselle attend to iterations and drafts along the way and the contexts of those revisions, they are still committed to authorial intention and to what McLaverty criticizes as an unexamined philosophical commitment to the idea that there is one intention or conception running through various versions of the work[42]. McLaverty raises the question of how to attend to the motives and contexts for the changes between versions of a text without viewing them as following the line of authorial intent toward an inevitable and complete final edition. What would

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[40] Sara Miglietti also weighs in on the role that context should play in explaining revisions between editions or iterations of a draft in her interpretation of how much context can explain Bodin's shift from a concept of limited sovereignty in the Methodus to a concept of absolute sovereignty in the Republique,"Meaning in a Changing Context: Towards an Interdisciplinary Approach to Authorial Revision," *History of European Ideas* 40, no. 4 (July 2014): 474–94, doi:10.1080/01916599.2013.826431.

[41] W. W. Greg, "The Rationale of Copy-Text," *Studies in Bibliography* 3 (1950): 19–36.

[42] James McLaverty, "The Concept of Authorial Intention in Textual Criticism," *The Library* s6–VI, no. 2 (1984): 121–38, doi:10.1093/library/s6-VI.2.121.

we privilege instead? The elements of the text that persist throughout the drafts for the longest? The patterns or trends of the changes?

Recent work suggest methods for privileging the process of textual production over the final product as a way to escape the teleological trap of authorial intention. Shillingsburg's *Resisting Texts* suggests describing texts as processes or sets of activities and not objects. Bushell, in "Intention Revisited", suggests that the only way to answer the questions, what did the author mean is to ask the more fundamental question, "How did the text come into being?"[43] Shillingsburg and Bushell both argue that genetic questions about a text's origins and how a text is made underlay and ought to be answered before questions about what a text means.[44]

While the debates among scholarly editors alert us to the dangers of viewing a set of drafts as possessing a directionality toward a final product driven by an author's intent, historians and sociologists of science working on representational practices caution us against the privileging of origins. As Woolgar and Lynch argue in their introduction to *Representations in Scientific Practice,* the "direction" in a series of representations is not a movement away from or toward an originary reality, but a movement of an "assembly line."[45] (8) That is, the representations that scientists create and circulate are usually assumed to refer to an original, underlying object. Scientific representations are assumed to "penetrate" the natural world and to bear a "genetic" resemblance or be a "'mathematical'" reconstruction of an original. In Foucauldian terms, representations or series of representations like Bohr's drafts, are understood by scientists and by many historians of science to

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43 Sally Bushell, "Intention Revisited: Towards an Anglo-American 'Genetic Criticism,'" *Text* 17 (2005), 67.

44 Bushell provides a schematic overview of French, German, and Anglo approaches to compositional criticism. French Genetic Criticism or *La critique genetic* emergered in the 1970's with an emphasis on pre-text (*avant-texte)* as fluid entity representative of many possible meanings. See especially Gresillon "La critique genetic, au gourd' hui et demand"

45 Michael Lynch and Steve Woolgar, *Representation in Scientific Practice*, 1st MIT Press (Cambridge, Mass.: MIT Press, 1990).

resemble one another, and this specific understanding of resemblance "presumes a primary referent, an original that any copy renders in a partial or imperfect way." Lynch and Woolgar argue here for something closer to similitude: a system of representations "which dance together, tilting and tumbling over one another."[46]

In other words, we might be tempted to imagine, as indeed Bohr does, that there is an underlying reality, a source that each stage of drafting and revision is revealing or reconstructing with various and layering degrees of success. Within this assumption, for this set of 1913 drafts, we would assume that there was a real atom that all of Bohr's writing and stacks of revision refer back to with greater precision. If that were the case, our task in interpreting these drafts would be to understand how each revision removed obstacles, peeling back layers of onion skin until the final version achieved an unobstructed view of the real atom.

What we can see instead as we spread out Bohr's drafts, viewing each of them as we would a frame in a film, is not a process that clears the debris clouding an already present truth, but rather a process of accumulation and construction. Bohr's revisions and writing assemble and construct the atom, pulling materials and tools from correspondence and earlier drafts. The progression of drafts presented in the secondary sources is a process of honing in on a truth that exists outside of the work, but here, in these drafts, we see the atom being built.

[46] Michel Foucault, *This Is Not a Pipe*, 24 (Univ of California Press, 1983), 46-48 cited in Michael Lynch and Steve Woolgar, *Representation in Scientific Practice*, 1st MIT Press (Cambridge, Mass.: MIT Press, 1990), 6.

Chapter 4: Reading the Atom

A baroque tower upon the Gothic base of classical electrodynamics

Niels Bohr's 1913 application of quantum theory to a model of the atom was of interest to physicists, chemists and astronomer. Across these communities the theorists and experimentalists who were attracted to Bohr's articles generally praised its predictive and explanatory power. Those who found it unpalatable (often the same people who found it attractive) criticized the lack of mechanism for a physical picture and found the theory's architecture to be a messy, patchy combination of incomparable elements from quantum theory and electrodynamics. As Henry Margenau later explained the latter critique, "Bohr's atom sat like a baroque tower upon the Gothic base of classical electrodynamics."[1] This was a criticism that would persist as atomic theory evolved into quantum theory and finally quantum mechanics. As Mara Beller explains it, quantum theory and quantum mechanics' refusal to yield a picture became a heuristic barrier that continued to earn it a cold reception with some physicists.[2]

Reading and Reception Practices

The reception story of Bohr's model of the atom is full of reminders that reception is comprised of practices, too. The aim of many reception studies in the history of physics is to use a national, language, or local community's reactions to a new theory or idea to better understand the style of physics practiced by that community; that is, by looking at the reception of special relativity as presented by Einstein in *Annalen der Physic* by physicists working in different institutions and national

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[1] Henry Margenau. *The Nature of Physical Reality; a Philosophy of Modern Physics.* 1st ed. (New York: McGraw-Hill, 1950), 166.

[2] Mara Beller cites the physicist Herman Weyl's complaint that "There was simply no space time gravy... in the matrix representation (math used in quantum mechanics)." Quantum mechanics was constructed more exclusively from mathematical expressions and lacked intuitive footholds and the tastiness of pictures provided by classical mechanics. Mara Beller, *Quantum Dialogue : the Making of a Revolution*, Science and Its Conceptual Foundations (Chicago, IL: University of Chicago Press, 1999), 30.

contexts, we can better understand the experimental priorities and the traditions of physics operating in those communities.[3] Einstein's article is constant, and yet what readers seem to take from it is very different, so what historians of science can describe from reception is the lens through which they are interpreting the theory and the institutional and cultural factors that inform that particular angle of interpretation. This approach to reception has yielded tremendous insights into the traditions and research priorities of different physics communities, and it is largely the approach I follow outlining the reception of the Bohr Atom in different national and language communities below.[4] But at the same time, it suggests something automatic about how a text gets in front of a reader and also something passive about how it is read. It also assumes that readers are reading the same thing.

When we think of the reception of the Bohr atom, we should not imagine of legions of physicists bent over identical copies of the July, September and November 1913 issues of *Philosophical Magazine,* reading "On the Consitution of Atoms and Molecules" front to back just as carefully as Bohr would have hoped.[5] Certainly many physicists learned about Bohr's model of the atom through his published articles in *Philosophical Magazine,* but even these readers would have

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3 For excellent examples of these kinds of reception studies see: Stanley Goldberg, "In Defense of Ether: The British Response to Einstein's Special Theory of Relativity, 1905-1911." *Historical Studies in the Physical Sciences* 2 (1970): 89–125; and Thomas Glick. 'Cultural Issues in the Reception of Relativity', in his (ed.) *The Comparative Reception of Relativity,* Boston Studies in the Philosophy of Science ; v. 103. Dordrecht ; (Boston: DReidel PubCo; Norwell, MA, 1987).

4 Writing in reference to the reception of relativity, Warwick argues against reception as a way to compare "national styles of physics" noticing that even within Cambridge special relativity was received very differently and a very different meaning was ascribed to it by different physicists. Reception, he argues, should instead be viewed as a powerful tool for the comparative study of far more local traditions of physics. In these contexts reception reveals "networks of collaboration and competition that are neither typical or, nor necessarily bounded by, the nation state." Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905–1911 Part I: The Uses of Theory." *Studies in History and Philosophy of Science Part A* 23, no. 4 (December 1992), 627.

5 Niels Bohr, "On the Constitution of Atoms and Molecules," *Philosophical Magazine* **26** (1913), 1-25; Part II. "Systems containing only a Single Nucleus," ibid., 476-502; Part III. "Systems con- taining Several Nuclei," ibid., 857-875.

received it through different channels and read it in different contexts. While some received the article through their own subscriptions to *Philosophical Magazine,* many others were sent pre-prints from Bohr, accompanied with correspondence directing their interest. Far more readers' first, and perhaps only, exposure to Bohr's ideas would come not from reading the full article with all of Bohr's finely tuned phrases but instead from abstracts or summaries. Bohr's articles were extensively abstracted in the English language *Science Abstract*s and the German language *Beiblätter* of the *Annalen der Physik*. These publications had a far greter readership than *Philosophical Magazine,* and the abstracts contained within them emphasized certain aspects of Bohr's articles while de-emphasizing or excluding others. Still other physicists would not have learned of Bohr's atom through any form of published writing, but instead through conversations at conferences, colloquium talks, and private correspondence.

While this chapter does not take on the practices of reading and reception with the same level of granular detail paid to dictation and revision practices in the previous chapters, I nonetheless seek to remind the reader that reception also involves rich and varied practices. Journal articles were mailed, carried on trains, shipped where and when they could be, and prevented from being shipped other places, particularly during WWI when correspondence between the United States and Europe stalled. {Missing citation} Bohr's theories were strategically ignored in formal conference preceding while actively debated in conversations in the hallways and courtyards between sessions. Reception, particularly in the United States, depended on the strong advocacy of a few key figures who included Bohr's atomic model on course syllabi and produced textbooks that included Bohr's theory.

