A Reexamination of Early Debates on the Interpretation of Quantum Theory: Louis de Broglie to David Bohm

by

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Submitted in Partial Fulfillment

of the

Requirements for the Degree

Doctor of Philosophy

Supervised by

Professor Theodore M. Brown

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2011

Curriculum Vitae

The author was born in Bethesda, Maryland on December 10, 1974. He attended the University of Rochester from 1992 – 1998, expanding the typical four year undergraduate curriculum with two supplemental programs of study. He spent the 1994-1995 academic year in Madrid, Spain working at Iberdrola, S.A. as an intern under the supervision of physicist, and Director of Innovation, Dr. José A. Tagle where he researched the relative merits of safety systems in nuclear fission reactors and the proposed systems for experimental fusion reactors (ITER) and then became a Take Five Scholar at the University of Rochester for the 1997-1998 academic year studying how art and music relate to religious experience. The author completed his double degree in 1998 earning both Bachelor of Arts and Bachelor of Science degrees, with concentrations in religion and physics respectively. He stayed at the University of Rochester and in the fall of 1998 began graduate studies in physics. He pursued his research in cold molecule collisions and spectroscopy under the direction of Professor Nicholas P. Bigelow and received the Master of Arts degree from the University of Rochester in 2001. In 2003 he transferred into a joint doctoral degree program in physics and history. He has pursued his research in the history of 20th century physics under the direction of Professors J.H. Eberly from the Department of Physics and Astronomy and Theodore M. Brown from the Department of History. Living in New York City since 2006, the author has been working to complete the requirements for his degree while working at New York University's Gallatin School, where he will begin a full-time visiting faculty appointment in September, 2011.

Presentations

- "Climbing Mt. Impossible: Reexamining David Bohm's Hidden Variables," *NYC History of Science Working Group*, New York University, May 2009, New York, NY.

- "Louis de Broglie: Prince of Waves," *Harvard-MIT-Princeton Phunday*, Harvard University, April 2011, Cambridge, MA.

Awards

- University of Rochester: Edward Peck Curtis Award for excellence in teaching, May 2006.

- University of Rochester: Department of Physics and Astronomy Excellence in Graduate Teaching Award, May 2000.

- University of Rochester: Department of Physics and Astronomy Excellence in Undergraduate Teaching Award, May 1998.

- University of Rochester: Alice DeSimone Student Life Award, 1998.

Acknowledgments

One of the drawbacks of walking such a long road in order to complete my doctoral dissertation is that the acknowledgements section inevitably becomes dauntingly difficult to complete. While an exhaustive list would be far too cumbersome to write, I will do my best to recognize those who have been critical throughout this long and winding process. It was because of Professor J.H. Eberly's quantum mechanics for freshman course in the spring of 1993 that I first felt drawn to the combination of quantum physics and history. Dr. Eberly's lectures that spring were memorable as he successfully made quantum mechanics exciting and intelligible. That semester we read, among other books, John Gribbin's *In Search of Schrödinger's Cat*.[1] As a result, I found myself becoming equally engrossed in the physics and the historical narrative of the quantum revolution. However, while I was continuously fascinated by the history of science, I had absolutely no intention of becoming an academic. It has been a wonderful trick of fate that Professor Eberly has become one of my advisors on this dissertation. His many revisions of the evolving chapters in flux were invaluable. In addition, my correspondence with him on the finer points of determinism was enormously enriching.

My doctoral journey really began in the basement of the Department of Physics and Astronomy in Bausch & Lomb Hall in the spring of 1998. I was working as a

[1] John Gribbin, *In Search of Schrödinger's Cat*, (New York: Bantam Books, 1984).

Teaching Assistant for Professor Nicholas P. Bigelow's Advanced Lab course when he pulled me aside and encouraged me to pursue graduate studies in physics. It's safe to say that without Professor Bigelow's early and sustained encouragement, none of this would have been remotely possible. He stood by me throughout the entire process, believed in me, and kept me imbued with a positive attitude. Both his deep faith in people and his unwavering optimism are characteristics that I strive to emulate.

Being a member of Professor Bigelow's Cooling and Trapping Group (CAT) was a wonderfully challenging experience. He created a collegial atmosphere amongst his graduate students and I am indebted to him and the whole CAT group for having introduced me to the finer points of experimental physics. In particular, I should thank Leslie Baksmaty for all his support over the years and his true friendship and brotherhood. In addition, I should note that amongst my fellow lab-mates Ben Weiss, York Young, Mishkatul Bhattacharya, and Chris Haimberger were especially influential. They taught me many invaluable lessons both in and out of the lab, as they inspired me with their extraordinary patience and dedication to the CAT group's collective work.

In 2003, as I was struggling with my chosen vocation as an experimental physicist, I met with Professor Theodore M. Brown, who literally changed the trajectory of my life. Spurred on initially by the interest of Drew Abrams, another physics graduate student, Professor Brown worked to develop a program of study that could lead to a joint interdisciplinary doctorate in history and physics. I was taken with this unique opportunity that would allow me to find true resonance in a superposition of the two disciplines about which I was most passionate. Thanks to Professor Brown's persistence in spearheading this proposal, his finely attuned diplomatic negotiating skills, and his diligence in planning the actual program of study, the University of Rochester accepted the joint interdisciplinary program. The completion of this dissertation is a direct reflection of Professor Brown's tireless will to make this a viable program of study, not to mention his unequalled excellence as a primary editor. Over the years he has become a wonderful mentor, generous advocate, and most importantly a friend.

I would be remiss if I failed to acknowledge the splendid instruction I received while taking graduate seminars in the Department of History between 2003 and 2006. Professor Joan Rubin's seminars were particularly transformative in developing my analytical skills as a historian. However, the most influential aspect of this period was my oral examination defense. I must thank Professor Brown and Professor Celia Applegate for helping me to arduously prepare the defense. Special thanks must also be directed to Dorinda Outram, who showed tremendous resolve and integrity in holding my feet to the fire. There is no doubt that struggling to pass both the Physics Preliminary and History Oral Examinations were momentous and defining hurdles in my training. This would have been impossible without the skilled instruction and pedagogical support I received from both departments.

Another special thank you needs to be directed to the entire University of Rochester community. I arrived in Rochester in 1992 at the tender age of 17 and have been associated with the River Campus in some capacity ever since. In my long tenure

at the University, I have experienced higher education from many different perspectives: as both an undergraduate and graduate student; as a staff member working as a Counselor in the Office of Minority Student Affairs; as a Graduate Head Resident with Residential Life; as an Instructor with the Physics Department and the College Writing Program; as the President of the Graduate Organizing Group; and as a student representative on a seemingly endless number of administrative, curriculum and search committees.

In my experience, the University of Rochester community, as a whole, has consistently embodied many of the best qualities that higher education should strive for. As such, over the years, the University has allowed me the freedom to pursue simultaneous undergraduate studies in physics and religion, carry out undergraduate research in Spain, spend three summers on archaeological digs in Israel, and become a Take Five Scholar. As a graduate student the University of Rochester gave me the opportunity to further my interdisciplinary interests by working towards a joint doctorate in physics and history, while also allowing me to sharpen my skills as a university instructor in physics and interdisciplinary writing. Most recently the Department of Anthropology has given me the opportunity to assist Joe Lanning in leading the annual Summer Malawi Immersion Seminar program.

In whatever capacity I have been involved with the University my experiences seem to always be characterized by a feeling that I am a part of a special community dedicated to excellence, openness, and Meliora. This characterization is especially true

for the amazing support staff throughout the University community and particularly in the Departments of History and Physics and Astronomy. Helen Hull in the Department of History embraced this logistically challenged hybrid doctoral program of study from day one and has been a champion throughout; thank you Helen. As for Barbara Warren, in the Department of Physics and Astronomy, my words of thanks can only fall short; she has been a rock throughout my seemingly interminable tenure as a graduate student and kept me on task when things looked bleakest. She is a master of making nearly impossible jobs look easy. Thanks Barb!

In addition, these acknowledgements would be completely inadequate without mentioning a few other individuals. Dr. José Antonio Tagle was one of my father's closest friends and has been a mentor to me throughout the years. In the year I worked for him at Iberdrola, S.A. I learned more than just physics. Dr. Tagle taught me that innovation sans compassion and ethics is a dangerous proposition. In addition, he impressed upon me the undeniable truth, that the beacon in the storm of a seemingly endless avalanche of pedagogical styles is a deliberate focus on formation not information. While studying abroad in Israel in the summer of 1996, William Scott Green stepped up as a grand mentor and father figure during a particularly hard period of my life. In many ways, his strength, generosity, and guidance have sustained me for the past fifteen years. Bill opened up his wonderful family and home to me and has been instrumental in helping me blaze my academic trail ever since. Matt Stanley has been a true friend, mentor, and sensei over the years and from the moment I told him I was choosing this interdisciplinary path, he has made my life in New York City manageable and been my biggest champion; as a result, I have found a new academic home at NYU's Gallatin School which has provided me with invaluable support throughout the past two years.

The following people need to be acknowledged due to their unwavering support and as representatives of a much longer list of friends and family that form the nucleus of an unfathomably strong personal network: Ben Ross, Joseph Yin, Jorge Rodriguez, Jorge Figueroa, Leslie Baksmaty, and the entire Thomas family - Ellen, John, Dan, and Joanna. This network has been a limitless fountain of love and given me a perpetual sense of belonging. Thank you all!

There is no doubt that I am most indebted to my amazing parents Luis and Dolores Perillán who always taught me, through their deeds more than their words, what it means to be a good person. They instilled in me the beauty of ceaseless curiosity, the need to be persistent yet adaptable, the choice to recognize the power of faith, and the importance of listening to and trusting one's instincts. Since I lost my father to cancer in 1996, I have relied on his ever-present spirit, the transcendent strength and stability of my mother, and a network of loved ones, confidants, and mentors that continuously give me strength and guide me. In particular, I would like to thank my wonderful siblings Pablo, Lucia, and Julio. I have looked up to the three of them my whole life and in recent years I have relied on them increasingly to remind me of our father, each in their own powerfully endearing ways. Thanks to my mother's strength and unwavering faith, I have never once felt completely alone. She is truly an amazing and transcendent spirit. I know that wherever I am and whatever I'm doing my family's love is unbounded and unconditional. Gracias familia!

My wife, Rebecca Thomas, requires her own section because without her none of this would be worth it, or even possible. Since we met, in Professor William Scott Green's "Theories of Religion" class at the University of Rochester in 1996, she has been the love of my life, the beat of my heart, the breath on my lips, and now the bearer of our future. Words cannot express what she means to me.

Abstract

The following dissertation reexamines the emergence and development of alternate formulations and interpretations of quantum theory from the 1920s to the early 1950s. Specifically, Louis de Broglie's alternate wave mechanical interpretation (1923 - 1927) and David Bohm's hidden variables program (1951 - 1952) are examined within their respective contexts of innovation. By presenting a more balanced and nuanced narrative of these 20th century physicists, and their work, subtleties within the scientific exchanges between those engaged in early quantum interpretational debates begin to emerge which can serve to dispel some of the myths that have persisted in the overall historical quantum narrative. While de Broglie and Bohm were ultimately interested in restoring determinism to quantum theory, their particular influences, arguments, methodologies, approaches, and receptions were unique to their respective historical periods and each physicist's position within the physics community. Combining a close reading of the two distinct programs of scientific innovation with a representative analysis of the historical context in which they worked allows us to reexamine de Broglie and Bohm in a new light. The following analysis serves to restore these two physicists' agency in carving out their particular places within the physics community and dispels simplistic myths of marginalization. It also sheds light on de Broglie and Bohm's important contributions to the early quantum mechanical interpretation debates, thereby restoring their importance within the larger historical quantum narrative. Some of the topics that will be addressed include the following: who they trained with; where they trained; what their primary influences were; what their motivations were for proposing their particular alternate interpretations; what their particular research programs consisted of; how these evolved; and how each was received within the particular physics community. In order to successfully reconstruct these quantum histories, various methods of analysis have been used, including close readings of published and unpublished scientific papers, personal and professional correspondence, and a meta-data analysis of aggregate biographical data that reveals new insights into the academic networks within their wider socio-political environments.

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Introduction:

[Between] diverging details on the one hand and … undue reductionism on the other, quantum history is in need of new perspectives.[2]

Table 1.1: Timeline For A Typical Traditional Quantum Narrative[3] -

1. ***1894: Albert Michelson*** summarized the state of physics at the end of the 19th century: "it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice".[4]

2. ***1900: Max Planck*** fixed the ultraviolet catastrophe with a mathematical "trick" that necessitated a quantization of the electromagnetic (EM) field and the introduction of a constant (h = Planck's constant). Planck did not believe that his mathematical trick had any real physical implications.

3. ***1905: Albert Einstein,*** in one of his five seminal papers written during his *Annus Mirabilis,* explained a long standing problem in physics (the photoelectric effect) using Planck's electromagnetic quantization scheme and a corpuscular theory of light in which he proposed the relationship $E = hv$. In 1905, the physics community subscribed strictly to the wave interpretation of EM radiation consistent with Maxwell's EM theory. Einstein in explaining this long standing conundrum proposed a dualistic representation of EM radiation, later known as wave-particle duality.

4. ***1911: Ernest Rutherford*** designed an alpha ray scattering experiment in which he showed atomic structure as a massive positively charged nucleus surrounded by negatively charged electrons of little mass. This planetary model served to supersede J.J. Thomson's "plum pudding" model of the atom.

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[2] Michael Eckert, "The emergence of Quantum Schools: Munich, Göttingen and Copenhagen as new centers of atomic theory". *Ann. Phys.* 10 (2001): 152.
[3] This Traditional Quantum Narrative is typical of what one finds in popular discussions on the quantum revolution and even within academic monographs written by physicists, philosophers, and some historians. Unfortunately, if this narrative accurately represents your knowledge of the quantum revolution, your history is probably based on a mythological picture of 20th century scientific innovation. One of the goals of the following discussion is to address these mythological distortions.
[4] Lawrence Badash, "The Completeness of Nineteenth-Century Science," *Isis,* Vol. 63, No. 1 (Mar., 1972): 52.

5. ***1913: Niels Bohr***, working in Rutherford's lab, extended the new orbital atomic model by allowing the "orbiting" electrons to reside only in specific energy states that correspond to specific quantized radii from the nucleus. Using his new atomic model and Einstein's quantum formulation ($E = h\nu$), Bohr was able to identify the source of previously unexplained EM wave spectra, associating them directly with atomic electronic jumps between discrete energy levels.

6. ***1913 – 1923:*** There was a period of stagnation in part due to World War I and in part due to the failures of Bohr's atomic model. This failed model, which came to be known as the "old quantum theory," was based too heavily on classical concepts (e.g. Bohr's Correspondence Principle).

7. **1923: *Arthur Compton*** performed his famous scattering experiments that verified Einstein's 1905 corpuscular quantum theory and his relationship: $E = h\nu$.

8. **1924: *Louis de Broglie,*** in his doctoral dissertation, extended Einstein's concepts of the dual nature of light to matter, becoming the first to propose matter waves associated with electrons.

9. **1925: *Werner Heisenberg*** and other younger physicists were not satisfied with Bohr's atomic model, so they devised a 'new quantum theory'. Heisenberg was the primary innovator as he created a new quantum mechanics based on the mathematics of matrices. Although it was mathematically abstract, it was more successful at explaining empirical results than the old quantum theory. As a result, the new quantum theorists sacrificed electron orbits and any notion of a well defined space-time picture at the atomic level, accepting that this realm was based on non-intuitive physical laws that were ultimately acausal and discontinuous in nature.

10. **1926: *Erwin Schrodinger*** could not accept the discontinuity and acausality that stemmed from Heisenberg's matrix formulation of the new quantum theory. He proposed an alternate formulation based on wave mechanics and independently derived, from a wave equation, the same quantum equations of motion. Schrodinger showed that both formulations were actually equivalent and that they produced accurate models of atomic phenomena.

11. **1927:** At the Fifth Solvay Council Bohr accepted both formulations (Heisenberg's matrix mechanics and Schrodinger's wave mechanics) as complementary pictures of atomic phenomena. Like wave-particle duality these pictures could never be represented simultaneously and it was ultimately up to the observer (or system of measurement) to decide which one they would employ. 'Complementarity' eventually became one of the cornerstones of the Copenhagen Interpretation of quantum theory along with the Heisenberg uncertainty principle and the probabilistic nature of all atomic phenomena. This interpretation was

championed by Bohr, Max Born, and younger physicists like Heisenberg, Wolfgang Pauli, and P.A.M. Dirac. They stood against conservative physicists who refused to relinquish determinism, represented by the likes of Einstein, Schrodinger, Planck, and de Broglie.

12. ***1928 – 1935:*** Clear battle lines had been drawn at Solvay (1927) between both camps but, because of the success of the new quantum theory in explaining an ever increasing amount of empirical data, the old guard began to erode. Eventually, Einstein remained as one of the few physicists unwilling to accept the Copenhagen Interpretation. His debates with Bohr became legendary as he continuously challenged the orthodox interpretation with thought (Gedanken) experiments that seemingly posed paradoxes. The most famous of these challenges was put forth in his EPR paper of 1935. Nevertheless, Bohr was always seemingly able to effectively parry Einstein's challenges.

Those that recognize the timeline in Table 1.1 as an accurate representation of the early developments of quantum physics from 1900 to 1935, probably have a somewhat distorted sense of what really happened during this critical epoch in the history of modern scientific development. Unfortunately, this group includes many practicing physicists and the vast majority of interested laypeople. Whether you learned the story of the quantum revolution from historically minded physics teachers or popular books on modern physics, you were most likely exposed to a narrative that is based on an inherited mythology. This is not to say that these quantum histories are completely false. The broad brushstrokes painted by many of these mythologies are based, in part, on factual historical evidence; nevertheless, they fail to represent a complete and accurate representation of what actually happened historically.[5]

[5] In George Gamow's 1966 classic historical account of the quantum revolution *Thirty Years that Shook Physics* (New York: Dover, 1985) we see a clear example of an early telling of this quantum mythology. Almost thirty five years later, Ian Duck and E.C.G. Sudarshan, in *100 Years of Planck's Quantum,* (Hackensack, NJ: World Scientific Publishing Co., Inc., 2000) base their historical account primarily on the traditional popular quantum narrative weaving their tale while failing to incorporate the vast amounts of corrective historical evidence. For a discussion on Quantum Mythologies see: Mara Beller, *Quantum Dialogue: The Making of a Revolution.* (Chicago, IL: University of Chicago Press, 2001) and Don Howard,

Early on in the development of quantum physics, the distorted traditional narratives were developed and used primarily within the physics community itself as a pedagogical tool with which to reinforce the generally accepted, orthodox interpretation of quantum theory. As John Heilbron appropriately points out in his discussion of the Bohr atom:

> Bohr used to introduce his attempts to explain clearly the principles of the quantum theory of the atom with an historical sketch, beginning invariably with the nuclear model proposed by Rutherford. That was sound pedagogy but bad history.[6]

Table 1.1 illustrates a traditional popular quantum narrative in which we tend to focus on several key "revolutionary" papers or experiments that seemingly come out of nowhere and suddenly transform the entire physics community's understanding of physical theory. These touchstone moments of brilliance and transcendence are usually painted as discrete revolutionary moments of genius-inspired innovation and are attributed to a small number of scientific heroes. In many of these narratives the heroes are painted as unique personalities and are distinguished from the "average" scientist as much as possible. This characterization of a special breed of scientist as the singular source of innovation being fundamentally distinct from the normal scientist due to some quirk in personality, training, or position tends to exaggerate the inevitability of a certain physicist's greatness and the inevitability of his or her revolutionary contributions.

In reexamining the traditional quantum narrative represented by Table 1.1, one can begin to understand the interconnectedness, richness, and complexity of the scientific community of quantum physicists and the early interpretation debates that helped inform our modern understanding of quantum theory during the first half of the 20th century. In particular,

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"Who Invented the "Copenhagen Interpretation"? A Study in Mythology," Philosophy of Science, Vol. 71, No. 5 (2004): 669-682.

[6] John L. Heilbron, "Rutherford-Bohr Atom," *Am. J. Phys.,* Vol. 49 No.3, (March, 1981): 223.

we are interested in reexamining two physicists, Louis de Broglie and David Bohm, and their specific contributions to early quantum interpretation debates, as well as the context within which they made their innovations. Both physicists were important to these debates yet have been inadequately studied in previous quantum historical narratives.

Over the past two decades there have been a number of groups within the physics community, in addition to an increasing number of philosophers of science, that have studied what has come to be called "de Broglie-Bohm" quantum theory or "Bohmian Mechanics"[7] as a deterministic alternative to the orthodox interpretation represented by the "Copenhagen Interpretation"[8] of quantum theory. Interestingly enough, in naming this deterministic approach de Broglie-Bohm quantum theory, some scholars have forcibly brought together two distinct research programs by de Broglie and Bohm that were independently developed and had their own complex historical contexts of innovation separated by almost twenty-five years. The tendency of many of these physicists and philosophers of science who champion this particular interpretation of quantum theory has been to raise its visibility and importance by emphasizing the contingent nature of the Copenhagen Interpretation through the use of counterfactual histories. In doing so, they raise de Broglie and Bohm up to be heroes who were unfairly marginalized and whose theories were unjustifiably suppressed. In an attempt to salvage a long

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[7] As examples of this literature see: Physics groups at the University of Insbruck- M. Penz, G. Grübl, S. Kreidl and P. Wagner, "A new approach to quantum backflow," *J. Phys. A: Math. Gen.* 39 (2006): 423-433; and Rutgers University- Dürr, D., Goldstein, S., Tumulka, R., and Zanghì, N., 2004, "Bohmian Mechanics and Quantum Field Theory," *Phys. Rev. Lett.* 93: (2004): 1-4. The most prominent Philosopher championing the de Broglie-Bohm theory was James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, (Chicago: University of Chicago Press, 1994).

[8] In studying the correspondence of the key players, it becomes clear that this so called "Copenhagen Interpretation" or orthodoxy was not as rigid as one might think. In *Quantum Dialogue* Mara Beller argues that the Copenhagen Interpretation was not a singular unified entity. Even after the orthodoxy began to congeal publically after 1927, there were disagreements between "Copenhageners" on a number of important issues. It's probably more accurate to describe the Copenhagen Interpretation as a bundle of different variants than a homogenous framework. However, in the late 1920s, when it was placed in contrast to alternate formulations this bundle became tighter and more homogenous.

ignored alternate theory, many of these “historical” narratives have essentially distorted the history towards the other extreme. Neither of these polarizing and distortive approaches is ideal.

The following dissertation constructs a history in which we can study both Louis de Broglie and David Bohm in their respective contexts of innovation without giving in to distortions encountered in traditional narratives of the quantum revolution. As such, we will be reexamining the emergence and development of alternate formulations and interpretations of quantum theory from the 1920s to the early 1950s. We have chosen to highlight de Broglie’s alternate wave mechanical interpretation and Bohm’s hidden variables program with the hope that a more balanced and nuanced story of the complex contexts of innovation can help dispel some of the myths about the roots of these theories. We rely on various methods of analysis including close readings of the published and unpublished scientific papers, correspondence amongst physicists, and aggregate biographical understandings of academic networks within their wider socio-political environments.

While both physicists were interested in restoring determinism to quantum theory, their particular approaches were very different. In addition, when placing each individual physicist into his appropriate historical context, we discern many important differences between the two research programs. Specifically, we are interested in the following: who they trained with, where they trained, what their primary influences were, what their motivations were for proposing their particular alternate interpretations, and how each was received within his physics community.

In chapter one, we set the foundations for the study by examining the roots of the quantum revolution. These roots stretch back farther than one might guess, necessitating a careful study of the quantum roots in the 19th century and how they led to the "slow rise" of quantum physics before the early 1920s. This approach is important, especially in light of the fact that most alternate interpretations of quantum theory relied heavily on William Rowan Hamilton's 19th century theories. In chapter two, we set the wider socio-political context of post World War I Europe and how it influenced the development of quantum theory. In particular, we look at the four elite quantum schools in Munich, Copenhagen, Göttingen, and Leiden and the tight academic network that connected them. We use aggregate biographical analysis employing a database to tease out patterns in physicists' movements during their formative training years.[9]

With the context set for the 1920s, chapters three and four focus on the emergence of wave mechanics and the development, especially by de Broglie, of alternate competing formulations and interpretations of the quantum theory. This complex and dynamic period is not characterized by an established interpretation. Instead what we see going into the 1927 Solvay Council is a community that has been fractured both politically and on many issues of fundamental importance to physics including the losses of particle trajectories and determinism. The presentation at Solvay by Born and Heisenberg on various equivalent formulations of quantum mechanics seems to mark a transition point for the physics community from one in which there were competing alternate interpretations and formulations of quantum theory to a community in which interpretation debates began to wane in favor of a rising pragmatism. In chapter five we examine the various reasons for the rising pragmatism within physics and how that transition corresponded to a lull in interpretation debates. We find that after World War II,

[9] The database was developed as part of the research and contributed to the author's analysis. It has since been expanded and renamed: The Aggregate Biographical Data Repository (ABDR).

and especially by the end of the 1940s, there was a growing tendency by some physicists to once again contemplate problems associated with the foundations of quantum theory.

The last chapter of our narrative picks up the thread from this point and examines David Bohm's science and his life. As it turns out, Bohm would develop his hidden variables program while suspended from Princeton University in the spring of 1951. By the time he published his famous papers in January, 1952, from Brazil, Bohm considered himself in exile thanks to American political intolerance. However, the question of why he produced his hidden variables theory has yet to be studied adequately. While scholars have tended to produce apologetic or counterfactual histories dealing with Bohm and his development of hidden variables theory, none of these have properly dealt with Bohm's context of innovation. A close reading of his quantum textbook and his hidden variables papers, both completed within his 1951 politically charged academic exile, paints a different story than the one we are accustomed to reading. In this narrative we are able to establish a more comprehensive understanding of the motivations and influences behind his important innovations. As is commonly portrayed, Bohm did not reverse his ideas completely between the writing of his textbook *Quantum Theory* and the development of his hidden variables papers. When understood in its context, his 1951 transition of interpretation becomes less stark and more comprehensible.

The importance of understanding this transition should not be understated, as Bohm's hidden variables theories ultimately served as a harbinger for what became a new subfield of physics focused on the studies of the foundations of quantum theory and the corresponding interpretations that arose. While these debates certainly did not begin with the publication of Bohm's hidden variables papers in 1952, his challenges to the accepted axioms of the orthodox Copenhagen Interpretation served as a catalyst for some physicists to engage, or reengage, quantum interpretation debates. The celebrated physicist John Bell would later say about Bohm's 1952 papers that he had seen the "impossible done."[10] As a result of Bohm's work on hidden variables theories, Bell went on to develop his seminal inequalities, undermine the von Neumann impossibility proof, and place these early interpretation debates into the physics community's mainstream dialog.

A note on terminology:

There are certain terms that will be used which have dramatically different meanings depending on the context of their usage. As this is a historical narrative roughly covering the first half of the 20th century, terms being used will need to be understood in their proper context. We begin with the notion of physics itself. Physics is the systematic study of the natural world using analytical methods based in 'theoretical' as well as 'experimental' techniques. Historical narratives of the 20th century tend to focus entirely too much on either the theoretical or the experimental practices within the physics community, when in actuality these two practices have been tightly coupled since at least the 19th century. In this quantum narrative we find that the dialog between 'experiment' and 'theory' is critical to understanding the developments. We will be careful and deliberate in using 'quantum physics' to denote the combination of 'quantum theory' and 'quantum experiment' without ignoring either of these contributing practices.

[10] John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, (Cambridge: Cambridge University Press, 1987), 160.

The seemingly interchangeable use of quantum mechanics and quantum theory has a historical context. When Max Born coined the term quantum mechanics in a 1924 paper,[11] the term quantum theory had been in use for almost twenty years, applied to theoretical research concerned with any radiation and material phenomena that exhibited 'quantum' effects - in other words, any theory that incorporated the use of Einstein's quantum hypothesis. The invention of the term quantum mechanics was due to the fact that, in 1924-25, quantum theory was in a transitional stage made up of a hodgepodge of inconsistent and at times ineffective formulations and interpretations. Born introduced the term quantum mechanics to try and induce a systematization of this quantum theoretical hodgepodge and ultimately pave the way for an effective transition from classical mechanics to a new, more consistent, profound, and generalized mechanics. Therefore, this term will be used only in the context that it was intended and will not be completely interchangeable with quantum theory.

As a last note on terminology, we should make a distinction between formulation and interpretation within physics. In this dissertation I have employed the term formulation to mean the mathematical structure upon which a theory is built. Just as physicists performing experiments use various gauges, detectors, and other apparatuses in their laboratories, the formulations are the basic tools employed by the physicist engaged in theoretical research. As we shall see throughout this narrative, when two formulations can be proven to be mathematically equivalent, then they are both valid under the same theoretical structure. For example, this is true of the matrix and wave mechanical formulations of quantum theory. However, since alternate formulations, like de Broglie's pilot wave formulation and Bohm's hidden variables theory, are not mathematically equivalent to the orthodox matrix and wave-

[11] The name "quantum mechanics" appeared for the first time in the literature in Max Born, "Über Quantenmechanik," Z. Phys. 26, 379–395 (1924).

mechanical formulations, they cannot be considered part of the same theory. We should be careful to note here that this lack of mathematical equivalency does not invalidate alternate formulations as a whole. If the alternate formulation can be used to explain and predict natural phenomena either in experiment or via theoretical models, then it should be seen as a valid theoretical tool. We should understand that there is plenty of room for alternate formulations, whether equivalent or not, and as long as these are deemed useful to physicists, they should not be discarded solely due to some theoretical inertia surrounding accepted theories.

Chapter 1: The Roots of Revolution- The Slow Rise of Quantum Physics

"…a thorough reading of the literature of [a certain] time shows [everyone] is worrying about something." –Richard Feynman[12]

1.1- Introduction

In the following discussion, we will examine the early part of the quantum revolution allowing us to appreciate some of the nuances within the larger scientific debate that were instrumental in leading to the emergence of quantum mechanics in all its various formulations during the height of the quantum revolution in the mid to late 1920s. It was not out of the ordinary in the study of the physical sciences in the late 19th and early 20th centuries to have simultaneous competing formulations and interpretations of a theory persist over time. While the deciding factor between simultaneous theories was almost always a theory's agreement with empirical evidence, this test was not always immediately conclusive. This is certainly the case with the complex scientific dialogs that made the height of the quantum revolution so dynamic and infused it with so much potential.

While most traditional quantum narratives seem to begin in 1900 and then divide the quantum revolution into two distinct stages, the old quantum theory (pre-1925) and the new quantum theory (post 1925), this seems to be an unwarranted and artificial periodization. The process of the quantum revolution was in fact seeded in the late 19th century and ran throughout the first three decades of the 20th century. It is true that this process was not continuous and had many bursts of activity and lulls where little progress was made on the subject but it would be disingenuous to say that the innovative research de Broglie, Heisenberg,

[12] Badash, "The Completeness of Nineteenth-Century Science," 49.

Schrodinger, Einstein, Born, and company were doing in 1925 was qualitatively different than the research being done by Einstein, Born, Planck, Sommerfeld, Bohr, and others between 1900 and 1924.

Far more interesting than the seemingly arbitrary categorization of two distinct quantum theories seemingly based on a forced picture over emphasizing generational transitions are other socio-political, economic, and academic factors that were formative to the process of scientific innovation. If we trace these various factors, what we get is a rich tapestry with a plurality of threads, woven together and eventually branded under a single subject of quantum theory. This evolution of a single revolution is inherently more interesting than setting up false contrasts between some 'old' and 'new' quantum theories. Ultimately, claims that an 'old' quantum theory gave way, or was superseded, by a 'new' quantum theory after 1925, only help to emphasize a falsely conceived inevitability of historical outcomes and not their true contingent nature.

1.2- The Roots of the Quantum Revolution

It is very common in traditional narratives of the quantum revolution to describe the ethos of late 19th century physics as accurately represented by Michelson's quote from Table 1.1. So, according to these narratives, in the 1890s the physics community generally agreed with Michelson that physics, as an academic pursuit, had established all its foundational elements and all that was left was to merely clarify the details. Unfortunately, the historical evidence does not support this claim. Not only were there many ongoing research programs studying foundational aspects of physics at this time, but it is not even clear what Michelson himself believed. In 1902 Michelson published his book *Light Waves and Their Uses* where he stated “the day seems not far distant when the converging lines from many apparently remote regions of thought…will meet on common ground.”[13] This comment, although hopeful, does not seem to indicate someone who believes that physics was on the immediate cusp of reaching some sort of summit. In any case, a far more representative picture of physicists’ perceptions of the state of affairs at the turn of the 20th century was that given by Heinrich Hertz in the introduction to his last published book *The Principles of Mechanics Presented in a New Form (1894)*:

> All physicists agree that the problem of physics consists in tracing the phenomena of nature to the simple laws of mechanics. But there is not the same agreement as to what these simple laws are…it is just here that we no longer find any general agreement. Hence there arise actual differences of opinion as to whether this or that assumption is in accordance with the usual system of mechanics, or not. It is in the treatment of new problems that we find a real bar to progress.[14]

Historical evidence does show that the revolution that supposedly began in 1900 with Planck’s adoption of quantized energy and his introduction of the constant ‘h’, had real, and profound, roots in the 19th century. In fact, there is no evidence for an instantaneous, incommensurable, and discontinuous jump in physical theory in 1900. If one is to understand the innovations that Planck and Einstein contributed between 1900 and 1905 we must understand them within the greater context of innovation extending back well into the 19th century.

The specific dynamics of these revolutionary transitions are difficult to conceptualize or generalize and each example has its own unique character.[15] In the case of the birth of quantum

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[13] Helge Kragh, *Quantum Generations,* (Princeton: Princeton University Press, 1999), 4.
[14] Salvo D’Agostino, *A History of the Ideas of Theoretical Physics: Essays on the Nineteenth and Twentieth Century Physics,* (New York: Springer, 2001), 93.
[15] Debates in the history and philosophy of science addressing this point go back at least to Thomas Kuhn’s *The Structure of Scientific Revolutions,* (Chicago, IL: University of Chicago Press, 1996). We will rely

physics, we notice that at the turn of the 20th century there was an extended transitional period where physicists borrowed from multiple overlapping theoretical frameworks simultaneously available to them so that they might cobble together an acceptable representation of nature. While there may have been a temporary tolerance of various competing frameworks due to their mathematical equivalency, as Hertz stated in 1894, there were real differences in the underlying physical assumptions governing natural phenomena. That said, physics research in the late 1800s was far from dead or done.

Most traditional narratives of the birth of quantum physics (See Table 1.1) begin in 1900 with the moment that Max Planck, in an attempt to find a mathematical fix so that he could correctly model the experimentally obtained blackbody radiation (electromagnetic radiation from a cavity at thermal equilibrium) and avoid the paradox that had come to be known as the "ultraviolet catastrophe", incorporated a new constant h (quantum of action) and assumed discrete energy quanta. The story goes that the full physical significance of Planck's mathematical solution remained unrealized by Planck himself and would have to wait five years for Einstein's revolutionary mind to arrive at a physical interpretation of the quantization of electromagnetic radiation.

In reality, 1900 is not the ideal moment to start any narrative of quantum physics. In order to understand Planck's innovation one must understand the context of that innovation. Why was Planck interested in this problem? Who did he study with? Did his training inform his methodology? What scientific dialogs or debates was Planck a part of? In other words, what were the factors that contributed to his innovation? In order to answer these, and many other questions, so as to construct a reasonable context of innovation one would need to extend the

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on Kuhn's theoretical frameworks to study the quantum revolution as they are too restrictive. In particular we take issue with his requirement that paradigms be incommensurable between them.

narrative back well into the 19th century. Obviously, our narrative is concerned with the emergence of alternate quantum theories in the 1920s and 1950s, so with that in mind we will keep this excursion brief and focused on extracting general brush strokes or themes that are particularly relevant to our later quantum narrative.

There are three critical scientific and social threads that arose during the 19th century that are particularly relevant to our analysis of the quantum revolution. The first has to do with the development of the relationship between theory, experiment, and industry; the second, deals with the rise of plurality in theoretical frameworks; and the third has to do with the heterogeneity of contexts along national and institutional lines.

At the end of the 18th century and beginning of the 19th century the traditions of natural philosophy, natural history, and chemical philosophy contained activities and research interests that we would consider a subset of the modern scientific fields of physics and chemistry. However, it took until the middle of the 19th century for physics to begin to take shape, as we understand it today. Towards the end of the 18th century and in the early decades of the 19th century there was an emphasis, especially in France, on the maturation of analytical mathematical methods. Mathematicians and natural philosophers worked to extend and generalize the analytical methods of calculus, applying these new robust tools to create mathematical models for myriad mechanical, optical, hydrodynamic, acoustic, thermal, and electrical phenomena. These were the foundations of what would later be known as "theoretical physics".

By the latter half of the century, these new fields of study were being axiomatized and represented mathematically within generalized physical theories.[16] It is no coincidence that these experimental and phenomenological forms of inquiry, on a large scale, were being added to the more abstract 18th and early 19th century traditions of mathematical natural philosophy; after all, the industrial revolution was transforming societies as it spread throughout Europe and the United States. As a result of this and other factors, academic interests began to couple themselves to industrial interests and the lecture halls of traditional natural philosophy began to share time with the new teaching laboratories springing up at universities.

By the close of the 19th century, investigations in experimental physics were being undertaken at laboratories that resembled enormous industrial complexes, churning out experimental data that required ever increasing levels of precision measurements. While this theory-experiment-industry relationship in the 19th century was developing a codependent nature, it became clear that it would not be a trivial matter to pigeonhole all natural philosophers as either experimentalists or theorists nor to distinguish between academic and industrial research programs. Natural philosophers like William Thomson (later to become Lord Kelvin)[17], James Clerk Maxwell, Hermann von Helmholtz, Gustav Kirchhoff, J.J. Thomson, and Heinrich Hertz investigated natural phenomena using any tools at their disposal and were adept at both theory construction and experimental techniques making it impossible to discretely categorize them based on their activities. Not all physicists were adept in both mathematical

[16] Mary Jo Nye, *Before Big Science: The Pursuit of Modern Chemistry and Physics, 1800-1940,* (Cambridge, MA: Harvard University Press, 1999), xiv – xv.

[17] William Thomson was knighted in 1866 and became Baron Kelvin in 1892 (London Gazette: no. 26260, p. 991, 23 February 1892*)*. Since J.J. Thomson (no relation to Kelvin) will be referenced later in the chapter, in order to avoid confusion we will refer to William Thomson from here on as Kelvin.

and experimental methods but even in Planck's case we see that although he himself was not at home in the laboratory, he was very keen to work in tight coordination with experimentalists.[18]

In *A History of the Ideas of Theoretical Physics: Essays on the Nineteenth and Twentieth Century Physics*, Salvo D'Agostino argues that historians of science have not done enough to connect the seeds of 20th century physics (quantum theory and relativity) to the epistemological concerns with framework plurality and the theory-experiment relationship from the 19th century.[19] We know that in the 1840s and 1850s there was a trend towards serious investigation into the notion of interconvertibility of forces by the likes of Helmholtz, Kelvin, and Maxwell. Kelvin had studied Ampère's mathematical formulation of Faraday's electrostatic force, which was well in line with the French analytical school and securely based in the Newtonian principle of force acting at a distance, but he chose a different approach. Instead of relying on some mysterious action at a distance, Kelvin ventured to express electricity as mathematically analogous to a hydrodynamic fluid like heat. Via this analogy, Kelvin was able to show the mathematical equivalence of two distinct interpretive frameworks describing the same underlying physical phenomenon.[20] As a result of his investigations into hydrodynamics, Kelvin went on to develop many innovative concepts like the electromagnetic vector potential and a full-blown theory of atomic phenomena based on vortices. How could two distinct phenomenological interpretations, action at a distance and a physically connective field of force acting as a fluid, both be simultaneously valid?

Building off of Helmholtz and Kelvin's studies into hydrodynamics, Maxwell in 1855 began to argue for a heuristic approach to theory building that involved both iteration and analogy as cornerstones. According to Maxwell: "The chief merit of a temporary theory is that it

[18] Nye, *Big Science*, 115.
[19] D'Agostino, *A History of the Ideas of Theoretical Physics,* 219.
[20] Nye, *Big Science,* 65 – 67.

shall guide experiments, without impeding the progress of a true theory when it appears…it is a good thing to have two ways of looking at a subject, and to admit that there are two ways of looking at it."[21] Eventually, his reliance on iteration and analogy led Maxwell to build his theory of electromagnetism using Lagrange's abstract mathematical formulation and without the need for Helmholtz and Kelvin's idea of an electrical hydrodynamic fluid. In fact, in his celebrated 1864 publication *Dynamical Model of the Electromagnetic Field*, Maxwell avoided rooting his framework in any description of the underlying mechanistic dynamics.[22]

The acceptance of multiple coexisting frameworks was so basic to research in the latter part of the 19th century, twenty years after Maxwell's celebrated publication Heinrich Hertz, a former student of Helmholtz, was still studying the various contrasting electrodynamic theoretical frameworks. In 1884 Hertz published his paper *On the Relations between Maxwell's Fundamental Electromagnetic Equations and the Fundamental Equations of the Opposing Electromagnetics*, where the opposing equations he refers to are those predominantly held in Germany, most notably by his mentor Helmholtz.[23] As a final testament to the importance of plurality within theoretical physics in the late 19th century we have the opinion of Ludwig Boltzmann. On September 22, 1899 in an address to natural scientists in Munich, Boltzmann stated:

> Maxwell warned against regarding a particular view of nature as the only correct one merely because a series of consequences flowing from it has been confirmed by experience. He gives many examples of how a group of phenomena can be explained in two totally different ways, both modes of explanation representing the facts equally well.[24]

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[21] D'Agostino, *A History of the Ideas of Theoretical Physics,* 78.
[22] Nye, *Big Science,* 74.
[23] D'Agostino, *A History of the Ideas of Theoretical Physics,* 167.
[24] Ibid., 202.

Apart from the maturations of the analytical and experimental methodologies, the widening scope of subject areas, and the growth of pluralistic theoretical frameworks within the physical sciences, inquiry began to take on national and institutional characteristics. The development of the modern university systems and heterogeneity in training methods throughout Europe and the United States during the 19th century reveals the uneven nature of these developments.

In her survey of 19th century physics and chemistry (*Before Big Science*), Mary Joe Nye touches on these trends. While in the early 1800s Paris was the destination for international or peripatetic scholars wanting to study physics and chemistry at the *École Polytechnique*, the Sorbonne, the Museum of Natural History, and the Collège de France by the 1860s French scientists were concerned that the German university system had surpassed their own in this regard. By the mid 19th century students from Britain and the United States were flocking to Berlin as well as more remote places like Königsberg, Leipzig, Göttingen, and Halle to study within the reformed German university system. The German educational reform program had begun in reaction to what was interpreted as a superior Napoleonic university system. In the French academy emphasis was on national examinations and less so on individual research projects leading to a *doctorat d'État*. In contrast to this system however, the Germans emphasized individual research and the writing of doctoral and habilitation theses as prerequisites for a career in academia.

By the latter half of 19th century the British and American educational reform initiatives had begun to borrow heavily from the blueprint established by the German university system. Based around seminar, colloquia, and laboratory training the emphasis began to evolve towards more specialized subfields and higher precision methodologies (both analytically and in the lab).

At this point it was clear that although there was a strong resemblance among most European research programs in the physical sciences there remained real and perceived pedagogical, philosophical, and methodological differences between German, French, and British programs. While Nye's survey of 19th century "physics" and "chemistry" is adequate as an introduction, it is important to also consult more contextual studies like Andrew Warwick's *Masters of Theory*[25] and Myles Jackson's *Spectrum of Belief*[26] for a deeper understanding of the varied contexts of 19th century scientific pedagogy and research in England and Germany respectively. In these two seminal studies we gain a sense of how important national, cultural, and institutional contexts were to the development of physics in the 19th century. Both Warwick and Jackson paint a picture of varied training methods, pedagogical philosophies, and nationalistic tendencies playing critical roles in scientific innovation.

For example in *Spectrum of Belief* Jackson describes a discourse Hermann von Helmholtz gave on March 5th, 1887 in the hall of the Berlin Rathaus celebrating the centennial anniversary of Joseph von Fraunhofer's birth. Helmholtz took the opportunity to distinguish German science from that of British and French sciences. He claimed that Germany had the opportunity to claim supremacy in the physical sciences based on the notion that Fraunhofer was the first in a line of German master artisans who had contributed uniquely to the progress of experimental techniques. Moreover, Helmholtz claimed that given the right nurturing (ie. funding), Germany would surely remain a world leader in laboratory experimentation. He warned however, that if the German government was not willing to support projects like the proposed sprawling national laboratory *Physikalisch-Technische Reichsanstalt* (Imperial Institute of Physics and

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[25] Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics,* (Chicago, IL: The University of Chicago Press, 2003).

[26] Myles Jackson, *Spectrum of Belief: Joseph von Fraunhofer and the Craft of Precision Optics,* (Cambridge, MA: MIT Press, 2000).

Technology - PTR) on the outskirts of Berlin, the British would quickly take the dominant role in all experimental sciences. The decision to fund the PTR was voted on by the *Reichstag* only three weeks after Helmholtz delivered this address and he was naturally made its first director. From Jackson's exposition it becomes clear that Helmholtz was using the banner of nationalistic pride to advance his scientific agendas, a tactic not discarded since.[27]

The three threads that have been discussed: the development of the relationship between experiment and theory, the rise of plurality in theoretical frameworks, and the heterogeneity of contexts along national and institutional lines; are critical to understanding not only Planck's context of innovation in 1900 but the legacy these threads maintained within the physics community throughout the early decades of the 20th century. In Planck's case it is important to understand him as a German mathematical physicist with a varied palette of theoretical frameworks to choose from, someone who had studied under Helmholtz and Kirchhoff, alongside Hertz, and as a theorist who worked in tight collaboration with experimentalists at the PTR. This characterization makes his innovative leap significantly more comprehensible. However, while Planck's story of innovation is one of the more widely studied episodes in the history of modern science, we must note that it is not immune to historiographic controversy.

1.3- The Seeds of Quantum Physics – Discontinuity and Blackbody Radiation

In the 1970s and 1980s scholars such as Thomas Kuhn and Martin Klein entered into hotly contested debates as to whether Planck should be considered a classical physicist who set

[27] Jackson, *Spectrum of Belief*, 181-188.

the stage for Einstein's revolutionary plunge or a quantum revolutionary in his own right.[28] This controversy has not been sufficiently settled and although eminent scholars like Peter Galison have tried to bridge the gap between the two primary historical interpretations, the controversy remains. In 1978 Kuhn published his now famous *Black Body Theory and the Quantum Discontinuity, 1894-1912* where he challenged the pre-existing narrative most notably disseminated by Klein's 1962 *"Max Planck and the Beginnings of Quantum Theory"* in which Klein interpreted Planck's innovative steps in 1900 as the conscious invention of the quantum discontinuity. Kuhn's reinterpretation of the historical data delayed Planck's realization of the significance of his own work by almost a decade pushing it back to 1908 - 1909. The historical facts interpreted by both scholars are the same, but Kuhn and Klein draw dramatically different conclusions. As Kuhn stated in his 1984 rebuttal of his critics:

> [My] interpretation of Planck's early theory is clearly non standard. Also, it could be wrong. No single piece of available evidence demands it, and evidence incompatible with it could yet be discovered, a letter, for example, or an unpublished manuscript. As things now stand, however, evidence for [my] reinterpretation seems to me overwhelming.[29]

While the facts in both interpretations are essentially the same, it seems to me that Kuhn's reading of those facts is more reasonable. The following discussion of Planck's contributions will be based largely on Kuhn's interpretation; where there are discrepancies they will be noted.

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[28] For more on this historical debate primarily between Kuhn and Klein see: Olivier Darrigol, "The Historians' Disagreements over the Meaning of Planck's Quantum." *Centaurus,* Vol. 43, (2001): 219–239; Thomas S. Kuhn, *Black-Body theory and Quantum Discontinuity, 1894-1912* (Oxford: Oxford University Press, 1978); Thomas S. Kuhn, "Revisiting Planck," *Historical Studies in the Physical Sciences* 14, (1984): 231-252; Martin Klein, ''Max Planck and the beginnings of the quantum theory,'' *Archive for the history of exact sciences*, 1, (1962): 459–479; Martin Klein, Contribution to ''Paradigm lost? A review symposium,'' *Isis*, 70, (1979): 429–433; Peter Galison, ''Kuhn and the quantum controversy,'' *British journal for the philosophy of science*, 32, (1981): 71–85; and John L. Heilbron, *The Dilemmas of an Upright Man: Max Planck and the Fortunes of German Science***.** (Cambridge, MA: Harvard University Press, 2000).

[29] Kuhn, "Revisiting Planck," 233.

Max Planck, born in 1858, was a German mathematical physicist who studied within the thermodynamic and electrodynamic traditions established at the Universities of Munich and Berlin under Helmholtz, Kirchhoff, and Jolly. He presented his habilitation thesis on *Equilibrium states of isotropic bodies at different temperatures* in 1880 and later focused his studies on entropy. After Kirchhoff's death in 1887, Planck became his successor as the University of Berlin's only theoretical physicist and published a textbook on the fundamentals of thermodynamics (1st ed. Published in 1893) in which he attempted to reconcile thermodynamics and electromagnetism without resorting to unnecessary reductionism, a reliance on atomic theory, or Boltzmann's probabilistic approach to mechanics.

Upon his arrival in Berlin, Planck had joined the local Physical Society and thus began collaborating with experimental physicists working out of the PTR. In 1894, one of the focal points of research at the PTR optics laboratory was the study of temperature effects on the luminosity of certain materials. This research was critical for Germany's fledgling electric lighting industry. While the experimentalists were making progress on improving the spectral analysis of emission and absorption energy distribution curves for different materials, little theoretical headway had been made in understanding Kirchhoff's 1859 thermal radiation law.[30] According to Kirchhoff's law, the surface of a body, at thermal equilibrium, will have equal emissivity and absorbance regardless of the substance employed. In its ideal form of full emissivity/absorbance, this became known as blackbody radiation and was known to be dependent only on temperature. By 1884 Josef Stefan and Ludwig Boltzmann had shown that the relationship between the blackbody radiation energy and the blackbody temperature was: $u = \sigma T^4$, where u is the total energy density, T is the absolute temperature, and σ is the Stefan-

[30] Nye, *Big Science*, 115-117.

Boltzmann constant. Boltzmann had taken a problem that had been addressed primarily in the realm of thermodynamics, and applied Maxwell's electromagnetic theory to it.

While it was helpful to be able to model the blackbody Energy density – Temperature dependence, commercial and industrial interests (via labs like the PTR) were beginning to look to the spectral distribution of blackbody radiation as a focal point of research. Wilhelm Wien had presented his doctoral thesis in 1886 based on experiments he had conducted in Helmholtz' lab at the University of Berlin between 1883 and 1885. In 1890, after a hiatus from academia, Wien returned to work with Helmholtz but this time under the auspices of the new PTR of which Helmhotz had been made director. In 1893, Helmholtz had Wien working with Ludwig Holborn researching different methods of measuring high temperatures with a Le Chatelier thermocouple element (a relatively new technology at the time). Simultaneously, Wien was working on theoretically modeling the radiation of heat. As a result, between 1893 and 1894 Wien showed that if one knew the spectral distribution of blackbody radiation at a certain temperature it could be deduced for any other temperature. His displacement law:

$$u(\lambda T) = \lambda^{-5}\Phi(\lambda T)$$

now showed a relationship between energy density (u) and both wavelength (λ) and absolute temperature (T) through some, still unknown, displacement function ϕ(λT). Wien developed his final blackbody distribution law in 1896 unveiling the displacement function ϕ(λT) to be of exponential form: $e^{\left(-\alpha/\lambda T\right)}$, where α is a universal constant.

At first the Wien distribution law was very successful both as an explanatory and predictive model. There were ever more precise experiments which were conducted up through 1899 that verified this theoretical model. However, while Wien's mathematical formulation seemed to be pragmatically successful, it was based on empirical observations and thus lacked a fundamental derivation from first principles that might make it generally applicable. Planck, who had been collaborating with Helmholtz and the PTR laboratory, took on the task of trying to derive Wien's distribution law from first principles. Throughout this period there was a deep division within the physics community around Boltzmann's program to derive thermodynamic laws using statistical and mechanistic models. Planck was initially not inclined to follow Boltzmann's approach and was determined to reconcile electromagnetism and thermodynamics without resorting to what he considered unnecessary reductionism and a reliance on unconfirmed atomic theory. Specifically, Planck had been working on a way to derive the second law of thermodynamics relating to entropy using the fundamental equations of Maxwell's electrodynamics. If Planck could derive the law of entropy from the first principles of electromagnetic theory and Wien's distribution law was a direct result then Planck would have an experimentally verifiable way of reconciling thermodynamic theory and electromagnetism.

Planck, firmly rooted in his thermodynamic training, was convinced of the soundness of its second law necessitating the increase in entropy over time of all isolated physical systems and the unavoidable consequence of time irreversibility. He was also well aware of the potentially paradoxical consequences of Boltzmann's statistical mechanics program, as presented in his polemical papers of 1872 and 1877, thanks to the criticisms leveled by Johann Loschmidt's reversibility paradox. From 1866 through 1877 Boltzmann had attempted to derive the second law of thermodynamics from basic kinematic and mechanistic assumptions.

His papers of 1872 (*Über die Beziehung*) and 1877 (*Weitere Studien*) are two of the most widely cited of the 19th century. In his 1872 paper Boltzmann introduced his concept of the H-theorem and showed how this system could model a gas' irreversible evolution towards an equilibrium state. His function: $H(t) = l(f \log f)$, where f (q,p,t) is the function representing the fraction of molecules in a certain element of phase space. Boltzmann argued that as H(t) approached a minimum the gas would reach equilibrium described by a Maxwellian distribution. In that sense, H was simply the inverse of the thermodynamic Entropy (S) and Boltzmann claimed that he had been able to derive the second law of thermodynamics. As a result of these claims, Boltzmann faced firm criticism from, among others, Loschmidt in 1876 with his "reversibility paradox". Loschmidt claimed that it was absolutely impossible to derive a theorem of macroscopic (i.e., large numbers of particles N) irreversibility from purely mechanistic principles on a microscopic scale (i.e., individual kinematic particle dynamics) since they themselves were symmetric in time and thus reversible.[31]

In the late 1890s while deriving Wien's distribution law from first principles, Planck was in essence retracing Boltzmann's polemical work by focusing his efforts on the reconciliation of the second law, and its irreversibility, with Maxwell's theory of electromagnetism. After all, Loschmidt's irreversibility paradox applied equally well whether you started with molecular trajectories or resonators producing electromagnetic radiation. In order to gain a foothold in the first principles of electrodynamics, Planck looked to an old colleague, Heinrich Hertz. Like Planck, Hertz had studied for his habilitation in Berlin under Helmholtz and Kirchhoff. Both Hertz and Planck completed their degrees contemporaneously, in 1880, so Planck was well aware of Hertz' experiments on electromagnetic radiation. When, in 1886-1887, Hertz discovered radio waves in his laboratory and was able to square them with Maxwell's equations, he simultaneously deflated Helmholtz' theory of electromagnetic radiation and gave Planck the

[31] Harvey R. Brown, Wayne Myrvold, and Jos Uffink, "Boltzmann's H-theorem, its discontents, and the birth of statistical mechanics," *Studies in History and Philosophy of Modern Physics* 40 (2009): 177.

resonators he would need to build his theory of blackbody radiation. In 1899, based on Hertz' and Maxwell's theories, Planck was able to successfully derive an expression for the entropy of an electromagnetic resonator and thus build, from first principles, Wien's blackbody distribution law.

Experimental techniques in the late 1890s had advanced tremendously, thanks in large part to the research efforts in laboratories like the PTR. The precision with which experimenters could measure temperature and wavelength deviations was unprecedented. In the fall of 1899 experiments at the PTR by Otto Lummer and Ernst Pringsheim began to show deviations from the Wien-Planck blackbody spectra. As it turns out, the distribution law held true at short wavelengths (small values of λT) but was flawed at longer λs (larger values of λT). On October 19th 1900, Planck presented a modified version of his (and Wien's) distribution law for blackbody radiation that fit the experimental results. He was forced to assume a different expression for the entropy of the individual oscillators and then re-derive the general expression. The result was Planck's blackbody radiation law:

$$u(\nu T) = \frac{A\nu^3}{\left[e^{\left(\beta\nu/_T\right)} - 1\right]}$$

where ν is the angular frequency, T is the absolute temperature and A and β are constants.

Of course, Planck had not arrived at this expression as a derivation from first principles because he could not explain the new expression for the entropy of a resonator so, he immediately went to work trying to rationalize this new expression for entropy. As stated earlier, Planck was familiar with Boltzmann's papers from 1872 and 1877 and believed that Boltzmann's approach to deriving entropy from mechanistic first principles was flawed not because he had used probabilistic arguments but because he had relied on the notion of chaotic molecular mechanics. Over the next two months Planck worked towards the derivation of his new distribution law following closely Boltzmann's probabilistic derivation for entropy from 1877. However, Planck made it clear that he was not using Boltzmann's N molecules but was referring to N electromagnetic "resonators". He was following Boltzmann's probabilistic arguments for deriving the law of entropy but was basing it on the foundations of Maxwell's electrodynamic theory.

On December 14, 1900 Planck presented a paper before the Berlin Academy where he derived a new distribution law paralleling Boltzmann's methods for deriving entropy. Whereas in 1872 Boltzmann had developed his H-theory and pointed out the inverse parallel with entropy, in his 1877 paper he used probabilistic techniques to derive Maxwell's distribution. Boltzmann began with a gas of N molecules and a total energy E and worked out the different ways in which the total energy could be distributed amongst the N molecules. Boltzmann then divided the total energy continuum into P energy elements of size ε. Therefore, for the gas as a whole, Pε = E. All N molecules would then be randomly distributed among the P energy elements (w_1, w_2, w_3,…,w_p). So, how many molecules would be present in each element w_n? As it turns out, there are many possible distributions of the N molecules amongst the energy elements, and because the molecules are indistinguishable from one another, for each unique distribution there are $N!/w_1!w_2!...w_p!$ number of ways for it to be arranged. This number, derived from the combinatorial, is proportional to the probability of the particular distribution actually occurring. Boltzmann concluded that the distribution at equilibrium was the one with the maximum probability. For the gas to be in a state of equilibrium it implies that the entropy has reached a maximum value and is no longer increasing, so, the logarithm of this maximal probability must be proportional to the entropy of the gas.

In Planck's case the situation was slightly different. As he was dealing with resonators and not molecules he wanted to allow for different size energy elements ($\varepsilon_{1,}\ \varepsilon_{2,}\ \varepsilon_{3,}\ldots\varepsilon_p$) to be proportional to the individual resonator's frequency of vibration (v_n). In which case $\varepsilon_n = hv_n$ where h is the constant of proportionality known as Planck's constant or quantum of action. From here the derivation of the probability of being in one distribution state or another becomes more complicated. Planck used the combinatorial relationship describing the total number of ways of obtaining all possible distributions $(N + P\text{-}1)!/(N\text{-}1)!P!$, took its log and made that proportional to entropy. Planck was actually the first to express Boltzmann's entropy in the form $S = k \log W$ where S is the entropy, k is Boltzmann's constant, and W is a combinatorial representation of possible distributions.

Evidence shows that at first, Planck was not willing to offer a physical interpretation or a "quantum hypothesis". In fact, he did not consider his "energy elements" to carry any physical significance, especially not "energy quanta". Planck assumed a physical continuum of energy (not discrete energy levels) that was mathematically divided for derivation purposes and that eventually the discreteness would be sorted out. Planck viewed his invention of h as a necessary iteration or modification in a derivation from first principles. However, he did not see any discontinuity or incommensurability with the classical foundations of physics. Planck had achieved his research goals of deriving entropy from electrodynamic principles and providing a distribution function that did not deviate from experimental observations at long wavelengths. So, from 1900 to 1908 almost nobody, Planck included, seemed to pay any attention to his quantum of action.

In April 1908, Hendrik A. Lorentz gave an address in Rome where he surveyed past research on the problem of blackbody radiation. In his lecture, Lorentz described a bifurcation point in theory and gave two choices moving forward. The first was a theory based on the Rayleigh-Jeans formula which was, in principle, satisfactory but failed when tested empirically. The second was Planck's theory based on his distribution formula which while empirically successful was problematic for classical concepts such as an understanding of a continuous energy spectrum. While Lorentz first sided with the Rayleigh-Jeans distribution formula, the challenges from the German contingent eventually swayed his conviction. After drawing attention to the "problematic" nature of Planck's distribution law, Lorentz seems to have inspired some debate amongst the physics community in regards to the implications of such a 'quantized' formula. For Planck, it would take several years, and extensive correspondences with both Lorentz and Paul Ehrenfest to finally accept the physical significance of his 'quanta of action'.[32]

In retrospect, during a Nobel lecture in June 1920 Planck stated:

> What initially was a problem of fitting a new and strange element, with more or less gentle pressure, into what was generally regarded as a fixed frame has become a question of coping with an intruder who, after appropriating an assured place, has gone over to the offensive; and today it has become obvious that the old framework must somehow or other be burst asunder. It is merely a question of where and to what degree.[33]

As you may have noticed, the term "ultraviolet catastrophe" has yet to appear in our narrative of blackbody radiation. The reason for this omission is that Ehrenfest did not coin the term until 1911 (well after Planck's seminal work), and although Lorentz had hinted at a polemical theory choice between the Rayleigh-Jeans distribution formula and Planck's version in his 1908 lecture, the so-called ultraviolet catastrophe was never really a driving force for

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[32] Kragh, *Quantum Generations,* 63.
[33] Planck, Max. "The Genesis and Present State of Development of the Quantum Theory." Nobel Lecture June 2, 1920. http://nobelprize.org/nobel_prizes/physics/laureates/1918/planck-lecture.html

quantum innovation. Furthermore, whichever narrative you subscribe to (Klein's or Kuhn's), Planck's impetus for "fixing" the blackbody distribution formula was not the one from our traditional quantum narrative. Table 1.1 illustrates how many quantum mythologies rely on the notion that Planck, in order to fix the ultraviolet catastrophe and work out a single unifying blackbody radiation distribution law, invented h as a mathematical "trick". While Planck was obviously well aware of Wien's distribution law, by the time the Rayleigh-Jeans distribution formula had been derived it was well after 1900. We have also pointed out another important distortion in the quantum myth; far from some *ad hoc* mathematical trick, Planck's innovative steps were well rooted in established mathematical and theoretical techniques of the late 19th century.

Everyone seems to focus on Einstein's physical interpretation of Planck's "quantum of action" as the next logical step in the traditional mythological narrative. However, Einstein's seminal paper "On a Heuristic Viewpoint Concerning the Production and Transformation of Light" published on June 9, 1905 during his *annus mirabilis*, did not even use Planck's distribution law from 1900. In his paper Einstein was proposing something he thought was "very revolutionary", a radically new conception of electromagnetic radiation and the energetic properties of light.[34] Unfortunately, in 1905 Einstein did not believe that Planck's theory of blackbody radiation was consistent with this new conception so he based his revolutionary derivation on Wien's flawed distribution law. It wasn't until 1906 that Einstein was able to reconcile Planck's theory of black body radiation with his new corpuscular theory of light. In his 1905 paper, Einstein worried that his corpuscular theory of radiation was so radical that it would

[34] From a letter that Einstein wrote to a friend, Conrad Habicht, in May, 1905 as quoted in: Peter Galison, "Solvay Redivivus," Published in David Gross, Marc Henneaux, Alexander Sevrin, eds. *The Quantum Structure of Space and Time: The Proceedings of the 23rd Solvay Conference on Physics*, (Singapore: World Scientific Publishing, 2007), 5.

be ignored so he tried to show that it was empirically useful by applying it to multiple unresolved problems. Famously among them was the photoelectric effect.

Again, the mythology distorts this episode as well. While it is true that Hertz had observed the photoelectric effect as early as 1887, it was one of his assistants, Philipp Lenard, who in 1902, performed the critical experiments in which he showed that irradiating metal, in a vacuum tube, with ultraviolet light would result in the emission of cathode rays (electrons). Far from a long standing paradox that had the physics community in crisis, the photoelectric effect was somewhat of a novelty in 1905. While the nature of this emission was not well understood, it certainly was not overly troubling. Einstein took advantage of a current problem that had not been sufficiently addressed and applied his theory of corpuscular radiation to help resolve it. In fact, Einstein's contribution was more predictive than explanatory as Lenard and others had yet to measure the energy of the emitted cathode rays (E), as a function of the frequency of the incident light (v). Einstein predicted that eventually the measurement of this relationship would result in his photoelectric effect formula $E = h\,v - P$, where P is the work function of the particular metal. It would take close to a decade for the first experimental verification of Einstein's photoelectric effect formula.

In experiments between 1912 and 1915, Robert Millikan finally made the critical measurements that showed the linear relationship between the emitted electron's kinetic energy and the frequency of the incident light. Millikan's intent was to show that Einstein's theory of corpuscular light was wrong but as his experiments verified that the relationship $E = h\,v - P$ was correct, he was forced to moderate his objections. Even in 1916, while he had accepted Einstein's photoelectric effect formula, Millikan was still apt to deride the hypothesis that explained the phenomenon characterizing it as a “bold, not to say reckless hypothesis.”[35] As it turns out, Millikan was not alone in his reservation about the physical significance of the quanta of light. In a 1913 nomination of Einstein for inclusion into the Prussian Academy of Sciences the nominators (including Planck) stated that “he may sometimes have missed the target in his speculations, as, for example, in his hypothesis of light-quanta”.[36] Although Einstein’s light-quanta hypothesis was tenuous and had its vocal detractors it nevertheless became the focal point of the first Solvay Council in 1911. This new forum for international scientific collaboration served to propel Einstein’s quantum hypotheses and his international reputation.

Ernest Solvay was a wealthy Belgian industrialist, and a chemist by training, who had acquired his fortune by discovering a new method for creating sodium carbonate, or soda ash, from salt and limestone in the 1860s. By the 1890s Solvay was a powerful philanthropist throwing his weight behind social, political, and intellectual causes. He was also engaged in his own theoretical physics research. In 1887 when pluralism in theoretical modeling had alternatively placed the ether, energy, entropy, molecular dynamics, and electrodynamics at the center of various ‘unified’ theoretical frameworks; Solvay created his own throw-back theory (“Gravitique”) with gravity as its focal point.[37] Looking for an outlet to his intellectual interests, Solvay reached out to Walther Nernst in the spring of 1910 and together they decided to arrange an international physics conference. The first Conseil de Physique (later to be known as the first Solvay Council) was inaugurated on October 30, 1911 at the Hotel Metropole in Brussels. Since Lorentz had given his famous address in Rome in 1908, the physics community

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[35] Kragh, *Quantum Generations,* 68.

[36] Max Jammer, *The Conceptual Development of Quantum Mechanics (History of Modern Physics, 1800-1950).* (New York: Tomash Publishers, 1989), 44.

[37] Galison, “Solvay Redivivus,” 2.

was becoming more intrigued with the consequences of the quantum hypothesis so Nernst decided to focus the majority of the Council's discussions (five out seven topics presented) on problems relating to radiation and the quantum hypothesis:

- Planck presented on the laws of heat radiation and the hypothesis of the elementary quantum of action;
- Lorentz presented on the application of the equipartition theorem to radiation theory (this would be used to show the "catastrophic" nature of the Rayleigh-Jeans-Lorentz radiation formula);
- Arnold Sommerfeld presented on the quantum of action and non-periodic molecular phenomena;
- Nernst presented on the application of the quantum theory to physiochemical problems;
- Finally, Einstein presented on his work (beginning in 1907) on the relation of specific heats to the quantum hypothesis.[38]

It is interesting to note that while Nernst was intent on discussing the early state of quantum theory, Planck had been ambivalent when approached about the chosen topics, thinking the time was not yet right for discussions on the quantum hypothesis. Planck's hesitation here makes sense if understood in the context that he was just beginning to accept the notion of quantum discontinuity himself.[39] Nernst on the other hand, although unsure of its soundness, was very excited about opening up a dialog on the new quantum hypothesis:

> I believe that, as regards the development of physics, we can be very happy to have such an original young thinker, a "Boltzmann redivivus"; the same certainty and speed of thought; great boldness in theory, which however cannot harm, since the most intimate contact with experiment is preserved. Einstein's "quantum hypothesis" is probably among the most remarkable thought [constructions] ever; if it is correct, then it indicates completely new paths both for the so-called "physics of the ether" and for all molecular theories; is it false, well, then it will remain for all times "a beautiful memory." (W. Nernst to A. Schuster, Lausanne, 17 March 1910, Royal Society, London)[40]

[38] Kragh, *Quantum Generations*, 72.
[39] Remember that Lorentz had just given his Rome address in 1908 and it wasn't until after this meeting that Planck began the process of accepting the consequences of his introduction of *h*.
[40] From a letter Nernst wrote to Arthur Schuster as quoted in: Diana K. Barkan, "The Witches' Sabbath: The First International Solvay Congress in Physics," *Science in Context* 6, 1 (1993): 62.

So it was, that in 1911 many of the top physicists in Europe, dominated by the German contingent, came together to discuss quantum phenomena.[41] Figure 1.1 is a photograph taken at the 1911 Solvay Council. By the names in the caption it is easy to see that most of the brightest physicists working were in attendance. Einstein, the youngest participant at age 32, was not yet an elite figure in the physics community in the fall of 1911, but his participation in this first Solvay Council left an indelible impression among the brightest scientific minds that he was one of the brightest young minds working in physics. We have seen that from 1900 right up to the Solvay Council in 1911, Planck was somewhat ambivalent about the soundness of the quantum hypothesis; moreover, by Nernst's letter to Schuster in 1910 we know that he was also unsure of the veracity of Einstein's hypothesis, but what may be more surprising is that Einstein, himself, was vacillating on this point. While in the traditional narrative we impose on Einstein this tremendous foresight to see the physical truth of the quantum hypothesis, in 1911 it's not evident that Einstein saw the matter any clearer than Planck, Nernst, or Lorentz. In remarks at the Council, Einstein stated:

> We all agree that the so-called quantum theory of today, although a useful device, is not a theory in the usual sense of the word, in any case not a theory that can be developed coherently at present. On the other hand…classical mechanics…can no longer be considered a sufficient schema for the theoretical representation of all physical phenomena.[42]

[41] See caption in Figure 1.1. No American or other non-European physicists were in attendance. One could argue that Rutherford was a non-European (born in New Zealand and working for years in Montreal, Canada), but by 1911 he was firmly ensconced at Manchester University in England. German science was heavily represented with seven active participants – the next highest was France with four. More significantly, of the five presentations related to the quantum hypothesis, four were given by Germans.

[42] Galison, "Solvay Redivivus," 6.

Even more reflective of his ambivalence, just after Solvay, Einstein sent a letter to Lorentz in which he commented that "[t]he *h*-disease looks ever more hopeless" (Einstein to Lorentz, 23 November 1911.)[43]

Figure 1.1. Photograph of the first conference in 1911 at the Hotel Metropole. Seated (L-R): W. Nernst, M. Brillouin, E. Solvay, H. Lorentz, E. Warburg, J. Perrin, W. Wien, M. Curie, and H. Poincaré. Standing (L-R): R. Goldschmidt, M. Planck, H. Rubens,A. Sommerfeld, F. Lindemann, M. de Broglie, M. Knudsen, F. Hasenöhrl, G. Hostelet, E. Herzen, J.H. Jeans, E. Rutherford, H. Kamerlingh Onnes, A. Einstein and P. Langevin.[44]

By taking a closer look at the context of innovation surrounding Planck's and Einstein's activities regarding blackbody radiation and the establishment of light quanta it becomes clear that the traditional quantum narrative, represented by Table 1.1, is seriously flawed. First and foremost, the 'revolution' did not begin in December of 1900; this seems to be as arbitrary a date as any other. One could easily argue that it began with Boltzmann's pioneering work in the

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[43] Barkan, "The Witches' Sabbath: The First International Solvay Congress in Physics," 68.
[44] Picture from http://www.numericana.com/fame/solvay.htm#hostelet

1860's and 1870s or, that it really did not get on track until after Einstein's quantum hypothesis became a serious discussion point during the 1911 Solvay Council. What is clear is that to understand the quantum revolution one needs to study its seeds in the 19th century and its slow diffusion throughout the first decade of the 20th century. It is undeniable, that the foundations of quantum theory, as established by Planck in 1900, were firmly rooted in the 19th century classical frameworks of thermodynamics, statistical mechanics, and electromagnetism. The evidence also clearly points to the notion that his revolutionary insights were highly contingent on many factors including his German nationality, his academic training under Kirchhoff and Helmholtz, his association and collaboration with the PTR and its industrial interests, his acceptance of theoretical plurality, and his desire to reconcile the irreversibility of entropy with electromagnetic theory. All of these are necessary for a proper understanding of his context of innovation.

1.4- Applying Quantum Principles to Atomic Physics

> Bohr used to introduce his attempts to explain clearly the principles of the quantum theory of the atom with an historical sketch, beginning invariably with the nuclear model proposed by Rutherford. That was sound pedagogy but bad history. – J.L. Heilbron[45]

According to Kuhn,[46] from 1900 to 1910 quantum theory was essentially synonymous with blackbody radiation theory and was almost entirely ignored by the majority of the physics community. After 1910, in large part due to the first Solvay Council, there was a significant increase in the number of physicists publishing on quantum theory and the majority of those

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[45] John L. Heilbron, "Rutherford-Bohr Atom," Am. J. Phys., Vol. 49, No.3, March, 1981: 223.
[46] Kuhn, *Black-Body theory and Quantum Discontinuity, 1894-1912*.1978.

were no longer investigating blackbody radiation theory. So what were physicists using, the still primordial, quantum theory to examine? One answer is atomic physics. Obviously the whole story is more nuanced than that, and one can really say that the application of the quantum hypothesis to things other than blackbody radiation began with Einstein's 1905 paper and his explanation of the photoelectric effect. However, as we noted in the previous section, the diffusion of these ideas was a slow and convoluted process. By 1911 Einstein, Planck, Nernst, Lorentz, Sommerfeld, and Heinrich Rubens had all begun to apply the quantization principles to various fields of study, most significantly to the theories of solids and specific heat capacity, but, as reflected in the attendance of the 1911 Solvay Council this program had been almost entirely continental and more precisely, German.

