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## Special Issue – Historiography of Physics

### Power Relations in Science: The Bohr and Wheeler-Everett Dialogue on the Foundations of Quantum Mechanics<sup>1</sup>

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#### Abstract:

Pierre Bourdieu challenged the notions of science when he presented it as a field of peers competing for the monopoly of scientific authority. As scientific capital equals power, science disputes become disputes for power. Yet, simultaneously, those disputes occur within the internal logic and language of the scientific field. In this article, we present those ideas and examine a case study within the history of quantum mechanics, a dispute inside the ongoing controversy about the foundations of quantum mechanics. We present the Wheeler-Everett and Bohr dialogue in terms of Bourdieu's sociology of science and discuss the insights that such ideas can bring into the history of science.

**Keywords:** Pierre Bourdieu; Hugh Everett; Niels Bohr; History of Physics; Quantum Mechanics

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## Introduction

It is well established in the science studies that scientific controversies are privileged moments for the analysis of scientific production (Latour, 2000). This privilege is even more pronounced whenever the participants in the controversy have very unequal positions with regard to the prestige in the scientific communities involved in the controversy, as these cases can also show the existing power relations in what Bourdieu (1975) called the scientific

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field. The case of the controversy over the foundations and interpretations of quantum physics – an episode in the history of physics that spanned throughout the 20<sup>th</sup> century – serves to discuss both the concrete circumstances of the production of science and the power relations between scientists.

Quantum mechanics has become one of the most fascinating themes of contemporary science outside the academic circles for two reasons: first, for the impressive technological development it allowed and, therefore, the great impact it had on the life of society and people; second, for the permanent philosophical “revolution” that went with it, which was the result of an unceasing dissatisfaction with the possible lessons that one may extract from its foundations for the understanding of its meaning. The latter reason, considered as a historical episode in its own, helps us understand the dynamics of scientific practice and social relations within science during the 20<sup>th</sup> century.

Quantum mechanics was established in 1925–1927, but physicists never reached an agreement about the meaning of its formalism and its underlying worldview. Indeed, historical accounts seem to suggest three periods for the controversy regarding the foundations of quantum theory. The first period goes from 1925 to the early 1950s. The Copenhagen interpretation – developed by Niels Bohr and his collaborators – was then dominant among physicists and there were just a few dissidents, among them Albert Einstein and Erwin Schrödinger. The historian Max Jammer called that first period the “monocracy of the Copenhagen school in the philosophy of quantum mechanics” (Jammer, 1974, p. 250). One of us has suggested two other periods (FREIRE, 2003; 2004; 2015). The third and last period begins in the early 1970s and seems to continue to this day, is characterized by an institutionalized controversy, in which debates about the quantum foundations are accepted as part of mainstream physics and specialized journals and meetings guarantee the circulation of its researches. The second period – from which we will take a case study as the object of the present work – became known as a transition period, when the Copenhagen monocracy began to be undermined and new attempts to interpret quantum mechanics were developing, but not without great resistance from the physics community.

During this transitional period, Hugh Everett III, a young doctoral student under the guidance of John Archibald Wheeler, developed the relative-state interpretation of quantum mechanics, now known as the many-worlds interpretation. This interpretation is so widespread that its fifty anniversary in July 2007 was the subject of *Nature's* – the world's leading scientific journal – cover and editorial. However, when Everett developed his interpretation, it did not arouse great interest in the scientific community. More than that, his interpretation – which seemed to bring original contributions on the nature of the quantum domain – suffered a severe blow when Wheeler decided to put it, still in a first draft of the PhD thesis, into discussion with Niels Bohr. Wheeler expected to get Bohr's endorsement and to publish the thesis in the proceedings of the Danish Academy of Sciences; however, the reception by Bohr and his collaborators in Copenhagen was extremely negative. They did not recognize the problems that Everett aimed to solve and considered that their own interpretation – the Copenhagen interpretation – was already capable of covering all interpretive issues, closing the way for new candidates.

We already discussed, in other articles, the historical, conceptual, and historiographic dimensions of these episodes. In this article, we want to examine how power relations in the field of physics influenced the young Everett's career and the fate of his interpretation. In fact, disenchanted with the obstacles posed to the acceptance of his interpretation, Everett abandoned the physics research and went to work at the Pentagon. His interpretation remained forgotten for more than ten years.

An analysis of power relations in science in situations of scientific controversy suggests the use of Pierre Bourdieu's notion of scientific field. Coincidentally, one of the first uses of the notion of scientific field in the analysis of science discussed events closely related to our theme: Pinch (1977) analyzed the challenge posed by David Bohm to a famous mathematical

proof (against the existence of alternative interpretations of quantum mechanics) that had been formulated by the John von Neumann.<sup>5</sup>

In section 2 of this paper, we present the notion of scientific field as formulated by Bourdieu and discuss its use by Trevor Pinch. In section 3, we summarize the historical events that will be analyzed in the following section. In section 4, we re-read Everett's case in light of the idea of scientific field. In the last section, we discuss the importance of a clear approach to subversion strategies in science and conclude by discussing how a theory of power allows us to understand the dynamics of science, overcoming the externalism-internalism dualism.