United Kingdom: The "very weighty" justification of success

The earliest and liveliest reception of Bohr's atomic model occurred in the United Kingdom. As Helge Kragh has already noted, Bohr's involvement in British scientific societies and core institutions goes a long way toward explaining his reception in the U.K.[6] Although Bohr wrote "On the Constitution of Atoms" with his wife Margrethe Bohr in Denmark on their honeymoon, this period of intensive writing was preceded and followed by life and work in England. Bohr had sequential postdocs at Cambridge (with J.J. Thomson) and Manchester (with Ernest Rutherford) before the publication of his article, and then regularly attended meetings of British scientific societies through 1915. He also published in this period exclusively in the British journals *Nature* and *Philosophical Magazine*.

Because of his own personal and professional connections forged through his time in the United Kingdom, Bohr and his theory had powerful allies and advocates there. Ernest Rutherford, who had supervised Bohr's research in Manchester when he first took up questions about the structure of the atom, provided the earliest feedback to Bohr on his 1913 draft articles and became the atomic theory's earliest advocate. While Rutherford voiced concerns about the model in his private correspondence with Bohr in the months immediately preceding publication, after publication, he was a staunch supporter for Bohr's new theory in both his private correspondence and his public published articles. In an exchange of letters immediately preceding the publication of "On the Constitution of Atoms" Rutherford expressed concerns about how the style and length of the article that might hinder its reception. In these letters he also expressed concerns about how the electron "decided" where to be and which quantum transition to perform.[7] But after publication, Rutherford took on the role of advocating for Bohr's theory with far less ambivalence. In supporting Bohr's theory of the atom, Rutherford was supporting the a theory that extended and

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[6] Helge Kragh, "Reception and Early Developments" in *Niels Bohr and the Quantum Atom: The Bohr Model of Atomic Structure 1913-1925* (Oxford: Oxford University Press, 2012).

[7] Letter to Bohr, March 20, 1913, reproduced in "The Rutherford Memorial Lecture," Niels Bohr, *The Philosophical Writings of Niels Bohr* (Woodbridge Connecticut: Oxbow Press, 1963), 41.

justified his own earlier version of the nuclear atom.[8] In the October 1913 issue of *Philosophical Magazine* Rutherford cited and endorsed Bohr's work by referring to the hydrogen and helium atoms "assumed by Bohr in a recent interesting paper on the constitution of atoms, and which have been shown by him to yield very promising results."[9] Writing a year later on "The structure of the atom" Rutherford acknowledged the challenges of accepting Bohr's new theory and framed Bohr's work as a promising beginning to tackling a fundamental problem, "There no doubt will be much difference of opinion as to the validity of the assumptions made by Bohr in his theory of the constitution of atoms and molecules, but a very promising beginning has been made on the attack of this most fundamental of problems, which lies at the basis of Physics and Chemistry."[10] Rutherford's support continued even, and perhaps especially, in circumstances where it would not have been politically savvy for the young Bohr to advocate for his theory himself. Rutherford was outspoken, for example, in his criticism of J.J. Thomson's ongoing efforts to produce a model of the atom that ignored Bohr's work and theories; Bohr himself remained mostly silent on the topic.

Outside of the most immediate audience of Rutherford and Bohr's other colleagues at Manchester, Bohr's theory reached the broader scientific community of British physicists not only through his publications in *Philosophical Magazine*, but also through the discussion of his theories at meetings of scientific societies. As early as the fall of 1913, Bohr's atomic model was a topic of prominent discussion at British conferences, and through these meetings and the publications that came out of them, British physicists became aware of Bohr's theory. Bohr himself traveled to Birmingham for the September 1913 meeting of the British Association of the Advancement of

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8 See Ernest Rutherford, "The scattering of α and β particles by matter and the structure of the atom." *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 21, no. 125 (1911): 669-688.

9 Ernest Rutherford and John M. Nuttall, "LVII. Scattering of α particles by gases." *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 26, no. 154 (1913): 702-712.

10 Ernest Rutherford, 'The structure of the atom', *Scientia* **16**,(1914): 351.

Science to advocate for atomic theory. Oliver Lodge's Presidential Address briefly referenced Bohr's theory in spite of Lodge's own deep skepticism regarding any and all departures from classical theory, indicating that, in Great Britain at least, even skeptics could not completely ignore the general excitement about Bohr's papers. Several other high profile lectures mentioned Bohr's theory, including James Jeans' lecture, which explained Bohr's atomic model in great detail and offered praise that echoed the broader reaction to Bohr's theory by British physicists, "The only justification at present put forward for these assumptions is the very weighty one of success."[11] Witnessing Jeans and others discussing his theory, Bohr was encouraged. He described his theory's reception in a letter to his wife, Margarethe, "I think that he is convinced that there is at least some reality behind my considerations."[12] By referencing Bohr's work at conferences and professional meetings, well-established figures in British science amplified the reach of Bohr's atomic model.

Bohr's atomic theory reached still wider circles of English speaking scientists in the United Kingdom and the United States through *Science Abstracts*, an abstract journal produced by the Institution of Electrical Engineers in collaboration with the Physical Society of London (and, in the United States, the American Physical Society and the American Institute of Electrical Engineers). In *Science Abstracts,* George De Tunzelmann summarized *On the Constitution of Atoms* in a way that portrayed it as displacing J.J. Thomson's model: "The author's primary aim is to show that the introduction of Planck's constant, the elementary quantum of action, will serve, in Rutherford's

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[11] James Jeans, "Discussion on Radiation," *Report, British Association for the Advancement of Science*, 1913, 379.

[12] Niels Bohr to Margarethe Bohr, September 14, 1913, Niels Bohr Archive, Copenhagen. Helge Kragh, "Reception and Early Developments," in *Niels Bohr and the Quantum Atom: The Bohr Model of Atomic Structure 1913-1925* (Oxford University Press, 2012).

model, to take the place of the radius of the positive sphere (from Thomson's model), and so make stability possible."[13]

Because the prominent references to Bohr's theory in journals and conferences and the widely read abstracts emphasized the unproblematic successes of Bohr's model— that it was a stable model that fit with experimental results— many British physicists failed to notice how drastically Bohr's atom departed from classical physics. They did not realize, for example, that Bohr's theory required accepting that classical mechanics were not applicable in systems of atomic dimensions. British physicists who realized the radical departures from classical physics implicit in Bohr's atomic theory often opposed it. The Cavendish physicist Norman Campbell was one of the earliest prominent readers to praise the atomic model as a mathematical abstraction but critique any claims it might be making as a description of reality. As a mathematical representation it was "simple, plausible, and easily amenable to mathematical system" but "to attempt to explain Bohr's theory in terms of [classical] principles is useless."[14] Though the central claim of Campbell's critique, that Bohr ought to let the model be *just* a model, was a fairly rare line of critique in written reception at the time, it was a more prominent if implicit line of critique embodied by other atomic models of the day.[15] Campbell's concerns foreshadowed the debate that would embroil theoretical physicisits a decade or so later when questions about whether quantum mechanics should be a system of effective but hard to visualize approximations or a plain description of nature came to a head.

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13 George Tunzelmann, "Abstract of Bohr 1913c," *Science Abstracts*, Series A, no. Physics 17 (1914): 6–8.

14 Norman Campbell, "The structure of the atom." *Nature* 92 (1914): 586-587.

15 See "Atomic Models as a Site of Reception" below.

Germany: the "completely monstrous" work of "an original genius"

Reception of Bohr's atomic model came later in Germany with fewer German physicists citing his work or discussing it formally in conference papers between 1913 and 1915, the first two years after publication of his articles in *Philosophical Magazine*. It is clear from correspondence that Bohr's theories reached a small circle of German physicists in 1913 and were discussed vigorously if informally at conferences. Germany was home to some of Bohr's fiercest critics and allies, and he faced more fundamental and widespread concerns from German colleagues about the project of describing or modeling the atom itself.

To look at the published record of German physics journals and conference proceedings, one would assume that Bohr's ideas did not penetrate German physics until late spring 1915, but there was vigorous engagement, both critical and supportive, of Bohr's work traveling in informal and unpublished channels long before then. Most readers of *Annalen der Physic,* the primary journal for the German physics community, would not have come across Bohr or his atomic model between the years 1913-1915. German[16] physicists would also not have heard about Bohr's work through attending the official talks given at conferences or reading the conference proceedings. Physicists from Munich, Zurich and Gottingen gathered at two important conferences immediately following the publication of Bohr's articles in *Philosophical Magazine* without a single conference paper directly engaging with Bohr or Bohr's articles. First, physicists from Munich and Gottingen were present at the second Solvay conference in Brussels October 27-31, 1913. Despite the conference theme of "La Structure de la Matiere," none of the published papers referenced Bohr's work. The annual meeting for Gesellschaft deutscher Naturforscher und Ärzte (Association of German Scientists and Physicians) took place in Vienna in the fall of 1913 after the publication of Bohr's atomic theory.

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[16] Bohr's theory was published in the more minor German journal, *Verhandlungen,* where the first German research paper relating to Bohr's theory was published by Emil Warburg in Dec. 1913.

