CHAPTER 3.4

Uses and appropriations of Niels Bohr's ideas about quantum field measurement, 1930-1965

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Abstract

The concept of quantum field measurement was elaborated by Niels Bohr and Léon Rosenfeld in the early 1930s. In the current article we show that, contrarily to the shared belief among historians of science, a considerable number of physicists were interested in Bohr and Rosenfeld's ideas. We suggest that the emergence of pragmatic stances in physics during the mid-20th century is related to a shift in the way this article was used and interpreted.

Key words: Niels Bohr; quantum field measurement; pragmatism; appropriation.

SCI.DAN.M. I · ONE HUNDRED YEARS OF THE BOHR ATOM: PROCEEDINGS FROM A CONFERENCE
THE ROYAL DANISH ACADEMY OF SCIENCES AND LETTERS · 2015

1. Introduction

In 1933, Niels Bohr and Léon Rosenfeld published a subsequently renowned article about the quantum theory of the electromagnetic

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field.[1] Their reasoning had the purpose of settling a controversy begun three years before between, on the one hand, Bohr and Rosenfeld, and, on the other, Lev Landau and Rudolf Peierls. The latter two argued that the quantum theory of the electromagnetic field was an inconsistent theory and should be discarded, while the former two disagreed with both statements. This fierce dispute was dubbed by Oskar Klein "the small war of Copenhagen."[2]

Considered to be the winner, Bohr and Rosenfeld's article soon became a classic. It exposes a sophisticated reasoning, full of epistemological intricacies, but the final result is quite simple: quantum electrodynamics is a consistent theory. According to Abraham Pais, a friend of Bohr once said: "It is a very good paper that one does not have to read. You just have to know it exists."[3] Pais asserted that the 1933 article, in spite of being a classic, "has been read by very very few of the aficionados."[4]

Some historians claim that the article was forgotten not only because its arguments were hard to grasp, but also because its reasoning soon lost its relevance. During the mid-20th century, theoretical physics departed from deep epistemological arguments, such as those of Bohr and Rosenfeld, and moved toward a more pragmatic, utilitarian, instrumentalist approach. In this pragmatic view of physics, there would be no room for the kind of epistemologically minded arguments presented in the 1933 article. The young physicists were too concerned with computations and experimental verification and "either had difficulty grasping the subtleties of the older generation's critical reflections or were too impatient to care about them."[5] Thus, the loss of importance of the article has been seen as a consequence of a larger change, related to how physics worked and to which kind of arguments were relevant. The historian David Kaiser, in a witty statement, wrote that Bohr and Rosenfeld won the 1933 "battle" of Copenhagen against Landau and Pei-

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1. Bohr and Rosenfeld (1933), reprinted in Bohr (1996), pp. 57-121.

2. Jacobsen (2011), p. 384.

3. Pais (1991), p. 362. On the article's intricacies, see interview of Walter Heitler by John Heilbron, 18 March 1963, OHI.

4. Pais (1991), p. 362. See also Darrigol (1991), pp. 177-178.

5. Jacobsen (2011), p. 394.

erls, but lost the greater "war" about how physics should be done.[6] In this historical explanation, the fate of Bohr and Rosenfeld's paper was to be forgotten as a piece of old physics.

In this chapter, we challenge this view of the decline in relevance of Bohr and Rosenfeld's article by analyzing how it was read, interpreted, and used during the period from 1933 to 1965. Contrary to what is said by several historians of science, the article was considerably read during the entire period.[7] Some physicists even formulated solid research projects based directly on the 1933 ideas. Our goals in this text are, first, to draw attention to these works and, second, to understand how and with which purposes these physicists used Bohr and Rosenfeld's arguments. We will show that they used Bohr and Rosenfeld's ideas in ways going far beyond their original intentions, in ways that sometimes were in fact quite pragmatic and instrumentalist. The young physicists did not extend the 1933 ideas, but appropriated them for their own projects. In the everyday practice of physics, the concepts presented in the 1933 paper quietly obtained new meanings. We are interested in analyzing here this historical development in order to see the silent crumbling of old ideas, and their untold reinterpretation for new purposes.