So far, when discussing the First Solvay Congress of 1911 we have focused on the blackbody radiation law. While this concept was definitely at the heart of the proceedings, there is another way to understand this pivotal international conference; that is, through Nernst's insistence on a thorough reexamination of the classical understanding of the kinetic theory. Planck and Einstein's earlier work on radiation theory had done just that, but there was still much resistance to this line of inquiry, especially from British scientists. Lorentz had already pointed to this tension between the traditional conception of radiation based on classical kinetic theory (Rayliegh-Jeans) and Planck's new radiation law based on a novel quantum kinetic distribution in his 1908 lecture. In preparing for the 1911 Solvay Council Nernst emphasized this same tension:

> It appears that we are currently in the midst of a revolutionary reformulation of the foundation of the hitherto accepted kinetic theory. On the one hand, its consistent elaboration leads to a radiation formula which conflicts with all experience, a situation that no one disputes; on the other hand, the consequences of that same theory include theories about specific heats (constancy of the specific heat of gases with changes of temperature, validity of the Dulong-Petit rule to the lowest temperatures), which are

> completely contradicted by many measurements. As Planck and Einstein especially have shown, these contradictions vanish if one restricts the motion of electrons and atoms about their rest positions (doctrine of energy quanta); but this view is so foreign to the previously used equations of motion of material points that its acceptance must doubtless be accompanied by a wide-ranging reform of our fundamental conceptions.[47]

We should note here, that while there was clear German dominance at the Council (see Figure 1.1), Nernst's initial intent was to have more British involvement particularly with Lord Rayleigh presiding over the proceedings. Rayleigh turned down the offer and other notable British scientists like J.J. Thomson, Joseph Larmor, and Arthur Schuster failed to participate as well, leaving the British contingent substantially depleted. Nernst had hoped that the Council could serve as a melting pot where differences between the opposing conceptions of basic kinematic theory were hashed out. While this particular goal was not realized, the Council was successful enough that Solvay decided to make it a permanent institution. In his closing remarks Nernst was clear about what needed to become the next point of discussion of those participating in the Solvay Council:

> A personal discussion among more or less directly involved researchers may not provide a definitive decision but would open the path to the solution of these questions through a preliminary critique. A great task would be accomplished for the smooth development of atomism if one could achieve clarity as to which of our present kinetic views are in accord with observations and which ones will have to be submitted to a far-reaching revision.[48]

In 1911, atomic theory was far from a new concept. In fact, when discussing atomic theory, many historians like to begin their narrative in Ancient Greece. While the roles of Leucippus and Democritus in the history of modern atomic physics should be nothing more than a curious footnote, it is important to recognize that atomic physics did not begin with

[47] Barkan, "The Witches' Sabbath: The First International Solvay Congress in Physics," 70-71.
[48] Ibid p. 71.

Rutherford's 1911 atomic model as our traditional quantum narrative would suggest.[49] In the first few decades of the 19th century investigations into the constituents of matter were already being performed by natural philosophers and chemists like John Dalton, Joseph-Louis Gay-Lussac, Amedeo Avogadro, and Jöns Jacob Berzelius who were debating the plausibility of various theories based on the fundamental concepts of atoms, corpuscles, molecules, and chemical equivalents. In fact, the terminology and the visualizability in connection with these studies were, for the most part, fluid and highly ambiguous. By the time of the Karlsruhe international conference in September of 1860, there was little in the way of convergence surrounding chemical principles of nomenclature, weights and measures, and underlying mechanical theories. While Karlsruhe was intended to establish some sort of standardization around these topics, decades after this landmark conference, there was still ample disagreement as to the veracity of any postulation of physical atoms.[50] As Nye discusses in her survey of 19th century science towards the middle of the century physics and chemistry, as academic disciplines, were beginning to define themselves in modern terms. Part of this definitional process involved research programs studying atomic theory, beginning to migrate from a sub-discipline of chemistry to that of physics.[51]

As was discussed earlier in connection with the development of radiation theory, the 1840s and 1850s were ripe with speculation on the interconvertibility of forces. From his research on this topic Helmholtz was able to arrive at his celebrated three "laws" of hydrodynamic vortex interactions which he published in 1858.[52] The concept of vortices within

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[49] Once again see Heilbron's point quoted earlier on Bohr's pedagogical telling of quantum history.

[50] Gregoire Wallenborn and Pierre Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* (Basel : Birkhäuser, 1999), 71.

[51] Nye, *Before Big Science*, Chapter 2.

[52] Vyacheslav Meleshko and Hassan Aref, "A Bibliography of Vortex Mechanics 1858 – 1956," in *Advances in applied mechanics, Volume 41*, ed. Hassan Aref and Erik van der Giessen, (London, UK: Elsevier, 2007), 203.

hydrodynamics was not novel but the properties that Helmholtz derived for his vortex filaments were based on the assumption of an ideal fluid with no viscous forces. Among other properties, the resulting vortex filaments could form rings that would be forever stable and would not dissipate over time. It was this characteristic that sparked Kelvin to associate Helmhotz' vortex filaments with atoms physically manifested in an all-pervading ether. In 1867, Kelvin, inspired by having seen a smoke ring demonstration wrote to Helmholtz:

> The absolute permanence of the rotation, and the unchangeable relation you have proved between it and the portion of the fluid once acquiring such motion in a perfect fluid, shows that if there is a perfect fluid all through space, constituting the substance of all matter, a vortex-ring would be as permanent as the solid hard atoms assumed by Lucretius and his followers (and predecessors) to account for the permanent properties of bodies (as gold, lead, etc.) and the differences of their characters. Thus, if two vortex-rings were once created in a perfect fluid, passing through one another like links of a chain, they never could come into collision, or break one another, they would form an indestructible atom; every variety of combinations might exist.[53]

The vortex atomic theory, although not adopted by all, was by far the most extensively developed atomic theory of the 19th century. In fact, while Helmholtz contributed the spark that led Kelvin to develop his atomic theory in 1867, he was never a proponent. The atomic vortex theory became a distinctly British theory owing its progress to mathematical physicists like Kelvin and J.J. Thomson, who picked up on Kelvin's vortex atomic theory and used it to explore aspects of chemical phenomena in the 1880s. Thomson's most notable effort in this regard was his treatise resulting from his 1882 Adam's Prize essay on the stability of various vortex ring configurations.[54] In this study, Thomson combined the hydrodynamic mathematics of Kelvin's vortex rings and the experimental results of Alfred Mayer, an American, who had studied various configurations of equally magnetized needles floating on water. In doing this, Thomson

[53] Helge Kragh, "The Vortex Atom: A Victorian Theory of Everything," Centaurus 44 (2002): 39.
[54] Ibid., 2-3.

became the first physicist to thoroughly study chemical valences and Mendeleev's periodic system using vortex theory.[55]

While many historical narratives have discounted the importance of this early atomic theory, there was initially plenty of support within the scientific community. Thus Maxwell in his famous 1875 Encyclopedia Britannica article on atoms discussed the vortex atomic theory:

> But the greatest recommendation of this theory, from a philosophical point of view, is that its success in explaining phenomena does not depend on the ingenuity with which its contrivers 'save appearances', by introducing first one hypothetical force and then another. When the vortex atom is once set in motion, all its properties are absolutely fixed and determined by the laws of motion of the primitive fluid, which are fully expressed in the fundamental equations . . . [Kelvin's] primitive fluid has no other properties than inertia, invariable density, and perfect mobility, and the method by which the motion of this fluid is to be traced is pure mathematical analysis. The difficulties of this method are enormous, but the glory of surmounting them would be unique.[56]

The atomic vortex theory was surprisingly successful in modeling spectral evidence of the day, and while Kelvin and Thomson tried desperately to show that their atomic theory could accurately account for ideal gas behavior and kinetic theory, there were serious shortcomings when it came to modeling phenomena such as gravitational and magnetic effects.[57] Kelvin himself accepted the fate of his vortex theory in a letter to American physicist Silas Holman:

> I am afraid it is not possible to explain all the properties of matter by the Vortex-atom Theory alone, that is to say, merely by motion of an incompressible fluid; and I have not found it helpful in respect to crystalline configurations, or electrical, chemical, or gravitational forces . . . I wish I could say a great deal more on the subject which has never ceased to interest me. We may expect that the time will come when we shall understand the nature of an atom. With great regret I abandon the idea that a mere configuration of motion suffices.[58]

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[55] Ibid., 30 – 32.
[56] James C. Maxwell, "Atom," in *The Scientific Papers of James Clerk Maxwell.* (2 Vol.) edited by by W.D. Niven, Cambridge 1890, Vol. 2, 470.
[57] Kragh, "The Vortex Atom: A Victorian Theory of Everything," 44.
[58] A letter to Silas Holman, quoted in: Kragh, "The Vortex Atom: A Victorian Theory of Everything," 44.

By 1890 most of its strongest adherents had abandoned the atomic vortex theory, but it was still very much alive in the mathematical methodologies of Cambridge physicists like J.J. Thomson and Joseph Larmor. As is argued in Warwick's treatise on 19th century theory construction at Cambridge University, Thomson and Larmor's generation of Cambridge-educated mathematical physicists were strongly rooted in Maxwell's approach of analogy and temporary theory as a means to an end.[59] From this understanding of their methodological training it should not be a surprise to find that the notion of vortex theory would play a significant role in the development of electron theory and early 20th century atomic theory.[60] Thomson would later state:

> One thing that appealed to me was the analogy between the properties of vortex filaments and those of the lines of electric force introduced by Faraday to represent the electric field . . . In fact, it seemed that even if the vorticity did not suffice to represent matter it might yet give a very useful representation of the electric field.[61]

Although J.J. Thomson is often given credit for "discovering" the electron in 1897, the concept had been in use for some time. In 1874, George Stoney had proposed the "electrine" as a unit of electrical charge while in 1881 Helmholtz independently had suggested that "atoms of electricity" were responsible for Michael Faraday's observed electrolytic phenomena. Although early declarations such as these began hinting at the modern notion of the electron, they were ultimately too abstract, pointing instead to a poorly defined quantity of electricity and not an actual particle. Meanwhile, in Germany there was extensive research being undertaken in the study of cathode rays, most notably by Johan Hittorf, Eugen Goldstein, Hertz, Philipp Lenard,

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[59] Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics,* 286 – 376.
[60] Kragh, "The Vortex Atom: A Victorian Theory of Everything," 69 – 73.
[61] Ibid. p. 70.

and Wilhelm Röntgen. However, by the mid 1890s, while there was a lot of speculation, it had not been determined whether cathode rays were in fact particles, waves, or if they were somehow related to the hypothesized electrons.

The particle notion of the electron was born out of electromagnetic field theory. In the early 1890s Lorentz and Larmor independently worked on electromagnetic field theories that incorporated loose notions of electronic particles. In fact, in 1895, Larmor went so far as to suggest "a molecule [atom] to be made up of, or to involve, a steady configuration of revolving electrons".[62] The following year Pieter Zeeman, who had studied under Lorentz, discovered the effect which bears his name. Zeeman observed a broadening of the spectral lines of sodium under the influence of an external magnetic field. While this phenomenon could not be explained using the current field theories of Lorentz and Larmor, Lorentz quickly realized the significance of Zeeman's discovery and predicted that an external magnetic field acting on vibrating "ions" would cause an actual splitting of the resultant spectral lines based on the e/m (charge-to-mass ratio) of the ions themselves. He calculated the e/m ratio that the "ions" needed to have in order to produce the observed "blurring" and found that it was 1,000 times larger than the ratio for hydrogen. From these experiments and further theoretical calculations many became convinced that the electron was a particle that was very small compared to the hydrogen atom and negatively charged.

J.J. Thomson's famous cathode ray experiments in 1896-1897 were not undertaken to "discover" the electron. In fact, even though Thomson's measured e/m ratio agreed with Zeeman's he did not believe that the "primordial atoms, which we shall for brevity call corpuscles" he had identified as the constituents of the cathode ray were the Zeeman-Lorentz-

[62] Kragh, *Quantum Generations*, 39.

Larmor electrons at all. In his landmark October 1897 paper, Thomson used his experimental results to propose a new atomic theory, claiming that his newly identified corpuscles were a universal subatomic constituent of all matter. It was left to George Fitzgerald to immediately see the connection between the Zeeman-Lorentz-Larmor electrons and Thomson's subatomic corpuscles.

Thomson's first corpuscle atomic model was primarily qualitative:

> I regard the atom as containing a large number of smaller bodies which I will call corpuscles…In the normal atom, this assemblage of corpuscles forms a system which is electrically neutral. Though the individual corpuscles behave like negative ions [charges], yet when they are assembled in a neutral atom the negative effect is balanced by something which causes the space through which the corpuscles are spread to act as if it had a charge of positive electricity equal in amount to the sum of the negative charges on the corpuscles."[63]

By 1904, Thomson had refined his new atomic model from an amorphous cloud of negatively charged "corpuscles", somehow dynamically interacting within the atom while being balanced out by a mysterious positive charge, to a concrete stable electron ring configuration that was based heavily on his past experience with atomic vortex models. Thomson was able to extend his new model by adding stable rings of electrons at varying radii, thus providing an atomic model that could qualitatively account for physical phenomena like emission, beta scattering, photoelectricity, and the normal Zeeman effect. He believed that the electron was the core constituent of all matter and was, by extension, the cause of all physical phenomena from electricity to gravity. Thomson ultimately believed that while the negatively charged electronic corpuscles were definite constituents of atomic theory the amorphous positive charge was simply a byproduct of their configurations:

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[63] Ibid., 45.

> I have always had hopes (not yet realized) of being able to do without positive electrification as a separate entity, and to replace it by some property of the corpuscles…One feels, I think, that the positive electrification will ultimately prove superfluous and it will be possible to get the effects we now attribute to it, from some property of the corpuscles.[64]

Throughout the first decade of the 20th century, Thomson's model was most successful in the eyes of physicists like Lorentz who were interested in creating universal theories based entirely on an electromagnetic worldview. Also, due to the extensive calculations based on his vortex mathematics, Thomson had undertaken to determine the stable corpuscle ring configurations, his model had a veneer of empirical significance. It was, without a doubt, the most extensive atomic theory of its time. Thomson's identification of the electron as a core constituent of all matter had sparked many atomic models throughout this period. Oliver Lodge published his 1906 book *Electrons* in which he described the various competing models of the time:

- Lord Kelvin had developed an atomic model based on stable rings of "electrions" similar to Thomson's but less extensive.
- Lenard had his own model which was based on rapidly rotating dynamids.
- Lord Rayleigh and James Jeans, in an attempt to account for spectral frequencies, developed models with both positively and negatively charged electrons.
- Jean Perrin and later Hantaro Nagaoka (with his so called "Saturnian" atomic model) produced astronomically inspired models where rings of electrons "orbited" a central nucleus of positive charge.[65]

All of these models were rough highly qualitative sketches which for one reason or another did not rise to overtake Thomson's hold on atomic theory in the first decade of the 20th century.

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[64] Per F. Dahl, *Flash of the Cathode Rays: A history of J.J. Thomson's electron*, (Bristol, UK: IOP Publishing, 1997), 324.
[65] Kragh, *Quantum Generations*, 49 – 50.

While shortcomings of Thomson's model were identified early on, it was not discarded by the physics community until well after the end of the decade, when anomalies with established empirical evidence had accumulated to make his theories untenable. Thomson had assumed that the crucial parts of his model were the electronic corpuscles, which led to the various physical characteristics of all atoms. In order to support this theory he had been forced to assume that the stable rings within the atom had thousands of corpuscles even for the lightest of elements. How else could he account for the atomic mass and the thousands of lines of emission spectra? If each line of spectra came from a separate vibrating corpuscle, as Thomson thought, then there must be thousands of these within each atom.

In 1904 there was no way to know how many electrons resided in a real atom so Thomson's assumptions estimating this number in the thousands were taken as a real possibility, however, by the end of the decade experiments performed within his own laboratory at the Cavendish had proven that Thomson's modeling of electrons being in the thousands was off by almost three orders of magnitude. X-ray and β-ray scattering experiments showed that instead of the electrons contributing in large part to the element's atomic mass, they were directly responsible for its atomic number. Using his atomic theory, Thomson had calculated that the number of electrons within an atom would vary on a scale that was 1000A, where A was the observed atomic weight, the scattering experiments gave a result of 2A![66]

Our quantum mythology tells us that Hans Geiger and Ernest Marsden, under Ernest Rutherford's direction at the University of Manchester, performed a series of alpha scattering experiments in 1909 that unequivocally showed the nuclear structure of the atom. Moreover, the surprising experimental results led Rutherford to develop his nuclear atomic model which

[66] Heilbron, "Rutherford-Bohr Atom," 224.

immediately supplanted Thomson's atomic model in 1911. In reality, various nuclear models had previously been proposed by Perrin (1901), Nagaoka (1904), and J.W. Nicholson (1911), so that was nothing new. We also know that Rutherford, who was a student of Thomson's at the Cavendish in the late 1890s, was actually a proponent of Thomson's atomic theory right through 1910 and would not have been as 'shocked' by the scattering results as he claimed to be later in life. In 1936, towards the end of his life, Rutherford remarked that when he heard about the Geiger-Marsden results he considered it "the most incredible event that has ever happened to me in my life…almost as incredible as if you fired a fifteen-inch shell at a piece of tissue paper and it came back and hit you."[67] Geiger first reported the seemingly anomalous scattering results to Rutherford in 1908 and teamed with Marsden to further refine their empirical results. By 1910 Rutherford was intrigued enough by the anomalous scattering to begin his own investigation. He first tried to compare his student's results with the Thomson atomic model's handling of beta scattering and soon realized that this would be fruitless because while Thomson's model for multiple beta scattering agreed reasonably well with experiment it had no hope of explaining the anomalous alpha scattering results. In his studies of alpha particles, Rutherford had determined that the alpha particle was a point-like particle of positive charge with a mass similar to that of a helium atom. Therefore, for some of the alpha particles incident on a thin metal foil to have scattered at angles greater than 90 degrees, would necessitate a large concentration of positive charge. Thomson's atomic theory allowed only for a uniform distribution of positive charge.

As a result, by 1911, Rutherford was forced to propose an atomic model which consisted of a massive charge Ze (where Z is the atomic number and e is the unit of charge) at its center surrounded by a homogenous cloud of electrons. His landmark paper titled: "The Scattering of

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[67] Kragh, *Quantum Generations,* 51.

α and β Particles by Matter and the Structure of the Atom" was focused more on deriving the correct scattering formula than describing the exact model of an atom. As such, Rutherford's new atomic model initially had no decisive claim about atomic electron configurations. While this paper has become an indispensible part of the quantum mythology as a landmark achievement, in its day it was mostly ignored by the greater physics community. It was, after all, primarily a moderately successful theory of alpha scattering from which Rutherford had extrapolated a nuclear model of the atom that was seen as incomplete, unoriginal, and too ambiguous. Rutherford himself was not exactly trumpeting his new nuclear atomic theory, at the 1911 Solvay conference (which he attended) there was no mention of it and in his 1913 textbook titled *Radioactive Substances and their Radiations* there was only a small reference to it. Rather than place it in the quantum mythology as a landmark atomic model, it may be more accurate to represent Rutherford's nuclear atomic model as a tenuous transitional stepping stone between Thomson's and Bohr's.[68]

Following the enormous success of the first Solvay Council in the fall of 1911, Lorentz convinced Ernest Solvay to make a permanent institute that would sponsor periodic Councils moving forward. On May 1, 1912 Solvay heavily endowed a newly formed foundation (International Institute of Physics) in his native Belgium.[69] From October 27 to 31, 1913 the second Solvay Physics Council was convened in Brussels. As the topic of discussion had been made *The Structure of Matter* the composition of participants was significantly more balanced between Germany and Britain than the first Council had been. As we noted earlier, the first Solvay Council in 1911 had been heavily dominated by the German contingent, in part this was

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[68] Heilbron, "Rutherford-Bohr Atom," & Philip Stehle, *Order, chaos, order: the transition from classical to quantum physics,* (Oxford: Oxford University Press, 1994).
[69] Jagdish Mehra. *The Solvay Conferences on Physics: aspects of the development of physics since 1911*, (Boston, MA: D. Reidel Pub., 1975), 75.

due to the topics chosen - *Radiation Theory and Quanta.* However, at the second Council, in 1913, as the topic chosen was more central to research being undertaken in England, British physicists had equal standing with the German contingent.[70]

Thomson's atomic model had evolved for over fifteen years and it had developed a complex and refined mathematics that, while not exactly accounting for all empirical evidence, was at least stable and certainly the best option available in 1913. Thus, even though Rutherford's nuclear atomic model was more than two years old and Niels Bohr had already published his first paper applying Planck's quantization conditions to electronic states within the Rutherford model the first and most prominent presentation on atomic modeling came from J.J. Thomson himself. In fact, the only mention of the Rutherford atomic model during the entire proceedings came from Rutherford during a discussion point after Thomson's presentation. In his talk, Thomson attempted to explain experimentally verified phenomena like the photoelectric effect, X-ray emission, electronic valence structure, and molecule formation using his model. Serious objections were raised by several participants including Madame Curie, Lorentz, and Rutherford. Rutherford used this opportunity to explain that according to alpha scattering experiments carried out in his Manchester laboratory by Geiger and Marsden the only possible atomic model had to be a nuclear one.[71]

The Rutherford-Bohr atomic model in 1913 was not overly compelling, complete, nor novel. In 1911, while Rutherford was working hard to interpret Geiger and Marsden's experimental results, John W. Nicholson, a mathematical physicist working at the Cavendish, was busy reviving Nagaoka's 1904 "Saturnian" nuclear model and extending it to try and

[70] Ibid., 74 – 92. German contingent had seven (not including Einstein who was counted as Swiss), while the British had six. There were two formal presentations by Germans (Voigt and Gruneisen) and three by the British (J.J. Thomson, W.H. Bragg, W. Barlow + W.J. Pope).
[71] Wallenborn and Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* 119 – 121.

account for the varying atomic weights of the fundamental elements and their observed line spectra. Assuming that the nuclear core contained the positive charge and that the electrons somehow orbited this nucleus in stable configurations, Nicholson's atomic model was based entirely on classical mechanics and electromagnetism. However, as we noted earlier, in 1911 the notion that one could cobble together a semi-classical theory based on classical foundations, yet somehow incorporating Planck's quantization condition was beginning to percolate within the physics community. Curiously enough, Nicholson did just that, applying Planck's quantization condition in order to account for the line spectra. This naturally led him to the conclusion that angular momentum had to be a multiple of a constant in the form $L = nh/2\pi$. Unfortunately, although he continued to develop this model for several years, he was too steeped in the classical frameworks to take the extra step that Bohr eventually took in order to account for the mechanisms of emission and absorption.[72]

Throughout 1912 and 1913 Niels Bohr, a young Danish physicist who was an ardent fan of all things British worked in Manchester with Rutherford on extending the nuclear atomic model that had been inferred from the Geiger and Marsden scattering experiments. Even though he did his research under Rutherford's supervision, it seems as though J.J. Thomson had a decisive influence on Bohr's research methodology. Bohr, along with others in the physics community, realized that along with the implication that there was a central, positively charged nucleus in all atoms; recent scattering experiments had refined the Cavendish estimates of the number of electrons within atoms from 2A to 1/2A. Applying this to the periodic table of elements meant that the number of electrons could now be definitely matched to the atomic number Z. Beginning with a simple nuclear model and knowing the number of electrons that comprised it, Bohr attempted to retrace Thomson's theoretical research program. Just like

[72] Kragh, *Quantum Generations,* 50.

Thomson, he was interested primarily in deducing the stable configurations of electrons within the atom and how atoms might form molecules. Bohr tried to approach these problems from a purely classical framework but was initially hampered by the lack of stability of the orbiting electronic states. Consequently, he assumed a long-standing 19th century tradition exemplified by the likes of Maxwell, Kelvin, and Thomson and allowed himself to see his model not as a literal representation of reality, but more as a heuristic approximation to reality. With mechanical stability temporarily on the back burner, Bohr proceeded to extend his model so as to match and then surpass Thomson's atomic program as an empirically useful model. By 1913, Bohr had developed his approach enough that he could explain electron ring configurations and roughly match them to empirical evidence from chemical and physical research on valence structure and diatomic molecule formations.[73]

While it is common to associate Bohr's innovation of 1913 with his first paper on the spectrum of hydrogen, titled "On the Constitution of Atoms and Molecules", this was not what he had worked on while working with Rutherford in Manchester. Just as Thomson had ignored spectrum analysis in his research program, Bohr too initially chose to avoid this discussion. Upon his return to Copenhagen in 1913, Bohr learned about Nicholson's work on the nuclear atom and by extension Johann Balmer's empirically deduced 1885 formula for calculating hydrogen atoms' spectral emission lines and immediately understood their significance.[74] He quickly expanded his own atomic theory to allow for the derivation of the Balmer formula, higher stationary states, and quantization conditions proportional to Planck's constant. Historically, we recognize his first paper of 1913 dealing with the hydrogen spectrum as being

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[73] Heilbron, "Rutherford-Bohr Atom," 226.

[74] The first application of Planck's quantum conditions to atomic theory was by Erich Haas, an Austrian mathematical physicist who used Thomson's model and attempted to combine it with Planck's theory. Using assumptions based in classical mechanics he obtained mathematical relationships with both Planck's constant and the Rydberg constant. See Kragh, *Quantum Generations,* 48.

seminal, but his last two papers published in *Philosophical Magazine* as Part II and Part III dealt with higher order electron configurations and diatomic molecular formations and were, chronologically, the basis of his extensive research retracing Thomson's investigations.[75]

The Rutherford-Bohr atomic model was far from an instant success. It was essentially ignored at the 1913 Solvay Council whose central theme was *The Structure of Matter* and really did not gain general acceptance until almost a decade had passed. The main stumbling blocks for general acceptance were rooted in Bohr's two famous assumptions:

> (1) That the dynamical equilibrium of the systems in the stationary states can be discussed by help of the ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis.
>
> (2) That the latter is followed by the emission of a homogeneous radiation, for which the relation between the frequency and the amount of energy emitted is the one given by Planck's theory.[76]

These initial assumptions, by 1918 had led directly to Bohr's two fundamental quantum postulates. First, the atomic electrons orbit the nucleus in mechanically stable *stationary states* of special radii that allow for conservation of momentum ($L = mvr = nh/2\pi$) yet deviate from classical electrodynamic theory as they do not radiate. Second, that the energy and frequency of emitted radiation is associated with a *quantum jump* of an electron from a stationary state of higher energy to one of lower energy ($E_2 - E_1 = h\nu = hc/\lambda$).[77] While deviations from electrodynamic theory were beginning to gain wider acceptance (see discussion of first Solvay Council), the notions of a hybrid model relying on 'stationary states' that retained classical mechanical stability yet simultaneously ignored classical electrodynamic restrictions and the unintuitive

[75] Heilbron, "Rutherford-Bohr Atom," 226 – 227 and Kragh, *Quantum Generations,* 54.
[76] Niels Bohr, "On the Constitution of Atoms and Molecules," Philos. Mag. 26, 1, (1913): 7.
[77] Oliver Darrigol, *From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory,* (Berkeley, CA: University of California Press, 1992), 86 and 146.

introduction of 'quantum jumps' were too much for the majority of the physics community to initially accept.[78]

From 1913 to 1916 Bohr worked to extend and apply his new theory by reconciling it with empirical evidence. While spectral lines corresponding with the Balmer and Paschen series n = 2 and n = 3 had been observed Bohr predicted the, as yet unobserved, lines n = 1 and n > 4 in the extreme ultra-violet and the extreme infra-red. There was cause to be hopeful about the power of Bohr's new theory when in 1914 Theodore Lyman observed spectral lines corresponding to n = 1.[79] Further confirmation came from the work of British physicist Henry Moseley who based an extensive study of X-ray emissions on Bohr's new model and found that it was highly successful in reproducing the periodic table of elements. Finally, we will mention the famous experimental verification of stationary states by James Franck and Gustav Hertz in Berlin, Germany. In experiments carried out between 1913 and 1916, Franck and Hertz set out to study ionization potentials by bombarding mercury vapor with electrons. In 1915, Bohr argued that the two experimentalists were misinterpreting their results and instead of measuring ionization potentials they were actually measuring the energy difference between energy states in mercury atoms. Thanks in part to Bohr's intervention Franck and Hertz received the 1925 Nobel Prize and their experiment is reproduced annually in undergraduate laboratories across the world.[80]

One could argue that due to the temporary derailment of science research in many countries brought upon by the First World War, Bohr's atomic model and his novel application of the quantum theory, were given time to germinate without being exposed to widespread

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[78] Heilbron, "Rutherford-Bohr Atom," 227.
[79] The n = 4 and n = 5 lines would be observed by Brackett (1922) and Pfund (1924) respectively. Wallenborn and Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* 131.
[80] Kragh, *Quantum Generations,* 57.

attention and critique. Many able-bodied physicists were pressed into military service and forced out of their academic environments.[81] The infamous "Appeal to the civilized world", was published in October of 1914 infusing science with a highly volatile political edge. The Appeal was a declaration signed by 93 of the most respected German intellectuals, including Planck, Röntgen, Nernst, and Ostwald defending Germany's position in the war and denying many of the accusations of brutality and barbarism perpetrated by the military. This Appeal would have serious ramifications for post-war scientific collaboration. The Solvay Council that was scheduled for 1916 was cancelled and the next one would not be held until the spring of 1921. However, in 1914 there were some physicists who were able to avoid the war directly, allowing them to continue their research efforts. In Germany, Sommerfeld and Einstein were too old to serve in the military and were allowed to continue their research programs in Munich and Berlin respectively, while Ehrenfest and Bohr were living in neutral countries and could work unimpeded. Their research was crucial in nurturing the fledgling quantum theory along during the First World War.[82]

At the outbreak of the war, Sommerfeld had arranged for two "enemy aliens" to stay and assist him in his research; Paul S. Epstein and Wojciech (Adalbert) Rubinowicz became integral in Sommerfeld's program to extend and formalize Bohr's quantum atomic model.[83] From 1915 to 1916 in a series of papers they were able to apply formulations from classical mechanics that were used in studying multiperiodic astronomical systems to Bohr's atom and subsequently account for the fine structure and the Stark and Zeeman splittings observed in the

[81] Moseley for instance was killed during the Battle of Gallipoli in August 1915. Mehra, *The Solvay Conferences on Physics,* 77.
[82] Darrigol, *From c-Numbers to q-Numbers,* 145 – 149.
[83] Heilbron, "Rutherford-Bohr Atom," 227.

hydrogen spectrum.[84] The analytical calculations used to arrive at these results were essentially a revival of the 19th century Hamilton-Jacobi first-order nonlinear partial differential equation. Moreover, it was shown that the *action variables* derived from these equations were equivalent to the quantum rules which were by definition adiabatically invariant for all stationary states. In Copenhagen, Bohr was not alone. In 1916, he had taken in a young Dutch doctoral student, Hendrik Kramers, who became instrumental in helping Bohr develop his atomic theory. Also in 1916, Paul Ehrenfest published a review of the extensive work he had done on adiabatic invariance since 1905; Bohr would subsequently find this work invaluable in developing his extensions to quantum theory.[85]

Meanwhile, during 1916 and 1917 in Berlin, Einstein was extending Bohr's model in an entirely different direction. Bohr had thought about radiation as a means to an end. He wanted to use the observed spectra as a way to substantiate the atomic model he had developed. Einstein, on the other hand, was deeply interested in the actual processes which govern radiation emission and absorption. He assumed that if the electronic transitions between Bohr's stationary states were discontinuous that they would need to be described by transition probabilities. Einstein developed the notion of three independent processes surrounding atom-radiation interaction: spontaneous emission (when no radiation is present), stimulated emission, and absorption. He set out to derive the blackbody radiation law and Bohr's quantum frequency rule by assuming a relationship between the coefficients of absorption and emission that would statistically even out and allow for an equilibrium state. While Bohr embraced the

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[84] Darrigol, *From c-Numbers to q-Numbers,* 145. Simultaneously yet independently from Epstein, Karl Schwarzschild published a paper accounting for the Stark effect using quantum theory. The difference was that Schwarzschild was publishing from the German-Russian front! Although his quantum paper was seminal, he is most famous for his other two publications from that same year in which he gave an exact solution to Einstein's general gravitational equations and laid the foundations for the study of black holes. In 1916, Schwarzschild died from complications due to an illness he contracted on the front.

[85] Marta Jordi and Enric Perez, "The Ehrenfest Adiabatic Hypothesis and the Old Quantum Theory, Before Bohr," (Preprint of talk at conference HQ-1 July, 2007), 1 – 2.

notion of these different atomic transitions leading to and arising from atomic interactions with an abstract notion of light quanta, he was unwilling to follow Einstein's reasoning that led the great German physicist to pronounce the real nature of these electromagnetic corpuscular radiations with corresponding energy and momentum. In fact, to Bohr's dismay, Einstein proposed that when an atom emits or absorbs a quantum there is a corresponding momentum transfer and with a directional recoil.[86]

When Bohr realized that physicists in Germany and Holland were using his model as a foundation on which to build extensive theories of matter-radiation interaction he became inspired to build off of their work. He did this by developing two core principles which he thought would guide future innovations in quantum theory. In the great 19th century tradition of transitional theory building, Bohr saw the quantum program as tentative and subject to future extension and modification. He believed that he had already established a strong foundation for the theory based on his two core postulates and adding the two principles would ensure that future work in quantum physics would be well directed.

The first of these principles was the *correspondence principle*. By 1918 Bohr had further developed his model and quantum theory by introducing perturbative calculation techniques. In the case of a system that has been mechanically perturbed, Sommerfeld's approach depended on exact solutions to the differential equations governing said system (which is generally impossible). However, Bohr's perturbation approach was very different. He relied on calculating only approximations of the perturbed motion and then applying his correspondence principle to attain the actual perturbed spectrum. The correspondence principle, in its most basic sense, allows for a statistical asymptotic equivalence between a quantum system and its

[86] Darrigol, *From c-Numbers to q-Numbers,* 119 – 120.

corresponding classical system. While this principle was not fully developed by Bohr until 1920-1921, as early as 1917 he was already beginning to use it in calculations. Historically, we tend to downplay the significance of the correspondence principle by alluding to its more meta-physical implications but early on it was, for Bohr, firmly rooted in celestial mechanics techniques of perturbation and approximation. The second principle was heavily based on Ehrenfest's adiabatic hypothesis and renamed by Bohr as the *principle of mechanical transformability*. Essentially, Bohr wanted there to be continuity between discrete stationary states. In terms of atomic systems, Bohr envisioned the connection between stationary states as a continuous deformation of the atomic system itself. Therefore all stationary states could be "compared" in some abstract sense because they were part of a deformed continuous whole.[87]

The third Solvay Council had been delayed since 1916 due to the war and would now be held in the spring of 1921.[88] This same year Bohr published two letters in *Nature* which announced his new atomic model. In 1913 Bohr's first attempt at an atomic model had relied on electron ring configurations thereby basing itself heavily on Thomson's 1904, vortex inspired, ring model. In fact, as we noted earlier, while Bohr's 1913 atomic model was more successful at explaining observed phenomena like the Stark and normal Zeeman effects, it was never as electrodynamically and mechanically stable as Thomson's. Thomson had used his experience with hydrodynamic vortex theory to build a stable model, for Bohr the primary objective was to retrace Thomson's atomic program relying on a nuclear model that was more successful at explaining empirical results. By 1920, however, Bohr's original model was running into serious problems. Throughout the war years, Sommerfeld and others had worked independently of Bohr to help extend his theory (recognized as the Bohr-Sommerfeld theory) from simple circular

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[87] Ibid., 147 - 149.
[88] Wallenborn and Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* 30.

orbits with one quantum number n, that selected permissible radii, to a more complex, astronomically inspired, theory that included elliptical multiperiodic orbits with multiple selection rules. By 1920, it was clear that this program had run its course. While the Bohr-Sommerfeld theory was highly successful as an explanatory model, it failed as a realistic atomic portrait in that it was neither mechanically nor electrodynamically stable. Competing atomic models began emerging that did away with all orbital motion, instead relying entirely on static atomic models. From the summer of 1920 to the early months of 1921 Bohr worked out a new atomic model that would retain orbital motion but would replace the flat ring orbits that his first model had relied on.[89]

Basing his work on the two quantum postulates and on the two principles he had developed, Bohr embarked on a theoretical construction project to build atoms with precise electron configurations in groups and subgroups. He would allow for groupings to be defined by two quantum numbers:

- n - the principle quantum number denoting energy levels and the acceptable corresponding radii. Where n = 1,2,3,…
- k - the azimuthal quantum number corresponding to the multiperiodic precession dynamics. Where the selection rule for k = ± 1.[90]

On March 24, 1921 Bohr published a letter in *Nature* where he presented examples of his newly constructed atoms and their corresponding electron configurations. He focused on a group of elements which he referred to as “inactive gasses” (noble gases).

> For the atoms of these elements we must expect the constitutions indicated by the following symbols:
>
> Helium (2_1), Krypton ($2_1 8_2 18_3 8_2$),

[89] Darrigol, *From c-Numbers to q-Numbers,* 171 – 172.
[90] Ibid., 154.

> Neon ($2_1 8_2$), Xenon ($2_1 8_2 18_3 18_3 8_2$),
> Argon ($2_1 8_2 8_2$), Niton ($2_1 8_2 18_3 32_4 18_3 8_2$)
>
> where the large figures denote the number of electrons in the groups starting from the innermost one, and the small figures the total number of quanta characterizing the orbits' of electrons with each group. [N_n where N = number of electrons in the group and n = principle quantum number.]
>
> These configurations are distinguished by an inherent stability in the sense that it is especially difficult to remove any of the electrons from such atoms so as to form positive ions, and that there will be no tendency for an electron to attach itself to the atom and to form a negative ion.[91]

Within a week of his "Atomic Structure" letter being published in *Nature*, Bohr was scheduled to present a paper on "The application of the theory of quanta to atomic problems" at the third Solvay Council. The other Solvay participants were eagerly anticipating his presentation since they were well aware of his recent innovations. After all, the slated topic for the 1921 Solvay Council was *Atoms and Electrons* so the paper by Bohr was set to be a cornerstone of the proceedings. Unfortunately, Bohr became ill and was unable to attend the Council so Ehrenfest read Bohr's paper to the gathered physicists. Ehrenfest also presented a complementary report summarizing Bohr's correspondence principle.

The participants in the 1921 Solvay Council celebrated the great success of what they referred to as the "Rutherford-Bohr" atomic model. Since 1913, Rutherford had continued his work studying scattering phenomena and made significant advances in understanding the nature of the atomic nucleus. It is interesting to note that Rutherford presented the first paper at the Council on the "Structure of the Atom" and spent the vast majority of his talk focusing on the development of nuclear physics, introducing protons and the notion of isotopes. He barely mentioned Bohr's work, and when he did it was a comment on the successes in explaining

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[91] Niels Bohr, "Atomic Structure," *Nature* 106: (1921): 104–107.

spectral phenomena.[92] Participants at Solvay were generally supportive of Bohr's approach to the integration of the quantum principles with atomic theory. While they realized that his theory was not a definitive one they certainly saw it as the best available option. Thus, Maurice de Broglie stated: "In spite of everything it still lacks, the Rutherford-Bohr model is without a doubt the best adapted to the way we are all currently thinking."[93] After, Bohr's paper had been presented by Ehrenfest, the Austrian-Dutch physicist concluded his own presentation of the correspondence principle with a declaration of the tentative nature of quantum theory.

> The most profound significance of Bohr's efforts on correspondence is that they provisionally seem to bring us closer to this future theory we are all waiting for, to remove the problems we face as we attempt to treat radiation phenomena both in a classical manner and using quantum methods. For this reason, and with a view to eventually finding a theory which can be automatically applied as far as possible, we should not be too quick to cast the condition of correspondence in stone. At this stage it is still changeable and tentative.[94]

Once the effects of Bohr's new atomic theory which he had presented both in his March letter to *Nature* and at the Solvay Council in April began to sink in, the physics community realized the power behind Bohr's two postulates and his corresponding principles. Sommerfeld who had not initially been a proponent of Bohr's perturbation theory or his correspondence principle declared in the third edition of his *Atombau* treatise in 1922:

> We have to recognize the complete superiority of the correspondence principle in the matter of atomic models. For here Bohr seems to have succeeded, by using classical mechanics and electrodynamics, in arriving at definite statements about the periodic system and the atomic shells which would have been inaccessible by any other route.[95]

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[92] Mehra. *The Solvay Conferences on Physics,* 104 – 105. I wonder if anti-German sentiment restricted those participating from recognizing the extent of Sommerfeld's contributions to the theory.
[93] Ibid. p. 104.
[94] Wallenborn and Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* 130.
[95] Darrigol, *From c-Numbers to q-Numbers,* 158.

Thus the stage was set for Bohr to be welcomed triumphantly at Göttingen in what came to be known as the "Bohr Festival". In a series of lectures during June 1922, Bohr delivered what was to date the most thorough discussion on quantum theory and its applications to atomic spectroscopic phenomena. Almost forty years later Friedrich Hund, who had attended the "festival" as a student recalled: "…we felt that it was something very important. The glamour that surrounded this event cannot be communicated in words today; for us it was as brilliant as the Händel-Festival of those days in Göttingen."[96]

The understanding of atomic phenomena physicists' ability to explain and predict empirical results had grown tremendously over the preceding three decades, thanks in large part to the developments of the nascent quantum theory. While the Bohr-Sommerfeld theory was limited and eventually was replaced by more powerful approaches one should not make the arbitrary distinction of there being an old versus a new quantum theory. This periodization has been traditionally overemphasized and forced upon the historical narrative, thereby contributing to the quantum mythology of Table 1.1. In this chapter we have argued against the distortions in this traditional narrative. As is evidenced by Ehrenfest's comments at the close of the 1921 Solvay Council, the Bohr-Sommerfeld theory was a self acknowledged tentative theory waiting for inevitable innovations that would certainly transform it down the road. Identifying the "new quantum theory" which is usually given its "birth" in 1925 with Werner Heisenberg's development of his matrix mechanics seems to me quite arbitrary.

As we will see in later chapters, there were important innovations such as de Broglie's wave mechanics and the Born-Kramers-Slater (BKS) theory that were absolutely critical to Heisenberg's work. In order to understand the complex dialog between physicists that led to

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[96] Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Theory: Volume 1.* (New York, NY: Springer, 1982), 345.

the innovative period which took place in the mid to late 1920s and has been memorialized as the quantum revolution we must first understand the larger context of the international physics community. In particular, we need to understand post World War I Europe and how external forces such as social, political, and economic factors contributed to the structure and dynamics of the physics community. This analysis will allow us to better frame and explain the complex internal scientific dialog that led to the development and the subsequent reception of de Broglie's alternate wave mechanical research program.

Chapter 2: Quantum Divides- The Establishment of Quantum Schools in Post World War I Europe

> *Science has no native country, or rather: the country of science includes the entire of humanity.. . But even if science does not have any native country the scientist should particularly occupy himself with that which brings honour [sic] to his country. In every great scientist you will always find a great patriot. - Louis Pasteur 1884*[97]

2.1- Introduction

In chapter one, we laid the foundations of our story by exploring the roots and the early days of the slow rising quantum physics. By the early 1920s the Bohr-Sommerfeld quantum theory had shown its usefulness in explaining important empirical results but it had also shown its limitations. It would take a dramatic reformulation of quantum theory, the development of multiple simultaneous versions of quantum mechanics between 1923 and 1927, and a complex interpretation debate to establish a coherent notion of quantum physics. Our ultimate goal of studying the alternate formulations and interpretations of quantum theory necessitates us having a thorough understanding of the dynamics within the international physics community during this revolutionary period.

In order to capture the most comprehensive context for these alternate quantum formulations and interpretations we must first engage in various interdependent historical lines of inquiry. Because science is a human enterprise taking place within specific socio-political and economic environments it would be naïve to want to write an historical narrative based solely on the published scientific record. In the historiography of science this point was driven home and reinforced by the internalist-externalist debate, of which the Forman thesis became a flash

[97] Helge Kragh, "Anatomy of a Priority Conflict: The Case of Element 72," *Centaurus* Volume 23, Issue 4, (June, 1980): 293.

point in the 1970s and early 1980s.[98] For some scholars, this debate has not been sufficiently resolved and while most agree that a narrative based purely on either internalist or externalist approaches is flawed, we are far from reaching consensus on what the "correct" balance looks like in a hybrid narrative.

In the following chapter we will explore the establishment of the most significant quantum schools within the turbulence of post World War I Europe. As a result, we begin to set the context for the complexities of the academic network which was critical to how the quantum revolution evolved in the 1920s. There are periods in the history of science when the internal and academic dialog of scientists seems to dominate the dynamics of innovation, thereby overshadowing all other historical factors. This period is certainly not among them. World War I left Europe in complete turmoil with no institutions, including the physics communities, immune to the post war turbulent effects of nationalism. While it would be excessive to place these nationalistic tendencies as the single most important factor contributing to the overall context of quantum innovation, it was a formative contributor to historical developments, and one that has not been adequately explored in the literature.

2.2- International Physics after the First World War

> Moved by insane delusion and reckless self-regard, the German people overturned the foundations on which we all lived and built. But the spokesmen of the French and British peoples have run the risk of completing the ruin, which Germany began, by a Peace which, if it is carried into effect, must impair yet further, when it might have restored,

[98] Internalism will be identified with approaches to the history of science that focus primarily on the scientific dialog as it pertains to the development of the science itself. Typically, Internalist narratives focus on the published and unpublished papers of the scientists themselves and their correspondence. On the other hand Externalist narratives tend to focus more on the socio-cultural forces that affect the development of science. Unfortunately, this boundary is not always well defined and limiting a study to one or the other only gives a partial picture, so in this thesis we have taken the approach that we will examine both.

> the delicate, complicated organization, already shaken and broken by war, through which alone the European peoples can employ themselves and live. (…) In this lies the destructive significance of the Peace of Paris. If the European Civil War is to end with France and Italy abusing their momentary victorious power to destroy Germany and Austria-Hungary now prostrate, they invite their own destruction also, being so deeply and inextricably intertwined with their victims by hidden psychic and economic bonds. - John Maynard Keynes (1920)[99]

John Maynard Keynes was one of the foremost economists of the 20th century and a key member of the British contingent at the Paris Peace Conference of 1919 until he resigned and left Paris in protest before the Treaty of Versailles was signed. In this quote, Keynes was foreshadowing the calamity that he believed would arise as a direct result of the "European Civil War" that was surely not yet over and the belittling conditions of the tentative peace accord that the allies were advocating. The effects of the Great War and more significantly the "Paris Peace" that followed it were felt throughout every corner of Europe and in every social, cultural, political, economic, and scientific sense. In order to build an adequate context describing the ongoing quantum theory research in the 1920s one needs to first comprehend the full extent of the impact that the First World War had on the international physics community. The effects of international tensions had far reaching ramifications that set the stage for developments in quantum theory throughout the 1920s and in some senses, well beyond.

While historians may not agree as to which of the two factors, the treaty of Versailles or the Great Depression, was more significant in the rise of National Socialism in Germany during the 1930s there is no doubt that in the wake of World War I, the Paris Peace Conference left Germany with a substantially crippled economy and with significant social and political unrest.[100]

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[99] John M. Keynes, *The Economic Consequences of Peace,* (New York: Harcourt, Brace and Howe, 1920), Introduction (Chapter 1).

[100] There are historians who argue that the treaty of Versailles was actually advantageous for Germany – as an example see Gerhard Weinberg's *A World at Arms* (2005). There may be truth in this argument, as

In large part due to its industrial prowess, pre-war Germany had begun to dominate international science. As we saw in the last chapter, they were by far the most represented nation at the 1911 Solvay Council and were, along with Britain, the clear leaders at the 1913 Council. After the war, with its economy in tatters, and its citizenship in need of something to rally behind, Germany was determined to show the world that it was still a cultural and scientific force. Planck made this point repeatedly, but maybe most poignantly, in his address to the Prussian Academy of Sciences in November, 1918:

> If our enemy has taken from our fatherland all defense and power, if severe domestic crises have broken in upon us and perhaps still more severe crises stand before us, there is one thing which no foreign or domestic enemy has yet taken from us: that is the position which German science occupies in the world. Moreover, it is the mission of our academy above all, as the most distinguished scientific agency of the state, to maintain this position, and if the need should arise, to defend it with every available means.[101]

Planck was unequivocal in his address; the post-war international scientific community was not above elevating nationalism and sparking patriotic pride.

As the Treatise of Versailles and the subsequent formation of the League of Nations were isolating and debilitating Germany both politically and economically, the creation of the International Research Council (IRC) in 1919 was attempting to achieve the same effect scientifically. In 1925, the Dutch, Norwegian, and Danish delegations, through Lorentz, petitioned the IRC to end the exclusion of Germany, Austria, Hungary, and Bulgaria but were initially rejected. With the Locarno Treaties' ratification in 1926 and Germany's inclusion into the League of Nations, the IRC was finally forced to invite the excluded nations into the fold. In

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the treaty did not go far enough dismantling the German federalist state and therefore allowed it to remain arguably the largest and most dominant political entity in Europe. Even so, it's difficult to deny that the Treatise of Versailles was a bitter pill for the Germans to swallow and a blow to the German national psyche.

[101] Paul Forman, "Scientific Internationalism and the Weimar Physicists: The Ideology and its Manipulation in Germany after World War I," *Isis* 64 (1973): 163.

a harsh and reactionary move, German and Austrian scientists promptly rejected the invitation and thus failed to join the IRC until after the Second World War.

The effects of these boycotts went far beyond the IRC and Solvay Councils; from 1919 to 1925 German scientists were held out of 165 international scientific conferences and most international scientific institutes and agencies based in Germany relocated elsewhere. By one count, in 1914 there were 60 international scientific organizations with 14 located in Germany (just over 23%). However, by 1923, there were 80 such organizations with only 6 in Germany (7.5%).[102] This discrepancy is reflective of the dramatic downturn in Germany's place amongst the international scientific community.

As a result of the political tensions resulting from World War I and in particular due to the infamous 1914 German manifesto "Appeal to the civilized world" signed by so many scientists, participation at the Solvay Councils of 1921 and 1924 was heavily restricted. All German and Austrian physicists were excluded from these, even those who had not signed the "Appeal". The decision to exclude these scientists based on their nationality was not an emotionally charged, spontaneous reaction by non-German/Austrian scientists. In fact, members of the Solvay Councils' organizing committee engaged in a delicate dialog leading up to the 1921 meeting in order to decide which scientists, if any, should be excluded. As early as 1919, in anticipation of this dilemma, Lorentz, who spent the war in neutral Holland, wrote to Solvay:

> What should our attitude be towards the Germans? The misery and suffering they have caused worldwide, the injustices and atrocities committed by their government and by their armies, rightly abhorred by all decent people, have as you know made a deep and painful impression on me. In addition, I can understand perfectly well that for the time being, Belgian and French scientists want nothing more to do with them (…). And yet, if we are talking about Germans, we really mustn't lose sight of the fact that individually,

[102] Kragh, *Quantum Generations,* 143 - 145.

> they are all different. (…) There are also several physicists who would not sign the disgraceful manifesto of the 93(…). All things considered, I feel I must suggest to you that we should not formally exclude the Germans; in short, we should not close the door to them for ever [*sic*].[103]

On the other side of the debate was Marcel Brillouin who wanted not only to exclude all German scientists, including the famous pacifist Einstein, but also any other nationals who may have be seen as pro-German, like Peter Debye. Eventually, the organizing committee settled on full exclusion except for Einstein who they considered “of some ill-defined nationality”. Einstein initially accepted the invitation to the 1921 Solvay Council but ultimately decided against attending.[104] When in preparation for the 1924 Solvay Council the same exclusion policy was to be upheld, Einstein wrote to Lorentz:

> This letter is hard for me to write, but write it I must. I am here with Sommerfeld. He is of the opinion that it is not right for me to take part in the Solvay Council, because my German colleagues have been excluded from it. I am of the opinion that politics should have no place in scientific matters. It leads to a situation where individuals are judged responsible for acts of government in their native countries. If I took part in the Council, I would be actively supporting a decision which I consider to be deeply unjust. (…) I would be grateful if you would send me no further invitations to the Councils. I hope not to be put in a position where I am obliged to refuse an invitation, as such a gesture might hinder the progress being made towards re-establishing amicable collaborations between physicists of all nationalities.[105]

German physicists were finally allowed to participate in the 1927 Solvay Council as a result of Germany becoming a member of the League of Nations, and then, only with the explicit permission of King Albert of Belgium.[106]

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[103] Wallenborn and Marage (ed.), *The Solvay Councils and the Birth of Modern Physics,* 113 – 114.
[104] Ibid. p. 115.
[105] Ibid. p. 115.
[106] Ibid. p. 115.

From the preceding description of the antagonism within the international scientific community, one might surmise that German science would have suffered tremendously both during and after World War I but in fact, within the field of quantum theory, the opposite is true. As we saw in the last chapter, Germans, especially in Berlin with Einstein and in Munich with Sommerfeld, were extremely active in quantum theory research throughout the war and in its immediate aftermath. So how can we explain this seeming paradox? With increased isolation and a heavily crippled socio-economic infrastructure, how did German physics not only survive, but thrive?

As the spokesperson for German science, Planck's call after the war to promote and defend German scientific supremacy at all costs was critical in this regard. The individual German states had traditionally funded their own universities and research institutions, but in the period after the Paris Peace Conference, they were failing to provide the funding necessary to answer Planck's challenge. As a result new sources of funding became necessary for the survival of German science. Founded in 1920, the *Notgemeinschaft der Deutschen Wissenschaft* (Emergency Association of German Science) cobbled together by domestic and international sponsorship, became a centralized scientific funding lifeline for German science. As a new federal organizing body, the *Notgemeinschaft* (NG) superseded the previous system of state controlled universities and thereby centralized all decision-making as it pertained to funding and research policy. During the early 1920s, amidst the general economic crisis and international scientific boycott, the NG became very adept at fundraising from both domestic and foreign industries and the German central treasury. While the IRC had excluded Germany from its ranks in 1919, private American industrial powers such as General Electric recognized the larger importance of German science and began donating significant funds in support of the Electrophysics Committee, a subcommittee of the larger NG.[107]

It was not only the restructuring of funding agencies that helped German quantum physics research thrive after World War I; the role of scientific collaboration with colleagues from neutral countries played a decisive role as well. As it turns out, the international boycott against German science may have been more detrimental to French and British quantum research programs than to the German's. During and immediately following the war Bohr in Copenhagen, Sommerfeld in Munich, Ehrenfest in Leiden, and Born in Göttingen spearheaded critical efforts in the advancement of quantum theory. As allied countries exacted their patriotic revenge by instituting the international scientific boycott of Germany, the neutral countries did not follow their lead and retained an active scientific dialog throughout. As was noted earlier, Lorentz' repeated lobbying for the re-inclusion of German scientists in the greater international arena was rejected by both Solvay and the IRC. That post-war polarizing tendency by the allied front served as a catalyst to bring neutral countries such as Holland and Denmark into closer alignment with German science. By the summer of 1922, this alignment was in full regalia at the "Bohr Festival" in Göttingen where Bohr was the honored guest lecturer on quantum theory.

While we have discussed various socio-political, economic, and cultural factors that contributed to the overall context of quantum innovations in the 1920s, the general scientific alliances or blocs that were formed did not necessitate a unified or homogenous approach to the study of quantum physics. It is important to understand that while these contextual socio-political, economic, and cultural binding forces may have been formative they certainly fell far short of being defining. Furthermore, while it is tempting to tie a clear, direct causal link

[107] David C. Cassidy, *Uncertainty: The Life and Science of Werner Heisenberg.* (New York: W.H. Freeman, 1993), 160 and Kragh, *Quantum Generations,* 141 – 143.

between these alliances and the eventual establishment of the Göttingen-Copenhagen Interpretation in the late 1920s, we should be wary of such alignments. The factors we have examined played a role in the story of quantum innovation but to what extent they caused or directed the dynamics of innovation is not at all certain.

In the rest of this chapter we will take a closer look at the quantum community of physicists themselves, their interpersonal relationships, and their scientific innovations. While the previous sections dealt primarily with contextual issues pertaining to factors outside of academia these next sections will deal directly with academic concerns.

2.3- The Academic Social Network

There can be no doubt that the ties that bound the quantum research programs from the universities at Göttingen, Munich, Copenhagen, and Leiden in the aftermath of World War I were due, in part, to the political and economic fallout from the Paris Peace Conference. However, these were certainly not the only factors binding this group of researchers. As we saw in the last chapter, the Austro-German university system's unique pedagogical philosophy that rose to prominence in the 19th century had a distinct effect on other countries' academic philosophies. In this same vein, Bohr's institute in Copenhagen and Ehrenfest's in Leiden were heavily influenced by the German system of higher education.

In Austrian and German universities there was a long tradition of the "physicist-philosopher" epitomized by the likes of Helmholtz, Mach, Boltzmann, and Planck. In fact, the German academic system actively pressured scientists to engage in meaningful discourse with other scholars from the humanities and the social sciences. This discourse became available for wider public consumption beginning in 1913 with the founding of Arnold Berliner's popular science journal *Die Naturwissenschaften*.[108] Under this rubric, esteemed physicists were expected to address their colleagues periodically in the humanities and social sciences in a healthy cross-disciplinary dialog. This policy was not in place solely to elicit a dissemination of basic scientific concepts to laypeople; the goal was a discourse that would provide a deeply penetrating philosophical reflection on the latest scientific innovations. There is no doubt that this institutional policy gave Austrian and German physicists a structured format in which to contemplate the possibilities of relinquishing classical foundational Kantian concepts like absolute space, causality, and gravitational mass.

There was another legacy of institutional policy within the Austrian-German university system that was critical to the development of quantum theory and the eventual Göttingen-Copenhagen alliance, the tradition of peripatetic learning. It was quite common for someone studying physics at the university in Germany and Austria in the late 19th and early 20th century to travel to multiple cities so that they may study with specific academics whose research interests or reputations piqued an interest. The pedagogical focus on experiential and immersion learning which, in some respects, influenced the quantum schools at both Leiden and Copenhagen was a key determinant of the orthodox alliance. The pedagogy was experiential, in the sense that all physicists were expected to show some experimental acuity, gleaned from working on the front lines of scientific innovation in some of the leading scientific laboratories in the world. It was immersive, in the sense that university students were allowed to jump right in and, with the permission of the professor, could tackle advanced research problems relatively early in their academic careers instead of taking a layered approach to a subject by slowly

[108] Michael Stöltzner, "The Causality Debates of the Interwar Years and their Preconditions," Preprint. Some of the arguments Stöltzner uses are also found in his article: Michael Stöltzner, "Vienna Indeterminism: Mach, Boltzmann, Exner," *Synthese*, Vol. 119, No. 1/2.

building up strata of mathematical tools necessary for later research. This sink or swim attitude was very useful for identifying and quickly propelling the more advanced students to academic innovation and success.

There were numerous byproducts of the Austro-German tradition of peripatetic learning that had formative effects on the quantum alliances. In the following discussion I will briefly highlight four that seem to me critical. First, within each generation of physicists many of the most successful minds had a shared academic experience. Along the road to their doctorates and habilitations they had, at one point or another, crossed paths and studied with each other, or at a minimum shared an intellectual legacy of mentors. Second, due to these various points of contact, many of the leading physicists had a personal bond that dated back to their days as students. These bonds of friendship and collegial respect are evident in the correspondence which we find between the various leading members of the Göttingen-Copenhagen alliance. This is not to say that there was no correspondence between physicists outside this bloc, but the character of the dialog was substantially different. These first two points significantly contributed to the process of conceptual cross-pollination between different centers of quantum research. How much of this cross-pollination is due to peripatetic movements and how much is due to personal correspondence is a complex matter to decipher. In any case, both factors played a role and we will leave it for another study to determine which of these two was more effective at conceptual cross-pollination. In any thorough analysis of the quantum dialog one must take these two factors into careful juxtaposition with a third axis, the published scientific papers.

The third important byproduct of the peripatetic nature of the Austro-German educational legacy was the fact that as physicists without a singular intellectual environment, these scientists were being trained to be elastic in their thinking. This elasticity was a direct result of experiencing the multiple, sometimes jarring, shifts from an immersion in one advisors' framework to the next. Lastly, the freedom of the best and brightest students to chose where, and with whom, to study elevated the level of the elite research programs even more as these would inevitably attract a vast majority of the most promising young academics. In this sense it is easy to see how Bohr, Sommerfeld, Born, and Ehrenfest were able to build academic dynasties in Copenhagen, Munich, Göttingen, and Leiden respectfully.

When describing the most important contributions to quantum theory during and after World War I, we included Einstein, at the University of Berlin, as a key contributor. However, Einstein is conspicuously absent from our discussion relating to the great quantum schools of the 1920s and the scientific alliances that led to the Göttingen-Copenhagen bloc. While Einstein was a phenomenal contributor to research in this field, he was not known for his skills as a teacher and mentor. In fact, according to Abraham Pais Einstein accepted the offer from Planck and Nernst to move to Berlin in large part because "…he had had enough of teaching classes. All he wanted to do was think."[109] It's not that Einstein disliked interacting with younger colleagues, but he was not a natural teacher and felt that his attentions were more appropriately directed towards research. As we shall see in the next chapter, his limitations as a teacher in conjunction with his varied foci on a wide range of scientific and non-scientific problems had their own effects on the dynamics that would arise in the latter half of the 1920s. To round out this point about the peripatetic nature of physicists associated with the leading quantum schools and their relationship to the interconnected web of academic legacy

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[109] Abraham Pais, '*Subtle is the Lord…': The Science and the Life of Albert Einstein.* (Oxford: Oxford University Press, 1982), 238.

relationships that linked Munich, Copenhagen, Göttingen and Leiden during and after World War I, we will briefly examine each of these quantum schools in turn.

Ludwig Maximilians University of Munich (LMU)

When Wilhelm Röntgen, the discoverer of X-rays and the recipient of the first Nobel Prize in Physics, invited Arnold Sommerfeld to become the first director of the newly formed *Theoretical Physics Institute* at the Ludwig Maximilians University of Munich (LMU) in 1906, Sommerfeld jumped at the opportunity. While his goal was nothing less than to build this new institute into what he termed, a world renowned "nursery of theoretical physics" research, he was not oblivious to the importance of experimental physics as a complementary pursuit. In fact, within the confines of his new institute of theoretical physics he housed a laboratory in which they discovered x-ray diffraction in 1912, a discovery he would later call "the most important scientific event in the history of the institute".[110]

As we saw with Planck, and other German physicists trained in the late 19th century, Sommerfeld had learned to explore his lines of inquiry within mathematical physics while keeping grounded in the experimental results that served to justify his conclusions or forced him to adapt a new approach. Formed in the tradition of the physicist-philosopher, Sommerfeld was well aware of the philosophical debates surrounding the new developments in quantum theory, nevertheless he remained highly pragmatic in his approach to scientific innovation, preferring to ground his theoretical work with corresponding empirical evidence. These skills were honed while he studied first in Königsberg, at Albertina University, under mathematicians Ferdinand von Lindemann and David Hilbert, and physicist Emil Wiechert then later in Göttingen under Felix Klein and Theodor Liebisch.

[110] Michael Eckert, "The Emergence of Quantum Schools: Munich, Göttingen and Copenhagen as new Centers of Atomic Theory," *Ann. Phys.* 10 (2001): 152-153.

In the winter months of 1914 through 1915, Sommerfeld prepared to lecture on atomic physics and for the first time familiarized himself with the Bohr-Rutherford atomic model. As we saw in the last chapter, during and immediately after the First World War Sommerfeld and his students worked to extend the Bohr-Rutherford atomic model. Thanks to all the contributions from Munich, eventually the atomic quantum theory became known as the Bohr-Sommerfeld theory. In 1919 Sommerfeld published the first edition of his seminal text book on atomic theory *Atombau und Spektrallinien* (Atomic Structure and Spectral Lines). This became regarded by most physicists as one of the early foundational teaching texts on atomic physics and the early applications of quantum theory.

Sommerfeld's institute did not have the single-minded approach that seemed to exist in other quantum schools like Bohr's group in Copenhagen and within Born's group in Göttingen. His preference was to always be aware of many simultaneous problems within physics. While he was very active in the early developments of quantum theory he was simultaneously pursuing research programs in multiple areas of interest. As a result, students under his mentorship received the freedom to pursue myriad research initiatives and gained a strong general understanding of various research methodologies. Sommerfeld's "nursery" was extraordinarily successful at placing its alumni within top flight research programs where many went on to become world renowned physicists. In particular, from Table 2.1 we notice that the vast majority of the physicists who studied with Sommerfeld in Munich, did so first before going on to study at one of the other three renowned quantum schools. The most convincing evidence for referring to Sommerfeld's institute as the quantum nursery is the list of names of its alumni who went on to study and produce groundbreaking research elsewhere. A partial list includes names like Paul Epstein, Peter Debye, Alfred Landé, Max von Laue, Wolfgang Pauli, Werner Heisenberg, Walter Elsasser, Hans Bethe, Fritz London, Linus Pauling, and Walter Heitler.

University of Copenhagen

Niels Bohr's studies in England and his early work on atomic physics were part of the narrative we explored in last chapter's look at the slow rise of quantum physics. In contrast to Sommerfeld in Munich, Bohr was not recruited to his university by a Nobel Laureate and given the reigns of newly formed, well funded institute for theoretical physics. At the University of Copenhagen, the young Dane had to build the institute himself. After returning from his post-doctoral educational stint in England Bohr began appealing to the Danish ministry of education that they establish a professorship for theoretical physics at the University of Copenhagen. In 1916, Bohr was finally appointed his professorship at the University but it would take another five years for his institute of theoretical physics to be formally inaugurated.

In the meantime Bohr was actively trying to establish the University of Copenhagen as a destination not just for Danish physicists, but for the entire international physics community. During the war, a Dutch student by the name of Hendrik Kramers from the University of Leiden had made his way, uninvited, to Copenhagen so that he might work with Bohr. When Kramers first arrived at the University in 1916 Bohr was not yet an international figure within the physics community and had little more than a cramped office to offer the young doctoral student. As the war trudged on and Bohr and Kramers realized that other groups, especially Sommerfeld's in Munich, were working to extend the Bohr-Rutherford atomic model, Copenhagen's standing in the international physics community began to skyrocket.

A combination of the gravitational pull of Bohr's research agenda, his country's political neutrality, and the force of his personality made it easy for physicists like Sommerfeld to visit

Copenhagen while on international speaking tours. After just such a visit in 1919, Sommerfeld was so impressed by the potential of Bohr's fledgling research program that he began actively lobbying for funding on behalf of Bohr. In a recommendation letter to the Carlsberg Foundation in support of a grant proposal for Bohr, Sommerfeld stated:

> The Institute of Mr. Bohr should not only serve the upcoming generation of Denmark, it will also be an international place of work for foreign talents whose own countries are no longer in a position to make available the golden freedom of scientific work,…[111]

After receiving such a glowing recommendation from Arnold Sommerfeld, one of the most respected physicists in the international scientific community, there was no doubt that Bohr would not only receive the Carlsberg Foundation's grant but would also see substantially increased traffic from visiting physicists from around the world.

Even before his institute was officially founded In March of 1921, Bohr was receiving visitors like Sommerfeld, James Franck, George de Hevesy and Alfred Landé. Within a month of the establishment of his institute in Copenhagen, Bohr's paper was read at the Solvay Council and the following summer, in June 1922, he was invited by Franck and Max Born to lecture at the University of Göttingen in what came to be known as the famous "Bohr Festival". Over the next decade Copenhagen became one of the leading, if not the leading, centers of theoretical atomic research in the world with over sixty physicists, from seventeen different countries, making extended visits to Bohr's institute in Copenhagen. In a 1979 study by Peter Robertson, *The Early Years: The Niels Bohr Institute 1921-1930*, the author provides us with a list of these visitors to Copenhagen during the 1920s. Without a doubt, for most physicists interested in atomic or quantum theory, the Bohr Institute became the most illustrious of destinations.

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[111] Eckert, "The Emergence of Quantum Schools: Munich, Göttingen and Copenhagen as New Centers of Atomic Theory," 155.

Bohr's particular approach to collaboration was seen as quite innovative. He refused to work on his theoretical frameworks in isolation, preferring a mix of students and colleagues that could serve both as a source of creative inspiration and as sounding boards for his own thoughts. Those who studied under Bohr or worked alongside him were integral parts of a collaborative team.[112] This creative tension between collaborators was instrumental in helping form the innovations that percolated throughout quantum theory during the 1920s. As opposed to Sommerfeld's more pragmatic, empirically based approach to theory building, Bohr was famous for grounding his theoretical explorations in axiomatic principles and then extrapolating from there. In chapter one, we saw the importance of Bohr's axiomatic methodologies in his extensive use of the Correspondence Principle in constructing the earliest formulations of quantum theory.

University of Göttingen

While the quantum schools at Munich and Copenhagen were largely centered on the two personalities of Sommerfeld and Bohr respectively, at the University of Göttingen things were quite different. The University of Göttingen had a long and rich tradition in mathematical physics stemming from the 19th century. We noted earlier that Sommerfeld's connection to Göttingen stemmed from his tenure there first as a student under Felix Klein and Theodor Liebisch and then as a *privatdozent* lecturing alongside Klein, Liebisch, and David Hilbert. As head of his own institute in Munich Sommerfeld was later able to help place one of his best students, Peter Debye, as a professor of theoretical physics at the University of Göttingen.

[112] Mara Beller, *Quantum Dialogue: The Making of a Revolution.* (Chicago, IL: University of Chicago Press, 2001), 3.

Table 2.1: Peripatetic Physicists with Visits to the Four Schools of Quantum Research–

Name	Country	Born	Copenhagen	Göttingen	Munich	Leiden
Kronig, R.	USA/Germany	1904	1924-25, 1927			1924
Dennison, D.	USA	1900	1924–26, 1927			1927
Kemble, E. C.	USA	1889		1927	1927	
Oppenheimer, J.	USA	1904		1926–27		1928-29
Pauling, L.	USA	1901	1927		1926–27	
Dirac, P. A. M.	U.K.	1902	1926–27	1927		1927
Klein, O.	Sweden	1894	1918-22, 1926–31			1926
Epstein. P. S.	Russia	1883			1911–17	1918-21
Uhlenbeck, G. E.	Netherlands	1900	1927	1927		1919-22, 1925-27
Casimir, H.	Netherlands	1909	1929-30			1926-31
Coster, D.	Netherlands	1889	1922-23			1916-22
Debye, P.	Netherlands	1884		1913–20	1906–11	
Goudsmit, S. A.	Netherlands	1902	1926–27			1922-27
Kramers, H. A.	Netherlands	1894	1916–26			1912-16
Fermi, E.	Italy	1901		1923–24		1924
Heisenberg, W.	Germany	1901	1924–25, 1926–27	1923–24, 1925–26	1920–23	192?
Elsasser, W.	Germany	1904		1924–27	1923–24	1927
Heitler, W.	Germany	1904	1926–27	1927–33	1924–26	
Landé, A.	Germany	1888	1920	1913–14	1912–14	
Nordheim, L.	Germany	1899	1928, 1929	1922–27, 1928–32	1922	
Delbrück, M.	Germany	1906	1931–32	1926–30		
Ewald. P. P.	Germany	1888		1912–13	1907–12, 1913–21	
Franck, J.	Germany	1882	1921	1921–33		
Fues, E.	Germany	1893	1927		1918–20	
Hund, F.	Germany	1896	1926–27	1920–26		
Jordan, P.	Germany	1902	1926-27	1922–27		
Kratzer, A.	Germany	1893		1920–21	1916–20, 1921–22	
London, F.	Germany	1900		1921–27	1917–21	
Rosenfeld, L.	Belgium	1904	1935–40	1927–29		
Pauli, W.	Austria	1900	1922–23	1921–22	1918–21	
Rabi, I. I.	Austria	1898	1927		1927–28	
Rubinowicz, A.	Austria	1889	1920, 1922		1916–18	
Weisskopf, V.	Austria	1908	1932–33	1928–31		

Source: Table 2.1 is a subset of a larger dataset Aggregate Biographical Data Repository (ABDR) for Quantum Physicists compiled from various sources of biographical material: Michael Eckert's 2001 study *The Emergence of Quantum Schools: Munich, Göttingen and Copenhagen as New Centers of Atomic Theory*, Peter Robertson's 1979 study *The Early Years: The Niels Bohr Institute 1921-1930*, and entries in the New *Dictionary of Scientific Biography*.

In 1920, looking to replace Debye who was leaving for a position at the *Swiss Federal Institute of Technology (ETH) Zürich*, the University of Göttingen reached out to Max Born to found a new Institute of Theoretical Physics. Born was no stranger to Göttingen as he had spent the better part of a decade working there under Hilbert, Klein, and Minkowski first as a doctoral student, then on his habilitation, and finally as a *privatdozent.* It is not surprising then, that Born was tapped to head up his own theoretical physics institute there in 1921. While negotiating the details of his post with the Ministry of Education in Berlin, Born made his placement contingent on the hiring of close friend and collaborator, James Franck. The Ministry complied and hired Franck who was an exceptional experimentalist responsible, in part, for the Franck-Hertz experiment that helped confirm the Bohr-Rutherford model of the atom in 1912-1914 as the head of his own Institute for Experimental Physics. Born, Franck, and Hilbert would establish a formidable collaboration that would become the center piece of Göttingen's new quantum school.