And, once again, none of the published addresses by Einstein, James Franck, Max von Laue, Johannes Stark, and Max Born refer to Bohr or Bohr's theory. This forms a strong contrast with reception in Great Britain where Bohr's work was almost immediately cited in prominent journals and discussed as a part of prominent papers given at conferences and professional meetings. Even though Bohr's work was not cited in prominent journals or discussed as a part of conference presentations attended by German speaking physicists, correspondence and colloquia programs reveal that by the spring of 1914 Bohr's work was well known throughout German speaking Europe. Following the Gesellschaft deutscher Naturforscher und Ärzte meeting, Hevesy wrote to Rutherford about Einstein's positive reaction to Bohr's work. Geiger wrote directly to Bohr offering congratulations and asking for pre-prints for a colleague. {Insert Citations}

If Bohr's theories were not widely published or featured in the official proceedings of German speaking conferences, how did they become known to figures like Einstein and Hevesy? Bohr's ideas rose to the awareness of German speaking and reading physicists through correspondence, in person visits, and the extensive abstracting of the three articles in he *Beiblätter* of the *Annalen der Physik* and also in the *Chemische Central-Blatt.* (Seeliger 1914a.) Bohr's atomic model first reached Germany through pre-prints Bohr himself sent of the July issue of Philosophical Magazine to several colleagues.[17] Arnold Somerfeld, then a professor of theoretical physics in Munich, received his copy from Bohr in September of 1913. In Leon Brillouin's recollection, "Sommerfeld immediately saw the importance of this new idea. I happened to be in his office when he opened the issue of Philosophical Magazine, which had just arrived; he glanced through it and told me, 'There is a most important paper by N. Bohr, it will mark a date in theoretical physics." Brillouin's recollection seems colored by later knowledge of the impact of Bohr's theories and an impulse to claim to have recognized it from the start. Sommerfeld's September 4, 1913 postcard to

[17] Kragh, "Reception and Early Developments." Oxford University Press, 2012.

Bohr expresses praise but also a healthy dose of skepticism: "I thank you very much for sending me your highly interesting work, which I have already studied in Phil. Mag. The problem of expressing the Rydberg–Ritz constant by Planck's *h* has for a long time been on my mind. Though for the present I am still rather sceptical about atomic models in general, calculating this constant is undoubtedly a great feat...From Mr. Rutherford, whom I hope to see in October, I may perhaps learn more details about your plans."[18] Sommerfeld's note to Bohr demonstrates a theme running through the broader reception of Bohr's atomic theory in Germany. Many German physicists were excited by the calculation of the constant, but remained wary of the project of atom building more generally, which, according to Kragh, they recognized as belonging to the priorities of British physics at the time. In the United Kingdom, several physicists were working on models of the atom, but that was not part of the training or research programs on the continent.[19]

From Göttingen, Harald Bohr reported in a letter to his brother Niels interest mixed with skepticism, "People here are still exceedingly interested in your papers, but I have the impression that most of them—except Hilbert, however—and in particular, among the youngest, Born, Madelung, etc., do not dare to believe that they can be objectively right; they found the assumptions too 'bold' and 'fantastic.'"[20] Harald Bohr may have been softening the news for his brother. The mathematician Richard Courant described a somewhat cooler, sometimes violent reception for Bohr's theory at Göttingen. Courant had known Bohr personally through a meeting in Cambridge in 1913 and found his theories immediately persuasive. But he had a hard time convincing his

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[18] Niels Bohr, *Collected Works: Work on Atomic Physics (1912-1917)*. Edited by Ulrich Hoyer. Vol. 2. 13 vols. Amsterdam: North-Holland Pub-2008, 1981.

[19] Helge Kragh, "Reception and Early Developments." Oxford University Press, 2012, 123.

[20] Harald Bohr to Niels Bohr, undated but most likely October 1913, in Niels Bohr, *Early Work (1905-1911), Edited by J. Rud Nielsen* (North-Holland Publishing Company, 1972), 567.

colleagues at Göttingen, "When I reported these things here in Göttingen, they laughed at me that I should not take such fantasies seriously."[21]

In person visits by Bohr also played a role in elevating awareness and debate about his theories. Bohr traveled to Germany to give a series of small talks at both Göttingen and Munich. As Alfred Landé later recalled, "He spoke rather poor German with his usual soft voice, and in the front row were all the big wigs. 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 abolutely 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."[22] All of the sources for reception in Gottingen report a generational divide with younger physics mostly excited if skeptical about the findings while older and more established physics dismissed it with energy.

Paul Ehrenfest who would later become a great friend of Bohr's and defender of Quantum Theory was one of the slowest to warm up to the theory. In a letter to Lorentz in August of 1913 he expressed immediate and complete dislike of Bohr's theory and of the threat it posed to physics, "Bohr's work on the quantum theory of the Balmer formula (in the Phil. Mag.), has driven me to despair. If this is the way to reach the goal, I must give up doing physics."[23] As late as 1916 Ehrenfest continued to refer to Bohr's model as "completely monstrous."[24]

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21 Constance Reid, *Hilbert-Courant* (Springer Science & Business Media, 1970), 45.

22 Interview with A. Landé, 5 March 1962, by Thomas S. Kuhn and John Heilbron. American Insitute of Physics, Niels Bohr Library & Archives. http://www.aip.org/history/ohilist/4728_1.html

23 Ehrenfest to Lorentz, 25 August 1913, as quoted in Martin J. Klein, Author, Robert C. Heilborn, and Reviewer. "Paul Ehrenfest, Volume 1, The Making of a Theoretical Physicist." *American Journal of Physics* 39 (1971): 1546, p. 278

24 The German phrase is 'gang kanibalischem.' Ehrenfest to Sommerfeld, April-May 1916, in Sommerfeld, Arnold. *Wissenschaftlicher Briefwechsel*. Edited by Ackert and Marker. Berlin: Deutsches Museum, 2000.

German reception provides an especially clear example of the importance of informal circulation of pre-prints in the dispersion of Bohr's ideas. {Elaborate across a few more sentences}

United States: Overcoming Nicholson

Bohr's theory was received even later and with less interest by scientists in the United States. Until 1916 there was no mention in the journal of the American Physical Society of the Bohr atom. Physics in the United States was overwhelmingly experimental during this period, so developing a description of the structure of the atom was not a priority within their research. . By 1915 there was general awareness of the Bohr atom, with most physicists in the United States holding a hostile view of it. The physical scientist community warmed to Bohr's theory in 1916 after generally preferring Nicholson's models of the atom up to that point. We can trace the mainstream acceptance of Bohr's theory by the American physics community to Robert Millikan's reference to the theories in his 1916 address to the American Physical Society at their annual meeting in 1916. Millikan was president of the society, and in his address he framed the standing objection to Bohr's theory, that it gave no picture of the mechanism of the electron's jump from orbit to orbit, as a strength instead of a weakness, stating that Bohr's theory was similar to fundamental laws of thermodynamics which were "true irrespective of a mechanism."[25]

Although the Bohr atom reached physicists in the United States through English language publications like *Nature* and *Philosophical Magazine*, its reception truly hinged on a few key figures who took on the role of zealously promoting Bohr's work. Raymond Birge, a self-described, "real missionary of the Bohr atom" lectured on the Bohr atom as early as 1913. Owen Willans Richardson, a Cavendish physicist who became a professor at Princeton in 1906, included Bohr's

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[25] Robert Andrews Millikan, *The Electron: Its Isolation and Measurement and the Determination of Some of Its Properties* (University of Chicago Press, 1917), 326.

model of the atom in his July 1913 textbook the *Electron Theory of Matter* describing it as an alternative to Thomson's model of the atom. In a second edition of *Electron Theory of Matter* published in 1916, Richardson continued to present Bohr's model as an alternative to Thomson's but described the Bohr atom at greater length and in much more detail.

The reception of the Bohr atom in the United States also draws our attention to the impact of WWI on the spread of ideas in theoretical physics. In 1914 correspondence between physicists in Europe slows and moves to the trenches. While news of Bohr's model of the atom crossed the Atlantic with relative ease before communication was slowed by WWI, Sommerfeld's updates to the theory did not reach the United States until after the war, leaving physicists in the United States less persuaded of Bohr's theories than scientists in Europe who were more aware of further research that validated and elaborated Bohr's theories.

Importance of Informal Networks

The reception of Bohr's atomic model across these national contexts makes clear the importance of informal modes of transmission, both oral and written, in the reception of ideas by theoretical physicists. I am reminded of what I take to be one of the central claims of Latour's ethnography *Laboratory Life*, that most scientific communication takes place in talking, but when they talk, what they talk about most is writing.[26] Bohr's theories traveled through and were debated in mostly unpublished routes and networks. Oral histories and archived correspondence suggest that many prominent physicists first learned of Bohr's theory by receiving a pre-print sent to them directly from Bohr or, almost as frequently, through an in person meeting with Bohr. These pre-prints, accompanied by letters that directed the reader's attention, caused colleagues like Arnold

Sommerfeld, to give the Bohr's theory a second read because they felt compelled to respond to Bohr. Informal, in person meetings also play a prominent role. Owen Willans Richardson, the Princeton professor responsible for bringing Bohr's theory into his textbook in the United States, learned of Bohr's theory not through *Philosophical Magazine* but through a conversation he had with Bohr while visiting Cambridge in 1913. Richard Courant, Bohr's earliest and, for awhile, only advocate at Göttingen, promoted Bohr's atomic model not based on the article but based on their conversations when they also met in Cambridge in 1913. And though published conference proceedings would indicate that physicists in Germany and the United States were largely ignoring Bohr's atomic model for the first few years after his articles were published, private correspondence between conference attendees and from attendees to Bohr indicates that Bohr's ideas were in fact hotly debated in the corridors.

Rival Atomic Models as a Site of Reception

We can see how physicists responded to Bohr's atom in the range of ways discussed above—private correspondence, published articles, colloquium talks, private conversations and public conference presentations, syllabi and lecture notes, course descriptions, and contributions to text books. We can also see the reception in the competing atomic models that coexisted alongside Bohr's. Niels Bohr was by no means the only physicist to take on the task of describing the structure of the atom. Both J.J. Thomson, his supervisor at Cambridge, and Ernest Rutherford, his supervisor at Manchester, were involved in efforts to produce models of the atom before Bohr, and structures were proposed around the same time and immediately following Bohr's articles in Philosophical Magazine. As noted above, atom building, was a lively topic among British physics research but was less of a research priority in the United States, France or Germany in this time period. To what extent did these atomic models build off of Bohr's? What aspects of his model did they incorporate or ignore?