In drawing attention to these many uses of Bohr's ideas we hope to make a step toward a more precise acknowledgment of his influence on 20th century physics and, in particular, on quantum field theory. Despite being sometimes contrary to his original intentions, these uses of Bohr's ideas show us not only his importance to the physicists we analyze here, but also the fertility of his ideas, which were used in many ways and in different contexts.

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[6] Kaiser (2007), p. 4. See also Jacobsen (2011), p. 395.

[7] Due to limitations in space, we have no intention of being exhaustive in our presentation. A more detailed exposition may be found in Hartz (2013), pp. 14-54. A full translation of this thesis chapter will soon be published elsewhere.

2. The "small war of Copenhagen" (1930-1933)

We begin by revisiting the Bohr–Rosenfeld vs. Landau–Peierls controversy.[8] Quantum field theory, which today is a well established physical theory, was for a long time severely criticized for suffering from a great problem: the occurrence of infinite quantities, which undermined some computations, and prevented physicists from obtaining finite values for some well defined physical quantities. Physicists worked hard to solve this problem during the 1930s and 1940s, until a solution was finally found in 1947.[9]

Many of these physicists approached the problem by seeking its origin. According to Landau and Peierls, the source of all the trouble was the very concept of a quantum field, which they claimed was meaningless.[10] Following Bohr's idea that "the consistency ... can be judged only by weighing the possibilities of definition and observation," Landau and Peierls developed, in 1930, a thought experiment in which the electromagnetic field could be measured.[11] This measurement can be done by analyzing the action of the field on a test charge. They argued that the limitations entailed by the test charge's radiation reaction for the measurement of a quantum electromagnetic field are so restrictive that there is no agreement between the measurability of a quantum electromagnetic field and the expected uncertainty relations of the theory. Such inconsistency, they claimed, would entail a fundamental limitation in the applicability of the quantum electromagnetic field concept. Thus, the analysis of the measurement of a

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[8] There are some excellent studies on this controversy: Kalckar (1971), Darrigol (1991), Jacobsen (2011), and Jacobsen (2012), p. 71-90. See also Jammer (1974), pp. 142-145, Pais (1991), pp. 359-364, Schweber (1994), pp. 111-112, Kragh (1999), p. 198, and Kojevnikov (2004), pp. 87-88.

[9] See Rueger (1992).

[10] Landau and Peierls (1931).

[11] Quotation in Bohr (1928b), p. 580, reprinted in Bohr (1985), pp. 147-158. See also Rosenfeld in the interview of Oskar Klein by J.L. Heilbron and Léon Rosenfeld, 28 February 1963, OHI. Consistency is the balance between the concepts defined in the theory and the possibilities of their measurement, cf. Bohr (1928a), p. 578, reprinted in Bohr (1985), 113-146; Bohr (1934), p. 114, reprinted in Bohr (1985), 236-253; Bohr and Rosenfeld (1933), p. 64, Kalckar (1971), p. 127, and Darrigol (1987), p. 139.

quantum field showed the inappropriateness of the quantum field concept, and suggested that the theory should be discarded.

Bohr could not accept this conclusion, since it would result in the abandonment of quantum electrodynamics and of the correspondence argument that governs the connection between classical to quantum theories.[12] In collaboration with Léon Rosenfeld, he began in 1930 an extensive effort to respond to Landau and Peierls. Bohr and Rosenfeld showed that if one considers a macroscopic test charge, the radiation reaction may be eliminated.[13] In other words, Landau and Peierls had not proposed the best measurement possible. In this way, Bohr and Rosenfeld neutralized Landau and Peierls's criticism. Quantum electrodynamics is a consistent theory.

Quantum field measurement played different roles for each side of this controversy.[14] For Bohr and Rosenfeld the analysis of measurement was not a means to judge and discard theories. What was the most important aspect for them was that the field measurement analysis revealed some fundamental aspects of complementarity that had not appear in the non-relativistic problems of quantum mechanics, for instance, the relation between the macroscopic aspect of the test charge and the use of classical concepts in quantum field theory.[15]

The 1933 debate echoed far beyond Copenhagen. In the following sections, we will introduce some physicists that were inspired by this debate.[16] They used the idea of quantum field measurement with

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12. Bohr and Rosenfeld (1933), pp. 3-4, and Bohr to Heisenberg, 22 May 1935. Bohr (1996), p. 451.