## Pierre Bourdieu's Scientific Field

Bourdieu elaborated his notion of scientific field in contrast with the notion of scientific community as it appeared in the works of Robert K. Merton and Thomas S. Kuhn. Instead of a community of peers, Bourdieu highlights one of the inherent characteristics of the scientific community: Competition. For him,

As a system of objective relations between positions already won (in previous struggles), the scientific field is the locus of a competitive struggle, in which the specific issue at stake is the monopoly of scientific authority, defined inseparably as technical capacity and social power, or, to put it another way, the monopoly of scientific competence, in the sense of a particular agent's socially recognised capacity to speak and act legitimately (i.e. in an authorised and authoritative way) in scientific matters. (Bourdieu, 1975, 19)

Thus, once unveiled the struggles inherent in the scientific community, formed by competing pairs, the term community gives rise to the "scientific field" notion, which, for Bourdieu, is not an explanatory scheme designed to account only for certain aspects of scientific activity, excluding from the explanation the properly cognitive contents of science. Strictly speaking, the very distinction between the historical, conceptual, and power-relation dimensions, formulated by us in the introduction of this work, would be foreign to Bourdieu's thought.

An analysis which tried to isolate a purely "political" dimension in struggles for domination of the scientific field would be as radically wrong as the (more frequent) opposite course of only attending to the "pure", purely intellectual, determinations involved in scientific controversies. For example, the present-day struggle between different specialists for research grants and facilities can never be reduced to a simple struggle for strictly "political" power: in the social sciences, those who in the USA have reached the top of the great scientific bureaucracies (such as the Columbia Bureau of Applied Social Research) cannot force others to recognise their victory as the victory of science unless they are also capable of imposing a definition of science implying that genuine science requires the use of a great scientific bureaucracy provided with adequate funds, powerful technical aids, and abundant manpower; and they present the procedures of large-sample surveys, the operations of statistical analysis of data, and formalisation of the results, as universal and eternal methodology, thereby setting up as the measure of all scientific practice the standard most favourable to their personal or institutional capacities. Conversely, epistemological conflicts are always,

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<sup>5</sup> It is interesting to notice the influence of Bourdieu's sociology among the protagonists of the new sociology of science when it was still in its first moments. This interaction has reduced over time, as can be noticed in Bourdieu's last course at the *Collège de France*, where he presented an appraisal of the sociology of science that was quite critical of the authors close to the Edinburgh School and, as well, of more recent authors, like Bruno Latour (Bourdieu, 2001).

inseparably, political conflicts: so that a survey on power in the scientific field could perfectly well consist of apparently epistemological questions alone. (Bourdieu, 1975, 21)

Bourdieu denaturalizes, in this way, the most elementary procedures of scientific activity. “Every scientific ‘choice’ – the choice of the area of research, the choice of methods, the choice of the place of publication – (...) is in one respect – the least avowed, and naturally the least avowable – a political investment strategy, directed, objectively at least, towards maximisation of strictly scientific profit, i.e. of potential recognition by the agent’s competitor-peers.” (BOURDIEU, 1975, 22-3). For Bourdieu, this recognition is the symbolic, immaterial capital proper to the scientific field and, therefore, called scientific capital, which can be accumulated, inherited, or acquired, just like the economic capital.

It is the accumulation of scientific capital that guides the researcher’s “choices” and expectations. Prestige, understood by Bourdieu (1984, 9) as “[...] one’s position in strictly intellectual or scientific hierarchies,” is the result of a successful investment and is conferred by the agents of the field, especially by those who hold scientific authority. That is, the more scientific capital an agent has, the more capital he can provide, securing other agents in the field. The initial capital (for instance, school, economic, and cultural capital), although relevant to the acquisition of scientific capital in the market for scientific symbolic goods, is not determinant in itself; therefore, the subjects’ strategies are also fundamental to obtain a symbolic profit.

Thus, the entry of a newly graduated scientist in a scientific field is a crucial moment in defining his career, since it implies choices that will define his struggle strategies in the field. According to Bourdieu, roughly speaking, the young scientist (the “new entrant”) must choose a succession strategy or a subversion strategy. This choice will be conditioned by his previous insertion in the structure of the field itself. In the case of a recent doctor, for example, the strategy is conditioned by the prestige of the institution in which he graduated and of his advisor. These two strategies can be defined, according to Bourdieu, in this way:

It is the field that assigns each agent his strategies, and the strategy of overturning the established scientific order is no exception to this. Depending on the position they occupy in the structure of the field (and also, no doubt, on secondary variables such as their social trajectory, which governs their assessment of their chances), the “new entrants” may find themselves oriented either towards the risk-free investments of succession strategies, which are guaranteed to bring them, at the end of a predictable career, the profits awaiting those who realise the official ideal of scientific excellence through limited innovations within authorised limits; or towards subversion strategies, infinitely more costly and more hazardous investments which will not bring them the profits accruing to the holders of the monopoly of scientific legitimacy unless they can achieve a complete redefinition of the principles legitimating domination : newcomers who refuse the beaten tracks cannot “beat the dominant at their own game” unless they make additional, strictly scientific investments from which they cannot expect high profits, at least in the short run, since the whole logic of the system is against them. (Bourdieu, 1983, 138)

The risky option for the use of subversion strategies brings the burden of a reconfiguration of positions in the scientific field for them to be successful. However, this possibility is more viable as the larger the scientific capital accumulated by the scientist who makes this bet. For Bourdieu (1984), this choice is given to new entrants who, in addition to accepting the rules of the competitive game in the scientific field, have a symbolic capital that gives them prestigious positions. Although more costly, investments in successful subversion

strategies can generate significant gains, such as the accumulation of scientific capital and, with the reconfiguration of the field, the monopoly of scientific authority.

