Leggett, Decoherence, Style

Leggett, Descoerência, Estilo

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Para Iolanda, Chico, Cláudio, Kátia, Luana, Rodrigo e Olival,
que aguentaram um monte até que tudo ficasse pronto, e
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Abstract

Decoherence is one of the most important subjects in contemporary research about foundations of quantum mechanics, but very little is known about how it became so and, even further, how it became a phenomenon. This work goes in the direction of answering the problem by focusing on the work of Anthony Leggett and how he was trying to challenge quantum mechanics. His research was mainly directed to finding concrete situations in which quantum mechanics would break down, but he ended up confirming even further the theory to new directions, including what would later be known as decoherence. All this confirmation heavily contributed to the development of physics, which awarded him the Nobel prize, but left him out of tune with other researchers on foundations of quantum mechanics. At the same time, while the majority of researchers on foundations faced problems in their careers, Leggett never faced any at all. To account for this, we use the idea of style of research and how this shaped the reception of Leggett's work. As style is a concept that is originally from the field of art, we examined the controversy about it in aesthetics in order to reflect about it and find a suitable notion for our use. The result was the identification of style as an individual trait that is both intentional and controllable.

1.History of Physics. 2.History of Quantum Mechanics. 3.Decoherence. 4.Foundations of Physics. 5.Anthony Leggett.

Resumo

A descoerência é um dos mais importantes temas da pesquisa contemporânea em fundamentos da mecânica quântica, porém se conhece muito pouco sobre como esse processo se desenvolveu e, além disso, como se tornou um fenômeno. Esse trabalho se dedica a responder esse problema ao focar no trabalho de Anthony Leggett e em como ele tentava desafiar a mecânica quântica. Sua pesquisa estava principalmente dedicada a encontrar situações concretas nas quais a mecânica quântica falharia, mas ele terminou comprovando a teoria mais profundamente, levando-a a novas direções, incluindo o que viria a ser conhecido como descoerência. Todo esse trabalho contribuiu fortemente para o desenvolvimento da física, o que o agraciou com o prêmio Nobel, mas o deixou fora de sintonia com outros pesquisadores em fundamentos da mecânica quântica. Ao mesmo tempo, enquanto a maioria dos pesquisadores em fundamentos enfrentou problemas em suas carreiras, Leggett nunca teve um desafio semelhante. Para dar conta disso, nós usamos a ideia de estilo de pesquisa e como ela condiciona a recepção do trabalho de Leggett. Como estilo é um conceito que vem originalmente do campo das artes, nós examinamos a controvérsia sobre ele dentro da estética para permitir uma reflexão e encontrar uma noção adequada para o nosso uso. O resultado foi a identificação de estilo como uma característica individual que é tanto intencional como controlável.

1.História da Física. 2.História da Mecânica Quântica. 3.Descoerência.
4.Fundamentos da Física. 5.Anthony Leggett.


IT COULDN'T BE AVOIDED.

Table of Contents

In order to make it easier to navigate through the digital file, we use two different sets of numbers. The first one is printed on page and is individual to each separate text, while the second one is used by the software and is the same for the entire dissertation.

Introduction

Why Decoherence

There are maybe several possible answers to why this topic is such an important endeavor for historical research. The first one that comes to mind is that decoherence is one of the major scientific topics in the twentieth first century. Every now and then, science promises spectacular technological applications, yet most of them are unfulfilled. One recalls nuclear fusion, which promised to provide clean and unlimited energy[1], and is yet to happen, or our last great promise, the human genome program, that was about to finish by the transition of the centuries, making this the century of biology. Maybe it is too soon to deny that we will have a "biological" century, but the genome program is now old news, with most, if not every promise, still a

[1] By 1982, despite all the doubts, mainly regarding costs, the fusion program was still a great promise. Jeremy Bernstein, in an article for The New York Times, claimed that "Now, more than 30 years after they began, it appears that these fusion scientists will, indeed, be successful. Thanks in large measure to the research achievements of the past three years, particularly at Princeton, it seems clear that nuclear fusion could eventually solve a major world energy problem -the production of electricity - and it could do so with acceptable environmental hazard" (Bernstein, 1982). As we know, this was just wishful thinking and, for some critics, it may indeed be impossible to overcome all difficulties related to commercial production of power. In an article published at Science Journal, Parkins claims that the engineering problems related to commercial nuclear fusion electricity doesn't seem to be financially solvable, at least when compared to other power options, such as fission. For him, "fusion power is still a dream-in-waiting. The explanation has more to do with engineering than with physics" and that "the history of this dream is as expensive as it is discouraging" (Parkins, 2006). Seife, in his book about the history of the research on nuclear fusion, claims that the long term research for practical uses of the fusion to generate power is actually dangerous to science, since "the community seems to be in thrall to a collective delusion" (Siefe, 2008).

dream[2], while we constantly hear about the next major scientific application[3]: The quantum computer.

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[2] It is instructive to look at how a major newspaper presented the human genome promises within a 10 year interval. In 2001, when the first draft of the "final" results was being published, it was just a matter of time before we could "decipher the genetic basis of many diseases and in time revolutionize medicine". The big question emerging was "what will the genome tell us about human nature" (Wade, 2001). Genetic determinism was important and society was also wondering if we could understand human nature directly from the tiniest genes. A decade later, the tone used by the same reporter was quite different. Recalling that the USA president at the time, Bill Clinton, stated that the genome project would "revolutionize the diagnosis, prevention and treatment of most, if not all, human diseases", the article argues that "the primary goal of the $3 billion Human Genome Project — to ferret out the genetic roots of common diseases like cancer and Alzheimer's and then generate treatments — remains largely elusive. Indeed, after 10 years of effort, geneticists are almost back to square one in knowing where to look for the roots of common disease" (Wade, 2010).

[3] While out of the scope of our current work, it seems important to note that, at least in the cases here mentioned, even if technological applications didn't become true (yet it is way too soon to affirm that they won't), their scientific results were remarkably important. Both cases have had a major impact on the way we understand aspects of nature and remain as very respectful scientific topics. What is to be questioned is whether it is fair to use this kind of strategy to obtain either public or private funding, at such large amounts of resources, with so little warranties, and if in the long run such strategy might not jeopardize the trust society and governments have in science, making harder to obtain funds for both large and small projects. Yet, it's not fair to only mention failed projects. There are several cases in which the promises indeed became true, and to name just one, we may choose the transistor, developed at Bell Labs. With it, we were able to develop the electronics that would later allow us to achieve microelectronics, and the advent of computers, making this the era of information, dramatically reshaping the way we live. Yet, both the transistor and the integrated circuit were heavily dependent on military funds, and if the military did not buy almost every production of those before they were commercially viable, the revolution that happened because of this would probably be delayed by a few decades. Others good examples are lasers, optical fibers and CCD sensors. See (Kragh, 1999).

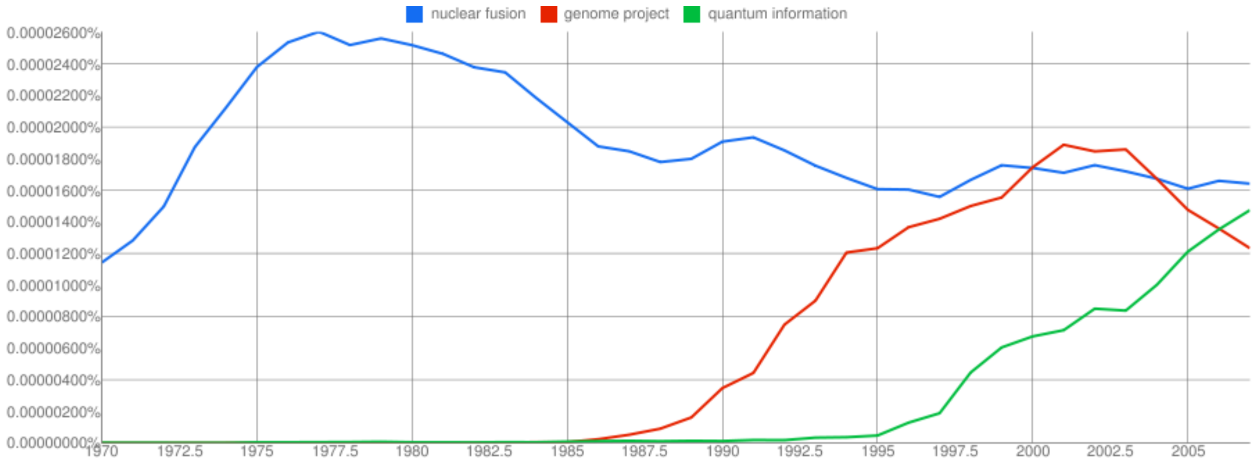

Figure 1 - Occurrence of "nuclear fusion", "genome project" and "quantum information" in books published from 1970 to 2007 with a smoothing of 2. Google Ngram, at May 1st, 2011. It is possible to see the decline of the terms upon the realization that their promises might not be fulfilled, but they remain with great scientific importance.

In a society that is currently driven by computers, there is today the promise of an almost ultimate computer, the quantum computer, capable of processing data and running algorithms in seconds that a regular computer would take millions of years. To be able to accomplish that, a quantum computer uses q-bits, instead of regular bits. So, while a regular computer bit can assume values that are either 1 or 0, a quantum computer bit can assume both 1 and 0 and all the values in between at the same time, benefiting from the quantum superposition principle.

Yet, as powerful as the superposition principle is to deal with processing information, this condition is extremely subtle and very hard to maintain in practical applications. In order to say that a system remains functioning in a state of superposition, all parts of the system must remain entangled, i.e., they must not, by any means, interact with anything outside of the system. They must remain in really perfect isolation. Physicists say that if a system remains entangled, with no outside interaction whatsoever, it is a coherent system, having the property of coherence. Yet, if there is any single interaction with the outside, the environment, as small as it may be, the system loses this property of coherence and it is no longer an entangled system[4].

The only thing that indeed holds back quantum computers is decoherence. Although we do already have operational quantum computers, they are very restricted, with the largest one operating with 15 qubits[5]. These amounts of qubits have no practical applications at all, and the attempts to scale the numbers up face the problem of exponentially growing errors in processing, making a quantum computer spend more time correcting its own errors than with actually calculating, and most of these errors would be caused by

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[4] Technically, it still may remain both coherent and entangled. But, now, in those cases, the system is not entangled just within itself, but with a new larger system. Yet, for practical purposes, this new system does not allow one to make calculations and all the information is "lost". So, for most cases, it makes more sense to understand it as losing its coherence.