13. Macroscopic means that the atomic constitution can be neglected.

14. This was already recognized by Darrigol (1991), pp. 176-177, Jacobsen (2011), pp. 387-388, and Kaiser (2007), pp. 3-4. See also the manuscript in Bohr (1996), p. 209, and Heisenberg to Pauli, 12 March 1931, Bohr (1996), pp. 440-441.

15. Bohr and Rosenfeld (1933), p. 47 and pp. 64-65. See also Bokulich (2003), pp. 28-30, and Bohr to Heisenberg, 22 May 1935 (footnote 12).

16. As far as we know, the letters, manuscripts, and articles we analyze in the following sections have not been previously mentioned in the history of science literature in connection to quantum field measurement. The only exceptions are: Bronstein (1936) and Solomon (1938) are mentioned by Stachel (1995), pp. 317-319, Stachel (1998), pp. 528-532, Stachel (2004), pp. 170-174, Gorelik (1992), Gorelik (2005), Gorelik and Frenkel (1994), pp. 99-112, and Rickles (2010), p. 181; Bronstein (1936) and DeWitt (1962a) are mentioned by Amelino-Camelia and Stachel (2009), p. 1108,

Figure 1. Bohr's family and Rosenfeld in Tisvilde in 1931. Courtesy of the Niels Bohr Archive.

purposes that went beyond those of the 1933 debate. In analyzing these uses, our purpose is not to judge them, but to understand the relation between the uses and each physicist's general attitude toward the mathematical and epistemological aspects of physical theories.

3. The debate outside Copenhagen (1932-1933)

In 1931, the recent Nobel laureate Louis de Broglie read Landau and Peierls's article and got excited about it. He too had at that time reservations about quantum field theory, and appreciated very much their argument.[17] He soon published a free translation into French of their article, with comments.[18]

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and Stachel and Bradonjić (2014), p. 211; and Henley and Thirring (1962) is mentioned by Kalckar (1971), p. 127 and p. 153, and Darrigol (1991), p. 179. Most of these mentions are just incidental. The only remarkable exception is Gorelik, who presented in his articles a careful study of how Matvei Bronstein used quantum field measurement (see footnote 23).

[17] On de Broglie's opinion about quantum field theory, see Vila Valls (2012), pp. 71-82.

[18] De Broglie (1932).

De Broglie's translation attracted the attention of at least one French researcher, the 24-year-old Jacques Solomon, who in spite of his young age was already the leading voice of quantum field theory in France. He was the son-in-law and a former student of Paul Langevin as well as a close friend of Rosenfeld, and decided to respond to the provocation in defense of quantum field theory.[19] The year was 1932 and no physicists had publicly confronted Landau and Peierls's argument yet. Solomon was certainly aware that Bohr and Rosenfeld were writing their article, since he visited Copenhagen twice during 1931 and 1932.

Solomon's article, which was published before Bohr and Rosenfeld's, is in fact an admission of failure. He could not eliminate completely the radiation reaction of the test charge, although he could reduce it considerably. His conclusion was that Landau and Peierls did not make the best possible measurement, notwithstanding that he was not able to do much better. He argued:

> We can be sure, a priori, that it is possible to find a [thought] experiment that agrees with the commutation relations of [quantum electrodynamics]. In a general perspective, it seems that it would be quite hard to make progress in [quantum electrodynamics] if we rely on "[thought] experiments," since the demonstration remains always to be done later. ... A [thought] experiment may concretize a theoretical result, [but] it seems unlikely that the former may precede the latter.[20]

Therefore, Solomon did not agree with Landau and Peierls because they held the judgmental view that the thought experiment could overrule the theory. According to Solomon, there should be a primacy of the abstract (the mathematical theory) over the concrete (the thought experiment). Quantum field theory having already been developed, quantum field measurement may play the role of giving the scientist a concrete counterpart of the abstract reasoning. This kind of argument puts Solomon close to the French rationalism of his times, in particular to Gaston Bachelard.[21]

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[19] For Solomon's relation with Langevin and Rosenfeld, see Bustamante (1997).