J.J. Thomson proposed his "plum pudding" model in 1904. This model of the atom positioned electrons or "corpuscles" in coplanar rings around the circumference of a "sphere of uniform positive electrification"[27] that neutralized the electrons and kept the atom stable. While this became the model of the atom for which Thomson was best known, he did not stop suggesting models for the atom in 1904. In fact, he produced several models around the same time as Bohr in 1913 and 1914. These models shared with Bohr's a concern for how to configure electrons of the simpler elements in a way that produced a stability while also corresponding to and explaining the known periodicity of the elements. While Bohr and Thomson's models were clearly rivals, Thomson did not cite Bohr's work in 1913 or in any of his work after 1913. The refusal to even acknowledge Bohr's atomic model within his own similar efforts voiced his reception in a way that was clear to his colleagues and to Bohr; Thomson did not approve of the Bohr atom, he did not think it was true or lasting, and he pursued his research without so much as acknowledging it. Bohr took Thomson's lack of support and collaboration silently and pursued his own work without engaging with Thomson, but his mentor, Rutherford, was publicly upset about Thomson continuing to model the atom without bending toward Bohr's new theories. Writing to the radio-chemist Bertram Boltwood at Yale in March of 1914, he reported, "J.J. Thomson knows that I think his atom is only fitted for a museum of scientific curiosities. The idea of a nucleus atom is really working out exceedingly well. You will have seen the work of Bohr and Moseley." And to the secretary of the Royal Society,

[27] Sir J. J. Thomson. "XXIV. On the Structure of the Atom: An Investigation of the Stability and Periods of Oscillation of a Number of Corpuscles Arranged at Equal Intervals around the Circumference of a Circle; with Application of the Results to the Theory of Atomic Structure." *Philosophical Magazine Series 6* 7, no. 39 (March 1, 1904): 237–65.

Arthur Schuster, he wrote, "I believe that he (J.J. Thomson) knows in his hear that his own atom is not worth a damn and will not do the things it has got to do."

Thomson's models differed from Bohr's in two key ways, both of which were important to Thomson and go a long way toward explaining his total refusal to accept or acknowledge Bohr's atomic theory. First, all of his models from the plum pudding model forward were resolutely classical. Thomson believed that classical mechanics and thermodynamics governed the behavior of matter within the atom just as it governed the behavior of matter on larger scales. Second, as Heilbron and Kuhn first noticed, Thomson made no claims that his atomic models were descriptions of reality, and he was in fact aware of their artificiality. For Thomson, these models were merely tools or helpful illustrations that could be useful in predicting or understanding new experimental results. Because he did not view any of his models as providing a description of the real atom, he would sometimes use conflicting models of the atom within the same article or talk, relying on whichever model best illustrated or explained the concept at hand.[28]

Aside from Thomson, the second most prominent atomic model at the time of the Bohr atom belonged to English mathematical physicist John William Nicholson. Nicholson's atom preceded Bohr's by two years, but because it was published mostly in astrophysics journals, it did not become part of the general awareness of physicists until after Bohr's 1913 articles in *Philosophical Magazine*, in which Bohr cited Nicholson extensively. Nicholson's work explored the relationship between stellar spectroscopy and the periodic table of the elements. He proposed a model of the atom similar to Thomson's atom of 1904 but with a much smaller dimension for the positive sphere. Without the larger sphere of positive charge to contain the rotating electrons, Nicholson's atom faced instability. Nicholson was the first to suggest that Planck's constant (h) could quantize the

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[28] See, for example, Thomson's use of models in Sir J. J. Thomson M. A. "XVII. On the Theory of Radiation." *Philosophical Magazine Series 6* 20, no. 115 (July 1, 1910): 238–47.

angular momentum of co-planar electron rings, correcting the atom's instability. Nicholson found the ratio of potential energy of the ring to the frequency of the rings to be an integer multiple of *h*.[29]

Through their atomic models, both Nicholson and Bohr sought to account for periodic properties of the elements and to account for and predict spectral frequencies. Both articulated a role for Planck's constant in limiting the size of the atom. As McCormach describes it, "Each built a characteristic halfway house between the new and the old ideas." But Nicholson's project also differed from Bohr's. Nicholson's primary interest was providing an account of the elements present in nebulae "in order to proceed to a more detailed theory of the evolution of early stars." In the context of his larger project an atomic model that correlated spectral data with elements was a means to an end. He made no claims that his atom represented the real structure of the atom, and as became clear in his critique of Bohr's work (see below), he did not believe that others should claim that their models were real either. Because they were only visualizations, Nicholson's atomic models possessed a certain flexibility ("positive charges surrounded by a ring of electrons in circular or elliptic motion.") For Bohr on the other hand, the purpose of producing a model of the atom was to describe the actual innerworkings of the atom. His "little piece of reality," as described to his brother Harald when he first began working on the atomic structure in 1912.

Nicholson's atomic modeling was motivated and sustained by a commitment to the applicability of ordinary mechanics and electrodynamics within the atom. Bohr made the opposite move and framed his model with the assertion that ordinary mechanics did not apply at the atomic scale. Neither the stability of Bohr's "stationary states" nor the electron's transitions between a pair of stationary states could be explained by classical mechanics. It may be important to remember at

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[29] Nicholson defined the potential energy for the electron ring as mn^2w^2, where *m* was the electron mass, *n* the number of electrons, *a* the radius, and *w* the angular velocity. See John William Nicholson, "The Constitution of the Solar Corona." *Monthly Notices,* no. 72, 679 (1912). For a detailed treatment of Nicholson's theory of the atom and his engagement with Bohr see: McCormmach, Russell. "The Atomic Theory of John William Nicholson." *Archive for History of Exact Sciences* 3, no. 2 (1966): 160–84.

this juncture that Nicholson's use of Planck's constant to define the angular momentum of the electron rings did not signal an embrace of quantum theory or break from classical mechanics. The move to frame the discovery of Planck's constant as the origin of quantum theory and therefore a point of rupture with classical mechanics is one made retrospectively by historians and historical actors once quantum theory was far better established. Planck himself did not view the discovery of his constant as any kind of break with classical mechanics. And in the period in which Nicholson was working whether the use of Planck's constant signified an embrace of the new physics depended greatly on how it was used.

After the publication of Bohr's atomic theory in 1913, Nicholson responded frequently and increasingly critically to Bohr's rival theory. In the years between 1913 and 1915 he devoted fully half of his publication to discussing and critiquing Bohr's theory.[30] Nicholson's responses ranged from priority claims, to highly technical analyses which Bohr would quip in an unsent draft letter to *Nature* involved far "more elaborate mathematical treatment than anything attempted in [his] own papers."[31] Underlying Nicholson's critique of Bohr's model was his commitment to classical electrodynamics and mechanics and a view that atomic models should be useful illustrations and not lay claims to a description of reality. Responding to Bohr's theory in the spring 1914 edition of *Monthly Notices*, Nicholson acknowledged the extraordinary correlation between hydrogen spectra and Planck's constant in Bohr's work, but he maintained that this correlation did not signify anything "real" about the structure of the atom. Where Bohr's use of Planck's constant to define the angular momentum of the electron represented a claim about how the atom really worked requiring the abandonment of classical mechanics in favor of a new physics, Nicholson's use of Planck's constant was merely a

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30 Russell McCormmach, "The Atomic Theory of John William Nicholson," *Archive for History of Exact Sciences* 3, no. 2 (1966), 179.

31 Drafts of answer to Nicholson," Spring, 1914. "Answers to Nicholson II" AHQP, Bohr's Scientific Correspondence, Archive for the History of Quantum Theory, Harvard University.

summary statement of a complicated process, which was not understood, by which the electron was bound in that atom. "At one point in the theory of the simple-ring atoms… Planck's constant enters-- in the relation between different systems by virute of angular momenta; and we may ask-- Is this already an infringement of the ordinary dynamics (classical mechanics), and should not energy, when radiated, be radiated in quanta? This does not appear to be necessary. For the use of electrostatic forces is only a means of mathematically representing the real binding of an electron in the atom, which may be a complicated process." ("The Constitution of Nebulae," *Monthly Notices of the Royal Astronomical Society*, 74, 491 (1914).) Nicholson is willing to leave certain parts of atomic structure unknown in order preserve an adherence to classical mechanics. Bohr drafted but did not mail replies to Nicholson's objections in the form of letters to *Nature* and *Philosophical Magazine*. Though he never tangled with Nicholson publicly or privately, in a letter to his friend H.M. Hansen, he made clear that he understood Nicholson's criticisms to be born out of the fact that Nicholson's project was fundaementally different from his own. "You have probably seen quite a bit of criticism, which has appeared; especially from Nicholson. I do not think it has any foundation. I feel that Nicholson treats the question not as physical, but as a purely literary one."[32] "Purely literary" is a phrase Bohr deployed to describe prose that communicated flexible, subjective experience, not language with strong, clear claims to a description of nature. Bohr was thus acknowledging the space between Nicholson's atom a flexible vehicle that could account for stellar nebulae and his own which hoped, at least in part, to describe the mechanisms at work within the atom.

Nicholson's was the most prominent in a group of atomic models that embodied critiques of Bohr's model by attempting to mirror and build on his structure while eliminating non-classical components. William Conway also produced a classical physics based variant of Bohr's atom, one that eliminated discontinuous electron jumps. William Peddie, who produced what he called a

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32 Bohr to Hansen, May 12, 1915, in Ulrich Hoyer, *Niels Bohr - Collected Works*, 1981,Vol.2, 517-18.