[5] *Currently, in 2021, this number has risen to thousands, but this metric is no longer used. The new measurement is called quantum volume and was developed so one could compare different computers with different architectures, since the number of q-bits can mean very diverse computational power depending on several aspects.*

decoherence. So, if decoherence is to be blamed for prohibiting quantum computers, it would be useful to know its history beforehand[6 and 7].

But this is not the only reason to study the history of decoherence. Decoherence has quite an interesting path. The idea that the interaction with the environment might be extremely important in quantum mechanics is somewhat old. One of the first instances[8] is Nevill Mott's approach to the problem of a linear particle-like trajectory in a Wilson chamber from a spreading spherical wave function of an alpha particle. As he says in his 1929 article,

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[6] It is just too soon to make any guesses on whether or not a quantum computer is a viable promise, but after more than two decades of research, there are a few voices indicating that maybe we will never be able to build an actual useful quantum computers, although it seems that it represents a minority, even if not so small. See (Abbott, 2003).

[7] *Again, in 2021, it remains too soon to evaluate whether or not quantum computers will be able to, in fact, revolutionize data processing, but quantum computers are already being used to run tasks and have been able to outperform classical computers in some of them.*

[8] As mentioned by (Bacciagaluppi, 2007).

> the difficulty that we have in picturing how it is that a spherical wave can produce a straight track arises from our tendency to picture the wave as existing in ordinary three dimensional space, whereas we are really dealing with wave functions in the multispace formed by the co-ordinates both of the α-particle and of every atom in the Wilson's Chamber.[9]

In a very similar way, Heisenberg, just one year later, approaching the same problem, tells us that

> 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. Consider first the latter alternative. The system to be observed then consists of one a-particle only, and the position measurement resulting from the ionization [of the atoms at the Wilson's Chamber].[10]

Even if they both were among the first to use those ideas, they wouldn't remain alone for long. After them, yet completely unrelated with the problem of particle tracks in a Wilson Chamber, John von Neumann, in 1932, in his classical book Mathematical Foundations of Quantum Mechanics, and Fritz London & Edmond Bauer, in 1939, would discuss in terms of the measurement problem ideas close to decoherence. This would later influence Adriana

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[9] (Mott, 1929).
[10] (Heisenberg, 1949).

Daneri, A. Loinger and G. Prosperi (known as DLP) (1962) on reexamining von Neumann's proposal to deal with macroscopic quantum apparatus on the measurement problem, examining the thermodynamic behavior of the macroscopic apparatus and its relation with the microscopic system. A few years earlier, in 1955, Everett would also use the idea of coupling the system with the apparatus and the environment to understand it, although this idea would only be published in 1973[11].

All of this would be important for Dieter Zeh, particularly the DLP article. Originally from the field of nuclear physics, Dieter Zeh would make a quite detailed discussion of the role of the environment in the measurement process. Yet, in his seminal article of 1970, he would describe much more a proposal of a research program than to develop actual physical results. During the 1970's, the discussion around his ideas would be completely marginal, occurring in low prestige journals and informal bulletins, outside mainstream physics. Zeh himself more recently called this period the "dark ages of decoherence"[12].

[11] All of the mentioned works were reprinted in (Wheeler & Zurek, 1982), except for Everett thesis, which is published in (DeWitt & Graham, 1973). This initial sequence is based on (Stamp, 2006), and for a detailed account of Everett history, see (Osnaghi, Freitas, & Freire Jr., 2009).

[12] (Zeh, 2005)

It is only during the late 1970's that decoherence would emerge, although being baptized as such only in 1991. Two different groups, working on different problems and independently[13], would also approach the question of the environment, but this time their research would reshape the field of foundations of quantum mechanics and help the emergence of a new field, namely, quantum information. The first of these two groups was formed because Amir Caldeira, a Brazilian student, went to England to pursue his Ph.D. in Physics at Sussex University. There, he would meet his advisor, Prof. Anthony Leggett. Together, they would study the effects of thermal dissipation upon tunneling in SQUIDS, a superconducting device at very low temperatures. The other would emerge from a course taught by John Wheeler at the University of Texas, in Austin, about quantum theory and measurement. In this course, the Polish student Wojciech Zurek asked what would happen if you put a "demon" observing the paths in a Stern-Gerlach experiment. These two completely different physical problems[14], together with Zeh's, would converge on the same question, to understand the impact of

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[13] Also independent of Dieter Zeh's research and problems.

[14] Yet, not so different if we consider that they both use the apparatus of Quantum Mechanics. Indeed, this is exactly what brings these two problems together, and the first one, used by Zeh. So, in some sense, such different ideas and approaches arriving at the same conclusion is a consequence of the huge applicability of quantum theory, allowing physicists to approach such a different set of problems with the same theoretical background.

the environment upon quantum-like properties. In this sense, it gets hard to declare a genuine "birth certificate" to decoherence. The idea is quite old, almost as old as quantum theory itself, but it took a long time to evolve. And, once evolved, it did so in three quite different contexts and problems[15].

Roughly, decoherence began in 1929. Yet, the first uses of this word would only begin during the end of the 1980's, skyrocketing in 1991, with the publication of Zurek's article on Physics Today[16]. Before, it was mainly either dissipation on quantum systems, mostly associated with the research being conducted by Caldeira and Leggett, or superselection rules, associated with the questions brought by Zurek and Wheeler, and also Zeh and his students. But both names mean much more than decoherence. Dissipation is a classical topic in physics, and it is used in the most diverse contexts. Superselection rules are also quite old, and stem from the attempt to find an explanation to why certain theoretically possible states are never to be found in experiments. One quite famous example of such criteria is Bohr's correspondence

[15] Although not common, this is not the only occasion that a scientific idea established itself through extremely different paths. Fox Keller, in a couple of articles, shows that the idea of self-organization entered science first in biology, during late 1800's, then in cybernetics, from early 1900's through mid-century, and finally through complex systems physics, from 1960's, this time in a more definitive way (at least until now). See (Keller, 2008) and (Keller, 2009).

[16] Before, decoherence was being used in a different meaning. The term decoherence curve was quite familiar in the physics literature and meant the coincidence rate versus the spatial separation of a pair of detectors, such as a Geiger counter.

principle, which has no relation whatsoever with decoherence[17]. What this means is that, until roughly 1991, decoherence effects were just plain quantum mechanics. They were not different effects but simply applied quantum mechanics. Yet, after 1991, decoherence became something different, a new phenomenon we might say, a new effect with its own name. This raises two very interesting questions: How did applied quantum mechanics become a “new” effect, and why did this “effect” get a name?

[17] For a concise explanation of the correspondence principle, see the quite interesting article from Darrigol, A simplified genesis of quantum mechanics, the section on the correspondence principle, pages 154-155 (Darrigol, 2009).

Historical Studies on Foundations of Quantum Mechanics

In some sense, quantum mechanics was *ready* by the late 1920's. There was still a lot of work on developing and learning about the theory, but the core of it was complete with the works of Werner Heisenberg, Erwin Schrödinger, Paul Dirac and others. A different problem, namely how to understand this core, was just beginning. In this spirit, debates over this interpretation became quite famous, e.g., at the Solvay conferences (between Bohr and Einstein), and, later, during the 1935 EPR affair, with the response from Bohr immediately after. In the same year, Schrödinger's cat marked the end of this first period of debates, with Bohr and his close circle from Copenhagen being recognized as solving all the philosophical quarrels. This first period does benefit from several historical studies because of two reasons: First it coincides with the birth of quantum mechanics, so all the studies revolving around this major revolution inside Physics also focus on the interpretative dimension. Second, anything related to Einstein and Bohr certainly draws a lot of attention. As the debate got older, however, these studies would become rarer.

When Helge Kragh published his Quantum Generations in 1999, he wrote that

> there are, unavoidably, many interesting topics and sub disciplines that I do not include, in part because of lack of space and in part because of lack of secondary sources. Among the topics that I originally contemplated to include, but in the end had to leave out, are optics, material science, chemical physics, geophysics, medical physics, physics in the third world countries, *and the post-1950 discussion concerning the interpretation of quantum mechanics*"[18].

As Kragh correctly pointed out, there was a lack of historical studies regarding the late debate about foundations of quantum mechanics after the first period, but this void has been fulfilled since then.

Max Jammer did one of the first studies about this period, published as his book, The philosophy of quantum mechanics: the interpretations of QM in historical perspective, in 1974[19]. For him, until the early 1950's, there was the "almost unchallenged monocracy of Copenhagen", that survived apart from some discussion in the soviet context and from prominent names. Yet, in the 1950's this would change.

Olival Freire, in a series of studies[20], argues that we may divide the history of this debate in three different periods. The first one, following Jammer, lasts

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[18] (Kragh, 1999), our emphasis in italics.

[19] (Jammer, 1974)

[20] For this description, see mainly (Freire Jr, 2003), (Freire Jr, 2004) and (Freire Jr, 2009).

from early quantum mechanics to the 1950's. The second, which he names the intermezzo, lasts from the early 1950's to the early 1970's. For him, this period is strongly marked by the philosophical label that was attached to the debate. During that period, most physicists identified the research that questioned Bohr's vision and quantum theory itself as a philosophical quibble, outside the domain of physics. This would have a quite strong impact on most of the physicists that dedicated themselves to this theme, especially on those that decided to focus completely on those problems. In this period we have as main protagonists David Bohm, John Wheeler and Hugh Everett, John Bell, Eugene Wigner, Bryce DeWitt, John Clauser, Abner Shimony and several others, with a special mention of Léon Rosenfeld, because of his relation to most of these cases in defending Bohr's view.

The third period begins in the early 1970's, and is marked by the release of the first issue of the journal "Foundations of Physics" and the realization of the Varenna Summer School, by the Italian Physics Society, dedicated to foundations of Quantum Mechanics. Freire regards both as indications that a community interested in this research had been formed and that it was recognized "officially" as such, marking the establishment of a real controversy over the meaning of quantum theory, a situation that would last until the present day.