By the time Born and Franck brought their physics expertise to Göttingen in 1921, David Hilbert had spent over a quarter century building a world renowned mathematical physics institute from the ground up. In 1900, Hilbert presented a field-defining lecture at the second international congress of mathematics in Paris, where he challenged the international mathematical community by presenting a list of twenty-three seminal unsolved questions or problems from all areas of mathematics that needed to be addressed moving forward. These became the critical problems to tackle for many mathematicians of his and subsequent generations. For Hilbert scholar Ulrich Majer, it was this 1900 lecture that marked the beginning of one of the most dynamic periods in the history of mathematical physics. Hilbert established a dynasty in Göttingen, from which he and his collaborators, transformed mathematical physics and helped refine and substantiate physics' 20th century revolutions, relativity theory and quantum theory.[113]

Born and Sommerfeld shared a common intellectual heritage from their days at Göttingen. Like Sommerfeld, Born tended to be pragmatic in his theory building and worked closely with experimentalists like Franck to confirm his models. Both physicists saw each other as collaborators and at times, friendly competitors. As such, in 1925, Born decided to publish his own textbook on atomic physics titled: *Vorlesungen über Atommechanik* (Lectures on Atomic Mechanics) just months before Heisenberg arrived at his matrix mechanics. In this first volume Born claimed that this treatment was based on atomic theory as it stood at that moment and that a final theory would not be far behind. Of course, he was not expecting Heisenberg's discovery to come so quickly and invalidate his newly published book. Five years later, Born and Jordan published the second volume of *Vorlesungen über Atommechanik* with the supposed final quantum theory. Unfortunately, as we will see later, they chose to cover only the matrix formulation of quantum mechanics making their approach undesirable to many working physicists.

While they already shared a common intellectual heritage, Sommerfeld and Born would go on to build their connections by exchanging more than just ideas. In the spirit of peripatetic studies, these two exchanged their students. In the winter term of 1922/23 Sommerfeld accepted a visiting professorship at the University of Wisconsin (Madison) and entrusted four of his top students, including Werner Heisenberg, to Born in Göttingen. In a letter to Sommerfeld, Born expressed his admiration for Heisenberg:

[113] Ulrich Majer, "The Axiomatic Method and the Foundations of Science: Historical Roots of Mathematical Physics in Göttingen (1900-1930)," in *John von Neumann and the Foundations of Quantum Physics*, edited by Rédei, M. and Stöltzner, M. (Dordrecht: Kluwer Academic Publishers. 2001), 21-22.

> I have in addition to your four descendents 9 doctoral students,…[as for Heisenberg] I am very fond of him, When I asked him what he intended to do afterwards [i.e., after the doctoral work with Sommerfeld] he responded: 'That is not up to me to decide. This Sommerfeld will decide!' So you are his self-chosen guardian, and I have to ask you for permission to lure him away to Göttingen.[114]

Lure away he did. Heisenberg became Born's assistant the following year and developed his matrix mechanics while still working under Born in 1925.

University of Leiden

Mostly a forgotten force in narratives concerning the development of quantum theory, the University of Leiden, in the Netherlands, was actually quite critical to the cause. While understanding that Leiden's contributions were overshadowed by those of the triumvirate of Munich, Copenhagen, and Göttingen, as a member of the bloc or alliance it is imperative that we avoid dismissing its contributions. In his 2001 study of "Quantum Schools", Michael Eckert claimed that the historical study of quantum physics was in need of new perspectives. Navigating his narrative between too many biographical details and arbitrary social constructivism, Eckert set out to use preexisting educational institutions as guides for his study.

While I agree with the general approach and tenor of Michael Eckert's analysis of the quantum schools, I think his examination can be usefully extended. Eckert focuses his study on the three quantum schools: Munich, Copenhagen, and Göttingen. I agree that these are critical to the quantum narrative, but including Leiden results in a more representative picture of the important quantum schools from this time period. It is interesting to note that in his conclusion Eckert does list secondary schools worth studying that were founded in the late 1920s and early 1930s by some of the "descendents" in Leipzig (Heisenberg), Zürich (Pauli), MIT (Slater), and

[114] Eckert, "The emergence of Quantum Schools: Munich, Göttingen and Copenhagen as new centers of atomic theory," 156.

Bristol (Mott). After examining Table 2.1, it seems clear that there was enough exchange, both intellectually and physically, between Leiden and the other three schools to warrant inclusion into this overall analysis. Let me also note that while I think these four quantum centers were the most important from World War I through the late 1920s it would be instructive to follow Eckert's suggestion and expand this study to include other universities. While this study would fall outside the purview of our current analysis, it would certainly be informative to add other schools such as those from Germany (Berlin and Leipzig), Switzerland (Zürich and ETH), England (Cambridge and Bristol), and Italy (University of Rome).

Leiden became a bastion of theoretical physics at the end of the 19th and beginning of the 20th centuries thanks in large part to its most famous physicist Hendrik Lorentz who held the chair of theoretical physics at Leiden from 1878 through 1912. After thirty-four years as the chair, Lorentz was stepping down to head up a research institute at the Teylers Museum in Haarlem. Based on Lorentz' recommendation the chair was given to Paul Ehrenfest, an Austrian theoretical physicist. Ehrenfest had spent his peripatetic studies in Vienna studying under Boltzmann, with Hilbert and Klein in Göttingen, and with Lorentz in Leiden.

In 1912, Ehrenfest founded the Institute for Theoretical Physics at Leiden and went about exploring issues in the fledgling field of quantum theory. While we have already noted some of Ehrenfest's early contributions to the development of quantum theory in chapter one (adiabatic invariants) his most important contributions may have been as a teacher and mentor. As a product of the Austro-German peripatetic educational system he was always encouraging his students to study abroad and gain research experience elsewhere. One of the earliest beneficiaries of this encouragement was Hendrik Kramers who went to Denmark during the war to work with Bohr and as a result spent the next decade in Copenhagen at the center of the quantum revolution. However, he would certainly not be the last. We see from Table 2.1 that during the 1920s, Ehrenfest sent other Dutch notables such as George Uhlenbeck, Dirk Coster, Samuel Goudsmidt, and Hendrik Casimir to Copenhagen and accepted international students in return including Werner Heisenberg, J.R. Oppenheimer, P.A.M. Dirac, Paul Epstein, Ralph Kronig, and Enrico Fermi. In addition, Ehrenfest's program made Leiden a common destination for visiting physicists giving workshops or lectures. For example, Einstein maintained a regular appointment at the University to teach annual seminars from 1920 through 1930.

It seems clear that taking aggregate biographical data and looking for intellectual networks based on the four quantum schools highlighted above can be a useful exercise. In doing so, we have uncovered patterns that might be difficult to detect otherwise such as the notion that the vast majority of the quantum physicists who spent time in Munich with Sommerfeld made that their first stop and subsequently visited one or more of the other three schools. Another trend that we may have previously suspected due to anecdotal evidence but is now empirically justified is that the Leiden and Munich schools seem to have served more as feeders to the Copenhagen and Göttingen schools. This would explain the dominant place of these two in the establishment of the Copenhagen- Göttingen interpretation of the late 1920s.

Specifically, we note that the effects of World War I on the organization of the international physics community were very real, and have not been given their due attention in most quantum narratives. The international science boycotts, and the resultant alliances that developed between the Austro-German centers and those in the neutral countries like Denmark and the Netherlands helped set the stage for quantum innovation in the 1920s. In Table 2.1, one can see that of all the physicists training in more than one of these centers of quantum activity during the 1920s, there were only five Americans, and one British physicist who defied the boycott. Of the five Americans, only three visited Germany while the other two preferred to stay in neutral lands. Also, the first of these visits did not occur until 1924, with the majority coming after 1925. If we look at other World War I allied countries, we notice that Belgium has only one visitor and Léon Rosenfeld's visits came after Germany had joined the League of Nations in 1927. What about France? After examining the entire dataset of close to ninety physicists I can find no examples of French physicists visiting any of these schools between 1914 and 1935.

There seems to be an inversely proportional relationship between number of physicists of a certain nationality who visited these centers during the post war period and how much these countries sacrificed during the war. France, which withstood the highest casualty rate of all allies during the war by losing 4.3% of its population and suffering an additional 4.27 million injuries (or 11% of its population), had no representation, while the United States which in comparison suffered much less (losing only 0.13% of its population) sent the highest number of allied representatives.[115] The relatively high level of American involvement should not surprise us as we have already discussed the economic assistance that American industrial powers such as General Electric and philanthropic organizations such as the Carnegie, Ford, and Rockefeller Foundations gave in supporting research grants and fellowships for Germans as well as American physicists studying abroad.

It should be clear by now that the quantum alliance formed between German universities like Munich and Göttingen and quantum schools from neutral countries like Leiden and Copenhagen was real and formative in the evolution of quantum theory. However, this

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[115] Philip Haythornthwaite, *The World War One Source Book.* London: Arms and Armour Press, 1996. Also from the two volume set – edited by Anne C. Venzon, *The United States in the First World War: An Encyclopedia* New York: Routledge, 1999 and Spencer Tucker (ed.), *The European Powers in the First World War: An Encyclopedia.* New York: Routledge, 1999*.*

aggregated biographical data should not stand alone in a historical analysis of quantum theory, because in isolation it can easily serve to misrepresent and distort the actual scientific dialog that took place. Someone looking at the previous analysis may be tempted to conclude that this quantum alliance led naturally and directly to a homogenous Copenhagen-Göttingen interpretation due to the tight nature of the academic web woven between the four quantum schools. As we noted earlier, this would only serve as a simplistic reduction of the actual complex human dynamics that were at play within the larger quantum community during the 1920s. While this dimension of the fuller analysis is important we should actively work to incorporate it in the larger context. So far we have constructed a multidimensional contextual framework based on socio-political, economic, cultural, pedagogical, and institutional factors that contributed to the evolution of quantum theory during the 1920s. In this next section we look at an example of arguably the most important dimension of all for analysis in the history of science, something Mara Beller termed "creative dialogical flux".[116]

2.4- Emblematic Case Study – Conflicts and the Discovery of Elements 70-72

In this final section of our discussion of the emergence of elite quantum schools in post World War I Europe and their impact on scientific innovation during the 1920s, we examine the circumstances surrounding a particular case of innovation. The complex nature of nationalism's influence on science was emblematic of this period and will help us understand and reconstruct the dynamical context within the international physics community at the pinnacle of the quantum revolution which we will explore in the following two chapters. An important result of exploring this case study is its clear illustration of the need for us to study both external and

[116] Beller, *Quantum Dialogue,* 3.

internal factors within the larger context of innovation. In this chapter we have focused primarily on factors external to the science itself so as to lay a foundation for our analysis moving forward, in the coming chapters we look to combine this discussion with evidence culled from personal correspondence and published scientific works. This account serves as a template for this type of analysis and illustrates some of the complexities in simultaneously examining these distinct factors.

The day after receiving his Nobel Prize on December 10th, 1922, Niels Bohr gave his Nobel lecture on "The Structure of the Atom" in Stockholm, Sweden. In concluding remarks to this thorough survey of modern atomic theory, Bohr made a brief yet poignant reference to a brewing priority conflict between physicists working in his laboratories in Copenhagen and those working in Maurice de Broglie's laboratory in Paris:

> Before concluding this lecture I should like to mention one further point in which X-ray investigations have been of importance for the test of the [atomic] theory. This concerns the properties of the hitherto unknown element with atomic number 72. On this question opinion has been divided in respect to the conclusions that could be drawn from the relationships within the Periodic Table, and in many representations of the table a place is left open for this element in the rare-earth family. In Julius Thomsen's representation of the natural system, however, this hypothetical element was given a position homologous to titanium and zirconium…[consistent with the current atomic theory outlined above].
>
> [Recently] a communication was published by Dauvillier announcing the observation of some weak lines in the X-ray spectrum of a preparation containing rare-earths. These were ascribed to an element with atomic number 72 assumed to be identical with an element of the rare-earth family, the existence of which in the preparation used had been presumed by Urbain many years ago. This conclusion would, however, if it could be maintained, place extraordinarily great, if not unsurmountable, difficulties in the way of the [current atomic] theory… In these circumstances Dr. Coster and Prof. Hevesy, who are both for the time working in Copenhagen, took up a short time ago the problem of testing a preparation of zircon-bearing minerals by X-ray spectroscopic analysis. These investigators have been able to establish the existence in the minerals investigated of appreciable quantities of an element with atomic number 72, the chemical properties of

> which show a great similarity to those of zirconium and a decided difference from those of the rare-earths.[117]

There are multiple reasons for discussing this particular controversy in the context of this chapter. First, it highlights the importance of the experimental practices in the early development of quantum theory. In too many historical accounts, the quantum revolution is characterized as being guided primarily by theoretical innovation, with singular experiments playing important supporting roles in the narrative. As a counterpoint to this view, it is important that we keep in mind the continuous and seminal symbiotic relationship between theory and experiment throughout the evolution of quantum physics. Second, this episode is emblematic of the post World War I quantum divide between scientific communities in the allied countries, especially in France, and those from "quantum bloc" countries, highlighted by the exclusionary practices of various international scientific organizations. Lastly, while there is no direct evidence of this priority dispute playing a direct causal role in the relatively limited nature of responses to Louis de Broglie's innovative work of 1923-24 it should at least be acknowledged as a possible contributing factor. In echoing a theory proposed by the renowned Hungarian philosopher of science Imre Lakatos, Helge Kragh took this very stance:

> …conflicts over priority are not only conflicts between individual scientists, but are also (very often, at least) conflicts between two or more competing research programmes. The decision to accept one priority claim and to reject another is also a *defacto* decision to strengthen one research programme in expense of a rival one. The result of a priority conflict then sets out the road for a future (internal) research policy.[118]

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[117] Bohr, Niels. 1922. Nobel Lecture: http://nobelprize.org/nobel_prizes/physics/laureates/1922/bohr-lecture.pdf

[118] Kragh, "Anatomy of a Priority Conflict: The case of Element 72," 278.

As we noted in a previous section there were no French representatives who visited any of the four quantum schools from 1918-1930. It is undeniable that after World War I all the cultural, political, and networking effects that we have discussed thus far were formative in this mutual isolationism between French physicists and members of the four quantum schools from the Universities of Munich, Göttingen, Copenhagen, and Leiden. However, dissecting this particular priority controversy will add probative value to our analysis by illustrating the particulars of a conflict brimming with nationalism and residual tensions from international scientific policy.

We begin this particular story in 1878 with the discovery ytterbia a new rare mineral found in a sample of Swedish soil and subsequently analyzed by the Swiss chemist Jean Charles Galissard de Marignac. Dmitri Mendeleev had developed his chemical laws of periodicity nearly a decade earlier leading to the proposal of many theoretically predicted elements that subsequently needed to be discovered and chemically isolated. Upon completing his analysis of the rare mineral he had found, Marignac became convinced that he had isolated a new chemical element and he designated it ytterbia. As Mendeleev 's periodic table grew and incorporated newly discovered elements it became increasingly clear that ytterbia was not an element itself, but a composite compound of multiple elements; unfortunately, the process of separating out these elements by "repeated fractionation" was extremely difficult work. From 1904-07, the French chemist Georges Urbain and the Austrian chemist Carl Auer von Welsbach independently set about to break ytterbia down and isolate its constituents.

By 1907, both chemists had managed to independently use the repeated fractionation to clearly isolate what they believed were the two elementary components of the ytterbia compound. While Welsbach published preliminary findings in 1906, Urbain became the first to publish a detailed report and was thus, initially, given priority status in the ensuing conflict. As was generally the case, the scientist with the priority of discovery was given the honor of naming the new element. Urbain decided that the element corresponding to Mendeleev's atomic mass number 70 would be named néo-ytterbium (Yb) and the one corresponding to 71 would be named lutetium (Lu). In 1911, Urbain made another dramatic claim, in the intervening years the chemist had managed to isolate a third elementary component of the ytterbia composite. The French chemist claimed that this element corresponded to the atomic mass number 72 of Mendeleev's periodic system and proposed the name celtium (Ct). In this way, Urbain claimed that he had filled in the table between thulium and tantalum. However, the empirical evidence Urbain collected in 1911 using the repeated fractionation method was not as decisive as his 1907 data and was thus not generally accepted within the international chemical community.

In chapter one, we briefly discussed the British chemist Henry Mosley's contributions to atomic theory. In 1913, Mosley showed empirically that that Mendeleev's periodic table was inconsistent because it relied on atomic masses to organize the elements periodically, instead a more reliable indicator of periodicity laws and the elements' general chemical nature was their atomic number Z. While this notion had already been proposed by Bohr, it was actually Moseley's experimental work in Manchester under Rutherford, and in collaboration with Charles G. Darwin, on x-ray spectroscopy that allowed him to develop the empirical law that bears his name. While the origin of x-ray spectroscopy is a story that is too involved for a full exposition here, a few words on the topic are relevant to our discussion.[119]

Towards the end of 1912, Moseley had read about the various experiments that Max Laue had designed in Munich, carried out by Walter Friedrich and Paul Knipping, on the interference of x-rays by use of a copper sulfate crystals and the extensive diffraction

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[119] A thorough exposition of x-ray spectroscopy can be found in: Bruce Wheaton, *The Tiger and the Shark: Empirical roots of wave-particle dualism,* (Cambridge: Cambridge University Press, 1983).

experiments that the team of W.H. and W.L. Bragg had carried out in England, and proposed to corroborate their results and then extend them to gamma-ray (γ-ray) phenomena.[120] While the physical understanding of x-rays was by no means universal in early 1912, Sommerfeld's detailed theoretical study of x-ray diffraction was surely representative of the latest research. Since 1900, Sommerfeld had been one of the early and most influential physicists to study x-ray phenomena. After his initial insights which led to the proposition of x-rays being modeled by square pulses of electromagnetic radiation in a Fourier superposition called "impulses," he subsequently developed a model of x-rays that cast them as distinctly dualistic radiation phenomena that could simultaneously explain both aperiodic and periodic behavior.

In Laue's experiments of 1912 the experimental team was looking for possible interference effects from the periodic components of the x-rays, also called the "fluorescent" or "secondary rays," as these were considered to have typical wave-like characteristic properties including partial polarization, frequency homogeneity, and temporally extension. In contrast, they believed that the main impulse components of the x-rays, which were in Sommerfeld's conception aperiodic, unpolarized, and clearly frequency inhomogeneous would not show any interference effects.[121] This clear physical duality was quickly negated to some extent, by Lorentz who argued that Sommerfeld's square impulse was untenable and that irregularly shaped pulses could theoretically lead to interference. If this were the case, x-rays would be fully comprehensible only if wave and corpuscule effects were simultaneously explained. The Braggs' 1912 experiments and their subsequent explanations of x-ray diffraction phenomena in a crystalline structure corroborated Lorentz' theoretical suppositions and made it evident that the full composite x-ray was interfering, not just homogenous secondary rays.

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[120] Wheaton, *The Tiger and the Shark,* 212-213.
[121] Ibid., 202-203.

The impulse aspect of the x-ray model was certainly still necessary for explaining phenomena such as the ionization of a gas by passing x-rays, however, the empirical evidence for x-ray interference began to shift the characterization of these "rays" more in line with that of optical light waves. Moseley quickly capitalized on this notion by noticing in January 1913: "[X-rays] are a kind of wave with properties no wave has any business to have."[122] His and Darwin's extensive program of research using crystal diffraction methods to perform x-ray spectroscopic analysis between 1913 and 1915 was critical to later developments in this field. However, World War I interfered with their progress as both Moseley and Darwin enlisted in the war effort. As we noted in the last chapter, Moseley was tragically shot and killed during the battle of Gallipoli in August, 1915.

After the war, the division of labor around experimental work regarding x-ray spectroscopy shifted noticeably. As Bruce Wheaton notes in *The Tiger and the Shark*, his extensive study of empirical contributions to the quantum revolution:

> [After World War I] the most important work [on x-ray spectroscopy] was done outside Britain and Germany. Although important background work had been accomplished in Rutherford's Manchester laboratory, the intervention of the war effectively stopped this research. … [Subsequently,] the significant empirical work came from America and … France.[123]

As we shall see in this priority conflict, while Wheaton is correct to notice general trends in the shifting landscape of various national experimental research programs after the war, it seems a bit limiting to say that the significant empirical work in x-ray spectroscopy came solely from France and the United States. The story of element 72 shows that Denmark, specifically Bohr's institute in Copenhagen was also a significant player.[124]

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[122] Ibid., 199.
[123] Ibid., 260.
[124] We should also note that the physicist Manne Sieghahn in Sweden was very influential in establishing the early methods of x-ray spectroscopy.

As we noted earlier, Urbain had attempted to isolate and identify element 72 as the rare earth metal celtium (Ct) in 1911 via the method of repeated fractionation. This process had produced ambiguous results which he tried unsuccessfully to corroborate using the newly discovered experimental technique of x-ray spectroscopy in 1914. With the onset of the war and the realization that even with this new technique he had failed to convince the scientific community of the validity of his discovery, Urbain temporarily set his program aside. In the spring of 1922, Urbain realized that the techniques of x-ray spectroscopy had advanced significantly in the intervening years and decided to reinvestigate the composition of his ytterbia mineral sample. In the post World War I international scientific community, one of the leading x-ray laboratories in the world was conveniently Maurice de Broglie's laboratory in Paris. Urbain was soon teamed up with Alexandre Dauvillier and together they set out to identify the various components of ytterbia. While the results were far from irrefutable, they consisted of two faint lines that loosely corresponded with Moseley's Law; they were nonetheless heralded as conclusive enough to give the priority for the 1911 discovery of the rare earth metal celtium to Urbain.

In 1921 Niels Bohr had written a seminal paper on the periodic structure of the elements based on his latest quantum atomic theory. When he realized in the spring of 1922 that Urbain and Dauvillier were claiming that they had corroborated Urbain's discovery of celtium as the rare earth metal with atomic number Z = 72, he knew they must be mistaken. From his atomic theory, it was clear to Bohr that Z = 72 could not be a rare earth metal at all; instead, it should undoubtedly be a homologue to zirconium. In the summer of 1922, Bohr convinced two experimental physicists George de Hevesy and Dirk Coster to study the case of Z = 72 and verify that his theory of periodicity was in fact correct. Bohr had been close with Hevesy, a Hungarian chemist who would later win the Nobel Prize in Chemistry, since 1911 when they both had worked in Rutherford's laboratory in Manchester, while Coster was a physicist who had received his doctorate while working under Ehrenfest at the University of Leiden.

Hevesy and Coster began their experiments in the fall of 1922. By late November, they had preliminary spectroscopic evidence derived from samples of Norwegian zirconium-rich minerals that Bohr's assertion about Urbain and Dauvillier being mistaken was in fact correct. The element identified by Z = 72 corresponded not to a rare earth metal, as the French team had assumed, but to a new element that was a homologue to zirconium. After rushing to obtain irrefutable evidence of this claim, Hevesy and Coster were able to phone Bohr in Sweden on December 9th to relay the new findings.[125] As we noted earlier, two days later, Bohr announced these same findings to the larger scientific community in his Nobel Lecture. On January 2nd, 1923, the group from Copenhagen published their results with six distinct lines in the x-ray spectrum that agreed precisely with Moseley's predicted values. Hevesy and Coster claimed clear priority for the discovery while making sure to discredit the work of Urbain and Dauvillier. In doing so, they usurped the honor of naming their newly discovered element and decided on hafnium (Hf).

At first, confusion reigned in London as two different names for Z = 72 came out of Copenhagen identifying the new element. In their original letter submitted to the journal *Nature*, Hevesy and Coster had used the term hafnium as the name for the newly discovered element in honor of the Latin equivalent of "Copenhagen." However, right before the letter's publication a debate arose in Bohr's institute with regards to the naming of the element. While Coster and Kramers preferred to stick with hafnium, Bohr and Hevesy preferred the name danium in honor of the full Danish nation. The decision was finally made in favor of danium and

[125] Helge Kragh, and Peter Robertson, "On the Discovery of Element 72," *Journal of Chemical Education, Volume 56, Number 7*, (1979), 456 – 459.

edits were quickly dispatched to London to reflect the change. Somehow, the edits were missed because in the January publication of Nature the name hafnium remained. Meanwhile, in Copenhagen, Bohr's institute was trumpeting the newly discovered element of danium. While this naming miscue had no lasting effects it was emblematic of the confusion surrounding the whole process of discovery.[126]

The double naming of hafnium was not the only source of confusion and controversy coming from London. Soon after the initial letter appeared in *Nature*, Adam Scott, one of the most esteemed chemists in England, challenged the Copenhagen group for priority. He had studied a sample from New Zealand in 1918 from which he was able to isolate what he thought was a new oxide. While he hadn't published his results in 1918, after reading about Urbain and Dauvillier's discovery in January 1923 he made the claim that the new oxide he had isolated five years earlier was none other than an oxide of hafnium. The British press, driven by national pride, declared immediate victory in the priority dispute but the eminent scientists Rutherford and W.H. Bragg, moved quickly to facilitate a more bilateral resolution with the Copenhagen group. After sending the sample to Copenhagen for analysis, it was determined that the oxide was a composite of other known elements and did not have anything to do with hafnium.[127] The "priority dispute" with Scott was a non-starter since it was never based on concrete empirical evidence and thus quickly faded into obscurity. However, the controversy with the Parisians Urbain and Dauvillier was another case entirely.

On February 17th, 1923, Urbain and Dauvillier responded to these claims of a newly discovered element publicly, by publishing a note in Nature titled: "On the Element of Atomic Number 72." In this note they claimed that Coster and Hevesy's results were "significant" only in that they had been able to isolate significantly more celtium than the French research group

[126] Kragh and Robertson, "On the Discovery of Element 72," 457.
[127] Kragh, "Anatomy of a Priority Conflict: The case of Element 72," 280.

had. However, they unequivocally rejected the notion that the Copenhageners had discovered a new element and accused them of trying to "discredit [the Frenchmens'] proper results."[128] A week later Coster and Hevesy responded to these accusations with a note of their own. They submitted a letter to *Nature* titled: "On the New Element Hafnium," the Copenhageners took issue with the characterization of their research and attacked the very notion of the French group's claim that element 72 was a rare earth metal called celtium. According to further studies carried out by Hevesy and others over the previous two months they had gathered irrefutable proof that element 72 was definitely "hafnium" with chemical properties similar to those of zirconium and different from those of other rare earth metals. In addition, optical spectroscopic analysis showed that the element isolated by Coster and Hevesy (hafnium) was not the same as the element isolated by Urbain in 1911.[129]

This new evidence was definitely a blow to the French group's claims of priority but it was not yet crippling. While they recognized that the 1911 sample isolated and studied by Urbain had not been element 72, they were not willing to cede on the notion that their 1922 research was completely invalidated. Surely, they argued, the two x-ray spectroscopic lines identified in 1922 were associated with Z = 72, whether or not this element was a rare earth metal or a homologue to zirconium, and that was enough to grant them priority. In an attempt to buttress their claims, Dauvillier performed the 1922 x-ray spectroscopy experiments again and was able to replicate the results from the previous year, finding two characteristic lines that he claimed agreed with Moseley's predicted values for Z = 72. He finally published the plates with these lines in December, 1923 but the lines he had found were still quite faint and not definitively conclusive. In the meantime throughout 1923, the two groups from Paris and

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[128] G. Urbain and A. Dauvillier, "On the Element of Atomic Number 72," *Nature,* 111 (17 February, 1923), 218.

[129] D. Coster and G. Hevesy, "On the New Element Hafnium," *Nature,* 111 (24 February, 1923), 252.

Copenhagen traded barbs and accusations entrenching themselves into their respective priority positions.[130]

For Bohr and the Copenhageners it was clear that the Parisians had no leg to stand on in this dispute, and as a final blow they attempted to call into question the very scientific integrity of both Urbain and Dauvillier. They were not satisfied with simply showing that the optical spectrum for celtium that Urbain had produced in 1911 was not in agreement with hafnium's spectrum or that the chemical properties for Z = 72 were not similar to rare earth metals but to zirconium as Bohr's atomic theory had predicted. In March of 1923 they attacked the very foundation of Urbain's academic reputation. Two chemists from Bohr's institute H.M. Hansen and S. Werner set out to re-settle the old priority dispute between Urbain and the Austrian chemist Carl von Welsbach in 1907 with respect to the discoveries of elements 70 and 71.[131] Since they knew that Urbain's 1911 spectrum did not correspond to element 72, they decided to try and figure out what it did correspond to. Using spectroscopic analysis and comparing various spectra from Welsbach and Urbain's 1907 results, Hansen and Werner showed that the optical spectrum which Urbain claimed to be celtium (element 72) in 1911 actually corresponded to Welsbach's 1907 spectrum for element 71- the element Urbain had named lutetium. This in turn could only mean that the evidence Urbain had used to show his priority of discovery for lutetium in 1907 was not actually a spectrum of lutetium at all.[132]

In 1909, the *International Committee on Atomic Weights (ICAW)* had decided in favor of the French chemist in the Urbain-Welsbach priority dispute over elements 70 and 71. While the vast majority of the international chemical community agreed with this decision, Welsbach had never accepted his defeat in large part because, in 1909, Urbain had been serving as the

[130] Kragh, "Anatomy of a Priority Conflict: The case of Element 72," 284-285.
[131] H. M. Hansen and S. Werner, "On Urbain's Celtium Lines," Nature, 111 (7 April, 1923), 461.
[132] Kragh, "Anatomy of a Priority Conflict: The case of Element 72," 286-287.

Committee's chairman. In the summer of 1923, Hevesy travelled to Berlin where he convinced the German equivalent of the ICAW the *Deutsche Atomgewichtskommission (DA)* of Urbain's lack of priority with respect to all three elements 70, 71, and 72. Armed with the new evidence presented by Hevesy and uncovered by the series of Copenhagen experiments, Urbain's priority claims to elements 70, 71, and 72 were all revoked. In 1924 the DA officially recognized Hevesy and Coster as having priority in the current dispute over element 72 thereby naming it hafnium and in addition they decided to change element 71's name from lutetium to cassiopeium (Welsbach's name preference) and element 70's from néo-ytterbium to ytterbium in honor of the great Swiss chemist Marignac.[133]

Dauvillier's reputation was not marred as publically or dramatically as Urbain's but within the physics community Bohr and other Copenhageners made it clear that his experimental results were not to be trusted. In a letter to Rutherford in March of 1923 Bohr wrote: "…we have reason to believe that the observation of Dauvillier is a self-illusion."[134] Bohr was of course referring to the two faint lines in the x-ray spectrum from the supposed celtium sample Dauvillier had analyzed for Urbain the previous year. The accusation of self-illusion should not be taken lightly as it points at best to mental deficiency, when interpreted as a subconscious act, and possibly even a case of scientific fraud, when interpreted as an act of conspiracy between Urbain and Dauvillier. In either case, Dauvillier's reputation within the greater scientific community was certainly being thrown into question by a Nobel laureate and one of its foremost members.

Whether these attacks were a result of a strategic assault on the Parisians by the Copenhageners or they were simply a result of many individual events within a complex scientific dialog is not clear, but what is clear is that by the end of 1923 this scientific priority

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[133] Ibid., 287.
[134] Ibid., 284.

conflict had escalated dramatically and had taken on patriotic and nationalistic overtones that resonated with the quantum divide we described in the previous sections. One can naturally assume that the various national press corps would pick up on this story and inflame the tensions, and that is precisely what happened. However, what makes this story emblematic of the dynamics within the international quantum physics community of the early 1920s and thus important to our narrative are the combative and intractable reactions within the scientific community itself.

This priority conflict exemplifies the fissures in the quantum community we have discussed in this chapter. The quantum bloc discussed earlier, and comprised of the four quantum schools in Germany, Holland, and Denmark certainly rallied behind Hevesy and Coster's priority claim, while the French rallied unilaterally and forcibly behind Urbain and Dauvillier. On the other hand, the British and the Americans had a more nuanced reaction with some of their leading physicists (Rutherford and Bragg) siding with the Copenhageners and many in the chemistry community backing the French cause. This scientific nationalism could not have been made any more explicit then when, at Rutherford's behest, Hevesy attempted to publish a paper on the chemical properties of hafnium in the British Journal *Chemical News* only to be told by the editor and president of the Chemical Society of London W.P. Wynne:

> We adhere *to* the original word celtium given to **it** by Urbain as a representative of the great French nation which was loyal to **us** throughout the war. We do not accept the name which was given it by the Danes who only pocketed the spoil after the War.[135]

Another clear example of this scientific nationalism came in 1925; a year after the DA had stripped Urbain of any priority in the discovery of elements 70-72, the *International Committee on Chemical Elements (ICCE),* at that time dominated by French, American, and British chemists, published a periodic table of elements without including any mention of

[135] Ibid., 294.

element 72![136] Due to the climate of the times, within three short years the initial priority dispute had evolved to a tense international standoff that threatened to nationalize scientific knowledge itself. This standoff was not actually resolved until after 1930 when the international scientific community reorganized itself, ended the international boycott, and reverted to a paradigm of inclusivity and cooperation. The final resolution was a diplomatic compromise on both sides that appeased the majority of the scientific community while being outwardly rejected by both the French scientific establishment and by pockets of German and Austrian chemists who were loyal to Carl von Welsbach. Element Z = 70 was left as ytterbium (Yb), Z = 71 was named lutetium (Lu), and Z = 72 was finally recognized as hafnium (Hf).

The preceding story of innovation has been a case study of the important effects of nationalism on the development of science in post World War I Europe. While the priority dispute of the discovery was eventually settled via negotiations and compromises that left many parties unhappy, the science itself was in the end definite and irrefutable. The elements 70, 71, and 72 could be empirically isolated and exhaustively studied in any laboratory anywhere in the world. In this case the reproducible nature of science was an important criteria for the validation of innovation. However, one cannot discount the very real effects of patriotic pride and prejudice that molded and amplified the scientific dispute. These forces would carry over seamlessly to other subjects of study, including the evolving quantum theory. As the young Louis de Broglie was completing his doctoral research in Paris, working to revolutionize the foundations of quantum theory, this controversy over “hafnium” was in full swing. It had tainted his colleague Dauvillier, and by association all French science including the work of the two de Broglie brothers.

[136] Ibid., 291.

Chapter 3: Synthesizing Duality- The Emergence of Wave Mechanics

> *[Wave-Particle duality] is like a struggle between a tiger and a shark, each is supreme in his own element, but helpless in that of the other. – J.J Thomson, 1925*[137]

3.1- Introduction

In Mara Beller's *Quantum Dialogue*, the author uses her analysis of the so-called "creative dialogical flux" associated with the evolution of quantum theory in the 1920s to argue against a distorted quantum mythology that proposes a picture of an inevitable, unified, and homogenous Göttingen-Copenhagen Interpretation that became quantum orthodoxy. In contrast to this picture, Beller shows that when the full breadth of the scientific dialog between various quantum innovators almost exclusively from the Göttingen-Copenhagen group is studied it invariably leads to the conclusion that the details of the innovations were highly nuanced, complex, varied, tentative, and always in flux.[138] While Beller declared that her narrative showed the contingent nature of the later Göttingen-Copenhagen orthodox interpretation, there was little or no attempt to extend her analysis to alternate formulations or interpretations of quantum theory. In the following discussion we will do just this.

Before embarking on an analysis of the scientific dialog between many of the most important inventors of quantum mechanics it was critical that we first set the social, political, economic, and academic context within which the larger international physics community worked. In chapter two we saw that as a result of the after effects of World War I the physics community was fractured and under a considerable amount of tension. The four quantum schools that we highlighted in Munich, Göttingen, Copenhagen, and Leiden were instrumental in

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[137] Wheaton, *The Tiger and the Shark,* Title page. Quote taken from: Thomson, J.J. 1925. "The Structure of Light", Phil. Mag., ser. 6, 1, 118I-1196.
[138] Beller. *Quantum Dialogue,* p. 2

establishing an academic network of physicists that remained highly influential for decades to come. While not completely defining the context of innovation that resulted from quantum physics research in the 1920s, the influence of these schools is certainly an important factor to consider. The following discussion looks to reexamine the emergence of wave mechanics within this context.

At the end of chapter one, we saw that by the time of the Bohr Festival in the summer of 1922 at the University of Göttingen, quantum theory was becoming synonymous primarily with two lines of inquiry: atomic physics and extensions of Einstein's light quantum hypothesis. Bohr's group in Copenhagen and Sommerfeld's group in Munich had contributed substantially to the extension of the Bohr-Sommerfeld atomic theory based on hybridized celestial mechanics, adiabatic invariants, and various selection rules. It was clear that while the model was fairly successful in modeling the hydrogen atom and could explain phenomena such as the Stark and normal Zeeman effects and fine spectral line splitting, it was nevertheless a colossal failure when extended to more complex atoms like helium.

We also noted in chapter one that Bohr, and other prominent physicists of the time, remained steadfastly unconvinced by Einstein's extensions to his light quantum hypothesis, specifically by the notion of a physical quantum of light carrying both characteristic energy and momentum. These skeptics believed that Einstein's theories associated with his studies of the photoelectric effect and later extended to atomic radiation were highly speculative and unsubstantiated by empirical evidence. Most traditional quantum narratives claim that prior to 1922, Einstein was essentially alone in his belief of the light quantum hypothesis. They typically quote the skeptics like Millikan and Bohr as well as Einstein himself who in 1918 wrote to Michele Besso: "I no longer have doubts about the reality of light quanta—even though I'm still quite alone in this conviction." These narratives typically paint the 1922-23 Compton scattering experiments as the major turning point, after which, the majority of physicists believed in Einstein's light quantum hypothesis. Once Arthur Compton's scattering experiments had converted the majority of the physics community, wave particle duality became an accepted conceptualization of light and the following year Louis de Broglie was able to extend this notion to matter waves thus paving the way for the rise of the new quantum mechanics in 1925.

Once again we need to take issue with this traditional story. It reduces a complex and nuanced narrative and distorts it by simplifying causal links between various innovative lines of inquiry. As is generally the case, a more thorough examination of the historical evidence reveals the lost nuance. As we shall soon see, the debate surrounding Einstein's light quantum hypothesis stretches back much farther than the arbitrary setting revealed in most quantum narratives. Compton's 1922-23 x-ray scattering experiments and his interpretation of the increase in the secondary radiation's wavelength and its corresponding trajectory as corresponding precisely with a change in the momentum of a scattered electron originating from a collision, was groundbreaking because while a significant part of the physics community was aware of the dual nature of electromagnetic radiation, this notion had not yet been placed on the sound scientific footing of a synergistic relationship between theory and experimental verification.

By the time Compton put all of this together in his laboratory at the University of Washington in St. Louis, a significant trail of theoretical and experimental work had already focused on this problem. Compton's ideas were not born into a vacuum and they did not arise as a result of a spontaneous reflection on some forgotten light quantum hypothesis postulated by Einstein in a 1917 paper. Compton's scattering theory was a direct result of a rich and layered scientific dialog. In chapter one, we noted some of this complexity in examining the early rise of quantum theory and the first applications of Einstein's light quantum hypothesis. We saw how Ehrenfest, Nernst, Poincaré, and Stark were early adopters of this conceptualization and how Bohr, Debye, Millikan, Planck, and Sommerfeld were early skeptics of the physical consequences of this hypothesis. More importantly we saw that the reactions to Einstein's theory cannot be categorized simply as for and against. In general the reactions of physicists were more nuanced than a simple polarized picture would indicate. In fact, since most of these scientists' positions evolved over time the picture is far from static.

The history of the dual conceptualization of light radiation as wave and particle within the studies of natural philosophy and physics has been researched thoroughly and is well understood. The debates between Newton and Huygens over the true nature of light as a particle or wave have been extensively documented. The dynamics and swings in the "accepted" conceptualization of light over the following two-hundred years between particle and wave has been used by many historians of science to show the importance of contextualizing scientific knowledge within a period as opposed to granting it some objective truth.[139] However, as with all historical narratives that cover such a broad period, there arise simplifications and reductionisms that serve to distort the story. The oversimplified narrative is usually presented along the lines that the scientific community first accepted Newton's prescription of light as particles in the 18th century, only to see that paradigm shift in favor of Huygens' wave theories in the early 19th century, thanks to Young's double slit experiment, and

[139] For example: Thomas Kuhn, "Essential Tension," in *Essential Tension*, (Chicago: University of Chicago Press, 1977).

finally was revolutionized when both paradigms were fused together by Einstein's explanation of the photoelectric effect in 1905.[140]

While some version of this succinct narrative has been told and retold countless times by historians of science to dispel the notion that the pursuit of science is completely decoupled from human dynamics and that the old, heroic view of science as a linear journey towards objective truth is fallacious, modern historiography shows us that this may be good pedagogy, but it is undoubtedly bad history. Stripped of important context many of these narratives miss nuance and creative tensions that arise from the explorations of the particular dialectics at play. In chapter one, we identified several threads from the 19th century that were critical to the slow rise of the quantum revolution. Among these was the notion that due to the highly developed analytical tools applied to a wide range of natural phenomena there was a natural plasticity, or flexibility, in developing theoretical models by extension or analogy. This practice resulted in the acceptance of a plurality of simultaneous representations of nature that may have seemed to contradict each other in key ways at the time, but were accepted as transitional theories.

Thanks in large part to an early manifestation of the modern resonance between experiment and theory in the natural sciences; Thomas Young and Augustin-Jean Fresnel were able to establish the wave theory of light in the early 19th century. From that point on, it seems clear that throughout the remainder of the century the predominant mode of conceptualization of radiation phenomena was wave-like. However, the roots of the dualistic representation of light radiation as both particle and wave were laid by 19th century mathematicians and mathematical physicists like Sir William Rowan Hamilton who between 1825 and 1833 set out to

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[140] In addition to Kuhn see: Ralph Baierlein, *Newton to Einstein: The trail of Light*, (Cambridge: Cambridge University Press, 2001); Michael Sobel, *Light,* (Chicago: University of Chicago Press, 1989); Arthur Zajonc, *Catching the Light: The Entwined History of Light and Mind,* (Oxford: Oxford University Press, 1995); and David Park, *The Fire Within the Eye*, (Princeton: Princeton University Press, 1999).

unify the seemingly disparate analytical representations of classical mechanics and optical phenomena. In striving for the unification of all mathematical representations of natural phenomena under one continuous, over-arching system of partial differential equations, Hamilton was not claiming that the nature of light was both particle and wave-like, instead he was using formal analogy to develop a single formalism that would describe both the trajectory of a massive particle and the propagation of a wave.

Hamilton made significant strides towards this unification by extending the works of Euler, Maupertuis, Lagrange, and others, but his legacy of unification was somewhat forgotten in the latter half of the 19th century as mathematicians and mathematical physicists like Jacobi, Liouville, and Poincaré worked to extend his analytical work on classical mechanics by decoupling it from his work on optics.[141] We have already discussed how important hydrodynamic analogies were to Lord Kelvin and J.J. Thomson's work on vortex atoms and to the subsequent electronic-atomic models developed throughout the first decade of the 20th century. Hamilton's unification program is another example of the willingness of 19th century scientists to be open to a methodology of analogous extensions in their analytical representations of natural phenomena.

It is important to note that although it was certainly not a mainstream conceptualization, the notion of light being both wave and particle-like was certainly taken up by some physicists in the latter half of the 19th century. One clear example of this was the work done by Baron Nikolai Dellingshausen who in 1872 published his treatise on the *Foundations of a Vibration Theory of Nature*. In this treatise Dellingshausen described atoms as standing waves

[141] Jammer, *The Conceptual Development of Quantum Mechanics,* 243.

and generally referred to massive bodies as "extended centers of vibrational motions."[142] Obviously, this is not the place for an exhaustive look at 19th century unification theories but it is important that these examples highlight the fallacy of the notion that in the latter half of the 19th century there was a homogenous paradigm identifying light strictly as a wave.

By the end of the first decade of the 20th century there was little doubt that alpha and beta radioactive emissions were particles thanks in part to their ionization capacities and the scattering experiments that physicists like Rutherford had made famous. However, the true nature of x-rays was a different story. As we discussed in chapter two, from the moment x-rays were discovered by William Röntgen in 1895, there was a shroud of doubt surrounding their origins and their physical characteristics. The experimental evidence that accumulated revealing the particle-like characteristics of x-rays was unmistakable; to this end J.J. Thomson had already identified their capacity to ionize a gas in 1896.[143] But the uncertainty around their true nature remained due to experiments showing x-ray diffraction such as those done in the basement of Sommerfeld's institute in 1912. British physicist William H. Bragg summed up the bipolar nature of x-ray theories perfectly in a review of x-rays towards the end of 1912: "The problem becomes, it seems to me, not to decide between two theories of x-rays, but to find, as I have said elsewhere, one theory which possesses the capacity of both."[144]

The initial qualitative investigations into the nature of radiation were performed while studying the characteristics of both x-rays and gamma rays. As mentioned in the previous paragraph, J.J. Thomson had studied the ionization of gasses due to x-rays as early as 1896 and as a result had developed a pseudo corpuscular theory of radiation. In 1904 A.S. Eve published a paper "On the secondary radiation caused by the beta and gamma rays of radium," where he

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[142] Ibid. p. 246
[143] J.J. Thomson, "The Röntgen rays," *Nature* 53, (1896): 391-392.
[144] W.H. Bragg, "X-rays and crystals," *Nature* 90, (1912): 360-361.

noted that upon scattering the secondary gamma rays differed in their energy from the primary rays. Further gamma scattering experiments were conducted by R.D. Kleeman, J.P.V. Madsen and D.C.H. Florence between 1908 and 1910. In 1912 D.A. Sadler and P. Mesham moved to extend these qualitative investigations of gamma scattering to x-ray scattering and showed conclusively that both gamma rays and x-rays were qualitatively indistinguishable. By the onset of World War I, x-ray diffraction effects had been studied in Munich and as Bragg's quote illustrates there was recognition by some physicists of a need to find a theory that could accommodate both undulatory and corpuscular representations of radiation phenomena.

J.J. Thomson's atomic model had been the predominant explanatory framework for over a decade and by World War I it was clear that it was not sufficient to describe and explain the empirical results coming from the gamma and x-ray scattering experiments. The development of the Bohr- Rutherford and Bohr-Sommerfeld atomic theories was critical to the explanation of these phenomena and others. In 1921, basing his work on a combination of the Bohr-Sommerfeld quantum theory and Einstein's extended theory of light quanta which not only proposed mechanisms for absorption and emission of quanta but also imbued these with a linear momentum, Robert Emden found that along with the typical derivations of established laws of radiation like Planck's radiation law he could also derive the formula for the Doppler effect, which had always been considered a quintessential wave-like phenomenon. A year later Schrödinger provided a much more detailed and thorough analysis of the Doppler effect in his paper on "The Doppler principle and Bohr's frequency condition." In this analysis Schrödinger derived, for the first time, a complete corpuscular explanation of this seemingly undulatory phenomenon by showing how one could arrive at the actual experimentally observed Doppler shifts of spectral lines.[145]

As has been shown by the stance of physicists like Bragg, de Broglie, and Schrödinger there was a clear momentum within some parts of the physics community to recognize the dual nature of light as both corpuscular and wave-like well before 1923. We saw in chapter one how Bohr was willing to accept certain consequences implied by Einstein's quantum hypothesis, like the discreteness of energy states and the mechanisms of absorption and emission of radiation, yet he was vehemently opposed to the notion of a physical quantum of light with characteristic energy and momentum.[146] However, Bohr was not alone in his reluctance to accept the consequences of a dual picture of radiation; these were not generally accepted within the scientific community until much later.

Many historical narratives of this time have pointed to either Compton's 1922-23 experimental results or Einstein's 1921 Nobel Prize in physics for his "discovery of the law of the photoelectric effect" as evidence that the corpuscular nature of light had been fully accepted by the early 1920s. However, drawing this conclusion is somewhat problematic when the full breadth of the historical evidence is examined. In the following discussion we will examine these two events within their proper contexts.

After Arthur Eddington's astronomical observations in May 1919 had helped verify the theory of general relativity, Einstein saw his popularity skyrocket. By all accounts he quickly became the most famous and publically recognizable physicist in the world. It has become an

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[145] Ilaria Bonizzoni and Giuseppe Giuliani, "The undulatory versus the corpuscular theory of light: the case of the Doppler effect," Preprint: http://fisicavolta.unipv.it/percorsi/pdf/undulatory.pdf. p. 2-3.

[146] Please see Chapter 1 where we saw that Bohr celebrated Einstein's 1917 paper describing the mechanisms of radiative emission and absorption while decrying his notion of a physical quantum of light with characteristic energy defined by $E=h\nu$ and a characteristic momentum defined by $p=h\nu/c$.

interesting footnote in the history of 20th century physics that the Nobel Committee of Physics awarded Einstein his prize, not for his theory of relativity but for his work on the photoelectric effect. Is it simply a case of historical irony that the Nobel committee recognized Einstein's scientific accomplishments not for the work that had just made him a world figure but for work he had done almost two decades earlier?

The effects of political and cultural forces on science are nowhere more evident than in this episode regarding Einstein's Nobel Prize of 1921. It may seem a bit absurd in retrospect but a great debate erupted within the Nobel Committee of Physics in 1920 and 1921 as to whether Einstein should receive a Nobel Prize in physics at all. In 1920, Nobel nominations for Einstein came in from notable physicists from around the world like Lorentz, Bohr, and Planck claiming that with the confirmation of his theory of general relativity he had "placed himself in the first rank of physicists of all time."[147] However, these nominations were set aside due in large part to non-scientific objections from esteemed physicists like Philipp Lenard who was openly anti-Semitic and had referred to relativity theory as highly speculative and philosophical, something he considered typical of "Jewish science". Svante Arrhenius a Swedish physical chemist and member of the Nobel committee, who in 1922 became a founding member of the Swedish eugenics institute known as the State Institute for Racial Biology, wrote an internal report echoing Lenard's criticisms and objecting to Einstein's nomination. As a result Einstein was passed over for the 1920 Nobel Prize.

In 1921, tensions over the clear bypassing of Einstein for physics' greatest award rose significantly as the Nobel committee received 14 official nominations on Einstein's behalf. Eddington went as far as to compare Einstein to Newton and Marcel Brillouin pointed out that

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[147] Walter Isaacson, *Einstein: His Life and Universe,* (New York: Simon and Schuster, 2007), 311.

fifty years in the future the omission of Einstein's name from the list of Noble Laureates would be seen by all, as a great folly of the Swedish Academy of Sciences. Even in the face of such vehement support for Einstein the Nobel Committee refused to fully succumb to the pressure. They initially decided against awarding Einstein, or anybody else, the 1921 Nobel Prize for physics voting instead to leave it vacant and tabling a final vote on the matter. Nevertheless, in 1922, the Nobel Committee of Physics added a member that ended the Einsteinian impasse. Seeing that the tension surrounding Einstein's nomination was centered on the acceptance of relativity theory, Carl Oseen had instead nominated Einstein for the 1921 award for his work on the photoelectric effect. The following year Oseen was made a member of the Nobel committee and he was finally able to push through Einstein's nomination only after agreeing to certain caveats. The compromises Oseen negotiated were on two fronts. First, Einstein would not win the Nobel Prize for relativity theory and second, he would be recognized for the discovery of the "law" of the photoelectric effect as opposed to the underlying quantum hypothesis. In this way the Nobel committee recognized only the predictive power of Einstein's photoelectric effect formula and avoided the controversy surrounding the nature of light. As Bohr was receiving his Nobel Prize in 1922 "for his services in the investigation of the structure of atoms and of the radiation emanating from them," the Nobel Foundation simultaneously awarded Einstein the 1921 Prize because his photoelectric law had made it possible for Bohr to develop his atomic model.[148]

It is no accident that Oseen chose to nominate Einstein for his work on the photoelectric effect. Around the same time that Schrödinger was working on using the corpuscular nature of light to derive the Doppler shift in spectral lines, Oseen was applying the corpuscular theory to

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[148] Ibid. p. 312-314.

Maxwell's equations and showing that these admit corpuscular solutions.[149] The irony of this historical episode comes from the fact that even as the Nobel Foundation awarded Einstein the 1921 Nobel Prize for physics in December 1922, the Swedish Academy of Sciences was still reluctant to recognize the corpuscular nature of light. Far from an affirmation of wave-particle duality, it highlights the complex nature of political and cultural forces in the historical development of physics.

In 1923, as Compton was developing his theory of x-ray scattering in St. Louis based on an exchange of momentum and energy during a collision between incident x-ray quanta and electrons scattered from target atoms, Peter Debye was independently working on a theory that proposed the very same explanation for the longer wave-lengths of the resultant x-rays. Debye published his paper from Zürich one month before Compton's first report was made public and while he was attempting to convince colleagues to take up the challenge of experimentally verifying his theory he discovered Compton's publication and realized he had been beat to the punch.

At the third Solvay Council, in 1921, Maurice de Broglie,[150] who had served as a secretary at the first two Councils, was reprising his role with the added bonus of presenting a short talk on experimental techniques associated with atomic physics. In this talk de Broglie discussed experiments he had performed in which he used x-rays to eject electrons from various elements and study their resultant "electron particle velocity spectra". The results of these experiments had confirmed Bohr's latest atomic model (published in Nature just before the Solvay Council) thereby experimentally matching the atomic electronic structure predicted.

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[149] Jammer, *The Conceptual Development of Quantum Mechanics,* 171-172.

[150] Maurice de Broglie was the older brother of Louis de Broglie who was made famous by his later work applying Hamilton's wave mechanical analogy for optics to the elucidation of matter-waves.

However, de Broglie noted that there was an aspect to the experimental results that did not match the accepted wave nature of x-rays (a form of electromagnetic radiation). As it happened, the velocities of the ejected electrons were insensitive to the distance between the x-ray source and the target atoms. If x-rays were waves as Bohr and many others believed they were, as the wave-front spread throughout space the intensity of the x-ray would dissipate and the resulting velocities of the emitted electrons would be highly dependent on the position of the x-ray source. Obviously, the lack of dependence of electron velocities on the source-target distance was a clear indication to de Broglie that the x-ray radiation “must be corpuscular…or, if it is undulatory, its energy must be concentrated in points on the surface of the wave.”[151]

In this series of experiments in early 1921, Maurice de Broglie had helped confirm Bohr’s latest atomic model and had simultaneously begun a movement to try to confirm the corpuscular nature of electromagnetic radiation. However, de Broglie did not stop there, in a 1922 book on x-rays he wrote:

> There is something kinetic in the vibratory radiation, and something periodic in the projections of corpuscles; all of this suggests, more and more every day, that the same reality manifests itself, sometimes under its kinetic face, some other times under its undulatory face.[152]

While this seems to be a surprisingly early statement of the possibility of an extension of Einstein’s wave-particle duality beyond radiation and into the realm of matter, de Broglie was not the first to propose this notion. Traditionally, this extension has been attributed to Maurice’s younger brother, Louis de Broglie. As we shall see shortly Louis’ innovations are due in large part to his associations with Maurice.

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[151] Mary J. Nye, “Aristocratic Culture and the Pursuit of Science: The De Broglies in Modern France,” *Isis*, Vol. 88, No. 3 (Sep., 1997): 412
[152] Ibid. p. 412

When Maurice de Broglie addressed the third Solvay Council and mentioned the dual faces of nature ("kinetic face" and "undulatory face"), it was not exactly something completely out of context. De Broglie was in fact one of the foremost x-ray experimentalists of his time and he was echoing something Bragg, and others, had posited much earlier. In addition to his duties as a secretary at the Solvay Councils Maurice de Broglie had built a reputation within the experimental physics community as someone on the leading edge of x-ray spectroscopy. In 1923, when Compton presented his famous interpretation of x-ray scattering experiments he had conducted between 1921 and 1923, de Broglie quickly ran his own experiments to confirm Compton's findings. During a meeting of the American Physical Society in Washington, D.C. in December 1923, Compton's findings came under attack from William Duane and others. De Broglie was present at that meeting and quickly jumped to Compton's defense by presenting his own photographic plates of x-ray scattering spectra that agreed with Compton's findings.[153]

3.2- Louis de Broglie - Prince of Waves

As Mary Jo Nye states, Louis de Broglie was "the one member of the Sciences Faculty [at the University of Paris] from social origins designated "grand proprietaire noble." The other 112 scientists appointed to the faculty from 1901 to 1939 were from middle class families who tended to be more sympathetic with the republican cause. While Jean Perrin's paternal grandparents were peasants, Madam Curie's parents were a school teacher and an administrator, and Paul Langevin's grandfather was a locksmith, Maurice and Louis de Broglie descended from the highest nobility including princes, dukes, ambassadors and cabinet ministers. Throughout his career in Paris, as the lone aristocratic scientist on the faculty, Louis

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[153] Jammer, *The Conceptual Development of Quantum Mechanics,* 170.

de Broglie was considered a bit of a loner and in biographical narratives has been characterized as a reclusive and marginalized figure. It is no wonder that he felt somewhat isolated because as 20th century aristocratic scientists the de Broglies were anachronistically caught between two worlds.

On the one hand the French Academy of Science had become highly proletarian at the dawn of the 20th century and on the other, the French noble class still expected their princes to find a vocation worthy of their status. This vocational expectation meant that the de Broglie brothers were expected to find careers at the highest levels of society allowing them to become social dignitaries and leaders. A career as a scientist was certainly seen as qualifying beneath their status. To his family's displeasure Maurice became a negative role model for the young Louis when he began splitting his time between his naval duties and his passion for science. He had long been a tinkerer of batteries, coils, and sparking apparatuses so he set out to formalize his studies of physics at the Sciences Faculty in Marseilles between 1897 and 1901. While in Marseilles, Maurice became fascinated with the newly discovered electrons, x-rays, and uranium rays. Worried that the future sixth duc de Broglie would not rise to a vocation worthy of his family's name, Maurice's grandfather wrote to him in 1900: "You are always thinking about scientific studies and ending your military career. Science is an old woman that one can court later and who does not fear the tributes of old men. Academies do not make for happiness."[154]

In 1906, Maurice and Louis' father died making Maurice the sixth duc de Broglie. Because Louis was seventeen years Maurice's junior and still only 14 years old, he was placed under his brother's partial guardianship. From this time on, their relationship was not simply one of normal siblings as it was infused with a parental air. Maurice had built a private

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[154] Nye, "Aristocratic Culture and the Pursuit of Science: The De Broglies in Modern France," 404.

laboratory adjacent to his residence in Paris which Louis visited at every opportunity. While these visits made a definite impression on the younger de Broglie he remained uncommitted to a future in the sciences. Meanwhile, advised by Paul Langevin, the duc Maurice de Broglie defended his doctoral thesis in physics before a Sorbonne jury in 1908. Langevin suggested to Nernst and others organizing the first Solvay Council of 1911 that the duc de Broglie be named as a secretary to the meetings and a co-editor of the subsequently published proceedings. Upon his return to Paris in November 1911, Maurice showed the young Louis the page proofs from the Council's proceedings and this became a turning point for the youngest de Broglie. Reading a first-hand account of scientific debates between the greatest physicists of the time on topics that were at the forefront of international physics research was absolutely transformative. Pauline de Broglie, Louis' older sister, remembered this as a seminal moment in his intellectual development. She stated that after reading the Council proceedings proofs:

> The amiable petit prince and charmer that I had known all through my childhood had disappeared forever. With a determination and an admirable courage he was transforming himself little-by-little every month into an austere scientist leading a monastic life.[155]

To this point in his educational career Louis de Broglie had been interested primarily in studying history, and for a short time law. In fact, he received his *licence ès lettres* in 1910 from La Sorbonne after studying history. But apparently, neither the studies of history nor law could measure up to the excitement he felt after reading Maurice's proofs in 1911. As Pauline rightly observed in her memoire, from 1911 on, the younger de Broglie became determined to become a world class physicist so he too could participate in future Solvay Councils.

During World War I the de Broglie brothers were both pressed into military service, Louis was part of the corps of engineers stationed at the Eifel Tower as a member of the

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[155] Ibid., 406.

wireless telegraph unit while Maurice finished the War working for the Ministry of Inventions in Paris. Unlike Sommerfeld in Munich or Bohr in Copenhagen, the de Broglie brothers were temporarily severed from their physics careers during the war years. However, after the War both de Broglie brothers returned to their careers in science, Maurice as an experimental physicist working independently in his laboratory and Louis as a student at the Collège de France. In the early 1920s Louis studied under Paul Langevin all the while working with Maurice at his private laboratory. He later recalled how Maurice impressed upon him "the importance and the undeniable accuracy of the dual particulate and wave properties of radiation."[156] Moreover, in autobiographical notes, he reminisced on his brother's influence on his later insights into wave particle duality:

> I had long discussions with my brother on the interpretation of his beautiful experiments on the photoelectric effect and corpuscular spectra… These long conversations with my brother about the properties of x-rays…led me to profound meditations on the need of always associating the aspect of waves with that of particles.[157]

In looking at de Broglie's influences there is no doubt that we should place his early exposure to his brother's laboratory work and Solvay Council proceedings at the forefront, but in particular we should single out Maurice de Broglie's convictions that the true nature of light and other forms of electromagnetic radiation need to be explained using dual pictures of particles and waves. In later reminisces, Louis de Broglie acknowledged the importance of his brother's influence on his scientific development but also highlighted two other factors that should be mentioned here. The first was Langevin's teaching through which de Broglie was exposed to Einstein's theory of relativity and specifically the notion of relativistic time. Langevin's lectures on the Clock Paradox, eventually inspired de Broglie to formulate his own

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[156] Ibid., 412.
[157] Jammer, *The Conceptual Development of Quantum Mechanics,* 245.

notions of “phase waves”. “This difference between the relativistic variations of the frequency of a clock and the frequency of a wave is fundamental; it had greatly attracted my attention, and thinking over this difference determined the whole trend of my research.”[158]

The second factor we will note here is the series of papers published by Marcel Brillouin between 1919 and 1922 on the hydrodynamic model of a vibrating atom. De Broglie would later recognize Brillouin’s papers as the “real precursor to wave mechanics.”[159] As we saw in chapter one, by 1922, Bohr’s atomic model was fairly successful at explaining the interior electronic structure of simple atoms and Einstein’s extended light quantum theory was successful at explaining the external interactions of atoms and ambient radiation but one of the dilemmas was the breakdown of Maxwell equations in the area just outside the nucleus. In these three papers Brillouin first attempted, but failed, to reconcile Bohr’s model of electronic structure within the atom with its radiative properties using a hydrodynamic approach and assuming that the electrons were vibrating and orbiting the nucleus within an elastic medium. Brillouin then attempted a second approach to the problem by employing a Lagrangian formulation, but in both cases his attempts fell short. At the end of his third paper, Brillouin stated that: “[P]erhaps there exists a different, third road on which a young audacious investigator may achieve success.”[160] This was a challenge that Louis de Broglie certainly took to heart.

In 1922, de Broglie, no doubt influenced by his brother’s research, was investigating the “dual faces” of radiation and trying to reconcile both its wave and particle characteristics. He began this line of inquiry by going back to one of the primary sources of quantum theory, Einstein’s light quantum hypothesis. Paralleling Einstein’s earliest quantum program de Broglie treated blackbody radiation as a gas of light quanta and submitted it to analysis using classical

[158] Ibid., 245.
[159] Ibid., 246.
[160] Ibid., 247.

statistical mechanics, thus deriving Wien's distribution law.[161] In his second publication of 1922, de Broglie tried to go straight to the heart of the matter by attempting to reconcile Einstein's light quantum hypothesis with undulatory effects like interference and diffraction.[162] In trying to reconcile these notions he realized that the biggest stumbling block was the physical conceptualization of periodicity associated with Einstein's light particles or quanta. However, after reading Brillouin's challenge issued in his latest paper on the subject, de Broglie became convinced that a synthetic picture of radiation phenomena could be achieved that would reconcile Einstein's quantum hypothesis and Bohr's atomic theory. By the end of the summer of 1923, the various independent ideas within de Broglie's head began to fall into place as he fully developed his notion of "phase waves" extending them not only to radiation quanta but also to the bound electrons "orbiting" the atomic nucleus. De Broglie's 1923 summer realization came in two steps:

> Firstly the light-quantum theory cannot be regarded as satisfactory since it defines the energy of a light corpuscle by the relation $W = h\nu$ which contains a frequency ν. …This reason alone renders it necessary in the case of light to introduce simultaneously the corpuscle concept and the concept of periodicity.
>
> On the other hand the determination of the stable motions of the electrons in the atom involves whole numbers, and so far the only phenomena in which whole numbers were involved in physics were those of interference and of eigenvibrations. That suggested the idea to me that electrons themselves could not be represented as simple corpuscles either, but that a periodicity had also to be assigned to them too.
>
> I thus arrived at the following overall concept which guided my studies: for both matter and radiations, light in particular, it is necessary to introduce the corpuscle concept and the wave concept at the same time. In other words the existence of corpuscles accompanied by waves has to be assumed in all cases.[163]

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161 This paper was published as: L. de Broglie, "Rayonnement noir et quanta de lumière," *Journal de Physique 3*, (1922): 422-428.
162 Published as: L. de Broglie, "Sur les interférences et la théorie des quanta de lumière," *Comptes Rendus 175*, (1922): 811 - 813.
163 Excerpt from Louis de Broglie's 1929 Nobel Lecture "The wave nature of the electron" (p. 246 – 247)

As a result of this two step realization, within a month, de Broglie had published three papers in quick succession revealing his new synthetic model of "ondes et quanta" or quantum waves and two extensions of this conceptualization to interference effects and Fermat's Principle as understood via the kinetic theory of gases.[164]

In 1924, de Broglie revisited these three papers while writing a summary, in English, for the British journal *Philosophical Magazine.* We will use this paper as the foundation of our exploration of de Broglie's first synthesis of wave-particle duality. As a testament to his brother's influence on him, he opens the paper with the following statement:

> The experimental evidence accumulated in recent years seems to be quite conclusive in favour of the actual reality of light quanta… I shall in the present paper assume the real existence of light quanta, and try to see how it would be possible to reconcile with it the strong experimental evidence on which was based the wave theory.[165]

Thus de Broglie's very first assumption, that light quanta are real, flew in the face of the fundamental beliefs of the remaining corpuscular skeptics in the physics community like Bohr.

De Broglie began his analysis by marrying Brillouin's notion of a massive vibrating particle with a relativistic interpretation. If one starts with a particle of rest mass m_0, internal vibrational frequency v_0, and internal energy $E = m_0c^2$ traveling at velocity $v = \beta c$ with regard to a stationary observer, then due to time dilation effects corresponding to relativity theory there will be a discrepancy between what the proper frequency of vibration is v_0 and the frequency due to time dilation v. In order to account for this discrepancy, de Broglie proposed what he

[164] The three papers that de Broglie wrote in the fall of 1923 were all published in *Comptes Rendus 177*: "Ondes et quanta," 507 – 510; "Quanta de lumière, diffraction et interferences," 548 - 550; and "Les quanta, la théorie cinetique des gaz et le principe de Fermat," 630 - 632. In 1924 he published an article in English summarizing the three *Comptes Rendus* papers. L. De Broglie, "A tentative theory of light quanta," *Philosophical Magazine 47*, (1924): 446 – 458.
[165] De Broglie, "A tentative theory of light quanta," 446.

termed a “fictitious wave” or phase wave that was associated with all moving particles and served in “guiding the displacements of the energy.”[166]

De Broglie applied this principle to an electron “orbiting” the nucleus of an atom and traveling in a periodic closed trajectory. By assuming the stability condition of the electron coincides only with trajectories where the electron is in phase with its corresponding guiding wave, he was able to arrive at the Sommerfeld quantum condition $J = nh$. Furthermore, he showed that the electron’s real velocity $v = \beta c$ was identical to the group velocity of the phase waves. In de Broglie’s own words:

> This property [the theorem of group velocity], a direct consequence of the Hamiltonian equations, allows us to consider the material point as a singularity of the group of waves, the motion of which is governed by the principle of Hamilton-Fermat....But the whole theory will become really clear only if we succeed in defining the structure of the light wave and the nature of the singularity constituted by the quantum, the motion of which should be predicted looking at things from the wave point of view.[167]

The most famous result from all this work was of course his equation connecting the notion of periodicity to a particle’s momentum:

$$\lambda = \frac{h}{p}$$

In this relationship, the characteristically undulatory property of wavelength (λ) was being tied to an electron’s momentum ($p = mv$) through the constant of proportionality h (Planck’s constant). Finally, de Broglie concluded that “any moving body may be accompanied by a wave and that it is impossible to disjoin motion of body and propagation of wave.”[168] In fact, he had shown the equivalence of applying Fermat’s principle to the dynamics of the phase wave and the application of Maupertuis’ variational principle to a particle’s trajectory. De Broglie thus

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166 Jammer. *The Conceptual Development of Quantum Mechanics,* 250.
167 De Broglie, L. 1924. C.R. Acad. Sci. (Paris) 179, 1039. As translated by Georges Lochak “Louis de Broglie (1892-1987),” *Foundations of Physics, Vol. 17*, No. 10. 1987.
168 De Broglie, “A tentative theory of light quanta,” 457.

concluded that "the dynamically possible trajectories of the moving body are identical to the possible rays of the wave.''[169] Furthermore, in situations where the particle passes through an aperture with dimensions similar to the wavelength of the phase waves, de Broglie proposed that the particle would follow a curved trajectory consistent with the diffracted phase waves.

Louis de Broglie defended *"Recherches sur la Théorie des Quanta,"* his doctoral thesis, before the *"Commission d'examen"* (Sorbonne jury) at the University of Paris on November 29, 1924. It was a thoughtful and thorough recapitulation of his publications from the previous two years and while many narratives of this episode point to various statements "attributed" to Langevin which seem to indicate that de Broglie's own advisor was not sure about the significance of his work, the evidence of his actions tells us otherwise. Well before the defense of his doctoral thesis Langevin was discussing his student's work on the biggest scientific stage in the world. In April 1924 the fourth Solvay Council on "Electric conductivity of metals and related problems" was convened and Langevin was one of the invited presenters. He took the time to present de Broglie's work to the likes of Madame Curie, Lorentz, Rutherford, Bragg, Debye, and Schrödinger. If he had really thought that the work was so "far-fetched" would he have risked his scientific reputation on such an important international stage?

In far too many accounts of the development of wave mechanics de Broglie's doctoral thesis is characterized as a brief yet brilliant intuitive leap where the young French physicist, who is poorly trained in the rigors of mathematics, notices that one should be able to extend Einstein's quantum hypothesis to matter. Naturally, in most of these accounts the focus is squarely on the famous relation: $\lambda = \frac{h}{p}$. Unfortunately, this is where most narratives abandon de Broglie's innovative work. Evidence of this is not hard to come by. While presenting a paper

[169] De Broglie, L. 1924. Doctoral Thesis. p. 56. As translated by Georges Lochak "Louis de Broglie (1892-1987)," *Foundations of Physics, Vol. 17*, No. 10. 1987.

on de Broglie at a recent history of science conference at Harvard University, a graduate student working on 20th century history of physics approached me and asked how long de Broglie's dissertation was. In a somewhat hyperbolic, yet serious tone, he proceeded to explain that in all his years of studying the history of science he had always been told that de Broglie's famous thesis was pure genius, but no longer than "four pages"! This budding historian of science was genuinely shocked to learn that de Broglie had actually written close to one hundred and thirty pages, extensively summarizing his published research from the previous two years and taking great pains to place this research in a larger scientific context. This one graduate student is certainly not unique in harboring the misconception that de Broglie's thesis was simply a brief intuitive derivation of his famous wavelength formula.

In fact, de Broglie was so conscious of placing his wave mechanical insights into the larger scientific context that he began his dissertation with a surprisingly extensive historical introduction beginning with Newton's work on mechanics and his corpuscular treatment of light, he then followed the parallel developments of mechanics and optics via Maupertuis' introduction of the least action formulation of Newtonian mechanics and Fermat's development of the laws of geometrical optics. He noted that in the beginning of the 19th century Young and Fresnel's work in establishing the wave conceptualization of light was essential, because it naturally led Hamilton to realize and develop his wave mechanical analogy incorporating a form of the least action principle and showing its equivalency to Fermat's geometrical optics.

In this opening section de Broglie makes an observation that was somewhat prophetic as it would later become an important call to arms for alternate formulations and interpretations of quantum theory:

> "When two theories, based on ideas that seem entirely different, account for the same experimental fact with equal elegance, one can always wonder if the opposition

> between the two points of view is truly real and is not due to an inadequacy of our efforts at synthesis."[170]

De Broglie ends his historical introduction by reviewing several key scientific developments from 1900 to 1924, highlighting Einstein's treatment of the photoelectric effect, Bohr's work on atomic physics, and the research done with x-rays and γ-rays to show various effects of wave-particle duality- in particular Compton's work on x-ray scattering. From his comments at the end of this section it is clear that de Broglie saw his doctoral research as contributing to this narrative by: "unifying the corpuscular and wave points of view and [going] a bit more deeply into the true meaning of the quanta."[171]

This historical introduction by de Broglie was not a vain attempt at placing himself as the culmination of all of these seminal historical innovations. If that were his goal he could have made the historical introduction into a three page oversimplification of history that made his accomplishments seem that much more important. However, the thirty page historical analysis was important for de Broglie because it had clear probative value as to his actual scientific research. As a trained historian who had migrated to the study of physics, de Broglie had extensive experience reading the actual scientific treatises of scientific greats. For him the actual theories of these 16th – 19th century physicists were not antiquated but full of insights long forgotten. It was not enough to stand on the shoulders of giants so as to see farther ahead, one should also be careful to look down from time to time and examine the very foundations that the giant represents. While the vast majority of the physics community in 1924 had long forgotten Hamilton's wave mechanical analogy and his seminal insights in connecting

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[170] Guido Bacciagaluppi, and Antony Valentini, *Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference.* (Cambridge: Cambridge University Press, 2009), 39-40. Translation of excerpts from Louis de Broglie 1924 dissertation (p.25).

[171] Ibid., 40. De Broglie, L. Dissertation (p.30).

geometrical optics and the principle of least action, de Broglile capitalized on this very notion and made it the centerpiece of his wave mechanical research program.