In his analysis of the implicit challenge in the alternative interpretation of the quantum theory formulated by David Bohm in 1952, Pinch's focus was precisely the distinction between these two strategies (Pinch, 1977). He argued that Bohm successfully pursued a succession strategy after finishing his PhD at Berkeley under Robert Oppenheimer, who had been the scientific director of the Manhattan Project. Bohm then worked as a professor at Princeton University and did some relevant work in plasma physics. In 1952, however, he changed his strategy to a subversive one by publishing an alternative interpretation to the interpretation of quantum mechanics. Pinch questioned whether this change was suitable for Bohm, as it would necessarily trigger a succession strategy between the defenders of the dominant position among physicists, namely, the defenders of von Neumann's proof.

Though interesting as a pioneering attempt at a sociological analysis of the very contents of science, Pinch's analysis has its limitations, as we have pointed out (Freire, 2005, pp. 26-27). His historical analysis did not realise that Bohm's interpretation represented an even greater challenge to dominant positions in the field of physics – the challenge to the von Neumann's proof was only part of a much larger challenge. After all, Bohm's proposal implied replacing a probabilistic interpretation of quantum phenomena, such as that supported by the Copenhagen school, with a causal interpretation of quantum mechanics. The historical analysis, moreover, reveals that the greatest critics of Bohm's interpretation – those aligned with the Danish physicist Niels Bohr – used little of von Neumann's proof argument when rejecting his proposal.<sup>6</sup>

## Case Study: A Summary of the Dialogue between Bohr and Everett-Wheeler<sup>7</sup>

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The youngest character in our narrative graduated in chemical engineering at the Catholic University of America, in Washington, DC, *magna cum laude*. After graduating, Everett decided to pursue a PhD in physics at Princeton University. He requested a letter of recommendation from his graduation professor, William Boone, for his doctorate selection. Boone depicted Everett as a true genius and added: "This is a once-in-a-lifetime recommendation for I think it is most unlikely that I shall ever again encounter a student I can give such complete and unreserved support."<sup>8</sup> Boone goes on to state that of all the students he has ever had contact with, Everett was by far the best. "Everett has a better knowledge of mathematics than most of the graduate students at Catholic University and probably no graduate student is his equal in native ability." That mathematical ability allowed Everett, even during his undergraduate studies, to attend several classes in advanced mathematics – some of them only as a listener, due to a university rule concerning the amount of credits hours in which students could enroll. In this way, even majoring in chemical engineering, he attended so many classes that he could have received a *major* in mathematics. In short, the impression we have read the letter of recommendation is that Everett was a brilliant student. Due to this academic record, Everett received a *National Science Foundation* graduate

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<sup>6</sup> It is curious to note that years later Pinch criticized Bohm for defending, according to Pinch, an excess of creativity in science (Freire, 2019, 188)

<sup>7</sup> The historical outline developed here is based on Freitas (2007), Freitas and Freire (2008a; 2008b), and Osnaghi, Freitas and Freire (2009), that was reprinted in Freire (2015, chap. 3). For a biography of Everett, see Byrne (2010).

<sup>8</sup> Letter from William Boone to Hugh Taylor, Dean of Graduate Studies, April 17, 1953. Alumni File of Hugh Everett III, Seeley G. Mudd Manuscript Library, Princeton.

scholarship.<sup>9</sup> Even though it did not grant him a free pass into the graduate program entrance exams, it certainly helped his admission. The scholarship, in attesting his quality, in other words, increased his symbolic capital, being explicitly mentioned by his selection committee. As a result, he was accepted in the Princeton PhD program in Mathematics. About a year later, in 1954, he transferred to the physics department. John Archibald Wheeler then became his advisor.

Wheeler had received his PhD in physics in 1933 from the prestigious John Hopkins University. During the 1930s, he gave important contributions to theoretical physics. His 1939 liquid-drop model, developed with Niels Bohr, played an important role in understanding the nuclear fission process, which later was a fundamental step in the construction of the atomic bomb. As an expert in nuclear physics, Wheeler worked intensively on the Manhattan project, like most of the great American physicists of the time. Later on, he became an important character in the construction of the American H-Bomb. He joined Princeton University as a professor in 1938. Thus, in the 1950s, Wheeler was already a renowned physicist. However, in this period he decided to change his research focus, moving from nuclear physics to general relativity, which was, at that time, a less prestigious field, without the attraction power of nuclear physics. Wheeler was, in the 1950s, one of the main responsible for restructuring the research on general relativity and cosmology (Blum et al. 2015; Rickles, 2018).

When Everett approached Wheeler, Wheeler's main research interest was not quantum mechanics, but rather the quantization of gravitational interaction, which aims at unifying the General Theory of Relativity with quantum theory. However, Everett was not interested in Gravitation nor cosmology, but rather in providing a new interpretation for quantum mechanics. In any case, Wheeler's style of doing physics had already led him previously to guide Richard Feynman in the development of a different mathematical formulation of quantum theory (the so-called path integral formulation), and thus he was apparently open to advise another student on the foundations of quantum theory.