Joan Bromberg takes a slightly different approach on this periodization[21]. While Freire indicates that mostly cultural factors were responsible for the revival of the interest about foundations of quantum mechanics, Bromberg argues that the main factor was the development of new experimental techniques. She does agree that the 1970's were a turning point for foundational studies, but mainly because of the experiments that were prompted by the discovery of Bell's inequalities. So, one conclusion that one may draw from her position is that foundational issues were "sleeping", awaiting the technical possibilities that would allow it to develop, as it happened during both 1970's and, specially, 1980's. David Kaiser agrees that the question of the role of instruments in this history is an important one, but ponders:

> that still leaves the question of which came first: revived interest in foundations or new instruments with which to investigate those foundations? Put another way, how are we to weigh the importance of material culture against other likely "causative factors" in the renaissance of interpretative work in QM?[22]

Kaiser himself also examined how research on foundations of QM evolved in the post-war period. Examining how the textbooks used at the universities

[21] See both (Bromberg, 2008 and 2006).
[22] (Kaiser, 2007)

dealt with foundational problems, he identified that questions that were central in those topics disappeared from textbooks as the number of enrollments rose exponentially. This would begin just after World War II and would last until early 1970's, when the enrollments began to drop and foundational and essay-type questions returned to the curriculum[23]. Foundational issues disappeared from American Physics because of the pragmatic necessities of the cold war context, returning to the scene when those necessities were gone and a new context for training Ph.D. 's in Physics had emerged. As this happened at the turn from the 1960's to 1970's, he is closer to Freire's approach on placing more importance to cultural factors than to the lack of instruments to understand how foundational issues returned to the scene in Physics. More recently, he proposed that the approximation between physicists and the New Age movements was

[23] See his forthcoming book *American Physics and the Cold War Bubble* (Kaiser, Forthcoming) and his article (Kaiser, 2002), and for a more simplified presentation, see (Kaiser, 2007). Jammer had already brought attention to the role textbooks had in the disappearance of foundational debates. He wrote: "Impressed by the spectacular successes of quantum mechanics in all fields of microphysics they were interested primarily in its applications to practical problems and in its extensions to unexplored regions. / These pragmatic tendencies in research also had their effect on academic instruction: most textbooks concentrated on teaching how to solve problems and paid little attention to the meaning of the concepts involved. The need to acquire new mathematical techniques left little room for philosophical analysis." (Jammer, 1974). For more information on the role of pedagogical studies for history of science, see (Kaiser, 2005).

instrumental for the return of foundational questions inside mainstream physics and also for the creation of quantum information research[24].

Apart from the debate regarding periodization, several case studies have been developed. They have been instrumental to understanding both the broader context in which the field of foundations of quantum mechanics has been developing since the 1950's and how individual careers and lives were affected by this. Among those studies, maybe the protagonist that has been most analyzed is David Bohm. Freire investigates the reception of his Hidden Variable program, how he was persecuted during McCarthyism because of his connections with communism, his stay in Brazil and how later he developed his ideas[25]. Christian Forstner uses the collectives of thought from Ludwik Fleck to understand why he decided to start a new approach to quantum theory. In the same spirit of understanding Bohm, Alexei Kojevinkov discusses how his Marxist background was important for his research about collective movement[26]. From the same Princeton School, the Everett affair, regarding Hugh Everett's relative states interpretation and how it was sent to Copenhagen by his advisor, John Wheeler, to be "judged" by Niels Bohr

[24] (Kaiser, 2011)

[25] Freire has published several papers on different aspects of this history. For a more complete presentation of Bohm's program and how it developed over time, see (Freire Jr., 1999) and (Freire Jr., 2011). For the context in which he was exiled from the US, see (Freire Jr., 2005).

[26] (Forstner, 2008) and (Kojevnikov, 2002).

before the official Princeton's committee, was first presented by Osnaghi, Freitas and Freire. Later, Peter Byrne published a biography of Hugh Everett. Also, Freitas and Freire examined this case using Pierre Bourdieu's concepts of scientific field and capital[27]. Also in Princeton, Eugene Wigner got involved in the debates in this new context. Just prior to his 1963 Nobel Prize, in 1961 he suggested that consciousness could have an important role during the measurement process. Besides this, as Freire shows, Wigner also had a positive impact on other researchers on foundational issues, bringing his prestige to the field[28].

Outside the Princeton School, the most important development was around Bell's inequalities. The more general work of John Bell deserves a more complete study. Nevertheless, Freire has studied the context in which Bell developed his inequalities as well as their extensions by John Clauser and Abner Shimony, which led to the first experimental tests, and latter to the ones performed by Alan Aspect, and how this was able to change the way foundations studies were perceived. Also, two decades earlier, Bill Harvey examined the context of one of those experiments and how its outcome,

[27] (Osnaghi, Freitas, & Freire Jr., 2009), (Byrne, 2010) and (Freitas & Freire Jr., 2008).

[28] See (Freire Jr., 2007). One student of Freire, Frederick Santos, analyzed how Wigner's philosophical thought evolved during his career in a master dissertation. See (Santos, 2010).

which contradicted quantum mechanics, was ultimately not published, and was only circulated as a pre-print[29].

From this series of studies, Freire coined the term quantum dissidents. For him, those dissidents

> fought against the dominant attitude among physicists at the time according to which foundational issues in quantum mechanics had already been solved by the founding fathers of the discipline. Thus, they challenged the bias against the research on the foundations; and many of them were hard critics of what they recognized as the complementarity interpretation. Their common ground, however, was minimal and focused solely on the importance of the research into the foundations of quantum mechanics. Critical of each other's work, they supported different interpretations of this physical theory and chose different approaches and issues in their research[30].

[29] See (Freire Jr., 2006) and both (Harvey, 1981) and (Harvey, 1980).

[30] (Freire Jr, 2009, p. 281)

Those dissidents, focused only around the period of 1970, were Dieter Zeh, John Bell, John Clauser, Abner Shimony, Eugene Wigner, Léon Rosenfeld[31], Bernard D'Espagnat, Franco Selleri and Bryce DeWitt.[32]

[31] Rosenfeld's case is a little different from the others, as he assumed the role of defending Bohr's interpretation of QM, and, as such, had a major impact on most of the dissident's career. For him, see, besides the works mentioned earlier, (Jacobsen, 2007) and her forthcoming biography of him, "Between Bohr and Marx: Leon Rosenfeld in Physics and Ideology".

[32] Other important works not cited above, without trying to be exhaustive, are the analysis of the Tausk's case at Trieste (Pessoa Jr., Freire Jr., & de Greiff, 2008), the articles examining how the notion of a single Copenhagen interpretation was constructed (Howard, 2004), (Heilbron, 2001) and (Camilleri, 2009b), and, specifically for this research, Camilleri's work on the history of entanglement, focusing more specifically on the early origins of decoherence (Camilleri, 2009a).

Sources and Methodology

This is a work on the history of contemporary science. Kragh, in the preface of his Quantum Generations, says that he has "endeavored to write an account that goes all the way up to the present and so includes parts of very recent developments that normally would be considered 'not yet historical'. There are problems with writing historically about recent developments, but these are practical problems and not rooted in contemporary science being beyond historical analysis"[33] . In a similar vein, we do have to face all those *practical problems* that appear when one does history of contemporary science. The first problem, of course, is sources. Most of the time, one will only be able to use official sources, as most informal documents, such as letters, will not yet be available since the characters are still alive. While this is a real problem, Eric Hobsbawm takes a different approach to it:

> I have little to say about the most obvious limitation on the contemporary historian, namely the inaccessibility of certain sources, because this strikes me as among the least of his or her problems. Of course we can all think of cases where such sources are essential. Clearly much of the history of the Second World War had to be incomplete or even wrong until writing about the famous

[33] (Kragh, 1999).

> code breaking establishment at Bletchley became permissible in the 1970s. *Yet in this respect the historian of his own times is not worse off than the historian of the sixteenth century, but better off. (...) In any case the fundamental problem for the contemporary historian in our endlessly bureaucratized, documented and endlessly enquiring times is an unmanageable excess of primary sources rather than a shortage of them.*[34]

This research, as is the case in most of the history of contemporary Physics, with the exception of themes that are classified, does benefit from a huge amount of published works in peer-reviewed journals. While this is not sufficient, it is, indeed, a great help.

Since we lack letters, we can, then, use other personal sources, and this is specific to contemporary history: we can use oral interviews. Most debates on oral history regards people without written history, in which the speech is (almost) all we have. This is certainly untrue in the case of oral history of science in most cases, and even when we have only an oral report, the interviewee usually is someone with such background that most of debates do not apply well (for instance, the debates about how to deal with a different vocabulary from the formal speak). So, even if we recognize that the scientist's speech is somewhat constructed on the interrelation of interviewer/interviewee, its status is certainly not the same as in the general

[34] (Hobsbawn, 1997, p. 238-9).

case of oral history, as the speech has to be regarded as just one more source, not a privileged source, and, as such, should be handled with the same amount of trust/doubt as everything else. Indeed, even written documents can be partial, incomplete or mistaken.

So, the way we approach oral history in this present work is by acknowledging that every information made available needs to be evaluated in terms of how it was constructed through the interview process and how it agrees with other sources. As for the first part, the information being constructed in a dialogue among interviewer and interviewee, Alessandro Portelli argues, using Vann Woodward, that "each documented interview has two authors: the questioning person and the answering". Portelli adds that "once initialized the dialogue, the distinction between these two functions is never rigid and absolute." And, as such, "this kind of history [life history] is, in fact, a result of the intervention from a specialized listener and 'questioner': an oral historian with its project. He begins the meeting and creates the narrative space for the narrator – who has a story to tell, but wouldn't do in such a way in another context or to another person". While Portelli draws this on his experience doing oral research with mine workers from the Apalache, Lillian Hoddeson, working with Nobel prize winners and top rated physicists, arrives at the same conclusions:

> Interviews are affected by the interests, knowledge, and experiences of the historian who will shape her narrative to fit the interests of her audience. She has the power (and often draws on it) to pattern the historical fabric she weaves. Even the way the historian selects and arranges materials from the oral and written accounts is affected by her ideas, interests, and accumulating knowledge derived from many sources and studies./ In this essay I will explain why, for the case of oral history, I feel this is as it should be. The injected feedback of historians can bring interviewees to usefully rethink, refine, and, in some cases, alter their recollections when they conflict with documents. The process of historical research using interviews may be compared with studying the kind of system (ranging from the quantum-mechanical to the psychological) in which the process of observation changes what is being observed, thus injecting a degree of indeterminism. Interviews can be seen as dialectics that operate between historian and interviewee, between present and past, and between interviews and every other kind of source[35]

As a young historian, Eric Hobsbawm also worked with oral history, when he was dealing with survivors of the pre-Fabian society. From that, he says he learned two lessons: first, that "it wasn't even worth interviewing them, unless [he] had discovered more about the interview theme than they could

[35] For Alessandro Portelli, see his excellent (Portelli, 2010). The citation is from page 212, translated by us. For Hoddeson, see (Hoddeson, 2006), citation on page 206.

remember themselves”; and, second, that “regarding independently verifiable facts, their memory had a tendency to fail”[36].

So, as we see, the role of the researcher constructing the oral interview is central and, to be able to do it adequately, other resources are truly indispensable. In this work, apart from archival material and published articles, which need no further discussion on their use, we also drew on scientometrics.