[20] Solomon (1933), p. 386.

[21] Bachelard believed that the "concrete stage" of science was just a first step toward

Figure 2. Bohr, Rosenfeld, and Solomon at the 1932 Copenhagen Conference. Rosenfeld is beside Bohr, wearing glasses. Solomon is the third on the right side of Bohr, behind Paul Ehrenfest. Courtesy of the Niels Bohr Archive.

Despite his attempt, Solomon had to await Bohr and Rosenfeld's paper to see the quantum field theory being set free of Landau and Peierls's criticism. A few years later, as we will see, another quantum field measurement challenge appeared, and Solomon promptly defended quantum field theory once again. During the same period, two physicists in the USSR also became interested in quantum field measurement.

4. Quantum field measurement in the USSR (1934-1936)

Vladimir Fock, in an important paper, wrote that the analysis of quantum field measurement made by "Bohr and Rosenfeld elucidated the region where the theory [quantum electrodynamics] was

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the "abstract stage." The quantum theory was an example of the "abstract stage" of physics. He stated: "quantum mechanics, Louis de Broglie's wave mechanics, Heisenberg's physics of matrices, Dirac's mechanics, abstract mechanics ... doubtless there will soon be abstract physics which will order all the possibilities of experience." Therefore, he asked: "since the concrete ... is correctly analyzed by the abstract, why should we not agree to make abstraction the normal and productive approach of the scientific mind?" Bachelard (2002), pp. 17-20.

applicable."[22] According to him, the domain of applicability of the theory was thus established, and the theorists should then turn their attention to the mathematical formalism, in order to find a solution to the infinities that still plagued the theory.

A similar opinion was presented by Matvei Bronstein in his PhD thesis on the quantization of the gravitational field.[23] He was aware that he could not apply the standard methods of quantum field theory to the full, non-linear gravitational theory, so he decided to approach this theory using Bohr and Rosenfeld's measurement ideas. He designed a thought experiment in which the linearized quantum gravitational field could be measured, obtaining an uncertainty for a single component of the Christoffel symbol. The uncertainty gets smaller as the mass density of the test body increases. Since in the linear region there is no limit for such a density, this component may be measured with complete precision. However, in a non-linear region a similar expression should appear, and since the density would then have a limit, a single component would be measurable only with a limited precision. This single component uncertainty had no counterpart in the mathematical formalism. Bronstein concluded that "without a profound revision of [the] classical concepts [of the general theory of relativity], it seems hardly possible to extend the quantum theory of gravity to [the non-linear] region."[24] Assuming a similarity between quantum field measurements in the linear region and in the non-linear region, Bronstein used them to explore a theory beyond its current limits of applicability.

5. Solomon's second defense of quantum field theory (1937-1941)

In the mid-1930s, three well-known physicists – Walter Heitler, Lothar Nordheim, and Edward Teller – formulated a naive thought experiment with the purpose of showing the impossibility of meas-

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[22] Fock (1934). Quoted from the English translation, Fock (2004), p. 332.

[23] Bronstein (1936). On Bronstein, see Gorelik and Frenkel (1994), Gorelik (1992), and Gorelik (2005).

[24] Bronstein (1936), p. 150.

uring the quantum gravitational field. Possibly aware of the naivety of their argument, they never published it. However, George Gamow considered it to be an interesting idea, and decided to write about it, giving the due credit to the three physicists.[25]

Solomon read Gamow's article, decided to tackle the challenge, and wrote quickly an article supporting Bohr and Rosenfeld's approach to field measurement. He argued that Gamow and the three physicists were wrong, since, as Bronstein had shown, the quantum gravitational field is, in fact, measurable in the linear approximation. In spite of being supposedly Bohrian in his approach, Solomon wrote about quantum field measurement on the atomic scale, while Bohr and Rosenfeld had clearly stated that quantum field measurements should be done with macroscopic test bodies. Solomon focused so much on the measurability problem that he did not pay sufficient attention to the macroscopic aspect of the measurement apparatus.[26]