In his first chapter entitled “The phase wave,” de Broglie introduces the mechanism for the synthesis between particle and wave descriptions of nature. As we saw in his papers from 1923, he begins his analysis with the basic quantum relationship: $E = h\nu$, and then extends this to Einstein’s notion of mass-energy equivalency $E = m_0c^2$. In applying these two relationships to a corpuscule of mass (m_0) traveling with velocity (v) de Broglie points to a problem in understanding the relationship between a system’s proper frequency and its time-dilated frequency. His solution is to introduce his “theorem of phase harmony” in which he proposes a “phase wave” that is always in phase with the moving corpuscle’s periodic phenomena. This phase wave has a characteristic frequency (ν) and phase velocity: $v_{ph} = {c^2}/{v}$. De Broglie proposes an interpretation in which this phase wave, whose group velocity corresponds directly to the physical corpuscle’s velocity, represents the spatial distribution of the phases of a phenomenon.[172]

In chapter two entitled “Maupertuis’ principle and Fermat’s principle,” de Broglie attempts to conceptually ground the notion that:

> Fermat’s principle applied to the phase wave is identical to Maupertuis’ principle applied to the moving body; the dynamically possible trajectories of the moving body are identical to the possible rays of the wave. … We think that the this idea of a deep relationship between the two great principles of Geometrical Optics and Dynamics could be a valuable guide in realizing the synthesis of waves and quanta.[173]

If one begins with the assumption of relativistic particles in a electromagnetic field, de Broglie argues that one can write the two principles in the following equivalent 4-vector form where A and B are two points in space and μ = 0,1,2,3 and i = 1,2,3. Maupertuis’ principle of least action

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[172] Ibid., 41. De Broglie, L. Dissertation (p.36)
[173] Ibid., 43. De Broglie, L. Dissertation (p.56)

can be written in the form: $\delta \int_A^B w_i dx^i = 0$, where w_i is derived from a more general 4-vector field on spacetime in which $w_\mu = \left(\mathrm{v}/_\mathrm{c}, -\left(\mathrm{v}/v_{ph}\right)\boldsymbol{n}\right)$ ($\boldsymbol{n}$ being the direction of propagation) reducing to w_i when μ = 0 corresponds to a constant. On the other hand, Fermat's principle can be written as: $\delta \int_A^B \nu/v_{ph}\, dl = 0$ which based on similar 4-vector substitutions gives: $\delta \int_A^B p_i dx^i = 0$. In this case p_i is derived from the canonical energy-momentum 4-vector p_μ. De Broglie proposed to think of the two 4-vectors p_μ and w_μ as playing symmetrical roles in the two formalisms and governed by the general relationship: $p_\mu = hw_\mu$. This last expression is just a generalized form of the quantum relation: $E = h\nu$. De Broglie concluded that there was a powerful unity in the two principles (Maupertuis and Fermat) and Planck's constant was the link between them.[174]

After the general discussion of Maupertuis-Fermat equivalency, de Broglie goes on to apply this to concrete examples describing the dynamics of free electrons, electrons moving in electrostatic fields, and those moving in more general electromagnetic fields. He calculates the phase velocity, which he notes depends on the electromagnetic potentials, and observes that the propagation of this phase wave in an external field is dependent on the mass of the moving body and its charge. While clearly determining the full dynamics of a system via this process is complicated, de Broglie believed that he was opening the door to a line of research that could eventually provide quantum theory with a pragmatic solution to the wave-particle dilemma.[175]

As we noted earlier, de Broglie was influenced by Brillouin's work attempting to determine atomic electron trajectories. Chapter three in his dissertation addresses this very issue. De Broglie begins with an explanation of the Bohr, Sommerfeld, and Einstein conditions

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[174] Ibid. 41-42.
[175] Ibid.,43.

for stationary orbits resulting from the requirement that electrons in all such orbits must have an angular momentum that is an integral number multiple of h. In other words:

$$\oint p_i dq_i = n_i h$$

De Broglie interpreted this condition as meaning that as the phase wave associated with the electron propagates along this orbit it must remain in phase with itself and with the electron. Therefore, the path length of the orbit "l" must be equal to an integral number of phase wave wavelengths. For phase waves of constant wavelength this is simply: $l = n\lambda$. However, in a general sense it can be written as:

$$\oint {}^{\nu}\!/_{v_{ph}}\, dl = n$$

This is just the integrand appearing in Fermat's principle on page 28. Based on the equivalence between the principles of Maupertuis and Fermat derived in his previous chapter, this new "beautiful result" allowed de Broglie to frame a conceptual explanation for Bohr's atomic condition and the stability of atomic electron orbits.[176]

In chapters four through seven, de Broglie does his best to apply his new wave mechanical theory to various phenomena, including two-body systems (hydrogen atom), light quanta (for which he gives a small yet finite rest mass $m_0 = 10^{-50}$ g), x-ray/γ-ray phenomena, and the statistical mechanics of light quanta. In his conclusion, de Broglie admits that his theory is rather tentative and that much work will be required to refine his novel ideas and extend them to general physical systems. His last paragraph states this explicitly:

> I have deliberately left rather vague the definition of the phase wave, and of the periodic phenomenon of which it must in some sense be the translation, as well as that of the light quantum. The present theory should therefore be considered as one whose

[176] Ibid., 44. De Broglie, L. Dissertation (p.65)

> physical content is not entirely specified, rather than as a consistent and definitely constituted doctrine.[177]

There is little doubt that the history of science has been less than fair in accounting for de Broglie's research and his contributions to the quantum revolution. Whereas his doctoral dissertation has tended to be dismissed by historians of science without any regard to its nuance, de Broglie himself described his doctoral dissertation as "…a more complete account of the new ideas that we have proposed, of the successes to which they have led, and also of the many gaps they contain."[178] Many of his contemporaries were equally impressed with its insights. For example, Charles G. Darwin, grandson of the great evolutionary biologist and a well respected physicist, stated that de Broglie's thesis was "truly one of the most important documents in the history of scientific research."[179]

Einstein refused to attend the 1924 Solvay Council in protest over the exclusion of all German scientists from this and other international scientific meetings. As a result, Einstein and the vast majority of the quantum physicists from the four quantum schools (Munich, Göttingen, Copenhagen, and Leiden) did not have a chance to hear about de Broglie's innovative work. However, Langevin did meet Einstein in Geneva later that year where he presented a sketch of de Broglie's findings.[180] This discussion piqued Einstein's interest as he had been working on extending the work of Satyandra Nath Bose who had published a paper (translated into German by Einstein) deriving Planck's radiation law for light quanta independently of classical electrodynamics. As a result, Einstein's extension of Bose's work from light quanta to a monatomic gas would become the foundations of Bose-Einstein statistics. By assuming these

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[177] Ibid., 48. De Broglie, L. Dissertation (p. 128)
[178] Ibid., 39. De Broglie, L. Dissertation (p.30)
[179] Jammer, *The Conceptual Development of Quantum Mechanics,* 250.
[180] Nye, "Aristocratic Culture and the Pursuit of Science: The De Broglies in Modern France," 413.

new statistics governed his gas, Einstein re-derived an expression he had arrived at in 1909 for the mean-square energy fluctuation of a "gas" of light quanta:

$$\bar{\varepsilon}^2 = \bar{E}h\nu + \frac{c^3\bar{E}^2}{8\pi\nu^2 v d\nu}$$

This time he was applying it to a monatomic gas and the two additive terms above took on a new significance in light of de Broglie's work. The first term in this expression ($\bar{E}h\nu$) was clearly associated with the classical Maxwell-Boltzmann statistics for non-interacting molecules but the second term ($\frac{c^3\bar{E}^2}{8\pi\nu^2 v d\nu}$) could only be associated with interference fluctuations of wave-like phenomena. In his 1909 derivation applied to light quanta the first term had been hard to comprehend in light of an undulatory understanding of radiation but in his 1924 derivation applied to a monatomic gas it was the interference term that gave pause. Either way, this was now clearly a verification of de Broglie's extension of wave particle duality beyond the light quanta domain.

After his preliminary discussions with Langevin in Geneva, Einstein requested a copy of de Broglie's thesis which he received in December 1924. In his second paper dealing with the new quantum statistics and their application to an ideal gas, Einstein referred to de Broglie's work when discussing the interference term ($\frac{c^3\bar{E}^2}{8\pi\nu^2 v d\nu}$) and declared: "I believe that it involves more than merely an analogy."[181] While de Broglie may have considered his phase wave as a heuristic element within his theory, Einstein was hedging towards bestowing a more physical significance on these guiding matter-waves. On December 16, 1924 after reading de Broglie's thesis Einstein wrote to Langevin, placing the young French academic's discovery into context he stated: "Louis de Broglie's work has greatly impressed me. He has lifted a corner of the great

[181] Jammer, *The Conceptual Development of Quantum Mechanics,* 251.

veil. In my work I obtain results which seem to confirm his. If you see him please tell him how much esteem and sympathy I have for him."[182]

Due in large part to Einstein's publication many top physicists of the time became acquainted with de Broglie's work. In 1925 Paul Dirac working in Britain requested a copy of de Broglie's thesis and in Göttingen Max Born became intrigued by the new theory and discussed the matter with James Franck and Walter Elsasser. Apparently, in this meeting Elsasser commented that a quick way to test the hypothesis of matter-waves was to perform diffraction experiments with free electrons, to which Franck responded that experiments like these had already been done by Clinton Davisson and C.H. Kunsman at Bell Labs as early as 1919.

Elsasser and Franck set out to reinterpret the Davisson-Kunsman experimental results not as electron scattering, as the experimentalists had interpreted them, but as electron diffraction that could be explained perfectly by using de Broglie's expression for the wavelength associated with an electron: $\lambda = {}^{h}/_{p}$. They were able to use the accepted values of the mass and velocity of the electrons (p = mv) and find the corresponding wavelength which matched Davisson and Kunsman results. Elsasser took his analysis one step further and also reinterpreted something called the "Ramsauer effect". Carl Ramsauer, working as a teaching assistant to Philipp Lenard in Heidelberg, Germany between 1907 and 1909 discovered that for slow electrons with energy below 25 eV the scattering cross section of gases like argon decreases quickly making the atoms seem transparent to the incident electrons. With these reinterpretations adequately explained by de Broglie's theory of matter-waves, Elsasser published his findings in July 1925. Meanwhile between 1925 and 1927, Davisson and Lester

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[182] Letter from Einstein to Langevin December 16 1924 as quoted in: Jammer, *The Conceptual Development of Quantum Mechanics,* 255.

Germer set out to perform more extensive experiments that would prove the existence of de Broglie's matter-waves beyond any doubt.

Because of its high demand amongst some of the foremost physicists of the time, de Broglie's thesis was finally published in the *Annales de Physique* in 1925. This overall process of dissemination of de Broglie's conceptualizations of wave-particle duality is critical when trying to understand the internal dynamic and complexity of the quantum physics community of the 1920s. It reflects a politically and scientifically fractured international scientific community and particularly a quantum physics community that was exploring multiple simultaneous avenues of innovation with regard to the development of a workable and coherent mechanics. The various approaches by physicists inside and outside the elite quantum schools were distinct in their approaches and in their reception.

When studying Louis de Broglie and his development of wave mechanics in the early to mid 1920s it becomes clear that his most significant influences all came close to home. The closest was his older brother Maurice who exposed the young Louis first to proofs of the great 1911 Solvay Council and then to experiments trying to study the dual nature of radiation. Also, critical to his development were his professors Langevin and Brillouin at the Collège de France. In contrast to the young scientists who were part of the four quantum schools we discussed earlier in the chapter, de Broglie was not one to travel and explore new environments nor did he tend to encourage many others to join him in close collaboration, he was known generally as a monk-like loner. When de Broglie's mother passed away in 1928 Louis was a thirty-six year old bachelor still living at home. This insularity may help to explain why his greatest influences came from such few and proximal sources. One anecdote highlighting de Broglie's tendency towards insularity was told by George Gamow after visiting de Broglie in Paris:

> De Broglie, wearing a silk house coat, met me in his sumptuously furnished study, and we started talking physics. He did not speak any English; my French was rather poor.

> But somehow, partly by using my broken French and partly by writing formulas on paper, I managed to convey to him what I wanted to say and to understand his comments. Less than a year later, de Broglie came to London to deliver a lecture at the Royal Society, and I was, of course, in the audience. He delivered a brilliant lecture, in perfect English, with only a slight French accent.[183]

3.3- Other Hypothetical "Guiding" Waves Proposed in 1924

Although traditional quantum mythologies generally avoid any discussion of de Broglie's phase waves as "guiding" waves or the possibility that others in the physics community may have also been interested in this possibility, it is instructive to explore this particular line of inquiry because these proposals were very real. Furthermore, when placed into their appropriate contexts of 1924 these hypotheses do not seem outlandish. As we have previously noted a long standing problem in the physics community was the goal of developing a unification of wave and particle theories and while de Broglie was following Hamilton's wave-mechanical analogy and basing his theoretical constructions on the equivalency between Maupertuis and Fermat's principles there were others taking different approaches to the shared problem.

John C. Slater was an American physicist who arrived in Copenhagen in December of 1923 on a post-doctoral fellowship to work with the famous Niels Bohr.[184] In a later interview with Thomas Kuhn, Slater recalls the circumstances that led to the development, together with Bohr and Kramers, of their Bohr-Kramers-Slater (BKS) theory.[185] The ordeal left Slater bitter and disillusioned with the work being done in Copenhagen and in particular of Bohr's approach to

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[183] Nye, "Aristocratic Culture and the Pursuit of Science: The De Broglies in Modern France," 418.
[184] Slater, a University of Rochester undergraduate, went on to do his doctoral studies at Harvard University under the direction of Percy Bridgman before going to Cambridge and Copenhagen on a Harvard sponsored Sheldon postdoctoral fellowship in 1923-24. From John C. Slater AIP Interview with Thomas S. Kuhn and J.H. Van Vleck at Slater's Office, M.I.T; October 3, 1963 (http://www.aip.org/history/ohilist/4892_1.html)
[185] BKS Theory stands for Bohr-Kramers-Slater theory.

physics. According to Slater, upon his arrival in Copenhagen, he mentioned to Bohr that he had been working on a way of reconciling light quanta and wave phenomena during the fundamental interactions of light and matter at an atomic level. While Bohr saw promise in the approach he pressured the young American to give up on the definite notion of light quanta.

You may recall that when Einstein published his seminal paper "On The Quantum Theory of Radiation" in 1917, Bohr gladly accepted Einstein's notions of three distinct emission and absorption processes with related probability coefficients but rejected Einstein's argument for why light quanta carried an associated linear momentum in order to account for atomic recoil effects. By the mid 1920s, this disagreement over the true nature of electromagnetic radiation as corpuscular, undulatory, or both remained a key point of divergence between these two eminent physicists.

We saw in Chapter One how Bohr preferred to develop his atomic models based on foundational principles like the correspondence principle. His role in the development of the 1924 BKS theory is indicative of this approach and is useful for our understanding of the later developments in the dialectic between matrix and wave mechanical programs. By the end of January, Bohr and Kramers had usurped the young American's ideas about "virtual fields" and "virtual oscillators" associated with individual atoms that govern the exchange between EM radiation and matter on the atomic scale and written a paper that did away with all physical notions of light quanta. As a result of their new model, they were forced to abandon classical principles such as causality and the conservation of momentum and energy on the microscopic scale in favor of a statistical picture that retained these principles only for larger many body, statistical systems.

While Slater definitely signed off on the publication of the BKS theory, he later became bitter over what he considered strong-arm tactics by the eminent Bohr and his "yes-man"

Kramers in significantly distorting his approach. Based on these experiences, Slater would always come to consider his short stay in Copenhagen as a disaster:

> Bohr would always go in for this remark, "You cannot really explain it in the framework of space and time." By God, I was determined I was going to explain it in the framework of space and time. In other words, that was Bohr's point of view on everything, and that was the fundamental difference of opinion between us… The fact that Bohr was fundamentally of a mystical turn of mind and I'm fundamentally of a matter-of-fact turn of mind. I just think that our temperaments could never have fitted in. Unfortunately, I didn't realize that when I went there. If I had, I would have gone to Göttingen or Munich. I'm sure I would have made out very much better there… I've never had any respect for Mr. Bohr since.[186]

The theory was immediately seen as problematic by many important physicists such as Einstein, Sommerfeld, and Pauli who regarded the abandonment of classical principles of conservation and the space-time framework as too dramatic a sacrifice. Einstein's response to the BKS theory was captured in an April 1924 letter to Born in which he stated:

> Bohr's opinion about radiation is of great interest. But I should not want to be forced into abandoning strict causality without defending it more strongly than I have so far. I find the idea quite intolerable that an electron exposed to radiation should choose *of its own free will*, not only its moment to jump off, but also its direction. In that case I would rather be a cobbler, or even an employee in a gaming-house, than a physicist. Certainly my attempts to give tangible form to the quanta have foundered again and again, but I am far from giving up hope. And even if it never works there is always that consolation that this lack of success is entirely mine.[187]

By the time he arrived back in the United States in the fall of 1924, Slater had submitted his own follow-up paper "A Quantum Theory of Optical Phenomena" which was published in 1925 and in which he reintroduced the notion of corpuscular light quanta.[188] That same year a series of experiments by Walther Bothe and Hans Geiger as well as by Arthur H. Compton and Arthur Simon seemed to show that the conservation of energy and momentum was in fact upheld even

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[186] J.C. Slater AIP Interview with Thomas S. Kuhn and J.H. Van Vleck, October 3, 1963.
[187] Silvan S. Schweber, "Weimar Physics: Sommerfeld's Seminar and the Causality Principle," *Physics in Perspective* Volume 11, Number 3, (2009): 282.
[188] John C. Slater, "A Quantum Theory of Optical Phenomena," Phys. Rev. 25, (1925): 395-428.

on the scale of individual radiative exchanges. Ultimately, this evidence was impossible for the BKS theory to assimilate because it inevitably led to irrefutable proof of the existence of Einstein's radiation quanta.

While the BKS' tenure as a plausible explanation of matter-light interactions was short-lived it was nevertheless very significant because the introduction of virtual fields and oscillators subsequently had an enormous impact on the development of quantum mechanics in Göttingen. In the discarded theory, the virtual field, associated with a particular atom and produced by the superposition of many virtual oscillators, was not real in the classical sense as it was not observable and did not carry energy or momentum but was instead related, via its intensity, to the probability of the atom making a real electronic transition.[189] The general notions of an underlying probabilistic nature describing the interactions of radiation and matter undoubtedly served as a foundation for the matrix mechanics that emerged from the work of Heisenberg, Born, and Jordan in the summer and fall of 1925. In fact, most narratives of the matrix formulation describe this transition in great detail, highlighting the Göttingen physicists' decision to extend the BKS aversion to a concrete space-time framework even farther.[190]

3.4- Schrödinger's Response to de Broglie's Wave Mechanics

In many accounts of the development of quantum theory in the 1920s we are told that in the summer of 1925, while holed up sick with hay fever on the island of Heligoland, Werner Heisenberg developed his matrix mechanics formulation of quantum theory in an attempt to do away with the failings of the "old quantum theory" as represented by the work of Bohr and Sommerfeld. Subsequently, while Heisenberg, Max Born, and Pascual Jordan at the University

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[189] Schweber, "Weimar Physics: Sommerfeld's Seminar and the Causality Principle," 281.
[190] As examples of this see Beller, *Quantum Dialogue* and Jammer, *The Conceptual Development of Quantum Mechanics*.

of Göttingen were busy extending Heisenberg's theory, Schrödinger, in response to this innovation, seemingly came out of nowhere and put forward a competing formulation of quantum mechanics based on de Broglie's matter wave theory and the development of a new wave equation. This narrative is highly problematic.

First and foremost, the story of Heisenberg running off to an isolated island to convalesce only to then singlehandedly invent "matrix mechanics" is a distorted retelling historical events. The development of matrix mechanics was a result of extensive collaborative work that Heisenberg had engaged in throughout the previous academic year of 1924-1925 while in Copenhagen on a Rockefeller fellowship. Also, Heisenberg did not introduce the matrix notation himself; that was one of Born and Jordan's fundamental contributions to the new theory. As this is the story of the historical "winners" (ie. the Göttingen-Copenhagen Interpretation) the story was reshaped during the congealing of this interpretation in the 1930s and eventually told and retold in so many narratives, it has become an accepted myth. In the following discussion we will reexamine this historical period with complex and nuanced dynamics of innovation making sure to focus on Schrödinger's capitalization of de Broglie's work and his subsequent contributions to the quantum revolution.

There has been much debate about the motivations of Erwin Schrödinger to publish the set of five landmark papers from January to June 1926 which introduced a more intuitive and manageable wave mechanical formulation of quantum theory as an alternative to the matrix formulation being produced and refined in Göttingen. As is always the case when one studies the full context of scientific innovations, there are usually multiple motivations that are more accurately represented by a web of causality then by a chain of causal links. In 1921 Schrödinger moved to Zürich to take over as professor at the University of Zürich.

It was during this period in Zürich when Schrödinger seems to have blossomed as a theoretical physicist. During the early to mid 1920s, Peter Debye and Hermann Weyl were professors at the ETH, also in Zürich, and together with Schrödinger they formed a productive collaboration. Weyl would later say of this period of Schrödinger's career that the Viennese scientist "did his great work during a late erotic outburst in his life."[191] Furthermore, Walter Moore, a Schrödinger biographer claims that Schrödinger's complex and tense home life provided creative tension for his scientific innovation and that Debye and Weyl were somewhat complicit in his 'erotic outbursts'. "Their Zürich circle provided ample opportunities for amorous adventures, and if Erwin cared to venture outside the usual academic liaisons. Both Weyl and Debye were available as competent guides to the uninhibited night life of the city."[192]

We saw earlier how de Broglie had been inspired by Brillouin and Langevin's work to apply relativistic principles to vibrating electrons in closed orbits within the Bohr-Sommerfeld atomic model. But this was not the only attempt to apply relativity theory to electronic atomic orbits. After all, the conceptualization of mechanically and radiatively stable electronic orbits within the atom was something all atomic physicists had struggled with from Thomson to Bohr. In 1919, commenting on this problem Charles G. Darwin stated that:

> I have long felt that the fundamental basis of physics is in a desperate state. The great positive successes of the quantum theory have accentuated all along, not merely its value, but also the essential contradictions over which it rests… It may be that it will prove necessary to make fundamental changes in our ideas of time and space, or to abandon the conservation of matter and electricity, or even in the last resort to endow electrons with free will.[193]

It should therefore come as no surprise that in 1921, Weyl published a paper applying the four-vector notation of general relativity to the electron problem. Schrödinger picked up on

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[191] Walter J. Moore, *Schrödinger, life and thought.* (Cambridge: Cambridge University Press, 1992), 191.
[192] Ibid*.,* 191.
[193] C.G. Darwin as quoted in: Jammer, *The Conceptual Development of Quantum Mechanics,* 178.

his colleague's ideas and published his own treatment the following year, applying Weyl's notions of resonance to a system of a bound electron and a proton (ie. the hydrogen atom). While this attempt failed to produce an adequate explanation for the stability of the atomic electron, Schrödinger saw Weyl's extension of relativity to the problem of the Bohr-Sommerfeld quantization rules as an intriguing step. He would later see this as a direct parallel to de Broglie's research program outlined in his doctoral thesis.[194]

The Schrödinger-Einstein correspondence of 1925 and 1926 gives us exceptional insight into Schrödinger's process of innovation surrounding his work on wave mechanics. The various letters show us a progression in his understanding of the new Bose-Einstein statistics and explain how he was ultimately turned on to de Broglie's thesis via Einstein's 1925 paper. As we already noted, Einstein published a series of papers on the quantum theory of ideal gasses, and in the second of these publications recognized de Broglie's thesis as proposing an important shift in the conceptualization of wave-particle duality. On February 5, 1925 only days before this second paper was published, Schrödinger wrote to Einstein claiming that there must be an error in the calculations of his statistics. As it happens, Schrödinger had not understood the novelty of Einstein's first paper extending Bose's new counting methods and assumed that the famous physicist was simply making a mistake in the application of the classical Maxwell-Boltzmann statistical distribution.[195]

Schrödinger was very sensitive to the application of these classical Maxwell-Boltzmann statistics because from 1906 to 1910 he had trained at the University of Vienna. While he had arrived just after the tragic suicide of the great Boltzmann, Schrödinger's advisors (Fritz Hasenöhrl and Franz Exner) were steeped in the Boltzmann school. In discussing Boltzmann's

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[194] Paul Hanle, "The Schrödinger-Einstein correspondence and the sources of wave mechanics," *Am. J. Phys.* 47(7), (July 1979): 645-6.
[195] Ibid., 644-5.

indirect influence on him, Schrödinger would later state that "His line of thought may be called my first love in science. No other has ever thus enraptured me or will ever do so again."[196] Maybe it was this blind "love" that restricted Schrödinger from immediately realizing what Einstein and Bose had really accomplished. In any case, three weeks after his initial query, on February 28, Einstein responded to Schrödinger's "serious reservations" about his miscalculations by stating unequivocally that there was no error in calculation and by thoroughly explaining the discrepancy between his and Boltzmann's modes of counting highlighting the novelty of not treating the gas molecules completely independently.[197]

The next letter in the correspondence would not be sent for seven months and even then it would be at the hands of Einstein not Schrödinger. Could it be that the Viennese physicist did not respond to Einstein's latest letter because he was embarrassed by the February exchange? Schrödinger finally responded to Einstein's February 28 letter on November 3, almost eight months later!

> Only through your letter [of February 28] did the uniqueness and originality come to me of your statistical method of calculation, which I had not understood previously… [Y]our theory of gas degeneracy is really something fundamentally new – and that I did not grasp at all at first. Pardon me for not answering at all this letter that was so valuable to me. I heard you would be in America, so postponed the answer and then procrastinated.[198]

As it turns out, Schrödinger, like other quantum physicists of the day, were drawn to de Broglie's doctoral thesis initially because of their interest in Einstein's work on the quantum statistics of ideal gases. In that same letter from November 3rd Schrödinger admitted just that:

> I have read with greatest interest a few days ago the ingenious thesis of Louis de Broglie, which I finally got hold of; with it also [Section] 8 of your second degeneracy work has become completely clear to me for the first time. The de Broglie interpretation of the quantum rules seems to me to be related in some ways to my note in the Zs. f.

[196] Jammer. *The Conceptual Development of Quantum Mechanics,* 156.
[197] Hanle, "The Schrödinger-Einstein correspondence and the sources of wave mechanics," 644.
[198] Ibid., 645.

> Phys. 12, 13, 1922, where a remarkable property of the Weyl "gauge factor" $e^{-\int \emptyset_i dx_i}$ along each quasi-period is shown. The mathematical situation [rechnerische Sachverhalt] is, as far as I can see, the same, only from me much more formal, less elegant and not really shown generally. Naturally de Broglie's consideration in the framework of his large theory is altogether of far greater value than my single statement, which I did not know what to make of at first.[199]

In fact, there is uncertainty surrounding the exact way in which Schrödinger obtained de Broglie's thesis. While it was published in *Annales de Physique* in 1925, apparently it was not easy to come by in Zürich. In one account later retold by Edmond Bauer, Victor Henri, a French professor of physical chemistry at the University of Zürich, claimed that during a visit to Paris in 1925, he had been given a copy of de Broglie's thesis by Langevin and upon his return to Zürich showed it to Schrödinger because he could not understand it. Apparently, after two weeks, Schrödinger returned the thesis with a note characterizing de Broglie's work as "rubbish." Henri mentioned Schrödinger's response to Langevin who immediately suggested: "I think Schrödinger is wrong; he must look at it again." Apparently, Schrödinger did just that.[200]

Regardless of how Schrödinger came by de Broglie's thesis or what his initial thoughts may have been, the fact is that he did manage to secure a copy and this work, more so than any other factor, became a critical influence on his development of quantum wave mechanics in the winter and spring of 1926. While the initial seeds for Schrödinger's innovations may have been planted by Weyl's application of general theory of relativity to the micro-world and Einstein's work on quantum statistics and de Broglie's doctoral research was critical in laying the foundation of the Austrian's approach to wave mechanics, we should not discount the notion that the actual development of his new mechanics was undoubtedly due in large part to his Zürich collaboration with Debye. Schrödinger and Debye co-led a series of physics colloquia that included both students and faculty and met every fortnight alternating between the ETH and the

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[199] Hanle. "The Schrödinger-Einstein correspondence and the sources of wave mechanics," 645.
[200] Jammer, *The Conceptual Development of Quantum Mechanics,* 259.

University of Zürich. It was through these symposia in the fall of 1925 that Debye and Schrödinger first attempted to seriously grapple with de Broglie's doctoral thesis. Specifically, it fell to Schrödinger to prepare a colloquium reviewing de Broglie's research for the last week of November. The preparations for this talk may have been the impetus that ended Schrödinger's self described "procrastination."

As we saw from the correspondence, while the initial impetus of discussion may have been de Broglie's reproduction of Planck's work on quantum statistics in an attempt to better understand the new Bose-Einstein statistics, attention quickly turned to other sections of the doctoral thesis. Specifically, Schrödinger became interested in de Broglie's application of his matter waves to the Bohr-Sommerfeld quantum conditions for stable atomic-electron states. Two weeks after his letter to Einstein, Schrödinger was already discussing the possibility of extending de Broglie's work to develop a more comprehensive atomic theory. In a letter to Alfred Landé, Schrödinger explained that:

> I was especially pleased with your news that your work would be 'a return to wave theory'. I am also strongly inclined that way. I have been intensely concerned these days with Louis de Broglie's ingenious theory. It is extraordinarily exciting, but still has some very grave difficulties. I have tried in vain to make for myself a picture of the phase wave of the electron in the Kepler orbit… This however, gives horrible 'caustics' or the like for the wave fronts.[201]

It seems likely that in this dialog with Landé we see the first inclinations of Schrödinger to extend de Broglie's work to atomic theory.

Felix Bloch was a student in Zürich and a participant in the Debye-Schrödinger run colloquia in 1925-1926 and later reminisced on these meetings and specifically on what he recalled as a turning point in Schrödinger's thinking of the wave-theoretic eigenvalue problem:

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[201] Moore, *Schrödinger, life and thought,* 192.

> Once at the end of a colloquium I heard Debye saying something like: "Schrödinger, you are not working right now on very important problems anyway. Why don't you tell us some time about that thesis of de Broglie, which seems to have attracted some attention?"
>
> So in one of the next colloquia, Schrödinger gave a beautifully clear account of how de Broglie associated a wave with a particle and how he could obtain the quantization rules of Niels Bohr and Sommerfeld by demanding that an Integer number of waves should be fitted along a stationary orbit. When he had finished, Debye casually remarked that this way of talking was rather childish. As a student of Sommerfeld he had learned that, to deal properly with waves, one had to have a wave equation. It sounded quite trivial and did not seem to make a great impression, but Schrödinger evidently thought a bit more about the idea afterwards.
>
> Just a few weeks later he gave another talk in the colloquium which he started by saying: "My colleague Debye suggested that one should have a wave equation; well I have found one!"
>
> And then he told us essentially what he was about to publish under the title "Quantization as Eigenvalue Problem" as the first paper of a series in the Annalen der Physik. I was still too green to really appreciate the significance of this talk, but from the general reaction of the audience I realized that something rather important had happened, and I need not tell you what the name of Schrödinger has meant from then on. Many years later, I reminded Debye of his remark about the wave equation; interestingly enough he claimed that he had forgotten about it and I am not quite sure whether this was not the subconscious suppression of his regret that he had not done it himself. In any event, he turned to me with a broad smile and said: "Well, wasn't I right?"[202]

Felix Bloch's anecdote, while full of drama, is a perfect example of a physicist telling a first person account of history in a way that distorts the actual historical events. Upon hearing this anecdote one could surmise that the burst of creativity and innovation associated with Schrödinger's development of wave mechanics was a quick flash of brilliance on his part inspired by Debye's poignant challenge. In fact, many modern historical narratives of quantum theory tend to build off of this distortion adding that Schrödinger was on Christmas vacation in the Alps

[202] Felix Bloch, "Heisenberg and the Early Days of Quantum Mechanics," *Physics Today* (December, 1976): 23-24. Bloch himself recognized that this account did not "conform to the strictest standards of history" and while he was not able to "render the exact words I heard on those occasions," he assured his audience that in spirit his account was "the truth and only the truth."

convalescing with a mistress when he developed wave mechanics.[203] This degrades and obfuscates the real process of innovation. Understanding the larger context of discovery based on our analysis allows us to appreciate the complexity and nuance in the causal web that actually led to Schrödinger's innovations. Bloch's anecdote may be true, as far as he can remember, but it omits the direct influences of Weyl, Einstein, and de Broglie and serves to truncate the timeline of creative discovery from a process that took nearly a year, down to a romantic jaunt in the Alps.

There seems to be little doubt that Schrödinger did spend time over the winter holidays in Arosa, a vacation spot in the Swiss Alps. However, the details of what happened there seem to be lost to the great historical abyss, as Schrödinger's personal diary for 1925 has gone missing. Schrödinger biographers like Walter Moore claim that he invited an unknown mistress from Vienna to spend time with him in Dr. Otto Herwig's sanatorium disguised as a Villa which Erwin and his wife Anny frequented. Moreover, they claim that it was during this two-week stay in Arosa that Schrödinger was able to formulate the version of his non-relativistic wave-function that he would first publish in January, 1926.[204] While anecdotes like Bloch's recollection of Debye's challenge to Schrödinger and his working vacation in Arosa may be good drama they don't reveal much about the process of innovation. For that we need to rely on a wider range of historical evidence including analyses like that done by Paul Hanle in which he traces the Einstein- Schrödinger correspondence of 1925 and studies the influences of de Broglie's doctoral thesis on the later development of wave mechanics.

In studying de Broglie's thesis for the initial colloquium presentation at the end of November, Schrödinger was essentially reproducing de Broglie's arguments. While he had mentioned 'wave theory' in his correspondence with Landé, it seems likely that there was some

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[203] Moore, *Schrödinger, life and thought.* 194-195.
[204] Ibid., 195.

exchange between Debye and Schrödinger in which the ETH professor challenged Schrödinger to find the corresponding 'wave equation' to the de Broglie wave phenomena. Based on his unpublished scientific journals, Schrödinger's initial attempts at developing this wave equation were unsuccessfully based on relativistic electrodynamics, following the lines of inquiry Weyl had sparked in 1921 and Schrödinger had followed with his 1922 paper. As the notion of spin had not yet been developed, in retrospect, this failure was inevitable. Even though it was initially a failure, Schrödinger did not completely abandon the overall project, choosing instead to reattempt a derivation based on non-relativistic electrodynamics. By early January of 1926 he had found his now famous wave equation that bears his name.

While his ideas and methods borrowed extensively from de Broglie's doctoral thesis, including the use of Hamiltonian analysis, there is no doubt that Schrödinger deserves full credit for extending the Frenchman's work and producing a set of papers that were completely transformational within the greater physics community. Having said that, it seems likely that the level of thoroughness of Schrödinger's later analyses and the relative speed with which they were published in the spring of 1926 were due, in large part, to the sound foundational work of de Broglie's research, and in particular the Frenchman's historical contextualization of his work grounded in Hamilton's analogy. In that sense the present discussion has been an attempt to correct a distortion in quantum narratives of this period which fail to recognize the extent to which the development of Schrödinger's wave mechanics was based on de Broglie's extensive insights and the nuanced web of causality that led to his seminal contributions.

3.5- The Evolution of Schrödinger's Wave Mechanics

All undergraduate physics students in their first quantum mechanics course are exposed to a simplified reproduction of the derivation of the Schrödinger equation. We will not reproduce the full derivation here but we will give a quick historical sketch of Schrödinger's five seminal publications from the winter and spring of 1926 including "Quantisierung als Eigenwertproblem" (Quantization as an Eigenvalue Problem) which was published as four separate communications between January and June and his paper showing the equivalence between his formulation and Heisenberg's matrix mechanics. All five of these papers were originally published within volumes 79 – 81 of the journal *Annalen der Physik*. However, in this study I have relied on the original papers as they appear in the volume of *Collected Papers on Wave Mechanics* published in November, 1926 and subsequently translated in 1928.[205]

As we recognized earlier, de Broglie realized that the Sommerfeld quantization condition J = nh comes from the expression for the integral of momentum around a closed loop: $\int p\ dq = nh$, but since $p = \frac{h}{\lambda}$ this implies that $\int \frac{1}{\lambda}\ dq = n$, which for Schrödinger pointed to a governing theory represented by a differential 'wave' equation and an eigenvalue problem.[206] He was acquainted with eigenvalue problems and knew that they would allow for the discrete energy spectrum which the Bohr-Sommerfeld atomic model called for. More importantly, he was hoping that a derivation of this spectrum from first principles could do away with the ad hoc nature of the quantization conditions and explain some of the more mysterious elements of the latest atomic models, specifically the electron jumps.

As a first approximation, Schrödinger used the Hamiltonian to set forth derive his time-independent wave equation. Beginning with the general form for the Hamiltonian:

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[205] Erwin Schrödinger, *Collected Papers on Wave Mechanics.* (New York: Chelsea Publishing Company, 1982). Actually this is a later edition of these collected papers that was augmented to include other papers.

[206] Eigenfunctions, in differential equations theory, are a group of functions f_n where n = 1,2,3… that are solutions to the governing differential equation. These functions need to be 'admitted' as solutions both by the corresponding boundary conditions and a group of scalars called eignevalues $\lambda = \lambda_n$ where n = 1,2,3… The associated governing differential equation is defined as a linear operator **A**, that when acting on the function f returns the same function multiplied by its own corresponding eigenvalue. $\mathbf{A}f = \lambda f$.

$$H\left(q, \frac{\delta S}{\delta q}\right) = E$$

Schrödinger substituted Hamilton's characteristic function S, with a separable function K log ψ which lead him to:

$$H\left(q, \frac{K}{\psi}\frac{\delta\psi}{\delta q}\right) = E$$

He also replaced the 'ad hoc' Sommerfeld quantization conditions with a new postulate. Schrödinger defined his new eigenfunctions ψ_n as single-valued functions that were real, twice differentiable, and corresponded to the Euler-Lagrange wave equation when the associated variational integral over all q space was an extremum.

$$\delta J = \delta \iiint \left[(\nabla\psi)^2 - \frac{2\mathrm{m}}{\mathrm{K}^2}\left(\mathrm{E} + \frac{\mathrm{e}^2}{\mathrm{r}}\right)\psi^2\right] dV = 0$$

Schrödinger's use of this variational integral was in some respects analogous to de Broglie's use of Maupertuis and Fermat's principles of least action in his doctoral thesis. Of course, in de Broglie's case he failed to extend this notion completely and derive the subsequent Euler-Lagrange wave equation. Schrödinger, with the help of his colleague Hermann Weyl, was able to arrive at the wave equation Debye had challenged him to find:

$$\nabla\psi + \frac{2\mathrm{m}}{\mathrm{K}^2}\left(\mathrm{E} + \frac{\mathrm{e}^2}{\mathrm{r}^2}\right)\psi = 0$$

From this wave equation Schrödinger was able to derive the discrete eigenvalue spectrum associated with the various atomic energy levels.

$$E_n = \frac{-me^4}{2K^2n^2}$$

Where n = 1,2,3… When he set K = h/2π he was left precisely with Bohr's energy spectrum for the hydrogen atom. Schrödinger thus concluded his first paper on the wave equation by stating:

> One may, of course, be tempted to associate the function ψ with a vibrational process in the atom, a process possibly more real than electronic orbits whose reality is being very much questioned nowadays. …[but] the essential point, which in my opinion, is the fact that the mysterious 'requirement of integralness' no longer enters into the quantization rules but has been traced, so to speak, a step further back by having been shown to result from the finiteness and single-valuedness of a certain space function…[Furthermore,] it is hardly necessary to point out, how much more gratifying it would be to conceive a quantum transition as an energy change from one vibrational mode to another than to regard it as jumping of electrons. The variation of vibrational modes may be treated as a process continuous in space and time and enduring as long as the emission process persists.[207]

In his first two communications, Schrödinger laid out the general framework for what he initially referred to as his "undulatorische mechanik" (undualtory mechanics). In the second paper, Schrödinger set out to formalize and extend his new mechanics. He based this second presentation directly on Hamilton's optical-mechanical analogy and showed that the reason Hamilton's optical-mechanical analogy had never before been extended to the full formulation of an undulatory mechanics was the failure to reconcile the difference between phase velocity and particle (or group) velocity. Schrödinger claimed that de Broglie had been one of the first to recognize the importance of this link and that is what had allowed him to bridge the wave particle chasm. As de Broglie before him, Schrödinger recognized the deep and powerful analogy in the notion that ordinary mechanics is an approximation to undulatory mechanics as geometrical optics is an approximation of undulatory optic theory.

> Our classical mechanics is perhaps the complete analogy of geometrical optics and as such is wrong and not in agreement with reality; it fails whenever the radii of curvature and the dimensions of the path are no longer great compared with a certain wavelength, which has a real meaning in q space. Then an undulatory mechanics has to

207 Erwin Schrödinger, "Quantisierung als Eigenwertproblem," *Annalen der Physik 79*, (1926): 372.

> be established, and the most obvious approach to it is the elaboration of the Hamiltonian analogy into a wave theory.[208]

In this second communication, Schrödinger extended his mechanics beyond the confines of the hydrogen atom. He recognized the parallel with radiation theory and the early work Debye and others had done on the optical phenomena of wave groups and wave packets. Basing his mechanics on a more generalized Hamiltonian he was able to apply his theory to the problems of a linear harmonic oscillator, to the rigid rotator, and to the vibrational rotator (diatomic molecule). As it turns out, his solution for the Planck oscillator was in full agreement with the one found using Heisenberg's matrix mechanics.[209]

Schrödinger's third publication chronologically, was not actually part of the "Quantisierung als Eigenwertproblem" series of communications. In this third publication which appeared in *Annalen* that spring, Schrödinger showed what he referred to as the "formal mathematical identity" between his formulation of undulatory mechanics and the matrix mechanics of the Heisenberg-Born-Jordan formalism proposed from Göttingen in the autumn of 1925.[210] As it so happens, Schrödinger was actually not the first to attempt to bridge this Göttingen formalism which, as it was based in algebraic methods, emphasized the discrete and corpuscular character of quantum theory, with the continuous principles of traditional mathematical analytical methods. In December 1925, mere weeks before Schrödinger began publishing his communications, the Hungarian mathematical physicist Kornél Lánczos from his post at the University of Frankfurt had showed that the Göttingen matrix formalism could be

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[208] Erwin Schrödinger, "Quantisierung als Eigenwertproblem," (Second Communication) *Annalen der Physik 79*, (1926): 437-490.

[209] Jammer, *The Conceptual Development of Quantum Mechanics,* 264.

[210] Erwin Schrödinger, "Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen," Annalen der Physik 79, 734-756. "On the relation between the quantum mechanics of Heisenberg, Born, and Jordan, and that of Schrödinger," from the *Collected Papers on Wave Mechanics*, 45-61.

made equivalent to a continuous analytical formulation grounded in integral equations. According to Max Jammer, Lánczos' paper published in January 1926 in *Zeitscrift für Physik*, was probably lost in the subsequent torrent of Schrödinger publications. In part due to the sheer volume of work that Schrödinger put out but also because Lánczos' work was less intelligible as it relied on integral equations instead of the more ubiquitous differential equations. On the other hand, while Schrödinger claimed to have shown a mathematical equivalence between matrix and wave mechanics, it was never considered as rigorous an equivalency as was later accomplished by John Von Neumann in 1929.[211]

Towards the end of this second publication Schrödinger claimed that his new undulatory mechanics formulation was applicable to much more than just the simplest, fully solvable, problems. In his fourth publication, which became the third communication of his "Quantisierung als Eigenwertproblem" series, Schrödinger described in detail how one might apply his formulation to address complex physical phenomena. The process he described involved the application of time-independent perturbation theory, which he based primarily on Lord Rayleigh's earlier work on acoustical vibrations.[212] According to Rayleigh:

> The rigorous determination of the periods and types of vibration of a given system is usually a matter of great difficulty…It is therefore often necessary to fall back on methods of approximation, referring the proposed system to some other of a character more amenable to analysis, and calculating corrections depending on the supposition that the difference between the two systems is small.[213]

In his communication, Schrödinger successfully applied his generalized time-independent perturbation theory to the problem of the Stark effect, which both Paul. S. Epstein and Karl Schwarzschild had worked out during World War I using the Bohr-Sommerfeld theory.

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[211] Jammer, *The Conceptual Development of Quantum Mechanics,* 273 - 274.
[212] Lord Baron Rayleigh (or John W. Strutt) published the first volume of his *The Theory of Sound* in 1877.
[213] Jammer, *The Conceptual Development of Quantum Mechanics,* 264.

In the fourth, and final, communication of the "Quantisierung als Eigenwertproblem" series, Schrödinger again extended his formulation by developing a time-dependent perturbation theory. In doing so, he obtained his now famous time-dependent Schrödinger wave equation:

$$-\frac{h^2}{8\pi^2 m}\nabla^2\psi + U\psi = i\frac{h}{2\pi}\frac{\partial\psi}{\partial t}$$

In deriving this equation, he quickly realized that there was a complex-valued coefficient modifying his diffusion equation. This led him to restate his postulate for the characteristic eigenfunctions (ψ_n) allowing them to be complex-valued functions. Schrödinger now referred to the eigenfunctions ψ_n as the mechanical field scalar and spent the last part of his paper discussing his interpretation of the physical significance of ψ. He considered:

> It has repeatedly been pointed out that the ψ function itself cannot and may not in general be interpreted directly in terms of three-dimensional space – however much the one-electron problem seems to suggest an interpretation – because it is in general a function in configuration space and not in real space.[214]

It had taken Erwin Schrödinger more than a year from the time he had first encountered Einstein's novel brand of statistics to the completion of his series of communications. While most historical narratives may truncate the process of innovation by ignoring the larger web of causality or the five month span over which Schrödinger worked on his seminal "Quantisierung als Eigenwertproblem" series of communications, doing so distorts the actual historical events. In the case of Schrödinger's wave mechanical innovations we note the dynamic and evolving

[214] Erwin Schrödinger, "Quantisierung als Eigenwertproblem," (Fourth Communication) *Annalen der Physik 81*, (1926): 135.

nature of his ideas throughout the process, as well as the their critical dependence on de Broglie's work in 1923-24.

3.6- Wide Spectrum of Reaction to Schrödinger's 1926 Quantization Papers

While we have noted that de Broglie certainly laid the groundwork for Schrödinger's innovative leaps, it was undoubtedly the Austrian physicist who capitalized most successfully on extending the Hamiltonian undulatory-mechanical analogy to the quantum domain. In his publications of 1926, Schrödinger further developed the Frenchman's mature ideas of wave-particle duality based on Hamilton's 19th century insights and then dedicated himself to the invention and application of a new quantum formalism based on a wave equation, that could immediately be employed to explain quantum phenomena. While in general we can characterize the reception of Schrödinger's work as almost universally positive, heralded by many as a fundamental breakthrough in quantum theory research, when we study the particulars of the reception from different perspectives and over time we begin to see a more subtle, wider spectrum of reactions emerge. This understanding is critical if we hope to place the subsequent controversy over the very foundations of quantum theory into a comprehensible context of innovation.

In discussing the spectrum of reaction to Schrödinger's wave mechanics most narratives jump on the controversy that erupted between the revolutionary "Göttingen-Copenhageners" and the conservative "old guard" led most prominently by Einstein, Planck, and Schrödinger himself. Obviously this highly conflict-driven story makes for captivating drama which keeps readers engaged; however, it is based on mythological quantum narratives rather than sound historical analysis. In reinforcing the mythological distortions, these narratives have traditionally tended to overemphasize the polarizing conflict, making it seem as though there were only two camps from the very beginning of quantum mechanics, divided by deep seeded philosophical differences based on governing physical principles such as determinism. On one side of this fictitious aisle you have the "older" more conservative physicists that wanted to retain classical elements of reality at all costs, and on the other side you have the "young" brash physicists willing to do away with all classical notions of the natural world in order to match their theoretical models with empirical results. As we saw in the last chapter these types of oversimplifications fail on multiple levels, so again we need a more careful and nuanced analysis of the scientific dialog involved in this process of innovation.

In looking to the spectrum of reaction to Schrödinger's work we can help reconstruct this nuanced context and avoid the pitfalls of a polarizing distortion. While there was certainly conflict between Schrödinger and some of the physicists from Göttingen and Copenhagen it was far from homogenous or consistent and it certainly was not characteristic of the greater physics community's receptiveness. For example, Heisenberg's initial vitriolic responses to Schrödinger's wave mechanics stands in stark contrast to Born's level-headed yet uneven acceptance and subsequent application of the new wave-mechanical formulation. However, as we shall see, these reactions were not static in time; they evolved based on ongoing dialogs both publicly and privately with other physicists. In addition, we should note that these particular tensions were not the only catalysts present within the physics community. Effects including professional networking and training legacies as well as the lingering tensions from post WWI exclusionary policies all contributed to the wide spectrum of reactions to Schrödinger's innovations from the physics community at large.

This spectrum of responses included those who were: indifferent, completely enchanted by the continuous nature of his wave mechanics, were optimistic but nevertheless probed the details of his papers looking for stumbling blocks, railed against his approach, and saw them as a solid foundation from which to build a more deterministic quantum mechanics. In a letter to Schrödinger dated 4 June 1926, Planck empathized with his Austrian colleague's predicament and succinctly captured the myriad reactions to the new wave mechanics and overall convoluted *zeitgeist*: "What a cross-fire of critical, enthusiastic, and questioning acclamations might now besiege you! But still, it is a thing with incredible prospects."[215] In the following discussion we will examine a sampling of these various reactions as they aid us in reconstructing a representative picture of the creative flux in play.

When Erwin Schrödinger died in 1961 his widow, Mrs. Annemarie Schrödinger, requested that his correspondence having to do with the development of wave mechanics be published by the Austrian Academy of Sciences. In 1962, Karl Przibram was given the task of collecting the appropriate correspondence and editing the volume titled, *Letters on Wave Mechanics*.[216] In reading Schrödinger's correspondence with Planck, Einstein, and Lorentz from 1926-27 one gets an immediate sense of how diverse the responses were, from some of the most celebrated physicists of the day, to his new quantum formulation and interpretation. Planck was very enthusiastic and supportive of Schrödinger's quantization work calling it "epoch making"[217] and ensuring that the Austrian, then teaching in Zürich, was available to visit Berlin and lecture at both the Academy of Sciences and at the University before the end of the 1926 spring semester. It's clear that Planck was quickly taken by Schrödinger's approach seeing a

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[215] Karl Przibram ed., Translated by Martin Klein, *Letters on Wave Mechanics: Schrodinger-Planck-Einstein-Lorentz.* (New York: Philosophical Library Inc., 1967), 13.
[216] Ibid., vii-viii.
[217] Ibid., 6.

fundamental resonance with his own sense of the micro-world. On April 2, 1926 Planck wrote to Schrödinger thanking him for a preprint of his second quantization paper and noting that: "I read your article the way an inquisitive child listens in suspense to the solution of a puzzle that he has bothered about for a long time, and I am delighted with the beauties that are evident to the eye…"[218]

Meanwhile, Einstein was more cautiously supportive of Schrödinger's early papers than Planck. In fact, an initial misunderstanding by Einstein as to the actual formulation of the wave equation led to an ironic exchange and a temporary role reversal of the 1925 dynamic between the two physicists. As we saw in chapter two, Schrödinger had misunderstood Einstein's paper on quantum degeneracy and had critiqued it, only to be embarrassed later when the misunderstanding was pointed out to him by the great physicist himself. In this 1926 exchange, it was Einstein's turn to misunderstand Schrödinger's essential contribution by challenging him and claiming that the wave equation, as stated, did not satisfy the requirements of "additivity" as he saw them. In this case, it was Einstein who quickly realized his mistake and sent a follow-up letter with an apology. Perhaps fearing that he had been too critical of Schrödinger, he added in the margins of his letter "The idea of your article shows real genius."[219] This generous compliment along with Einstein's retracted critique led Schrödinger to respond with a sincere outpouring of emotion:

> Your approval and Planck's mean more to me than that of half the world. Besides the whole thing would certainly not have originated yet, and perhaps never would have, (I mean, not from me), if I had not had the importance of de Broglie's ideas really brought home to me by your second paper on gas degeneracy.[220]

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[218] Ibid., 3.
[219] Ibid., 24.
[220] Ibid., 26.

Again in this April 1926 exchange, Schrödinger is referring to the correspondence between Einstein and himself throughout 1925 in which he was embarrassed by a misunderstanding of one of Einstein's degeneracy papers leading him directly to a thorough and deliberate study of de Broglie's dissertation, and as a result the development of his own reformulation of quantum mechanics. Finally, at the end of April 1926 Einstein assured Schrödinger of his alliance stating: "I am convinced that you have made a decisive advance with your formulation of the quantum condition, just as I am equally convinced that the Heisenberg-Born route is off the track."[221]

Of these three eminent physicists, Lorentz was certainly the oldest and the most penetratingly critical. In a series of long probing letters, Lorentz, who by this time was 72 years old, addressed very specific concerns that he had with Schrödinger's quantum mechanical formulation. While he made many interesting and poignant critiques there was one in particular that could not be explained away by Schrödinger and would continue to harass the wave mechanical formulation. In his letter of May 27, Lorentz pointed to the problematic characteristic of transience in the physical nature of a "wave packet":

> If I have understood you correctly, then a "particle", an electron for example, would be comparable to a wave packet which moves with the group velocity…But a wave packet can never stay together and remain confined to a small volume in the long run. The slightest dispersion in the medium will pull it apart in the direction of propagation, and even without that dispersion it will always spread more and more in the transverse direction. Because of this blurring a wave packet does not seem to me to be very suitable for representing things to which we want to ascribe a rather permanent individual existence.[222]

While Schrödinger complained to Planck that he felt Lorentz was being "very critical" of his attempt to reinterpret classical mechanics as wave mechanics, Lorentz did give Schrödinger

[221] Ibid., 28.
[222] Ibid., 47.

reason to be somewhat optimistic. Along with his laundry list of critiques, Lorentz declared "If I had to choose now between your wave mechanics and the matrix mechanics, I would give the preference to the former, because of its greater intuitive clarity…"[223] While this undoubtedly gave Schrödinger a sizeable ego boost by going right to the heart of the ongoing quantum mechanical debate, he responded by declaring: "…may I emphasize several serious difficulties of a fundamental nature in the matrix mechanics…which have gradually become clear to me and in which I see an advantage in the wave mechanics, quite apart from its intuitive clarity." He then went on to discuss some of the advantages of wave mechanics including the general ease of "symmetrization of the Hamiltonian function" and its facilitation in deriving "completely determined eigenvalues."[224]

From this correspondence one gets the sense that Schrödinger was already quite sensitive to the various conflicts brewing between himself and some of the physicists at Göttingen who had produced and elaborated on the matrix mechanics. Schrödinger's sensitivity was not unwarranted because as we know there were critical voices emanating from Göttingen physicists, in particular, Heisenberg's response to Schrödinger's wave mechanics was surprisingly vitriolic and emotional. In a letter to Pauli he described himself as "deeply disturbed" by the prospect of the new formulation and stated: "The more I reflect on the physical part of the Schrödinger theory the more detestable I find it."[225] Words like "disturbed" or "detestable" seem unusually harsh for a scientific disagreement yet Heisenberg was clearly moved enough to use them. According to Beller's analysis, Heisenberg's vitriolic outbursts derived, in part, from a need to defend his territory. In his eyes the Göttingen physicists had

[223] Ibid., 44.
[224] Ibid., 63-65.
[225] M. Fierz and V.F. Weisskopf eds., *Theoretical Physics in the Twentieth Century*, (New York: Interscience Publishers Inc., 1960), 44.

been the first group to develop a comprehensive and cohesive quantum mechanics, and it seemed to him that Schrödinger was arriving late to the party and managing to garner all of the attention.[226]

In fact, while severely criticizing Schrödinger's approach on the one hand, Heisenberg was quick to notice the potential benefits it might yield as an analytical tool if it could be incorporated into the Göttingen quantum mechanical picture. In a letter to Dirac dated April 9th, 1926 he explicitly states this desire:

> A few weeks ago an article by Schrödinger appeared…whose contents to my mind should be closely connected with quantum mechanics. Have you considered how far Schrödinger's treatment of the hydrogen atom is connected to the quantum mechanical one? This mathematical problem interests me especially because…one can win from it a great deal for the physical significance of the theory.[227]

The severity of Heisenberg's emotionally charged response was not unanimous among all the young physicists who supported the Göttingen matrix program. For example, while Jordan, Dirac, and Pauli seemed initially to react negatively or dismiss the new wave mechanical formulation, each physicist's initial reaction was more subdued than Heisenberg's and seemed to evolve along its own unique path. Pauli's response is particularly instructive as it evolved rather quickly due to various factors. In the fall of 1921, after receiving his doctorate from Munich under the direction of Sommerfeld, Pauli spent the next two years working on quantum theory with Born in Göttingen and Bohr in Copenhagen. The Austrian physicist was particularly close with Heisenberg and Bohr and developed an extensive catalog of personal and scientific correspondence with many other physicists. Pauli had a reputation for being generally quick witted and acerbically critical of notions he disagreed with. In particular, he was known for

[226] Beller, *Quantum Dialogue*, 32-33.
[227] As quoted in: Beller, *Quantum Dialogue,* 36. (from Heisenberg to Dirac, 9 April 1926 AHQP).

developing many of his most innovative concepts within the dynamic of his correspondence and not always following through by publishing the fruits of his research.[228]

In the early winter of 1925-26 Pauli was working as a lecturer at the University of Hamburg, when he set out to apply the Göttingen matrix formulation and derive hydrogen's observed electromagnetic spectrum. One year earlier, Pauli had endeavored to explain why atomic electrons were grouped into shells as had previously been shown by the likes of Irving Langmuir and Niels Bohr. Pauli explained the atomic electronic shell structure by proposing four quantum degrees of freedom (corresponding to the modern canonical quantum numbers n, l, m_l, and m_s) and developed his "exclusion principle" which provided a guarantee that each electron within an atom would occupy a state characterized by a unique combination of these four quantum degrees of freedom. As a result, he predicted a new two-valued quantum number which within the year would be identified as electron spin (m_s) by Ralph Kronig, George Uhlenbeck, and Samuel Goudsmit.[229]

In the spring of 1925 Pauli was despondent about the state of affairs in quantum theory. He saw the constant shifting of the foundations as haphazard and unsystematic. In his opinion, the quantum dialectic was pulling physics in too many seemingly irreconcilable directions. In a moment of weakness Pauli confided in a friend that "Physics is very muddled again at the moment; it is much too hard for me anyway, and I wish I were a movie comedian or something like that and had never heard anything about physics!"[230] By that fall, Pauli was no doubt in much better spirits as the Göttingen physicists had managed to bypass some of the problems

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[228] Charles P. Enz, *No Time to be Brief, A scientific biography of Wolfgang Pauli*, (Oxford: Oxford University Press, 2002).

[229] The evolution of quantum numbers is detailed in Darrigol *From c-Numbers to q-Numbers*. Pauli's exclusion principle introduced the fourth quantum number but the first three had already been postulated by Bohr and others in the previous decade.

[230] Przibram, *Letters on Wave Mechanics,* x.

encountered by earlier incarnations of quantum theory and had developed their matrix mechanics formulation. In successfully deriving the hydrogen spectra from the matrix formulation, Pauli gave the Göttingen physicists one of the few, early verifications of the new quantum mechanics.

After seeing Schrödinger's first quantization paper in February, Pauli wrote to Sommerfeld, his former doctoral advisor, complaining that the new formulation was "*verrückt"* (crazy). To Pauli's surprise Sommerfeld had already developed a deep appreciation for Schrödinger's approach, eventually characterizing it as "…the most astonishing among all the astonishing discoveries of the 20th century."[231] In fact, in Munich there was considerable support for Schrödinger's program during the spring and summer of 1926 as Sommerfeld, his assistant Gregor Wentzel, and Wilhelm Wien, all believed that the wave mechanical approach was more practical and closer to what David Hilbert had famously referred to as the doctrine of "preestablished harmony between nature and mathematics"[232] than matrix mechanics could ever be. Swayed by Sommerfeld's support of Schrödinger's approach, Pauli reconsidered Schrödinger's wave mechanical formulation and then proceeded to prove its equivalence to matrix mechanics. His efforts in April of 1926 were never published and only appeared in his personal correspondence with Jordan.[233] Nevertheless, they stand as a testament to the unique character of each physicist's intellectual struggle with the evolving foundations of quantum mechanics and their implications on the Interpretations of quantum theory.

[231] As quoted in Beller, *Quantum Dialogue,* 32.
[232] Schweber, "Weimar Physics: Sommerfeld's Seminar and the Causality Principle," 269.
[233] Beller, *Quantum Dialogue,* 44.

In subsequent years, Pauli would become a staunch advocate of the Schrödinger wave-mechanical formulation going so far as to acerbically criticize Born and Jordan's 1930 book *Elementary Quantum Mechanics* for failing to mention the Schrödinger equation even once:

> Many results of quantum theory can indeed not be derived at all with the elementary methods [matrix mechanics] defined above, while the others can be derived only by inconvenient and indirect methods. …The restriction to algebraic methods also often inhibits insight into the range and the inner logic of the theory.[234]

By the late spring of 1926, Pauli had sided with Sommerfeld and thus distinguished himself from his young Göttingen colleagues by his acceptance of Schrödinger's new formulation. This assertion is strengthened when one reads Schrödinger's correspondence with Planck from May 1926 in which we learn that the Austrian had organized meetings in Zürich for the following month at which "a number of foreign physicists (among them Sommerfeld, Langevin, Pauli, Stern, [and] P. Weiss) are meeting here for lectures and discussions."[235] It seems that Sommerfeld was so impressed by Schrödinger's approach to quantum theory that he decided to devote an entire summer colloquia series to its study. As Hans Bethe later recalled, "It was clear to him that this was the theory of the future, and that every thesis student would have to know it."[236]

3.7- Why did de Broglie Not Develop His Own Wave Mechanics in 1925-26?

In 1969 V.V. Raman and Paul Forman published a paper entitled: "Why Was It Schrödinger Who Developed de Broglie's Ideas?" in which they argue for three determining factors that contributed to the fact that Schrödinger was the singularly qualified physicist who in

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[234] As quoted in Beller, *Quantum Dialogue,* 38.
[235] Przibram, *Letters on Wave Mechanics,* 8.
[236] Hans Bethe, *Physics in Perspective Vol 2*, (Basel: Birkhaüser Verlag, 2000), 3.

1925-26 could have developed de Broglie's ideas regarding the synthesis of wave-particle formulations via the use of the Hamiltonian analogy. First, they claim that due to de Broglie's problematic reputation in the field of quantum theory anyone seriously engaged in theoretical spectroscopy and atomic physics in 1925 would tend to ignore or dismiss his contributions. Furthermore, because according to the authors, Schrödinger was a "marginal" figure in these fields of study, he was more apt to accept de Broglie's ideas. Second, the authors point to the pre-existing division in the quantum community between Einstein's supporters and those that followed Bohr's lead into the later interpretation debates. While most narratives point to 1927 as the breaking point between the two "mythological quantum camps," Raman and Forman claim that the division significantly predates 1927 and that in 1925 Schrödinger was already naturally more aligned with Einstein and de Broglie in the interpretation debates. Lastly, the two authors argue that Schrödinger and de Broglie had a strong fundamental resonance in their overall body of work, which made it very easy for the Austrian to pick up where de Broglie left off.[237]

While Raman and Forman's fundamental question seems reasonable enough, and some of their analysis is reflected in my own discussions throughout the past two chapters, there are aspects of the authors' argument that seems a bit dated. Raman and Forman seem to emphasize certain specific controversies between Louis de Broglie and Copenhagen physicists making it seem like his reputation, in particular, was more tainted than it really was. The authors miss the opportunity to incorporate into their analysis the residual international tensions from World War I in which the entire scientific community was embroiled. As we noted in chapter two there was certainly heightened tension in Copenhagen in regards to the

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[237] V.V. Raman and P. Forman, "Why Was It Schrödinger Who Developed de Broglie's Ideas?" *Historical Studies in the Physical Sciences*, Vol. 1 (1969): 291-314.

priority dispute with Urbain and Dauvillier, but this controversy should be understood in its full context including the individual scientific flux and the underlying tensions emblematic of a general international tension in the scientific community. In addition, in their analysis Raman and Forman seem to overemphasize the preexistence of the two quantum camps that have dominated mythological narratives. In claiming that Schrödinger was aligned early on with both Einstein and de Broglie in a deterministic camp the authors reduce the complexity and nuance of the quantum flux existing at the time.

To truly understand the context of scientific reactions one must place each particular school within the larger post World War I international scientific milieu. There were multiple lines of inquiry all simultaneously trying to further develop quantum theory so each physicist's reaction to de Broglie's ideas represents his own state of mind in 1924-25. For example, as we have already seen, in Copenhagen they mostly ignored de Broglie, but we should understand that reaction in conjunction with the fact that Bohr and Kramers were very busily focused on developing their own novel quantum theory (BKS theory) in 1924-25. While in Göttingen, the response to de Broglie was actually quite favorable as both Franck and Born were somewhat receptive to his ideas. As we saw, encouraged by Born and Franck, Elssaser went on to successfully apply de Broglie's theory to preexisting unexplained empirical results. Ultimately, it seems too limiting to generalize or homogenize a reaction to de Broglie's ideas by the entire quantum community. As I have begun to argue in this chapter and will develop further in the next chapter, the divisions in the quantum community were neither homogenous nor polarized. There were many diverse opinions on the development of quantum theory and while there were things Einstein, de Broglie, and Schrödinger certainly agreed on, they were far from in sync on many points. In the following chapter we will see some of these nuances and discuss the highly fractured nature of these two mythological camps.

As a last point, I would like to address the very nature of the question Raman and Forman posed in their title. From the question posed in their title, it seems that they approached their study presupposing that de Broglie had no agency of his own. By asking the question the way they did, the authors placed the onus of innovation on Schrödinger and did not bother asking what de Broglie was actually doing while his ideas were being extended in Zürich. In other words there is an inherent assumption that de Broglie was not developing his own parallel brand of wave mechanics in Paris. As you will soon learn, this assumption is far from true. De Broglie did not produce Schrödinger's wave mechanics because he was busy producing his own. While the two theories were fundamentally different from each other both in formulation and interpretation, they played an integral part within the larger landscape of intense speculation and innovation associated with quantum theory between 1923 and 1927. As we have shown, there are many contributing factors that need to be understood in any thorough historical analysis of quantum wave mechanical innovations. Having said that, there is one simple factor that always seems to be overlooked and which was certainly critical to the way in which the wave mechanical narrative played out. From studying Schrödinger's correspondence we know that he was a very effective self-promoter and that he fought very effectively for his brand of wave mechanics. On the other hand, de Broglie was an insular scholar who's personality did not contribute to his networking capabilities. It is important that we keep this factor in mind, along with all the other contributing contextual parameters, when considering their contributions to the quantum revolution.

The ideal historical study of this period in the development of quantum physics, and in particular wave mechanics, would include a superposition of various individual analytical threads including the international scientific community's post World War I dynamics, the various particular interactions and networks between the four quantum schools and those outside the quantum bloc, each physicist's individual agency and personality, and the particular scientific dialog that propelled each innovative step. In the first three chapters of this dissertation we have examined these various contributing threads, attempting to paint a more complete and nuanced picture of early quantum innovations and those physicists who were responsible for their creation and dissemination. In the next chapter we will examine the period leading up to the Fifth Solvay Council in the fall of 1927 and show that de Broglie's deterministic wave mechanical theory that he presented at this historic scientific meeting was not a flash in the pan idea, but was a result of his parallel wave mechanical program that had begun in 1923 and had continued right through to the fall of 1927.

Chapter 4: Coupled Quantum Narratives- Various Attempts at Alternate Quantum Theories

> *[Physics is running] the grave danger of getting severed from its historical background. … History is the most fundamental science, for there is no human knowledge which cannot lose its scientific character when men forget the conditions under which it originated, the questions it answered, and the functions it was created to serve. –E. Schrodinger*[238]

4.1- Introduction

In Chapters two and three, we looked at the quantum physics community of post World War I and how its internal networking, organization, and complex scientific dialog contributed to the emergence of the first instances of quantum wave mechanical theories. We noted that other historical narratives have fallen short by focusing almost entirely on the development of the matrix formulation and leaving the inclusion of the seminal de Broglie-Schrödinger creative dynamic to other scholars.[239] An incorporation of this dialog into the overall story of the development of quantum physics has been one of our primary goals throughout this narrative.

We have seen de Broglie's role in the quantum revolution, as portrayed in Table 1.1, reduced to almost a mere footnote. According to these histories, de Broglie's insights from his work leading up to his doctoral dissertation were only important in that they extended Einstein's quantum hypothesis to the realm of matter and served as a spark for the true innovative leaps from more famous quantum theorists such as Schrödinger. Our discussion in chapter three was aimed at redressing this distortive appraisal of de Broglie's place in the history of the development of quantum physics. A reading of de Broglie's 1924 dissertation and a subsequent

[238] Przibram, *Letters on Wave Mechanics,* xiv-xv.
[239] Beller, *Quantum Dialogue*

analysis of the impetus behind Schrödinger's barrage of papers, in the winter and spring of 1925-26, served to unequivocally restore the Frenchman's seminal status within the early maturation of quantum theory. So why have most traditional historical narratives chosen to downplay de Broglie's contributions? This question is central to our discussion in chapters four and five, in which we address the establishment of quantum orthodoxy around the equivalent matrix and wave-mechanical formulations established by the Göttingen physicists and Schrödinger respectively, and the correlated abandonment of alternate interpretations and deterministic wave-mechanical formulations.

Just as we did in chapter three when we redressed de Broglie's role in the quantum revolution we can once again point to a deficiency in the traditional quantum mythology represented in Table 1.1. In these mythologies, one is taught that after de Broglie had arrived at his wave particle duality insight in 1924, Heisenberg, with the help of Born and Jordan, took the extreme step of developing a fully integrated and widely applicable quantum mechanics based on a matrix formulation throughout the summer and fall of 1925. Then, after recognizing the power of this new quantum formulation, Schrödinger proceeded to independently develop his own version of quantum mechanics using a differential wave equation. By the late spring of 1926 the two formalisms were shown to be mathematically equivalent representations with the matrix formulation geared more to particle dynamics and Schrödinger's wave equation formulation more applicable to continuous phenomena. In this historical telling, by the summer of 1926, when Born developed his probabilistic interpretation, both the matrix and the wave formulations were considered fully mature theories that simply needed to be interpreted and then widely applied. As a consequence, a deep division grew around the acceptance of the two possible formulations along interpretive lines: the conservative ranks of the physics community embraced Schrödinger's continuous wave equation in the hopes that determinism would be restored; while the young, malleable, and revolutionary minds began to grasp the implications of discontinuity revealed in the matrix formalism and pushed to set aside many of the heretofore accepted classical assumptions about nature.

In this chapter we will show that this traditional telling falls short because it oversimplifies the complexities of the formulation and interpretation debates from the very inception of quantum mechanics in 1925. Here, we are interested in understanding the emergence, development, and eventual abandonment of alternate formulations and interpretations of quantum theory during the late 1920s. Only in understanding the complex dynamics surrounding the subsequent abandonment of these research programs and the establishment of the Göttingen-Copenhagen orthodox interpretation, can we later hope to understand the full context of David Bohm's later reexamination of the foundations of quantum theory in 1951.

As many quantum narratives focus only on the establishment of the Göttingen-Copenhagen Interpretation of quantum theory, they naturally avoid devoting significant analysis to the development of alternate quantum formulations and interpretations. However, in order to understand the true contingent nature of quantum revolution and explain why the Göttingen-Copenhagen Interpretation gained orthodox status while other variants of the theory were abandoned, it is this very avoidance that needs to be studied. As we shall see in this chapter, the establishment of what became the orthodox quantum interpretation was more complex than familiar narratives would suggest. It involved many parallel yet coupled threads of dialog by physicists who were simultaneously engaged in research that was both speculative and innovative. As such, there were several alternate potentially viable formulations and interpretations of quantum theory that were proposed in 1926-1927 that built from

Schrödinger's wave mechanics in different ways. What is clear from the historical record is that for various correlated reasons these alternate variants of quantum theory were almost entirely abandoned by the late 1920s.

Many scholars have characterized this process of abandonment as a 'marginalization' of alternate formulations and interpretations of quantum theory. While the term marginalization may be appropriate when applied to aspects of the history of alternate quantum theories themselves, the evidence shows that it was certainly not the defining characteristic of this revolutionary period. Simplifying the abandonment of certain research programs and lines of inquiry, by labeling them an act of marginalization by the orthodox establishment, is just as distortive as the mythological narratives we have argued against in this study. Therefore, it is critical that we avoid using terms such as marginalization in connection with more nuanced and complex historical processes. We should not succumb to temptation and vilify a certain group of scientists or unfairly victimize certain physicists by subverting their personal agency. Too many scholars have relied on the abstract notion of marginalization to argue for alternate contingent histories of quantum theory.[240] Maybe the best known of these was by the well known philosopher of science, James T. Cushing who produced a counterfactual historical study of hidden variables theories:

> It is astounding that there is a formulation of quantum mechanics that has no measurement problem and no difficulty with a classical limit, yet is so little known. One might suspect that it is a *historical* problem to explain its marginal status.[241]

[240] In particular let me point to the work of G. Bacciagaluppi, M. Beller, J. T. Cushing, D. Durr, S. Goldstein, R. Omnès, A. Valentini, N. Zanghi, and W.H. Zurek.
[241] James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, (Chicago: University of Chicago Press, 1994), Xii.

The present analysis will attempt to develop a more representative picture of the quantum revolution's peak years 1926-27 thereby reestablishing the complexity and nuance of a two-year period that was so formative in the history of modern physics that it shaped the approach to research in the physical sciences for decades to come. As a result of this period of innovation characterized by multiple simultaneous competing approaches to quantum theory, the physics community transitioned into an extended period exemplified by pragmatism, an abandonment of interpretation debates, and a consolidation of orthodoxy.

There is an obvious coupling between the suppression of alternate interpretations such as de Broglie's wave mechanics and the establishment of quantum orthodoxy but often-times simplistic explanations have sufficed when interpreting this complex coupling. In the following discussion, we will do our best to capture the required nuance so that we can extract a more complex quantum narrative of this period. Was this abandonment due to a purposeful marginalizing power play by a growing "hegemony," in order to defend an orthodox view as some scholars have argued? Or, was it a conscious abandonment, solely due to internal scientific logic based on concrete empirical data available at the time that drove de Broglie and others to sweep their theories into the great rubbish bin of defunct scientific hypotheses? The answer is, not surprisingly, more nuanced than either of these two extremes would seem to indicate and yet both threads have a place in our story.