While there is a lot of debate regarding the uses of scientometrics for science policies, and this is certainly something that will help us learn more about the meaning of such measures, its use as just one more source in history of science, and more specifically in history of physics, is now established. Citations analysis and the growth of the articles of a field/topic can be commonly found in historical articles, and, in fact, it’s very hard to find articles on the field of foundations of quantum mechanics that do not use such resources. In an earlier work, resulting from my first research on the history of science, we analyzed the potential of such uses in the history of physics, focusing on case studies from foundations of quantum mechanics. The conclusions that we arrived at are that they can be both important to confirm some information that you gathered from other sources and as a

[36] (Hobsbawm, 1997).

heuristic tool, giving some information that otherwise would be virtually impossible to find. An especially important use of these methods is to understand when some article became influential, or when some term was applied in the literature.[37]

Every historian knows that it is impossible to write a definitive account as, if not from other reasons, the questions being asked by each generation of historians change[38]. So, while it is possible to think that contemporary history is a temporary account, that will be waiting to be re-written after some time, we need to understand that this is also true to any other history, so we should not worry too much about it.

History of Decoherence

This dissertation is about the history of decoherence. Yet, we don't try to do a full history of decoherence, trying to find its first origins and to map every single time a physicist came to an idea close to the essence of decoherence, but, because of any reason, it didn't have any permanent impact on the field. What we do is to study how decoherence became what it is today: a major

[37] See (Freitas & Freire Jr., 2004). For other works using this kind of data, see (Freire Jr., 2006), (Kaiser, 2002) and (Kragh, 1999) for instance.

[38] For Peter Burke, "it is important to rewrite history for each generation. Each generation, living with present problems, questions the past thinking in its own problems" and "but the historian writes for its own time, conscious that the next generation will do its work in a different way". See (Burke, 2009).

topic of contemporary physics, a textbook chapter, a crucial problem to quantum computers.

And, as we understand it, this history of decoherence begins with Dieter Zeh at Heidelberg. How he turned from a very talented researcher in theoretical nuclear physics to a dissident questioning the very foundations of the theory he once used to apply. From a career publishing on important journals, he was then sending informal articles to the "Epistemological Letters", the very prestigious informal bulletin that hosted some of the most important questions from the community that was debating foundational issues, but had almost zero recognition from mainstream physics. In great part, this fact had to do with the institutional place Zeh was, namely, Heidelberg University, with the influence of Hans Jensen, the 1963 Nobel Prize winner who did not care very much about foundational questions, and always consulted his great friend about those issues, Léon Rosenfeld. The latter, as Bohr's champion, couldn't at all like Zeh's ideas.

After surviving the 1970's, Zeh would return to research when the field was reconfigured in the following decade by Amir Caldeira and Anthony Leggett. This time he was able to gather some students and, with one of them, Erich Joos, he would publish the classical article on the watchdog effect, now with more than 1.5k citations. Despite publishing such an important work, Joos was also heavily affected by the research topic he chose, being unable to secure a permanent position. The history of Zeh's trajectory will be our first chapter.

In the following chapter, we deal with Amir Caldeira and Anthony Leggett, and discuss how they were able to completely reshape the questions around decoherence and the way they should be dealt by physicists. With his unique research style, Leggett approached those problems in a different manner than foundational researchers used to do. Since the late 60's, while still dealing with superfluidity problems, he was already questioning the foundations of quantum mechanics, but instead of using ideal situations and contexts to discuss the consequences of applying QM in them, he instead used extreme situations, modeling them as close as possible to practical realizations. This is the strategy he used to solve the Helium 3 superfluidity problem, for which he was awarded the 2003 Nobel Prize, and also later on the research program to build Schrödinger's cat states in the laboratory. This is the program that he would start when Caldeira was doing his Ph.D. under him at Sussex, in the late 1970's. Together, they studied the impact of thermal bathes on SQUIDS. More specifically, they tried to understand what would be the effect of dissipation on the tunneling effect inside SQUIDS.

In the third chapter, we will focus on the path that led Wojciech Zurek to write his famous 1991 article on Physics Today, an article that may be considered the "official" birth of decoherence – this term has been widely accepted and used only after the publication of Zurek's paper. About a decade earlier, Zurek was being formed inside the newly formed "school" of John Wheeler at the University of Texas, in Austin. Despite not doing his Ph.D. under Wheeler, Zurek shared the interest about foundational issues and enrolled in a course taught by Wheeler, Quantum Theory and Measurement, in which he studied such issues and, later, in 1982, they published together a collection of classical articles on this subject in a book with the same title. In the final essay of the course, Zurek developed the idea of a "demon" atom inside a Stern-Gerlach device that would watch the path of the atoms. These would be the roots of his 1981 and 1982 articles that paved the way for decoherence.

We conclude in the last chapter by discussing how decoherence became something different than just applied quantum mechanics; how it got a name. We will show that until 1991, there were two different studies in which decoherence was being developed, superselection rules and macroscopic quantum interference. After that, decoherence was the new standard and became a phenomenon per se. We will also focus on why Leggett and Caldeira were commonly forgotten, linking the answer with the style of their research and the philosophical and programmatic discourse that was attached to it, because, in some sense, they were out of resonance with other's foundational researchers perspectives at that time, as Leggett was quite critical of QM, and was trying to prove it wrong, while almost everyone else was marveled with the consequences of the experimental tests of Bell inequalities, and the word of order was nonlocality and its implications. Yet, despite that, their approach on foundational problems reshaped how this kind of research would be done and showed to mainstream physics that they also could study those problems, without a need to commit themselves to a particular critique or interpretation.

Also, we will examine how this case study fits the literature on the development of the field of research on foundations of QM after 1970's, using careers trajectories to evaluate how the decision to do research on foundational issues affected each protagonist. It's noticeable that everyone, with the exception of Caldeira and Leggett, had problems related to their choices of doing foundational research. It seems that only after the emergence of quantum information science foundational questions became a safe topic for a Ph.D. student.

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Postscript

There has been over ten years since this was written. Upon returning to finish this dissertation, some choices had to be made and, as postscripts usually are written last, the final product stands, in some points, a bit far away from what the introduction text described. As it was being reviewed, it became clear that some changes could be made, it would not take much work. Still, I decided to keep it as it was, with just a few commentaries, stylistic changes, and minor corrections. Why keep it as it was?

The first answer is that the text remains useful as a general introduction to the main theme of the dissertation. It dealt with several aspects of the field and, fortunately, it still makes an acceptable description of it[39]. It also has several flaws, mainly a rather shy discussion of methodological aspects of the research. This leads us to the second answer that the preceding text is a portrait of my reflections at the time. Despite its problems, this pleased me, it allowed me to consider some choices that were made then and to understand how I have changed over time. For the reader, it may not be possible to see

[39] This could, obviously, be disputed. The main problem, in this aspect, is the lack of mentioning the several works that appeared after it was written. The reason for not updating it to include those works is that, while they advanced the field, they did not change the general landscape of it. This is due mainly to how Freire's work had already set the main theoretical aspects that should be faced by researchers on the history of foundations of quantum mechanics when the text was first written.

this clearly, but as part of my own process, this became something with value that should not be overlooked in such work.

It is, for instance, the reason that the second annex will be present in the dissertation. “Power Relations in Science: The Bohr and Wheeler-Everett Dialogue on the Foundations of Quantum Mechanics” was a side project that was written in the early years of this dissertation. It began as a communication in a conference and its results were more satisfactory than it was expected first. As a historian, I do make the choice of not committing myself with any theoretical models. I shall use them as they appear useful in the specific cases I am working and leave them as they are no longer necessary. They are an important and integral part of my work, but the narrative, not the model, the questions that arise and the discussions they entail are the main focus. Still, it appeared as a fun project to see how well Bourdieu’s description of the scientific field could be applied to describe one specific case, one that we had already dealt with in a previous work, which was my master thesis and the articles that it produced. The study of Bourdieu, despite the difficulty associated with it, seemed interesting and the theoretical questions that it brought seemed necessary to understand the contemporary practice of science. The field of opposing forces, of competing peers that want to reach the monopoly of authority over scientific capital appears as a suitable description of a science that is more and more focused on recognition. Press and public relations departments are now a necessary part of any major university, as the dimension of relative autonomy of the sciences stands as a problem: One needs recognition that is given by its own peers, but the search for funding can be heavily dependent on the public perception of the importance of such research. This could be further discussed, but it leads us away from our own problems. The point is that Bourdieu's description strongly helps us to find important questions that are certainly useful to be answered that helps us to understand contemporary science. Despite being a full published article, it enters the dissertation as an annex for reasons of coherence.

There is also one further reason for it to be a part of the final dissertation. Despite returning to the Ph.D. program in 2018, beginning once again in 2019, the work remained mainly stopped. The same reasons that held this work to be ready for so long were still affecting the pace of work, but in 2020, during the beginning of the pandemic, I was invited to publish this work in a special edition dedicated to Bourdieu and historiography. They offered to translate the text and they would be responsible for reviewing the translation, I could publish it just as it was. Yet, the invitation led me to review it and it was possible to identify several aspects of the text that could be improved. As the main changes involved explaining better Bourdieu's thought, I had help from Dr. Iolanda Faria, a specialist in the use of Bourdieu to study the scientific field, who also happens to be my wife, so this part was developed to a more precise description of his ideas and the final result was a much improved version of the earlier work. But if the work was only satisfactory, the psychological effects of it were extremely valuable. It helped me with the task of facing a text and writing it, with both a beginning and an end. For quite some time, this didn't seem possible, but this article helped change it.

Returning to the question of why not change the introduction, we then face the problem of the three chapters that were described in it, with just one being part of the final work. There are two answers for this question, the first about the importance of the chapter that remained in the dissertation and the second about my own personal questions and motivations on academic work.

To write this dissertation, there was extensive field work. We performed five interviews in 3 different countries, consulted several archives in diverse cities, spent over six months travelling to consult those and discussed it in several conferences and with a lot of colleagues and researchers about it. And, as the work progressed, it became clear that the main part of the dissertation was the history of Caldeira and Leggett. It is not that this part is more important than the others, but that the questions and problems that arise as a consequence of it are, in some sense, more urgent. First, in the general theme of the dissertation, as it will become clear reading the specific chapter, the role Leggett and Caldeira had in the development of decoherence were much more central. They developed the main technical results that established the framework for others to further study the topic. In comparison, Zeh's and Zurek's works were much more limited in this sense. They did, also, solve important technical problems, but their work was much more focused on a very specific dimension of use of decoherence. While all of them were completely focused on foundational problems, only the work of Caldeira and Leggett could be used in a much more neutral stance. As such, focusing first on their history had the advantage of explaining more of the dynamics decoherence faced to be understood as a phenomenon than the other characters. As, of course, the others remain with importance in the current context of the field of history of foundations of quantum mechanics, we shall deal with them later, just after the dissertation.