Between 1933 and 1938, Solomon not only accepted the use of thought experiments, he became an emphatic supporter of them. The problem of understanding why Solomon changed his opinion about this subject is not only of biographical relevance, since it may indicate how an opinion about field measurement is related to a broader philosophical stance. With the growth of fascism in Europe, Solomon got involved with some political movements. Notably, in 1933 he joined the French communist party. Under the influence of George Politzer and other Marxist philosophers, Solomon adhered to dialectical materialism.[27] From that moment on, he started opposing Bachelard's rationalism. "It is not correct to state, as Mr. Bachelard does, that 'the true solidarity of reality is mathematical in essence': it is the reality, in fact, that dictates and verifies the mathematics", he wrote in an unpublished text, probably from circa 1941.[28] Thus, his change of view with regard to quantum field meas-

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[25] Gamow (1937), p. 814.

[26] Solomon (1938).

[27] Bustamante (2002), p. 8.

[28] Solomon (1945), p. 51. Solomon quotes verbatim Bachelard's statement "la véritable solidarité du réel est d'essence mathématique," Bachelard (1966), p. 88, originally published in 1940. Translation in Bachelard (1968), p. 75. On Solomon's criticism of Bachelard, see Redondi (1978), p. 233.

urement was concomitant with a philosophical change regarding the relation between mathematics and reality. Such a change seems to be related to the political episodes of his times.[29]

Another French physicist, Jean Mariani, also became aware of the Heitler-Nordheim-Teller idea and, contrarily to Solomon, welcomed it quite warmly. Mariani was one of the many physicists of the 1930s who believed that the quantum field theory problems would be solved by the consideration of a fundamental length scale.[30] From his point of view, the failure of measuring the quantum gravitational field as described by Gamow indicated that the theory should be altered, possibly by the introduction of a fundamental length.[31]

6. Quantum field measurement in the United States (1947-1965)

In the late 1940s, physicists found a solution to the infinities of quantum field theory called renormalization. It solved the problem in a pragmatic way: one can isolate the infinities and incorporate them into some parameters of the theory, in the end obtaining finite results.[32] While lacking full justification, the renormalization process worked, and that was what really mattered.[33] In this way of doing physics, one should trust the theoretical results not because of consistency arguments, but because the computations are feasible and fit the experimental data. In this context, it seems that the importance of Bohr and Rosenfeld's argument would be considerably reduced. However, contrary to such expectation, quantum field measurements were not forgotten.[34]

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29. On Solomon's profound changes of view during the 1930s, see Rosenfeld (1979a).

30. On these physicists, see Kragh (1995).

31. Mariani (1939).

32. See Schweber (1994), pp. 595-605.

33. Schweber (1986), pp. 96-98.

34. Due to the limitation of space, we will restrict our discussion about the 1950s to the United States context. Thus, we will discuss neither Bohr and Rosenfeld's 1950 article nor Ernesto Corinaldesi's PhD thesis.

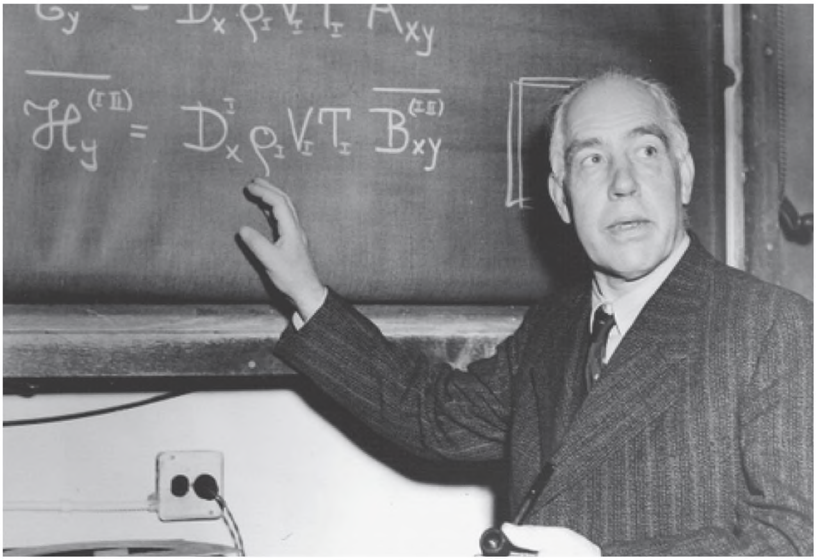

Figure 3. Bohr lecturing on quantum field measurement in Princeton, 1948. Courtesy of the Niels Bohr Archive.