"spherical counterpart to the tubular atom of Sir J.J. Thomson" sums up the general feeling of this tradition of classical atom builders in the wake of Bohr's atomic model: "It does not seem to me that we are yet under compulsion to forsake the laws of ordinary dynamics in connexion with atomic properties, of the doctrine of continuous wave-front in aether, or even, apart from magnetic action, the notion of central symmetry in atomic motion." {Contextual Conway and Petty; provide missing citation}

While these models all harbored subtle critiques of Bohr's atom by incorporating certain aspects of the model while rejecting others, at least one physicist in the United States used his atomic modeling to level a far more explicit critique of Bohr's work. Albert Cushing Creshore, a physicist and inventor, presented a sixty page description of a new model of the atom at the American Physical Society meeting in December of 1916. His model, which extended Thomson's model using a "corpuscular-ring gyroscopic theory" was titled "A criticism of the Rutherford-Bohr atomic hypothesis based upon a theorem of phase-equilibrium of two electron."[33]

Circulation of the Literal Orbit

Whether or not they directly acknowledged being in coversation with Bohr, atomic models produced after 1913 constitute an important site to examine the reception of Bohr's atomic theory. The construction of atoms was a lively tradition within British physics at the time of Bohr's writing, and in the incorporation and rejection of different elements of his model by rival models, we can see vividly some of the factors that governed his reception in more conventional sources (journal article citations, conference proceedings, personal and professional correspondence) as well as some additional forms of critique less prominent in these other sources. As a group, the rival models embodied the critique voiced by so many physicists that a break from classical mechanics at the

[33] American Institute of Physics, American Physical Society, and Cornell University, *The Physical Review - ser.2 v.9 (1917)* (New York etc: Macmillan and Company etc; Lancaster, Paand Ithaca, NYetc: Macmillan and Company etc, 1917), 170.

atomic scale was neither necessary nor acceptable. Strikingly, even as physicists like Conway, Peddie and Cushing Creshore rejected elements such as electron jumps or stable stationary states that violated classical dynamics, they incorporated other aspects of Bohr's atomic structure into their own. Bohr's planetary model, small, compact nucleus, and electron orbits were widespread in these models as was the use of Planck's constant as an expression of angular momentum. Bohr’s writing practices had transformed the electron orbit from the figurative to the real, giving it a durability and longevity that it did not have before his 1913 articles. Even as they dismantled other parts of his atomic model, Bohr’s critics treated his orbit as real.

While J.J. Thomson's 1913 and 1914 atomic models and Nicholson's 1913-14 publications perform their own embrace of classical mechanics and rejection of Bohr's break with classical mechanics at the quantum scale, they also reveal a new vein in the argument against Bohr's atomic modeling project. We encounter in Nicholson and Thomson a narrower definition of what an atomic model should *be* and *do*. Thomson's atomic models were heuristics; they enabled the visualizations or calculations, and he would use conflicting models to illustrate different experiemental results or concepts without worrying about their incommensurability. They were afterall only illustrations. Nicholson operated within this same tradition, comfortable with undefined or mysterious mechanisms at work within the structure of the atom. His models were vehicles for discerning the composition of celestial nebulae; as such, the atomic model needed to correlate spectra with elements consistently, but beyond that he did not concern himself with whether the orbit was circular or eliptical, whether the picture of the atom suggested by the model was real or not. Nicholson was comfortable assuming that there was an unknown, but most definitely classical mechanism that bounded the electron to the nucleus. Thomson and Nicholson's atomic models echo Norman Campbell's published critique of Bohr's, a critique that was an outlier in the general trends of Bohr's published and unpublished written reception. Campbell also advised caution on conflating atomic models with reality. He advised Bohr to set aside any claims to a description of reality and instead pursue a model of atomic structure that was self-consciously a statistical or mathematical approximation.

Bohr's Understanding of his own Reception

Bohr tracked the reception of his atomic theory carefully. He was frequently present at the conferences where his work was discussed; he read (and reviewed) the textbooks where his model was treated; he read even his harshest critics carefully and drafted responses, though he did not always send them. As historians like McCormach have noted, many physicists in this community used the challenge of one another's ideas to sharpen their own theories. But even among a peer group whose work was in constant dialogue, Bohr's attention to how he was read and by whom was exceptional. His systems and practices for tracking readers and their responses were involved and through these systems reception played a foundational role in the writing of new work.

Bohr's response, or lack of response really, to one of his harshest critics provides a telling example of how closely he monitored his reception and how reception became the impetus and groundwork of new writing. Nicholson's public critique of Bohr began in 1913 and culminated in an article in *Philosophical Magazine* in 1914 in which he claimed that Bohr's model which worked so neatly for hydrogen could not be extended to the helium atom, and more damningly that it "[could] not develop in the manner which its earlier success appeared to foreshadow." (Phil Mag, ser. 6, 28, 90-91 (1914).) Though Bohr never published a response to Nicholson, from his correspondence with friends and colleagues and from archived drafts, it is clear that he tracked Nicholson's responses to him carefully and planned to respond, in fact began to respond several times.

As early as 1913 Bohr drafted a public letter to Nicholson to be published in *Nature*. The letter, which was never completed or sent, emphasized the similarities between Nicholson's theories and his own, especially their shared understanding of angular momentum.[34] In April of 1914, after Nicholson's critique had intensified, he wrote to Rutherford to assure him that he was preparing a response to Nicholson and that he was not concerned about his criticisms. A month and a half later her wrote to Hevesy and told him that he was working on a response to Nicholson which would be completed by the start of summer vacation, and that he also planned to complete a paper which would handle Nicholson's objections.[35] Summer of 1914 came and went and Bohr still had not responded to Nicholson. Instead of a letter, Bohr's answer to Nicholson came more than a year later in a full paper published in September of 1915.[36] Though Bohr never responded to Nicholson, he paid careful attention to his criticisms. Through reflecting on this aspect of his reception and a number of false starts in never posted responses, Bohr defined the scope and direction of his next work. His attention to Nicholson's responses eventually became the basis for his next published piece of writing.

Administrative Systems for Tracking Reception

Bohr's understanding of his own reception fed back into his writing practices in tangible, material ways. Through distinctive administrative systems, his tracking of his reception became the basis of new writing. He used copies of publications interleaved with blank pages to keep track of feedback and suggest changes to himself for the next edition or next article. In this way notes on reception became the scribbled basis of new writing. He also kept track of who his readers were and made sure his work reached a rather extensive list of colleagues by sending his own preprints or bound

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[34] Draft of a not posted letter to 'Nature' concerning Nicholson's letter in that journal" Autumn 1913 at the American Philosophical Society, Archive for the History of Quantum Physics.

[35] Letter from Bohr to Hevesy, May 25, 1914, American Philosophical Society, Archive for the History of Quantum Physics.

[36] "On the Quantum Theory of Radiation and the Structure of Atoms" *Philosophical Magazine* ser. 6, 30, 394-415 (1915).

copies, accompanied with personal letters. He kept track of this correspondence in a separate, carefully organized book.

Beginning with his doctoral thesis, “An Electron Theory of Metals” Bohr systematically promoted his theories with what Kragh appropriately calls "great determination." As a part of his efforts to make sure that his theories were received by other physicists and read as he hoped they would be, Bohr kept careful track of feedback he received on his writing. He had two primary paper systems for doing this, both of which he developed with Margrethe Bohr for his doctoral thesis (1910) and continued to use throughout his career. The first system was a blank, lined notebook which contained lists of the people to whom he had sent a copy of the manuscript or pre-print. The notebook used for this purpose for his doctoral thesis was labeled "N. Bohr. A list of names of those to whom the doctoral thesis has been sent" ("N. Bohr. Navnlesiste over dem, til hvem Doktordisputatsen er sendt").[37] Running across the pages in order are the letters of the alphabet with two consecutive pages reserved for each letter. Closer examination reveals that Bohr used one page to list the Danes (organized by last name) to whom he sent his thesis. The facing page contained lists of non-Danes (also organized by last name) to whom he sent his thesis. The names are sometimes in his handwriting, but more frequently written by Margrethe Bohr. Some of the names are crossed off while others are marked with dashes, crosses or dots. I assume that these marks indicate that a copy has been sent. When a response was received, the name is marked "Answer" ("Svar"). All told, Bohr sent copies of his doctoral dissertation to 242 people throughout Europe and the United States, with clusters of recipients falling unsurprisingly in Cambridge, Manchester, Gottingen, Berlin, Munich, Zurich, Breslau, Paris, and Princeton. He used the same notebook to record personal and professional correspondents, with names ranging from his then