A second aspect is also central to why this part is more urgent than the others. While Leggett is, by no means, an unknown in the world of physics, as he won nearly every possible prize that is available, including the Nobel, his identity as a researcher in foundations of physics is virtually unknown, or, maybe, unrecognized. We can take, for instance, his work awarded with the Nobel. He meant it as a challenge to the applicability of quantum theory, trying to show that it would break down in the specific problem of the superfluidity of He3. While he arrived at the exact opposite result, confirming even further quantum theory, his motivations, his main line of research, disappeared from the general perception related to this. It is not that physicists do not know that he is interested in the foundations of physics, as he has been quite the opposite of shy in defending his ideas. It is just that they ignore this dimension of his work. And, as a consequence of this, we mainly don't know so far how much he was involved in the field, as there is not a single article written dealing with this.

Then, we have the third reason why his history is so important. This dynamics, by itself, is already something very interesting to look further, to understand how this happens. It is a dynamic of someone trying to do something, but the majority of the field just ignores what he was trying to do and uses its work in a different sense. And, if this general description is already interesting, in this case we have an additional reason. Nearly every single physicist that decided to fully dedicate itself to foundations of physics in the second half of the XX century had faced problems in his career, and here problem is an understatement, most of them had their careers halted or severely affected because of such a decision. Leggett, on the other hand, had not a single drawback because of this. This problem helps make this history stand in importance. Also, this demanded a major theoretical effort in order to explain this exception. We found the answer by examining how he chose to tackle the objects he researched and decided to call this his style.

Style, of course, is a rather common word, with a not so clear meaning, despite the fact that everyone, in some way, understands what is said when someone says style, even if they don't understand it exactly the same way. Its use is also a bit common in science, but remains loose in some sense. Style, as a concept, comes first from literature, but soon became a central problem in Art studies, or aesthetics in general. So, it seems just natural to examine the kind of problems they deal with when using style in order to help us think what exactly we mean when we talk about style. While this was supposed to be just an accessory discussion related to the chapter on Leggett, this became a major problem by its own merits and became a chapter by itself. To understand the discussion about the concept of style was a much bigger endeavor than we thought at first, but facing it also brought more fruits than it was expected at first. In this chapter, the second one in this dissertation, we also did a brief examination of style on three different physicists, Leggett, David Bohm and John Wheeler, showing how this concept can enrich the studies on the history of quantum mechanics.

When the first draft of the style chapter was ready, one question emerged regarding the superposition of the notion of style and those of symbolic capital and strategies from Bourdieu. I did begin to sketch an answer in what was first meant to be a footnote. The problem grew and I decided to make it a two or three pages long annex. In the end, we have a 4000-words essay on the specificity of science and art. The reflections the text arrived at are more speculative than the rest of the dissertation. While the major part of it is a reflection on the specific nature of each field and how, even being different, science and art can get very close to each other, just as Bourdieu analysis in terms of intellectual fields, in the end there is a discussion that allows us to consider the possibility of making art through science and vice-versa. While this is not something that happens, when we consider how other intellectual fields developed during the last few years, the thought of science entering the domain of art does not seem like complete nonsense.

As it should be clear by now, the theoretical development entailed by the first chapter led us to a direction that was not predicted when the dissertation began, or when it was half written about ten years ago. This takes us back to the question of why only one of the three originally intended chapters became ready in the end. If there was a necessity to reflect on the question of style, the main reason this was so further developed is mainly due to the fact that the kind of challenge I had to face to write this dissertation changed abruptly from the beginning of this project towards the end. When it first started, I was a very young student that had become somewhat known for having some talent for the history of quantum mechanics. This was very important to me and it seemed like a very natural path to follow. In the meantime, my life changed, the way I could, and couldn't, work became something else and, in this new context, it was harder to do the same kind of things that was so easy to accomplish a few years back. This is not something that was immediately clear, and the trouble with being able to understand that is one of the main reasons it took so long to finish it. At some point, earlier this year, the possibility of reflecting in different ways about the nature of science, the nature of the field of foundations of quantum mechanics, expanding the discussion with the presence of other fields, brought back the kind of intellectual challenge that once was so important for me. The final result of this dissertation reflects this extremely diffuse path that took me to the place I am today. In its essence, the two chapters, and also the annexes, deal with the same problem: How can we address the problem of what science is in the contemporary period? Even if we still focus mainly on the foundations of quantum mechanics, and the narrative in this field is also a major and important part of this work, a general reflection of what is this thing called science is what allowed the work to be done. I do hope that the final work has made some contribution to this specific question.

Tony Leggett's Challenge

Tony Leggett's challenge to quantum mechanics and its path to decoherence

Introduction

It would be complete nonsense to even consider the idea of a Nobel Prize winner as someone unknown. However, the way one understands the career of others can be quite selective, separating what is most valuable in a specific context from what is undesirable, that needs to be forgotten. In some sense, this is exactly the case of Sir Anthony Leggett. He has virtually all the recognition a physicist can have, such as the Nobel Prize, the Wolf Prize, becoming a fellow of the Royal Society, and a Knighthood from the Queen of the United Kingdom. Yet, most of this recognition arose because of his technical solutions to very difficult problems, more specifically understanding the superfluidity phase of Helium 3 and how macroscopically large quantum systems behave upon interaction with the environment. However, while these problems did indeed contribute to the development of physics, there was also great philosophical insight involved in these solutions that, in some sense, remain ignored by most physicists. Furthermore, it is important to emphasize that this kind of reasoning, namely unifying physics and philosophy, came way before his recognition as a major physicist. While Anthony Leggett started his research program that led to decoherence only at the end of 1970's, the decision that drove him to foundational studies was taken long before. We could say that it happened before he even became a physicist.

Decoherence is the brand name for the coupling between a quantum system and its environment. Through this coupling the system loses the superposition of its eigenstates, which is the singular signature of a quantum system. Technically, the system's mathematical description evolves from a pure state to a mixture, and conceptually decoherence concerns the transition from the quantum to the classical description. Modelling such evolution was the goal pursued by physicists who worked on it during the 1980s. Eventually, it was taken to the lab and the experiments were the rationale for the 2012 Physics Nobel prize awarded to Serge Haroche and David Wineland. There were several distinct and independent roads to conceptualizing and calculating decoherence and the physicists involved in this endeavor included others such as H. Dieter Zeh, Erich Joos, and Wojciech H. Zurek, (Camilleri, 2009; Freitas, 2010). In this paper we focus on the road taken by Leggett and pay some attention to the contributions of Caldeira, who was his PhD student. By focusing on Leggett's trajectory, we are interested not only in his contribution to the establishment of decoherence, but also in his singular approach to the foundations of quantum mechanics.

The paper is organized as follows: sections 1 and 2 are dedicated to Leggett's path from philosophy to physics and his own singular way towards research on foundational issues in quantum physics; while sections 3 and 4 deal with his conceptual and technical approach to the foundations of quantum mechanics. Section 3's heading - How to put Schrödinger's cat in a lab? – refers to his attempts to devise systems which could be used to describe the existence of linear superposition of macroscopically distinguishable states and still apt to become a real experiment instead of a Gedankenexperiment. Macroscopic quantum tunneling with SQUIDS was for Leggett the best candidate for it. Section 4 presents the work of Caldeira, under Leggett's supervision, on how to model such systems and its main conclusion, namely, that damping always tends to suppress quantum tunneling. Section 5 is where we discuss how Leggett challenged the validity of linear superpositions, that is quantum mechanics validity, at the macroscopic level. We suggest an explanation for why he did not suffer professional obstacles related to his point of view. To conclude, in section 5, we present another explanation for a different but related issue, namely, the undervaluation and even the scant acknowledgement of his views on the foundations of quantum physics among physicists and philosophers who work in this field.

Physics and Philosophy

When we look at the history of foundations of quantum mechanics, we tend to separate it into two different periods regarding the background formation of physicists. If we approach the founding fathers and mostly in the pre-War European context, we usually consider that the protagonists had a solid knowledge of philosophy. Einstein, Bohr, Heisenberg, Pauli and several others, having studied in the late XIX century and early XX, had a somewhat strong philosophical background[1], as was shown in the debates around the newborn quantum theory. As we move to the second wave of debates, mostly after World War II, and more specifically during the early context of the Cold War in the United States, the training of physicists was quite different. With early roots in the pragmatic character of American Physics and the new needs of Cold War for scientific training, philosophy was not considered an important topic[2]. One interesting point of comparison is the different receptions to both newborn Quantum Theory and QED renormalization techniques. The fathers of Quantum Theory engaged in long lasting debates over the meaning of the new theory during the 1920's and 30's, yet when Feynman, Schwinger, Tomonaga and Dyson reformed QED during the 50's,

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[1] At least for physicists' standards.

[2] See Kaiser (Kaiser, 2002) and (Kaiser, Forthcoming) for the training of quantum mechanics and (Kevles, 1977) for a broader context about physicists in the United States. See also (Schweber, 1986) for a general tendency for pragmatism among United States theoretical Physics.

applying very efficient “patches” to solve infinity problems, there were no deep philosophical questions involved. The important thing was that these “patches” worked and solved the problems they were intended to solve. Physics in its practice and the training of physicists had changed. As Schweber suggests, “The workers of the 1930s, particularly Bohr and Dirac, and also Heisenberg, had sought solutions in terms of *revolutionary* departures. Special relativity and quantum mechanics had been created by revolutionary steps. The solution advanced by Feynman, Schwinger, and Dyson, was *pragmatic* and *conservative*” (Schweber, 1986, pp. 97-8).

Leggett would, somehow, be misplaced in this new context, belonging to an older era. While we are used to physicists embarking on a science career still very young, Leggett, born to a couple of Physics and Mathematics teachers, actually wanted to follow the more prestigious path at the time, the humanities, more specifically the Greats[3]. Going to Oxford in 1955, he had the opportunity to study the classics, Greek and Latin, classical literature, and a considerable amount of philosophy, the field he actually considered following in his later career. In fact this decision had already been made quite early, when he was 13. Such an early decision also had another impact; he studied hardly any science before college, nor during it. The joke he tells about the influence of such studies in his later career is that, unlike his fellow

[3] Also known as classics or by its official name, *Literae Humaniores*.

physicists, he knows the actual meaning of Greek letters. As we shall see, this influence goes far deeper than that[4].

Figure 1 – Anthony Leggett, 1983. Courtesy of AIP Emilio Segre Visual Archives. Credits: University of Illinois.