In 1954, Everett's ideas about quantum theory were not fully developed yet, which only happened a year later. In this process, it is possible to clearly identify Wheeler's style: Everett's goal was to develop an interpretation without any additional postulate, just following what the equations say, taking them to their extreme. Wheeler has the same approach to the equations of general relativity. By taking these equations to their extreme, he arrived at important results, for example, with black holes. Thus, even though the original idea was really Everett's, his research development bears a strong imprint of Wheeler's style, which developed in the latter a kind of father's affection with that interpretation.

In 1955, with more clearly developed ideas, Everett began to put his interpretation down on paper. During the writing of the thesis, Wheeler probably suggested that Everett should present his ideas less technically, reducing as much as possible the mathematical formalism. In September 1955, Everett delivered the first draft of the thesis to Wheeler – a version that consisted of three short papers, that were never published – , whose answer could not be more direct: “I am frankly bashful about showing it to Bohr in its present form, valuable & important as I consider it to be, because of parts subject to mystical interpretation by too many unskilled readers.”<sup>10</sup> In fact, Wheeler considered the work to be of great value, an assessment that he maintained throughout the process, but the important question is: why should he show it to Bohr?

Niels Bohr was one of the most influential physicists of the 20th century. He was born in Denmark in 1885 and received the Nobel Prize in 1922, for his 1913 atomic model. After

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<sup>9</sup> *National Science Foundation* scholarships are not linked to courses, so Everett would have the scholarship even if he went to a university other than Princeton.

<sup>10</sup> “Probability in Wave Mechanics,” Everett Papers, Box 1, Folder 6. The answer is a handwritten note from Wheeler to Everett, September 21, 1955, Everett Papers, Box 1, Folder 5, American Institute of Physics, College Park, MD.

achieving a great prestige in the physics community, he created in 1921 the Institute for Theoretical Physics in Copenhagen, with the funding support of the Carlsberg Foundation. This institute soon became one of the world's leading centers for quantum physics. During the period of establishment of quantum mechanics, between 1925-27, Niels Bohr had an important role, as the proponent of the Complementarity Interpretation of that theory, which became practically hegemonic among physicists until the 1950s. His debates with Albert Einstein, which lasted from 1927 to 1935, contributed to Bohr's prestige – both were then two of the greatest physicists alive. Einstein opposed, in particular, one of the fundamental aspects of the new theory, the so-called Uncertainty Principle, using thought experiments to show that this principle was incorrect (or, more precisely, could be violated). Bohr advocated the theory, pointing out the flaws in Einstein's reasonings, which made his imagined situations not feasible and showed that the new theory was consistent and adequate to describe atomic phenomena. Later, in 1935, Einstein and two collaborators, Nathan Rosen and Boris Podolsky, developed what later was considered his mature critique of quantum theory. He came to accept that the theory was correct, but claimed that it was nevertheless incomplete, since it did not contemplate certain aspects of the physical reality. Bohr published an answer that same year. He criticized Einstein for not appreciating the contextual aspect of the experimental situation. Einstein did not push the discussion forward, even though he never fully accepted Bohr's answer. Therefore, Bohr was seen, afterwards, by the physics community as having solved all the interpretive problems of quantum mechanics. The Copenhagen hegemony was so well established that the most common view among physicists, until the early 1950s, was that there were no interpretive problems in quantum theory and that the Complementarity Interpretation was, in fact, not an interpretation, but part of the theory itself. Moreover, in addition to his contribution to physics and its epistemological problems, Niels Bohr was considered a great charismatic leader (Beller, 1999).

In addition to the reputation that Bohr had in 1955 concerning the interpretation of the quantum theory, he had written, in collaboration with Wheeler, that 1939 article on nuclear fission. Wheeler had been a postdoctoral researcher in Copenhagen in 1934, under the guidance of Bohr, and they were good friends ever since. Thus, it is understandable that Wheeler wanted to show his student's work to Bohr: not only was Bohr the greatest authority on the foundations of quantum theory, but he had been his mentor. However, Wheeler did not consider that Everett's first draft of the thesis was worthy of being shown to Bohr, due to the way in which certain results were presented. Everett should first improve his presentation.

He transformed those three short unpublished papers into a long thesis, which was sent to Copenhagen in April 1956, even though it was considered still a draft by both Everett and Wheeler. All the results were already there. Everett presented his interpretation in detail, with a long formal development, then exposed some problems with the foundations of quantum theory, and finally suggested six alternative approaches to the interpretation of the quantum theory, including Bohr's and his own. After showing that his own interpretation was formally consistent, he argued that it was also the most appropriate.