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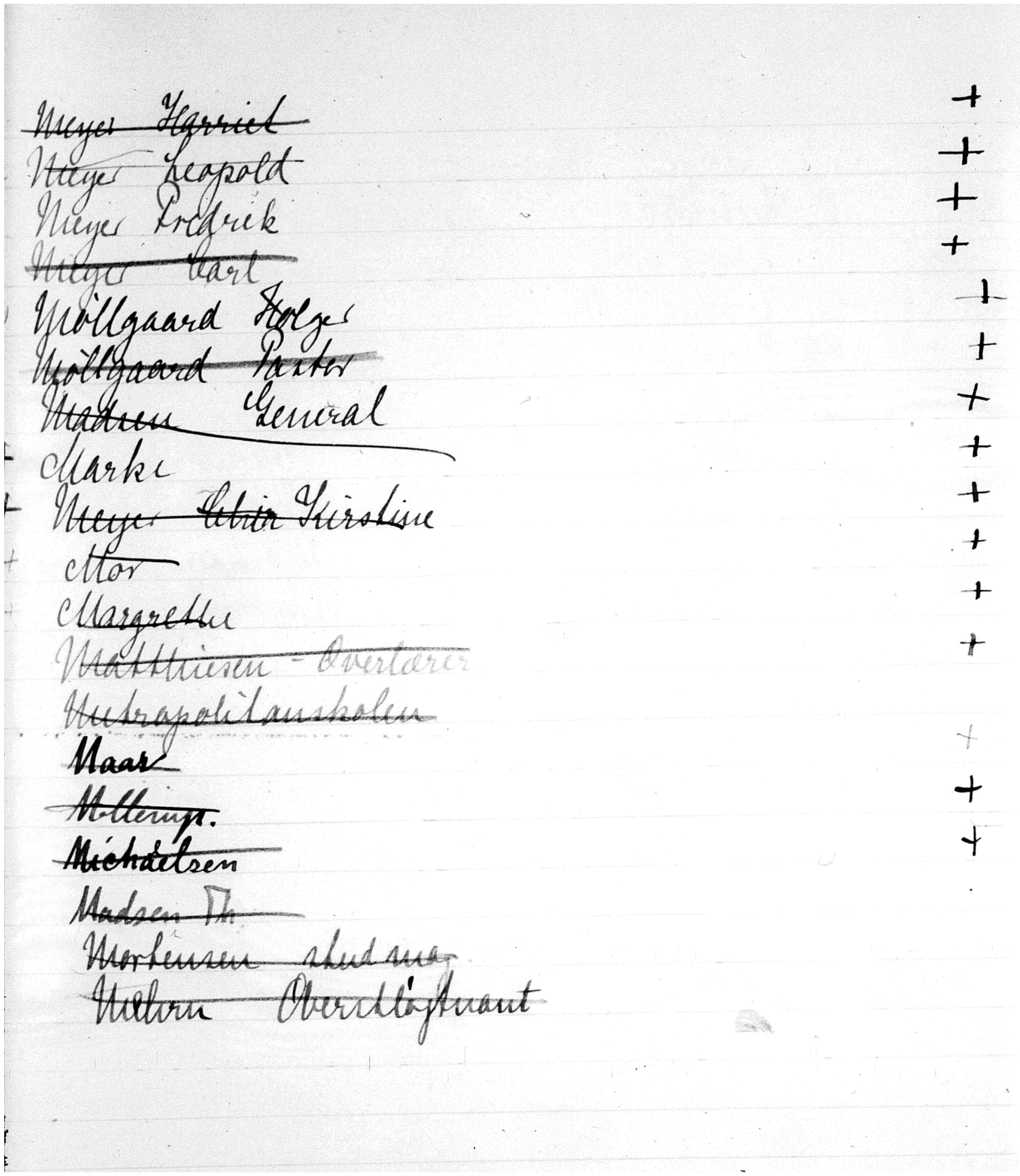
37 Niels Bohr, “Niels, Bohr, "A List of Names to Whom the Doctoral Thesis Has Been Sent 1911, BMSS-2 3” 1911, Film A 603.5 (2), Archive for the History of Quantum Physics.

fiance, Margrethe (listed by her first name only on the "M" page) to his brother (one of a long line of Bohrs listed on the "B" page) to the likes of "A. Einstein, Zurich," "H. Poincare, Paris," and "Lord Baron William Rayleigh" (whose form of address Bohr practiced in his best handwriting on the inside cover of the notebook). Bohr could not have known many of the people he sent his thesis to, and it is not clear to me how he developed this list, but I am hardpressed to find any physicist who published in the previous two years *Annalen der Physic*, *Nature*, or *Philosophical Magazine* who is not listed in this record. Certainly every living person cited in his thesis is on the list.



Meyer Harriet
Meyer Leopold
Meyer Frederik
Meyer Carl
Møllgaard Helge
Møllgaard Peter
Madsen General
Marie
Meyer Kirstine
Mor
Margrethe
Matthiesen - Overlærer
Metropolitanskolen
Maar
Mollerup.
Michaëlsen
Madsen Th.
Mortensen stud. mag.
Meløn Overlieutenant

Figure 9: The Danish "M" page of "A List of Names to Whom the Doctorall Thesis has been Sent"
BMSS 2_3 19110000, AHQP, Harvard University

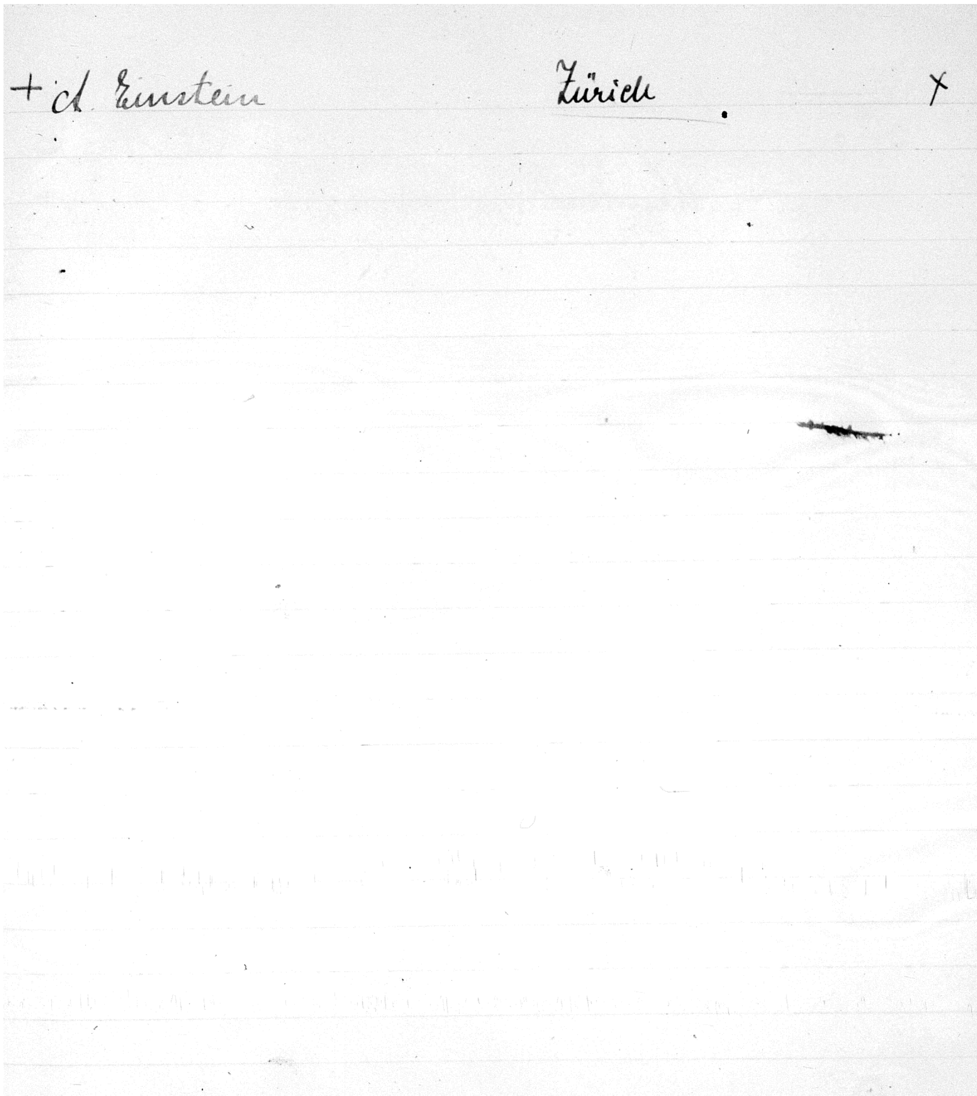
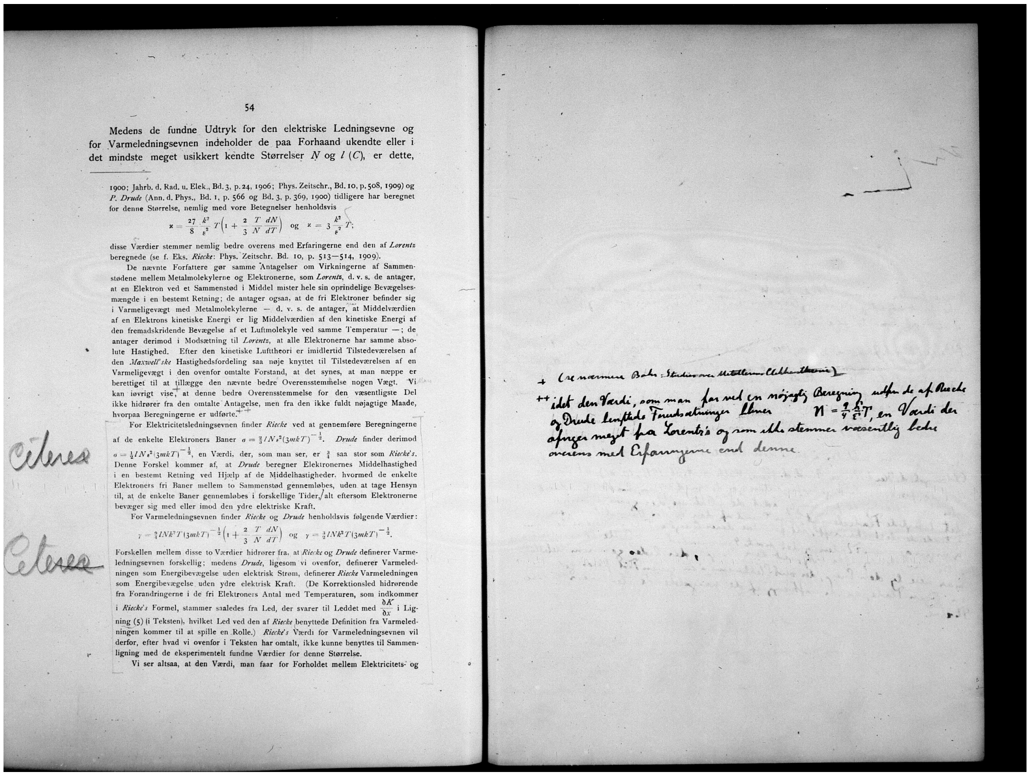