The main promoter of Bohr and Rosenfeld's ideas in the United States was John Wheeler. He had been interested in these ideas since the late 1940s, when he wrote a short text about the subject.[35] In a letter sent to Bohr in January 1950, Wheeler confessed why he had "always been so interested" in these ideas.[36] He was trying to develop a new theory of elementary particles based entirely on positive and negative electrons. He wanted to show that in fact such picture was viable. He derived qualitatively, using only negative and positive electrical charges, many known results about nuclear physics, but he faced one problem. "Spin and statistics are absolutely inconsistent with these views," he complained to Bohr, "and completely rule out this picture, if it is supposed that neutron and neutrino, for instance, form systems that are really isolated from each other."[37] Wheeler imagined a hydrodynamical analogy. In the

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35. Pais (1948), pp. 42-45.

36. Wheeler to Bohr, 21 January 1950. WP, Series I, Box 5, Folder "Bohr, Niels #2."

37. Wheeler to Bohr, 21 January 1950 (footnote 36).

same way as two vortexes in a fluid seem to be isolated but in fact are linked by a field that satisfies the circulation theorem, neutrons and neutrinos could somehow be linked in a deeper way. That could solve, according to Wheeler, the spin and statistics problem. Therefore, it was necessary to understand how a system of positive and negative charges would disturb another system of positive and negative charges. Bohr and Rosenfeld's work, as an analysis of disturbance, could teach him something about his new theory.

Wheeler never published these ideas.[38] Nevertheless, he mentioned the 1933 article a few times in his published articles, arguing that Bohr and Rosenfeld's reasoning could provide the scale of quantum fluctuations of fields. He used this argument to show the stability of geons – a gravitational wave held together in a confined region by its own gravitational attraction.[39] Thus, in Wheeler's hands, quantum field measurement became a heuristic tool for estimating quantum fluctuations.

Wheeler discussed Bohr and Rosenfeld's ideas in his *Advanced Quantum Mechanics* graduate course taught at Princeton in 1954 and 1955.[40] Hugh Everett was among the students who attended the course. According to him, the 1933 paper showed how the mathematical formalism of quantum electrodynamics should be interpreted. Everett used this example in his claim – which was already heterodox, but was yet to claim any rupture with the Copenhagen interpretation – that "always the theory itself sets the framework for its interpretation."[41] He was suggesting that the field measurement

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[38] In fact, these ideas about the unification of physics in one single ontology (in this case, particles) may be interpreted as the first version of Wheeler's geometrodynamics project, which was formulated a few years later and aimed at unifying the ontology of physics using only fields. See Wheeler (1962).

[39] Wheeler (1955), p. 514, Wheeler and Brill (1957), p. 475, Wheeler (1962), p. 76, and Wheeler to Bohr, 24 April 1956. Copy of latter in "Relativity IV Notebook" (p. 64). WP, Series V, Box 82.

[40] "Quantum Electrodynamics Notebook" (pp. 21, 37, 64, and 65). WP, Series V, Box 82. "Advanced Quantum Mechanics, Wheeler notes: notes on graduate school course, circa 1955" (section 21). EP, Series I, Box 1, Folder 7.

[41] "On the foundations of quantum mechanics, 1957". EP, Series II, Box 2, Folder 3. Later published in Everett (1957), p. 455. The claim that the theory itself sets the framework for its interpretation is a central idea in his interpretation of quantum

analysis had the purpose of constraining the possibilities of interpretation of the formalism.