In 1956, Wheeler spent six months in Holland, at the Leiden University. Just before traveling, he sent that long thesis draft of Everett's to Bohr and, a few weeks later, went in person to the Institute for Theoretical Physics in Copenhagen to discuss the matter with Bohr. In a previous work, we argued that this informal moment was, in fact, Everett's first PhD committee (Freitas and Freire, 2008a). Wheeler had two goals when he sent Everett's thesis to Bohr: the first was to get his friend's assessment of his student's work, which he considered to be new and of great value; the second, and most important, was to obtain Bohr's endorsement in order to publish the thesis in the proceedings of the Royal Danish Academy of Sciences and Letters. According to Wheeler, "I feel that acceptance in the Danish

Academy would be the best public proof of having passed the necessary tests.”<sup>11</sup> However, this desire ended up frustrated and the thesis was not even submitted for publication in the Academy. Bohr, along with his collaborators in Copenhagen, rejected the new interpretation from the very beginning, but the discussion process involving Wheeler, Bohr and Everett lasted for several months. After a first conversation in Copenhagen, Wheeler wrote to Everett indicating that the objection to his interpretation was a matter of wording and that his development of the mathematical formalism remained unshaken. Wheeler insisted that Everett should spend a few months in Copenhagen to fight with the greatest of fighters, humbly accepting criticism, but insisting on the fundamental points that formed the core of his new interpretation. Everett ended up going to Copenhagen only much later, in 1959, so the discussion continued in 1956 through personal correspondences. At the end, Bohr’s position remained unchanged. According to him, Everett’s work did not bring novelties to quantum theory and Everett himself did not fully understand several aspects of the theory.

Everett’s work was greatly affected. Wheeler, upon returning to the United States, insisted that Everett should write a new version, which was much more neutral than the original one and much less critical of Bohr’s interpretation, claiming to be simply a generalization of Bohr’s approach. In fact, right in the introduction of that final version of the thesis, Everett is concerned with saying that his new interpretation is not a radical break with the traditional one and that it would be possible to derive the latter from the former. The presentation, as a whole, was greatly affected. The version sent to Copenhagen in 1956 was about 130-page long, while the final version – that was defended in March 1957 and published a few months later with minor changes in style – was only 30-page long. The journal chosen for publication was the *Reviews of Modern Physics*, which was an important journal, but was obviously a modest choice for a text that might revolutionize physics. There were other journals more suitable for unprecedented and important results, even more as it was not a review article. In addition, the article was published in a special issue, in the middle of the proceedings of a conference on gravitation, further reducing the visibility of the text. In the end, for more than ten years the text did not arouse the interest of other physicists.

Indeed, that was Everett’s only publication in physics. Although the long version of his thesis was published in 1973, his participation was limited to sending a copy to the editors of the volume, Bryce DeWitt and his doctoral student Neill Graham. Everett pursued a successful career within the Pentagon, having subsequently founded companies that provided services for the United States Department of Defense and died, in 1982, millionaire and without contact with physics. This is an unusual ending for someone who – according to both his undergraduate professor and his PhD supervisor – was expected to have a bright future in his academic career. In fact, more than once, Wheeler indicated that Everett should take some time to transform his thesis into a more suitable version and look for an academic post that would give him the freedom to develop his valuable ideas on the foundations of quantum theory.

## The Everettian Heresy from Bourdieu’s Perspective

Following the methodological agenda outlined by Bourdieu, we now analyze Everett’s case from the perspective of a struggle for scientific capital and the strategies adopted to obtain it and to keep it, which in this case were unsuccessful due to the adoption by Everett of a subversive strategy.

Before proceeding with the characterization of Everett’s strategy, it is interesting to pay attention to the strategy adopted by Wheeler during the period in which Everett was his doctoral student. Wheeler followed a succession strategy during his own career until the

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<sup>11</sup> Wheeler letter to A. G. Shenstone, May 28, 1956. Wheeler Papers, Box Di, series # 2, American Philosophical Society, Philadelphia, PA.



1950s, which proved to be an excellent choice for him. Having received his PhD in physics from the important Johns Hopkins University, Wheeler decided to pursue a career in physics by doing two postdoctoral research, one in the United States, at New York University, and the other at the Institute for Theoretical Physics, in Copenhagen, Denmark, under the guidance of Niels Bohr. During this period, Wheeler developed the standard physics research agenda of those times: the application of quantum theory to several domains. Wheeler specialized in nuclear physics, becoming a highly prestigious scientist. At the end of his second postdoctoral research, he was hired as an assistant professor at the University of North Carolina at Chapel Hill. Shortly thereafter, following his succession strategy, he received a tenure offer, promoting his career at the same university.<sup>12</sup> He also received a job offer from Johns Hopkins University, also with tenure, and, finally, an offer from Princeton University, this without a tenure. Wheeler decided to go to Princeton, reasoning that even with a less prestigious position within the institution, he could have the collaboration of a greater number of notable scientists, which would certainly help in the development of his career, a strategy that effectively worked. Wheeler stayed at Princeton until shortly before his compulsory retirement, later returning as an *emeritus* professor. Having a well-established career in the most prestigious area of physics in those times – nuclear physics – and doing important research of military interest was part of his succession strategy. Therefore, in the early 1950s, Wheeler had accumulated enough scientific capital to change his strategy.