4.2- Applying, Extending, and Interpreting Schrödinger's Formalism

As we noted in the previous chapter, there were myriad of responses to Schrödinger's wave mechanical innovations within the physics community. Without a doubt, his series of five publications between January and June of 1926 caused a significant stir. From the autumn of

1925, when the Heisenberg-Born-Jordan matrix formulation of quantum theory first emerged from Göttingen, the physics community had been struggling to grapple with the new abstract and opaque formalism. We saw how Lánczos' work in Frankfurt on an equivalent formalism based on integral equations was a perfect example of this particular struggle. When Schrödinger's wave equation appeared on the pages of *Annalen der Physik,* in early 1926, it was a much celebrated innovation. After all, it was a quantum formulation 'equivalent' to the matrix formulation in explanatory and predictive power, allowing one to describe the discrete-like phenomena emerging from the atomic realm; while remaining firmly planted in an analytical mathematical language that was continuous, somewhat visualizable, and already familiar to the greater scientific community. Upon the arrival of Schrödinger's wave mechanics there was little doubt which equivalent formulation was the preferred one amongst the majority of working physicists. Even Max Born, who had been one of the fathers of matrix mechanics, initially admitted the superiority of Schrödinger's wave mechanical approach when he stated in a paper published during the summer of 1926 that "…I am inclined to regard it as the most profound formulation of the quantum laws"[242]

While the vast majority of quantum physicists recognized the power of Schrödinger's new formalism, problems began to arise surrounding some of his interpretive stances. Early on during Schrödinger's outpouring of communications there were those who had already begun to apply his new mechanics with great success. Apart from Schrödinger himself and Pauli, as we noted in chapter three, there were others such as the Russian Vladimir Fock and the American Carl Eckart who independently published a series of papers in which they quickly took advantage of the familiar wave equation formalism proposed by Schrödinger and successfully applied the new wave mechanics to the study of various physical phenomena. However, by the summer of

[242] Jammer, *The Conceptual Development of Quantum Mechanics,* 301.

1926 it was becoming clear that although his wave equation was pragmatically successful, Schrödinger's physical interpretations of his nascent theory were highly problematic. The notion that particles could, in general, be fully described by a localized wave-packet and that this wave group would not spread out in space was shown to be erroneous by Heisenberg. Moreover, Born, who initially thought Schrödinger's wave mechanical picture was so profound, nevertheless could not agree with him on his interpretations of the physical significance of the wave function. We will see in the next section how Born developed his probabilistic interpretation of the wave function as a direct counter to Schrödinger's conceptualizations. Born's development of this probabilistic theory ultimately led to the reinforcement of an opaque interpretation of quantum phenomena which eventually formed the basis of the Göttingen-Copenhagen Interpretation.

4.2.1- Formulation vs. Interpretation in the Development of Quantum Theory

We know that Planck had been insistent on securing a visit from Schrödinger to Berlin before the 1926 summer session began so that he might lecture on his new wave mechanics at the University and at the Academy of Sciences. As a result, Sommerfeld took advantage of Schrödinger's planned visit to Berlin and invited him to lecture in Munich as well. On July 21st, 1926 Schrödinger gave a talk as part of Sommerfeld's colloquia summer series in which he introduced his wave-mechanical formulation and fielded questions. While Sommerfeld, Wien, and Wentzel were all supportive of his work, Schrödinger did encounter some resistance; in particular, there was one voice of opposition that stood out from the rest. Heisenberg had arranged to spend July with his parents in Munich specifically so that he could confront Schrödinger in person about his quantum mechanical formulation. After the lecture Heisenberg began his critique by first stating his admiration for the beauty of the mathematics behind the wave mechanics. He then quickly followed this compliment by objecting to Schrödinger's "continuous" formulation claiming that it could not effectively account for many established quantized physical phenomena. He identified the photoelectric effect as particularly problematic; he also "pointed out that Schrödinger's conception would not even help explain Planck's radiation law." Wien immediately jumped in to defend Schrödinger's mechanics chastising Heisenberg and claiming that "…[matrix] mechanics was finished, and with it all such nonsense as quantum jumps…" While Schrödinger was less dogmatic, he nevertheless wholeheartedly agreed with Wien that the problems that were as yet unresolved by his formulation of quantum theory would be worked out in due time. He also agreed with the eminent German physicist in his characterization of Bohr's quantum jumps as "nonsense."[243]

This division between continuous and discrete formalisms was not new. We have already seen in previous chapters that the very reality of light quanta was questioned throughout the early 20th century by many physicists. However, by the mid 1920s, Niels Bohr represented a vocal yet shrinking minority of the scientific community that stood defiantly against the notion of real light quanta, even as a growing bevy of experimental data pointed to their undeniable existence. As a result, the murkiness surrounding the dual nature of light and matter was by no means resolved in 1926. That very spring, after Schrödinger's first quantization papers emerged Enrico Fermi published a paper titled "Argomenti pro e contro la ipotesi dei quanta di luce," translated as "Arguments for and against the hypothesis of light quanta."[244] In this short article Fermi unequivocally stated that "at the present time the state of science is such that one can say that we lack a theory that gives a satisfactory account of optical

[243] Schweber, "Weimar Physics: Sommerfeld's Seminar and the Causality Principle," 286.
[244] Enrico Fermi, "Argomenti pro e contro la ipotesi dei quanta di luce," *Il Nuovo Cimento* **3** (1926), Rivista, XLVII–LIV; reprinted in *Enrico Fermi, Collected Papers*. *Vol. I. Italy 1921–1938*. Edited by E. Amaldi, et al. (Chicago: The University of Chicago Press, 1962), 201–206.

phenomena." He also went on to claim that the gravest challenge facing physics was to find a way of reconciling a quantum explanation of the atomic interactions of light and matter with the corresponding macroscopic optical phenomena.[245]

Fermi took on a pragmatic approach which was representative of many physicists who did not particularly emphasize the interpretive concerns of quantum theory and instead wanted to apply whichever formulation allowed them to solve an existing problem as effectively as possible. There were many notable physicists in this grouping who were equally happy using whatever formulations were available so long as they worked. Sir William Bragg's famous 1921 quote about using both wave and particle theories on alternating days of the week, could be applied equally well to the use of wave and matrix mechanics in 1926.[246] Dirac, Wentzel, Fritz London and Charles Darwin were all examples of physicists who cared less about interpretation and more about applicability of the two distinct formulations. In particular Darwin wrote to Niels Bohr in December 1926 saying this much explicitly:

> It is part of my doctrine that the details of a physicist's philosophy do not matter much… Because the best sort of contribution that people like me can make to the subject is working out of problems, leaving the questions of principles to you. In fact even if the ideas on which the work was done are wrong from the beginning to end, it is hardly possible that the work itself is wrong in that it can easily be taken over by any revised fundamental ideas that you may make.[247]

The attitudes reflected in the comments by Bragg in 1921 and Darwin in 1926 are indicative of what many historians of this period have identified with the later development of a pragmatic

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[245] Schweber, "Weimar Physics: Sommerfeld's Seminar and the Causality Principle," 290.
[246] William Bragg, "Electrons and Ether Waves," The Robert Boyle Lecture 1921, *Scientific Monthly*, *14*, (1922): 158. Recall the quote from chapter three: "On Mondays, Wednesdays, and Fridays we use the wave theory; on Tuesdays, Thursdays, and Saturdays we think in streams of flying energy quanta or corpuscles."
[247] Beller, *Quantum Dialogue,* 4. Taken from the December 1926 correspondence between Darwin and Bohr (AHQP).

approach to the physical sciences which in many quantum narratives first arises in the early 1930s and can be encapsulated by a David Mermin quote: "Shut up and Calculate!"[248]

While a strong driver for many, this pragmatism surrounding the notion of formalism selection was not ubiquitous within the physics community. There were definitely physicists who placed their particular principles above pragmatism when developing or applying theoretical models. As we have repeatedly noted, Niels Bohr was the quintessential example of this type of physicist. Moreover, when perusing the list of quantum physicists who trained or worked in Copenhagen during their formative years, one immediately notices that Bohr's tendencies must have left an indelible mark on a significant portion of the quantum community. Having said that, it is clear that many physicists like Darwin and Fermi had a lower tolerance for philosophical speculation surrounding the interpretation of physical theory than did physicists such as Bohr, Einstein, de Broglie, and Heisenberg and preferred to focus on the practical applications of these novel theories.

One may object to this characterization of the physics community and claim that the prototypical physicist has always been engaged in both endeavors simultaneously, as a pragmatist applying formulations of theories to empirical evidence and as the abstract thinker exploring the philosophic interpretations of those very formulations. While these two endeavors exist in all physics research, it seems evident that generally there is greater emphasis placed on one program or the other based on the physicist's personality, training, and their particular context. This division between physicists who tended towards the pragmatic and those that preferred speculative research programs was clearly a central point of tension within

[248] N.D. Mermin, "Could Feynman Have Said This?" *Physics Today,* (May 2004): 10-11. While this quote is often attributed to Richard Feynman, according to Mermin he himself was the first to have uttered it in 1989.

the physics community as early as 1926-27. In fact, Oliver Lodge wrote a piece for *Nature* in March 1927 where he addressed this very problem.

> I know that this is a presumptuous letter, open to attack from many sides; but I do want to enter a protest against the idea creeping into scientific philosophy…that we are not out for truth, but only for handy methods of formulation; and, further, that we can contemplate absurdities, geometrical and other, not only as auxiliary and contemporary and intermediate aids, but also as representing something actually existing in the universe, although repugnant to our mental make-up.[249]

Lodge goes on to lambast those physicists, like Darwin, who were content to rest on some useful or pragmatic "convenience of expression" that happened to model empirical evidence effectively but abandoned the deeper search for ultimate ontological truth about the universe. He claims that statements approaching ontological truth like the conservation of energy would never have been discovered if physics was an exercise in pragmatic formulation construction. Lodge leaves no doubt what he thinks about these pragmatists, acerbically stating: "I venture to urge that contentions of that kind ought to be stigmatized and rebelled against as heresy."[250]

While Lodge was clearly opposed to the pragmatist approach to physics, he was equally opposed to interpretations that only scratched the surface and were willing to accept, too easily, unsatisfactory frameworks that opposed our commonly held ontological truth. He saw the various interpretations coming out of Göttingen and Copenhagen as highly problematic because they were conveniently content with resting on what he termed "useful absurdities" and not pursuing deeper ontological truth, as he understood it. He qualifies the notion that "all manner of absurdities can be tolerated if they help us arrive at results." By stating:

> Well, I admit that they can be tolerated in an interim manner; … but they always disappear from the final result, or at least from the interpretation of that result. The tendency that I fear is that some of the absurdities will be retained by philosophically minded interpreters, and will be foisted into their scheme of reality. For example, in dealing with the quantum drop of an electron from one orbit to another, some writers

[249] Oliver Lodge, "Truth or Convenience" *Nature, No. 2994, Vol. 119.* (1927): 423-424.
[250] Ibid., 424.

> seem willing to contemplate the extinction of an electron in one place and its re-creation in another; so that its world-line, instead of being continuous, is dotted. … [In addition to the] assumption that destination affects emission, or…that we can influence events which are already past.[251]

From Lodge's comments it is clear that already in the spring of 1927 the argument between the pragmatic use of formulations and the speculative research into deeper interpretations was in full swing.

4.2.2- Wave-Mechanical Innovations From Within the Four Primary Quantum Schools

As we noted earlier the development of the matrix and wave mechanical formulations of quantum theory did not happen in isolation from each other and were instead, highly entangled narratives. In studying the evolution of the matrix formulation one notices a dramatic shifting of interpretive statements and an evolving formulation in reaction to Schrödinger's wave mechanical picture. In fact, because Schrödinger's approach was initially so successful within the physics community, the Göttingen group was forced to quickly adapt their theory.[252] The transition from Heisenberg's first approach which radically did away with all geometry (and thus space-time) inside the atom to the notion that only probability mattered and finally, to the full restoration of the space-time container by Born all happened in under a year.[253] In addition, the idea that from the outset, there was a deep ideological conviction by the Göttingen physicists along the lines of a purely corpuscular interpretation of the matrix mechanical picture is historically untenable. Instead, we know that Heisenberg, Born, and Jordan based their new

[251] Ibid., 424.
[252] Beller provides ample evidence throughout her book for this from across the physics community. Born himself recognized this. In a later interview he claims that by 1930 the matrix formulation had been "completely pushed into the background" Beller, *Quantum Dialogue,* 36.
[253] Born was the first to restore to atoms the notion of space-time, followed quickly by Pauli and Jordan and then eventually by Heisenberg. Beller, *Quantum Dialogue,* 19.

mechanics, in part, on the BKS theory which was adamantly opposed to the notion of radiation quanta. As Born put it "We therefore take from now on the point of view that the elementary waves are the primary data for the description of atomic processes; all other quantities are to be derived from them."[254] Far from being opposed to a wave interpretation, the original matrix mechanics relied heavily upon it. After all, the only "observables" allowed by the matrix approach were the frequency, intensity, and polarization of the emitted radiation associated with an atomic transition.[255]

While Max Born's reaction to Schrödinger's formulation was quite different from the other physicists in Göttingen, its evolution was critical to the development of the overall quantum dialog in 1926. In the winter of 1925-26 as Schrödinger was first developing his wave-mechanical formulation, Born was lecturing at MIT about the need to address the incompatibility between kinematics and wave theory. While Born was one of the fathers of matrix mechanics, he was also one of wave mechanics' first champions. In studying collision theory and scattering in the spring and summer of 1926 he was immediately taken by the power of Schrödinger's new quantum mechanical formulation. It seems that Born was not initially looking to reinterpret quantum theory or decide upon the true nature of radiation as wave or particle, instead he was attempting to elucidate Bohr's notion of discrete quantum jumps (discrete energy transitions) by studying atomic collision processes. As he stated in his first collision paper submitted in June 1926, in choosing to use Schrödinger's wave-mechanical quantum formulation over the matrix formalism, he was simply being pragmatic. "Of the different forms of the theory only Schrödinger's has proved suitable for this process [collisions]…and exactly for that reason I might regard it as the deepest formulation of the

[254] As quoted in Beller, *Quantum Dialogue,* 22.
[255] Ibid., 22.

quantum laws."[256] This is a far cry from Heisenberg's early reactions and seems more in line with Planck or Sommerfeld's sentiments.

In this first collision paper Born avoided all sense of interpretive debates about what the ψ function represented and developed his analysis based fully on extending the notion of matter-waves proposed by both de Broglie and Schrödinger. This should not be hard for us to believe given that, as we saw in the last chapter, thanks to Einstein's immediate validation Born had been impressed by de Broglie's thesis and had even encouraged Walter Elsasser to pursue a line of research employing de Broglie waves to explain poorly understood phenomena like the Ramsauer effect. Of course as we know from the traditional quantum narrative, Born's belief in the depth of Schrödinger's formulation did not last beyond the early summer months. After extensive correspondence between the two physicists and Born's continued dialog with his unsympathetic Göttingen colleagues he became convinced that Schrödinger's wave mechanics would never amount to more than just a useful mathematical formulation without significant physical meaning. By the time he submitted his next two collision papers in late July, Born had already changed his mind about Schrödinger's approach. While he retained the useful wave-mechanical mathematical formulations, he did away with any physical notion or representation of matter-waves. By August Born had convinced himself that collision theory, and by extension all of quantum theory, should be studied assuming a discrete energy spectrum and a discontinuous or corpuscular dynamics guided by the laws of probability.[257]

As it turned out the disappearance of common ground between the two main formalisms was multi-faceted and depended on which two particular physicists were engaged in dialog. In the fall of 1926 quantum theory was truly in a state of flux and there were many distinctive issues that resonated more or less forcibly with each individual physicist. In many

[256] Ibid., 44.
[257] Jammer, *The Conceptual Development of Quantum Mechanics,* 301-302.

cases conflicts or debates were due to a lack of agreement on one or a combination of various factors such as the wave-particle dualistic picture, the physical interpretation of the wave function ψ, the discontinuous quantum jumps, the nature of wave-packet formation and dispersion, or the use of probability as a governing principle in quantum interactions. Any one of these issues was enough to undermine general consensus. What is clear from the previous discussion is that the fall of 1926 was not representative of a simple bifurcation of the physics community into two distinct ideological camps each advocating for competing formulations and interpretations of quantum theory.

Up to this point, we have shown that the origins of quantum mechanics involved complex and nuanced dialogs of both a public and private nature, between many different physicists each with their own corresponding interpretive lens. Furthermore, it was commonplace for interpretive frameworks and by extension interpersonal dynamics to shift and evolve over time. While this makes it difficult to get a general sense of where things stood in the "physics community" as a whole, it cannot be reduced to a polarized debate between two camps centered on wave mechanics and matrix mechanics. After all, by the fall of 1926 Born had essentially taken Schrödinger's mathematical formulation, decoupled it from any of its original physical meaning, and applied it successfully to the analysis of collision phenomena thus merging it with the discontinuous matrix formulation. In some sense the coupling of these two equivalent formulations was beginning to become a central feature of quantum theory, yet the controversy over interpretive frameworks was beginning to reach a fever pitch.

Within this context various physicists extended and altered the Schrödinger formulation and in doing so reinterpreted the very notion of the wave function ψ. These various lines of inquiry had different goals and underlying assumptions, in some cases they were primarily attempts at explaining a wider set of empirical results, in others they were geared towards reconciling the quantum theory with a core physical principle. When placed in this context these early alternate quantum wave-mechanical theories can be seen as a natural outgrowth of the general state of the times. Furthermore, while these alternate theories were undoubtedly eventually abandoned by their authors it is imperative that we gain a more nuanced understanding of the factors that led to this abandonment. The traditional characterization does not do this process of innovation justice and ultimately serves to distort.

We begin by briefly recalling Schrödinger's own interpretation of the physical significance of the wave function discussed in chapter three. In Schrödinger's first three papers he had originally imbued ψ with a phenomenologically real significance in strong accordance with de Broglie's notion of matter-waves or phase waves developed in his doctoral dissertation. In this conception, by a linear superposition of de Broglie-Schrödinger waves one could construct a wave packet that represented a particle of finite mass. Originally, the de Broglie-Schrödinger waves referred to real fields waving or resonating in space both inside and outside the atom, causing all manifestations of atomic phenomena. This program was based on the Hamiltonian analogy which made kinematics an approximation to wave mechanics in the same way that geometrical optics was an approximation to wave theory. Schrödinger had thus derived a wave equation that governed these wave functions and allowed one to solve real quantum mechanical problems based on a dynamic system's Hamiltonian conditions (H=T+V) kinetic plus potential energy constraints.

By early summer, 1926 when Schrödinger was working on his fourth quantization paper he had dramatically shifted his interpretation of ψ. In applying his new wave-mechanical formulation to time-dependent perturbations he arrived at his time-dependent wave equation which seemed to resemble a diffusion equation with a complex-valued diffusion coefficient:

$$-\frac{h^2}{8\pi^2 m}\Delta\psi + U\psi = \frac{h}{2\pi i}\frac{\partial\psi}{\partial t}$$

Where the wave equation was now a complex-valued function: $\psi = \varphi(q)e^{\left[2\pi i\left(\frac{E}{h}\right)t\right]}$

Schrödinger was forced to accept that in order to be applicable to quantum systems in general, the wave function could not be real-valued and instead, must reside in configuration space and be complex-valued. As a result Schrödinger's interpretation of ψ began to evolve. By the summer he was interpreting $\psi\psi^*$ within an electrodynamic framework, as a "weight function" of the quantum system's charge distribution so that when applied to electrons with charge e, $\rho = e\ \psi\psi^*$ was the electron charge density. Although their ideas had evolved a bit, Schrödinger and de Broglie adhered to the basic notion that massive particles were actually wave groups or "packets" made up of a superposition of an infinite number of wave functions where the group velocity of this wave group is what we observe as the particle's physical velocity. This notion remained central to Schrödinger's wave-mechanical quantum theory. After being criticized by Lorentz and others about the transience of wave-packets in space and time, Schrödinger felt compelled to address this particular problem. In a paper titled "On the continuous transition from micro- to macro-mechanics" published later that summer, Schrödinger tried to show this explicitly by constructing the wave packet for a linear harmonic oscillator.

While Schrödinger's approach worked well specifically for the linear harmonic oscillator, it was not a tenable solution for most other quantum systems corresponding to other potential energy configurations such as the "central potential" associated with atomic structure. By the end of the summer Born began to fall In line with Lorentz's and Heisenberg's critiques of the physical interpretation of ψ. As it turned out, not only was a stable wave packet seemingly impossible to construct for many physical systems, but when studying collision phenomena, Born became convinced that relying on Schrödinger's wave mechanical approach made it extremely difficult to explain how a particle conserves its shape during and after a collision process. In fact, Born decided that, in general, modeling quantum phenomena based on wave packet construction was not realistic and that while Schrödinger's wave equation and corresponding wave function were really useful mathematical tools they needed a dramatically new physical interpretation.

Ultimately, Born rejected Schrödinger's interpretation of ψ outright and reinterpreted the wave function by associating it with a concept that Einstein had put forth years earlier called the "Gespensterfeld"- usually translated as Phantom or Ghost field. Einstein had originally developed, but never published, this concept to explain the correlation between oscillating electromagnetic fields and light quantum trajectories. Born interpreted Einstein's ghost fields as guiding fields for the physical corpuscular light quanta in which he took their wave-amplitude squared as the probability of a light quanta being present at a certain position and time, or within a larger statistical sample, as the measure of the density of said quanta (intensity of light). Born argued that since de Broglie had shown that wave-particle duality is equally applicable to radiation quanta and to the dynamics of particles with mass, the notion of ghost fields should translate from radiation phenomena to kinematics as well. However, if he assumed a light-particle equivalence due to de Broglie's work then, as with the ghost field interpretation, statistically speaking $|\psi|^2$ would be the probability density for particles as well.[258] In correspondence from 6 November 1926 Born made his change of heart regarding the interpretation of ψ explicitly clear to Schrödinger.

[258] Jammer, *The Conceptual Development of Quantum Mechanics,* 302-303.

> You know that immediately after the appearance of your first works I expressed very strongly my enthusiasm for your conceptions in my treatise. Heisenberg from the beginning did not share my opinion that your wave mechanics is more physically meaningful than our quantum mechanics; yet the treatment of the simple phenomena of aperiodic processes (collisions) led me initially to believe in the superiority of your point of view. In the meantime, I found myself again in agreement with Heisenberg's position.[259]

This letter by Born was in response to a letter he had received from Schrödinger on November 2nd, in which the Austrian physicist pleaded with Born to remain open on interpretive debates until more research had been done. In this letter, Schrödinger made it clear that he believed Born, Heisenberg, Bohr and others were being too dogmatic in prescribing the notion of discontinuous and instantaneous quantum jumps rather than trying to reconcile micro and macro domains through a consistent space-time framework.[260]

> I have, however, the impression that you and others, who especially share your opinion, are too deeply under the spell of those concepts (like stationary states, quantum jumps, etc.), which have obtained civic rights in our thinking in the last dozen of years; hence you cannot do full justice to an attempt to break away from this scheme of thought.[261]

In an interview, Jordan later recalled a telling anecdote from this period in Göttingen. As Jordan tells it Born was already creating an antagonistic environment into which he was indoctrinating a large group of promising younger physicists:

> Once on a walk in Göttingen with Dirac and Oppenheimer and perhaps with one or two persons…Born read a letter from Schrödinger aloud, and Oppenheimer told me jokingly that Born, as if he were commander in chief summoning his army, explained to everybody how false were Schrödinger's ideas at the time.[262]

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[259] Beller, Quantum Dialogue, 44-45.
[260] Ibid., 46.
[261] Ibid., 47. AHQP (Schrodinger to Born November 2, 1926)
[262] Beller, Quantum Dialogue, 46 (AHQP Jordan interview) Oppenheimer plays a central role in our later narrative. See chapters five and six.

Jordan's recollection fits in with the tense confrontational mood that began to settle into the quantum field in the fall of 1926 and was carried through to 1927. In this period it became clear that while Born had been one of Schrödinger's most zealous champions he had reached a point where his evolving understanding of the theoretical foundations of quantum theory in conjunction with consistent exposure to experimental results produced in Franck's laboratory in Göttingen could not justify anything other than a corpuscular, discontinuous, and probabilistic micro reality.

Meanwhile, two months earlier, as a result of the confrontation between Heisenberg and Schrödinger in Munich, Bohr invited Schrödinger to visit Copenhagen, where Heisenberg was then working as a visiting researcher, in order to try and reach a common ground on interpretational matters. In his visit to Copenhagen Heisenberg and Bohr directly confronted Schrödinger about the wave mechanical interpretation challenging him on his preference for a purely undulatory and continuous theoretical framework. This visit has entered many traditional quantum narratives as the moment when Bohr and Heisenberg ganged up on Schrödinger and criticized him so severely that he fell ill and then ultimately abandoned interpretative debates for close to a decade. Unfortunately, there are no reliable primary sources which documented the debates as they unfolded, so these narratives have relied heavily on recollections and memoirs by Bohr and Heisenberg. Particularly influential has been a memoir written decades after the events took place, by Heisenberg in which he describes the events in detail.[263]

[263] Heisenberg's account is generally taken from his memoir *Der Teil und das Ganze* (*The Part and the Whole*), a recollection of the events that took place in Copenhagen during Schrödinger's September – October 1926 visit. This particular account is taken from Moore, *Schrödinger, life and thought,* 162-163.

According to Heisenberg's account he and Bohr met Schrödinger at the rail station and immediately engaged him in long, vigorous, and contentious debates that went on for days and challenged the besieged Austrian on his continuous interpretive framework. The representatives of the Copenhagen school claimed that the purely wave-mechanical approach was extremely problematic and could not be reconciled with important experimental evidence. On the other hand Schrödinger continued to stand his ground claiming that those experiments were not definitive enough and that there was still significantly more work to be done to reconcile the micro and macro domains before a final interpretation could be settled upon. From his perspective giving up on space-time continuity at this early stage and accepting what he called "ad-hoc assumptions" like the "damn quantum jumps" was not the best way to approach a solution. In an oft quoted outburst during one of these debates in Copenhagen Schrödinger apparently stated: "If we are going to have to put up with these damn quantum jumps, I am sorry that I ever had anything to do with quantum theory." To which Bohr reportedly replied: "But the rest of us are very thankful for it… [because] your wave mechanics in its mathematical clarity and simplicity is a gigantic progress over the previous form of quantum mechanics."[264]

From Heisenberg's recollections, while Bohr was very complimentary about Schrödinger's mathematical formulation of quantum mechanics, much to Heisenberg's disappointment, he was also extremely critical of the Austrian's interpretations: "[A]lthough Bohr as a rule was especially kind and considerate in relations with people, he appeared to me now like a relentless fanatic, who was not prepared to concede a single point to his interlocutor [Schrödinger]…" Heisenberg claimed that due to all this drawn out confrontation, Schrödinger

[264] Moore, *Schrödinger, life and thought,* 162-163.

became mentally and emotionally exhausted and as a result fell ill. As a result, after leaving Copenhagen in October 1926, Schrödinger felt demoralized and defeated.[265]

When examining the correspondence dating to this period it becomes clear that Heisenberg's recollection is somewhat distorted. While it seems clear that there were serious interpretational debates between the physicists, there is no evidence that they became so contentious. We know from his correspondence and from his published articles that Schrödinger had serious reservations about the discontinuous quantum jumps and we also know that Bohr and Heisenberg did challenge him thoroughly on the applicability of his theoretical framework to various experimental results. However, after leaving Copenhagen, Schrödinger was far from hostile with his Danish colleague; as a matter of fact he was very gracious in his correspondence with Bohr, recalling their discourse as "a truly unforgettable experience" and one that was particularly formative for him. Yet, Schrödinger remained unconvinced that the experimental results Bohr and Heisenberg had cited were sufficient to prove that the quantum world should be fundamentally discontinuous. Ultimately, he was not willing to abandon hopes of reconciling the micro and macro domains while retaining the continuous space-time framework.[266]

Ironically enough, it was the empirical evidence that was used in 1924-25 to discredit the BKS theory that Bohr invoked to challenge Schrödinger on his continuous space-time interpretation. Needless to say, by the fall of 1926, Bohr had convinced himself that the Bothe–Geiger and Compton–Simon scattering experiments irrefutably showed that the conservation of energy was preserved at the level of individual collisions thus invalidating the BKS theory.

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[265] Moore, *Schrödinger, life and thought,* 162-163.

[266] Helge Kragh, "Quantenspringerei*: Schrödinger vs. Bohr*." RePoSS: Research Publications on Science Studies 14. Århus: Department of Science Studies, University of Aarhus. url: http://www.ivs.au.dk/reposs. 80805, Munich. (Feb. 2011): 13-15.

However, he was also able to invoke these same results along with the Frank-Hertz experiment to argue against Schrödinger's wave mechanical interpretation. According to Bohr's interpretation of these experiments there was no way to avoid the discontinuous quantum jumps that Schrödinger disliked so vehemently. Whether you believe Heisenberg's account that he and Bohr dealt a blow to Schrödinger's confidence during his stay in Copenhagen what is clear is that their arguments based on empirical evidence did not fall on deaf ears. Moving forward, Schrödinger would continue to advocate for a reconciliation between micro and macro domains via a continuous theoretical framework, but after his visit to Copenhagen he became, for a time, more moderate in his stance against quantum discontinuity. It is this moderation that is usually taken as evidence that Schrödinger abandoned all interpretive debates for close to a decade.[267] If one considers the totality of the correspondence from this period and Schrödinger's subsequent publications this claim seem to be overstated.

4.2.3- De Broglie's Continuing Alternate Wave-Mechanical Research Program

We have now seen how different physicists reacted to Schrödinger's new formulation of quantum mechanics in 1926 and particularly how some of those reactions morphed over time into hardened stances on interpretation. By now it should be clear that reaction to Schrödinger's wave mechanical interpretation was far from homogenous throughout the physics community but there is one factor that seems to be missing from many analyses of this period and that is the notion of networking access. Contemporarily with these dynamics between physicists from the four main quantum schools there were other physicists who were not part of these strong academic networks who nevertheless were also attempting to apply, extend, and

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[267] Slobodan Perovic, "Schrödinger's interpretation of quantum mechanics and the relevance of Bohr's experimental critique," *Studies in History and Philosophy of Modern Physics* 37, (2006): 275–297.

reinterpret Schrödinger's novel wave mechanical formulation. In the following discussion we will take a closer look at a particular example of this type of research program.

As we discussed in chapter three, Louis de Broglie had worked on his initial wave mechanical formulation of quantum theory as part of his doctoral research in Paris in 1923-24. With the help of Einstein's international status, his ideas made their way throughout the larger physics community and became a cornerstone of Schrödinger's 1926 wave mechanical innovations. Unfortunately, the historical literature has not done an adequate job of addressing de Broglie's continuing wave mechanical research program. Too often his efforts are reduced to a flash insight in 1924 and, on occasion, scholars will mention a misguided attempt at a deterministic wave mechanical theory in 1927. A closer examination of de Brogllie's work during the interim period between 1924 and 1927 shows that he continued to pursue a program of synthesis between the wave and particle pictures.

This part of our narrative begins in the fall of 1925. While Schrödinger was preparing to submit his first seminal paper on wave mechanics for publication, de Broglie was in Paris contemplating the best way to extend what he undeniably understood to be his tentative unification theory proposed in his dissertation. In January, 1926 de Broglie published a paper in the *Journal de Physique et le Radium* entitled "On the Parallel between the Dynamics of a Material Particle and Geometrical Optics," in which he reexamined the core principle from his dissertation work and the foundation for the development of his variant of wave mechanics.[268] In this paper, de Broglie attempts to buttress the parallels that he had already made in his dissertation work between the dynamics of a particle and geometrical optics. He recognizes

[268] Louis de Broglie, "On the Parallel between the Dynamics of a Material Particle and Geometrical Optics." *Journal de Physique et le Radium, Vol. 7*, (1926): 1. Reprinted in: *Selected Papers on Wave Mechanics,* Translated by Winfred Deans, (London: Blackie & Son Limited 1928), 9-17.

that the corpuscular theory of light has always been problematic when studying propagation in refracting media, so he proposes to derive the usual equations of dynamics (Hamilton's dynamical equations) from wave theory, and in particular a dispersion formula. As a result of this new derivation de Broglie argues that he can circumvent a "supposed contradiction between the principle of least action and Fermat's Law."[269]

During the spring of 1926 there is little doubt that de Broglie was closely following the development of Schrödinger's wave mechanics and his development of a dynamical wave function that made calculation and application of his theory intuitive for most physicists. During the summer months of 1926, while Schrödinger's wave mechanical interpretation of psi (ψ) was being debated in the core quantum schools of Germany and Denmark, de Broglie was not sitting idle. He had seen what Schrödinger had been able to accomplish by combining the Hamilton wave mechanical analogy and a wave equation, so in August 1926, he published his own wave equation approach to quantum mechanics using the notions of phase waves that he had developed in his doctoral research. His research program in 1926-27 was not dissimilar to his doctoral dissertation. Again, his primary goal was to devise a quantum mechanical formulation that could synthesize the seemingly disparate pictures of undulatory and corpuscle phenomena.

As he had done in 1923 de Broglie published an initial paper dealing only with the simplest test case of light quanta and then wrote two subsequent papers in which he developed the formulation so that it could be used in the realm of micromechanical particles. In his first paper from the fall of 1926,[270] de Broglie dealt only with light quanta and attempted to reconcile

[269] Ibid., 1.
[270] Louis de Brogllie, "Sur la possibilité de relier les phénomènes d'interférence et de diffraction à la théorie des quanta de lumière," *Comptes Rendus 183*, (1926): 447-448. Reprinted in: *Selected Papers on Wave Mechanics,* Translated by Winfred Deans, (London: Blackie & Son Limited 1928),

Born's probabilistic ideas governed by the Schrödinger wave equation and the probabilistic interpretation of psi (ψ) with a notion that Einstein had proposed back in 1909, discussing light quanta as singularities on the front of a plane wave.[271] His approach was a distinctly hydrodynamical explanation of light quanta allowing him to obtain a velocity potential for the quanta themselves and a corresponding light-quanta density that was proportional to the intensity of the propagated radiation.

In order to model the dynamics of Einstein's singularities, de Broglie began with the classical optical wave equation:

$$\nabla^2 u = \frac{1}{c^2}\frac{\partial^2 u}{\partial t^2}$$

and then argued that the solution to this wave equation that could best account for general optical boundary conditions like screens and apertures was a plane wave of the form:

$$u = f(\boldsymbol{x},t)e^{-i\omega[t-\varphi(x,t)]}$$

where the function $f(\boldsymbol{x},t)$ was now a function of t and represented the novel addition, due to quantum theory, of "mobile singularities" on the wave's planar fronts of constant phase ($\varphi(\boldsymbol{x},t) = constant$). While Einstein had used this notion of singularities in 1909 as a heuristic approach to understanding the new phenomenon of light quanta, for de Broglie, these singularities were assumed to be real representations of the physical quanta and could thus be further studied by applying Born's evolving probabilistic quantum formulation. After substituting the generalized plane wave function back into the wave equation and taking only the real part, de Broglie arrived at:

[271] Jammer, *The Conceptual Development of Quantum Mechanics,* 308.

$$\frac{\partial\varphi}{\partial n}\frac{\partial f}{\partial n}+\frac{1}{2}f\nabla^2\varphi=-\frac{1}{c^2}\frac{\partial f}{\partial t}$$

where **n** represents the trajectories normal to the phase front (i.e., what in geometrical optics would be considered a “ray” collinear with the wave number **k**). In the above treatment, de Broglie assumes that $\varphi(\boldsymbol{x},t)=\beta h\omega_0\left(t-\frac{\boldsymbol{v}\cdot\boldsymbol{x}}{c^2}\right)$ is the generalized phase relationship over all space.

At this point de Broglie made a key hand-waving assumption, at the point of propagation where $\boldsymbol{x}=\boldsymbol{v}t$, the wave front would be characterized by a singularity and the ratio $f/\frac{\partial f}{\partial n}$ would vanish, thereby leaving the simplified equation:

$$\frac{\partial\varphi}{\partial n}c^2=-\left(\frac{\frac{\partial f}{\partial t}}{\frac{\partial f}{\partial n}}\right)$$

where $\boldsymbol{v}=-\left(\frac{\frac{\partial f}{\partial t}}{\frac{\partial f}{\partial n}}\right)$ is the velocity of light quanta. As a result, when introducing the hydrodynamic analogy, the phase $\varphi(\boldsymbol{x},t)$ became the velocity potential corresponding to the “fluid” of light quanta with a corresponding density ρ proportional to the classically observed radiation intensity. In the case of the density $\rho(\boldsymbol{x},t)$, de Broglie showed that it obeys the continuity equation:

$$\frac{\partial\rho}{\partial t}+\nabla\cdot(\rho\boldsymbol{v})=0$$

In his brief second note published in February 1927 de Broglie extended the formulation developed for light quanta to an analogous application of quantum mechanics to particles with associated mass and dynamics described by a double solution of the wave equation.[272] The first solution was just Schrödinger's continuous wave function psi (ψ), which had already been appropriated by Born as part of his probabilistic interpretation to quantum mechanics. The second solution was the singularity function $u(\boldsymbol{x}, t)$ allowed for the discontinuous nature of light and matter and contained an amplitude term $f(\boldsymbol{x}, t)$ that represented a moving singularity. The key assumption here was that these two solutions shared the same phase $\varphi(\boldsymbol{x}, t)$ and that it was the dynamical nature of this phase that determined the velocity potential and ultimately the full particle dynamics.

De Broglie published a third, more elaborate, and much more widely read paper "Wave mechanics and the atomic structure of matter and radiation" in May 1927 in the *Journal de Physique et du Radium*.[273] In this paper he first summarized the findings from his first two publications; he then proceeded to further develop his novel double solution approach showing explicitly how it could be extended from a single particle to an ensemble system; finally and most polemically, he finished it off by introducing the notion of a possible "pilot wave" interpretation of ψ. Before we address de Broglie's proposal of an alternate interpretation for ψ, it is worth noting several important points that he makes explicit throughout the discussion that leads to the final section of his paper.

In his discussion of a possible double solution of the wave equation it becomes clear that for de Broglie what is critical is the synthesis of corpuscular and wave pictures. He is explicitly looking for a formulation that will allow for extended wave behavior while simultaneously describing particle-like dynamics. In other words this program is a clear

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[272] De Broglie, Louis. (1927). "La structure atomique de la matière et du rayonnement et la mécanique ondulatoire," *Comptes Rendus 184*, 273-274.
[273] De Broglie, Louis. (1927). "La mécanique ondulatoire et la structure atomique de la matière et du rayonnement," Journal de Physique et du Radium *8*, 225-241.

extension of the foundational principle driving his doctoral research: the equivalency between the two classical frameworks of Maupertuis and Fermat. According to de Broglie, the essence of their equivalency is manifest in the idea of the double solution; as a result, he pointed out that the particle dynamics could be fully described in two distinct ways. The first was the method that had been developed by Born and others using Schrödinger's wave equation in conjunction with a probabilistic interpretation of psi. This method certainly worked, but according to de Broglie sacrificing the very notion of particle trajectories and positions was too high a price to pay. So he proposed an alternate approach, instead of solving the wave equation for psi, one could solve it for the singularity function u and then derive the dynamics of the particle trajectories thereby retaining classical determinism.

De Broglie complained that in his formulation of quantum mechanics Schrödinger had chosen to change his domain from real space to configuration space, thereby interpreting psi as a non descript wavepacket with no concrete trajectory or position. For de Broglie this was too abstract and could never result in a physical picture of psi. So, instead of thinking about psi as being a wave propagating in 3N-space de Broglie advocated for a picture that centered on N waves propagating in 3-space. This of course necessitated a formulation that could model multi-particle behavior. The difficulty in this came with actually understanding that each particle's velocity potential would be affected by the other particles' relative positions and trajectories. While it was sometimes possible to know the initial conditions of a system with one particle, and therefore solve for the dynamical guiding equation involving the velocity potential, for general configurations of $N > 1$, solving multiple coupled partial differential equations quickly became an insolvable problem.

Recognizing that his deterministic double solution of the wave equation is tentative and may be impractical, de Broglie posits an alternate approach in the last section of his paper. The section titled: “L’onde pilote” or “Pilot wave” became highly polemical and a lightning rod for criticism. Knowing that it was difficult to derive the velocity potential for most concrete problems, de Broglie suggested that one take it as a tentative postulate that this velocity potential exists and that psi is in fact a physically real “pilot wave” which guides the motion of the particle in question. This section describing a pilot wave solution shifted de Broglie’s program from one based on a hydrodynamic model through which one could, in principle, derive the dynamics of particles or “singularities” based on their corresponding velocity potentials to the temporary abandonment of a concrete particle picture and a full reinterpretation of psi as a physically real guiding wave.

It is a common theme in quantum narratives to portray de Broglie’s attempts at constructing a deterministic wave mechanical quantum theory as a flash in the pan. This seems to resonate with the commonly held picture of de Broglie as a misguided physicist who happened to stumble upon his explanation of wave particle duality only to veer off the empirically grounded road in order to develop a “hidden variables” theory in 1927. This characterization is far from an accurate portrayal of the French physicist. Moreover, as the historical record shows he was certainly not the only physicist engaged in this type of speculative research in 1926-27. It is more accurate to think of de Broglie’s research program as one of many such programs during this period of extreme flux.

4.2.4- Other Attempts at Developing Alternate Wave-Mechanical Theories

In this next section we will briefly describe other attempts at extending or reinterpreting Schrödinger's wave mechanical formulation of quantum mechanics. While the attempts by Erwin Madelung and Albert Einstein that came out of Frankfurt and Berlin respectively seem to be the most often cited within the historical literature, after perusing articles published in *Nature* and *Philosophical Magazine* (U.K.) and the *Journal de Physique* (France) during this period I was able to identify several other attempts to construct these types of alternate wave mechanical quantum theories. Each theory was its own distinct attempt at shaping the foundations of quantum theory, and while they were all quickly abandoned by their authors, it seems clear that within the physics community some interest definitely existed for the synthetic and deterministic agenda.[274] In fact, even before Schrödinger's innovations there were serious attempts to incorporate 'guidance fields' to define corpuscular trajectories, whether they were in the form of de Broglie's phase-waves, Einstein's ghost fields, or John Slater's virtual fields. Ultimately, de Broglie's research program from 1923 to 1927, with the goal of developing a synthetic and deterministic wave mechanical quantum mechanics, was undoubtedly reasonable in light of the period's scientific environment.

In the early 1920s Madelung had been made head of the theoretical physics group at the University of Frankfurt. It was during his early tenure in Frankfurt that he had hired the young Hungarian mathematical physicist Kornél Lánczos. Lánczos had received his doctorate in Budapest after working to extend Einstein's general theory of relativity but due to exclusionary laws against Jews in Hungary, he was forced to move to Germany to pursue his academic career.

[274] Some of these physicists were: C. Crehore, H. Bateman, C.G. Darwin, O Klein, T. de Donder, L. Rosenfeld, N. Wiener and D.J. Struik. For example: Klein, de Donder, Rosenfeld, and Wiener and Struik were working to unify the general theory of relativity with quantum theory (particle trajectories would become curved geodesics in resonance with de Broglie's analogy of geometrical optics).

After a short stay in Freiburg, Lánczos joined Madelung and Paul Epstein[275] as a lecturer at the University of Frankfurt in 1924.[276] While Lánczos' primary line of research was not in atomic theory, he was intrigued by the Göttingen School's use of matrix theory in their development of quantum mechanics. As we noted in the last chapter, Lánczos proposed an analogous integral form of quantum mechanics in December, 1925, well before Schrödinger began his famous barrage of papers on the subject. While it was clearly equivalent to both the matrix formulation and Schrödinger's own wave mechanical formulation it was deemed as less intelligible by the majority of physicists.[277] Perhaps due to the critiques but mostly due to alternate interests, Lánczos soon abandoned this line of research. It is interesting to note that within 10 months his Frankfurt colleague Erwin Madelung had developed his own alternate formulation of the new wave mechanics.[278]

As we have seen de Broglie's "double solution" program started from a generic wave equation and proposed a singularity function *u* as an additional, and physically significant, solution to the equation while relegating Born's probabilistic interpretation of Schrödinger's wave function psi, as nothing more than a non physical, heuristic analytical tool. Meanwhile, Madelung's approach in his hydrodynamic reformulation of quantum mechanics was strikingly different as it began with the assumption that Schrödinger's wave function psi was a unique and physically significant solution to the actual Schrödinger wave equation. Both physicists were

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[275] This is not the Paul S. Epstein of physics lore. This Paul Epstein was born in Frankfurt in 1871 and after fighting for Germany in WWI was appointed first as a lecturer and then as a professor at the University of Frankfurt. By the time the Nazi regime instituted the Nuremburg Laws, Epstein felt that he was too old to emigrate, and decided to stick it out. In 1939, after being summoned by the Gestapo, Epstein took his own life rather than be tortured. (http://www-gap.dcs.st-and.ac.uk/~history/Biographies/Epstein.html)
[276] http://www-history.mcs.st-and.ac.uk/history/Biographies/Lanczos.html
[277] Léon Brillouin "The New Atomic Mechanics" *Journal de Physique et le Radium, Vol. 7*, (1926): 135. Reprinted in: *Selected Papers on Wave Mechanics,* Translated by Winifred Deans, (London: Blackie & Son Limited 1928), 52.
[278] While I have found no concrete evidence that there was a dialogue on the subject, one might imagine that Lánczos and Madelung as colleagues might have had many exchanges on the subject of wave mechanics and the foundations of quantum theory.

looking for a synthetic model that could describe wave and particle behavior simultaneously and deterministically but de Broglie's argument was far more thoughtful and layered as he was engaged in a long research program that had laid down roots in the summer of 1923 and was continuing straight through to 1927. Meanwhile, in the fall of 1926, Madelung had scarcely begun to probe the depths of wave mechanics and his theory reflected the superficiality of his analysis. We should interpret Madelung's excursion into this realm as a cursory attempt to reinterpret and extend Schrödinger's earlier innovations.

Madelung accepted the Schrödinger wave equation:

$$-\frac{h^2}{8\pi^2 m}\nabla^2\psi + U\psi = i\frac{h}{2\pi}\frac{\partial\psi}{\partial t}$$

and proposed a solution of the form: $\psi = Re^{i(2\pi S/h)}$. When substituting this function into the Schrödinger wave equation and separating out the real and imaginary parts, Madelung quickly understood that he could ascribe clear physical significance to the terms R and S where $\rho = R^2$ could describe a fluid density and $\varphi = S$ would then be the corresponding velocity potential leading to a velocity vector field $\boldsymbol{v} = \frac{1}{m}\nabla\varphi$. With these substitutions, Madelung noticed that he naturally arrived at a hydrodynamic continuity equation describing the continuity of the fluid's mass.

$$\nabla \cdot (\rho\boldsymbol{v}) + \frac{\partial\rho}{\partial t} = 0$$

While the initial Schrödinger equation had been time dependent, Madelung was able to show that every eigenfunction that solved the wave equation could be interpreted as describing a stationary flow. Ultimately, Madelung abandoned his nascent hydrodynamic interpretation in large part due to the difficulties in applying it to actual physical problems like emission and absorption phenomena.[279]

On May 5th, 1927 Albert Einstein stood before the Prussian Academy of Sciences, in Berlin, and read a paper titled: "Does Schrodinger's Wave Mechanics Determine the Motion of a System Completely or Only in the Sense of Statistics?"[280] Although never published, Einstein's reading of his paper left an immediate impression on some within the community. Heisenberg, who had not heard the talk himself presumably heard about its impact and wrote to Einstein immediately confessing that he had a "burning interest" to know more about this mysterious theory. At the time of the reading Einstein was convinced that he had indeed discovered a way to show that Schrödinger's equation was a completely determined representation of a quantum system's dynamics. However, within days of receiving Heisenberg's letter requesting more information, Einstein notified the editor of the Journal that he was to leave the paper unpublished. People must have assumed that the great physicist had realized his initial enthusiasm for the proposed theory was misplaced and they soon forgot about his aborted theory. All that remains of Einstein's paper is a hand written draft version that sits in the Einstein Archives.[281]

It is clear why Einstein chose to write the paper. After all, as we noted earlier, in the spring of 1926, he was already displeased with Heisenberg, Born, and Jordan's quantum mechanical probabilistic interpretation and believed that eventually a deterministic form of Schrödinger's formulation would become the final form of quantum theory. However, by the

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[279] Jammer, *The Conceptual Development of Quantum Mechanics,* 307-308.

[280] This title was translated by Darren Belousek, "Einstein's 1927 Unpublished Hidden Variables Theory: It's Background, Context and Significance." *Stud. Hist. Phil. Mod. Phys.,* Vol. 21, No. 4, (1996): 437-461. Original German: "Bestimmt Schrödingers Wellenmechanik die Bewegung eines Systems vollständig oder nur im Sinne der Statistik?"

[281] Belousek, "Einstein's 1927 Unpublished Hidden Variables Theory: It's Background, Context and Significance," 437-438.

spring of 1927, not only was the physics community not getting closer to that final deterministic unified quantum theory, in Einstein's estimation things were getting worse. In mid April, he received a pre-print of Heisenberg's uncertainty paper, which seemed to axiomatize non-determinism in the quantum domain. For, Einstein, these uncertainty relations together with Born's probabilistic interpretation, placed a realistic and physically intuitive physics on the brink of extinction. In order to salvage what he could of physically intuitive mechanics, Einstein proposed to answer the question posed in the title of his unpublished paper and one that struck at the heart of the crisis.

By the way Einstein posed the title question and then immediately opened the paper with an unequivocal answer, it is clear that he was intent on doing away with the predefined statistical nature of quantum theory:

> *Does Schrodinger's Wave Mechanics Determine the Motion of a System Completely or Only in the Sense of Statistics?*
>
> As is well known, the view currently prevails that a complete space-time description of the motion of a mechanical system does not exist in the quantum-mechanical sense. For example, it is supposed to make no sense to speak about the instantaneous configuration and velocities of the electrons of an atom. In contrast, it shall be shown in the following that Schrödinger's wave mechanics suggests that one assign motions of the system uniquely to each solution of the wave equation.[282]

In his analysis of Schrödinger's wave mechanical scheme, Einstein worked within the n-dimensional configuration space allowing n different possible mutually orthogonal velocity vectors each with a uniquely defined directionality $\{\lambda_{(a)}\}$ and thus with a uniquely defined trajectory. According to Einstein, this was no different (except for the n-dimensional configurations space) to solving the classical Hamilton-Jacobi equation. Einstein's scheme was based on stationary state analysis (ie. time-independent Schrödinger equation) in which he derived a tensor of ψ-curvature from Schrödinger's original wavefunction ψ. He then showed

[282] Ibid., 446.

how he could use this non-Euclidean tensor to find the n unique initial conditions (position and direction) at each point. Armed with this knowledge, one could then determine completely the system's dynamics. In other words, Einstein used non-Euclidean geometrical arguments familiar to him from his general theory of relativity to argue for the possibility of real and defined quantum trajectories.[283]

While we certainly know why Einstein was interested in developing his deterministic unified theory, we unfortunately do not know exactly why he aborted this approach so abruptly. There is ample evidence supporting the notion that Einstein never came to accept the underlying statistical behavior of quantum theory and never gave up on finding a unified field theory that could encompass both the quantum and cosmological realms equally effectively. However, after his 1927 brush with the deterministic wave mechanical formulation, he would come to consider that particular line of research a dead end.[284]

It is important to note here that Einstein was not the only physicist pursuing this particular line of inquiry. As we stated earlier the German Oskar Klein, the Belgians Théophile de Donder and Léon Rosenfeld, and the Frenchman Louis de Broglie himself published on this very approach before May, 1927. Oskar Klein proposed a generalization of the Schrödinger equation by extending a scheme Theodor Kaluza had published in 1921 in which he described the general theory of relativity in five dimensions. In addition to the three traditional dimensions of space and one of time, Kaluza and Klein proposed introducing a fifth dimension, corresponding to an intrinsic periodic variable x^0 and related to the "atomicity of charge" e/m_0.

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[283] Ibid., 449.
[284] Ibid., 460-61.

During the late summer of 1926, Klein adapted this scheme to Schrödinger's wave mechanics in order to write a fully symmetrical wave equation.[285] In Klein's own words:

> …[elsewhere I have] shown that the differential equation underlying the new quantum mechanics of Schrödinger can be derived from a wave equation of a five-dimensional space, in which h does not appear originally, but is introduced in connection with a periodicity in x^0. Although incomplete, this result, together with the considerations given here, suggests that the origins of Planck's quantum may be sought just in this periodicity of the fifth dimension.[286]

Meanwhile, de Broglie remained fully engrossed in his particular program to find a single theory that could capture the deterministic dynamics of the quantum domain, but after seeing Klein's paper he recognized the power of the Kaluza-Klein approach and attempted to adapt it to his own unification program. In February, 1927 he published a little known paper in the *Journal de Physique et le Radium* titled: "The Universe of Five Dimensions and the Wave Mechanics". In this interesting paper, de Broglie attempts to avoid the notion of force altogether by invoking geometrical arguments (a la Einstein's GTR). While Einstein showed that gravitational force could be reduced to the idea of curvature in four-dimensional space-time, de Broglie noted that electromagnetic force could not. For this to be possible it was necessary to postulate a fifth dimension, corresponding to a new intrinsic variable, Kaluza and Klein's x^0, which would then allow all charged particles (with charge to mass ratios e/m_0) to have their own unique world-lines defined by geodesics.

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[285] This unification of gravitational and electromagnetic forces in a five-dimensional scheme later became known as K-K (Kaluza-Klein) theory which reemerged as a basis of modern string theory. For more on K-K theory: M.J. Duff, "Kaluza-Klein Theory in Perspective," in Lindström, Ulf ed. *Proceedings of the Symposium 'The Oskar Klein Centenary'*. (Singapore: World Scientific, 1994), 22–35.

[286] O. Klein, "The Atomicity of Electricity as a Quantum Theory Law," *Nature No. 2971 Vol. 118.* (1926): 516.

At the end of his paper, de Broglie arrives at what he believes is the general expression for the dynamics of a particle in five dimensions. Due to the "remarkable form" of this generalized dynamical equation:

$$\gamma^{ik}\left[\frac{\partial^2 u}{\partial x^i \partial x^k} - \begin{Bmatrix} ik \\ r \end{Bmatrix} \frac{\partial u}{\partial x^r}\right] + \frac{4\pi^2 c^2}{h^2}\left[{m_0}^2 - \frac{e^2}{16\pi G}\right] u = 0$$

de Broglie concludes:

> In order to get to the bottom of the problem of matter and its atomic structure, it will no doubt be necessary to study the equation systematically from the view-point of the five-dimensional Universe… If we succeed in interpreting the way in which the constants e, m, c, h, and G enter into [the] equation, we shall be very close to understanding some of the most perplexing secrets of Nature.[287]

De Broglie considered Schrödinger's 1926 innovations as a "brilliant," but nonetheless intermediate step in the development of wave mechanics. On the other hand, his continuing research from 1923-28 on the unified and deterministic representation of wave-particle duality based on the equivalence of Fermat and Maupertuis' principles was the ultimate goal. While the traditional quantum mythology has painted de Broglie's goal as being far outside the norm, we have shown that assertion to be untrue. As we have seen, there were actually several notable physicists in the international physics community who were simultaneously attempting to reach that same goal. Of all of these, de Broglie's was probably the most comprehensive and determined approach, but nonetheless his efforts should be placed in the full context of the other research programs hoping to develop alternate formulations of quantum mechanics in 1926-27.

As we have seen, most physicists such as Einstein and Madelung, positing deterministic wave mechanical formulations had abandoned those theories by the summer of 1927. The

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[287] De Broglie, Louis, reprinted in: *Selected Papers on Wave Mechanics,* 111.

powerful combination of Schrödinger's wave equation, Born's probabilistic interpretation of the wave function (ψ), Heisenberg's uncertainty principle, and the relentless succession of applications of the new quantum machinery to explain empirical phenomena was enough to convince the vast majority of physicists in the community that the formulations were correct. Powerful arguments, invoking experimental results, like those presented by Bohr to Schrödinger in Copenhagen in the fall of 1926 could not be denied and yet de Broglie remained convinced that he was on the right track and that a deterministic formulation of quantum mechanics was soon to come. By the time of the Fifth Solvay Council in the fall of 1927 the various confrontations between the physicists associated with the four elite quantum schools and those who struggled to challenge what seemed to be a growing propensity to discard classical notions such as determinism and continuity would come to a head and play out on a very large international scientific stage.

4.3- Lorentz' Scientific Diplomacy: Planning the Fifth Solvay Council

Because the planning period for the 1927 Solvay Council was contemporaneous with an 18 month period characterized by a highly dynamic scientific exchange concerning unsettled foundational issues within the evolving quantum mechanical picture, it is instructive to see how the planning process played out on a larger scale and how Lorentz managed to navigate the difficulties of international scientific diplomacy. The Fifth Solvay Council is traditionally assigned central importance in the overall quantum narrative because of the battles that were waged there, so it is critical that we place the planning and execution of this Council in their own proper historical context.

The seven Locarno treaties, including the Rhineland Pact, were an attempt by western European nations such as France and Belgium to normalize relations with Germany and diffuse the ongoing tensions resulting from World War I and the Treaty of Versailles. In particular they attempted to ensure the post-war western borders were secure and that reconciliation with Germany included its acceptance into the League of Nations. Negotiated in Switzerland during October 1925, the Locarno peace accords were officially signed weeks later in London on December 1; however, the ratification of these treaties was a drawn out and complicated process that would not be concluded until September, 1926. Meanwhile, as we noted in the last chapter, due to these post World War I international tensions, the exclusion of German and Austrian scientists and by extension anyone supportive of them by the international scientific community had persisted throughout the early and mid 1920s. As a key member of the Solvay scientific committee Lorentz had been trying unsuccessfully to end this group's exclusionary policy for years but with the signing of the Locarno Treatises, and the imminent inclusion of Germany into the League of Nations, the momentum began to swing his way. After meeting with the administrative commission and pleading his case directly in front of the Belgian King in early April, 1926, Lorentz, as chairmen of the Solvay scientific committee offered Einstein a seat on this permanent planning committee and extended invitations to other German physicists to participate in the Fifth Solvay Council to be held in Brussels in October, 1927.

In paralleling the planning of this Fifth Solvay Council from the spring of 1926 through the fall of 1927 with the development of Schrödinger's formulation and the various reactions to this research program we can appreciate the larger political context within which the various early attempts at interpretations of quantum theory were conceived of. During an initial planning meeting in early April, 1926 Lorentz and other members of the permanent Solvay scientific committee decided on a topic for the Fifth Council as well as preliminary lists of possible invitees and presenters. The discussion topic chosen at this planning session was: "The quantum theory and the classical theories of radiation." Actually, the naming of this Council topic was somewhat tenuous as originally Lorentz had sent a letter to Ehrenfest at the end of March in which he included the phrasing "the conflict and the possible reconciliation between the classical theories and the theory of quanta." Ehrenfest objected to this characterization complaining that this "flippant title" could lead to "slimy unclear thinking" and ultimately a gross "misunderstanding" amongst the physicists participating at the Solvay Council.[288] Lorentz immediately took Ehrenfest's advice to heart and toned down the title for the early April planning session, thus dropping the notions of conflict and a possible reconciliation from the title. As has been well documented throughout the historical literature, the official title would be revised again as the planned topic of discussion changed from a focus on light quanta to the final topic choice centered on "Electrons and Photons".

With a planned discussion of light quanta, the Solvay Council's scientific committee had initially come up with a list of possible presenters including three experimentalists: W.L. Bragg, A.H. Compton, and C.T.R. Wilson; and four theorists: L. de Broglie, H.A. Kramers, A. Einstein, and W. Heisenberg. A few alternate speakers were also identified, if needed, including Ehrenfest substituting for Einstein and Schrödinger substituting for Heisenberg.[289] In fact, in a letter from Einstein to Lorentz, the great German physicist proposed that based on Schrödinger's "development of genius of de Broglie's ideas," the Austrian, should take his place amongst presenters at the Fifth Solvay Council. Lorentz quickly responded to Einstein by assuring him

[288] Bacciagaluppi and Valentini, *Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference,* 3 and 9.
[289] Ibid., 9.

that Schrödinger was being considered as a substitute for Heisenberg to present on the "Adaptation of the foundations of dynamics to the quantum theory."[290]

By this point Schrödinger had begun submitting his series of quantization papers at the end of January 1926, and had seen them published successively in *Annalen der Physik* from March 13th to September 5th. When Lorentz corresponded with Einstein at the beginning of April and insisted that he and the rest of the scientific committee were seriously considering whether or not to substitute Heisenberg with Schrödinger as a presenter at the Fifth Solvay Council, it showed the immediate impact that the first two Schrödinger quantization papers must have had on the physics community.

That spring, Schrödinger began an important correspondence with Lorentz about his newly formulated wave mechanics by sending the famous Dutch physicist proofs of his first two quantization papers in late March. The two physicists would meet in Pasadena, California the following year and in a conversation there, Lorentz would inform Schrödinger about his plan to possibly replace Heisenberg's discussion about matrix mechanics by a discussion presented by Schrödinger on wave mechanics. By this time, Schrödinger was well aware of the escalating tensions between himself and some members of the Göttingen and Copenhagen quantum schools and was concerned that his outright substitution for Heisenberg at Solvay might spark unnecessary hostilities so he sent Lorentz a letter that summer asking him to consider an alternate solution. In a letter to Lorentz on June 23, 1927 Schrödinger indirectly hinted at the possibility of three presentations on quantum mechanics instead of two:

> …I nurtured the quiet hope that you would yet return to your first plan and entrust only Messrs [d]e Broglie and Heisenberg with reports on the new mechanics. But now you have decided otherwise and I will of course happily perform my duty.

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[290] Ibid., 9-10.

> Yet I fear that the 'matricians' (as Mr. Ehrenfest used to say) will feel disadvantaged. Should other thoughts emerge, after all bringing about the wish in the committee to limit the reports to two, you know, dear Professor, that I shall always happily remit my charge into your hands.[291]

From the early list of possible participants and invitees to the 1927 Solvay Council, compiled in early April 1926, we notice that initially important contributors to the Göttingen and Copenhagen quantum schools (the so-called 'matricians') such as Pauli, Dirac, and Jordan were not considered among the initial invitees. However, thanks in part to his correspondence with Schrödinger, Lorentz became intrigued by the latest developments in quantum theory, and he began following the various lines of research rather closely throughout 1926 and 1927. In fact, while lecturing in the United States during the 1926-27 academic year he focused extensively on both wave mechanics and matrix mechanics first at Cornell University during the fall semester and then at Cal Tech during the spring semester. While the official invitations to the Fifth Solvay Council had been mailed in January, 1927, Lorentz began to recognize the importance of having a more youthful and representative group both present and participate later that fall. By the end of the summer Lorentz became convinced that he should invite both W. Pauli and P.A.M. Dirac to participate in the discussions and that he should follow Schrödinger's advice and have three presenters, instead of two, on the foundations of quantum mechanics. Born and Heisenberg had decided to present a joint talk on the matrix mechanical formulation, while Schrödinger and de Broglie would each represent their corresponding wave mechanical formulations. It seems that the three presenter solution, hinted at in Schrödinger's June letter to Lorentz, became the official Solvay program just months before the actual Council was to meet.

[291] Ibid., 113.

When the details of the planning of the Fifth Solvay Council are interpreted through a lens that keeps in mind the dynamically evolving research programs as well as the private conversations occurring between physicists, it becomes easy to comprehend the careful interactions and the quickly changing plans that Lorentz and others had to negotiate in order to secure a successful meeting. Lorentz points to this exact notion in a letter he addressed to Ch. Lefébure, the secretary of the administrative commission for the Solvay Councils on August 27, 1927:

> Since last year, quantum mechanics, which will be our topic, has developed with an unexpected rapidity, and some physicists who were formerly in the second tier have made extremely notable contributions. For this reason I would be very keen to invite also Mr Dirac of Cambridge and Mr Pauli of Copenhagen. … Their collaboration would be very useful to us…I need not consult the scientific committee because Mr Dirac and Mr Pauli were both on a list that we had drawn up last year…[292]

The 1927 Solvay Council has traditionally served as one of the most recognizable historical markers in the history of physics because, as we have already noted, it is generally believed that at this prestigious international scientific meeting, a prodigiously fierce battle was fought between two opposing camps: the irrationalists, led by Bohr and the realists led by Einstein. The historical truth behind the events that unfolded in the fall of 1927 is somewhat less dramatic and yet more interesting.

4.4- Reflections on the Fall of 1927: Como and Solvay

On September 11th 1927, a who's who of the top physicists of the day congregated at Como, Italy to commemorate the centenary of Alessandro Volta's death. In a groundbreaking

[292] Ibid., 10-11.

lecture on the 6th day of the conference Niels Bohr attempted to describe an interpretation of quantum theory that he hoped would be helpful "in order to harmonize the apparently conflicting views taken by different scientists."[293] This address was Bohr's first public presentation of his principle of complementarity. Although his ideas on complementarity would shift over time, it is important that we understand, within the right context, Bohr's first public presentation of his newly formed physical interpretation of what remained a confusing and abstract mathematical formulation of quantum mechanics. In his lecture Bohr stated:

> On the one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time coordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively.[294]

The deciphering of underlying elements within Bohr's Como lecture can give us a unique vantage point into the state of the quantum physics community in the fall of 1927. We have seen that there had been a conscious attempt on the part of many physicists throughout the early and mid 1920s to synthesize or unify the wave and particle pictures of the quantum theory. While the problem was first associated with electromagnetic phenomena, de Broglie took the defining step of reviving Hamilton's wave mechanical analogy thereby applying wave particle duality to the world of matter. Other attempts at unification and synthesis followed

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[293] Max Jammer, *The Philosophy of Quantum Mechanics: The Interpretations of Quantum Mechanics in Historical Perspective*, (New York: John Wiley & Sons, 1974), 86.
[294] Ibid., 87.

eventually reaching a fever pitch in the fall of 1926 and the spring of 1927. It was in response to these attempts that Bohr vacillated and struggled with the very foundations of quantum theory and finally opted to develop his notion of complementarity. Mara Beller's thorough study of what she refers to as the "dialogical flux" surrounding Bohr's Como lecture led her to the conclusion that:

> Complementarity between [pictures of wave and particle and] space-time and causality [are] an imprecise umbrella concept that allow[ed] Bohr to cope locally with interpretive issues while entrenching his initial conceptions of stationary states and discontinuous energy jumps.[295]

Taking the Schrödinger formulation and the matrix formulations as mathematically equivalent, Bohr saw that due to Born's probabilistic interpretation of psi, and Heisenberg's uncertainty principle there was little doubt that quantum measurement was going to reign as a fundamental problem for any physical interpretation of quantum theory. In that sense he took the path that Lodge argued so passionately against: he disavowed the possibility of synthesis or unification and unequivocally stated that the two pictures of wave and particle were complementary yet incommensurable.

In his Como address Bohr was attempting to reconcile the various quantum interpretations that had been offered around a novel central tenet known as complementarity but instead of reconciliation his position began to divide the physics community into two distinct camps, those who were going to remain stuck in their ontological picture of reality and those who were willing to adapt and accept a new explanation of that reality. In Bohr's quote above, we notice that he is illustrating the seeming contradiction that in order to "define" a system it should be objectively measured in a way as to keep it unperturbed by external disturbances.

[295] Beller, *Quantum Dialogue,* 118.

However, since the "quantum principle" states that all measurement is, by definition, an external disturbance then "observation" in its traditional sense, is impossible. So, both observation (measurement) and the ideal of fully defining a quantum system by exactly defining its states are mutually exclusive. It follows, for Bohr, that there will always be some abstractness or uncertainty in the system if it is observed. Bohr was essentially describing Heisenberg's uncertainty principle which, as we have seen, defined the absolute limits of measurement of related (conjugate) pairs of variables such as position and momentum. However, in a larger sense, in his address in Como Bohr was attempting to bridge multiple divergent perspectives from key scientists embroiled in a clear scientific debate about the foundations of quantum theory. While Bohr's address did not immediately impact the debate in any ostensible way, as we shall soon see the disavowing of these alternate wave mechanical synthesis programs became a cornerstone of the Göttingen-Coppenhagen interpretation.

On October 24th, 1927, just a month after the conference at Como, some of the more elite physicists who attended Volta's centenary were also present in Brussels for the Fifth Solvay Council. Although some physicists including Einstein and Schrodinger had not attended the Como conference and had yet to hear the public presentation of Bohr's complementarity principle most participants at Solvay had, and so the stage was set for a critical and transformative moment in what to this point had been a fluid debate but was quickly becoming a tense and historic scientific confrontation. The topic to be discussed at the Fifth Solvay conference was 'photons and electrons' which would cover atomic physics, and particularly, the new quantum theory.

As in previous Solvay Councils, the presentations began taking a survey of the current state of experimental techniques and results associated with the topic of discussion. In that vein the first two talks were given by W.L. Bragg and A.H. Compton. Bragg, presented on the current state of experimental quantitative analysis using x-ray scattering in the examination of electron distributions within the atoms of crystals. According to Bragg, while the state of agreement between theory and experiment was still not satisfactory in the fall of 1927, the moderate experimental successes were encouraging many to pursue these techniques. The actual techniques relied on an application of Fourier analysis to give distributions of electron density within atoms. In this sense it was a classically grounded approach relying on Schrödinger's original interpretation of his wave function, psi, as the continuous charge density distribution in three-dimensional space.[296] Pauli responded to Bragg's claim by stating "In my opinion one must not perform the calculations, as in wave mechanics, by considering the density $|\psi(x,y,z)|^2$ in three dimensional space, but must consider a density in many dimensions."[297] After a back and forth on the accuracy of this quantitative analytical technique between multiple participants, Born chimed in with a seemingly definitive statement on the matter:

> The correct answer to the question of scattering by an atom is contained in the remark by Mr. Pauli. Strictly speaking there is no three-dimensional charge distribution that may describe exactly how an atom behaves…A model in three dimensions only ever gives a more or less crude approximation.[298]

While this statement did not end the debate it certainly drew a clear line in the sand.

Compton's presentation on the "Disagreements between experiment and the electromagnetic theory of radiation" was clearly meant as a chance to argue the opposing viewpoint to Bragg's. While Bragg had just finished pleading the case for the completeness of

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[296] Bragg, W.L. 1927. "The intensity of X-ray reflection". Published in the Fifth Solvay Council proceedings in 1928 and reprinted in: Bacciagaluppi, G. and Valentini, A. 2009. *Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference.* Cambridge: Cambridge University Press. p. 280.
[297] Bacciagaluppi and Valentini, *Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference,* 292.
[298] Ibid., 294.

the electromagnetic theory of radiation, Compton sought to demarcate this classical theory's very limits. In doing so he stated: "It is, however, only by acquainting ourselves with the real or apparent failures of this powerful theory that we can hope to develop a more complete theory of radiation."[299] After reviewing the most significant failures of the electromagnetic theory including its reliance on the ether concept, the inability to account for a radiation source, and its repeated failures to explain experimental results like the photoelectric effect or Compton scattering, Compton concluded that:

> [R]adiation of directed quanta of energy, i.e., of photons, and that energy and momentum are conserved when these photons interact with electrons or atoms…this result does not mean that there is no truth in the concept of waves of radiation. The power of the wave concept in problems of interference, refraction, etc., is too well known to require emphasis. Whether the waves serve to guide the photons, or whether there is some other relation between photons and waves is another and a difficult question.[300]

While Compton's report had been relatively brief compared to Bragg's lengthy exposition, the discussion section immediately following it was long and involved contributions from many of the participants assembled. As the papers had been pre-circulated, it was clear that responses had been well thought out and that the arguments being presented in the discussion reflected the current climate of scientific debate. Lorentz was critical of Compton's dismissal of the ether, while Bragg complained that Compton's measurement of a forward momentum imparted by the scattering of photons was problematic, and Brillouin criticized Compton's over emphasis on photon theory. However, for every critique there was a reasonable response from Compton himself, or from Dirac, Pauli, Bohr, and Born. Bohr reviewed the essential elements of his principle of complementarity and at the end of the

[299] Ibid., 301.
[300] Ibid., 323.

discussion Born commented that in studying collision dynamics employing classical versus quantum formulations, "The only difference is that in the old theory one introduces microscopic quantities…while in the new theory one avoids the introduction of these quantities."[301]

The final three reports were the presentations of three variants of quantum theory as outlined by Louis de Broglie, Born and Heisenberg, and finally Schrödinger. Lorentz had struggled to decide whether Schrödinger would present a report on his version of quantum mechanics and the final decision had been to allow for three reports. De Broglie presented a paper entitled "The new dynamics of quanta" which was divided into three main sections and 13 numbered subsections. In the first section "Principal points of view," de Broglie began, as he had done in his dissertation, by developing a historical narrative of sorts. First he summarized his own work on the synthetic formulation of wave mechanics, which he followed by a brief discussion describing how Schrödinger extended the de Broglie formulation grounded in the Hamiltonian analogy thereby developing an analysis based on the use of a wave equation, and finally he briefly commented on Born's rejection of Schrödinger's realistic interpretation of the wave function in favor of a probabilistic interpretation applied successfully to collision phenomena. In Section II of de Broglie's report he examined in more detail what his proposal of a double solution meant. He stated:

> Many authors think it is illusory to wonder what the position or the velocity of an electron in the atom is at a given instant. We are, on the contrary, inclined to believe that it is possible to attribute to the corpuscles a position and a velocity even in atomic systems, in a way that gives a precise meaning to the variables of configuration space… So far we have considered the corpuscles as 'exterior' to the wave ψ, their motion being only determined by the propagation of the wave. This is, no doubt, only a provisional point of view: a true theory of the atomic structure of matter and radiation should, it seems to us *incorporate* the corpuscles in the wave phenomena by considering singular solutions of the wave equations.[302]

[301] Ibid., 338.
[302] Ibid., 353 and 355.