However, at some point, Leggett decided that he did not really want to become a philosopher, as he "was very dissatisfied with the fact that there seemed to be no hard subject criteria in philosophy as to whether what you're doing was right or wrong" (Leggett, 2005). This also had to do with the way philosophy was being practiced in the English context, being much more around analytical philosophy and less focused on ontological and

[4] For his biographical data, see mainly his Nobel Prize biography (Leggett, 2003). Additional material used in this dissertation includes interviews by Babak Ashrafi on March 25, 2005 (from now on [Leggett, 2005], and by me, on August 3, 2011. I would like to acknowledge the Niels Bohr Library Archives and Babak Ashrafi for allowing me to consult this first version of the interview.

epistemological questions that would later be present in his professional career. Furthermore, with such limitations in mind, science became more appealing, more objective. “I kind of felt that I wanted to work in an intellectual discipline in which there were, in some sense, hard objective criteria on whether your ideas are right or not” [Leggett, 2005]. There is a bit of irony when we realize that later he would be doing research that has been classified as “Experimental Metaphysics”[5].

For the change to be possible, it would be necessary to get a second degree. It was usually very hard to obtain funding for second degrees, but he benefited from the Sputnik effect. With the launch of the first artificial satellite by the Soviet Union, the west became extremely worried about the shortage of scientific manpower. It became urgent to “produce” as many scientists and engineers as possible. As he has described, “I only note the debt I owe in this context to the former Soviet general Sergei Korolev.” (Leggett, 2020)

Even so, the transition was not easy for Leggett. First, he had no scientific background, and, second, he wanted to get a classified degree, which meant that he had to finish both his degrees in under 6 years. Now, he had 2 years as

[5] As coined by Abner Shimony to describe mainly those experiments to test more specifically Bell’s inequalities, and as a byproduct to test ontological and epistemological questions about the nature of space-time and of our knowledge of such. See, for instance, the volume dedicated to him, “Experimental metaphysics: Quantum mechanical studies for Abner Shimony” (Cohen, Horne & Stachel, 1997).

he had already spent four on his first degree, which meant that he had to finish his degree in physics in half of the regular time and with no background training in science. Of course this was no simple task, and the fact that indeed he was able to finish in time was evidence of the talent he would later show throughout his career. In 1961, he earned his second degree, and a few years later, in 1964, under the supervision of Dirk ter Haar, he obtained his Ph.D. in Physics, with a dissertation on Condensed Matter, a field that would mark his whole career.

The path to foundations

The 1950s and 60s marked a change in the foundations of quantum mechanics (Freire 2003, 2004, 2015). From the whole debate that took place during these years, two main themes would mark Leggett's path. The first one was David Bohm. In 1951, while still in Princeton but on the verge of going to Brazil, he developed his new formulation of Quantum Theory, the so-called Hidden Variable program. David Bohm proposed that we could use additional hidden variables, in the form of a quantum potential, in order to fully describe the dynamics of quantum systems, which would allow us to calculate trajectories for quantum particles. However, more importantly, his proposal marked a return to classical determinism. The Heisenberg relations would still remain valid, but not as a limitation from nature, emerging from the uncontrollable interaction of the quantum system with the measuring apparatus. If not from that, quantum particle movement would follow a pretty regular path with trajectories predicted by the theory. While its development lacked more concrete results, with its predictions remaining in a very limited set of results, with no relativistic generalization, this concept marked a whole generation of physicists interested in foundational questions by showing that a new conceptual scheme could, at least in principle, be developed and used to replace "regular" quantum mechanics.

The second concerns Bell inequalities. Extremely influenced by David Bohm, in 1964 John Bell questioned if it would be possible to construct a model with hidden variables that could yield different results from ordinary quantum theory, as Bohmian Mechanics does not. Although using hidden variables that were different from Bohm's (Bell's model used local hidden variables, as Bohm's were non-local), he could show that in some very specific experimental contexts, no theory using local hidden variables could predict exactly the same results as ordinary quantum theory. Even though he did not have a "theory" in the same sense as Bohm's, his result opened the possibility of testing if quantum mechanics could possibly be wrong, and if so, whether there would exist local hidden variables. Yet, his proposal still needed to be developed before reaching the laboratories, and it was mainly John Clauser and Abner Shimony who were responsible for this in 1969, with several experiments being performed in the following decades.

As Freire has indicated, these debates around foundations of quantum mechanics received the stigma of being non-scientific, philosophical (Freire Jr, 2009). While this would certainly be a problem for many physicists, Leggett, with such a unique background, became quite interested in them. Despite carrying out typical technical research on low temperature physics, he frequently paid attention to foundational debates. The actual turning point was in 1972, after a series of lectures given by Brian Easlea, at the time also at Sussex. Easlea had first developed a career in Physics, but then moved to other topics such as history and philosophy of Physics and later social sciences. During the late 1960's, he lectured on the classic foundational problem, the so-called measurement problem. This contact made Leggett rethink his entire career and take a drastic position: he would no longer do the kind of physics that was published in the Physical Review B, the main journal for low-temperature and solid state physics.

This was not a trivial decision. To stop doing mainstream physics and begin focusing on research that could easily be identified as at best philosophy, or at worst mumbo-jumbo, had been a problem for most scientists who chose this path. Fortunately, Leggett had an advantage that few taking the same decision as him also had, namely a permanent position. He was conscious of the problems such decisions could entail for his career, yet he knew that at least he would still keep his job. As we will see, it is interesting that despite committing himself almost full time to foundational research, this would never pose a problem for him.

While the regular path of "dissidents" (Freire, 2009, 2015) was to focus on very specific conceptual problems of quantum theory, Leggett took a different approach. With his training in low temperature physics, a field that had been blooming since the late 50s, with several new problems and theoretical challenges, he chose to face one that was quite unique: the superfluidity of Helium 3[6].

Helium 3 is one of the isotopes of Helium. It is extremely rare when compared to the most common isotope, Helium 4[7]. Its abundance is so low that it has become one of the most expensive materials on earth. At the same time, its applications are quite vast, including atomic bombs and nuclear fusion reactors, but more commonly they serve to refrigerate systems under 1 Kelvin, temperatures needed on almost every particle accelerator. The situation is so extreme that mining it on the moon is even being considered

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[6] For a portrait of the field at the time, see "Solid State and temperature and low temperature physics in the USSR", organized and half written by ter Haar, Leggett's Ph.D. advisor; and "Key problems of Physics and Astrophysics", written by Vitally Ginzburg, who shared the 2003 Physics Nobel prize with Leggett (and also Alexei Abrikosov). See (Organization for Economic Co-Operation and Development - OECD, 1964) and (Ginzburg, 1975).

[7] "The abundance of He4 is 10^7 times that of He3". (Ginzburg, 1975)

(Wittenberg, 1992). Recently, a shortage of He3 affected the working of several physics experimental centers[8]. However, if Helium 3 is quite hard to find, its theoretical and experimental studies were abundant. By 1962, it was not clear yet whether it would have a superfluid phase or not. Dirk ter Haar described it in 1964 as follows: "Gor'kov and Pitaevskii (1962) have studied the possibility of a transition of He3 into a superfluid state. This might happen through the formation of so-called Cooper pairs as in superconductors. (...) They estimate this transition to happen between $2 \cdot 10^{-4}$ and $8 \cdot 10^{-3}$ °K". Ginzburg also examined these, portraying them as extremely difficult problems: "L. D. Landau told me once that his attempts to solve the problem of the second-order phase transitions had demanded greater effort than any other problem he had worked upon". More specifically, "It has been discussed for ten years already that the atoms of ^3He may 'adhere' to each other forming pairs with an integral spin and undergoing Bose-Einstein condensation (...) transform to some superfluid state. Such a state is analogous to a superconducting state but, as ^3He atoms are neutral, the atom in this state must be superfluid rather than superconducting; however, superconductivity may also be called superfluidity, but in a system of charged particles. (...) Meanwhile, it was found in 1972 and 1973 that not one but two phase transitions occur in the liquid ^3He under very low but yet attainable temperatures of about $2.7 \cdot 10^{-3}$ and $2.0 \cdot 10^{-3}$K (under the pressure of about 34

[8] When it happened, the price of one liter of He3 rose to over 2000 US$ (Adee, 2007).

atm, though).” These were the works of Douglas Osheroff, Robert Richardson and David Lee, on the experimental part, and Leggett, on the theoretical dimension, and these phase transitions are precisely what Ginzburg called “exotic transitions”. The first three were awarded the 1996 Nobel Prize in Physics and Leggett the 2003 one. Ginzburg concluded that “studies into the superfluidity of ^{3}He will, undoubtedly, make up a whole new chapter in the physics of low, or, better, to say ultralow, temperatures”[9].

Leggett realized that the superfluidity of He3 was one of those phenomena that was indeed unique, and that it could reveal deeper aspects of nature. In fact, in such extreme conditions, it might even be possible to show that Quantum Mechanics would no longer hold. For him, this was the opportunity to show that QM would break down. But how could he show this? The answer was, in some sense, quite simple. He just had to apply quantum mechanics to the problem. Well, it was far from a simple problem. The quantum explanation of superfluidity was quite new, as we have seen. So, Leggett set himself the task of “solving” this problem, i.e. applying quantum mechanics to model and describe it. The catch to showing that QM would break down is indeed to be able to apply it “correctly”. Then, if it was well applied to the problem and in fact the theory was not able to handle such an extreme situation, the experimental results, already available, would differ from the

[9] All the citations above from Ginzburg are from (Ginzburg, 1975).

best theoretical models. “And, I had actually, I got so interested in the foundations of quantum mechanics over the last few years that I had actually been intending to go off and do that. And, I thought, in fact, I actually said to myself, ‘When I come back from this holiday in Scotland, I’m going to sit down and really start reading quantum measurement literature and so forth and really go into this in a big way.’ But, this result of Bob’s quite literally struck me so surprisingly that I seriously began to consider the possibility that it was evident, the first evidence that quantum mechanics was breaking down under these very extreme—because you have to remember, you’re dealing with a very dense system at very low temperatures where almost no one had been before. These were conditions which were really quite anomalous by ordinary terrestrial standards. And so, was it conceivable that quantum mechanics was actually breaking down?” [Leggett, 2005] Everything was on track until something unexpected, at least to Leggett, happened. In the end, he failed to show that QM would not work, rather, he showed that it worked perfectly!

From the point of view of foundational research, nothing interesting happened here, but from a wider perspective, Leggett solved an extremely challenging problem, one that saw him awarded several prizes. Since then, he has always been recognized as an extremely talented physicist, and was able to secure a high flying career, crowned in 2003 with the Nobel Prize in

Physics. While not central to the later development of research around superfluidity, there is a point which is important to highlight. Together with the hope that QM would break, there is also his perception that Solid State physics and theories were just as fundamental as microphysics, in the sense that they were not just a mere application of QM, but a fundamental theory of its own. It is the idea that the properties of large scale matter, in the context of solid state physics, would not be just a consequence of the properties of the individual atoms, and the theories are also not deductible from QM. For him, this would guarantee a consistent view. There are, of course, deeper meanings involved in this view, but with it he could both keep his research on superfluidity and approach a different problem, to show QM wrong[10].