+ A. Einstein Zürich x

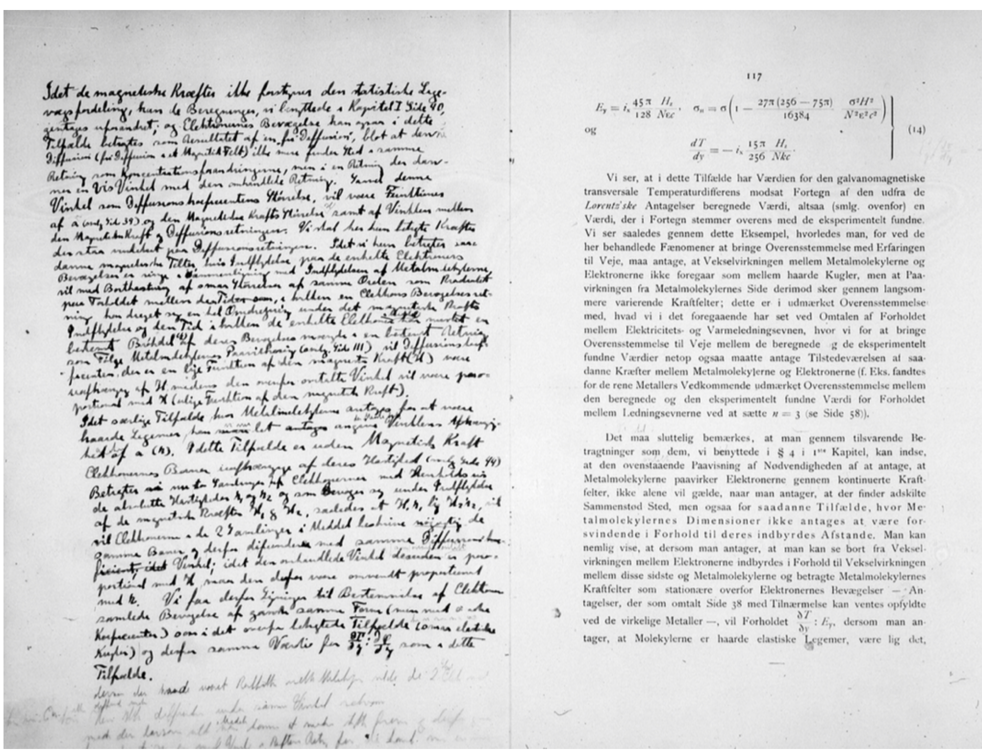
Figure 10: Non-Danish "E" page of A List of Names to Whom the Doctorall Thesis has been Sent
BMSS 2-3 19110000, AHQP, Harvard University

While other scientists certainly shared their work with key figures like J.J. Thomson or Ernest Rutherford who might be able to offer them space as a researchers in a postdoctoral type position (though postdoctoral fellowship do not really develop as an official post in physics until later in the century), the extensiveness of Bohr's list is unusual. This notebook and system are an early indication of a pattern of taking the promotion of his scholarship into his own hands, a trend that would only intensify and become more elaborate as his career progressed and he became the figure most associated with the new quantum theory.

Upon "completion" of any writing project, Bohr had a special bound copy of the book or article produced with blank pages interleaved between every typed page. The practice began with Bohr's doctoral thesis in 1910 and continued throughout his career. The first iteration, this special bound copy of his thesis, "A Study of the Electron Theory of Metals" ("Studier over Metallernes Elektrontheori"), was apparently originally intended to provide space to write notes to prepare the thesis for translation into England. However, Bohr's use of these pages extended well beyond notes for an English translation. He treated the blank pages as extended margins, frequently packing them with a variety of notes for revision, mostly in Danish and mostly in his own hand. While a few pages are left blank or used only to indicate sections of prose to be cut with Danish's efficient term "udgar" meaning "the following shall be ommitted" scrawled across the blank page, most of the pages are crammed full of Bohr's tiny handwriting indicating extensive additions to the text. Bohr also crossed out smaller sections of typewritten prose and to suggest subtle rewording. The unwieldy, "It is, as you know, in the attempt to describe the relationship between a gas' specific heat at constant pressure and volume that molecules of such gases are assumed to consist of one atom" on page fourteen is crossed off, and the more efficient, "Since as you know, once the molecules of one atom gases " is written on the facing page as a suggested substitution. But by far the most frequent use of the blank pages is to list and incorporate new work and citations. Again and again, Bohr writes new citations or instructions to himself to add a citation ("citere"). Citations on new work especially by Lorentz and Max Reinganum ("M. Reinganum") are proposed throughout the thesis. Some of the new citations are of articles suggested to Bohr in correspondence and responses to his thesis, and others he must have encountered in his own reading of journals and abstracts. The blank interleaved pages often contain more than one layer of writing; a page full of penciled notes are mostly obscured by neater and more fully polished writing in pen.

Bohr's request for the blank pages and his use of them, suggest a writing process that does not stop at publication. The completed dissertation or the published article become the basis of a new draft, made porous once again to the feedback of others through the interleaving of blank pages. Reception, the feedback he receives in the wake of publication (or in the case of his own doctoral thesis in the wake of his own distribution of the manuscript), directly feeds back into his writing process. It is almost misleading to talk of reception and Bohr's awareness of his reception filtering back into his writing process. Rather, by paying attention to the practices he set up for keeping track of his reception and incorporating feedback, we see how thoroughly Bohr's writing process blurs the line between production and reception of a text and reveals publication to be just another frame in an ongoing process.





Figures 11 & 12 Examples of interleaved pages from Bohr's doctoral thesis. The blank pages were apparently originally intended to provide space to write notes to prepare the thesis for translation into English;
however, Bohr's use of these pages extended well beyond this original intended use.
"A Study of Electron Theory of Metals (Danish) with many notes" BMSS2-4, AHQP, Harvard University

Reception and the Refinement of an Ideal Reader

On a more abstract level, Bohr's awareness of his reception continuously shaped and refined his notion of an ideal reader for quantum theory, the reader that he believed all editorial and revision decisions should be aimed at convincing. Reception, particularly reception that indicated to him that he had been misunderstood, also contributed to his understanding of reading as a performance that took place between the author and the reader, with the author holding few controls over the reader's understanding and experience of a text.[38]

In 1934 the first edition of *Quantum Theory and the Description of Nature* (ATDN) was published by Cambridge University Press. *ATDN* gathered together edited, English language versions of work by Bohr that had previously been published in article form or delivered as a conference papers and sequenced them in chronological order with a lengthy introduction written by Bohr.[39] The stated goal of the book, as Bohr explained it in his introduction, was to provide a history of the transition from classical to quantum theory and an accessible explanation of a way forward that would reconcile the contradictions between these two theories. *ATDN* can also be read as an attempt by

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[38] Keeping an ideal reader in mind through all stages of the writing process from dictation to publication motivated Bohr's writing and style (see Ch. 2). From one direction, this notion of an ideal reader was informed by Margrethe, the real reader he had before him throughout the process. On the other side of the process feedback on the reception of his ideas also shaped his definition of an ideal reader and his concept of what it would take in terms of word choice, language, style to reach such a reader.

[39] Many parts of the book had been published previous to the 1934 English edition. Chapter I, "Atomic Theory and Mechanics," and Chapter II, "The Quantum Postulate and the Recent Development of Atomic Theory," were both originally published in English in the journal *Nature*. "The Quantum of Action and the Description of Nature" was published in German in *Naturwissenchaften* (Natural Sciences), and "The Atomic Theory, and the Fundamental Principles underlying the Description of Nature" was published in Danish in *Fysik Tudskrift* (Physics Magazine). Tracing the chapters of the book back to their corresponding published articles gives only a partial sense of the readers that may have encountered this material before the 1934 publication of ATDN. Versions of three of the four articles were originally given as talks and the fourth was published as a contribution to a "jubilee pamphlet" celebrating the physicist Max Planck. The texts changed language several times before 1934. The first chapter, for example, was published as an article in English in 1925, but was given as a talk in Danish at a Scandanavian conference earlier that year. A version of ATDN that included three of the four essays was published in Danish in the Year Book of Copenhagen University in 1929 and in German in 1931 by Springer. By the time Cambridge University Press published the 1934 English edition of ATDN most portions of the text had moved through three different languages and had been read and heard by diverse professional audiences and readers.

Bohr to re-write his own reception, to reach readers he had missed and make Quantum Theory competitive with Einstein's relativity as a cultural force.

Beyond making visible Bohr's continued concerns about the reception of quantum theory, *ATDN* reveals how Bohr's understanding of the tradeoffs involved in observing and description the atom co-evolved with his understanding of the work and tradeoffs involved in the communication of science through writing, reading, and speaking. What becomes visible in *ATDN* are the co-influential lines between Bohr's concepts of reader and observer, and the way that both acts of reading and acts of observation are understood by Bohr to involve knowing very little about an underlying, abstract idea while constructing meaning by interpreting the interaction between tools and unobservable phenomena: All one can know of the atom is the productive interference between the atom and the tools used to probe and describe it. All one can know about the meaning of a text is the productive interference between a reader and the text.

The material qualities of the 1934 edition and subsequent English editions embody Bohr's idea of *ATDN* as a history book accessible to lay readers. The layout of the pages is strikingly simple for a scientific text. Aside from chapter headings, the font is consistent throughout. The articles that have been republished as chapters have no footnotes or endnotes. In fact, they do not resemble scientific articles at all. The visual simplicity of the page layout works together with the signature marks and the elaborate binding to emphasize the identity of this text as a real book, not a scientific journal. The bookishness of this book becomes a device to extend and support the idea that *ATDN* is meant for common readers not just physicists.