In 1953, when he was already one of the most renowned American physicists, Wheeler decided to leave his research in nuclear physics to dedicate himself to a topic whose prestige among physicists was modest: general relativity and cosmology. In fact, a course on general relativity had never been taught at Princeton University until 1953. That is quite surprising, since Princeton was one of the most important universities in the world concerning the research in physics, and had Albert Einstein – the inventor of general relativity – as a member of one of its institutes, the Institute for Advanced Studies. Wheeler was the first professor to offer a course on the subject. Thus, it is possible to state that his succession strategy changed into a subversion strategy, although not as radical as that which had been attempted by David Bohm at the same university a couple of years before. While Bohm aimed to completely reconfigure the research field of quantum theory, Wheeler did not attempt to change the foundations of the field of general relativity and cosmology, but to transform it into a field of great prestige. Wheeler was a leading figure in a historical movement that historians have been calling the Renaissance of General Relativity in the 1950s (Blum et al., 2016). His academic bet was subversive because there was no guarantee of recognition and accumulation of scientific capital, and because the bet tried to reconfigure the rules for the accumulation of his scientific capital, changing the objects that have value in the market. Wheeler used his own academic recognition to support his research in cosmology. There were two possible outcomes: the loss of his academic prestige, similarly to what happened to Einstein, who, in the view of physicists, had moved away from the frontier of research in physics since the 1930s; or achieve a reconfiguration of the status of research in general relativity and cosmology and be recognized for its pioneering efforts, being able to dominate the new market for scientific capital. In the end, the latter happened. Wheeler's bet worked. General relativity and cosmology have gained increasing recognition both within and outside the physics community (the Hubble telescope is more famous than any particle accelerator) and Wheeler has continued to be recognized as a pioneer in the field.<sup>13</sup>

<sup>12</sup> In the American academic system, *tenure* is equivalent to the stability of the employment contract in the institution.

<sup>13</sup> In fact, it is possible to think if the change in strategy was really fruitful. Wheeler, despite being considered a giant of the 20<sup>th</sup> century physics, was never awarded the Nobel Prize. That has been often considered a great injustice. This kind of reasoning is purely speculative, but with all his talent, if he

It was during this change in Wheeler's strategy that Everett first met him. Everett had graduated from a university not as prestigious as the one where he went to do his graduate studies, but he obtained, as we have seen, an excellent recommendation letter from his undergraduate professor William Boone. He had obtained a scholarship from the National Science Foundation, which gave him more scientific capital. Being accepted at Princeton, his scientific capital increased even more, and besides he was guided by a great researcher in physics. Thus, even though he was a young man entering the career, at least in principle he had sufficient institutional support to be able to choose his own strategy. Institutional support would not necessarily guarantee success if he bet on a subversion strategy, but at least he would have the opportunity to make the decision himself.<sup>14</sup> Had he been at a university with little or no academic prestige, a subversion strategy would have almost no chance of success.

The epistemological form of Everett's subversive bet was the development of the formalism of quantum theory to try to grasp its meaning. He claimed that the mathematical formalism of physical theories should be interpreted in a literal way. The idea of taking physics equations to the extreme is not, in itself, a subversive strategy, but it was also not a central strategy in the research agenda of the time.<sup>15</sup> However, in the specific case of research on the foundations of quantum theory, the widely accepted approach, attributed to Niels Bohr, condemned this type of attitude. Proceeding in that way, Everett was aware that he would be questioning Bohr's thinking, and that, in fact, did not bother Everett at all. He made explicit his dissatisfaction with Bohr's thinking both in the first version of the thesis and in his correspondence. Even the way that Everett decided to portrait the interpretational problems of quantum mechanics was already subversive. He adopted an axiomatizing approach, something that Bohr condemned. For Bohr, it did not make sense to axiomatize quantum mechanics, as its meaning would always depend on an experimental context and on concepts that could never be reduced to axioms. Everett, nonetheless, considered that the main problem of the theory was to solve formal issues, as formulated by John von Neumann, who also supported the axiomatization of physical theories. According to von Neumann, quantum mechanics had two modes of evolution. The first one was given by the Schrödinger equation, which is linear and deterministic, while the second one was the so-called projection postulate, which is abrupt and non-causal. Everett tried to eliminate the second mode of evolution, analyzing only the first mode of evolution of the equation and developed a new interpretive scheme for that purpose.

While the description above may seem somewhat technical, the important point is to emphasize that Everett sought much more than just solving interpretive problems that existed in the Copenhagen interpretation. From a larger perspective, his proposal was to reformulate the entire field of quantum mechanics, suggesting new problems and new solutions based on his own ideas and methods.

Thus, it is understandable that Bohr could not accept those ideas. More than that, Bohr, willing to maintain a monopoly on his scientific authority, used it to undermine the very

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had continued to follow a succession strategy, working with the main themes of the research agenda, would he have been awarded the Nobel Prize? The first Nobel related to the research field reconfigured by Wheeler came in 1978 and to date less than 10 awards have been directly related to general relativity and cosmology. At the same time, his student, Kip Thorne, who obtained a PhD under Wheeler in 1967, was one of those recipients, receiving the 2017 Nobel for research on gravitational waves.

<sup>14</sup> In fact, the greater the autonomy of the scientific field, the more only those already participating in that field will consume their products, and at the same time, provide capital for their market. Therefore, in such a field, any strategy will depend even more heavily on its scientific capital. Thus, even for a subversion strategy, it is essential to be part of the already established scientific enterprise.