In his third, and last, section de Broglie highlighted the experimental evidence that he believed directly supported his conceptualization of “the new dynamics of the electron.” This section was not meant to be chronological as he focused on various experimental arrangements that showed electron interference effects. First de Broglie described the work done by Dymond in 1926 involving an ‘electron gun’ and the effects of scattering by helium gas, then he discussed Davisson and Kunsman’s work from 1923 on the scattering of electrons at lower speeds which was originally misattributed to intra-atomic electron layers but eventually reinterpreted by Elsasser as representative of interference phenomena. While these two experiments may have tentatively shown interference phenomena their interpretation remained somewhat inconclusive. Therefore, de Broglie finally focuses on the two sets of experiments conducted by Davisson and Germer and G.P. Thomson and A. Reid, independently that same year, which unequivocally showed electron interference phenomena.[303]

In the discussion of de Broglie’s report Lorentz began by asking for a clarification on how the Frenchman had derived Sommerfeld’s quantization conditions when dealing with open or multiperiodic atomic orbits. De Broglie was quickly able to point to the section in his dissertation where he dealt with this very problem. After satisfying Lorentz’ query, de Broglie faced more acerbic challenges first from Born, related to real trajectories and their effects on elastic collision theory, and then from Pauli as it pertained to the very foundations of his deterministic approach. After a few more clarifying comments and questions from Schrödinger, Kramers, and Ehrenfest, Brillouin launched into a long impassioned defense of de Broglie’s deterministic wave mechanical approach.

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[303] Ibid., 358-361.

> It seems to me that no serious objection can be made to the point of view of L. de Broglie. Mr. Born can doubt the real existence of the trajectories calculated by L. de Broglie, and assert that one will never be able to observe them, but he cannot prove to us that these trajectories do not exist. There is no contradiction between the point of view of L. de Broglie and that of other authors…[304]

The following report at the Fifth Solvay Council was given by Born and Heisenberg and it dealt with the more standard formulation and interpretation of quantum theory. In this report simply titled "Quantum Mechanics" the two Germans present what almost everybody at the Council must have recognized as the evolving standard Göttingen-Copenhagen Interpretation of quantum theory based on a toolkit of interchangeable and equivalent mathematical formulations that are more or less readily applicable to various quantum phenomena. These different equivalent formulations included matrix mechanics, Schrödinger's wave mechanics, Born and Wiener's operator calculus, and Dirac's highly abstract, but nevertheless useful q—number theory. The authors were quick to point out that while matrix mechanics was certainly "clumsy" in dealing with non-periodic phenomena, thanks to the work of Göttingen mathematicians from Hilbert's group, especially John von Neumann, Schrödinger's wave mechanics had been shown to be simply a special case of a more generalized operator theory applicable to all quantum systems.[305]

Born and Heisenberg concluded their report with a definitive yet polemical statement in which they pointed to a closing down of alternate research programs that failed to meet their increasingly axiomatic interpretation of quantum theory:

> By way of summary, we wish to emphasize that…we consider *quantum mechanics* to be a closed theory, whose fundamental physical and mathematical assumptions are no longer susceptible of any modification. [Furthermore,] the assumption of

[304] Ibid., 365.
[305] Ibid., 379-380.

> indeterminism in principle, here taken as fundamental, agrees with experience. The further development of the theory of radiation will change nothing in this state of affairs, because the dualism between corpuscles and waves, which in quantum mechanics appears as part of a contradiction-free, closed theory, holds in quite a similar way for radiation. The relation between light quanta and electromagnetic waves must be just as statistical as that between de Broglie waves and electrons.[306]

It is a bit surprising that the discussion following Born and Heisenberg's thorough examination of quantum mechanics as they conceived it was limited to a benign question by Lorentz followed by a few short clarifying remarks by Born, Dirac, and Bohr. After all, the presenters had claimed that quantum mechanics, as they saw it, was a "…closed theory, whose fundamental physical and mathematical assumptions are no longer susceptible of any modification." With Einstein, Schrödinger, and de Broglie in attendance and knowing their dislike of some of the implications of this evolving interpretation, this would have seemed like a spark for debate. Nevertheless, it is possible that these and other polemical comments were being reserved for the subsequent general discussion.

The last report of this Fifth Solvay Council was given by Schrödinger and titled, very succinctly, "Wave Mechanics." In his report Schrödinger made a distinction between two types of wave mechanics. The first, was what he referred to as a multi-dimensional theory based in 3n configuration space (q-space) which he interpreted solely as a "mathematical tool" useful in solving quantum mechanical problems but ultimately not representative of real physical systems. As Schrödinger had shown in the spring of 1926, this brand of wave mechanics corresponded with the matrix formulation developed in Göttingen. The second version of wave mechanics was based in the real space-time continuum and was closer to de Broglie's original conceptualizations. Schrödinger referred to this wave mechanical formulation

[306] Ibid., 398.

as the “the more beautiful four-dimensional version”[307] While the multidimensional version was more generally applicable to concrete problems Schrödinger held out hope that a unification so these versions could be found so that a realistic interpretation of quantum phenomena could be attained.

Schrödinger’s four-dimensional version of wave mechanics reverted to an older interpretation of $\psi\psi^*$as a charge density. In his mind he was uncomfortable imbuing the configuration space wave mechanics with physical reality and he preferred to hold out for the possibility of a deeper understanding that could link the heuristic version with the real space-time version. In the discussion session following Schrödinger’s report, Bohr, Born, and Heisenberg were quick to critique the Austrian’s approach. Instead of a beautiful alternative to the configuration space description, Heisenberg saw it as an ineffectual approximation method.[308]

The Solvay Council closed with a general discussion regarding “the new ideas presented” in which all the participants interjected freely. In the proceedings published in 1928 that were edited by the Council secretary J.E. Verschaffelt and Lorentz and reproduced by Bacciagaluppi and Valentini the general discussion section has been divided into two distinct topic headings: “Causality, Determinism, Probability” and “Photons and Electrons” with a short interlude discussion on “Photons”. While this general discussion ranged over two days and included many discussions on various polemical issues that had been raised in the official reports presented during the Council, Lorentz seemed to point to the most significant point of tension in his opening remarks.

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[307] Ibid., 406.
[308] Ibid., 428.

> The image that I wish to form of phenomena must be absolutely sharp and definite, and it seems to me that we can form such an image only in the framework of space and time. …Obviously, such a theory may be very difficult to develop, but *a priori* it does not seem to me impossible…. If one wished to forbid me such an enquiry by invoking a principle, that would trouble me very much. It seems to me that one may always hope one will do later that which we cannot yet do at the moment…. Could a deeper mind not be aware of the motions of these electrons? Could one not keep determinism by making it an article of faith? Must one necessarily elevate indeterminism to a principle?[309]

As a direct response to this challenge, Bohr jumped into a rehashing of his principle of complementarity. Interestingly enough he must not have been completely satisfied with his presentation because in the published proceedings he requested that his commentary be replaced by a version of his Como lecture given in September.

Born then defended the use of a multi-dimensional configuration space and attempted to explain the concept of "reduction" of the probability wave packet that had surfaced as a result of Heisenberg's uncertainty principle earlier that year. Einstein then entered the fray by making comments that seemed abstract and confusing to many of the participants. In fact, Bohr would immediately interject "I don't understand what precisely is the point which Einstein wants to [make]."[310] In retrospect, Einstein's seemingly obscure critique of the prevailing probabilistic interpretation in 1927 was a veiled preliminary form of the celebrated 1935 EPR argument:

> If $|\psi|^2$ were simply regarded as the probability that at a certain point a given particle is found at a given time, it could happen that *the same* elementary process produces an action *in two or several* placers on the screen. But the interpretation, according to which $|\psi|^2$ expresses the probability that *this* particle is found at a given point, assumes an entirely peculiar mechanism of action at a distance… In my opinion, one can remove this objection only in the following way, that one does not describe the process solely by the Schrödinger wave, but that at the same time one localizes the particle during the propagation. I think that Mr. de Broglie is right to search in this direction. If one works

[309] Ibid., 432-433.
[310] Ibid., 442.

> solely with Schrödinger waves [it will lead in] my mind to a contradiction with the postulate of relativity.[311]

In addition, although potentially lost in its obscurity, this commentary was a direct affirmation of de Broglie's research agenda. While Einstein understood the tenuous nature of the deterministic double solution approach that de Broglie had taken, he also recognized that it was ultimately better than the Göttingen-Copenhagen alternative.

Pauli followed Einstein's obscure comments by defending Bohr's principle of complementarity stating that the fact that precisely defined simultaneous space and time descriptions were impossible to achieve substantiates the notion that "the mutual actions of several particles certainly cannot be described in the ordinary manner in space and time."[312] Dirac then continued the attack on determinism by pointing out that Bohr's central claim that "an isolated system is by definition unobservable… [and] one can observe a system only by disturbing it" is an unassailable truth which naturally leads to the conclusion that deterministic classical theories of atomic phenomena are untenable.[313]

In the remaining discussion there was a significant exchange over de Broglie's report and his deterministic wave mechanical approach. First, Kramers pointed to a deficiency in the double solution approach as it avoided any statements about the notion of a radiation pressure from a single photon only referring to radiation pressures due to a cloud of photons. De Broglie recognized this limitation and readily admitted to the Council participants that his dualistic theory "does not constitute a definitive picture of the phenomena."[314] After a long discussion of

[311] Ibid., 441.
[312] Ibid., 444.
[313] Ibid., 446.
[314] Ibid., 450.

the experimental evidence for the use of various quantum statistical models, Lorentz brought the discussion back to de Broglie's proposed theory by enquiring about the concrete connection between the propagation of the ψ waves, the corpuscular photon trajectories, and the electromagnetic theory of radiation. De Broglie responded to Lorentz' point by stating:

> At the present one does not know at all the physical nature of the ψ-wave of the photons. Can one try and identify it with the electromagnetic wave? That is a question that remains open. In any case, one can provisionally try to develop a theory of photons by associating them with waves ψ.[315]

After clarifying a further point for Lorentz about how his theory addressed the varying velocity of light in zones of interference caused by "a sort of force of a new kind,"[316] de Broglie faced his sharpest critique of the proceedings. As we noted earlier, Born had raised an objection to de Broglie's theory based on a failure to correctly account for all collision phenomena. Pauli began his commentary by acquiescing to the notion that de Broglie's theory was in fact "in full agreement with Born's theory in the case of elastic collisions, but that it is no longer so when one also considers inelastic collisions." To illustrate his objection to de Broglie's theory, Pauli called on the example, studied by Fermi, of a collision between a particle and a rotator in a stationary state and situated in the same plane as the particle motion. Pauli pointed out that when employing de Broglie's theory to calculate the angular velocity of the rotator after the collision, they would find it to be variable, which would inevitably contradict the original experimental setup. In this sense Pauli concluded: "Mr. de Broglie's point of view does not then seem to be compatible with the requirement of the postulate of quantum theory, that the rotator is in a stationary state both before and after the collision." He quickly added that this was not a particular concern solely limited to the rotator collision problem but was

[315] Ibid., 460.
[316] Ibid., 461.

representative of a more general objection to any quantum theory that did not enlist a multidimensional configuration space.[317]

While de Broglie's response to Pauli was seemingly ineffectual as he relied on an analogy from classical optical diffraction phenomena which did not seem to address Pauli's concerns it seems to have been brushed aside by a long interjection from de Donder, who embarked on a comparative analysis between the relative benefits of his grand canonical gravitational-electrodynamic unification program and de Broglie's approach to quantum synthesis. Rosenfeld and de Donder, working in Brussles, had collaborated briefly with de Broglie earlier in 1927 in regard to the five-dimensional unification approach. At Solvay, de Donder made it clear that he believed that Rosenfeld's and his approach in which they had been able to derive a quantum current and a related quantum potential was preferable to de Broglie's. He concluded rather definitely that their quantum current "will probably play a dominant role in still unexplained optical phenomena."[318]

As the general discussion wound down and the proceedings came to a close it was again Lorentz who made an insightful comment that seemed to address an unresolved tension still present in the quantum physics community. As he had done in his personal correspondence with Schrödinger, Lorentz pointed to the difficulty in using a wave packet to describe an electron because of its tendency to smear or spread out when initially confined to a spatial constraint the size of an atom. He concluded his critique of Schrödinger's approach by stating: "The picture of the electron given by a wave packet is therefore not satisfying…" On the other hand he was equally reticent to accept Bohr's picture of wave packets as only applicable to observable stationary states:

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[317] Ibid., 463.
[318] Ibid., 464-466.

> …after an observation [Bohr] again localizes the wave packet so as to make it represent what this observation has told us about the position and motion of the electron; a new period then starts during which the packet spreads again, until the moment when a new observation allows us to carry out the reduction again. But I should like to have a picture of all that during an unlimited time.[319]

Lorentz was again emphasizing the fact that he was not satisfied with the abandonment of determinism and the closing off of all possible future research into deeper quantum theories that might eventually unify seemingly incommensurable pictures of reality.

One thing that is clear from this recapitulation of the international physics meetings from the fall of 1927 is that there was a tremendous amount of confusion about the nature of the wave function ψ. It seems perfectly clear that de Broglie was not out of line when he reported on his proposed deterministic wave mechanical formulation. But what is also clear is that all proposed alternate formulations of quantum theory that attempted to unify Bohr's complementary pictures of reality were tentative in the fall of 1927, and while they seemed reasonable to some physicists they certainly did not have the same practical probative analytical value that the various mathematically equivalent and Göttingen-Copenhagen approved formulations enjoyed.

[319] Ibid., 468.

Chapter 5: Congealing of Quantum Orthodoxy- The Rise of Pragmatism and the Lull of Interpretation Debates

"[Scientists] have gained prestige as men of action, but they have lost credit as philosophers."

– Max Born 1948[320]

5.1- Introduction

While not much seemed to be accomplished at the 1927 Solvay Council past the public exposition of the various positions of interpretational matters, the fall conference did seem to serve as a turning point after which emerged a long period characterized in part by a significant lull in interpretation debates. In this chapter we attempt to reexamine this twenty year period between the Fifth Solvay Council in the fall of 1927 and the end of the 1940s when several factors aligned to allow for the revival of a more contemplative approach to the study of the foundations of quantum theory. This was the context in which David Bohm began to study, corroborate and then question the almost universally accepted interpretation of quantum theory.

A comprehensive analysis of the physics community during this period is outside the realm of this study, instead we will focus on the causes of this lull in interpretation debates. We should note that during this lull there were physicists such as Einstein, Bohr, and Schrödinger who remained active in these debates but while vocal they did not represent the vast majority of the physics community. We attempt to understand why the majority of the physics community accepted the orthodox interpretation of quantum theory being disseminated from Copenhagen, preferring to focus their attention on more pragmatic or practical pursuits by

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[320] Max Born, *Natural Philosophy of Cause and Chance.* (New York: Dover Publications, Inc., 1964), 2. First Published by Oxford University Press in 1949.

extending and applying the established formulations of quantum mechanics.[321] By the time the Fifth Solvay Council ended, it was clear to most working physicists that the analytic power of this new mechanics was unrivaled. Could the explanation for the rise of pragmatism be as simple as: the new quantum mechanics was so powerful and effective as an analytical tool, that physicists stopped caring what the philosophical implications were? There is no doubt that this was a major consideration for physicists working on practical applications of quantum theory, however, this does not explain why the pragmatism ran so deep and persisted so long.

When studying this period we are able to identify four important and distinct yet correlated dynamical threads that seem to have strengthened and become explicit within the international physics community after those landmark days at Solvay in November 1927 and are pertinent to our analysis of this lull in interpretation debates. First, the momentum of the synthetic alternate wave mechanical formulation program seemed to have lost steam as de Broglie, the most visible remaining holdout, began to hedge his bets and abandon the research program he had advocated for years. Second, alliances were formed among many of the physicists who had been associated with the four elite quantum schools, especially Göttingen and Copenhagen, and who were responsible for the most significant advances in quantum theory. Third, the ranks of pragmatic physicists continued to swell pushing harder for a full exploitation of the formulations which best served their particular purpose without care for the underlying interpretations. Finally, the foundations of quantum theory were formalized and seemingly cast in an explicit mathematical and conceptual rigor by the fathers of the revolution

[321] Throughout this chapter I will use the term pragmatism in physics to indicate the application of formulations and other analytical tools in order to attain empirical results. This pursuit distinguishes itself from the physicist-philosopher of the 19th century in that it disregards the need to consider the philosophical implications of applying the underlying physical theory.

with a slew of textbooks that helped to proselytize the congealing orthodox interpretation of quantum theory.

Our analysis will show how all four of these correlated threads played critical roles in the important historical transition of the international physics community, from a state of revolutionary upheaval where many long standing foundational concepts were challenged and overturned to a period of relative interpretational calm focused more on applying the new powerful analytic machinery than discussing the various interpretations of the foundations of quantum theory. While scholars have tended to simply extend this pragmatic period forward to the present as if it were a natural evolution of physics as a discipline, we shall see that it is better understood as a lull in the dialog of interpretation among physicists. By the late 1940s an increasing number of physicists had once again begun seriously considering foundational and interpretational issues in quantum theory, almost as if they were attempting to recapture the mantle of natural philosophy that Max Born admonished them against losing.

5.2- Abandonment Alternative Interpretive Lines and the Tentative Acceptance of Indeterminism

When studying the dynamics of the physics community, from 1926 to 1930, in chapter four, it seems fairly evident that the eventual abandonment of alternate wave mechanical formulations of quantum theory was closely tied to the temporary abandonment of interpretation debates and the tentative acceptance of indeterminism. As with all historical transitions this was not an instantaneous one. There were physicists like Heisenberg and Jordan who were ready to abandon interpretation debates and determinism, in favor of a more pragmatic approach to quantum theory almost from the very beginning of its inception. They seemed to elevate the empirical power of a theory above all other considerations, including the retention of intuitive classical concepts. In that sense, Heisenberg's initial approach to make the mechanics within the atom a 'black box' without concern for space or time considerations and to instead focus analysis only on measurable observables is a concrete example of this attitude.

As we noted in the previous chapter, the majority of the physics community was not as eager as Heisenberg to abandon classical intuitive principles like particle trajectories and determinism, which inevitably led to the development of all the various synthetic wave-mechanical programs we highlighted earlier. Most of these programs were short lived and some, like Einstein's were never published, so by the time the physics elite convened in Brussels for the Fifth Solvay Council in November 1927 most alternate synthetic wave-mechanical interpretations of quantum theory had been abandoned for one reason or another. De Broglie seemed to be the strongest stalwart and by the fall of 1927 he too was tenuously holding to his synthetic quantum program. He had worked diligently and in relative isolation for years on a program to bring a unified and deterministic quantum theory to the table and while he remained convinced that the program was possible in theory, he was well aware that his latest efforts did not represent a fully capable alternative.

It is commonly said that de Broglie was forcibly "converted" from his committed position in favor of determinism to that of Bohr's complementarity at the 1927 Solvay Council. As we saw in the last chapter, this characterization is not a fair and accurate representation of historical events. As is often the case, we can point to a superposition of causal factors that led to his conscious decision to abandon his alternate wave mechanical research program. First and foremost, as we know, de Broglie himself had been struggling somewhat with his formulation of quantum theory and was not blind to its difficulties of application with regard to real atomic systems. Second, he certainly recognized the practical power of the dominant formulations being espoused and the growing momentum from pragmatists trying to minimize speculative philosophical physics. Third, the Solvay meetings reiterated his isolation with respect to the international physics community, it was clear to de Broglie that he had never been an integrated member of the international quantum network centered on the four quantum schools.

Fourth, after he presented his alternate wave mechanical formulation at Solvay 1927, he encountered overwhelming opposition from Bohr's complementarity principle and in particular was challenged by Pauli and others on the practicality of his proposed interpretive formulation. In addition, this challenge by Pauli stood alongside a very powerful presentation by Born and Heisenberg on the congealing quantum formalism toolbox that was being used extensively and successfully throughout the physics community. Finally, in addition to this opposition, there was no sense of unity, and no real identity of a cohesive anti Göttingen-Copenhagen camp. Einstein, Schrödinger, and Lorentz were all displeased with certain aspects of the prevailing quantum theory but they were not on the same page with regard to all interpretational matters. While Einstein did voice some support for de Broglie to continue his efforts, overall, there was little support from the other Solvay participants. This lack of unity in the face of a seemingly congealing opposition must have given de Broglie pause to seriously reconsider his interpretive position.

It was a confluence of all these factors, and not any one of them in isolation, that led de Broglie to make the calculated and conscious decision to abandon his deterministic and synthetic wave mechanical quantum theory lest he become part of an older forgotten establishment. According to de Broglie, this conscious abandonment was not a result of being marginalized or bullied by a dominant group but a thoughtful and deliberate transition in his overall approach to quantum theory. As we shall see in chapter six this was not to be the last of his deliberate transitions between interpretive stances. According to one of his biographers, de Broglie was fond of quoting Voltaire and declaring that "It is only a stupid man who doesn't change."[322]

Outside of de Broglie's transition after the 1927 Solvay Council there was a general reluctant acceptance from the greater physics community of the need for indeterminism to be an integral part of any successful quantum theory. In 1929 Schrödinger gave a talk in front of the Prussian Academy of Sciences in which he discussed this very subject. The lucidity of the discussion cannot be taken for granted as many physicists in the first three decades of the 20th century had used causality and determinism interchangeably without proper context or definition. In his address, Schrödinger paid homage to what he considered his most influential teachers Fritz Hasenöhrl and Franz Exner. He recalled that Hasenöhrl had used Boltzmann's original conceptions of indeterminism to teach the young Schrödinger about the differences between indeterminism and acausality. For Schrödinger, indeterminism simply came from the inability to fully know the initial conditions of all the microscopic particles that make up a macroscopic composite particle which inherently leads to unpredictability, whereas, causality implied having knowledge about the dependence between two physical states. In other words, acausality implies an absence of any understanding of the dependence (or the 'why?') corresponding to the dynamics of physical phenomena. If all causality were categorically abandoned physics would cease to offer its most powerful analytic contributions. In this sense, understanding causal relationships is the very essence of physics.

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[322] Georges Lochak, "A Complementary Opposition: Louis de Broglie and Werner Heisenberg" Published in *Quantum Mechanics at the Crossroads.* Edited by James Evans and Alan Thorndike. (Berlin: Springer-Berlag 2007), 77.

According to Schrödinger, however, this understanding does not preclude the possibility of acausality in theory. Schrödinger pointed to Exner's claim of an "acausal" interpretation of the micro-world as long as the composite macro-system followed a "statistical" causality. These conceptions, outlined by Schrödinger in 1929, and based on Hasenöhrl and Exner's treatments of causality and determinism seems to have persisted for a long time within the interpretation of quantum theory. In fact, it is very similar to the later notions of causality and determinism subscribed to by the likes of von Neumann, Pauli, and Born. As an illustration of this point let me offer up four parallel statements by Schrödinger, von Neumann, and Pauli that span twenty years and essentially follow Exner's thinking on causality.

According to Schrödinger's 1929 conceptualization, the notion of macroscopic causality we are used to emerges only as a composite system tends to a large numbers of micro-particles:

> Within the past four or five decades physical research has clearly and definitely shown… that *chance* is the common root of all the rigid conformity to Law that has been observed, at least in the overwhelming majority of natural processes, the regularity and invariability of which have led to the establishment of the postulate of universal causality.[323]

However, according to Schrödinger:

> The demand for a causal absolute law in the background of a statistical law – a demand which at the present day almost everybody considers imperative – goes beyond the reach of experience. *The burden of proof falls on those who champion absolute causality, and not those who question it.* For a doubtful attitude in this respect is today by far the more natural.[324]

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[323] Erwin Schrödinger,1929. As quoted in Michael Stöltzner, "Opportunistic Axiomatics – Von Neumann on the Methodology of Mathematical Physics." in Rédei, M. and Stöltzner, M. (eds.) *John von Neumann and the Foundations of Quantum Physics.* (Dordrecht: Kluwer Academic Publishers. 2001), 55.
[324] As quoted in Yemima Ben-Menahem, "Struggling with Causality: Schrödinger's case" *Stud. Hist. Phil. Sci.,* Vol. 20, No. 3, (1989): 308.

In a statement from his landmark text on the mathematical foundations of quantum theory published in 1932, which we shall return to later in this chapter, von Neumann resonated with Exner and Schrödinger's position on causality when he declared:

> …the apparent causal order of the world in the large (i.e., for objects visible to the naked eye) has certainly no other cause than the 'law of large numbers'; and this is completely independent of whether the natural laws governing the elementary processes (i.e., the actual laws) are causal or not.[325]

In later commentary, Pauli also showed a similar take on the problem of causality and determinism:

> Causality, i.e., the possibility of describing natural phenomena in a rational way, is an expression of God's intelligence. The simple idea of deterministic causality must, however, be abandoned and replaced by the idea of statistical causality.[326]

Finally, in 1948 Born, gave a series of lectures at Oxford on the "Natural Philosophy of Cause and Chance" which were later collected in a volume published in 1949. In these lectures it is clear that Born is attempting to address the past misuse of the terms causality and determinism. He claims in his introduction that when language is used to explain physics instead of mathematics critical concepts become poorly defined and the treatment is generally messy and distorted. According to Born, this has led to the poor understanding of the distinctions between causality and determinism.

[325] John von Neumann, *Mathematische Grundlagen der Quantenmechanik.* First published in German in 1932 and then translated as *Mathematical Foundations of Quantum Mechanics*, (Princeton: Princeton University Press, 1955), 326. All page numbers are from the English version.
[326] James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony,* 151.

> The statement, frequently made, that modern physics has given up causality is entirely unfounded. Modern physics, it is true, has given up or modified many traditional ideas; but it would cease to be a science if it had given up the search for the causes of phenomena.[327]

While Born admits that causality and determinism are closely linked concepts and that causality can be used in physics with “several different shades of meaning,” he arrives at clearly distinct notions of the two somewhat in accordance with Exner’s original notions.

> …not causality, properly understood, is eliminated, but only a traditional interpretation of it, consisting in its identification with determinism. I have taken pains to show that these two concepts are not identical. Causality in my definition is the postulate that one physical situation depends on the other, and causal research means the discovery of such dependence. This is still true in quantum physics, though the objects of observation for which a dependence is claimed are different: they are the probabilities of elementary events, not those single events themselves.[328]

It seems clear that quantum physics was never in danger of giving up full causality as it was understood within the physics community. However, due to its renunciation of determinism and the subsequent conflation of these two concepts due to their evolving use, it became necessary for some physicists to clarify their position on this topic. In all my readings, Born’s 1948 lectures are the most lucid commentary on this topic. While it is not within the scope of this study to fully analyze the use of causality and determinism within the physics community during the first half of the 20th century. It is clear that along with the abandonment of alternate wave mechanical interpretations of the quantum theory and the adoption of the Göttingen-Copenhagen Interpretation of quantum theory, de Broglie and others were forced to

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[327] Born, *Natural Philosophy of Cause and Chance,* 3-4.
[328] Ibid., 101-102.

accept that indeterminism and microscopic acausality were necessary implications of Heisenberg's uncertainty principle. Nevertheless, we will see later in this chapter that while the vast majority of the community reluctantly accepted indeterminism, there was still a highly vocal and visible minority, spearheaded by Albert Einstein, which would not abandon any notion of causality.

5.3- The Congealing of a Heterogeneous Orthodoxy

With the conscious alignment between physicists such as Bohr, Born, Heisenberg, Jordan, and Pauli, and a falling off of any viable alternate approaches, the stage was set for a powerful congealing of an orthodox interpretation of quantum theory. As Bohr spearheaded this congealing effort, the interpretation became synonymous with the city of Copenhagen.

Heisenberg's initial reactions to Bohr's complementarity principle in the fall of 1927 were typical of many physicists as he and others were tentatively supportive, but by the summer of 1928 he was declaring that the "fundamental questions [in quantum theory] had been completely solved" and that Bohr's complementarity was the capstone for the whole interpretation. Furthermore, while touring in the United States during the spring of 1929, Heisenberg began talking of a "*Kopenhagener Geist der Quantentheorie*" or a Copenhagen spirit of quantum theory.[329] This spirit of Copenhagen was exactly what Oliver Lodge had railed against in his remarks published in *Nature* during March of 1927, and what luminaries such as Lorentz and Einstein found so detestable at the Solvay Council later that same year. There was a sense of ontological settling or renunciation imbued in this spirit. While any of the Göttingen-

[329] John Heilbron, "The earliest missionaries of the Copenhagen spirit," Revue d'histoire des sciences, Année 1985, Volume 38, Numéro 3, (1985): 201.

Copenhagen approved quantum formulations like Schrödinger's wave mechanics or Göttingen matrix mechanics along with Born's probabilistic interpretation and Heisenberg's uncertainty principle, defined an extremely effective toolbox for the physics community studying quantum phenomena, Bohr's complementarity was a statement of principle not seen by most as adding any probative value to the study of physics. However, in the context of a fledgling quantum theory this principle at least gave a consistent interpretive umbrella under which to stand.

In 1985, John Heilbron wrote a seminal paper entitled: "The earliest missionaries of the Copenhagen spirit" in which he traced the dissemination of this interpretation throughout the physics world. He referred to his study as following "the intellectual imperialism of Bohr's group throughout the 1930s."[330] The use of the term "missionaries" in the title reflects Heilbron's conclusion that some of the key quantum figures that helped shape the field early on could almost be considered religious zealots in how fervently they spread the principles, which eventually became synonymous with the Copenhagen Interpretation. Heilbron goes as far as to use the term "disciples" in referring to Bohr's most passionate supporters like Pascual Jordan and Léon Rosenfeld.[331] In the face of stern criticism from physicists and other intellectuals, these most passionate disciples extended Bohr's ideas on complementarity outside the realm of physics and developed general holistic epistemologies that addressed far-ranging subjects like biology, anthropology, theology, and Marxist philosophy.

As we have seen throughout this study, in contrast to the portrayal in many historical quantum narratives, responses from the physics community to innovations can never be reduced to a single stance; as a result there is almost always a diverse chorus of reception. This was certainly true with regard to Bohr's principle of complementarity, which experienced a far

[330] Ibid., 196.
[331] Ibid., 196.

ranging and highly dependent spectrum of reactions, each based on a particular set of contextual factors. Apart from the zealots like Jordan and Bohr himself, Rosenfeld made a transition from skeptic to zealot.[332] In the spring of 1927 Rosenfeld had travelled to Göttingen in order to work with Born and had completely abandoned any hope of continuing the deterministic unification work he had begun with de Donder on quantum currents and potentials. After a sharp interpretational transition, he quickly established himself as one of the most devoted followers of the Copenhagen spirit.

In addition to these zealots we can identify a group of firm supporters who chose to temper their support in different ways, exemplified by physicists like Heisenberg and Pauli, and also a dwindling group of physicists who rejected Bohr's particular brand of interpretation as being irrational and too convenient, and finally an ever-growing group of pragmatists focused more on the applications of the quantum formulation while remaining mostly indifferent to all forms of interpretive agendas.

While Pauli and Heisenberg were not as zealous about extending Bohr's interpretive agenda as Bohr's most devoted disciples they were certainly key members of its dissemination, especially in light of their high status within the physics community. We have already noted how Heisenberg, while not totally aligned with Bohr's complementariy at first, had by 1929 coined the term Copenhagen spirit. Meanwhile, during this same period Pauli was pushing to make complementarity a full blown "theory" on par with relativity theory.[333] For both of them, one of the key attributes of this spirit was a renunciation of directly attributable characteristics of atomic phenomena. As Pauli later stated so succinctly: "the renunciation of the unambiguous

[332] In our discussion of the proceedings of the Fifth Solvay Council in Chapter four, we discussed Rosenfeld's contributions to an alternate deterministic formulation of quantum theory that involved, similarly to de Broglie and Madelung's theories, a quantum potential and force. After the Council he worked with Born in Göttingen and eventually reversed course on his interpretational stances.
[333] Heilbron, "The earliest missionaries of the Copenhagen spirit," 202.

objectifiability of natural processes." This renunciation of an unambiguous understanding of natural processes translated into a forced choice for physicists between incommensurable frameworks of analysis. Ultimately, for those imbued with the Copenhagen spirit complementarity pointed, not to a technical limitation in measurement apparatus, but to an inherent ambiguity in a physicists' ability to distinguish between an observer and the object being observed during a quantum measurement. It is important to note that this alignment was not seamless, there were aspects of the spirit being disseminated that made some, otherwise supportive, physicists nervous. Pauli expressed this trepidation commenting on Bohr's "urge to close up one's intellectual structure, to bring everything under one roof."[334]

For those like Lodge, Einstein, Schrödinger, and Planck Bohr's stated umbrella principle was acceptable maybe as a temporary stopgap while a deeper theory based on synthesis was being developed but as far as it being a complete representation without any hope for further discovery, that was too difficult for these physicists to accept. From this perspective, the forced choice between complementary and incommensurable representations of nature was nothing but a false choice borne out convenience. F.A. Lindemann stated unequivocally that "renunciation has no appeal for us" and Planck was astonished by the Copenhageners' willingness to abandon the search for a deeper atomic theory.[335] In a critique of Bohr's complementarity, Carl Benedicks summed up his opposition to the Copenhagen spirit by questioning the very character of the physicists involved, pointing to their lack of patience and resiliency:

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[334] Ibid., 225.
[335] Ibid., 225.

> “The enthusiastic resignation of the Copenhagen group is [unacceptable]… Not knowing how to go further, they had succumbed to hopelessness; rather than trying to surmount their difficulties, they had raised them into barriers that no one can pass.[336]

However, in the face of this criticism, Rosenfeld made the clear counterpoint for the Copenhagen position while usurping the opposition’s very notion of “synthesis” to describe complementarity:

> The discovery of quantum laws, which are irrational from the standpoint of classical causality; but such a renunciation, imposed by the very richness of our experience, obviously has no admission of defeat; we are renouncing a prejudice to rise to a new synthesis.[337]

While these interpretation debates were certainly lively and dramatic and involved some of the biggest names in international physics, after the 1927 Solvay Council it became clear that the pragmatists were beginning to win the day with less value being placed on each successive statement of interpretation. In an earlier chapter, we saw Darwin’s reticence to engage Bohr in any form of interpretive debate. On this point, he was in strong alignment with what was becoming the silent majority. Most pragmatists were open to the quantum mechanical formulations that were espoused by the Copenhagen Interpretation but were unwilling to engage in the more speculative and esoteric interpretive dialog. They were thus able to distinguish clearly between what they considered useful formulations and speculative interpretations. One of the most often quoted statements was made by the quintessential pragmatist Dirac who later stated “I never liked complementarity…It does not give us any new

[336] Ibid., 221.
[337] Ibid., 203.

formula."[338] Another British physicist and Rutherford protégé A.S. Eve pointed out "We are too practical to think to find the truth through philosophical subtleties. When the key is lost [we] smash the lock and force the cupboard."[339] In the United States, J.C. Slater of BKS fame, made the pragmatic observation while presenting at a symposium in 1928 that "Wave mechanics is an extension, not of ordinary Newtonian mechanics, but of statistical mechanics." Therefore, if one avoids trying to deal with individual particles and instead focuses ensembles, all the peculiarities and "otherwise puzzling features" of quantum mechanics disappear. Slater had quickly reduced the principle of complementarity into puzzling yet transient features of quantum theory.[340]

The general tone in both British and American physics communities during this period was one of pragmatism towards quantum theory, however, there were those in Germany and elsewhere that shared their tendencies. For example, the approach taken in Sommerfeld's institute in Munich was highly pragmatic and his quantum school remained extremely influential as he continued to attract many of the most promising young physicists. In 1928, Hans Bethe, a 22 year old doctoral student, encapsulated the Munich approach as he wrote to a fellow student, that the more philosophical aspects of physics being argued in Copenhagen were "not to be taken too seriously."[341] This was almost an echo of Sommerfeld's own stance which he published in his 1929 edition of *Atombau und Spektrallinien*. In this edition of his seminal textbook on atomic physics, he readily accepts Born's probabilistic interpretation and Heisenberg's uncertainty principle and deals extensively with the measurement problem but never mentions Bohr's principle of complementarity or any of its more philosophical implications. In no uncertain terms Sommerfeld warns his readers that "I have essentially

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[338] Beller. *Quantum Dialogue,* 119.
[339] Heilbron, "The earliest missionaries of the Copenhagen spirit," 205.
[340] Ibid., 206.
[341] Ibid., 206.

limited myself to such problems as can claim immediate physical interest," implying that, similar to Dirac's critique, complementarity had no practical application.[342]

We have briefly outlined a spectrum of reaction to Bohr's principle of complementarity and the subsequent Copenhagen Interpretation that was disseminated by his supporters and disciples. After this discussion, it should come as no surprise that we believe reducing the Copenhagen Interpretation to one single thread or representative voice would be distortive, just as it would be equally distortive to reduce the physics community's reaction to this proposed interpretation as homogenous. As is true of most historical transitions, the causes behind the congealing of the Copenhagen Interpretation were many-fold. They rested on a confluence of factors such as the abandonment of plausible alternate deterministic wave mechanical quantum theories, the refining of equivalent formulations that could be applied to an increasing number of physical systems, the growing ranks of physicists uninterested in interpretive debates, and the diminishing opposition from engrained realists. In the following section we will examine in more detail some of the essential factors that help to explain why the physics community was tending towards pragmatism.

5.4- Spreading the Seeds of the Copenhagen Spirit - Catalysts to a Congealing Pragmatism

In chapter two we noted the importance of social and academic networking in the field of quantum physics during the 1920s. In particular, the four quantum schools that we highlighted in Munich, Copenhagen, Göttingen, and Leiden were instrumental in establishing the networks that would later be used to disseminate the Copenhagen spirit throughout the greater

[342] Ibid., 206.

international physics community. By 1930 the seeds carrying the very foundations of quantum theory were beginning to germinate throughout the world. As we noted in the previous section the diffusion and congealing of the Copenhagen spirit was due partly to a conscious alliance between influential European physicists and a rise in pragmatism within the field of physics, in this section we look more closely at the underlying reasons for this transition to pragmatism that served to dampen debates on the foundations and interpretations of quantum theory. Our subsequent analysis will focus on the movements of physicists and the pedagogical indoctrination led by the publication of popular textbooks on the subject.

In trying to understand the diffusion of the Copenhagen spirit throughout the physics community it is important to understand the evolution of its power centers due to the movement of physicists themselves. In the early 1930s a clear and dramatic power shift began to take hold from an international theoretical physics community that had been dominated by continental Europe to one that was beginning to be represented very adequately by young American theoretical physicists. While this shift came about primarily due to a deliberate policy within the American physics community it was reinforced and amplified by the rise of fascism in Europe and the subsequent migration of some of the most accomplished physicists.

The American physics community had been dominated by experimentalists during the first twenty years of the 20th century, especially influential were: G.W. Pierce, P.W. Bridgman, R.A. Millikan, K.T. Compton, H.M. Randall, and A.H. Compton working at leading universities such as Harvard, Caltech, Princeton, Michigan, and Chicago. After World War I it became clear to these physicists that in order to work on topics related to the quickly evolving quantum theory, they would need collaborators who were proficient in the mathematical machinery necessary to make the complex calculations they required. One way to achieve this was to send the brightest young physicists to study at the preeminent quantum schools with the leading theoretical physicists in the world and then have them return to the United States and establish their own research programs at elite American universities.

While this initiative was somewhat constrained by public funding for doctoral and post-doctoral fellowships and grants, these leading American experimentalists lobbied private philanthropic foundations, among them, the Guggenheim, Carnegie, and Rockefeller foundations to lend support. It was thanks to many of these privately endowed fellowships that bright promising physics students like Oppenheimer, Slater, and Rabi where able to study in Europe and then return to establish their own American quantum schools at Berkeley, MIT, and Columbia University respectively. However, when these elite theoretical physicists did return to establish their American quantum schools, their character was not European but quintessentially American in nature. They were molded as pragmatists, theoretical physicists trained to collaborate with experimentalists. Whereas in the European universities theoretical and experimental physicists were generally housed in separate institutes, American universities began to promote single physics departments in which collaboration between theorists and experimentalists was the expected institutional norm not the pleasant exception it was in Europe.[343]

In a 1938 paper, Slater described his conceptualization of theoretical physics which encapsulates the American pragmatic approach and resonates with what Born later referred to as the loss of philosophy within physics:

> A theoretical physicist in these days asks just one thing of his theories: if he uses them to calculate the outcome of an experiment, the theoretical prediction must agree,

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[343] Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States 1920-1950" *Historical Studies in the Physical and Biological Sciences, Vol. 17*, No. 1,(1986): 56-58.

> within limits, with the results of the experiment. He does not ordinarily argue about philosophical implications of this theory. Almost his only recent contribution to philosophy has been the operational idea, which is essentially only a different way of phrasing the statement I have just made, that the one and only thing to do with a theory is to predict the outcome of an experiment. Questions about a theory which do not affect its ability to predict experimental results correctly seem to me quibbles about words, rather than anything more substantial, and I am quite content to leave such questions to those who derive some satisfaction from them.[344]

While sending elite physics students to Europe on fellowships to study at the leading quantum schools was an effective way of growing the American theoretical physics community, a less intensive yet more efficient approach also arose after World War I. Almost from the birth of quantum physics in Europe at the turn of the 20th century we see a corresponding tendency for the leading physicists including Lorentz, Planck, Einstein, Sommerfeld, Born, Debye, and Heisenberg to periodically travel to the United States for extended stays as invited visiting faculty members in elite American universities. Obviously networking was important to the Europeans but what really drew the elite quantum physicists from their respective institutions was the comparatively high honoraria and salaries offered by their American host institutions.[345] By 1930, several universities had begun to make generous offers to make these visiting professorships permanent. In particular, Caltech was able to hire Paul S. Epstein and Richard Tolman; the University of Michigan recruited Oskar Klein, Otto Laporte, Samuel Goudsmit, and George Uhlenbeck; and Princeton University hired John von Neumann and Eugene Wigner.[346]

While the theoretical physics community in the United States was certainly well on the rise thanks to the deliberate post World War I policy initiatives, the power shift in the international physics community from continental European to Anglo-American dominance was

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[344] Ibid., 66.
[345] Ibid., 72-73.
[346] Ibid., 73.

given a strong catalyst by the rise and spread of fascism throughout Europe in the early 1930s. As a result of the Nazis coming to power in Germany in 1933, many of the most celebrated and capable minds working on quantum physics migrated away from continental Europe thereby taking with them what was left of the dominance of European physics. Both the United Kingdom and the United States benefitted dramatically from this emigration, and as a result, a new power dynamic within the international physics community was established. In fact, between 1933 and 1941 over 100 physicists immigrated to the United States from Germany, Austria, Italy, Czechoslovakia, and the Soviet Union. Among the more notable were Enrico Fermi, Walter Elsasser, Edward Teller, Hans Bethe, Felix Bloch, Victor Weisskopf, and George Gamow.[347] The downfall of the once mighty Göttingen quantum school was a clear example of the brain drain that Europe suffered during this period due to the forced emigration of important physicists. While it had been one of the most celebrated quantum schools in the world just a few years earlier, when the Nazis came to power in 1933 they decided to purge the University of its Jewish Faculty. As a result, eminent physicists and mathematicians including Max Born, James Franck, Edward Teller, and Richard Courant were forced out. From one year to the next, Göttingen made the transition from leading quantum school to a university with no physicists or mathematicians of note.

In addition to the dissemination of the Copenhagen spirit coming from the actual movements of physicists who had been associated with the quantum schools in the 1920s and now found themselves teaching, outside of continental Europe, many of the protagonists of the 1927 Solvay Council published a rash of quantum textbooks around 1930 which served to further the congealing of the Copenhagen spirit and establish the foundations of the new theory

[347] Ibid., 80-81.

for subsequent generations of physicists. The impact of the pedagogical factor on the congealing needs to be especially highlighted here since quantum theory was such a counter intuitive course to teach, and was therefore significantly more dependent than other subjects on the particular textbooks used.

In, or about, 1930 several seminal textbooks on quantum physics were published by some of the most celebrated protagonists of the quantum revolution. There were texts by Heisenberg and Dirac, but others by Born and Jordan and de Broglie himself certainly served as reinforcement to the dissemination of the Copenhagen spirit.[348] Obviously, each book was reflective of the author(s)' particular approach to the subject matter so there were clear differences in how the material was treated. We noted in chapter four that Pauli had been extremely critical of Born and Jordan's text as it stuck primarily to the matrix formulation and failed to discuss the equivalent Schrödinger wave mechanical formulation. On the other hand, de Broglie's appropriately named textbook tended to the other extreme emphasizing almost exclusively wave mechanical approaches. It is interesting that while de Broglie does reintroduce his ideas developed within his program on wave mechanics he is very careful to state up front in the introduction that "It is not possible to regard the theory of the pilot-wave as satisfactory. Nevertheless, since the equations on which this theory rests are sound, we may preserve some of its consequences…"[349] As these two examples show, the texts were certainly not redundant instead revealing the variety of approaches to quantum theory that had grown out of the complex flux of the 1920s. Having said that, we should point out that although each treatment had its own particular approach, most of the early quantum mechanical textbooks published at

[348] A perusal of the literature gives us at least the following texts all published from 1929-1930: Werner Heisenberg, *The Physical Principles of the Quantum Theory*; P.A.M. Dirac, *The Principles of Quantum Mechanics*; Max Born and Pascual Jordan, *Elementare Quantenmechanik*; and Louis de Broglie, *An Introduction to the Study of Wave Mechanics*.

[349] Louis de Broglie, *An Introduction to the Study of Wave Mechanics*, Translated by H.T. Flint. (London: Methuen & Co. Ltd.. 1930), 7.

this time had some important similarities. For example, the vast majority of these early books while recognizing the full quantum mechanical toolbox of equivalent formalisms chose to especially focus on the Schrödinger wave mechanical approach.[350]

Of all the common features that these textbooks displayed the one that is of particular interest to our study, is the notion that they reinforced the uneven dissemination of the Copenhagen spirit. As we noted earlier, for most physicists of the time, the Copenhagen spirit was clearly divided into the practical toolbox of formalism and the more philosophical interpretation espoused by Bohr under his complementarity principle. While all textbooks dealt extensively with the practical application of the formalism toolbox that Born and Heisenberg had laid out at the Solvay Council in 1927, including, Heisenberg's uncertainty principle and the statistical interpretation proposed by Born and extended by Dirac and Jordan in their transformation theory, the vast majority of these textbooks did not even mention the interpretive aspects of quantum theory promulgated from Copengagen. In fact, as an illustration of this point, in a survey of forty-three textbooks on quantum physics published between 1928 and 1937 only eight included any mention of the complementarity principle while forty examined Heisenberg's uncertainty principle.[351]

In order to gain a better understanding of the particulars of the pedagogical agent to the congealing orthodoxy, we will briefly discuss P.A.M. Dirac and Werner Heisenberg's texts as they became the two most important of these 1930 publications. While Dirac's approach in writing his textbook was in many ways diametrically opposed to Heisenberg's presentation, they both

[350] Apart from de Broglie's text on wave mechanics we some of the more notable texts on the subject were written by: H.F. Biggs, J. Frenkel, A.E. Haas, and A. Sommerfeld. See Jammer, *The Conceptual Development of Quantum Mechanics,* 397.
[351] Kragh, *Quantum Generations,* 211.

made applications and empirical justifications for the new quantum theory central pillars of their textbooks. Dirac's quantum mechanics textbook, *The Principles of Quantum Mechanics* was in many ways the most axiomatic and comprehensive of the early expositions of quantum theory and probably the most influential for future generations of physicists. One reviewer in 1931 praised Dirac's book by claiming:

> An eminent European physicist, who is fortunate enough to possess a bound set of reprints of Dr. Dirac's original papers, has been heard to refer to them affectionately as his 'bible.' Those not so fortunate have now at any rate an opportunity of acquiring a copy of the authorized version.[352]

Dirac began by introducing the basic underlying principles of the theory introducing the notions of operators, observables, and quantum states and then deliberately developed the associated mathematical machinery, focusing on representing ψ in a multidimensional vector space that would later be linked by von Neumann to the Hilbert space Dirac relied heavily on his evolving "symbolic" formulation of the theory. However, as he was interested in giving a comprehensive exposition of the new theory, Dirac set out to emphasize the unity between all the equivalent mathematical formalisms, including wave and matrix mechanics, and the practical nature of each approach.[353] By basing his discussion on the axiomatic and underlying physical principles of quantum theory he believed he could reveal a deeper understanding of fundamental concepts like transformation theory. After systematically laying out the foundations of quantum theory and its equivalent mathematical formulations; he then proceeded to apply them to a plethora of quantum phenomena thereby showing the new theory's probative power, robustness, and vast potential. This recipe was so successful that it

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[352] As quoted in Jammer, *The Conceptual Development of Quantum Mechanics,* 389.

[353] The algebraic formulation eventually morphed into his bra-ket notation which he added to his book during the revisions that led to its 3rd edition printing.

went through many editions and essentially became the blueprint for how quantum theory has been taught to physicists ever since.

Heisenberg, by then a professor at the University of Leipzig, published *The Physical Principles of the Quantum Theory*, based on a series of lectures he had given in Chicago during the spring of 1929 where he had first introduced the very notion of the *"Kopenhagner Geist der Quantentheorie."*[354] This short and elementary text was widely read and studied by physicists and non-physicists alike. Heisenberg's approach was to focus on explaining the physical or conceptual aspects of the new theory, relying heavily on experiments as empirical guides, and leaving the mathematical machinery to a cursory discussion in an appendix. According to Heisenberg the approach to the development of quantum theory was:

> …to introduce a great wealth of concepts into physical theory, without attempting to justify them rigorously, and then to allow experiment to decide at what points a revision is necessary.[355]

This approach was certainly followed to a tee in his exploration of the new theory captured in his text. After critiquing basic concepts in both classical corpuscular and wave theories he proceeded to introduce the statistical interpretation as a solution to the critiques and then dedicated the remaining portion of his exposition to discussing the "important" experiments. Among others he focused on the Wilson cloud chamber experiments; the various diffraction experiments by Davisson-Germer, Thomson, Rupp, and Kikuchi; canal ray experiments designed by Einstein and carried out by Rupp; as well as other landmark experiments including: Franck-Hertz, Stern-Gerlach, Compton-Simon, and Geiger-Bothe.

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[354] Werner Heisenberg, *The Physical Principles of the Quantum Theory*, (Chicago: University of Chicago Press, 1930), Preface.
[355] Ibid., 2.

One of these texts' common themes was emphasizing empirical evidence as well as experimental justifications for the congealing Copenhagen Interpretation, a critical asset to the early acceptance and further development of quantum theory. In addition, during this period there was a clear correlation between the growing pragmatism within the community and the need for experimental justification. As a result, these foundational texts played a key role not only in dissemination of the accepted Copenhagen spirit but also in the continuing perpetuation of that very same aura of pragmatism. In retrospect, this was certainly a justifiable tact to take as there had already been a slew of extraordinary successes in applying the various formulations of the new quantum theory by 1930.

As an exhaustive study of all the early applications of quantum theory lies out of the realm of our discussion, we will use this opportunity to briefly discuss one particular example of an application that came out of the quantum revolution and had both distinct and tightly correlated experimental and theoretical components. The study of particle trajectories in a Wilson cloud chamber allowed for an early validation of the new quantum theory and served to highlight the practical and probative value of the generally accepted formulations. It is critical for us to understand quantum physics' empirical contributions because as Heisenberg points out to the reader of his book before going on to discuss the previously mentioned "important" experiments: "…the principles of the quantum theory have all been discussed, but a real understanding of them is obtainable only through their relation to the body of experimental facts which the theory must explain."[356]

We saw in a previous chapters how C.T.R. Wilson's cloud chamber was used by J.J. Thomson and others early in the 20th century to show qualitatively the ionization of gases from

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[356] Ibid., 66.

passing x-ray and gamma radiation or similarly, the condensation tracks produced by the passing of charged alpha or beta particles through the hermetically sealed and precisely controlled chambers. While Wilson originally developed the idea in the 1890s to study cloud formations around dust particles, by the 1920s the technique and apparatus had shown a remarkable capacity to shed a qualitative light on some of the most compelling problems facing physics. However, it was really after the development of a more applicable quantum mechanics in the mid 1920s, that Wilson's innovation began showing its true potential and living up to Lord Rutherford's characterization of it as "the most original and wonderful instrument in scientific history."[357] In a later study published in the Reviews of Modern Physics on the impact of Wilson cloud chambers on the quantum revolution, the authors succinctly concluded:

> The beauty and ingenuity of the method can hardly be exaggerated. Previous to the discovery of the Wilson chamber, it was possible only to observe the behavior of matter in bulk. The Wilson chamber enables us to study the behavior of individual atoms, to visualize and photograph the actual paths of atoms and electros through gases and to study at leisure the complicated interactions taking place between individual atoms, nuclei, and charged particles.[358]

In 1927 A.H. Compton and C.T.R. Wilson shared the Nobel Prize in Physics for their respective groundbreaking experimental work and the impact it had on the development of quantum physics. In particular, Wilson's citation reads: "for his method of making the paths of electrically charged particles visible by condensation of vapour."[359] The following year, George Gamow, Ronald Gurney and Edward Condon independently developed general quantum mechanical theories for the alpha particle, conceiving them as spherical matter waves

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[357] N.N. Das Gupta and S.K. Ghosh, "A Report on the Wilson Cloud Chamber and its Applications in Physics," Rev. Mod. Phys. Vol 18, No. 2., (1946): 225.
[358] Ibid., 226.
[359] http://nobelprize.org/nobel_prizes/physics/laureates/1927/

emanating from atomic nuclei. Alpha particles had been identified, isolated, and even used in scattering experiments within cloud chambers for more than a quarter century but with the proposition by de Broglie in 1923 that all particles could be understood as matter waves, new ways of understanding alpha scattering were becoming possible. Born had published his series of landmark collision papers during the summer of 1926 in which he treated scattering phenomena quantum mechanically inventing a statistical interpretation. So in 1928, when Gamow and the others developed their alpha particle theories based on matter waves, Charles Darwin was immediately struck with the obvious conundrum. How could one studying an alpha matter wave, spreading spherically from the disintegrating nucleus, reconcile "one of the most striking manifestations of particle characters, the ray tracks of α-particles in a Wilson cloud chamber"?[360]

Six months later, Neville Mott used Born's collision theory, Heisenberg's uncertainty principle, and the transition theory developed by Dirac and Jordan in the spring of 1927 to study the scattering of alpha particles on the detector atoms within a Wilson cloud chamber. While noting the same seemingly contradictory behavior that Darwin had pointed to, Neville's analysis was much more thorough. Using first and second-order perturbation theory Mott showed that in the first order, the outgoing wave is concentrated in a sharp cone behind the ionized atom, in the direction of the incoming wave; and in the second order, there are only contributions when the two atoms lie inside the cone. The result was that the scattered wave propagated along a

[360] Charles G. Darwin, "A collision problem in the wave mechanics," *Proceedings of the Royal Society* A 124, (1929): 375–394.

classical path. Mott's work was considered an important contribution to the clarification of the relationship between micro- and macrophysics.[361]

As it happens, he had based his study almost entirely on the spirit of the Göttingen-Copenhagen approach, and in particular he had relied on Heisenberg's interpretation that the act of observation is a significant contributor to a quantum system's overall dynamics:

> We encounter at once the arbitrariness in the concept of observation already mentioned, and it appears purely as a matter of expediency whether the molecules to be ionized are regarded as belonging to the observed system or to the observing apparatus.[362]

In this approach, one cannot rely on the classical conception of a clean distinct seperability between observer and observed systems. As these alpha particles travel through the supersaturated gas of the Wilson cloud chamber they ionize the individual gas atoms. Subsequently, the two systems interact as successive ionizations are characterized by discontinuous transformations in the probability wave-packet governed by the Dirac-Jordan statistical transformation theory. Attempting to analyze these successive transformations inevitably leads to a convoluted and seemingly unmanageable analysis. According to Mott and Heisenberg's analysis, these systems can no longer be thought of as separate and must be thought of as governed by wavefunctions in superposition.

In the following decades the idea of studying elementary particles by having them interact with some system of "detection" in a closely controlled environment has been extended to a plethora of different subfields within physics. The most obvious extension has been the application of this methodology to the study of high energy physics (HEV or particle physics), but

[361] Mott, N.F. 1929. "The wave mechanics of α-ray tracks", *Proc. Roy. Soc.* A 126, (1929): 79.
[362] Heisenberg, *The Physical Principles of the Quantum Theory*, 66-67.

others like cosmic ray analysis and the methods used in cooling and trapping experiments have been equally invaluable. There is little doubt that the cloud chamber experiments by Wilson and others and their subsequent analysis using the new mathematical machinery developed primarily under the Göttingen-Copenhagen rubric helped to usher in a new era of pragmatic applications of quantum physics.

The idea that the quantum revolution was born out of a continuous and deliberate grounding and coupling between theoretical developments as a way to explain and model empirical phenomena being studied in the experimental community is of the greatest importance, and has been unfortunately overlooked in too many historical narratives. In our study we have attempted to paint a more balanced picture and avoid these distortive oversights. It bears repeating here, that although most narratives characterize the quantum revolution as one primarily engaged in theoretical physics with associated abstract mathematical pursuits, one in which the most important experiments came in the form of Gedanken speculations; it is clear that the practitioners themselves recognized the essential nature of physics as grounded in empirical pursuits. The contributions of experimental physicists working in tandem with theorists to continuously apply new formulations and models with the purpose of explaining or predicting experimental outcomes leads to a very different picture than we are typically shown, one where a symbiotic, intertwined, and inseparable relationship between theoretical and experimental practices emerges.

5.5- John von Neumann's Impossibility Proof as Reinforcement of an Abandonment of Interpretation Debates

It was into this context of growing pragmatism and abandonment of interpretation that John von Neumann interjected, in 1932, his seminal text on the mathematical foundations of quantum theory *Mathematische Grundlagen der Quantenmechanik.* We saw at the end of chapter four that at the 1927 Solvay Council, in one of the most extensive and clearly presented early expositions of the Göttingen-Copenhagen approach to quantum theory, Born and Heisenberg explicitly mentioned the work of Mathematicians at Göttingen. In their remarks they characterized, in particular, von Neumann's collaboration as being critical to their research goals of axiomatizing the new theory and deepening their understanding of the deeper workings and interrelations between the growing set of mathematical formulations applicable to quantum theory. Because much of what Bohm set out to do in 1951 with the development of his hidden variables theory served as a direct counterpoint and challenge to von Neumann's early axiomatizing contributions, in particular a refutation of his impossibility proof, it is imperative that we pause to briefly explore who von Neumann was and the context surrounding his axiomatic contributions to development of quantum theory in the late 1920s and early 1930s.

John von Neumann was born Neumann János on December 28, 1903, in Budapest, Hungary.[363] After John's father, Max, a wealthy banker purchased a title of nobility the 'von' was added to their family name.[364] From an early age it was clear that John von Neumann was a

[363] Traditionally in Hungary the family name came before the given name. From here on out we will refer to János by his adopted anglo equivalent of 'John'.
[364] For general biographical notes on von Neumann and his life: Macrae, Norman. *John von Neumann: The Scientific Genius Who Pioneered the Modern Computer, Game Theory, Nuclear Deterrence, and Much More*. Reprinted by the American Mathematical Society, 1999.

child prodigy, someone to whom the prefix 'poly' is naturally affixed.[365] His early dominance of multiple languages, insatiable curiosity for reading whatever he could get his hands on, alarming capacity to retain knowledge, and his unsurpassed abilities of calculation inevitably led him to earn the labels associated with only the highest and most celebrated geniuses. Miklós Rédei, a von Neumann scholar, has appropriately summarized the greatness of this celebrated Hungarian mathematician:

> If influence of a [mathematician] is interpreted broadly enough to include impact on fields beyond [math] proper then John von Neumann was probably the most influential mathematician ever lived: not only did he contribute to almost all branches of modern mathematics and created new fields but he also changed history after the second World War by his work in computer design and by being a sought-after technical advisor to the post-war military-political establishment of the U.S.A.[366]

After a privileged upbringing in Hungary that afforded von Neumann the highest educational preparation one could afford, the decision was made for him to study chemical engineering, an eminently practical degree, first in Berlin and then at the ETH in Zürich. In addition, however, he was allowed to simultaneously pursue his true academic passion for mathematics as he prepared a doctoral thesis through the University of Budapest while working, in absentia, on a novel axiomatization of George Cantor's set theory. After showing an early aptitude for mathematics, von Neumann had been tutored extensively on the subject and by the time he left for Berlin in 1921, he was a published author in his native Hungary.

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[365] As in the 18th century poem by Irish poet Thomas Moore: "*The Polymaths and Polyhistors, Polyglots and all their sisters"*

[366] This quote is taken from unpublished biographical notes entitled: "John von Neumann 1903-1957" by Miklós Rédei who co-edited the 2001 volume *John von Neumann and the foundations of Quantum Physics*.

During the spring and summer of 1926, at the tender age of 22, von Neumann both graduated from the ETH with a degree in chemical engineering and completed his doctoral degree in Mathematics, with honors, from the University of Budapest. With his poly-vocational academic studies behind him, he embarked for an academic year, on a Rockefeller Fellowship, as David Hilbert's assistant at the University of Göttingen. It was during this 1926-1927 Göttingen stay that von Neumann became enthralled with the nascent quantum theory being developed there, primarily by Born and his colleagues, and with Hilbert's program to treat this new theory axiomatically. As you may recall, quantum theory was still very much in a state of flux during this period, and as Born and Heisenberg indicated in their presentation at Solvay in the fall of 1927, Hilbert's group and in particular von Neumann's contributions had been critical for the development of the evolving Göttingen-Copenhagen Interpretation. Eventually, Hilbert and von Neumann's program to treat the new quantum theory axiomatically was seized upon by Born, Heisenberg, Jordan, and Bohr to substantiate the claim that the quantum toolbox that had been developed was a closed theory that could not be added to. In other words, for those advocating the Göttingen-Copenhagen Interpretation indeterminism became an *a priori* assumption (an axiom) of quantum reality and could not be avoided by enhancing or adding to the existing theory.

As we noted in chapter two, David Hilbert was a foundational presence at the University of Göttingen, where he taught, supervised, and collaborated with many of the most influential physicists of the day. While Max Born and James Franck headed up their respective leading theoretical and experimental physics groups, Hilbert had established a world renowned institute of mathematical physics. In addition, at his famous 1900 address in Paris, he had been transformational in setting the agenda for future research pursuits in the field of mathematics. At the second International Congress of Mathematics held in Paris, Hilbert had laid out what he believed were the most pressing mathematical problems to be tackled in the 20th century. From this list of twenty-three unsolved problems from pure and applied mathematics, number six was particularly important to our analysis here, the "Mathematical Treatment of the Axioms of Physics." In particular in problem number six Hilbert sought to:

> …treat those physical disciplines axiomatically, according to the model of geometry, in which already today mathematics plays a prominent role: these are in the first line the calculus of probability and mechanics.[367]

For the next three decades Hilbert and his colleagues attempted to axiomatize a wide-ranging spectrum of physical theories including mechanics, electrodynamics, relativity theory, thermodynamics, statistical mechanics, and quantum theory with varying degrees of success. In most historical narratives that deal with quantum theory, the establishment of the quantum orthodoxy is usually coupled to the axiomatization of the theory. When one thinks of an axiomatic process it tends to conjure up images of deductive logical rigidity. For example, if one establishes certain self-evident propositions, or axioms, then a process can be followed whereby statements of truth can be derived using deductive logic. Hilbert clearly believed that a rigorous axiomatic program, in which mathematics was formalized based on an agreed upon set of logical rules, was critical to the continued development of physics and that mathematicians should naturally employ this strategy in exploring the interplay between abstract thought and physical experience:

> …while the creative power of pure reason is at work, the outer world again comes into play, forces upon us new questions from actual experience, opens up new branches of mathematics, and while we seek to conquer these new fields of knowledge for the

[367] Transcript of David Hilbert, Lecture from the Paris conference in 1900: "Mathematical Problems," Published in the BULLETIN (New Series) OF THE AMERICAN MATHEMATICAL SOCIETY Volume 37, Number 4, (2000): 418.

> realm of pure thought, we often find the answers to old unsolved problems and thus at the same time advance most successfully the old theories. And it seems to me that the numerous and surprising analogies and that apparently prearranged harmony which the mathematician so often perceives in the questions, methods and ideas of the various branches of his science, have their origin in this ever-recurring interplay between thought and experience.[368]

While Hilbert expected mathematics, and in particular the axiomatic method, to take a "leading role in all science" thanks to its ability to "gain deeper insight into the essence of scientific thinking," not all physicists agreed with these assertions.[369] As a result, we see Born, early on in his tenure as a professor in Göttingen, struggling to recast Hilbert's axiomatic program in a light more agreeable to his fellow physicists:

> [Being] conscious of the infinite complexity he faces in every experiment [the physicist] refuses to consider any theory as final. Therefore – in the healthy feeling that dogmatism is the worst enemy of science – he abhors the word 'axiom' to which common use clings the sense of final truth. Yet the mathematician does not deal with the factual happenings, but with logical relations; and in Hilbert's terms the axiomatic treatment of a discipline does not signify the final assertion of certain axioms as eternal truths, but the methodological requirement: state your assumptions at the beginning of your considerations, stick to them and investigate whether these assumptions are not partially superfluous or even mutually inconsistent.[370]

It is interesting to note here that Born declares that for physicists, no theory is final and dogmatism is considered science's worst enemy! This certainly does not seem to fit with the typical characterization of quantum orthodoxy as a marginalizing force that arose in the 1930s. However, if one considers that the rise of quantum orthodoxy was not simply due to a

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[368] Ibid., 409.
[369] Stöltzner, "Opportunistic Axiomatics – Von Neumann on the Methodology of Mathematical Physics," 38.
[370] Ibid., 39.

Machiavellian conspiracy but was instead a result of a superposition of many contributing factors as we saw earlier in this chapter, then Born's comments here begin to make more sense.

Von Neumann's early axiomatic work on set theory which he turned into his doctoral dissertation and his first series of papers on quantum theory, published in 1927, which Born and Heisenberg referred to during the Fifth Solvay Council, followed Hilbert's axiomatic strategy to a tee. In fact, Hilbert had developed the notion of a Hilbert space, which von Neumann applied extensively, explicitly to ensure the consistent and rigorous axiomatization of quantum theory. 1930 was a transformative year for Hilbert's general axiomatization program and in particular for von Neumann's application of this methodology to the further development of quantum theory.

Von Neumann had spent the two years after his stay at Göttingen working as a *privatdozent* at the Universities of Berlin and Hamburg but by 1930 he was busy transitioning to a more permanent appointment at Princeton University.[371] On the other hand, Hilbert was beginning to recede from academic life as he retired from active professorial duties at the University of Göttingen. At the famous second Conference on Epistemology of the Exact Sciences that took place in Königsberg in September 1930, there were two presentations that seemed to mark out a problematic dialectical line. Von Neumann, by now very comfortable in his role as a logician and a strong advocate of Hilbert's formalizing program, was invited as a central protagonist and gave one of the main addresses at the conference. His lecture "On the axiomatic grounding of mathematics" was a loyal presentation of the Hilbert research program.

[371] At first he was hired as a lecturer by Princeton University but in 1933 he became a founding member of the prestigious and independent Institute of Advanced Study (IAS) established in Princeton.

Meanwhile, Kurt Gödel took an opposing view in challenging the very underpinnings of the notions of rigor and completeness espoused in the formalist approach.[372]

It was in the discussion session of the conference that Gödel announced the first version of what became known as Gödel's first incompleteness theorem. While many did not immediately understand the significance of Gödel's argument, von Neumann quickly grasped its transformative potential for the axiomatic foundation of mathematics and pressed Gödel for further details. After Gödel published his first and second incompleteness theorems in 1931, von Neumann understood that the Hilbert program had been irrecoverably compromised. In a letter to Rudolf Carnap on June 7, 1931 von Neumann unequivocally stated as much:

> Thus I am today of the opinion that
>
> 1. Gödel has shown the unrealizability of Hilbert's program.
>
> 2. There is no more reason to reject intuitionism (if one disregards the aesthetic issue, which in practice will also for me be the decisive factor). Therefore I consider the state of the foundational discussion in Königsberg to be outdated, for Gödel's fundamental discoveries have brought the question to a completely different level.[373]

While von Neumann was forever altered by "Gödel's fundamental discoveries," the transition was not instantaneous. In 1931 he was already producing what became a seminal text for those studying the mathematical foundations of quantum theory *Mathematische*

[372] Thomas Breuer, "Von Neumann, Gödel and Quantum Incompleteness," published in M. Rédei and M. Stöltzner (eds.) *John von Neumann and the Foundations of Quantum Physics.* Dordrecht: Kluwer Academic Publishers, 2001, 75 - 82.
[373] Miklós Rédei, "John von Neumann 1903-1957," 6.

Grundlagen der Quantenmechanik.[374] In many respects, this 1932 landmark text represents the apex of the quantum formalism program in which quantum theory is fully axiomatized and set out as a "complete" theory. In chapter four of his seminal text von Neumann clearly lays out an argument for quantum theory's completeness by formalizing what became widely known and wielded as the impossibility proof for hidden variables theories. It seems that at some level this became a last-ditch effort for the great Hungarian mathematical physicist to realize Hilbert's axiomatic program, as his letter to Carnap shows von Neumann was clearly already struggling with the implications of Gödel's incompleteness theorems.

In the following discussion we will explore this impossibility proof in more detail because as we shall see it became a central foil in perpetuating the lull of alternate interpretations of quantum theory and thus became a point of contention within the development of Bohm's hidden variables program in 1951. However, before we move on to the actual proof and Bohm's response, it is important that we note von Neumann's subsequent ideological and methodological transitions which are indicative of a softening stance towards axiomatic rigor on his part. Interestingly enough, while von Neumann eventually somewhat softened his stance on mathematical rigor and the possibility of a full axiomatization of mathematical physics, there were some in the physics community that seemed to hold quite immobile to his earlier dogmatic declarations. In later writings on the foundations of mathematics von Neumann cautioned against absolutism and excessive rigor:

I think that it constitutes the best caution against taking the immovable rigor of mathematics too much for granted. This happened in our lifetime, and I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode, and how they changed three times in succession! … The variability of the concept of rigor shows that

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[374] Von Neumann, *Mathematical Foundations of Quantum Mechanics*

something else besides mathematical abstraction must enter into the makeup of mathematics.[375]

Similar to what Hilbert proclaimed in 1900, von Neumann declared that the “something else" needed besides mathematical abstraction was empirical science, physics in particular “... some of the best inspirations of modern mathematics (I believe, the best ones) clearly originated in the natural sciences.”[376]

Furthermore, the problems of the interface between mathematical physics and theoretical physics were not lost on von Neumann as he declared that the two pursuits mixed and complemented each other especially when evaluating new theories:

> I think that in theoretical physics the main emphasis is on the connection with experimental physics and the methodological processes which lead to new theories and new formulations whereas mathematical physics deals with the actual solution and mathematical execution of a theory which is assumed to be correct...[377]

It seems clear that the sometimes convoluted interface between mathematical physics and theoretical physics based in part on the assumption that the theory is correctly modeling empirical data was something that von Neumann was well aware.

In his 1932 seminal text on the mathematical foundations of quantum mechanics, von Neumann was trying to contribute to the formalization and axiomatization of the theory by building off the work that Dirac had done in writing his textbook two years earlier. Dirac was one of the more mathematically rigorous theoretical physicists of the period but even he was

[375] As Quoted in Stöltzner, “Opportunistic Axiomatics – Von Neumann on the Methodology of Mathematical Physics,” 35-62. Originally from von Neumann’s 1947 “The Mathematician”, *Collected Works vol.1,* 1-9.
[376] Ibid., 34.
[377] Ibid., 35.

reticent to overemphasize rigor in a quantum mechanics textbook. While he recognized that any presentation of quantum theory needed to be “essentially mathematical,” he asserted unequivocally that “mathematics is only a tool and one should learn to hold the physical ideas in one’s mind without reference to the mathematical form. In this book I have tried to keep the physics to the forefront…”[378] On the other hand, von Neumann’s goal was clearly to emphasize the importance of mathematics within the pursuit of physics by doing what Dirac and others were reticent to do, formalize quantum theory and attempt to complete Hilbert’s axiomatic program. To this end, the first three chapters of the book were essentially a recapitulation of the series of seminal papers he published in 1927 establishing the axiomatic foundations of quantum theory based on the explanatory framework of Hilbert spaces. Von Neumann then reserved the second part of his textbook, chapters four through six, for a thorough logical and deductive exploration of the two most stubborn problems confronting quantum theory, completeness and measurement. While he deals with completeness first, in chapter four, and then deals with measurement in the final two chapters, in our discussion we will reverse this order, after all, his discussion of completeness is the thread most relevant to our analysis.

As we saw in the previous section, Heisenberg, Darwin, and Mott had been keen to address the measurement problem through an analysis of alpha particle trajectories from a Wilson Cloud chamber in full accordance with Bohr’s ideas on the matter. Von Neumann followed suit and fell into line with Bohr and Heisenberg’s notions on this point. Using a description of observation or measurement as a transformation from a quantum mechanical pure state where only the observed system exists to a mixed quantum state characterized by the inevitable coupling between the observed system and the measuring apparatus. Von Neumann described this measurement transformation as a discontinuous and instantaneous

[378] P.A.M. Dirac, *The Principles of Quantum Mechanics*. (Oxford: Oxford University Press, 1982). viii.

reduction of the state, or wave packet, as a result of the awareness from a conscious observer. As a result, von Neumann concluded that it was impossible to formulate quantum mechanics without a reference to human consciousness, stating:

> …in the measurement we cannot observe the system *S* by itself, but must rather investigate the system *S* + *M*, in order to obtain (numerically) its interaction with the measuring apparatus *M*. The theory of the measurement is a statement concerning *S* + *M*, and should describe how the state of *S* is related to certain properties of the state of *M* (namely, the positions of a certain pointer, since the observer reads these). Moreover, it is rather arbitrary whether or not one includes the observer in *M*, and replaces the relation between the *S* state and the pointer position in *M* by the relations of this state and the classical changes in the observer's eye or even in his brain (ie., to that which he has 'seen' or 'perceived').[379]

After a discussion in which he defined the entropy of a quantum ensemble, and dealt with the irreversibility of macroscopic measurements as a result of the wavefunction's discontinuous transitions (or so called reductions), von Neumann attempted to resolve the issues of consistency in his measurement analysis. For the sake of consistency von Neumann addressed the possible interactions between the three elements of any observation: the quantum system defined by *S*, the measuring apparatus defined by M_1 and the observer defined by M_2. He subsequently went on to show that it makes absolutely no difference where we define the boundary between the observer and the observed. In other words, according to von Neumann, the analysis of $M_2 + M_1$ "observing" *S* is equivalent to the analysis where the boundary is defined by M_2 "observing" the composite system $S + M_1$.