While the physical form and comments in the general introduction give a general sense of who *Atomic Theory and the Description of Nature* was intended to reach, Bohr's most specific ideas about his reader concern not who his reader should be, but *how* his reader should read. *ATDN* makes clear that Bohr's understanding of reading ran parallel to his understanding of observation of the quantum state. His particular understanding of observation arose out of the predicament of observing atomic phenomena. Before atomic theory, physicists had tended to think of observation as a one-way process. Observations captured and measured objects in the real world, and observations could be treated as standing in for the thing that had been observed. When we measure the height of a glass of water, for example, we do not need to consider the whether the ruler impacted the measurement. When Bohr and Werner Heisenberg attempted to design thought experiments to observe the atom, they discovered that the very tools that they would use to observe the atom would change the atom. Because of the interference of the experimental apparatus, either the position or the momentum of an electron could be measured, but not both at once.[40] An observation at the atomic scale is necessarily made up of the object that one observes and the effect of the tools used to accomplish the observation. From Bohr's point of view, the observation therefore had to be understood to encompass the observer and his tools. The isolated atom is in fact just an abstraction, all we can know about the atom is the interference between the tools of observation and the atom.

Corrections could be made for the observer and the instruments of observation on the macroscopic level, but at the atomic level, the discussion of the object could not be separated from the discussion of the observational instrument. Any model of the atom had to be constructed from experiments in which the atom was necessarily altered. Or, as Bohr explained in the introduction to *ATDN*:

> [T]he magnitude of the disturbance caused by a measurement is always unknown, since the limitation in question applies to any use of mechanical concepts and, hence, applies to the

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[40] This limitation is expressed formally by Heisenberg in his Uncertainty Principle of 1927 which proposed a thought experiment in which the position of an electron would be measured using a gamma ray microscope. Heisenberg calculated that the experimenter would not be able to know the position and momentum of the electron simultaneously. A trade-off was required to obtain either measurement because of the tool for measurement altered the object being observed. A similar trade-off is articulated by Bohr in his concept of complementarity, also first articulated in 1927.

> agencies of observation as well as to the phenomena under investigation. This very circumstance carries with it the fact that any observation takes place at the cost of the connection between the past and the future course of phenomena. As already mentioned, *the finite magnitude of the quantum of action prevents altogether a sharp distinction being made between a phenomenon and the agency by which it is observed.*[41]

As Bohr pointed out in *ATDN*, both quantum theory and relativity included the observer as a part of the observation while classical theory did not. "The recognition of the relative character of the phenomena of motion, these being dependent upon the observer, already had played an essential part in the development of classical mechanics, where it served as an effective aid in the formulation of general mechanical laws."[42]

For Bohr, re-centering the role of the observer had implications that extended beyond physics. "The impossibility of distinguishing in our customary way between physical phenomena and their observation places us, indeed, in a position quite similar to that which is so familiar in psychology where we are continually reminded of the difficulty of distinguishing between subject and object."[43] He reminded his readers that although they were accustomed to thinking of physics as a realm of objective observation, "occasionally just this 'objectivity' of physical observations becomes particularly suited to emphasize the subjective character of all experience."(Bohr 1) The subjectivity within the observation in quantum physics highlighted for Bohr similar forms of subjectivity in other experiences, especially written communication, reading and writing.

Bohr understood the act of reading as being connected to the predicaments of observation. He repeatedly noted the impossibility of a reader directly accessing a text; there would always the interference of the meanings that the reader brought with them. The reader has agency in making

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41 Niels Bohr, *Atomic Theory and the Description of Nature*, 1934, 11. Emphasis is my own.

42 Ibid., 2.

43 Ibid., 1.

the meaning of the text just as the observer and the observational apparatus had agency in an observation. Reading like observation of quantum phenomena, was an activity that blurred the line between subject from the object or the reader and the text.

In *ATDN* Bohr acknowledged that reading about quantum theory would challenge his readers in many of the same ways that observing the atom challenged quantum physicists. He claimed that the obstacle to both tasks was rooted in the way people understood new experiences through their "customary points of view and forms of perception." Both readers and physicists were constrained by their previous ways of understanding, so they had difficulty understanding theories and results that broke old rules or could be translated into terms with which they were familiar. And because the human actor (reader or observer) played a role in determining the meaning of the text or the outcome of the observation, the "customary point of view" that constrained them also inevitably constrained the meaning of the text or the interpretation of the experiment.

Conclusion

Writing filled the daily lives of quantum theorists. But too often scholars have treated this writing as something that came after the work of science was already complete. This dissertation takes writing seriously as a central practice of the modern physical sciences. Across the chapters, I examined writing and drawing as tools not just for *representing* science, but also for *doing* science. All the preceding chapters drew our attention to the messy intersection of theory and practice in the production of the texts that make up quantum theory. What is clear here is that the causal arrows go in many directions at once, leaving us grasping for exactly the right verb to describe both how writing practices produce quantum theory and theories of communication and also how theories of writing lead Bohr to embrace particular writing practices.

Bohr's theories of how communication works structure his writing practices; his desire for cross- and extra-disciplinary intelligibility reinforce the utility of dictation to a non-scientist amanuensis. At the same time, Bohr's life-long dictation practices clearly formed the foundation for his understanding of what effective communication meant, who the ideal reader for science was, and how they ought to be reached. Throughout this nascent period for quantum theory, theories of writing when they are put into practice act in ways that are unpredictable and generative for quantum theory. This dissertation has argued that it is when Bohr's ideals for communication come into contact with his material and administrative practices of dictation, revision, publication, and reading that quantum theory is created. It is impossible to understand these theories disconnected from the labor that produced them.

Across all four chapters, writing and reading practices help us understand and interpret the content and tone of published work. At the same time, practice helps us to de-emphasize the published work and understand it to be one of many frames in Bohr's continuous writing process. Each chapter examined a different way in which writing practices construct theory, shaping tone, content, themes, and language in ways that are both intended and unintended by the author. Attending to these practices also made visible the different historical actors who were involved in the work of writing. Amanuenses, secretaries, administrators, colleagues, neighboring artists and children all played roles in Bohr's dialogic and collaborative writing process. Examining writing practices also brought into view the different places, including the Bohr home, the vacation cottage, a neighbor's painting studio, in which quantum theory was written. Within these more domestic sites we began to see how the institution of marriage functioned as a support and space for disciplining ideas about what counted as scientific knowledge.

While revision reminds us of the materiality of these practices by bringing into view the piles of paper, the crossing out, the systems of filing, sorting, binding, and the trips Bohr took to Fleet Street to see how long the physical production of a text would take and whether further changes could be made. Dictation reminds us of the corporeal dimension of writing, that writing is an embodied practice, the outcome shaped by the practices through which it was produced. The history of theoretical physics often treats its actors as if they were disembodied minds. Following early quantum theory's writing practices intervenes in the image of physicist as long, disembodied genius, and reminded us of the material, embodied nature of this work. As a whole, this dissertation invites us to think more about collaborative writing practices in the sciences, the role of administrative and transcription labor, the traces of these practices in the published papers.

After the period covered by this dissertation, Bohr's atomic orbit persisted (and continues to persist) in our understanding and visualization of the structure of the atom. This suggests long-term parallels between the ways in which Bohr wrote and the way that he was and continues to be read. Bohr's writing practices nursed the durability of a constructed object. The atomic orbit, originally a heuristic became real in Bohr's writing as an artifact of the repetition in his revision practice; he repeated it in so many drafts even as he purged figurative language, that it survived to publication as a real, literal orbit instead of the heuristic it began as. And the orbit remains real, a literal object and not a mere heuristic in the way that he is read. This invites us to think about how writing practices might figure in the durability of constructed objects in the sciences more broadly. Could writing and reading practices help us understand how other objects, initially intended to operate as heuristics, objects like Planck's constant for example, end up being read as real, physical objects?

Bohr's dictation practices provide the clearest example of how writing practices can leave traces in the content of a finished work as themes. Niels and Margarethe Bohr's particular practice of dictation/dialogue forms a sustained thematic thread through Bohr's work in which dialogue between an expert and non-expert, between writer and reader, is privileged even in the published texts. Bohr's practice of dictation informed both his ideals for the communication of quantum theory and his definition of who his ideal and intended readers should be. Margrethe, who most often served as his amanuensis and editor in the early years of his career, became the model for the ideal reader, and what counted as effective communication with her became the standard for effective communication more broadly. In his final published texts, Bohr speaks to his reader in very much the same way he spoke to Margrethe in dictation and revisions sessions throughout. He invites the reader, however reluctantly, to participate in creating the meaning of the text by combining his or her own sense impressions with the language on the page. The ideal reader imagined and referenced in the text is meant to question the text as they read, to interpret and create its meaning, as the active amanuensis Margrethe did.

Finally, understanding the reception of Bohr's ideas about quantum theory and the ways in which he himself tracked his own reception blurs the line we might imagine to exist between published and unpublished work. Bohr's use of interleaved pages to record and incorporate feedback on published work suggests a writing process that does not stop at publication. Instead of becoming a fixed text, the completed work immediately becomes basis of a new draft, made porous once again to the feedback of others. By paying attention to the paper systems he set up for keeping track of his reception and incorporating feedback, we see how thoroughly Bohr's writing process reveals publication to be just another frame in an ongoing process.

We can also see clearly in Bohr's interpretation of the reception of quantum theory and in his efforts to re-write his own reception through edited volumes that re-introduce revised ideas how concepts about the observing the atom co-evolved with concepts about communication, writing, and reading. The conventional take on Bohr's idea of complementarity is that it emerged from the challenge to observe a purely physical phenomenon and was later extended to non-physics domains like philosophy and psychology. Through examining his life-long writing practices, we see instead that a complementary understanding of communication precedes Bohr's articulation of complementarity's applications to atomic theory. Long before he understood that the position and momentum of the electron were in a complementary relationship, Bohr understood clarity and truth to be in a complementary relationship in the generation and communication of scientific ideas. He constructed everything about his writing practices from his choice of amanuenses to his use of plain language vocabulary in final drafts to navigate these incommensurable but vital goals. The early centrality of these ideas about scientific communication suggest that Bohr's theory of complementarity emerges first as a way of understanding his own writing and is then later applied to the description of the structure of the atom not the other way around. Seen this way, Bohr's writing practices and philosophy of communication emerge as an origin point for-- rather than an after thought to-- one of his most celebrated contributions to quantum theory.

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