Everett was not the only physicist who claimed that quantum field measurement analysis could constrain theories. The belief that Bohr and Rosenfeld's article had shown the necessity of quantizing the electromagnetic field was quite common at the time.[42] In 1956, Julian Schwinger wrote that "the complete agreement of [Bohr and Rosenfeld's] considerations with the formal implication of the operator commutation relations indicated the necessity and consistency of applying the quantum mechanical description to all dynamical systems."[43] However, the 1933 article did not argue for the necessity of quantization, and in the early 1960s Rosenfeld reacted emphatically against this idea.[44] He advocated that quantization can only be decided based on empirical evidence, never on measurement arguments.[45]

Quantum field measurement also had a considerable impact on research regarding the quantization of gravity in the United States. Tullio Regge, an Italian graduate student at Rochester University, wrote a short note analyzing the measurement of the quantum gravitational field.[46] Helmut Salecker and Eugene Wigner, inspired by Bohr and Rosenfeld, developed a thought experiment with the purpose of measuring the gravitational field on a microscopic scale.[47]

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theory (also known as the Many Worlds Interpretation). See Osnaghi, Freitas and Freire (2009), p. 107.

[42] For example, Henley and Thirring (1962), p. 3, Schweber, Bethe, and Hoffmann (1955), pp. 89-90, among other textbooks of that time. This opinion may be traced back to Heitler (1936), pp. 69-81.

[43] Schwinger (1958), p. viii.

[44] Rosenfeld (1963), and Infeld (1964), pp. 144-145, 219-222. See also Rosenfeld (1979b), originally presented as a talk at the Einstein Symposium in 1965.

[45] Rosenfeld was mostly right about his claims concerning quantum field measurement. It is not clear yet who was right about the necessity of quantization. For different perspectives, see Kalckar (1971), Huggett and Callender (2001), and Albers, Kiefer and Reginatto (2008).

[46] Regge, Tullio. "On the Measurability of Gravitational Field Strengths in Quantum Mechanics." WP, Series VI, Box 115.

[47] DeWitt-Morette and Rickles (2011), pp. 171-185. Salecker and Wigner (1958). See also Wigner (1962).

The most sophisticated use of field measurement ideas during this period was made by Bryce DeWitt. He had been interested in the subject since the early 1950s, when he translated Bohr and Rosenfeld's article into English.[48] A few years later, he suggested the measurement of the quantized gravitational field as a topic to one of his PhD students.[49] However, DeWitt was so interested in field measurement that he decided to tackle the problem himself. Since there was yet no mathematical formalism for the quantum gravitational field, he decided to develop the formalism starting from the analysis of measurement:

> [Our] indebtedness [to Bohr and Rosenfeld's article] may seem in one respect surprising, not, to be sure, because of any present-day diminution in the importance of this classic work, but because its content, as Bohr and Rosenfeld have repeatedly indicated, was guided in every way by the existence of an already developed formalism, whereas here we are trying to "put the cart before the horse"- to develop the formalism itself with the aid of the ideas of the theory of measurability.[50]

Instead of comparing, as Bohr and Rosenfeld had done, the uncertainties obtained from thought experiments and those obtained from the mathematical formalism, DeWitt used the former to define the latter, thus obtaining the commutation relations of the quantum theory of the gravitational field. He used the quantum field measurement to develop a mathematical formalism for quantum gravity.[51]

DeWitt did not see himself as going against Bohr and Rosenfeld.[52] It is quite curious that DeWitt and Wheeler, the two physicists who made the most pragmatic, instrumentalist uses of quantum field measurement ideas during the 1950s and 1960s, believed

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[48] Interview of Cécile DeWitt-Morette by Thiago Hartz, 4 August 2011, Austin, TX, USA.

[49] Yang Yeh (1961).

[50] DeWitt (1962a), p. 270.

[51] DeWitt (1961), DeWitt (1962a), and DeWitt (1962b).

[52] DeWitt to Bohr, 11 January 1961, BSC-Supp, folder 71. DeWitt to Rosenfeld, 11 January 1961, RP, box 8, folder 11-6. For DeWitt's later opinions on quantum mechanics, see Freire (2009) and Hartz (2013).

that they were making a completely acceptable use of the 1933 ideas; nevertheless, there was practically no common ground between their uses and Bohr and Rosenfeld's original purposes.