The second fundamental problem that quantum theory faced in 1932 and to which von Neumann addressed himself in chapter four of his book was completeness. As we saw in our analysis of the 1927 Solvay Council this had been a serious point of contention throughout the

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[379] Von Neumann, *Mathematical Foundations of Quantum Mechanics*, 232.

meetings and five years later it remained unresolved. This last point should not be too surprising; we noted that while the vast majority of the physics community had abandoned outward interpretive debates by 1932, latent tensions certainly remained unresolved. In his preface, von Neumann foreshadows his later discussion on the problem of completeness by indicating that he will be developing a "no hidden variables theorem," as part of his presentation in chapter four of his text:

> There will be a detailed discussion of the problem as to whether it is possible to trace the statistical character of quantum mechanics to the ambiguity (ie. incompleteness) in our description of nature. Indeed, such an interpretation would be a natural concomitant of the general principle that each probability statement arises from the incompleteness of our knowledge. This explanation "by hidden parameters" … has been proposed more than once. However, it will appear that this can scarcely succeed in a satisfactory way, or more precisely, such an explanation is incompatible with certain qualitative fundamental postulates of quantum mechanics.[380]

After a deductive argument based on the axioms he had set in the first three chapters, von Neumann finally gets to his discussion of completeness, where he unequivocally concludes that all alternate interpretations of quantum theory involving 'hidden parameters' are untenable:

> …we need no go any further into the mechanism of the 'hidden parameters,' since we know that the established results of quantum mechanics can never be re-derived with their help. … The present system of quantum mechanics would have to be objectively false, in order that another description or the elementary processes than the statistical one may be possible.[381]

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[380] Ibid., ix-x.
[381] Ibid., 324-325.

In other words, the statistical formulations of quantum theory are unavoidable because it is a logically closed theory and cannot be altered or reformulated in any way to reestablish determinism by introducing new unobservable "hidden parameters."

In order to prove this theorem, von Neumann relied on the quantum axioms he had established in the first three chapters. First he addressed the notion of "dispersion-free ensembles," and concluded that as a general rule, quantum ensembles can never be dispersion-free. On the other hand he had proven the existence of pure or homogenous ensembles, by showing that any ensemble whose statistical operator is a projection operator is by definition a homogenous ensemble. The assertion that von Neumann made next was in many ways, the key to his argument, he assumed that all quantum states based on the notion of hidden parameters were by definition dispersion-free. As a last point, von Neumann concluded that the statistical nature of a homogenous ensemble cannot be removed by simply redefining the ensemble as composed of a heterogeneous mixture of substrates each associated with a unique set of corresponding hidden parameters, because this would conflict with the very notion of dispersion-free ensembles. In other words, because the ultimate goal of introducing hidden parameters into quantum theory is to remove its statistical nature by accounting for the entire spectrum of different unobserved micro states, these descriptions must be by definition dispersion free. However, one of the foundational axioms in von Neumann's argument is that no quantum ensembles can be dispersion free, which invalidates the very purpose of introducing hidden parameters.

In the years immediately following the publication of his book, this section on completeness seems to have been mostly overlooked by the vast majority of physicists. In fact, the general tenor of reaction from increasingly pragmatically oriented physicists to the extreme rigors of mathematics, as pursued in von Neumann's axiomatic pursuits, can readily be summarized by two quotes. First, in a later recollection Pauli described an interaction with von Neumann in which, after the Hungarian was done explaining the finer points of one of his proofs, Pauli told him that "if a mathematical proof is what matters in physics you would be a great physicist."[382] The second quote comes from the great American physicist Richard Feynman who wrote:

> The mathematical rigor of great precision is not very useful in physics. But one should not criticize the mathematicians on this score…They are doing their own job. … Some day, when physics is complete, and we know all the laws, we may be able to start with some axioms.[383]

5.6- EPR Paradox

With this general pragmatic, anti-rigorous, and anti-interpretational tenor dominating the physics community, three physicists published a paper that has left an indelible mark on all subsequent interpretation debates. In 1935, Albert Einstein, working as a colleague of von Neumann's from the newly created Institute for Advanced Study (IAS) in Princeton, NJ, collaborated with two American physicists Boris Podolsky and Nathan Rosen to write a paper which came to be known simply as EPR (short for Einstein-Podolsky-Rosen). In their landmark paper the three physicists made a seemingly last ditch effort to claim that the growing orthodoxy surrounding the Copenhagen Interpretation was not based on sound footing. As Einstein had done before, the title of the paper directly addressed the physicists' fundamental

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[382] Walter Thirring, "J. v. Neumann's influence in Mathematical Physics," in Rédei, M. and Stöltzner, M. (eds.) *John von Neumann and the Foundations of Quantum Physics.* (Dordrecht: Kluwer Academic Publishers. 2001), 5

[383] As quoted in Stöltzner "Opportunistic Axiomatics – Von Neumann on the Methodology of Mathematical Physics," 35.

question: “Can quantum-mechanical description of physical reality be considered complete?”[384] The three authors proposed a definition of a complete theory, as one in which every element of “physical reality” has a counterpart in the theory. They proceeded to make the critical and polemic claim that: “If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.”[385]

So according to the EPR paper “reality” was associated with the ability to exactly determine a dynamical physical quantity of a system and a physical theory was considered complete only when the determined quantity has a numerical counterpart. The authors proposed a *gedanken* experiment in which two partial systems PI and P2 had interacted initially for a set time (t = 0 to t = T) and then for t > T had become spatially distinct from each other. They then showed that due to the widely accepted quantum mechanical interpretation, these two spatially separate systems were in fact correlated so that one could instantaneously deduce a dynamical quantity (such as position and momentum) corresponding to the physical reality of P2 by measuring P1. In this case, the act of measurement of a physical property of P1 would dictate to P2 what value its correlated parameter must be found in. Einstein, Podolsky, and Rosen considered this situation problematic for two key reasons. First, if confirmed it contradicted Einstein’s long established relativity postulate that no information signal can travel faster than the speed of light (v = c). If these two physically separated systems were somehow communicating with each other instantaneously, after a measurement, they were certainly violating this principle.

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[384] A. Einstein, B. Podolsky, and N. Rosen. 1935. “Can quantum-mechanical description of physical reality be considered complete?” *Physical Review 47*, 777-780.
[385] Ibid., 777.

If one accepted the initial assumptions by the authors, the other implication of this seeming paradoxical *gedanken* experiment, eventually known as the EPR paradox, was that it showed quantum mechanics to be incomplete. EPR concluded that it was only possible to "know" the exact physical quantity of system P2 because the single wave function ψ_{1+2} governing the dynamics of the composite system P1+P2 could at any one time only specify a single eigenvalue λ (corresponding to a physically measurable quantity) for the composite system. If both partial systems are considered epistemologically "independent" then only having one numerical quantity in your theory that corresponds to two actually independent physical quantities would imply incompleteness.

By this reasoning, according to EPR, it was clear that quantum mechanics could not be considered complete. Of course, according to the "accepted" interpretations of measurement espoused, among others, by Heisenberg, Bohr, and von Neumann, the very premise of an exact quantum measurement without "disturbing a system" was completely impossible. So the very epistemological foundations of the EPR argument were challenged on these grounds. Furthermore, just because the systems were spatially separated it did not imply that they were somehow epistemologically independent. In this very basic sense, the Copenhagen Interpretation's response was again elevating the quantum mechanical formalism above the commonly accepted notions of physical intuition.

After the publication of the EPR paper in 1935 the physics community waited for a response from Copenhagen. As we noted earlier, with Bohr's development of complementarity, Copenhagen had become the natural focal point for all matters regarding quantum interpretation. While physicists looked to apply the quantum formalism in its various equivalent incarnations to a slew of problems, when it came to the more philosophical aspects of interpretation, deference was paid to Bohr and the Copenhagen school. In addition, with the rise of the Nazi regime in 1933 and the subsequent disintegration of the Göttingen quantum school any influence Born's institute may have had in helping to shape the interpretation dialog was lost.

Inasmuch as it is true that most physicists assumed Bohr would respond to the EPR challenge and somehow find a way to rationalize their proposed paradox within the confines of his complementarity umbrella, some quantum physicists certainly discussed the issue in their correspondence. In the following discussion we will examine Bohr's famous response and then Heisenberg and Schrödinger's lesser known responses to the EPR proposed paradox, which while not published came in the form of correspondence between them and others.

Bohr's response to the EPR paper came several months after the original paper was published and although not entirely intelligible seems to have placated the physics community and set the pragmatists at ease. In his response paper Bohr concluded that:

> The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails - because of the impossibility of controlling the reaction of the object on the measuring instruments, if these are to serve their purpose - the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality. In fact, as we shall see, a criterion of reality like that proposed by the named authors contains - however cautious its formulation may appear - an essential ambiguity when it is applied to the actual problems with which we are here concerned.[386]

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[386] N. Bohr, "Can quantum mechanical description of physical reality be considered complete?" *Physical Review 48*, (1935): 696-702.

The ambiguity that Bohr was referring to in his response to the EPR paper was the condition that the three authors had referred to as primary to their paradox; a measurement "without in any way disturbing a system." In other words, for Bohr there was no question that the measurement of one twin brought about a physical disturbance in its correlated twin.

During the summer of 1935 after the EPR paper had been published, Pauli wrote to Heisenberg asking him to write a response to the proposed paradox and thus defend the accepted quantum interpretation coined by Heisenberg as the "Copenhagen spirit" seven years earlier. In response, Heisenberg wrote to Pauli: "Since [Bohr's] reply is very different from the thoughts that one might express in connection with the 'cut', I am actually still eager to write something about the cut in line with your suggestion."[387] In his letter to Pauli, Heisenberg included a draft of an article which he never actually published. In this draft, Heisenberg uses an argument he refers to as "the cut" to refute the EPR paradox and disallow any possibility of a future "completion" of quantum theory by use of a hidden variables theory.

Heisenberg's "cut" argument is based on two principle points. The first is similar to the one that was proposed by von Neumann in his 1932 text. The idea is that the cut, or delineation, between an observer and the system being observed is almost entirely arbitrary in quantum theory leading to equivalent results regardless of the placement of this cut. Heisenberg showed that whether the cut is placed at the measurement apparatus or in the conscious mind of the observer, it makes no difference in the actual application of the quantum theory formulation. The second central claim in Heisenberg's cut argument is that the statistical nature of quantum theory comes about exactly at this cut point, and nowhere else. In other words, there are both classical and quantum sides to the division and the cut itself is associated

[387] Guido Bacciagaluppi and Elise Crull, "Heisenberg (and Schrödinger, and Pauli) on Hidden Variables," *Studies in History and Philosophy of Modern Physics,* 40 (4), (2009): 374–382.

with the instantaneous, unobservable, and unanlyzable transition between the two domains. Based on his cut argument, Heisenberg believed that quantum theory was as complete as it could possibly be and that any alteration to the theory that included a deterministic approach to the cut would actually destroy many of the physically observable and experimentally verified quantum effects, such as particle interference. It is interesting that Heisenberg chose not to publish his response to the EPR and his argument for completeness in 1935 and instead deferred to Bohr's famous response. Was this indicative of the conscious alignment, even when full agreement was absent, that the proponents of the Copenhagen spirit entered into?

Schrödinger's response to the EPR paper is important because it shows that even though he was not in full agreement with Einstein as to his objections to the accepted interpretation being espoused by Copenhagen, Einstein's defiance in his challenge sparked Schrödinger to formulate his own critique. "I am very pleased that…you have publicly caught the dogmatic quantum mechanics napping over these things that we used to discuss so much in Berlin."[388] As we have previously seen Schrödinger was in line with the orthodox interpretation with regards to the inherent nature of indeterminism in quantum theory. As a result, he did not argue for completeness but did consider that the most problematic aspect of this accepted interpretation was the unanalyzable discontinuity brought about by the measurement problem.

In a June 17th letter to Schrödinger, Einstein explained his position on indeterminism and completeness in quantum theory and apologized for seeming to be an old reactionary:

> From the point of view of principles, I absolutely do not believe in a statistical basis for physics in the sense of quantum mechanics, despite the singular success of the formalism of which I am well aware. I do not believe that such a theory can be made general relativistic. Aside from that, I consider the renunciation of a spatio-temporal

[388] Arthur Fine, *The Shaky Game: Einstein Realism and the Quantum Theory,* (Chicago: The University of Chicago Press, 1986), 66.

> setting for real events to be idealistic-spiritualistic. This epistemologically-soaked orgy ought to come to an end. No doubt, however, you smile at me and think that, after all, many a young heretic turns into an old fanatic, and many a young revolutionary becomes an old reactionary.[389]

Throughout the summer of 1935, while waiting for Bohr's published response to the EPR paper Einstein and Schrödinger engaged in an extensive correspondence in which Schrödinger pointed to a more general conceptualization of the EPR problem and for the first time formulated EPR's correlated pairs as more generalized "entangled" states. In addition, Einstein informed Schrödinger that he had not been particularly active in the writing of the EPR paper which was mostly carried out by Podolsky and as a result the clarity of the argument had suffered. In his clarifying explanation he described a simplified *gedanken* experiment, without position and momentum correlations, which served to illustrate the EPR paradox and his main objection to the accepted quantum interpretation. As Einstein saw it:

> …the [EPR] paradox forces us to relinquish one of the following two assertions:
>
> (1) the description by means of the ψ–function is *complete*
> (2) the real states of spatially separated objects are independent of each other.[390]

Einstein's new simplified *gedanken* experiment was based on a system with one ball and two boxes. For Einstein the best scenario was that probability of the ball being observed in one of the boxes is equal to one and the probability of it being observed in each is 1/2. However, this assumed a reality that the ball was actually in one of the two boxes before opening either. If that were the case, then by a simple law of conservation the result of the second measurement could be exactly deduced. For Einstein the incompleteness of quantum

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[389] Ibid., 68.
[390] Ibid., 37.

mechanics comes from the fact that while this ball in a box scenario makes sense macroscopically, one cannot draw the exact parallel of this scenario on the quantum level.

As the two physicists exchanged insights into the implications of these *gedanken* experiments, Schrödinger began to realize that what was most troubling to him about the orthodox interpretation, more so than incompleteness, was the unanalyzable act of the measurement process. As a result, by the end of the summer he had developed, with Einstein's help, his own paradoxical *gedanken* experiment immortalized as Schrödinger's cat.[391]

Completeness was a problem long before von Neumann wrote his text on the mathematical foundations of quantum theory, and it remained a problem long after. In fact, his "no hidden parameters" theorem was initially mostly ignored by physicists and mathematicians alike. During the 1930s and 1940s, as we have seen, the vast majority of the physics community steered ever farther from interpretation debates and more into a state of dogmatic pragmatism. As Born noted in 1948 physicists were no longer engaged with the more philosophical subtleties of their analysis and as such, it should come as no surprise when they began wielding von Neumann's theorem as an indisputable "impossibility proof" against all efforts to complete quantum theory by introducing hidden variables.

[391] Ibid., 82-83.

As John Bell later observed, David Bohm became the first physicist to challenge what had come to be called von Neumann's impossibility proof in his two polemical hidden variable papers of 1952. It had taken twenty years for somebody to really explore beyond the veil of impossibility that had evolved with respect to possible solutions for quantum theory's innate indeterminism. As we shall see in the following section, Bohm made key research decisions during a one year period that left an indelible mark both on the study of the foundations of quantum theory and on his later career in academia.

Chapter 6: Climbing Mt Impossible- David Bohm and the Reemergence of Deterministic Quantum Theory

But in 1952 I saw the impossible done. It was in papers by David Bohm. Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessary reference to the 'observer,' could be eliminated...But why then had Born not told me of [de Broglie's] 'pilot wave'? If only to point out what was wrong with it? Why did von Neumann not consider it? More extraordinarily, why did people go on producing ''impossibility'' proofs, after 1952, and as recently as 1978? ... Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show us that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice?[392] *- J.S. Bell, 1987*

6.1- Introduction

December 4, 1950 was a touchstone moment in two distinct narratives that became, from that point on, forever entangled. On that day David Bohm was arrested in Princeton, New Jersey by a federal marshal and subsequently taken to Trenton, New Jersey where eight federal charges of contempt of Congress were read as part of his indictment. As a result, Princeton University's President Harold W. Dodds placed Bohm on indefinite paid leave and forbade him to teach students or visit campus until the trial was over. This controversial episode and subsequent isolation eventually lead Bohm away from a promising career as a leading plasma physicist and into academic exile.

In addition, this period of turbulence, isolation, and uncertainty coincided with an intellectual shift for Bohm, as he began to distance himself from a budding plasma research

[392] John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, (Cambridge: Cambridge University Press, 1987), 160.

program, preferring to focus on more speculative matters regarding the interpretation of quantum theory. As such, he first struggled extensively to comprehend the physical principles behind the prevailing interpretation and then, after gaining sufficient mastery over them, quickly developed a hidden variables approach that boldly challenged the established quantum orthodoxy. As John Bell would later recognize, Bohm's hidden variables program eventually served to reopen long abandoned interpretive debates within the quantum physics community.[393]

On May 31, 1951 in Washington, D.C. Federal Judge Alexander Holtzoff acquitted David Bohm of all charges of contempt of Congress citing that Bohm was within his rights in refusing to answer questions asked of him during hearings held by the U.S. House of Representatives Committee on Un-American Activities (HCUA)[394] in May and June of 1949.[395] Unfortunately, the damage to his once promising academic career had been done. For two years a cloud of suspicion had hung over Bohm and, although he was eventually acquitted of all charges of contempt of Congress, he was never able to escape the repercussions they had on his fledgling career. Someone who had been considered a critical part of the Manhattan Project and "[o]ne of the ablest young theoretical physicists that Oppenheimer ha[d] turned out"[396] found himself unemployed and unemployable. Even Albert Einstein's recommendations of Bohm were unable to overcome the stigma the HCUA hearings had indelibly affixed on him. Bohm's academic exile from the United States was permanent and, although he was able to find work in universities

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393 See quote at the beginning of this chapter.

394 The HCUA (House Committee on Un-American Activities) is also commonly known as HUAC but technically HCUA is the correct acronym.

395 From "Professor Bohm Gets Acquitted Of All Charges" Daily Princetonian, 4 June 1951. Also, Russell Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," *Isis*, 90, no. 4 (December, 1999): 745-746.

396 David Joseph Bohm, FBI FOIA File # 100-207045, 13 & 24.

outside the U.S., he was forced into a peripatetic life for years before finally settling down in London, England.[397]

Since Bohm's death in 1992, scholars such as Forstner, Freire, Kojevnikov, Olwell, Peat, and Sharpe have published biographical sketches of Bohm using different theoretical frameworks and interpreting his life from various perspectives.[398] Using psychological, historical, political, and sociological frameworks, these scholars have built narratives that differ in their scope, facts, and conclusions. While they may not agree on all aspects of Bohm's life such as the extent to which his Marxist ideologies influenced the development of his hidden variables theory, whether he was truly isolated while working in Sao Paulo, Brazil, or how important his apparent psychological instabilities were in defining his career, there are two key facts that seem to be well established in all narratives of David Bohm:

- Bohm was a promising young American physicist who became a 'marginalized' figure in U.S. academic circles and was forced to seek an academic post outside his native country.
- He developed a hidden variables theory, an alternate formulation of quantum theory that was itself 'marginalized' by the vast majority of the international physics community.

These two seemingly overlapping ideas seem to be critical to any narrative of David Bohm, however, the full context that surrounds them and the extent of the coupling between

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[397] Bohm did briefly visit the US on several poignant occasions such as the Death of his father in the early 1970s.

[398] In constructing this narrative I will be using a combination of primary sources such as David Joseph Bohm's FBI FOIA File # 100-207045, transcripts from Oral History Interviews (Center for History of Physics, American Institute of Physics AIP), and Newspaper Articles as well as secondary sources like the biographical studies by Forstner, Freire, Kojevnikov, Olwell, Peat, and Sharpe. These biographical sketches as a whole do not tell the full story of Bohm's marginalization and certainly do not focus on its interaction with the narrative of hidden variables.

them have not been adequately addressed in the literature. As we noted in our discussion on de Broglie's alternate wave mechanical program in chapters three and four the use of the term 'marginality' without a proper contextual analysis is highly problematic for multiple reasons among them, an unjustified demonization of the establishment allegedly doing the marginalizing, a usurping of the 'marginalized' individual's agency, and an unnecessary and ultimately distortive and simplistic reduction of the complex contexts that surround our subjects of study. While an exhaustive examination of Bohm's life may never yield a full picture of the connections between the various factors that led to his political persecution, professional derailment, and his simultaneous development of hidden variables theory it is an exploration that will certainly help us achieve a richer understanding of his context of innovation.

To that end, the following chapter will explore the coupling point between the two distinct narratives, a biography of David Bohm and a reevaluation of the development of his hidden variables theory in an attempt to elucidate a more nuanced understanding of the contextual dynamics surrounding the reemergence of debates on alternate quantum interpretations in the early 1950s. Furthermore, it was critical for us to first establish the context surrounding the emergence of these debates in the mid to late 1920s and then the significant lull that they experienced in the subsequent two decades because as Bohm reopened this line of inquiry in the early 1950s the wide spectrum of reactions from the physics community reflected a superposition of both Bohm's immediate and contemporary context and the long-range effects of the residual and unresolved interpretive debates from the earlier quantum revolution.

As we saw at the end of the last chapter, the quantum interpretation debate was abandoned rather abruptly in the late 1920s and early 1930s by the vast majority of physicists giving way to a period dominated by pragmatism and focused on the applications of the various equivalent quantum-mechanical formulations. Due to the abruptness of this transition within the international physics community much of the residual tension remained latent and unresolved throughout the twenty year period leading up to Bohm's work on hidden variables. Although a product of this pragmatic physics culture, David Bohm was never quite comfortable approaching physics from the purely pragmatist perspective and preferred a more contemplative and mathematically less rigorous approach. As we shall see, ultimately, it was his non-conformist approach within the prevailing scientific culture, in addition to certain key contextual factors outside of his control that led Bohm to blaze his own research path.

6.2- Bohm's Youth and Schooling

David Bohm was born in the mining town of Wilkes-Barre, Pennsylvania in 1917 to Samuel and Frieda Bohm. Samuel was a Hungarian Jew who had immigrated to the United States in 1913 and had found his vocation in furniture sales. Frieda was born in Russia but had lived in the United States since she was six years old.[399] In 1928, at the age of ten one of his father's employees left an issue of *Amazing Stories* and as David read the story entitled "Skylark of Space" he felt a moment of epiphany in his life. Even in later life he pointed to this as a critical moment in his development. Bohm's science fiction reading soon became insatiable. As he became increasingly fascinated by the science fiction genre he began to embellish on the

[399] DJB FBI File. Actually there is a discrepancy in the FBI file. It appears that the FBI investigation revealed that Samuel was born in Hungary and Frieda was born in Russia [p.194] but the CIC (Counter-Intelligence Corps) investigation (contained within the FBI file) has Samuel born in Czechoslovakia and Frieda born in Poland [p.111]. It is not clear if this discrepancy is due to Bohm himself. In the FBI files this information is derived from birth certificates and a passport application. The CIC source comes from a personnel security questionnaire filed by Bohm at the Berkeley Radiation Laboratory. Was Bohm worried about perceptions of his connections to Russia? Following WWI the small town of Munkacs, where Samuel was born, became part of Czechoslovakia so that discrepancy can be accounted for. However, Frieda was born in Russia and her birthplace later became Lithuania, not Poland.

stories he was reading and developed extensive and intricate fantasies in which he would travel to distant planets and meet new civilizations. In his fantasy worlds these civilizations were usually more scientifically and morally advanced than our own. It was Bohm's rich science fiction fantasy world that fed his passion for science as an academic pursuit. "[Bohm's] desire to go beyond the security of the familiar remained with him throughout his professional life. He always worked at the edge, accepted nothing at face value, sought out the unknown rather than the known, and let himself be guided by intuition and imagination rather than logic and empiricism."[400] Once the passion for science had been lit inside Bohm it consumed him and he made it his life's work to pursue a better understanding of the natural world.[401]

Bohm left Wilkes-Barre for Pennsylvania State University in the fall of 1935. Upon his arrival there, he quickly realized that there was very little serious research being done in physics and that most of the faculty and students preferred to work on problems related to applied sciences and engineering. Unbeknownst to Bohm this was a systemic problem in the United States, manifested by the fact that physics, as a discipline, had been grossly underdeveloped at American Universities until the 1920's.

In 1935 Penn State was not an elite university and thus it remained a relative backwater within the physics world. Fortunately for Bohm, he was not overly concerned with this as he preferred to be the big fish in a small pond rather than a small fish in a big pond. While at Penn State he always associated with the top physics students and had direct access to the best professors, thus allowing him unusually broad flexibility in creating his own independent

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[400] Ibid., 11.
[401] This seems to be a very common story in the lives of many physicists. See Michio Kaku, *Physics of the Impossible,* (New York: Knopf Doubleday Publishing Group, 2009), Forward. Also, in Arthur C. Clarke, *Profiles of the Future*, (London: Indigo, 2000), xiii. He states: "I do not for a moment suggest that more than 1 per cent of science fiction readers would be reliable prophets; but I do suggest that almost 100 per cent of reliable prophets will be science fiction readers - or writers."

curriculum.[402] "In his senior year at Penn State, Bohm devoted less time to formal coursework, preferring to spend most of his days walking [in the woods and hills surrounding the campus], thinking, and reading."[403] As he came to the end of his undergraduate experience, it became clear to Bohm that his vocation would be to become a professor of physics. In order to accomplish this he understood that he must go on to graduate school at an elite university.

Bohm applied to many graduate schools and was surprised to find that most rejected him. He could not understand why he was being rejected because his grades had been stellar and he thought he had strong recommendations from his faculty. Bohm later learned that the chair of his department had given him a strong letter of recommendation but had explicitly mentioned that Bohm had an "average personality" and that he was of Jewish heritage.[404] In the climate of 1939, the lone fact of his Jewish heritage may have been enough reason for many graduate schools to reject him.[405] Be that as it may, Bohm was accepted by two schools: the University of Rochester and the California Institute of Technology in Pasadena, California. With his sights set on the west coast, he headed for California. He was expecting an idyllic environment where he could finally reach his potential and flourish. Unfortunately, his stay in Pasadena was somewhat less than idyllic. Bohm was mostly put off by the pedantic academic environment which he encountered.

From Bohm's perspective the Physics Department at Caltech was too focused on teaching problem solving through rote calculation and insufferable and uninteresting problem

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402 David Bohm, AIP Interview with L. Hoddison on May 8th, 1981. (http://www.aip.org/history/ohilist/4513.html)

403 F. David Peat, *Infinite Potential: The Life And Times Of David Bohm,* (New York: Basic Books, 1997), 32.

404 DJB FBI File II: San Francisco, CA file number 100-17787, 2 [pdf: p.61]

405 The great physicist Richard Feynman was also a victim of this so called "Jewish quota"in the 1930s when he was not accepted at Columbia University and instead ended up at MIT. This story is told in: Jagdish Mehra and Kimball A. Milton, *Climbing the Mountain: The Scientific Biography of Julian Schwinger,* (Oxford University Press. 2003), 218.

sets that only seemed to breed competition between students. This rigid academic atmosphere left Bohm yearning for the independent freedom of his Penn State curriculum. He was much more interested in furthering and deepening his understanding and intuition of physics as a whole, including its assumptions and underlying conditions, than he was in besting his classmates in the latest problem set. Bohm believed that the practice of rote problem solving was a conformist approach to learning physics.

Even though he was unhappy at Caltech, Bohm initially showed signs of brilliance in his work. His roommate and friend Leon Katz tells of how they would work on problem sets together and Bohm would usually finish the whole set within an hour leaving him the rest of the evening to delve independently, and deeply, into studying subjects he was genuinely interested in. Even though he showed signs of being capable of solving problems via rote calculation, Bohm preferred an intuitively arrived at solution to a problem over a systematic mathematical solution. This became a trademark of Bohm's later scientific pursuits. As a physicist, he was always concerned with the larger picture that a problem presented and usually dismissed the details of calculations as tedious and mundane work.[406]

In the midst of his second year at Caltech Bohm's unhappiness with the program began to affect his general performance in courses, research, and as a teaching assistant. "[Bohm] appeared to grow more and more introverted and finally got to the point where he could not speak to others and walked with his head down in going from one classroom to another." In June 1941, at the end of his second academic year at Caltech, Bohm was told that due to his demeanor he would not be retained as a teaching assistant for the following academic year.[407] A friend, worried that Bohm may slip deeper into a depression, encouraged him to contact a

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[406] Peat, *Infinite Potential,* 34-36.
[407] DJB FBI File, 3.

young professor at the University of California at Berkeley, J. Robert Oppenheimer, who might resonate more with Bohm's intellectual style. Oppenheimer, recognizing Bohm's potential, immediately offered him a position in his theoretical physics group. The move to Berkeley in 1941 revived Bohm completely. He had finally arrived at his imagined Californian paradise. The forests of giant redwoods and the northern California climate were exactly what he needed for his long hikes and his physical well being. More importantly, the open atmosphere in Oppenheimer's group was exactly what Bohm needed to thrive intellectually.[408]

6.3- Graduate School at the University of California at Berkeley

Oppenheimer was a true renaissance man and was equally comfortable talking about politics, eastern philosophy, religion, art, or quantum physics. He had studied in Europe, in the mid 1920's, under Rutherford, Bohr, and Born, and alongside Heisenberg and Pauli as the new quantum theory was being hatched. He was a rising star in American physics so it is no wonder that Bohm immediately felt a resonance with Oppenheimer and settled comfortably into his new environment. Many of Oppenheimer's students reflected his eclectic interests and Bohm found that he could oscillate seamlessly between discussions of politics and physics with his colleagues. Unfortunately, this idyllic environment did not last past his first year at Berkeley. Bohm felt that after early successes while working on interesting research problems, Oppenheimer had withdrawn his positive reinforcement and constructive feedback. Bohm later recalled that this must have been a pattern with Oppenheimer, to shower the newer students

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[408] Peat, *Infinite Potential,* 39.

with early encouragement only to have this attention wane as the students progressed with their studies.[409][410]

Over his initial two year period at Berkeley, from 1941 to 1943, Bohm's outlets for his frustrations at school were primarily his long hikes into the Bay Area's beautiful natural surroundings, his long late-night contentious debates on the foundations of physics with fellow graduate students, and his exploration of leftist political philosophies.[411]

In our analysis, these last two outlets are of particular interest because they were certainly harbingers of what would come to define Bohm's life a decade later. While Bohm had already been exposed to quantum mechanics in his coursework at Caltech, upon his arrival at Berkeley, he seemed to find it difficult to understand, at times arbitrary in its application, and seemingly counter-intuitive. One of Bohm's colleagues and close friends in Oppenheimer's group was Joe Weinberg who recalled his friend's early aversions to quantum theory often referring to Bohm as "a convinced classicist."[412] However, as Weinberg later recalled, after sitting in on Oppenheimer's quantum theory lectures and caustically debating the relative merits and implications of the classical and quantum frameworks in late night philosophical battles, the young Bohm finally began to realize and accept the merits of Bohr's principle of complementarity and the inevitability of the Copenhagen spirit.[413]

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[409] Peat, *Infinite Potential,* 47-48.
[410] In his defense, Oppenheimer's apparent withdrawal from Bohm happens to coincide with ramping up his increasingly absorbing work on the Manhattan Engineer District (MED) in the summer and fall of 1942. Whether the withdrawal was just Bohm's interpretation of extenuating circumstances or if this really was Oppenheimer's MO is not clear. The important point is that Bohm felt this way, which may have influenced how he acted during 1942-1943.
[411] Peat, *Infinite Potential,* 43-44 & 49.
[412] Ibid., 50.
[413] Ibid., 51.

This transition for Bohm from classically grounded physicist to one that begrudgingly accepted the implications of the Copenhagen spirit was reinforced when, during the war, he and Weinberg were called upon to substitute Oppenheimer, who was by then working at Los Alamos, as lecturers in his quantum theory courses. Armed with Oppenheimer's richly annotated quantum theory lecture notes it was like seeing directly into the insights of Pauli, Heisenberg, Born, and Bohr. The opportunity to lecture on the subject gave both aspiring academics an opportunity to develop a deeper appreciation for the foundations of the subject. It was in this unpacking of Oppenheimer's notes and the continuous struggle to deliver them in a comprehensible way to the students he was lecturing, that Bohm was really able to begin to wrap his mind around the finer nuances and the fuller implications of the Copenhagen spirit. As we shall see, the practice of exploring these interpretational and foundational aspects of quantum theory would remain a central theme throughout Bohm's academic career.[414]

The other important outlet mentioned above critical to our analysis, was Bohm's explorations into leftist politics. In the fall of 1941, as Bohm began to incorporate himself into Oppenheimer's group, the milieu on the Berkeley campus encouraged open dialog and left-wing political associations. Representatives of the Socialist Party, Communist Party, and regional trade unions addressed public gatherings and regularly planned activities in conjunction with student organizations. With the U.S. in reclusive isolationism and only two years removed from the bloody defeat of the Spanish republic by the fascist military Falangists led by Francisco Franco, much of the dialog centered around the rise of fascism in Europe, and the only force that seemed ready to confront it. For Bohm, and many young intellectuals of the time, the Soviet Union seemed to be the only barrier standing between fascism and its spread around the

[414] Ibid., 64.

world. It is no surprise that many students began embracing socialism and communism as a direct result of this environment.[415]

Specifically, in the late 1930s and early 1940s within Oppenheimer's research group there was a clear tendency towards leftist philosophies. Although Oppenheimer himself was a self proclaimed "fellow traveler", to this day there is considerable debate about whether Oppenheimer was ever, actually, a Communist Party member. There is no debate, however, about his associations with confirmed communists, as his brother Frank was a self proclaimed Communist Party member and J. Robert's wife Katherine, had been an avowed Party member. Katherine's second husband had been Joe Dallet a prominent Communist Party leader out of Youngstown, OH who had died in 1937 while fighting for the Spanish Republic as part of the Abraham Lincoln Battalion. While Oppenheimer claimed that being a "fellow traveler" meant that he was sympathetic to the philosophies of the Party and its members he always maintained that he had never actually joined the Communist Party itself.[416]

As for Bohm, he clearly had a personal stake in the ramifications of Nazi aggression in Europe and so had justifiable reasons to be politically active on campus. It seems, however, that up until the Spring of 1943, he attempted to remain on the political sidelines and kept his interests in communism at an abstract intellectual level. Through discussion groups, Bohm began refining his political philosophy, being influenced heavily, first, by Marxism and later, specifically, by dialectical materialism. He briefly joined the Communist Party from the winter of

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[415] Christian Forstner, "The Early History of David Bohm's Quantum Mechanics Through the Perspective of Ludwik Fleck's Thought Collectives," *Minerva*, 46 (2008), 218.

[416] For further background on Oppenheimer and his communist affiliations see the following: Kai Bird & Martin J. Sherwin's *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Knopf Publishing, 2005); Atomic Energy Commission: "In the Matter of Robert J. Oppenheimer: Transcript of Hearing before Personnel Security Board" Atomic Energy Commission, Washington D.C., May 27th – June 29th, 1954 (Cambridge, MA: MIT Press, 1970); and "Findings and Recommendations of the Personnel Security Board in the Matter of Dr. J. Robert Oppenheimer" (http://avalon.law.yale.edu/20th_century/opp01.asp).

1942 through the summer of 1943 and left because he claimed that the meetings were "interminable." Although Bohm later remembered his "experiences in the Communist Party as a small, almost insignificant, part of his time at Berkeley" this brief association would profoundly shape the rest of his life.[417]

Whether the Great Depression sparked Bohm to move towards Marxist philosophies or the move was due more to other factors like the impression that the Soviet Union was the only force willing to stand up to the spread of fascism we will never know with certitude. However, the fact remains that Bohm began a life-long love affair with Marxism and briefly experimented with political action. While working at the Berkeley Radiation Laboratory (Rad Lab) on his dissertation research on the scattering of protons and deuterons in the fall and winter of 1942 Bohm was fixated on monitoring the news coming in from the battle of Stalingrad. Around this time he became inspired to join the Communist Party and along with fellow graduate students Rossi Lomanitz, Joseph Weinberg and others tried to unionize the Rad Lab by opening a local chapter of FAECT (Federation of Architects. Engineers, Chemists, and Technicians.) [418]

In the summer of 1942, with the specter of a German atomic bomb a real possibility, the *Manhattan Engineer District* (eventually known as the Manhattan Project) was formed under the military directorship of Brigadier General Leslie Groves and the scientific leadership of J. Robert Oppenheimer. In June, Oppenheimer had convened some of the brightest minds in theoretical physics, including Hans Bethe and Edward Teller, in a conference at Berkeley to review their work on the theory of fission reactions. At this conference, among other things, two critical questions were addressed: the feasibility of a nuclear fission bomb and a general idea of what the bomb assembly schema may look like. The consensus was that a fission bomb

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[417] Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," 740.
[418] Peat, *Infinite Potential,* 58; as well as: Alexei Kojevnikov, "David Bohm and Collective Movements," *Historical Studies in the Physical and Biological Sciences*, 33:1, (2002): 165.

was, in fact, feasible and some initial sketches were drawn as to what the assembly scheme might look like.

While Oppenheimer and the majority of the group focused on the theoretical framework necessary for a fission bomb (neutron absorption by U-235 with the result being a fission of the unstable U-236 nucleus), Edward Teller introduced the possibility of a fusion bomb (fusion of two nuclei deuterium H-2 and tritium H-3.) In theory, this “Super” (later called the H-bomb) would be capable of much more destruction then the fission bomb. While neutron scattering leading to nuclear fission was well understood and there was clear consensus about the feasibility of a fission bomb, Teller’s fusion bomb schemes based on superheated plasmas were much less clear cut and thus were initially set aside. However, research would continue into the possibility of a fusion bomb and Bohm’s plasma research would eventually be critical to the success of this project.

In October of 1942 Bohm was given a research position at the Rad Lab so that he may study neutron deuteron scattering. As his dissertation research progressed he initially retained his position as a teaching assistant at Berkeley, but by February of 1943 Bohm had abandoned his teaching duties so that he could dedicate his full attention to MED research projects at the Rad Lab. By early April, Bohm’s research had produced such fruitful results that Oppenheimer classified much of his findings as “top secret” and awarded Bohm his Ph.D. without the customary thesis, nor dissertation defense. In fact, Bohm’s research was to become regarded as a critical cog in the general MED Project at Los Alamos.[419]

With his Ph.D. expedited by both Oppenheimer and Berkeley, Bohm agreed to leave for the Los Alamos Laboratory immediately. He was scheduled to leave for the New Mexico top

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[419] Peat, *Infinite Potential,* 13.

secret site on April 11th; unfortunately, on March 29th, 1943 a certain "Joe" working at the Rad Lab had allegedly transferred atomic secrets to Steve Nelson, a known Communist Party organizer, who had connections to the Rad Lab and specifically to Oppenheimer's wife, Katherine.[420] The MED Project administration had been worried about these connections and had maintained a close watch on Nelson using CIC agents for both physical and technical surveillance.[421] Within days of the alleged transfer the CIC opened what came to be known as the CINRAD (Communist Infiltration of the Radiation Laboratory at Berkeley) investigation and several candidates for "Joe" were immediately identified amongst the Rad Lab employees. David 'Joseph' Bohm, Giovanni Rossi Lomanitz, and Joseph Weinberg were all close friends, seemingly inseparable, and active in the unionizing efforts at the Rad Lab earlier that year, so, with a cursory analysis of the facts in the CINRAD case the CIC investigators counted the young physicists among the possible suspects.

Apparently, David Bohm and Rossi Lomanitz quickly became the prime targets of the CIC investigation. With the hopes of uncovering the identity of "Joe", also known as 'Scientist X', both physical and technical surveillance were ordered on all suspects. To help bolster their CINRAD investigation, the MED Project administration requested that the FBI furnish them with all the background they could obtain on the suspects. The FBI opened its investigations into the subjects immediately, and within days had a confidential informant that had led them to

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[420] Steve Nelson was a close friend and colleague of Katherine's second husband Joe Dallet. They had organized unemployment protests in Chicago in the early 1930s and had fought together for the Abraham Lincoln Battalion in the Spanish Civil War. According to the MED, Nelson used this connection to Oppenheimer and his wife to infiltrate the Rad Lab and help establish a 'communist cell'.

[421] In a document produced by the US Army Military History Institute entitled: *"Counter Intelligence Corps: History and Mission in World War II"* it states specifically that between 1942-43 CIC agents conducted "thousands" of "loyalty" investigations on people with access to classified materials. Furthermore, it states that in the course of the investigation agents would use "technical investigative equipment." [p. 13-14]. It is very likely that Nelson was under technical surveillance (wire) by the CIC and that is why they had some information like the name "Joe" and some biographical information but not a picture or a clear physical description.

important background biographical information. In order to jumpstart David Bohm's investigation the San Francisco FBI field division sent a teletype request on April 3rd to the Los Angeles Field Division of the FBI for further information on his tenure at Caltech from 1939 to 1941. On April 5th they sent a similar background check request to the FBI division offices in Philadelphia.[422]

An April 7th correspondence in the CIC papers in Bohm's FBI file indicates that Bohm was being shoehorned as the "most logical suspect" to be "Joe" by the CIC investigators.

> As of April 7, 1943 it had not yet been possible to determine the identity of the individual known as [blacked out] but every effort was being made to do so at the time in cooperation with the local G-2 offices, which had been engaged in a search of personnel files of project employees.
> The most logical suspect up until April 7, 1943, was not [blacked out] but one David Joseph Bohm, whose personal history, insofar as it had been determined at the time, fitted closely that of "Joe".[423]

On that same day, April 7th, the FBI was informed that the CIC investigation was indicating that Bohm was, in fact, "Joe". In a memorandum to Assistant Director of the FBI Mickey Ladd from [name blacked out] on April 12th:

> It will be recalled that [blacked out] in his conversation with the man named "Joe", now identified by G-2 as David Joseph Bohm, instructed the latter to tell the party members who had been transferred away from Berkeley, California, to destroy their Party books.[424]

In turn, Ladd sent a memo to the Director of the FBI, J. Edgar Hoover, on April 16th in which he indicated that due to the suspicion surrounding Bohm and his identity as the "Joe" accused of espionage, Oppenheimer's request for Bohm to be transferred to Los Alamos on

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[422] DJB FBI File, 1.
[423] Ibid., 99.
[424] Ibid., 101.

April 11th had been “cancelled”.[425] On May 12th an unidentified person at the Bureau wrote a letter, which was then referenced by someone (name blacked out) in a separate correspondence addressed to (name blocked out) at the G-2, War Department, in which they inform him that:

> At one time the Military Intelligence Service was of the opinion that “Joe” was David Joseph Bohm. However, there were various discrepancies between the facts known regarding Bohm and the identifying data concerning “Joe”. These related particularly to “Joe’s” marital status, the location and number of members of his family and the period of time during which he had lived in California.[426]

On May 14th the special investigator for the San Francisco Field Division that was heading up the Bohm background investigation submitted his report, and on May 22nd FBI Director Hoover sent Major General George V. Strong (Assistant Chief of Staff G-2, War Department) a copy of this report. Included with the report was a note from Hoover that read: “No further investigation regarding Bohm will be conducted by this Bureau in the absence of a specific request by the Military Intelligence Service.”[427]

Apparently, the CIC’s definition of two personal histories being “close fits” does not include similarity in marital status or in the number and gender of immediate family members. It seems that while the CIC was very thorough in its physical and technical surveillance capabilities they may be found wanting in their thoroughness of Bohm’s basic personal background check. Due to this lack of thoroughness by the CIC, Bohm remained the prime suspect of an espionage investigation for six weeks and was refused transfer to the heart of the MED Project in Los Alamos. Although Hoover claimed that the FBI’s investigation of Bohm was

[425] Ibid., 101-102.
[426] Ibid., 104.
[427] Ibid., 10.

over, Bohm's extensive FBI file (the vast majority of which is dated post May 22, 1943) and his experiences with the HCUA hearings in 1949 show otherwise.

Bohm and his colleagues remained under strict CIC investigation and surveillance for years afterward and as the investigation into the identity of "Joe" wore on, the only incriminating evidence that was uncovered about Bohm was that he associated with known communists, he was an executive committee member of the Local 25 branch of FAECT and that he attended regular meetings of the Science for Victory Committee (both of these organizations were understood to be communist infiltrated organizations, or communist fronts, by the FBI, CIC, and MID).[428] By July, the CINRAD investigation had uncovered enough circumstantial evidence to tentatively link Joseph Weinberg to the "Joe" accused of transferring atomic secrets to Steve Nelson. Without direct discoverable evidence[429] there was no way to outwardly accuse Weinberg of espionage, however, the MED Project administration was still concerned about a possible communist conspiracy at the Rad Lab, so they found other ways to diffuse this security threat.[430]

With the MED Project administration intent on disrupting the perceived communist infiltration into the Rad Lab, steps were taken to mitigate the effect of the 'communist cell'. As we have seen already, Bohm although deserving, was not allowed to transfer to Los Alamos and spent the rest of World War II confined to the Rad Lab, but still working on critical research. Weinberg was fired from the Rad Lab after the summer and transferred away from the Bay Area, while Lomanitz' draft deferment status was not approved by the Army in late July and against strenuous objections by the Rad Lab director, E.O. Lawrence and Oppenheimer himself,

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[428] Ibid., 17 & 23.

[429] The term discoverable here is critical. The CIC certainly had obtained concrete evidence against Weinberg based on wiretaps and surveillance, however, much of this evidence was not usable at future trials as it had been obtained illegally.

[430] DJB FBI File, 121.

he was inducted into the Army and deployed on September 20, 1943.[431] As Lomanitz' deployment became an unavoidable reality Oppenheimer called a general meeting with his students to request that they all stay away from politics while employed in any aspect of the MED Project.[432] A clearer message could not have been sent. Bohm's short lived political activity was over. From that point forward, it seems that Bohm renounced his affiliations with communism and limited himself only to a continued exploration of Dialectical Materialism as a philosophy.[433]

According to a history of WWII CIC activities used for training purposes at the CIC School in Baltimore, M.D., one of the primary responsibilities of the CIC in the early 1940s was to safeguard military and classified information.

> During the years 1942-1943, agents of the Counter Intelligence Corps made thousands of loyalty investigations on military personnel and civilians assigned to duties requiring access to classified material…The efforts of the Counter Intelligence Corps,… denied access to vital industrial plants and to highly secret military installations to many persons whose loyalty to the United States was dubious.[434]

If the CINRAD investigation is any indication of how effective the CIC was in disrupting activities that they deemed a security risk, we have to assume that they were extraordinarily effective. Unfortunately the CINRAD investigation was far from indicative of the army's counter intelligence successes. In retrospect, we now know the extent of KGB infiltration in the MED project as well as other wartime scientific activity in the United States. While the CIC was busy disrupting what turned out to be a fairly innocuous graduate student "communist cell" at the

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431 Ibid., 38-39.
432 Ibid., 96.
433 Kojevnikov, "David Bohm and Collective Movements," 166.
434 U.S. Army Military History Institute, "Counter Intelligence Corps: History and Mission in World War II," 13-14.

Rad Lab, Klaus Fuchs, Theodore Hall, and David Greenglass had all been allowed surprisingly extensive access to the most sensitive and highly classified of all atomic secrets at Los Alamos. It is clear, that the vast infiltration of the MED project's nerve center netted the KGB design schematics and critical scientific insights that allowed the Soviets to follow the development of the atomic bomb with surprising transparency.[435]

Young scientists like Bohm, Lomanitz, and Weinberg had no chance against cloak and dagger investigations that were intent on covertly uncovering 'un-American' activities. The CIC CINRAD investigation had convinced the MED Project administration that, the "Joe" incident was clearly a disloyal act of espionage. Whether the formula that "Joe" had passed on in March of 1943 was used to facilitate the Soviet Union's atomic research program or not is unclear but there is no doubt that these investigations were deemed necessary. What seems to have been quite unnecessary was the stigmatization by association that the investigation left on bystanders such as Bohm and Lomanitz. There is no doubt that the CIC investigation left indelible stains on the nascent, and promising, scientific careers of these two physicists. For Bohm and Lomanitz, the consequences were both immediate and long-term. By drafting Lomanitz into the Army and barring Bohm from joining the Los Alamos labs in New Mexico, the CIC investigation certainly had an immediate stunting effect on their academic careers. As it turned out, although Bohm was initially able to overcome this early career stunting, his career was eventually more significantly derailed after the covertness of the CIC investigations had given way to the very public HCUA hearings.

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[435] J. E. Haynes and H. Klehr, *Early Cold War Spies: The Espionage Trials That Shaped American Politics.* (Cambridge: Cambridge University Press, 2006).

6.4- Life as a Professional Physicist

Due to Oppenheimer's political weight and Bohm's substantial contributions to the MED Project throughout the War, Bohm was offered an assistant professorship at Princeton University beginning in the fall of 1946. After deferring his appointment to the Princeton faculty until February, 1947, Bohm arrived in New Jersey ready to begin his career as a professional academic.[436] Prodded by Oppenheimer and others to choose a trendy field of research that may allow his career to flourish such as quantum renormalization or nuclear physics, Bohm tried his hand at several different topics including a theory of non-point elementary particles and superconductivity. Against the advice of many, he ultimately decided to focus primarily on plasma research. In 1947, plasma research was not a critical field of study within the larger scientific agendas that were being pursued across the country. However, this would soon change when, in response to the first Soviet atomic bomb tests in 1949, President Truman instituted a large-scale hydrogen (fusion) bomb project.[437] Unfortunately, by the time plasma research became a critical cog in the greater scientific agenda, in the early 1950s, Bohm was already working from exile in Sao Paulo, Brazil.

From 1947 through 1951 Bohm and his graduate students, particularly Gross and Pines, set out to develop a general theory of plasma dynamics based on the foundations laid out by the pioneering works of Landau and Vlasov in the Soviet Union and Massey and Bohm at Berkeley. Building on the Landau-Vlasov theoretical framework for plasma oscillations, Bohm and Gross were able to achieve important results. Apart from confirming the diffusion equation Bohm had worked on at the Rad Lab and calculating the excitation of oscillations from instabilities, they were able to model as yet unexplained effects observed in plasma dynamics like the Langmuir

[436] Ibid., 135.
[437] Kojevnikov, "David Bohm and Collective Movements," 170.

scattering of fast electrons. The most important result that Bohm and Gross arrived at, however, was the calculation that allowed them to propose the existence of a minimum wavelength below which no plasma oscillations occurred. The significance of this minimum wavelength was that locally, on a microscopic scale, plasma particles (electrons and ions) behave exactly like free particles within an ideal gas but macroscopically, at long distances, these same particles were coordinating their motion and producing collective oscillations.[438]

While his research project with Gross was important and a natural extension of the work he had done at the Rad Lab, Bohm's most significant legacy in plasma physics was due to his work with David Pines. As Gross was finishing his PhD research, Pines was beginning his own doctoral research. During his stay at Berkeley, Bohm had realized that the collective behavior being studied in the argon plasmas at the Rad Lab could equally be applied to the collective electron behavior in metals. Once at Princeton, he began to develop these notions in more detail. About the same time as Bohm was finishing his first semester teaching at Princeton, he became intrigued by the work of Julian Schwinger and his developments on quantum electrodynamics (QED) in which he had successfully used a series of canonical transformations to renormalize the theory. Schwinger was one of the brightest young minds in American physics. One year younger than Bohm, he had received his doctorate from Columbia University in 1939 at the tender age of 21. As one of I.I. Rabi's most celebrated students he first went to work with Oppenheimer at the Berkeley Rad Lab but when the Oppenheimer moved to Los

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[438] Kojevnikov, "David Bohm and Collective Movements," 178-179. As well as David Bohm and Eugene P. Gross: "Plasma oscillations as a cause of acceleration of cosmic-ray particles." *Physical Review*, 74 (1948): 624; "Theory of plasma oscillations. A. Origin of medium-like behavior." *Physical Review*, 75 (1949): 1851-1864; David Bohm and Eugene P. Gross "Theory of plasma oscillations. B. Excitation and damping of oscillations." *Physical Review*, 75 (1949): 1864-1876; and "Effects of plasma boundaries in plasma oscillations." *Physical Review*, 79 (1950): 992-1001.

Alamos Schwinger decide to move back east and spent the remainder of World War II working on developing radar at the MIT Radiation Laboratory.[439]

In early June 1947, what many would later consider the brightest young minds of the quantum physics community arrived on Shelter Island in Long Island, New York for a three day conference. At the first of three landmark conferences sponsored by the National Academy of Sciences (NAS), Oppenheimer dominated the proceedings in which the foundations of quantum theory were discussed in a casual intimate setting. In many ways this series of conferences was the American counterpoint to the Solvay Councils that had been so instrumental during the quantum revolution. With the alarming success of the MED Project, and by extension World War II, resting squarely on the shoulders of American physics, the NAS was intent on securing American scientific dominance by showcasing their contingent of the brightest stars in international physics.[440] In Figure 6.1 we see some of the participants of the conference casually leaning over a coffee table listening to Richard Feynman illustrate a point.[441]

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[439] Mehra and Milton, *Climbing the Mountain: the scientific biography of Julian Schwinger*.
[440] Schweber, Silvan. "A Short History of Shelter Island I". In R. Jackiw, N. Khuri, S. Weinberg, E. Witten. *Shelter Island II: Proceedings of the 1983 Shelter Island Conference on Quantum Field Theory and the Fundamental Problems of Physics*. (Cambridge MA: MIT University Press, 1985), 301–343.
[441] Picture taken from the NAS archives: http://www7.nationalacademies.org/archives/shelterislandpix.html

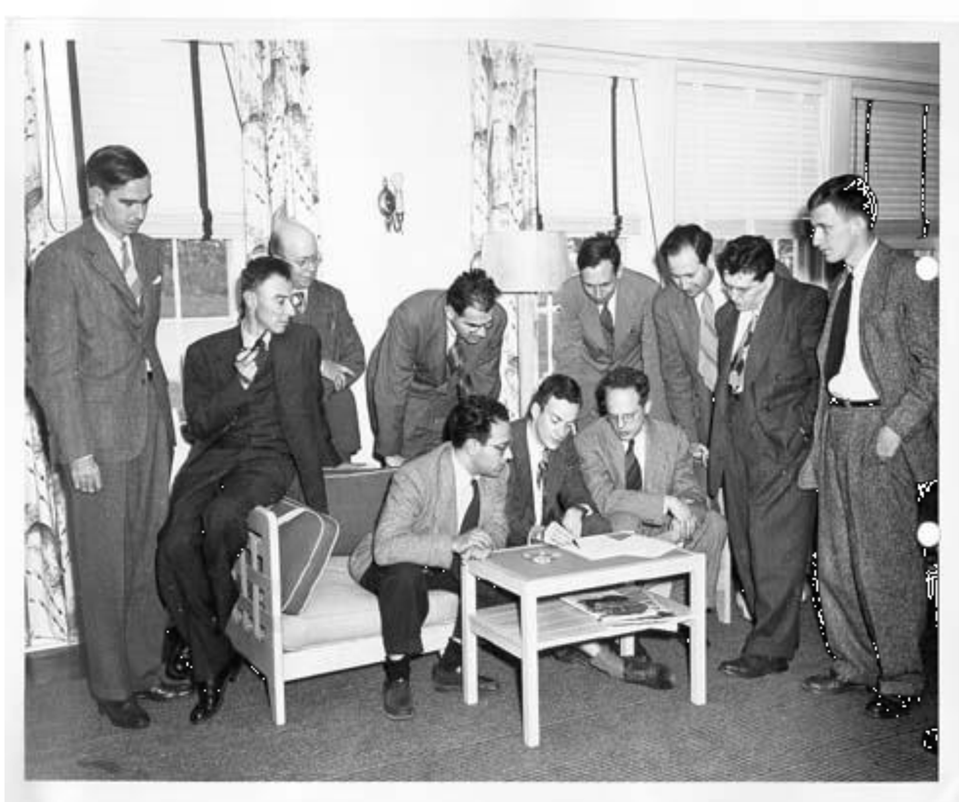

Figure 6.1- From left to right, standing, are: W. Lamb, K.K. Darrow, Victor Weisskopf, George E. Uhlenbeck, Robert E. Marshak, Julian Schwinger, David Bohm, From left to right, seated are: J. Robert Oppenheimer (holding pipe), Abraham Pais, Richard P. Feynman, Herman Feshbach.

After perusing the names of the other participants, the mere fact that Bohm was invited to participate at this seminal conference alongside some of the brightest minds in American physics should say something about what his standing was in the community in 1947. He had recently been hired by Princeton University after being regarded as a critical asset to the MED Project, he was a natural and inspiring teacher and advisor to graduate students, and he was beginning a successful plasma research program that would push the boundaries of the field. It was no accident that Bohm was at Shelter Island and as it turned out the conference played an important role in Bohm's later research.

It was at Shelter Island that Bohm first heard Schwinger give an extensive exposition on his renormalization research. He would later recall the moment he realized that he could apply Schwinger's canonical transformation technique to plasmas:

> So I thought the canonical transformation could be used here. Of course, the problem is more complicated there, but it had an additional difficulty here that there were no plasma variables at all. You see, Schwinger had the electrodynamic variables, and merely had to renormalize them. Here you had to produce the new variables, and the question of how to get this clear mathematically took some time… [Eventually] David Pines and I had [wrote] a paper in which we looked at the plasma, the electrodynamic modes of the plasmas, where we would not have to invent new variables, but just renormalize as Schwinger did. That was one paper, you see. The idea was to start there. And then the problem was to find a clear way of doing this without having these variables to start with for the longitudinal modes.[442]

Although John Bardeen had previously worked extensively on the quantum theory of solids in the mid 1930s while working with Eugene Wigner at Princeton, the central problems of many-body electron-electron and electron-lattice interactions remained impervious to analysis in the late 1940s. Particularly problematic was accounting for the dampening effects of these long-range interactions. With Pines, Bohm embarked on a research program that recognized the correlation between Schwinger's renormalization work and the idea, proposed in the 1920s by Debye and Huckel, regarding the notion of a screening cloud around each individual static particle. What made Bohm and Pine's problem significantly more complicated was modeling the 'dynamic' element of the screening that was required in conjunction with the motion of each particle in the many-body system. By recognizing the significance of Schwinger's renormalization scheme, the two physicists were able to make a significant contribution to

[442] Bohm AIP interview, 1981.

solving the problem of decoupling the long-range coulomb interaction effects from the short-range single-particle excitations.[443]

As a direct result of their joint work at Princeton, Bohm and Pines published papers, first on the "Screening of electronic interactions in a metal," in 1950 and then a series of four papers on "A collective description of electron interactions" between 1951 and 1953. In fact, while the first three of this series of papers were published as jointly authored papers, even after Pines and Bohm had both left Princeton, the last of the series was solely attributed to Pines.[444] This apparent slight would later bother Bohm, as he believed the basic work for this fourth paper was also established by the two of them while still at Princeton.[445] Towards the end of his stay in Princeton, New Jersey, Bohm began to apply the work he and Pines had done on electron interactions to the poorly understood phenomenon of superconductivity. By then Gross and Pines had completed their doctorates and he was supervising the doctoral research of a young Norwegian graduate student, Tor Staver.[446]

Unfortunately, as Staver and Bohm were getting serious about the research into superconductivity, Bohm was called before the HCUA to testify on his communist affiliations. As we shall soon see this period was a turbulent time for Bohm both personally and professionally

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[443] Hoddeson, Lillian. "John Bardeen and the Theory of Superconductivity," *Journal of Statistical Physics* vol. 103, nos. 3/4, 2001, pp. 625-640. Also, AIP Interview with David Bohm. 1981.
[444] David Bohm and David Pines: "Screening of electronic interactions in a metal," *Physical Review*, *80* (1950): 903-904; "A collective description of electron interactions. I. Magnetic interactions." *Physical Review*, *82* (1951): 625-634; "A collective description of electron interactions: II. Collective *vs* individual particle aspects of the interactions." *Physical Review, 85* (1952): 338-353; and "A collective description of electron interactions: III. Coulomb interactions in a degenerate electron gas." *Physical Review, 92* (1953): 609-625; In addition there was a disputed paper which Bohm thought he should be an author on: David Pines, "A collective description of electron interactions: IV. Electron interaction in metals," *Physical Review, 92* (1953): 625-636.
[445] AIP interview with David Bohm. 1981.
[446] David Bohm and Tor Staver, "Application of collective treatment of electron and ion vibration to theories of conductivity and superconductivity," *Physical Review, 84* (1951): 836-837.

as he was summarily suspended from all his teaching responsibilities at Princeton in the winter and spring of 1950-1951. According to Bohm's recollections, Staver continued their research mostly on his own until he was tragically killed in a skiing accident in Massachusetts. As a result, David Pines organized Staver's research and presented it as his doctoral dissertation posthumously.[447] In 1952, once Bohm was living in exile in Sao Paulo, Brazil, John Bardeen working on superconductivity at the University of Illinois, became intrigued with the potential applications of the research Bohm, Pines, and Staver had done at Princeton and invited Pines to collaborate with him as a post doctoral fellow. There is no doubt that based on his early contributions with David Bohm, David Pines was able to carve out a significant place in the plasma physics community after 1952.[448]

It seemed that Bohm had found a niche as a leading theoretical plasma physicist and was beginning to develop important results in a young and fertile field. On April 21st, 1949 upon receiving a subpoena to appear before the House Committee on Un-American Activities (HCUA) the fuse was lit that would eventually lead to Bohm's derailment from a fledgling yet promising American academic career track.

6.5- HCUA - The Cost of Silence

In March, 1949 Harold W. Dodds, President of Princeton University, gave a speech to an alumni group in San Francisco in which he showed his antipathy towards communism and anybody remotely associated with the political doctrine: "My brethren who say that Communists are members of another political party are missing the point. They are not just radicals. They are part of an international conspiracy." He went on to say that because they

[447] AIP Interview with David Bohm. 1981.
[448] Hoddeson, "John Bardeen and the Theory of Superconductivity," 630-631.

denied academic freedom, communists were unfit to teach in universities.[449] Moreover, in a similar speech he had given in Hawaii earlier that same month, Dodds declared that communists no longer had "…rights as persons, made in the image of God…" and that "Treason is the accepted code of conduct of all practicing communists."[450]

One month after Dodds' caustic speeches in Hawaii and San Francisco Bohm was subpoenaed to appear before the HCUA to answer questions about his un-American activities and his associations with the Communist Party while working at the Rad Lab in Berkeley. Specifically, the HCUA, relying on information gleaned from the CINRAD investigation wanted to pressure Bohm and his colleagues into furnishing the committee with the evidence it needed to formally indict Joseph Weinberg and Steve Nelson on espionage charges for their roles in the CINRAD affair six years earlier.

In April, 1949 the HCUA was chaired by Congressman John S. Wood (representative from Georgia) and was comprised of eight other U.S. Representatives, including Richard M. Nixon of California. Spurred on by the milieu of the time, the committee decided to investigate leaks in the MED Project during the War and as a result, held hearings on the "Communist Infiltration of [the] Radiation Laboratory and Atomic Bomb Project at the University of California, Berkeley, Calif." on April 22 and 26, May 25, June 10 and 14, 1949. [451] In the Forward to the published hearings the HCUA laid out their findings. According to the committee, in March of 1943 a 'Scientist X' (known in the CINRAD investigation as "Joe") met with Steve Nelson, a known Communist espionage agent, at his home and delivered to him a complicated formula which he asked Nelson to copy down so that he could return the formula to the Rad Lab first thing in the morning. Apparently, several days after this encounter, Nelson arranged to

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[449] Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," 744.
[450] Peat. *Infinite Potential,* 99.
[451] House Committee on Un-American Activities (HCUA) Transcript Hearings Vol.1, Title Page.

meet the Soviet vice consul Peter Ivanov “at their usual place…in the middle of an open park on the St. Francis Hospital grounds in San Francisco.” During this meeting Nelson was observed transferring something to Ivanov and several days later, Zubilin, the third secretary of the Russian Embassy in Washington, D.C. met with Steve Nelson at his home where he paid him ten bills of “unknown denominations.”[452]

Before the April hearings, the HCUA had already held a “secret session” in which it had interrogated Joseph Weinberg, the man the CIC had implicated as “Joe” or ‘Scientist X’ in 1943. In this session Weinberg had “denied that he had ever known Steve Nelson…or that he had ever given to any unauthorized person any formula or other classified information.” The committee was unsatisfied with this testimony and convened the April-June hearings with other Rad Lab personnel to “enlighten” them further on the Scientist X case. Another twist in the committee’s case pointed to a possible co-conspirator working at the Rad Lab at the time. The CIC evidence in the Scientist X case pointed to the fact that the formula “Joe” had taken to Steve Nelson’s home on March 29, 1943 was written in someone else’s handwriting. This detail pointed to the possibility that Weinberg may not have acted alone and could have been helped in his espionage by an unknown co-conspirator.

According to the Forward, in testimony before the committee Robert R. Davis and his wife Charlotte were the only witnesses to “fully cooperate” with the HCUA. All other witnesses were less cooperative. Frank Oppenheimer (J. Robert’s younger brother) and his wife Jacquenette were only willing to answer questions about their own Communist Party ties and refused to answer any questions that may implicate others. David Bohm and Giovanni Rossi Lomanitz both refused to answer any questions having any relevance to the Scientist X case, and

[452] Ibid., Forward (V-VI).

both Steve Nelson and Joe Weinberg were questioned briefly before the committee, but they too were unwilling to answer any relevant questions.[453]

The last of the three closed-door executive sessions dealing with the Rad Lab Communist infiltration was held on May 25, 1949 in Washington, D.C. After having waited patiently outside the hall to be called as a witness in the first two executive sessions held in April, David Bohm was finally called upon to testify before the HCUA. At this point Bohm had certainly been briefed by Lomanitz and had learned some valuable lessons from his friend's ordeal one month earlier. When he was sworn in at 10:30 a.m. that Wednesday morning in late May, Bohm was much better prepared for his congressional confrontation than Lomanitz had been. Not wanting to be blindsided by questions that might eventually put him into jeopardy, Bohm had hired Clifford J. Durr to represent him as counsel during the hearing.

Durr was an accomplished lawyer who was renowned for defending individuals accused of disloyalty or whose civil rights had been violated by the government.[454] Having been appointed to the Federal Communications Commission (FCC) by President Roosevelt in 1941 Durr resigned in 1948 as a protest against the newly enacted Truman loyalty oath policy requiring an administering of loyalty oaths and background investigations for all government employees suspected of CP affiliations.[455] In the spring of 1949 Durr was well known to the HCUA as he was representing several other clients, such as Frank and Jacquenette Oppenheimer, called to testify before the same committee. The mere fact of having a lawyer present, dramatically altered the dynamics between the witness (David Bohm) and the HCUA.

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[453] Ibid., Forward (V-VI).
[454] In Durr's most famous litigation he was co-counsel on Rosa Parks' defense team in her 1955 trial.
[455] Truman's Employees Loyalty Program: Executive order #9835 on March 21, 1947 by order of the White House. (http://coursesa.matrix.msu.edu/~hst203/documents/loyal.html)

Whereas Lomanitz' hearing in April had been very contentious and had devolved, at times, into a series of back and forth arguments on semantic issues of constitutional rights, Bohm's hearing was more straightforward. It seems that with a lawyer beside him, the Congressmen on the committee were not as willing to enter into drawn out legal debates on matters of constitutional law. From the HCUA published report we note that the transcript of Lomanitz' hearing on April 26th is 26 pages long while the transcript of Bohm's hearing on May 25th is limited to only 7 pages. Having Lomanitz' experience to draw on must have helped Bohm and Durr clarify their position on answering the committee's questions. This does not mean, however, that the hearing was not contentious at all. From the outset Bohm answered most questions posed to him by stating that he was not willing to answer the particular question on three grounds: 1) answering the question may tend to incriminate him (fifth-amendment); 2) that it was violating his rights under the first-amendment of freedom of assembly and association and freedom of speech; and 3) that it may tend to degrade a man's reputation and "…bar him from the normal opportunities of practicing his profession and obtaining employment."[456]

The HCUA did not recognize either of the last two reasons as valid for refusing to testify and moreover, they pushed to try and have Bohm explain why he believed he may be incriminated for answering certain questions about his political affiliations. Durr briefly sparred with several of the Congressmen on these issues and left it clear that Bohm would not answer any questions that had to do with his affiliations to the CP or his associations with individuals that were known to be Communists. While the committee stated that it was not illegal to be a member of the CP, Durr reminded the Congressmen that "We have at the moment an

[456] HCUA Hearings Vol.1, 324.

indictment and a trial of members of the CP on the ground of a conspiracy to teach prescribed doctrines."[457]

When Durr invoked the Supreme Court's ruling in the Lovett-Watson-Dodd case as a precedent for why Bohm should be protected from answering questions that could cause him to lose employment opportunities, Richard Nixon replied:

> As a matter of fact, counsel is well aware of the fact [sic] that the Lovett-Watson-Dodd case involved a specific act of Congress which denied those men their employment, and I think you are certainly stretching a dictum as far as I have ever heard of one being stretched in applying it to this case.[458]

Towards the end of the hearing, Congressman Moulder, frustrated that the committee was not eliciting adequate responses to the inquiries on Bohm's CP affiliations, engaged in the following exchange with Bohm:

> Mr. Moulder: Are you a member of or affiliated with any political party or association?
> Mr. Bohm: I think [after conferring with Mr. Durr] I would say definitely that I voted the Democratic ticket.
> Mr. Moulder: That is not responsive to my question. I asked if you were a member of any political party or association.
> Mr. Bohm: How does one become a member of the Democratic Party?[459]

Shortly after this exchange, Bohm was excused from the hearings. The effects from this ordeal would reverberate throughout his life and would eventually isolate him personally and professionally sending him on a peripatetic academic journey outside the United States and outside the traditional boundaries of physics.

[457] Ibid., 322.
[458] Ibid., 324.
[459] Ibid., 325.

6.6- Fall-out from HCUA

On August 29th, 1949, just three months after Bohm's testimony in front of HCUA, the Soviet Union exploded their first atomic bomb, a replica of the American plutonium bomb dropped over Nagasaki four years earlier. Although the American intelligence community had always assumed that the Soviets would eventually develop their own nuclear arsenal, the speed with which it came was alarming to the Washington, D.C. establishment. It was obvious to those who understood the workings of the MED Project itself, that the Soviets could not have arrived at a workable solution so quickly without help from very knowledgeable sources within the American atomic project. While the HCUA had already begun investigating the possibility of wartime espionage activity around the Rad Lab, with the news of the Soviet atomic test, the stakes of the investigation were amplified as the implications of wartime atomic espionage were thrust into the public spotlight.[460]

United States President Harry S. Truman made the announcement on September 23rd, 1949, that the Soviet Union had detonated their atomic bomb and was now a nuclear power. Six days later, the HCUA committee released their report entitled: *"Report on Soviet Espionage Activities in Connection with the Atomic Bomb"* implicating among others Bohm, Lomanitz, and Weinberg in the espionage ring associated with Berkeley's Rad Lab.[461] The political climate in the United States was decidedly caustic towards any situation that involved even the hint of communist sympathies. It was clear

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[460] Haynes and Klehr. *Early Cold War Spies,* 152.
[461] The document *"Report on Soviet Espionage Activities in Connection with the Atomic Bomb"* is partly reprinted in a summary report for the Joint Committee on Atomic Energy entitled: *"Soviet Atomic Espionage".* Printed by the United States Government Printing Office in Washington, D.C. April, 1951.

that due to a series of international tensions and numerous very public espionage cases the United States was being swept into a Second Red Scare.[462]

It was in the midst of this red scare atmosphere that Henry Smyth, the current Atomic Energy Commissioner who had recently left Princeton after serving as the chair of their Physics Department, wrote to Princeton University President Dodds in October 1949 warning of a possible over-reaction:

> Unless you feel like asking [Bohm] about this and getting a clear answer, I think the University must assume that he is innocent. These are the reasons why I personally feel very strongly that no action to terminate Bohm's appointment should be taken unless some further evidence comes out….It does not seem to me that he should be automatically precluded from reappointment or promotion.[463]

Based on his comments earlier in the year in Hawaii and San Francisco and the prevailing political climate, Dodds was clearly not moved by Smyth's heed and responded in November 1949:

> I am unwilling to agree that nothing has happened to date to bear on the reappointment of the man in question. In fact I am quite unhappy about the situation, and as it stands at present I could not recommend reappointment.[464]

By the following summer the Korean War had broken out, and Alger Hiss had been tried, convicted, and sentenced to five years in prison for perjury. What was even more problematic for Bohm and his cohort was that on January 24, 1950 Klaus Fuchs, an integral member of the MED Project, confessed to spying for the Russians throughout World War II. This admission, and

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[462] See footnote 73.
[463] Olwell. "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," 747
[464] Ibid., 747.

the information he provided quickly resulted in the uncovering of an extensive Russian espionage network which had managed to unveil and turn over critical atomic secrets to the communists both during and after the War. Throughout 1950, the domino effect continued to lead to new evidence of complicity and spying eventually resulting in arrests, convictions, and sentences for those involved. One only needs to think about the effect on our society of the most notable amongst these, the convictions and eventual executions of Ethel and Julius Rosenberg.[465]

In the midst of all the espionage arrests in 1950 and the intrigue surrounding the infiltrated MED Project, the HCUA cited fifty-six people for contempt of congress among these was Bohm, who was indicted, arrested, and eventually acquitted of all charges, but not before his career had been irreparably harmed. Based on his promising academic work, in November, 1950, the Physics Department recommended to the Princeton administration that the university reappoint Bohm as assistant professor when his contract came up for renewal the following summer. The chair of the Physics Department, Allen Shenstone, highlighted both his teaching qualifications and his accomplishments in scholarship. The administration was informed that Bohm was rated an “inspiring” teacher of graduate students and that “The graduate students whose thesis work is directed by Professor Bohm are devoted to him. He is constantly in demand for consultation by other graduate students.” As for his scholarship:

> The quality of his scholarship is attested by the manuscript of a book on Quantum Mechanics which is based on his graduate course on the subject, and promises to be one of the outstanding books on the subject. At present he is directing the theses of several of our best graduate students. His research is spread over a wide variety of subjects, from the epistemological foundations of quantum physics to the properties of

[465] Haynes and Klehr, *Early Cold War Spies,* 138 - 187.