His following step as a foundational researcher was down a different path. Going to the African continent to work a non-consecutive year at the Kwame Nkrumah University of Science and Technology, in the city of Kumasi, Ghana, Leggett had much more free time than he was used to. Because of this, in addition to teaching, actually the only time he presented a course on

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[10] For his ideas about the fundamental aspect of solid state physics, see mainly his 1992 article. Also, he presented those ideas in his popularization of physics book, The Problems of Physics, from 1987. Similar lines of thinking have been presented by both Philip Anderson in 1972 and, before, in 1961, by Brian
Pippard. Recently, Joe Martin has been studying how the debates around this problem had any effect on solid state physics in the United States. See (Leggett, 1992), (Leggett, 1987), (Anderson, 1972), (Pippard, 1961), (Martin, 2015) and (Martin, 2018). We would like to thank Christian Joas for bringing my attention to these debates and Joas and Joe Martin for valuable discussion on this.

Quantum Mechanics per se, he was also doing research. However, with very few resources and without being able to use the current literature, he decided to approach a topic for which a lack of literature available wouldn't make a difference: developing a new type of Bell inequalities. This is both an indication of his interest in the debates about the foundation of quantum mechanics, and also an indication of how this field was seen. The fact that a professional physicist understood that he was able to do research in such a field without literature shows how low the perceived prestige was and how little research was being done around it. Leggett did write a paper, but only published it this century[11].

How to put Schrödinger's cat in a lab?

Upon his return to Sussex, his new research program emerged. This time, he was not alone, but accompanied by Amir Caldeira, his new Ph.D. student. Originally from Brazil[12], where later he returned to follow his career, he had been a student at the Pontifical Catholic University of Rio de Janeiro (PUC), then one of the most prestigious Departments of Physics in Brazil. In 1964, Brazil underwent a military coup, in which the democratically elected

[11] See (Leggett, 2003). For the experimental tests, see (Gröblacher, et al., 2007). See also the editorial comment by Aspect (Aspect, 2007).

[12] All the biographical data comes from an interview by Olival Freire Jr. and Fabio Freitas, January 12, 2009. From now on, all the quotes are (Caldeira, 2009). As the interview was conducted in Portuguese, all translations are by the Author.

government was overthrown. The dictatorship, with direct support from the United States of America, initiated a policy of suppressing civil rights and persecuting civilians who had any sympathy for socialism and left wing parties, and later, more broadly, anyone who criticized the government. With this policy, several physicists had to flee Brazil, while others were arrested and tortured, including students. Despite not being directly affected by this climate, Caldeira was raised in this context. One year before he went to university, in 1969, Luiz Davidovich, today one of the most important Brazilian physicists, was expelled from the same university[13].

Despite the political turmoil, PUC was a distinguished university. Apart from Nicim Zagury, who supervised Caldeira's Master's degree, he also had classes with Andre Swieca, Luciano Videira, Moyses Nussenzveig, then at Rochester, and Jayme Tiomno, former student of John Wheeler, who also had been exiled from the country a few years earlier for political reasons. After starting engineering, he later switched to physics, graduating in 1974. He then chose to join the Masters Program in Physics at the same university, with Nicim Zagury[14] as supervisor.

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[13] Despite not being directly connected to Leggett and Caldeira, Davidovich would be a key member doing the theoretical part of Serge Haroche's experiments on decoherence in the 1990's, which led to the Nobel Prize of Haroche. (Freire Jr., 2015)

[14] Zagury would also be a part of the theoretical team from Haroche's experiments on decoherence.

In 1976, Caldeira presented his thesis, “A study on relaxation and parametric excitation in two coupled bosonic systems”. The problem, proposed by Zagury, involved studying the effect of dissipation in a coupled bosonic system interacting with a reservoir, using quantum mechanics. They were trying to develop “a systematic treatment for the study of relaxation and excitation of two coupled bosonic systems that interact with a reservoir”[15]. This research took six months longer than he expected, delaying his plans. Like most skilled students in Brazil at the time, he was eager to do a doctorate abroad. The obvious choice would be the United States of America, as the majority of Brazilians physicists had been trained over there. Yet, as the USA had been directly involved in the Brazilian coup, Caldeira's generation was not so keen to study there. Also, for them, together with the political contempt, the USA did not seem to offer the same kind of personal experience as Europe could provide. Yet, the American influence over Brazil would still help lead his fate, as he spoke English. The other choice of an English speaking country that had a tradition in Physics was, obviously, England. “Why England? For a simple reason, and it was a political reason, because the United States was that thing, that prejudice against Americans. Some colleagues may say they hadn't [prejudice], but at the time this was quite common. Also, living in Europe was more interesting.” [Caldeira, 2009]

[15] For the master thesis, see (Caldeira, 1976).

Caldeira was accepted at Sussex and received a scholarship from the Brazilian federal agency CAPES. While not his first choice, it soon became clear that his natural supervisor was Leggett. As he was still in Ghana, and then in the USA for a year, Gabriel Barton became a provisional advisor. Upon his return, Leggett posed a problem about nucleation on Helium 3, which should be caused by a false vacuum decay. By this time, Leggett had lost interest in it, but Caldeira was looking for a problem on phase transitions, so this would be something interesting. Leggett left Caldeira working alone on this very difficult problem: the system had 18 degrees of freedom. Despite liking it, it seemed more difficult than what was usually required for a Ph.D. Luckily, he soon got to know Terry Clark and SQUIDS.

SQUIDS, an acronym for Superconducting Quantum Interference Devices, are particularly sensitive magnetometers. They have several practical applications, most notably in biological systems, because of their extremely high sensitivity. Of their several applications, one possibility is as q-bits in quantum computers. While this is recent, its history goes back a little. Brian Josephson, as a doctorate student in 1962 at Trinity College, Cambridge University, at just 22 years old, began to be interested in the newly proposed concept of Broken Symmetry. Seeking ways to observe it experimentally at the Cavendish Laboratory, Josephson realized that he could set two superconducting devices, separated by a thin insulating layer, and focus on understanding the effect that the phase had on the supercurrents. Until then, the phase of the associated wave was not regarded as having a physical meaning, being just a mathematical artifact. Josephson was able to show that in such a setup, the currents would emerge even if you had zero voltage applied and that it also would be very sensitive to the magnetic field (in the case of zero voltages, the remaining term of such effect is $j_z = j_i sin\ \varphi$ [16]). The explanation is that a current emerges from the interference terms related to the currents on both sides, and the tunneling that could occur would be a function of the phase difference among the wave functions associated with each current. However, since all electrons on each side (actually all the Cooper pairs) would behave collectively, you end up with wave functions with a single degree of freedom describing a very large number of particles and, in this sense, describing a macroscopic entity. Therefore, since this entity, namely the current, can tunnel through the barrier, you end up with a macroscopic quantum tunneling.[17]

This is the kind of problem that could have deeper implications. While the idea of a kind of macroscopic quantum phenomena had been around for some time as a way to describe and explain both superconductivity and superfluidity, this might be different. Terry Clark, an experimental physicist

[16] This is known as one of the Josephson equations (1st Josephson relation), where j is the current, j_i is a constant known as the critical current and φ is the phase difference of the wave function for each side of the junction.

[17] For a broad discussion on this topic, see (Takagi, 2002).

from Sussex, understood just that in 1976, and thought that SQUIDS "given suitable experimental conditions (…) should display manifestly quantum mechanical behavior over macroscopic length scales" (Clark, 1991). This was not the first time that someone was contemplating quantum mechanical behavior over macroscopic lengths. Erwin Schrödinger had also contemplated them in 1935, not with some kind of electronic device, but with a living-dead cat. Clark was establishing the grounds for the "cat" to go into the lab. As it was already clear for him, such an extremely speculative research program might have been just madness, so he sought advice from an expert on foundational issues. He looked for David Bohm, who was at Bristol University and Bohm gave him enough encouragement to pursue it further, which he did.

A couple of years later, in 1978, cats became even more afraid of physicists, as this program received support, this time on the theoretical part. Clarke presented a course about semiconductors and, at some point, began discussing all the potentialities he had been envisioning around Squids for foundational purposes. One of the problems he presented had special appeal for our Ph.D student, Amir Caldeira. It was to understand thermal fluctuations in a Squid, and how it could affect the tunneling effect at the Josephson junction. Clark also had talked to Leggett, who approached Caldeira suggesting it to him. This time, both would be happy about it. First, it only had one degree of freedom, as it was in principle much simpler than those Helium 3 problems. Second, because Caldeira immediately saw how he could tackle this problem. Just as he did in his master's thesis, he could use dissipation in order to understand how Squids would behave in a thermal bath. This new problem would be a breakthrough for Leggett's research program. Together with Caldeira, they could set a new challenge to Quantum Mechanics, to see whether the macroscopic quantum behavior, with indeed macroscopic dimension, could be correctly described by Quantum Theory, and hopefully show where it would break.

By 1980 Caldeira had finished his doctorate, and presented his dissertation on "Macroscopic Quantum Tunneling and Related Topics (Caldeira, 1980)". While completely aware of the foundational implications of this work, he himself was not so keen on it. In the dissertation, there are no foundational considerations whatsoever, just the application of formal techniques to semi-ideal problems. Leggett, on the other hand, took a decisive step in his career, publishing his first two articles directly related to the foundations of quantum mechanics. In the first, published in 1978 ,he timidly gave directions regarding research on low temperature physics, but also indicated that SQUIDS might pose some deep questions on the measurement problem. In the second one, published in 1980, however, he was more explicit about his intentions: he wanted to test the hypothesis of whether or not linear quantum mechanics could be applied to macroscopic systems[18]. A few months later, Caldeira's dissertation would be ready.