7. Conclusions

In this text, we have analyzed many uses of Bohr's ideas regarding quantum field theory. These uses have been largely neglected in the history of physics. Now we are in a better position to evaluate the reception of Bohr and Rosenfeld's arguments about quantum field measurement.

The 1933 article was read and used extensively from 1933 to the mid 1960s. Thus, no decline in the importance of Bohr and Rosenfeld's arguments took place, as some authors supposed, concomitantly with the decline of the epistemologically oriented physics during the 1940s. Instead, the decline of epistemology changed the way physicists approached quantum field measurement. In the 1930s, Bohr, Rosenfeld, Landau, Peierls, de Broglie, Solomon, Fock, Bronstein, and Gamow, among others, used quantum field measurement with the purpose of reflecting on the theory, judging its hypotheses and its range of applicability. In the 1950s, Wheeler, Wigner, and DeWitt, among others, used quantum field measurement as a theoretical tool, an instrument for the formulation and justification of new theories.

Thus, according to our narrative, the pragmatic, utilitarian, instrumentalist stance that emerged in physics during the mid-20th century did not make the 1933 article irrelevant or obsolete, but rather caused a shift in the way this article was used and interpreted.[53] In order to capture here the emergence of this pragmatism in physics, we must focus on how Bohr and Rosenfeld's arguments were used by other physicists. Science consists of uses. We should not call these uses extensions, because uses have no fidelity to previous projects; instead, we must talk about appropriations. All use is

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[53] This idea, which is the central thesis of our text, emerged, in its final form, during a discussion we had with Alexei Kojevnikov in July 2012 in Salvador, Brazil, to whom we would like to present here our acknowledgement.

an appropriation. All use is an operation that takes the discourse out of the range of action of those who produced it.[54]

Such was the fate of Bohr and Rosenfeld's arguments. Some young physicists used the idea of a quantum field measurement for their own projects.[55] The old vocabulary and the bibliographical references were kept untouched, and the original arguments persisted, but with different purposes, with different meanings.[56] An untold change happened.[57] Rosenfeld's discontent in defense of Bohr's original intentions, though understandable and mostly correct, ignores that the fate of all theories is to be taken away from those who created them. This is this kind of historical development that we have tried to grasp here.[58]

Acknowledgements: We thank Finn Aaserud, Olivier Darrigol, Anja Jacobsen, Reinaldo de Melo e Souza, Adriana Hartz, and an anonymous referee for comments and suggestions on a previous version of this text; Cécile DeWitt-Morette, Saulo Carneiro, Amanda Bastos, Alexei Kojevnikov, Paul Forman, Martha Cecilia Bustamante, and Fábio Freitas for enlightening discussions; the LACIC (Laboratory Science as Culture, Brazil) members, where many previous versions of this article were discussed; Felicity Pors and Lis Rasmussen for welcoming us at the NBA; Gregory Good for welcoming us at the NBL; John Stachel for suggesting some interesting bibliographical references; Luis Gerardo Pedraza Saavedra for drawing our attention to Hsing Yang Yeh's article. This research was supported by the funding agencies CAPES (Brazil), CNPQ (Brazil), and Novo Nordisk Foundation (Denmark).

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[54] Foucault (1994a), p. 810. For a detailed analysis of the concept of appropriation in the quantum theory context, see Hartz (2013).

[55] Foucault (1994b), p. 753.

[56] De Certeau (1990), pp. xxxviii, 58,147-150. See also Koselleck (2004), pp. 89-90.

[57] We recall Albert Einstein's remark: in order to understand the scientists' methods, we must "not listen to their words, [but] fix ... attention on their deeds." Einstein, quoted in van Dongen (2010), p. 9.

[58] De Certeau (1975), pp. 191-197. For a similar situation, see Kaiser (2000), p. 52.

ARCHIVAL SOURCES

BSC-Supp	Niels Bohr Scientific Correspondence, Supplement, NBA.
EP	Hugh Everett III Papers, NBL.
NBA	Niels Bohr Archive, Copenhagen, Denmark.
NBL	Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA.
OHI	Oral History Collection, NBL.
RP	Léon Rosenfeld Papers, NBA.
WP	John Archibald Wheeler Papers, American Philosophical Society, Philadelphia, PA, USA.

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