> electric plasmas. In the last respect his results are particularly significant and he is one of the leaders of the field.[466]

In fact, after Bohm's arrest on December 4, 1950, the graduate students in the Physics Department quickly organized themselves and published a letter of support for Bohm in *The Daily Princetonian* on December 15th, which was then reprinted in *The New York Times* on December 16.[467]

Bohm once again was forced to retain legal counsel, and while this time it was one of Clifford Durr's assistants who handled his case, the concern for Bohm was lessened because seven days after his arrest, on December 11, 1950, the Supreme Court decided on the case of *Blau vs. the United States*. In the Blau case, the court ruled that due to the Fifth Amendment, the right against self-incrimination preceded the congressional committees citing contempt charges. Based on this decision, it seemed clear that Bohm, and others in his predicament, would have their cases thrown out. Not only was his case not thrown out but it was delayed from January to May of 1951. This was critical as, Bohm's suspension from Princeton was valid only through the end of the trial. Had his case been thrown out, in December, he may have been allowed to return to his teaching responsibilities as early as January. Needless to say, David Bohm was not acquitted of all charges until May 30, 1951, and his contract renewal at Princeton University was declined in June. With the support he had received from the Physics Department and in light of his promising scholarship in plasma physics, it seemed that the renewal of Bohm's contract would have been a simple formality; however, due to his drawn out legal trouble and the clear anti-communist rhetoric of the Princeton administration this renewal became completely untenable.

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[466] Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," 747.
[467] Ibid.,746.

In a letter to physics Professor Donald Hamilton dated February, 1951 Dodds explained that:

> After thorough consideration of all aspects of the case, I am convinced that the University should not reappoint David J. Bohm for another term as Assistant Professor of Physics. Before reaching this decision, I have had not only the advice of you and your colleagues in the physics department, but of others at Princeton and elsewhere. In addition, I have made study of all available evidence in respect to Professor Bohm's relations with the United States government. It is my firm belief that Professor Bohm's professional capacities and personal qualities, in combination, are not such to justify any renewed evidence of confidence in his future contribution as a member of the Princeton Faculty. On the contrary, I feel that the University must recognize that Professor Bohm has not indicated that he possesses those qualities of personality and judgment which should be expected of a member of its faculty.[468]

In focusing in on Bohm's "qualities of personality and judgment" it was perfectly clear that the decision not to renew his contract was politically motivated and that Dodds, and the Princeton administration, had allowed these interests to outweigh academic considerations. In fact, we know that Dodds, in arriving at his decision, had based his decision not solely on advice from the Physics Department but on the input of "others at Princeton and elsewhere." Like all universities of Princeton's caliber, there were important economic pressures and considerations at play in all decisions, government and private grants as well as donations from alumni were critical to safeguarding the continued viability of the institution. In the political context of the red scare, the opinions of these various economic donors certainly influenced Dodds' decision. For example in reference to Bohm's case, Robert Wood Johnson, of Johnson & Johnson wrote to Dodds: "an institution seeking private support should at this time put its house in order." Some

[468] Ibid., 747-748.

of the alumni were even more poignant in their remarks as one declared: "We want neither criminals nor cowards as teachers of our youths."[469]

With this level of extreme antipathy from the various power centers at Princeton University, it must have been clear to Bohm, long before June, that his contract would not be renewed. Even before his termination at Princeton in June, concerned friends and esteemed colleagues attempted to help the seemingly railroaded Bohm find another academic post in the United States. Henry Smythe, who had sent Dodds a letter defending Bohm, inquired at other universities as to possible academic positions, but soon found that Bohm was untouchable. The same was true at Bell Labs, RCA, and Project Matterhorn, where physicists like Lyman Spitzer recognized that Bohm's expertise in plasma physics would be a great asset to their research. Seemingly at every turn, there were scientists interested in hiring Bohm because of his past scholarly accomplishments and his potentially invaluable contributions moving forward, only to be summarily overruled by upper level management teams or administrations anxious not to poison their funding wells.[470]

All attempts to secure Bohm an academic position failed to produce results.[471] In the spring and summer of 1951, Bohm's life and career were clearly in a state of turmoil, he was first barred from the Princeton campus and then undeniably black listed from what seemed like all academic circles. Bohm's career was completely altered due to this period of turmoil, he was forced to pursue his academic scholarship in exile, first in Sao Paulo, Brazil, then in Tel Aviv, Israel, and finally in London, England. It is this transition from a leading plasma physicist working as a professor at an elite American university like Princeton, to a political outcast who made the seemingly conscious decision to transition the focus of his research from cutting edge

[469] Peat, *Infinite Potential,* 102.
[470] Ibid., 104.
[471] Ibid., 104-105.

plasma research towards questions concerning interpretation and the foundations of quantum theory.

6.7- Bohm Reexamines the Copenhagen Interpretation

In the vast majority of biographical sketches of David Bohm his period in Princeton, New Jersey is treated very superficially, if at all. It seems that most Bohm historians have preferred to focus on his work after his academic exile finding his deviations from the physics community inherently more interesting. When Bohm's Princeton period is mentioned it is usually employed solely as an initial backdrop to his political persecution and the preface to his later work on hidden variables theory. However, a more careful examination of his work during this particular period is absolutely critical to understanding Bohm's later career. In particular, when one takes into consideration a juxtaposition of his plasma work, the publishing of his *Quantum Theory* textbook, and his work on hidden variables theory we find all sorts of links between these distinct pursuits. One of the more obvious links is that they all happened contemporaneously with his political persecution by HCUA. In fact, during the spring of 1951 while he was serving his indefinite suspension from Princeton University, he was engaging all three of these lines of enquiry almost simultaneously.

When examined, as they generally are, as three singular pursuits they raise enigmatic questions about Bohm and his motivations for making research choices. In particular, two questions that seem to continuously baffle historians are why Bohm suddenly developed his hidden variables theory in 1951 immediately after publishing a faithful and representative exemplar of quantum theory as dictated by the generally accepted Copenhagen Interpretation, and what led to his eventual decision to abandon his plasma physics in order to pursue research in hidden variables theory? In order to answer these questions we must examine how Bohm transitioned from being a mainstream physicist working on plasma physics and writing a conventional quantum textbook, while teaching at an elite American university, to a blacklisted and exiled academic who decided to shift his research interests to challenging the quantum orthodoxy with the development of his hidden variables theory.

We have seen how, by 1951, Bohm had developed into a promising young mainstream plasma physicist and an excellent teacher of graduate students. One may recall that he participated in the famous Shelter Island conference in 1947 and was supervising talented graduate students like David Pines and Eugene Gross, publishing extensively on a wide variety of topics relating to plasma and solid state physics. Much of his this work was on the forefront of these research fields, and for years, would remain critical to further research developments. In 1949, after spending two years teaching an extremely successful graduate seminar on quantum theory at Princeton, and with his position in academia seemingly etched on the list of promising young American physicists next to those of Feynman and Schwinger, Bohm decided to publish a textbook on quantum theory.

When Bohm first began grappling with quantum mechanics during the early stages of his graduate studies, as we have seen, he was a “convinced classicist” uncomfortable with all the counter intuitive implications that the Copenhagen Interpretation proposed. In addition, what made his acceptance of quantum theory even harder was his personal approach to physics, in which he was highly reticent to blindly accept someone else’s physical intuition and pragmatically subscribe to the approach of ‘shut up and calculate’. It was only after sitting through Oppenheimer’s quantum mechanics lectures at Berkeley, engaging his fellow graduate students, in particular Joe Weinberg, in many long debates, applying the formalisms to his MED project plasma research, and substituting for Oppenheimer as lecturer in his quantum mechanics seminar, that Bohm began to understand and accept the foundations of quantum theory as interpreted by the Copenhagen Interpretation.

As a result, when he began teaching a graduate seminar at Princeton on quantum theory in 1947 it was natural for him to continue to grapple and deepen his understanding of the foundations of the subject matter. After two years of teaching his celebrated seminar he felt that he had reached a thorough and profound understanding of the basic principles of quantum theory and that no available textbook grappled sufficiently with the difficult task of helping physics students to develop a physical intuition of the complex and seemingly abstract subject matter. He felt that he could translate his seminar lectures along with the problem sets he had developed into an innovative and sound presentation of quantum theory which might become an important textbook on the subject. It took Bohm two years to publish his textbook entitled “Quantum Theory.” In those two years he was understandably somewhat distracted by the enveloping controversy stemming from his testimony before the HCUA in 1949. One should recall that Bohm was arrested in December, 1950 and then immediately and indefinitely suspended from his professorial duties by Princeton University. As he was beginning his period of suspension from Princeton, Bohm was busy finalizing edits for his textbook proofs and submitting them to Prentice-Hall publishing, in Englewood Cliffs New Jersey, for the book’s first publishing run in 1951.

In reading his treatment of quantum theory, it becomes clear that Bohm developed his textbook using two primary guiding principles. First and foremost, in the preface of his book, he articulates for the reader, what his goal in writing the book is and what pedagogical approach he prefers:

> The general plan adopted in this book has been to supplement a basically qualitative and physical presentation of fundamental principles with a broad range of specific applications that are worked out in considerable mathematical detail. … In this way, one avoids the need for introducing the basic principles of quantum theory in terms of a complete set of abstract mathematical propositions, justified only by the fact that complex calculations based on these postulates happen to agree with experiment.[472]

In contrasting this approach with one more akin to the axiomatic approaches in Dirac's 1930 text and especially that of von Neumann's 1932 textbook Bohm offers up three reasons why his approach is more advantageous. Of the three, the last one is the most relevant to our analysis. Bohm concludes that his approach is superior ultimately because:

> …it is less rigid in its conceptual structure, so that one can see more easily how small modifications in the theory can be made if complete agreement with experiment is not immediately obtained.[473]

This last quote reflects a critical point in Bohm's thinking, which is that he does not recognize the infallibility of quantum theory. While he recognizes its practical superiority and its current dominance within the physical sciences he does allow for the possibility that it might be modified in "small" ways. However, as we shall soon discover, while he was certainly open to small modifications of the theory, from his textbook, Bohm seemed fairly convinced, in early 1951, that there was no possibility of a hidden variables theory salvaging determinism.

The second guiding principle is closely related to the first and is similar to the one adopted by Heisenberg in his 1930 book. For both Bohm and Heisenberg physical intuition is far more important than mathematical rigor, and they believed that the most effective way to build that intuition was to show, through the heavy use of experimental descriptions, explanations,

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[472] David Bohm, *Quantum Theor,.* (New Jersey: Prentice-Hall, 1951), iv.
[473] Ibid., iv.

and analyses, the boundary between the classical and quantum domains. In the opening to his book, Bohm unequivocally declares this intent:

> The Quantum Theory is the result of long and successful efforts of physicists to account correctly for an extremely wide range of experimental results, which the previously existing classical theory could not even begin to explain. …In this book, the experimental and theoretical are presented in such a way as to emphasize [their] unity and to show that each new step is either based directly on experiment or else follows logically from the previous steps. In this manner, quantum theory can be made to seem less like a strange and somewhat arbitrary prescription…[474]

Ultimately, for Bohm, the success of a physical theory is not determined by its mathematical rigor or the logical consistency of its axiomatic formalisms, but by the power it displays in explaining and predicting experimental evidence in a physically intuitive way. As such he begins his treatment by launching into a 172 page discussion of the foundational concepts behind quantum theory and how they grew out of the failures of classical theory to account for an increasing number of experiments.

Among the quantum concepts Bohm highlights in this extensive discussion are wave-particle duality, discontinuity, the correspondence and uncertainty principles, de Broglie waves and Schrödinger wave packets, quantum probability, and what he calls the notion of incomplete determinism. In discussing the correspondence between classical and quantum concepts he points to three generalized quantum concepts, in particular, that cannot be reconciled with classical theory and thus must be replaced by new concepts. Bohm explains that the notion of continuous trajectory must be replaced by that of indivisible transitions, in addition, the concept of complete determinism must be replaced by "causality as a statistical trend," and finally the assumption of the possibility of an exhaustive reductionism must be replaced by the acceptance

[474] Ibid., iii and 1.

of the world as an indivisible whole whose individual parts appear as "abstractions or approximations, valid only in the classical limit."[475]

In chapter eight of Bohm's discussion on the physical picture of quantum theory we find a critical and well articulated exposition of his philosophy of physics and in particular his approach to quantum theory as it relates to the novel quantum conceptions mentioned previously. In this chapter Bohm proposes to:

> …provide a critical discussion of the classical concepts of continuity and complete determinism, in order to show that there is no *a priori* logical reason for their adoption. In addition the quantum concepts of indivisible transitions and incomplete determinism are not only just as self-consistent from a logical standpoint, but also much more analogous to certain naïve concepts that arise in many phases of common experience.[476]

It is clear that Bohm relies heavily on Bohr's complementarity principle as a foundation to his physical understanding and attempts to take this notion a step further by emphasizing what he calls the notion of the "world as an indivisible whole."[477] When read carefully and understood in juxtaposition to his later hidden variables theory, his seemingly enigmatic and sudden reversal from non-determinism to determinism begins to make more sense as we begin to see more continuity in his approach.

After a lengthy exposition recapitulating his understanding of the historical origins of notions such as "complete determinism" and causality, Bohm concludes that our reticence to drop complete determinism cannot be due to the fact that it is somehow an *a priori* human natural instinct. Instead he points to the evolution of humanity's understanding of causality from something that was associated with a physical systems' "tendency" to react to forces to

[475] Ibid., 144.
[476] Ibid., 144.
[477] Ibid., 145.

the development of complete determinism as a result of the scientific and subsequent industrial revolutions. While our everyday experience is still very much represented by the "tendency" of causes to lead to certain effects, we have become dependent on the reductionist methodology that arose during the scientific revolution and that allowed us to analyze our physical world and describe it in completely deterministic ways. However, these descriptions were only approximations and not universally applicable, so when quantum theorists introduced indeterminism they were not just abandoning complete determinism but instead, they were returning to a fuller agreement with our actual physical experience. According to Bohm:

> The complete determinism of classical theory arose from the fact that, once the initial positions and velocities of each particle in the universe were given, their subsequent behavior was given for all time by Newton's equations of motion. But in quantum theory, Newton's law of motion cannot be applied in this way to an individual electron, because the momentum and position cannot even exist under conditions in which they are both simultaneously defined with perfect accuracy. … Thus in quantum theory, as in common experience with the nonmechanical aspects of life, only a statistical trend in the course of events is determined and not the precise outcome in each case.[478]

It is interesting to note that when discussing the classically prescribed notion of complete determinism, Bohm makes the foreshadowing assertion that:

> From a purely logical point of view, however, the concept of force is redundant, because it is always possible, in principle, to express all classical physics in terms of the positions, velocities, and accelerations of all the particles in the universe.[479]

The notion of a system's dynamics being expressed exactly when the positions, velocities, and accelerations of all the particles are known became the basis of his hidden

[478] Ibid., 152.
[479] Ibid., 151.

variables theory which he developed later that same year. We must caution that, in *Quantum Theory,* Bohm was definitely not advocating hidden variables as a viable way of reintroducing complete determinism. In fact, while he did not subscribe to the von Neumann "impossibility proof" ruling them out completely, Bohm was clearly inclined to think that they could only be justified if some future experiment pointed to the necessity of their existence.

In his discussion on the physical formulation of quantum theory Bohm compares the probabilistic nature of statistical mechanics and its associated thermodynamic approximations to the probability inherent in quantum theory. In which case, the associated quantum measurements could be compared directly to the statistically averaged thermodynamic properties. If the parallel holds, obviously the "hidden variables" that create the necessity of statistical mechanics could be compared to the quantum hidden variables representing an exact and deterministic reality.[480] Bohm states:

> …even if there are hidden variables, we must conclude that no experiment made so far has ever depended on anything more than a random statistical average of these variables (analogous to the pressure and temperature in thermodynamics) and that no experiment has yet, therefore, supplied any evidence for the existence of hidden variables. … Of course, it is always possible that in some new range of experiment, not yet studied, the predictions of the quantum theory may turn out to be wrong, and that here we will discover phenomena in which the hidden variables are not averaged out. … At present, however, it seems extremely unlikely that we shall ever be able to obtain a totally deterministic description in terms of hidden variables.[481]

While Bohm was not outright denying any possibility of a successful hidden variables theory, he was claiming that it was extremely unlikely. In the early sections of his textbook, he mentions hidden variables several times and in each case it seems that his strongest argument

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[480] Ibid., 114-115.
[481] Ibid., p. 29.

against them is that no experimental results have yet been found that warrant their existence. However, in chapter twenty-two, at the end of his textbook, Bohm turns to a slightly more conceptually rigorous argument against their existence. He begins this discussion by engaging the question of whether quantum theory is a logically complete theory. Here, he begins by relying heavily on von Neumann's general argument for the completeness of quantum theory and its implications for the measurement process. The aspect of this discussion that is most relevant to our analysis is Bohm's treatment of the EPR paradox (or as he calls it ERP paradox) and its implications for the existence of hidden variables.

As we saw in the previous section the EPR paradox authored by Einstein, Podolsky, and Rosen in 1935 posed the seemingly straightforward question: "Can quantum-mechanical description of physical reality be considered complete?" EPR unequivocally offered the answer that quantum mechanical theory should not be considered complete. Based on some basic assumptions about reality and its one-to-one correspondence with any complete physical theory, the authors posed a *gedanken* experiment that showed the apparent paradoxical nature of the process of quantum measurement of two spatially separated yet quantum correlated systems. From the paradoxical nature of this proposed experiment, and due to their assumptions about the definition of complete physical theories and the nature of reality, EPR concluded that the only possibility was that quantum theory, as it was currently constructed, could not possibly be a complete theory.

In his analysis of this paradox, Bohm proposes a related but slightly different *gedanken* experiment to the one posed by EPR in 1935. In order to facilitate his analysis, Bohm specified the system as being a diatomic molecule with an overall spin of zero and two correlated spins for each of the component atoms equal to $h/4\pi$, and oriented in opposite directions. Bohm reasoned that if the molecule is dissolved into its component atoms via a process that conserves the overall angular momentum then the atoms may be spatially separated but will remain quantum mechanically correlated. Now, the quantum nature of the correlation comes into play, because of the uncertainty principle (and the non-commutation of the spin operators), only one of the three spin axes (σ_x, σ_y, σ_z) can be measured at any one time, and once that measurement along any one axis is completely determined, the information along the other axes is destroyed.

In what has come to be known as the EPR-B experiment, Bohm proposed that the two correlated atoms A and B, produced by the disassociation of the original molecule, separate in space and at some later time the spin of atom A is measured by an observer. If the observer decides, sometime after separation, to measure the spin of atom A along the spin axis z (σ_z), then due to the quantum correlation of A and B and the fact that they are defined by one single wave function ψ_{AB}, atom B's spin state will be instantaneously defined by the measurement of A. Therefore, without it being disturbed or measured directly, atom B's spin state can be exactly determined. The apparent paradox comes in, when one thinks that the observer could have chosen to measure along the x or y axes instead of the z axis well after the atoms had separated. In other words, how did atom B "know" that atom A was being measured along z axis and not along x or y axes?

For EPR the two fundamental problems that arose due to this seemingly paradoxical *gedanken* experiment were that there was apparently communication between spatially separate systems that violated the special theory of relativity and that the possibility of an indirect measurement of B's spin state violated one of the axioms of a complete physical theory, as defined by the EPR authors. Bohm recognized the significance of this EPR argument because he acknowledged that if one accepts that the quantum theory is incomplete then it opens the door for the possibility of finding a deeper theory based on hidden variables that might ultimately reinstate determinism. Bohm did not believe that the EPR challenge was substantial enough to warrant this and he critiqued their proposed paradox by disallowing the three authors' very assumptions.

First, Bohm reasoned that the EPR experiment was only paradoxical if one accepted that the universe was ultimately analytically reducible. He pointed to his earlier discussion of complete determinism and reiterated that the need for it was only a recent phenomenon and not an *a priori* human category. In Bohm's interpretation he saw the quantum world as an indivisible whole in which case spatially separate atoms like A and B would not be epistemologically independent. The second EPR assumption that Bohm challenged was the very definition of a complete theory. For Bohm, it didn't make sense to limit complete physical theories to only those that had a one-to-one correspondence between physical elements and numerical parameters within the theory. In his opinion:

> …we have come to the point of view that the wave function is an abstraction, providing a mathematical reflection of certain aspects of reality, but not a one-to-one mapping. To obtain a description of all aspects of the world, one must, in fact, supplement the mathematical description with a physical interpretation in terms of incompletely defined potentialities.[482]

For Bohm the argument against EPR and hidden variables went hand in hand, after all, "the analysis of the world into precisely defined elements and the synthesis of these elements according to precise causal laws must stand or fall together."[483] Ultimately, Bohm declared that accepting the possibility of hidden variables was equivalent to contradicting the uncertainty principle because it necessitated a simultaneous and exact description of all physical parameters

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[482] Ibid., 622.
[483] Ibid., 622-623.

even non-commuting variables. According to Bohm, the uncertainty principle was one of the most fundamental concepts in quantum theory, so disavowing it was tantamount to altering the very foundations of the theory. However, since quantum theory was so empirically successful there was no reason to do away with it. Therefore, he concluded that:

> …no theory of mechanically determined hidden variables can lead to *all* of the results of quantum theory. [However] such a mechanical theory might conceivably be so ingeniously framed that it would agree with quantum theory for a wide range of predicted experimental results.[484]

Bohm quickly inserted a footnote that qualified this conclusion somewhat prophetically stating that there were, to his knowledge, no examples of complete and successful hidden variables theories out there but that he could not completely rule out their possibility in the future.

Overall, Bohm's stance on hidden variables as presented in his book seems to have been mostly critical, yet open to them in an abstract sense. The tipping point for Bohm on whether they were actually feasible was clearly driven by yet undiscovered experimental evidence. Interestingly enough Bohm explicitly made the point that, as of 1951, no adequate hidden variables theory had been developed and that it would take an "ingenious" effort to come up with a reasonable approach. From his presentation in *Quantum Theory* it is apparent that Bohm was not aware of the extent of the work done by physicists such as de Broglie, Madelung, and Einstein in the late 1920s to make quantum theory completely deterministic.

After its publication, Bohm's textbook was warmly received within the physics community as it was used in many university courses throughout the country.[485] In addition,

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[484] Ibid., 623.
[485] Peat. *Infinite Potential,* 109. In addition, anecdotally, I met a physicist recently who told me that he had studied as an undergraduate at the University of Chicago in the 1960s and that Bohm's book was used there for a long time to teach quantum mechanics.

after Bohm sent preprints to some of the leading quantum theorists in the world, he received positive feedback particularly from Pauli who wrote Bohm telling him that he had achieved a lucid presentation of the subject matter while retaining an important balance between the physical intuition, mathematics, and philosophical implications of quantum theory. Einstein's reaction to Bohm's text was probably the most significant of all. While there is no concrete record of the conversation between Einstein and Bohm, we know that during his six month suspension from Princeton University, Bohm was allowed to use the IAS facilities to continue his research. According to Bohm, it was during this period at IAS that he was engaged by Einstein to discuss the book he had just published. The great German physicist was apparently impressed by Bohm's presentation of the accepted interpretation of quantum theory but challenged him on the notion of quantum completeness and his dismissal of the EPR paradox as well as his hand waiving argument for indeterminism.[486]

6.8- Bohm's Hidden Variables Program

There is no smoking gun that tells us why Bohm began developing his hidden variables theory. However, based on a close reading of his textbook and his initial hidden variables papers it is clear that these two projects were not really as disjointed as they seem at first sight. When studied in juxtaposition, there is little doubt that Bohm's work on *Quantum Theory* was necessary for his subsequent development of hidden variables theory. In fact, one can surmise from the last mention of hidden variables in his textbook and his qualified dismissal of their possible existence that Bohm seemed to leave the door slightly open for a serious attempt at their development. Was he already thinking of this possibility while he was finishing his

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[486] David Bohm Interview with Maurice Wilkins (1986-1987). See Peat, *Infinite Potential,* 109.

quantum textbook? Stating, at the end of his text, that it would take an "ingenious" approach to develop an adequate hidden variables theory almost seems like a self-imposed challenge. While there is no concrete evidence from his correspondence or papers that points to a decisive reason as to why he began to develop his hidden variables theory within weeks of publishing *Quantum Theory,* in later interviews and in the acknowledgements of his hidden variables papers, Bohm explicitly identifies his conversations with Einstein as a turning point in his thinking on the subject.[487]

Without a transcript of the conversations it is impossible to know exactly what was said between the two physicists, but one thing is for certain, Einstein developed a strong affinity for Bohm and considered him a young colleague with significant potential. Apparently, Einstein appreciated Bohm's particular approach to exploring the foundations of quantum theory by thoughtfully examining the real physical implications of the accepted interpretations and not just blindly following the counterintuitive assumptions that the Copenhagen spirit promoted. We know that Einstein, impressed by Bohm's continuing work on the foundations of quantum theory and seeing that Princeton's administration would probably refuse to retain the young physicist after his suspension was lifted, began to make inquiries as early as April, 1951 as to possible future academic appointments for Bohm.

First, Einstein attempted to hire Bohm as his assistant at the IAS, but that idea was dismissed by the Institute's director, J. Robert Oppenheimer, who claimed that, with all the political turmoil surrounding Bohm, hiring him would bring embarrassment to the IAS. With that avenue closed, Einstein then attempted to find Bohm an appointment outside of Princeton, New Jersey. In a letter dated April 17th to P.M.S. Blackett at the University of Manchester,

[487] Ibid., 109.

England, Einstein recommended Bohm as "one of the most promising of the original younger theoretical physicists [with] a clear mind [someone who] is very energetic in his scientific work and of a rare independence in his scientific judgment."[488] Einstein was clearly taken with Bohm's potential as a physicist and was one of his strongest supporters throughout his suspension and during his early exile in Brazil.

While the specifics of the Einstein-Bohm dialog during the spring of 1951 are not available for our analysis we can make a reasonable guess as to what Einstein took issue with in Bohm's *Quantum Theory* text. Even sixteen years after the EPR paper was published, Einstein was not convinced by the arguments against the notion that the accepted formulation of quantum theory could not possibly be complete. Within the Born-Einstein correspondence there is a copy of an article from a 1948 paper Einstein published in the journal *Dialectica* in which he restates his earlier complaints about the state of quantum theory and in particular the accepted interpretation espoused as part of the Copenhagen spirit. His long-standing complaints about the most troublesome shortcomings of quantum theory remained its inability to separate the measurement process of observable reality from the mind of the conscious observer, indeterminism, the nonphysical nature of the wave function, and non-locality which, according to the EPR paradox, proved the incompleteness of quantum theory. In particular he emphasized the point that:

> Unless one makes this kind of assumption about the independence of the existence of objects which are far apart from one another in space- which stems in the first place from everyday thinking- physical thinking in the familiar sense would not be possible"[489]

[488] Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," 750.
[489] Max Born, *The Born-Einstein Letters,* (New York: Walker, 1971), 170. Also see Arthur Schlipp, ed., *Albert Einstein: Philosopher-Scientist,* (Evanston, IL: Library of Living Philosophers, 1949).

The fact that Bohm had wholeheartedly embraced the Copenhagen spirit and all its implications, elevating many of them including indeterminism and non-locality to the level of physical axioms in his treatment of quantum theory, and had then used these in critiquing the EPR paradox was most likely very problematic for Einstein. He must have argued passionately with Bohm over his treatment of the EPR paradox and his acceptance of non-locality and his emphasis on interconnectedness and wholeness. In any case whatever Einstein said was clearly impactful for Bohm who consistently pointed to these dialogs as transformational for him and his research. It was only after these discussions that he decided that it was possible to complete quantum theory and that he might just be "ingenious" enough to accomplish the task.

Einstein was certainly aware of the possibilities of using something like a hidden variables theory to complete quantum theory as he had attempted to develop his own deterministic wave mechanical quantum theory in 1927 and had seen de Broglie's extensive efforts to do the same from 1923-1927 first hand. With this in mind, one might guess that Einstein could have mentioned these long abandoned research programs to Bohm in their discussions during the spring of 1951. However, we know that Einstein did not point Bohm directly towards hidden variables because, when writing his hidden variables papers during June 1951 Bohm genuinely believed that he had stumbled onto something completely original. Something that he believed was an innovative and groundbreaking approach to quantum theory. Had Einstein mentioned his unpublished 1927 attempt to reformulate quantum theory deterministically or de Broglie's 1923-1927 alternate wave mechanical program, Bohm would have referenced these in his publications and not found his own approach so innovative.

Instead, Bohm adjoined a note at the beginning of his papers stating explicitly that he had learned of these earlier abandoned deterministic programs only after completing his writing.[490]

In addition, from Einstein's reactions to Bohm's hidden variables program, we know that while he must have argued passionately for the incompleteness of quantum theory in their discussions, Einstein certainly did not mean for Bohm to use hidden variables to complete the theory. In a May, 1952 letter to Born, Einstein wrote to his friend "Have you noticed that Bohm believes (as de Broglie did, by the way, 25 years ago) that he is able to interpret the quantum theory in deterministic form? That way seems too cheap to me."[491] Einstein was adamant that quantum theory needed to be altered and completed to account for indeterminism and other problematic features but as far as he was concerned hidden variables was not the answer.

While Einstein did not directly lead Bohm to his "discovery" of hidden variables, Bohm was clearly moved to action by Einstein's argument that quantum theory could be altered and improved upon. As a participant at the seminal Shelter Island conference and the success of his plasma research program while at Princeton, Bohm felt that he was in the immediate group of elite young physicists along with Feynman and Schwinger who might be able to alter the foundations of quantum theory. If, as Einstein claimed, someone was going to find a way to complete quantum theory there is no reason that it shouldn't be him. Bohm's thorough examination of the foundations of quantum theory and their physical and philosophical interpretations for the writing of his textbook had put him in a unique position amongst his younger colleagues to understand the nuances of the arguments that Einstein, Bohr, Heisenberg, and others posed as part of the abandoned interpretation debate.

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[490] David Bohm, "A Suggested Interpretation of Quantum Theory in Terms of "Hidden" Variables. I," *Physical Review* Volume 85, Number 2, (January, 1952): 167.
[491] Born, *The Born-Einstein Letters,* 192.

Apart from the goal of introducing determinism into quantum theory, Bohm's hidden variables theory grew directly out of two primary realizations. First, if semi-classical approximations to quantum theory like the Wentzl-Kramers-Brillouin (WKB) approximation were able to establish "virtual" classical trajectories for quantum systems as a way of approximating the system's dynamics, maybe he could figure out a way to do the same exactly without the need for approximations. Again we find that his work on his quantum textbook helped prepare Bohm as he had been particularly concerned with the quantum-classical limit and had therefore dedicated a significant section of the book to treating the various semi-classical approximations to quantum theory. This should not be a surprise to us; after all, Oppenheimer's lectures were a major influence on Bohm's approach to teaching quantum theory. Oppenheimer had worked with Born in Göttingen during the late 1920s which had led to the development of an approximation that came to be known by their names and which was instrumental to spectroscopy.

The second realization was that, according to Bohm, there was a limit to the effectiveness of quantum theory at the "fundamental length" of 10^{-13} cm. For Bohm, this was reason enough to attempt a modification of the theory. He reasoned that:

> As long as the present form of Schrödinger's equation is retained, the physical results obtained with our suggested alternative interpretation are precisely the same as those obtained with the usual interpretation. We shall see, however, that our alternative interpretation permits modifications of the mathematical formulation which could not even be described in terms of the usual interpretation. Moreover, the modifications can quite easily be formulated in such a way that their effects are insignificant in the atomic domain, where the present quantum theory is in such good agreement with experiment, but of crucial importance in the domain of dimensions of the order 10^{-13} cm, where, as we have seen, the present theory is totally inadequate.[492]

[492] Bohm, "A Suggested Interpretation of Quantum Theory in Terms of "Hidden" Variables. I," 166.

The key point that Bohm was pointing to was that the present interpretation was very effective at atomic scales, but that problems dealing with subatomic systems close to the fundamental length would necessitate some modification to the accepted theory. Therefore, his suggested introduction of hidden variables as a modification to quantum theory would ultimately need to return exactly the same results as could be deduced from the use of Schrödinger's equation and also serve to enhance the current theory by broadening its conceptual basis and bridging it into problematic domains.

Bohm published his hidden variables theory in two parts submitted at the same time and published as subsequent papers in the January, 13 1952 issue of *Physical Review* entitled "A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables," I and II. In the first paper (I) he spends the first four pages summarizing the accepted interpretation of quantum theory that he had just finished exploring so thoroughly in his textbook. After identifying the basic postulates and critiques of the theory, Bohm goes on to suggest the need for his novel interpretation based on hidden variables:

> The usual interpretation is admittedly consistent; but the mere demonstration of such consistency does not exclude the possibility of other equally consistent interpretations, which would involve additional elements or parameters permitting a detailed causal and continuous description of all processes, and not requiring us to forego the possibility of conceiving the quantum level in precise terms.[493]

According to Bohm, there are two quantum postulates which while self-consistent within the usual interpretation are problematic and must be reexamined. These postulates are the notion that the probabilistic wave function serves as the most complete description of a quantum system possible and the idea that the transfer from observed system to measuring device must

[493] Ibid., 168.

be "unpredictable, uncontrollable, and unanalyzable."[494] For Bohm both of these can, and should be, challenged as fundamental postulates.

Bohm considered the Schrödinger equation fundamental to quantum theory and did not think that one could simply do away with a mathematical formulation that had proven so effective in the atomic domain for twenty-five years, so he proposed to reinterpret this long-standing equation in a more classically minded way. Beginning with the one-particle time-dependent Schrödinger equation:

$$-\frac{h^2}{8\pi^2 m}\nabla^2\psi + V(x)\psi = i\frac{h}{2\pi}\frac{\partial\psi}{\partial t}$$

Bohm proposed a complex solution $\psi = Re^{i\frac{2\pi S}{h}}$ where both *R* and *S* are real functions. When this solution is substituted into the Schrödinger equation we get the following two equations for *R* and *S*:

$$\frac{\partial R}{\partial t} = -\frac{1}{2m}[R\nabla^2 S + 2\nabla R \cdot \nabla S]$$

And,

$$\frac{\partial S}{\partial t} = -\left[\frac{(\nabla S)^2}{2m} + V(x) - \frac{h^2}{8\pi^2 m}\frac{\nabla^2 R}{R}\right]$$

From these two equations Bohm realized he could arrive at a classical interpretation based on the Hamilton-Jacobi equations if he substituted: $P(x) = R(x)^2$, where *P(x)* is the standard quantum mechanical probability density function. The two equations for *R* and *S* above then become:

494 Ibid., 169.

$$\frac{\partial P}{\partial t} + \nabla \cdot \left(P\frac{\nabla S}{m}\right) = 0$$

And,

$$\frac{\partial S}{\partial t} + \frac{(\nabla S)^2}{2m} + V(x) - \frac{h^2}{16\pi^2 m}\left[\frac{\nabla^2 P}{P} - \frac{1}{2}\frac{(\nabla P)^2}{P^2}\right] = 0$$

Now, Bohm explored the classical limit by letting h→0, which allowed him to interpret *S(x)* as a solution to the classical Hamilton-Jacobi equation. In this way Bohm was able to arrive at classical trajectories of quantum ensembles using, as Madelung had in 1926, the hydrodynamic analogy. He argued that if all trajectories of an ensemble of particles are normal to a given surface of constant *S* then they are normal to all surfaces of constant *S*, and the expression $\frac{\nabla S(x)}{m}$ can then be understood to be the velocity vector *v(x)* of any particle at position x. Substituting the velocity vector back into the equation for *P(x)* above, Bohm obtains:

$$\frac{\partial P}{\partial t} + \nabla \cdot (Pv) = 0$$

which can simply be read as an expression for the conservation of probability density.

In order to arrive at this expression Bohm had taken the classical limit as h→0. Obviously, Bohm was after a more general treatment of quantum systems so he tackled the general case of a nonzero h. In order to do this and still retain the reliable features of the Schrödinger equation, he proposed a “quantum-mechanical” potential U(x) defined as:

$$U(x) = \frac{h^2}{16\pi^2 m}\left[\frac{\nabla^2 P}{P} - \frac{1}{2}\frac{(\nabla P)^2}{P^2}\right]$$

which would act on all particles in addition to any classical potentials, V(x), associated with a quantum system. As a result, the force on a particle would now depend on both classical and quantum potentials:

$$\boldsymbol{F} = m\frac{d^2\boldsymbol{x}}{dt^2} = -\nabla[V(x) + U(x)] = -\nabla\left[V(x) - \frac{h^2}{8m}\frac{\nabla^2 R}{R}\right]$$

Bohm ultimately understood his new approach as a reinterpretation of the Schrödinger equation not as a fundamental alteration to the accepted formulations of quantum theory. Instead of the wave function, ψ, being strictly a mathematical entity with no physical meaning beyond a way to calculate probability densities, Bohm conceived of it as a "mathematical representation of an objectively real field."[495] In this new interpretation of quantum theory the ψ-field could be seen as analogous, but not identical, to other fields in physics.

> In the last analysis, there is, of course, no reason why a particle should not be acted on by a ψ-field, as well as by an electromagnetic field, a gravitational field, a set of meson fields, and perhaps by still other fields that have not yet been discovered.[496]

In addition, Bohm highlighted the point that just as the electromagnetic field is determined by Maxwell's equations, the ψ-field obeys Schrödinger's equation. If the field functions are fully defined at a certain time, for all space, their evolution would be determined for all future times. By extension, one could then calculate the force on a particle at any given time and if the initial position and momentum were known, the particle trajectory would be fully determined. While the usual interpretation limits the possibility of our knowledge of a quantum system by introducing an uncertainty principle, inherent indeterminism, and unanalyzable instantaneous transformations between stationary states, Bohm's interpretation

[495] Ibid., 170.
[496] Ibid., 170.

replaces complementary and seemingly incomplete pictures of reality with a "consistently causal" and fully deterministic approach.

After developing the basic machinery of his new approach to quantum theory Bohm extends his single particle analysis to many particles and then begins to show how it might be applied to experiments such as Franck-Hertz, the photoelectric effect, Compton effect, or barrier penetration (quantum tunneling), which traditionally showed quantum effects that seemingly contradicted our classical understanding. In doing this Bohm showed that his interpretation could lead to the same empirical results as the usual interpretation but done in a way that allows a broader conceptual base and retains physically intuitive classical notions of reality like trajectories and determinism.[497]

While he had presented a new interpretation of the Schrödinger equation based on an analogy with the classical Hamilton-Jacobi equation in this first paper, Bohm claimed that by reexamining the fundamental yet limiting quantum postulates of the usual interpretation, especially the self-consistent ones that limit our ability to know basic parameters of a system by making them *a priori* unanalyzable, he was opening up a door to an "…infinite number of ways of modifying the mathematical form of the theory that are consistent with our interpretation and not with the usual interpretation."[498] In essence, by breaking free of the self-consistent but limiting usual interpretation Bohm saw himself opening up a completely new field of research into alternate formulations that were at least equally powerful in explaining current empirical results and could prove to be fruitful as research moved beyond limiting domains like below the 10^{-13} cm fundamental length.[499]

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[497] Ibid., 175.
[498] Ibid., 179.
[499] Ibid., 178-179.

In his second paper (II) Bohm worked to reinforce his proposed hidden variables interpretation by supporting his previous arguments with more explicit applications of his new methodology and by anticipating possible objections to his new approach. In particular, Bohm dedicates a significant section of this second paper to tackling the problem of measurement in quantum mechanics, explains the quantum correlations that the EPR paper considered paradoxical, and attempts to dismiss von Neumann's general objections to hidden variables theories as irrelevant.

Bohm's conception of a quantum measurement was in a way very similar to Bohr's, but the two diverged on a key point. It was the same in the sense that, as it had been for those partial to the Copenhagen spirit; Bohm assumed that the measuring apparatus was an indistinguishable part of the whole quantum system. Therefore, in all experiments that had been carried out before 1951, measurements had resulted in an uncontrollable disturbance and transformation of the ψ-field. However, the key distinction from the usual interpretation was that for Bohm, the uncertainty principle, and by extension indeterminism, were results of a practical limit of our knowledge of a system, not an inherent quality of the system itself.

While all known acts of measurement may produce uncontrollable and unanalyzable transformations in the ψ-field that lead to uncertainty in those measurements, thanks to introduction of the quantum-mechanical potential and its corresponding force term Bohm claimed it was not inconceivable to consider a measurement that did not disturb the ψ-field. Therefore, in the future with new experimental methods Bohm believed it may become possible to avoid the complementary picture of quantum reality. If the initial position and momentum of all the particles' which make up a system could be measured, Bohm's interpretation would provide a way to fully determine classical trajectories.[500]

After reframing the measurement problem in his own interpretation, Bohm then turned his attention to explaining the EPR paradox in a similarly intelligible way. Interestingly enough he chose not to use the formulation of the problem he had developed in his textbook using spins, and instead reverted to Einstein, Podolsky, and Rosen's original 1935 conceptualization that we saw in chapter five, which relied on correlated particles and their corresponding position and momentum complementary states to show the apparent paradox. First, Bohm explained the EPR paradox from the author's perspective and then briefly mentioned Bohm's response to their claim that the usual interpretation of quantum theory is incomplete. In the new hidden variables interpretation, the apparent paradox can be explained in a way that seemed intuitively satisfactory to Bohm. Assuming an EPR pair of correlated particles in which their position and momentum operators do not commute, Bohm reasoned that:

> …if we measure the momentum of the first particle, uncontrollable fluctuations in the wave function for the system bring about, through the "quantum-mechanical" forces, corresponding uncontrollable changes in the position of each particle. Thus, the "quantum-mechanical" forces may be said to transmit uncontrollable disturbances instantaneously from one particle to another through the medium of the ψ-field.[501]

Finally, Bohm anticipates critiques of his new hidden variables interpretation by dealing with the most obvious one, von Neumann's proof of completeness and his corresponding theorem which disallowed any hidden variables theories to enhance the established interpretation of quantum theory. Bohm dismisses von Neumann's proof as consistent within

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[500] David Bohm, "A Suggested Interpretation of Quantum Theory in Terms of "Hidden" Variables. II," *Physical Review* Volume 85, Number 2, (January, 1952): 184-185.
[501] Ibid., 186.

the usual interpretation of quantum theory but “irrelevant” to his version of hidden variables because he has ultimately redefined both the process of measurement and the notion of observables. In his new interpretation, the notion of an observable corresponds to “potentialities whose precise development depends just as much on the observing apparatus as on the observed system.”[502] In other words, the statistical distribution of the hidden parameters which would be used for a measurement is unique to the measuring apparatus and so the problem which von Neumann was highlighting with this theorem about a simultaneous and exact measurement of conjugate variables does not apply to Bohm’s hidden variables theory.

This “refutation” of von Neumann seems a bit hand-waving. Bohm had previously stated that his initial presentation of hidden variables agreed with the usual interpretation of quantum theory in all empirical respects, and it was therefore subject in all practical sense to Bohr’s complementarity and Heisenberg’s uncertainty principles. However, his new interpretation was based on a broad conceptual base which could conceivably be extended to become fully deterministic if at some future time it became possible to know all initial positions and momenta (hidden variables) precisely. The reinterpretation of the Schrödinger equation had led Bohm to the possibility of a synthetic picture instead of a complementary picture of quantum phenomena and the introduction of the “quantum-mechanical” potential and force were keys to fully determining a system’s trajectory.[503] For Bohm it did not make sense to limit the nature of all future theories based on the limitations of the current accepted theory, so ultimately he understood his hidden variables theory as an effort to reopen the potentialities of future theories.

[502] Ibid., 187.
[503] Ibid., 188.

> …our epistemology is determined to a large extent by the existing theory. It is therefore not wise to specify the possible forms of future theories in terms of purely epistemological limitations deduced from existing theories.[504]

6.9- Reactions to Bohm's Hidden Variables Theory

Bohm's articles were received for publication on July 5, 1951, and while the HCUA proceedings and the subsequent contempt of congress charges were now finally behind him the stigma from the controversy had lasting repercussions on his career. As we saw in an earlier section, Princeton University refused to renew Bohm's contract and other American institutions shied away from hiring him; he was an out of work physicist with few prospects for finding a job, even with recommendations from Einstein, Oppenheimer, and other elite physicists. Finally, due to the efforts of two ex-students working in Sao Paulo, Brazil Bohm was able to secure an academic position.

The bifurcation point in his career must have been very self-evident, one year earlier Bohm had been a gainfully employed physicist working at one of the most elite universities in the world, with direct access to physics royalty like Einstein, Oppenheimer, and von Neumann, and a promising research program in plasma physics. In October, 1951 as he left the United States for Brazil, Bohm was fleeing the fallout from political persecution and leaving for a place he considered academic exile. As his life seemed to be unraveling, one of the few bright spots that he could hold out hope for was that his new interpretation of quantum theory using hidden variables was still waiting to be published and it could yet prove to be an important contribution to the physics community.

[504] Ibid., 188.

Just before the publication of his two papers "A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables," I and II, Bohm was sure that his new hidden variables theory would eventually become a landmark contribution to the foundations of quantum theory but he was unsure about how the "big-shots" who were the staunchest defenders of the Copenhagen spirit would react to his alternate interpretation. In a letter to a friend, Miriam Yevick, on January 5, 1952, Bohm could hardly suppress a genuine mixture of excitement and trepidation:

> I can't believe that I should have been the one to see this…It is hard to predict the reception of my article, but I am happy that In the long run it will have a big effect. What I am afraid of is that the big-shots will treat my article with a conspiracy of silence; perhaps implying privately to the smaller shots that while there is nothing demonstrably illogical about the article, it really is just a philosophical point, of no practical interest.[505]

Before he submitted his papers in July, 1951 Bohm had sent early draft versions of these articles to some of the most celebrated quantum physicists in the world such as Einstein, Bohr, Pauli, and de Broglie. Of these there seem to have been only two preliminary responses from Pauli and de Broglie. According to later recollections from Bohm, he was surprised by de Broglie's quick response informing him that he had attempted a similar approach and had presented it at the 1927 Solvay Council. In his letter, de Broglie, by now a proponent of the Copenhagen spirit, dismissed the possibility of any success via the hidden variables approach as he described Pauli's critique of the theory. Without waiting for a response from Bohm on his critiques, de Broglie went ahead and published a note in *Comptes Rendus* using Pauli's same arguments to criticize the hidden variables approach.

[505] As quoted in Peat, *Infinite Potential,* 113 and 125.

Bohm also heard directly from Pauli who also mentioned de Broglie's 1927 theory and reiterated his opposition to any approach that attempted to remove indeterminism from the foundations of quantum theory. Furthermore, he critiques the draft version because Bohm had failed to extend his interpretation beyond a one-particle system. As a result of the responses from de Broglie and Pauli, Bohm added a section where he extended his interpretation to multiple particles and also added an appendix to the second paper which dealt with these critiques in detail.

After an extensive back and forth via correspondence, Bohm was somewhat pleased that Pauli had finally recognized the self-consistency of his hidden variables interpretation, although he still negated its necessity claiming it to be problematic philosophically. On December 3, 1951 Bohm received a letter from Pauli in which he made this point explicit:

> I also have studied more thoroughly the details of your paper. I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics. … [However,] as far as the whole matter stands now, your 'extra wave-mechanical predictions' are still a check, which cannot be cashed.[506]

In a letter to Einstein later that same month, Bohm informed him that he had been able to convince Pauli about the self-consistency of his interpretation: "It may interest you to know that Pauli has admitted the logical consistency of my interpretation of the quantum theory, in a letter but he still rejects the philosophy."[507] Meanwhile, although Pauli had seemingly been gracious in his correspondence with Bohm, he was highly acerbic when discussing Bohm and his ideas with other physicists. In a letter to a colleague, Pauli referred to Bohm as acting like a

[506] Ibid., 127.
[507] Ibid., 116.

"sectarian cleric" trying to convert him to hidden variables, and characterized the whole Bohmian program as "foolish simplicity…beyond all help."[508]

About the same time that all of this correspondence was going back and forth about Bohm's, as yet, unpublished articles, Richard Feynman visited Brazil for a meeting of the Brazilian Scientific Society as an excuse to practice his bongo drumming. Feynman and Bohm had been friendly for years before his visit to Brazil, but it was at the conference in late 1951 that they became even closer. Bohm spent quite a bit of time with Feynman in which they discussed, among other things, the hidden variables interpretation. In recollections just months after Feynman's visit, Bohm was sure that Feynman had been convinced of the interpretation's self-consistency and its transformative potential declaring he was "…terrifically impressed by it."[509]

By the time the papers were published in January, 1952, Bohm was convinced that he had answered the critiques by de Broglie and Pauli and had been given a boost by the positive reinforcement that Feynman had given him. He was thus cautiously optimistic about how the physics community would respond to his innovation. It was very disappointing for Bohm when after the papers were published there was mostly silence. He was hoping to open up a new dialog about the foundations of quantum theory with Bohr and other proponents of the Copenhagen spirit, but instead there was no direct published response from Copenhagen.

According to Paul Feyerabend, while there was no published response from Copenhagen to Bohm's reinterpretation of quantum theory, Bohr and his colleagues were definitely disturbed by it. Feyerabend was visiting Bohr's institute during 1951-52, and he later recalled an instance after Bohm's papers were published, in which Bohr's colleagues at the

[508] Ibid., 128.
[509] Ibid., 126.

institute attempted to argue against the consistency of Bohm's interpretation and when their attempts failed, they resorted to the fallback argument "But von Neumann has proved…"[510] This would become a common stance to take against Bohm's hidden variables theory. Obviously, his argument against von Neumann's theorem had not been very convincing and most concerned readers must have assumed that von Neumann's "proof" still held.

While Bohr did not respond directly to Bohm one of his most loyal colleagues did. Leon Rosenfeld wrote to Bohm in May, 1952, and in a cutting and patronizing way categorically denied even the possibility that the usual interpretation could be reinterpreted in a deterministic way.

> There is no truth in your suspicion that we may just be talking ourselves into complementarity by a kind of magical incantation. I am inclined to return that it is just among your Parisian admirers that I notice some disquieting signs of primitive mentality.[511]

Bohm was absolutely furious at the lack of public acknowledgement from the physics community of his new theory. For Bohm, the indifference from the physics community was akin to him being told that what he had attempted to do in his hidden variables reinterpretation was not physics at all but merely metaphysics. Pauli intimated as much in his correspondence with Bohm, but Heisenberg was particularly clear in this regard. When considering alternate interpretations to the Copenhagen Interpretation, Heisenberg regarded them as simply repeating "the Copenhagen Interpretation in a different language."[512] In other words, they were not to be considered counterproposals but unnecessary repetitions.

[510] Ibid., 129.
[511] Ibid., 130.
[512] Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony,* 153-154.

One of the most telling responses to Bohm's new hidden variables theory came from his ex-colleagues at Princeton. In 1952 Max Dresden was working as a physicist at the University of Kansas, when his students, intrigued by Bohm's hidden variables theory, formed a study group to consider his new approach. Due to the insistence of his students, Dresden finally considered Bohm's papers and was startled to find that the theory seemed to be self-consistent. In January 1953, while visiting Princeton, Dresden was shocked to discover that Bohm's ex-colleagues were not only generally dismissive of his theory but actually caustic towards it on grounds other than purely scientific arguments. For example, according to Dresden, Oppenheimer was not out of step with the rest of the Princeton crowd when he referred to Bohm's interpretation as "juvenile deviationism" or when he exclaimed that "if we cannot disprove Bohm, then we must agree to ignore him."[513]

Since he had left for Brazil, the continued engagement in the Korean War and the general political climate in the United States had contributed to a trend towards a more acerbic stance against anybody sympathetic to the political left. As a result, McCarthyism was now in full swing. So, with the fear of being labeled communist sympathizers, academics like Oppenheimer and his colleagues at Princeton, were careful to distance themselves from anything even remotely having to do with communism. It is therefore not surprising that long after he had been acquitted of all charges by the federal courts, Bohm was being derided for being a "fellow traveler, a Trotskyite, and a traitor" by physicists in Princeton.

Bohm's anger over the general indifference of the physics community and in particular the caustic response from his ex-colleagues in Princeton boiled over in a letter to friend "[The indifference] cut at one's insides like a hot knife being twisted inside your heart," he wrote, "[I

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[513] Peat, *Infinite Potential,* 133.

have a] passionate desire to fight this stupefying spirit of formalism, and pragmatism in physics."[514] As his isolation in Brazil grew Bohm's feelings of being brushed aside and ignored by his colleagues mixed with his feelings of being betrayed by his country. He began to see the United States' way of life as the main problem:

> I seem to have only one strong emotion left - and that is hatred for the forces that have destroyed so many human beings, including myself. For relative to what I could have been, I regard myself as destroyed. … I cannot forgive [the American way of life] for turning most people into dull, limited creatures that they are.[515]

With all the personal and professional rejection he withstood from the United States, it should come as no great surprise to us that Bohm began to accept, as irreversible, the bifurcation he had suffered in his life. He was no longer a promising young American plasma physicist. It was in Brazil that Bohm began to reject all that America stood for including capitalism and pragmatism and as a result wandered closer to the political ideology of Marxism and redoubled his fight against the prevailing pragmatism within the physics community.

6.10 Reopening the Doors to the Foundations of Quantum Physics

As we can see from the quote at the beginning of this chapter, for J.S. Bell Bohm's 1952 papers were clearly an eye-opening breakthrough. Nevertheless, Bell did not capitalize on Bohm's innovations until twelve years later when he published his landmark paper on the EPR paradox and then subsequently his examination of hidden variables theories two years after

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[514] Ibid., 130.
[515] Ibid., 136.

that.[516] While Bohm's hidden variables theory did not immediately revolutionize the foundations of quantum theory, his hope that his papers would engender a more open dialog on possible interpretations of the conceptual basis of quantum theory eventually came to fruition. Bohm's bold challenge of indeterminism, complementarity, and what he interpreted as complacency within theoretical physics had real effects. It was as a result of Bohm's 1952 papers that Jean-Pierre Vigier, one of de Broglie's assistants at the Henri Poincaré Institute in Paris began a long collaboration with Bohm. Vigier visited Bohm in Brazil and together, they continued his program of challenging the quantum orthodoxy. In fact, Bohm spent the rest of his life developing alternate interpretations of quantum theory as he emigrated from Brazil, to Israel, and finally to England.

Like ripples in a pond the effects of Bohm's papers also eventually reached others outside of his immediate circle. In 1954 Henry Margenau wrote a paper in *Physics Today* entitled "Advantages and Disadvantages of various Interpretations of Quantum Theory," in which he reviewed a number of interpretations, including Bohm's, and then proposed his own more radical interpretation in which he did away with all notions of position.[517] While he dismissed Bohm's interpretation as too reactionary and conservative, his approach certainly challenged the accepted interpretation from Copenhagen. In addition to Margenau's paper, in 1956, Mario Bunge published a paper entitled "Survey of the Interpretations of Quantum Mechanics" in the *American Journal of Physics*.[518] In his survey, Bunge was non-committal on which interpretation he preferred. Nevertheless, what was important for Bunge was that the various different contemporaneous interpretations pointed to a crisis in the very foundations of

[516] J.S. Bell, "On the Einstein Podolsky Paradox," *Physics* 1: (1964): 195-200. And J.S. Bell, "On the Problem of Hidden Variables in Quantum Mechanics," *Reviews of Modern Physics* 38: (1966): 447-452.
[517] H. Margenau, "Advantages and Disadvantages of various Interpretations of Quantum Theory," *Physics Today* 7 (10), (1954): 6-13.
[518] Mario Bunge, "Survey of the Interpretations of Quantum Mechanics," *American Journal of Physics* 24, (1956): 272-286.

quantum theory which should be discussed openly within the physics community. Within four years of Bohm's landmark papers, one of his primary objectives in proposing his reinterpretation of quantum theory had begun to emerge.

Conclusion: So What?

We noted in the introduction that, since David Bohm's death in 1992, something called the "de Broglie-Bohm theory", also known as "Bohmian Mechanics" has become an increasingly popular subject of research amongst physicists and philosophers of science. As a result, significant attention has been paid to the history of the development of this theory and its acceptance and/or marginalization within the physics community. Unfortunately, the majority of existing histories have been authored by non-historians. The inescapable conclusion is that a real opportunity exists to reexamine these two physicists and their place in the history of the development of quantum theory.

In particular, James T. Cushing, a physicist turned philosopher of science was a big proponent of Bohmian Mechanics and wrote a counterfactual or "What if?" history of the quantum revolution. In it he essentially challenged what he understood to be a distorted mythological quantum narrative perpetuated by the historical winners of the quantum revolution- advocates for the Copenhagen Interpretation. However, the alternative that Cushing was offering in his book seemed more like science fiction than historical fact. It is hard enough to reconstruct a thorough context of a particular scientist's innovation without going down the rabbit hole of alternate, counterfactual, and hypothetical histories. Nevertheless, Cushing's challenge to historians is a reasonable one:

> It is astounding that there is a formulation of quantum mechanics that has no measurement problem and no difficulty with a classical limit, yet is so little known. One might suspect that it is a *historical* problem to explain its marginal status."[519]

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[519] James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, (Chicago: University of Chicago Press, 1994), Xii.

In a similar sense, the celebrated physicist John S. Bell pointed to a prevailing quantum mythological narrative that had been perpetuated by the established Copenhagen Interpretation:

> But why then had Born not told me of [de Broglie's] 'pilot wave'? ... Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show us that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice?[520]

Is it reasonable to expect physicists to teach all the theories that have not remained relevant over the centuries on the off chance that one of them will be readdressed, rehabilitated, or re-exploited? Without delving into a tangled and difficult debate about whether there is a need for pedagogical reform in physics, we can state unequivocally that, if our goal is a thorough understanding of the early history of quantum interpretation debates and the relevant place of alternate quantum formulations and interpretations in the development of quantum theory, careful historical reexaminations of Louis de Broglie and David Bohm's particular contributions are absolutely necessary. The purpose of this thesis has been exactly that.

In order to combine an internal history of the particular conceptual developments surrounding de Broglie and Bohm's innovations with a larger external context in which they took place- including an analysis of political climates and the various relevant scientific academic networks- our research focused on the following: a close reading of primary sources, an extensive survey of secondary sources, and the development of a tool for meta-data network analysis. Among the primary sources relied on were: published and unpublished scientific articles, Solvay Council conference proceedings, scientists' personal correspondence, American

[520] John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, (Cambridge: Cambridge University Press, 1987), 160.

Institute of Physics Interviews with the physicists themselves, Proceedings from Congressional hearings of the House Committee on Un-American Activities, David Bohm's archival papers housed at Princeton University's Mudd Library, and his FBI file. Among secondary sources, this study relied on a mix of work from professional historians, physicists, and philosophers of science.[521]

A thorough understanding of the 1920s European quantum physics community is obfuscated by its very nature, a tangled network of physicists that were, in general, highly collaborative and extremely mobile yet politically fractured along nationalistic lines. In attempting to understand the nuances of this academic network, a natural place to start is the new online version of the Dictionary of Scientific Biographies based on the classic original edited by Charles Gillispie and Thomas Kuhn in the 1970s. With this resource one can, in theory, get a quick sense of who studied and collaborated with whom, and when. Unfortunately, while this resource is online and searchable, its search functionality is inadequate for effective cross-referencing as it is limited only to searches of the text within individual biographical entries. This limitation leaves no way to access larger swaths of biographical data and do any significant meta-data network analysis on the larger quantum community. If we can mine the data from sources like the New Dictionary of Scientific Biographies and give researchers access to a research tool that allows them to cross-reference pertinent fields, we would undoubtedly have a very powerful analytical tool. Table 2.1 shows a sample relational table extracted from the Aggregate Biographical Data Repository (ABDR) of quantum physicists who visited or studied at the four major quantum schools- Copenhagen, Göttingen, Munich, and Leiden- during the 1920s, correlated with their respective nationality and age. Using this dataset, we were able to uncover trends within the early quantum community and its academic network.

[521] Please see attached bibliography.

From the ABDR analysis, we found qualitative evidence that after World War I, and throughout the 1920s, there was a high degree of collaboration and scientific exchange between primary European quantum schools in Germany, Denmark, and the Netherlands. We already knew that Allied scientists were particularly intent on instituting isolationist policies against Germany, boycotting German science, and prohibiting German participation in international conferences like the Solvay Councils. However, while one might think the primary German quantum schools in Munich and Göttingen would have suffered as a result of these boycotts, the schools in Copenhagen and Leiden remained neutral and collaborative after the war, thus allowing German science to remain networked and thriving. We found that in the 1920s these four quantum schools attracted and shared many of the brightest physicists from around the world. In addition to these results, comparisons showed that Munich and Leiden tended to serve as feeders to the other two quantum schools in Göttingen and Copenhagen. Maybe the most surprising revelation of all was that, of the 96 physicists tracked through the ABDR, there were no French visitors at any of the four quantum schools during the entirety of the 1920s and the only Belgian was Léon Rosenfeld, who didn't go to Göttingen until after 1927. It seems clear that, ultimately, the Allied isolationist policies backfired as they served to hurt Allied science more than German science! These findings highlight the useful nature of an historical relational database such as the ABDR.

A reexamination of de Broglie's context of innovation, in Chapters Two to Four, revealed discrepancies with the traditional quantum narrative. Let us review some of our study's findings. In reviewing de Broglie's publications from 1923 to 1927, we realized that his research program was sparked, not just from a singular moment of brilliant insight, but arose as a result of three major influences that were years in the making. In particular, his work in his older brother Maurice de Broglie's laboratory was formative and gave him an opportunity to have penetrating discussions about the need for a new model of electromagnetic radiation that synthesized both wave and particle phenomena. Another major influence came from Marcel Brillouin, who published a series of papers between 1919 and 1922 in which he attempted, and failed, to use a hydrodynamic model in accounting for the quantized electronic orbits in the Bohr-Sommerfeld atom.

In the summer of 1923 Louis de Broglie was contemplating the quantization of atomic electronic orbits when he came to the realization that the Bohr-Sommerfeld quantum numbers, indicating the allowed orbits for atomic electrons, seemed to be something associated more with interference phenomena than with particle phenomena. This insight led him directly to the use of Hamilton's 19th century optical-mechanical analogy, or in de Broglie's nationalistic interpretation: the equivalence of Fermat's optical principle (17th century) and Maupertuis' mechanical principle of least action (18th century). De Broglie's insights in this regard have not been highlighted sufficiently in traditional narratives. After all, this analogy and derived equivalency of optical and mechanical principles resonated with his desire for a synthetic quantum picture and became the core of all his subsequent alternate wave-mechanical formulations and interpretations of quantum theory. It was in large part thanks to his training as a historian and his isolation from the primary quantum schools that he was able to reinterpret these centuries-old principles. These findings serve to bolster the conclusion that Kuhn's concept of incommensurability between successive paradigms is a theoretical framework of questionable general validity and applicability to the history of innovation in the physical sciences.

A reexamination of de Broglie's doctoral thesis and its dissemination throughout the quantum community shows much more than a simple derivation of his famous electron wavelength relation: $\lambda = h/p$. In fact, his thesis was 130 pages long and was divided into seven chapters covering a rather large breadth of analysis. In addition, as a trained and licensed historian, he included a lengthy historical introduction in which he traced the origins of wave-particle duality and Hamilton's analogy. In order to explain the newly posited wave-like aspects of the electron, he introduced a fictitious "phase-wave" which propagated along in phase with the electron's physical motion. As a result, de Broglie realized that the wave packet must have a group velocity that was equal to the electron's physical velocity. He argued that it was these phase-waves with their corresponding "mobile singularities" that ultimately dictated the wave-like interference effects of particles. Just as geometrical optics was an approximation of electrodynamic modeling, he considered the electron's particle trajectory to be an approximation of this phase-wave modeling.

When one understands the true extent of de Broglie's dissertation, and juxtaposes it with Schrödinger's 1926 development of his quantum wave-mechanical theories, it becomes clear that he relied on de Broglie's insights much more than the traditional quantum narrative lets on. So one may be inclined to ask: Why did de Broglie not develop Schrödinger's wave mechanics? As it turns out, de Broglie was busy developing his own brand of deterministic wave mechanics during the years 1923 through 1927. We should also note that he was not alone in this pursuit. From 1926 to 1927 there were multiple parallel attempts to adapt wave mechanics into a deterministic interpretation of quantum theory. Among these was an unpublished paper by Einstein written in the spring of 1927. When seen as an integral research program, we conclude that apart from Schrödinger's wave mechanics, de Broglie's wave mechanical research program was without a doubt the most extensively developed during the 1920s.

In this new light, the Solvay council of 1927 seems very different from the one we often read about. It was not just two warring camps that fought over determinism. That was certainly part of the story, but Solvay also represented a normalizing force for the physics community. The Solvay Councils met every three years and 1927 was the first time since 1913 that Germans were invited to participate. This was a significant accomplishment and one which Lorentz, as the elder statesman of physics, had struggled with for years. As a result, he was careful and deliberate in planning Solvay 1927. It took him 18 months of careful negotiations to ensure that the conference would be balanced and representative along national and scientific lines. Reacting to the muddled and fractured state of quantum formulations and interpretations, Lorentz did not limit himself to just two presentations on quantum mechanics representing deterministic and non-deterministic camps. Instead, he asked for three. Heisenberg and Born shared a paper focusing on what was becoming the most polished and fruitful approach to quantum theory. The two eloquently and thoroughly described the latest advances in theory combining their matrix mechanics, with Born's probabilistic interpretation of Schrödinger's wave function, Heisenberg's Uncertainty Principle, and Dirac's Transformation Theory. On the other hand, Schrödinger and de Broglie presented on their two differing approaches to wave-mechanics.

After the Council closed it became clear that the building momentum and consensus was favoring the Göttingen approach. It was after this 1927 Solvay Council that de Broglie finally chose to abandon his research program and accept the indeterminism that was inherent in the interpretation becoming known as the "Copenhagen Spirit". This transition was not simply a quick reaction to some caustic attack at Solvay, but more so due to the thoughtful realization that his wave mechanics could not compete with the analytic power of the quantum mechanical formulation that had been refined in Göttingen and Copenhagen. This change by de

Broglie illustrates not only his individual agency in making a deliberate transition but also the importance of a rising pragmatism within the quantum physics community. In Chapter Five we discuss the rise of pragmatism and the corresponding lull in interpretation debates from the late 1920s through the late 1940s.

This trend of pragmatism in the sciences was accompanied by many other related trends including: the abandonment of interpretation in favor of a congealing of quantum orthodoxy (Copenhagen Spirit), the practical applicability of standard quantum mechanical formulations as powerful analytical tools, the rise of big science and its industrial and government patronage, the emigration of European physicists west especially to the U.K. and the U.S., and an increased influence of the American physics community on the world stage. As such, our reexamination of interpretation debates shifts to the late 1940s and David Bohm's influence on reopening these mostly abandoned lines of inquiry in Chapter Six.

In June 1947 the American Academy of Sciences sponsored the now famous Shelter Island Conference. Modeled in part after the famous Solvay Councils, this elite conference was designed specifically to give a limited number of the brightest young physicists, among them David Bohm, Richard Feynman, Julian Schwinger, Abraham Pais, and Robert Marshak an opportunity to exchange ideas about the foundations of quantum theory in a relaxed informal setting. In the lead up to the conference Victor Weisskopf, excited about the prospect of reengaging his fellow physicists on persistent concerns within the foundations of quantum theory, made explicit reference to the lull of interpretive activity that had gripped the quantum physics community over the previous two decades:

> [I]t is a good sign that somebody is again interested in discussing the foundations of quantum mechanics instead of thinking only of high voltage machines and how to produce mesons. The whole idea of a few quiet days in the country together with

> Heisenberg's 'Uncertainty Relations' seems to me extremely attractive, and reminds me of wonderful days twenty years ago in Göttingen or Copenhagen.[522]

In the late 1940s Bohm was part of an elite group of young American physicists that were intent on addressing persistent problems with the foundations of quantum theory. While Feynman, Schwinger, Dyson, Tomonaga, and Bethe worked out the divergence problems in quantum electrodynamics by utilizing renormalization techniques, these "fixes" were intended to be extensions of the accepted Copenhagen formulations and interpretations of quantum mechanics. On the other hand, David Bohm's hidden variables program became, in many ways, a much more ambitious endeavor as the young American decided to challenge some of the very postulates that the accepted interpretation was founded on.

While the immediate reaction to Bohm's 1952 hidden variables papers ranged mostly from neutral indifference to caustic attacks of "juvenile deviationism,"[523] there were a few theorists that took notice and, like ripples in a pond, the effects eventually revealed themselves in surprising ways. One of Bohm's earliest collaborators was Jean-Pierre Vigier who visited the American exile in Sao Paulo, Brazil to work on extending his hidden variables theory. As it happens, Vigier was a student of Louis de Broglie's in Paris and eventually persuaded de Broglie to reengage in the interpretation debate. While de Broglie had initially been dismissive of Bohm's 1952 papers, he eventually became a rejuvenated determinist.

Of those that took notice of Bohm's innovative hidden variables theory, John S. Bell was the most visibly outspoken proponent. It was due to Bell's work dispelling John von Neumann's seemingly iron-clad "impossibility proof" in the mid 1960s that placed Bohm's work on hidden

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[522] Silvan S. Schweber, "Shelter Island, Pocono, and Oldstone: The Emergence of American Quantum Electrodynamics after World War II," *Osiris*, 2nd Series, Vol 2., (1986): 275.
[523] Peat, *Infinite Potential,* 133.

variables in the foreground of later quantum interpretation debates. As Bell recalled, his reaction to Bohm's breakthrough was one of shock and amazement:

> But in 1952 I saw the impossible done. It was in papers by David Bohm. Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one.[524]

If we are to truly understand where Bohm's hidden variables interpretation came from we need to study the coupling between both the context surrounding the innovation and the development of the theory itself during the spring of 1951. Existing Bohmian narratives either ignore or oversimplify this innovative process. General consensus is that while Bohm was suspended from Princeton, he finished editing his orthodox quantum textbook and sent it to Einstein, who then asked to meet with him at the Institute for Advanced Study. It was after these discussions that Bohm, inspired by Einstein, apparently proceeded to reject the orthodox interpretation and develop his hidden variables theory from scratch. This overly simplistic explanation for his innovation seems unsatisfactory.

There is no doubt that Bohm's meetings with Einstein were influential but there were other important influences that should be accounted for. Without taking all of these factors into account, it seems difficult to adequately explain the sharpness of Bohm's interpretational turnaround. After all, how could someone go from being an entrenched advocate of indeterminism and the accepted Copenhagen Interpretation to, seemingly out of nowhere, developing an extensive deterministic hidden variables theory in a matter of weeks? My findings reveal other pre-existing factors and influences and point to a different reading of Bohm's innovation. After studying the coupling point between Bohm's biography and his 1951

[524] John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, (Cambridge: Cambridge University Press, 1987), 160.

innovation in considerable detail, first building the context of his suspension and then proceeding to study both his Quantum Theory text book and his subsequent hidden variables papers, it has become clear that an explanation of this seemingly sudden and dramatic transformation is possible. In fact the seeds of this transition were present well before 1951.

First, at Berkeley Bohm was an avowed proponent of classical mechanics, not liking the implications brought on by a loss of determinism. It was really after many debates with fellow graduate students and a chance to teach Oppenheimer's quantum mechanics class while his advisor was away at Los Alamos that led Bohm to tentatively accept indeterminism. This all happened fairly late in his development as a physicist, so it is not surprising that his change of heart regarding orthodoxy did not take. Second, the opportunity to participate in the elite Shelter Island and Pocono conferences where the foundations of quantum theory, and its persistent problems, were being discussed should not be overlooked as a stimulant to Bohm's innovations. Being an eye witness to Feynman and Schwinger's presentations on renormalization techniques were truly inspirational for Bohm. Third, a close reading of his supposedly "orthodox" account of quantum theory presented in his *Quantum Theory* textbook reveals that it actually contained the seeds of his subsequent hidden variables theory. While Bohm did not directly advocate a deterministic theory in his book, he was careful and explicit in leaving the interpretational door open. Lastly, there is little doubt that his political persecution and academic exile were formative in his emotional and psychological mindset. As a result, Bohm was more naturally inclined to be combative because of the way the HCUA and the Princeton University administration had persecuted him and his friends. Only when keeping all of these latent potentialities in mind can we explain, in a comprehensible way, how an encounter between Einstein and Bohm at the Institute for Advanced Study in the spring of 1951 could spark Bohm's subsequent development of an innovative hidden variables theory.

As a result of reexamining the emergence and development of alternate formulations and interpretations of quantum theory from the 1920s to the early 1950s, new insights have emerged which can serve to correct certain misconceptions and deepen our understanding of the development of quantum physics. Specifically, Louis de Broglie's alternate wave mechanical interpretation (1923 - 1927) and David Bohm's hidden variables program (1951 - 1952) were examined within their respective contexts of innovation. While de Broglie and Bohm were ultimately interested in restoring determinism to quantum theory, their particular influences, arguments, methodologies, approaches, and receptions were unique to their respective historical periods and each physicist's position within the physics community. Combining a close reading of the two distinct programs of scientific innovation with a representative analysis of the historical context in which they worked allows us to reexamine de Broglie and Bohm in a new light. This study has served to restore these two physicists' agency in carving out their particular places within the physics community and has dispelled simplistic myths of marginalization. It has also shed light on de Broglie and Bohm's important contributions to the early quantum mechanical interpretation debates, thereby restoring their importance within the wider historical quantum narrative.

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