<sup>15</sup> We will discuss this point again in the conclusions, but it seems important to us to emphasize that the greater the autonomy of a field, the closer the succession and subversion strategies appear.

meaning of Everett's work. The characterization Bohr and his collaborators gave of Everett's work was that he was unable to understand the bases of quantum theory and, therefore, was trying to solve problems that simply did not exist. In a previous work (Freitas and Freire, 2008b), we characterized the two approaches as incommensurable and, as such, only a subversion strategy could lead to a revolution in the field so that the new ideas could replace the old ones and thus obtain a monopoly on scientific authority.<sup>16</sup> What was considered as a problem according to one interpretation was not to the other and vice versa. However, Bohr was the holder of scientific authority at this moment and, therefore, it was up to him to define in what terms the research should take place and, therefore, what was and what was not a problem. So, the problems that Everett wanted to solve were definitely not perceived as problems at that moment. Everett's work could not arouse interest in that context.

It is interesting to compare this debate with the one that happened fifteen years later, in the early 1970s. In this new context, the field had been entirely reconfigured. Niels Bohr passed in 1962 and, in the following years, a controversy about the foundations of quantum theory took place (Freire, 2015). A letter by M. Hammerton published in the journal *Physics Today* in 1971 explained well that change in the physics community:

The very interesting contributions to the quantum mechanics debate in your April issue, and the paper by DeWitt which triggered them, exemplify the highly complex and subtle ways in which scientific opinion can change.

When I was an undergraduate reading physics 20 years ago, the Copenhagen interpretation was very firmly in the saddle. Indeed, I recall a seminar during which I suggested that it was merely a positivist-philosophical gloss, and being denounced as a metaphysician. The Copenhagen line was "scientific," anything else was meaningless, mumbo-jumbo, or, at best, mistaken.

Now the curious thing is that, as far as I am aware, there has been no major finding or theoretical insight that could be held to demolish or supersede this interpretation. Nevertheless, there is how considerable dissatisfaction with it, and a willingness to regard other points of view – for example, hidden variables – as being at least respectable. The considerations that have led to this change of attitude would themselves make an interesting and valuable study. (Trigg; Hammerton et al. 1971, 11)

In this new context, Everett's interpretation was reinvigorated, finding some supporters, but was once more the target of criticism. This time, however, the criticism basically involved understanding whether Everett really solved the problems he had proposed to solve. Many pointed logical inconsistencies that his supporters are to this day trying to resolve, but the important thing to note here is that, at that moment, Everett's interpretation was not simply dismissed because it aimed at problems that did not exist. The interpretation of quantum mechanics had become once more an open problem. With the reconfiguration of the field – characterized by a new scientific context in which there was an

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<sup>16</sup> According to Bourdieu, "Scientific revolutions that overturn the tables of epistemological values overturn in the same blow the hierarchy of social values attached to the various forms of scientific practice, and thereby the social hierarchy of the various categories of scientists. The new scientific regime completely redistributes the meanings and values associated with the various scientific choices by imposing new norms of interpretation and new categories of perception and of appreciation of importance. As in those perceptual restructurings that ambiguous forms allow, what was central now becomes marginal, secondary, insignificant, while objects, problems, and methods hitherto considered minor and therefore left to minor and secondary agents, find themselves brought to the forefront, in broad daylight, bringing a sudden visibility to those connected with them." Bourdieu, 1991, 14-5.

established controversy on the foundations of quantum theory – it was acceptable to address those interpretative problems.

## The Subversion Strategy and the Reconfiguration of the Field

Although the strategy adopted by Everett was a strategy of subversion, it was not presented as such in the final version of his thesis, in particular, due to the way Wheeler interfered in the process, as we explained in section 3. As Bourdieu explains:

As accumulated scientific resources increase, the requirements for entry continue to rise, and access to scientific problems and instruments, thus to scientific competition, requires an increasingly large amount of embodied capital. It follows that the opposition between strategies of succession and strategies of subversion tends more and more to lose its meaning, insofar as the accumulation of the capital necessary for revolutions to succeed and the acquisition of the capital gained by successful revolutions tend more and more to be carried out according to the regular procedures of a career. (Bourdieu, 1991, 18-9)

That is, even though succession and subversion strategies are quite similar in their forms and methods, they still have different goals in the symbolic capital market. A subversion strategy should stress the differences and not hide them, especially when the proponent has little scientific capital compared to the competitor. Everett, as much as he had the support of Princeton and Wheeler, still had no way to reconfigure the field directly against Bohr, without the support of others. Bohr would not be willing to give up his monopoly on the philosophy of quantum mechanics and to allow a new interpretation.

However, Wheeler was not prepared to face Bohr, at least not directly. Rather than presenting his student's interpretation as an opposition to the existing state of affairs, Wheeler decided to present it as part of that state of affairs, phrasing it as an attempt to generalize Bohr's interpretation, so it would be a particular case of Everett's approach. This strategy could have worked if Bohr had been convinced that there was a need for a reformulation of the field. In that case, he would be able to maintain his scientific authority by being a pioneer in supporting the new order, maintaining his hegemony. As already mentioned, there was no indication that this was necessary, so the worst happened to Everett. His bet on a high-risk strategy turned out to be fruitless and, disgusted by the very low prestige that his interpretation had in the early years, he ended up giving up his career in physics.<sup>17</sup> Had this interpretation really been presented as a subversive strategy, there would be the possibility of obtaining support from other researchers, accumulating scientific capital, which would allow for some survival and, with some chance, even if small, for a reconfiguration of the research perspectives. Bohm's case is a good example of this. Even without being able to reconfigure the field, his interpretation, together with the accumulated scientific capital, was sufficient for him to continue to move within the scientific field, obtaining positions at universities and continuing within the rules of the game.

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<sup>17</sup> In this specific case, it should be noted that Everett's decision to abort entry into an academic career has also been greatly influenced by social status and the high salaries paid to Pentagon employees. However, several other scientists shared during part of their careers academic research with research of military interest, including Wheeler. Everett, even after publishing his thesis, even though he did not seek an academic post, visited Copenhagen in 1959 to discuss his interpretation, but the result was predictable. Bohr continued considering that Everett did not understand quantum theory. Everett even participated in some congresses until 1961, but with very little scientific capital and completely unmotivated for not having obtained what he expected with his bet, he abandoned physics for good.

## Conclusions

As we have seen, the analysis in terms of power, in terms of renegotiation of scientific capital, in terms of prestige, allows us to establish an interesting perspective when examining scientific controversies. The critique of science shows that what is at stake is not the truth about nature, even though many scientists keep believing it. If neither Wheeler nor Everett nor Bohr, from the historical point of view, sought the truth, what did they seek? In this case, they sought the monopoly of scientific capital. It is no wonder that the period when Bohr dominated was called Copenhagen monarchy.

While, at first, this approach to the history of science – which describes science as a field of forces that compete for symbolic capital using strategies that can either try to succeed the rules of this field or, otherwise, subvert them, establishing a new order – seems to fit into an externalist perspective, such orders, rules and values have meaning only within the rules of the common language of the specific area and its terms can only be understood inside this internal logic, indicating that classification as strictly externalist does not allow one to understand the necessary steps for such analysis. Bourdieu proposes an order for science that is social in nature, but with *internal dynamics* that take place according to that social order.

Paul Feyerabend, in the preface to the third edition of *Against Method*, poses the following problem:

In sociology, the attention to detail has led to a situation where the problem is no longer why and how “science” changes but how it keeps together. Philosophers, philosophers of biology especially, suspected for some time that there is not one entity “science” with clearly defined principles but that science contains a great variety of (high level theoretical, phenomenological, experimental) approaches and that even a particular science such as physics is but a scattered collection of subjects (elasticity, hydrodynamics, rheology, thermodynamics, etc., etc.) each one containing contrary tendencies (...). For some authors this is not only a fact; it is also desirable. (Feyerabend, 1993, x-xi)

Bourdieu thus allows the problem of the unity of science to be solved while maintaining its plurality of methods and approaches. The scientific field remains as such, as it is part of the process of accumulating power. If science split up each time that there is a revolution – that is, a successful subversion strategy – power would decrease, instead of increasing. The strength of science as a relatively autonomous field depends heavily on its unity.<sup>18</sup>

As Everett’s case study shows – even if what was at stake was, in Bourdieu’s terms, the monopoly of scientific authority – the understanding the historical process necessarily involves careful dissection of the internal dynamics of the scientific dispute. The

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<sup>18</sup> This answer that appears here quickly, while short and with a simple appearance, is complex on a level that is not possible to develop in this space. However, for its development, it is necessary to go further in the investigation and understanding of academic, social, and political capital as an integral part of the scientific enterprise. While the plurality of methods, languages and objects exists within science, in the dimension of political struggle, unity overlaps all of that. If research takes place in specific institutes, it is the university that fights the battle of working conditions, it is the broad societies, such as the Brazilian Society for the Progress of Science (SBPC) or the American Association for the Advancement of Science (AAAS), the academies such as the Brazilian Academy of Sciences (ABC), National Academy of Sciences (NAS), the Royal Society and so on, which face the battle with other fields. Thus, depending on the level of analysis and the dynamics of the object of study, the unit or separation will appear more or less strongly.

understanding of career strategies is only possible when one deeply understands the conceptual issues involved. Understanding, for example, that the axiomatization proposed by Everett had a meaning in the field that was quite distinct from von Neumann's axiomatization, and that they were contemporaries of the holder of the authority monopoly in these matters (namely, Niels Bohr) requires an appreciation of the internal dynamics of quantum mechanics. A historical analysis following Bourdieu's approach *requires* an understanding of the rules that are in force at the moment in the scientific field, of how these rules are being disputed, and of the significance of this dispute for the symbolic capital market. Bourdieu, then, dissolves the separation of what is external to science with what is internal by unifying everything within a single dynamic that is interdependent. The fight is about power, but the logic of that power is the very logic of the scientific field. Bohr, Everett, and Wheeler aimed at increasing their own scientific capital. That historical episode was a power struggle. Simultaneously, Bohr defended his contextual (pragmatic-transcendental) interpretation, Wheeler approached the problem of a universal wave function for a universe without an external observer (and therefore without context), while Everett developed an interpretation that did not depend on an additional postulate. Therefore, a cognitive dispute. For Bourdieu, despite written in different languages, both descriptions, the one around power and the one about the cognitive dimension of the problem, are similar and are a part, as a single entity, of the analysis of the scientific field.

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