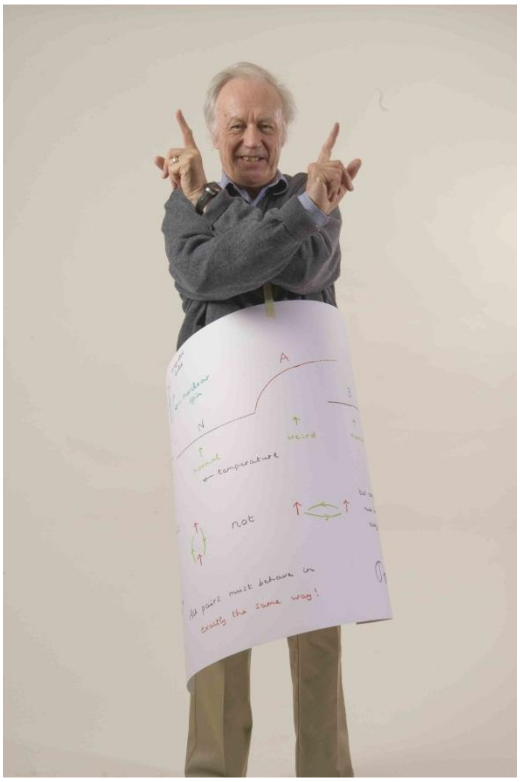

Figure 2 – Anthony Leggett in the exhibition "Accelerating Nobels", under the project "Nobel Drawings: Conceptual photography project with Nobel laureates", by Volker Steger, for the inauguration of the LHC. Credits: Volker Stegert. Courtesy of CERN/Volker Steger.

The Birth of Decoherence

As we have mentioned, Caldeira's dissertation did not have any foundational discussion as a major topic. Yet, within it, Leggett's program was contained. The dissertation revolved around two major questions. The

[18] See (Leggett, 1978) and (Leggett, 1980).

first one, “Is there any physical system which may exhibit quantum tunneling on a macroscopic scale?” (Caldeira, 1980) is in a broad sense Leggett’s program, just changing quantum tunneling for quantum behavior, and he always believed that SQUIDS with quantum tunneling was the best candidate so far to study this type of quantum behavior. From then on, Leggett would be fully dedicated to it. The second problem, “Once a macroscopic closed system shows quantum tunneling, would the coupling to a reservoir exert any sort of influence on the tunneling rate?” (Caldeira, 1980) was the actual birth of decoherence for physics. While today even posing this question might seem weird, as we are so used to understanding that quantum-like properties tend to disappear because of such interactions, then this was really an open problem[19]. Caldeira himself, after emphasizing that this was, in fact, the main problem of their work, answered “At the end we concluded that damping always tends to suppress quantum tunneling”, adding that “Although our last result was proved only for a specific model interaction with the reservoir we believe it can be generalized (...) however, this is a subject for more careful investigation” (Caldeira, 1980). Indeed, it would be possible to generalize it.

[19] In some sense, even today it still is an open problem (but in a different sense). Leggett has argued that despite the extremely fast coupling of the quantum system with the environment, this coupling can be adiabatic in an extremely large number of cases, so not only in principle but also in specific experimental contexts it should be possible to deal with this level of interaction without losing the quantum-like properties of macroscopic systems. Yet, despite his position, it remains true that most believe that all quantum-like properties disappear because of such interactions and this is the base of what is called decoherence.

One year later, together they published their first letter, but the first mention of their results appeared in 1980. Roger Koch, van Harlingen and John Clarke, through preprints, mentioned that Macroscopic Quantum tunneling should decrease with higher damping. Yet, about the same time, Allan Widom, with Terry Clark as co-author, in some letters argued in the opposite direction. In their second letter, in 1982, they claimed that "In the quantum tunneling regime, dissipation increases the barrier transmission probability", and they "attribute the difference between the results here reported and those of Caldeira and Leggett to a divergent renormalization". After an exchange of comments in the Physical Review Letters, Widom and Clark concluded that they "look forward to reading the forthcoming article by Caldeira and Leggett and remain open minded to the possibility of our statements might be in error". They went on to add: "However, we do not see such an error at the present time". The 1983 article would solve this[20].

Right at the beginning, Caldeira and Leggett said: "[we] attempt to motivate, define, and resolve the question 'what is the effect of dissipation on quantum tunneling'"[21]. However, while this was the general topic, the main interest was slightly wider, as we shall see in the following section. Right now it is important to emphasize how important their work was being perceived.

[20] See (Koch, van Harlingen, & Clarke, 1980), (Clark & Widom, 1981), (Widom & Clark, Probabilities for quantum tunneling through a barrier with linear passive dissipation, 1982), (Caldeira & Leggett, 1981), (Caldeira & Leggett, 1982) and (Widom & Clark, 1982).

[21] See (Caldeira & Leggett, 1983).

Leggett's new research program was able to convince the physics community of the importance that the environment had upon quantum-like properties, and, in the same vein, that there were enough problems around it for students and researchers to dedicate their time to it.

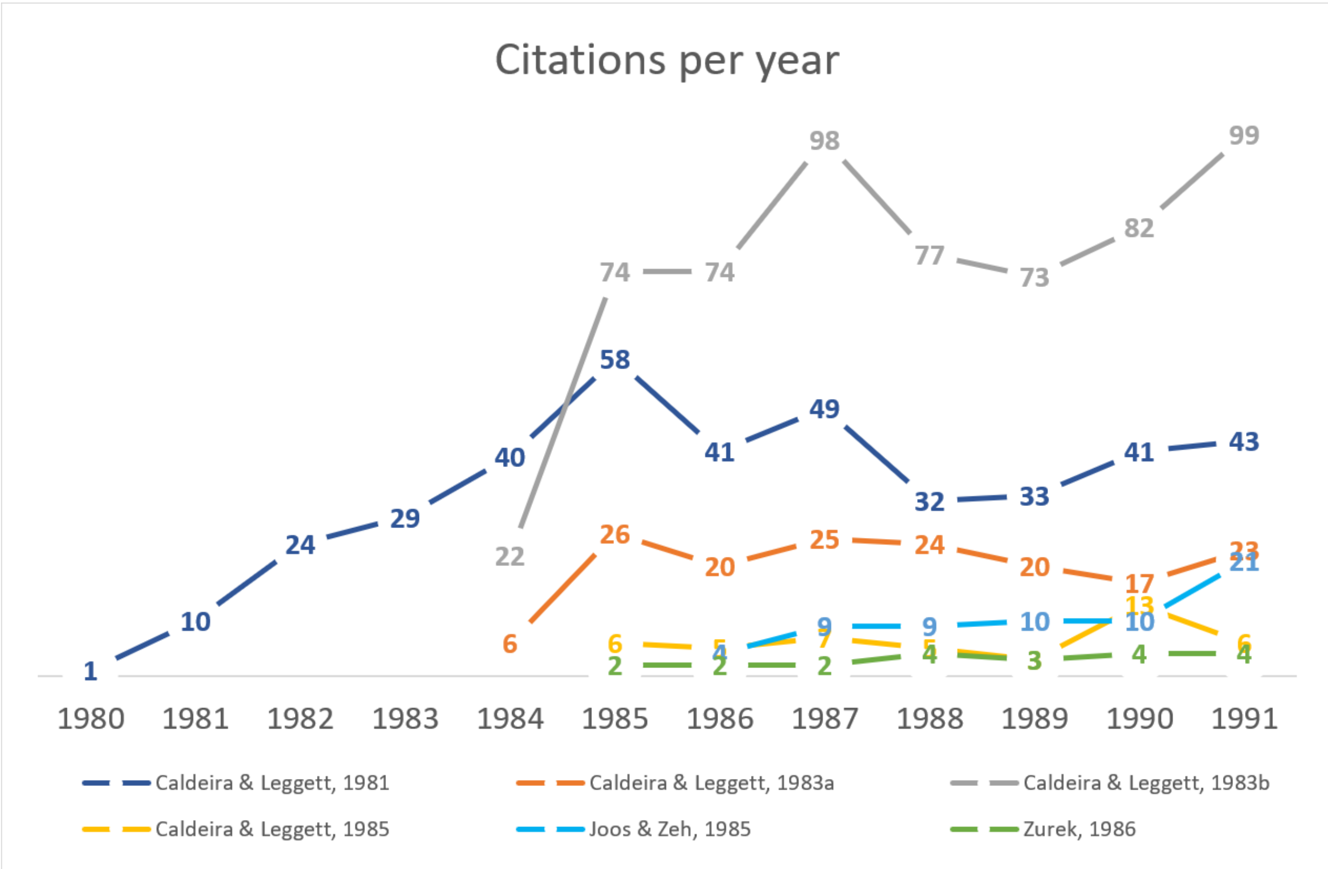

Figure 3 – Citations per year from 1980 to 1991, representing the immediate reception to the works. Source: ISI/Web of Science.

Still in 1983, the third article arising from Caldeira's dissertation would appear in print[22]. In it, the Feynman-Vernon path integral approach was applied to study brownian particle motion, but as in the general program,

[22] See (Caldeira & Leggett, 1983).

under the influence of the environment. Finally, their last article together would be published in 1985[23]. In this one, the theme of decoherence would become even more explicit and closer to what would be done later. This was calculating how long it would take for a quantum system to lose its quantum-like properties. They argued that the environment would serve as a sort of quantum apparatus, claims that had been presented before both by Dieter Zeh and Wojciech Zurek, but without a concrete example of how this would work and without a more complete development of the physics around it[24]. In fact, both Zeh, with his Ph.D. student Erich Joos, and Zurek would develop more technical works during the 80's, but mostly they focused on simpler systems than the ones being modeled by Caldeira and Leggett and quite far away from any experimental tests. Joos and Zeh's main technical work during this period was (Joos and Zeh, 1985) and Zurek's was (Zurek, 1986). Joos and Zeh's dealt with a simpler system than those used by Caldeira and Leggett, and Zurek's was largely based on the previous work of Caldeira and Leggett. It is possible to infer their impacts from 1980 to 1991 with their citations dynamics presented in Figure 3, with Caldeira and Leggett works receiving way more attention. With this, in no sense are we saying that these works and their other works were not important both for the development of

[23] See (Caldeira & Leggett, 1985).

[24] Camilleri presents an overview of both Zeh and Zurek's arguments regarding how they view the philosophical implications of what would later be known as decoherence (Camilleri, 2009).

physics and, in particular, to the development of decoherence studies. What we mean is *that for the development of what would later be called decoherence, even more during the decade of 1980, Caldeira and Leggett's work set the physical basis from which other works would follow from, but not only those works that would later be connected to foundations of quantum mechanics, also those connected with more practical applications of QM* and those that were seeking experimental evidence.

As it is possible to infer from the graph above, these works were extremely well received. Apart from the 1985 article, which had smaller generality, their first three articles received over 1000 citations each. Their 1983 article from the Annals of Physics was soon receiving over 50 citations each year, an article that talks in its introduction about Schrödinger's cat and the inapplicability of QM on the macroscopic domain. As we may see, this did not matter for the physics community, because apart from that, there was enough physics to be done around it, not just on foundational and philosophical physics. As such, the physics of decoherence indeed began here. From now on, the transition from quantum to classical was a true research program with defined physical problems, physical methods and also a philosophical background dispute regarding the future of QM.

Proving quantum mechanics wrong

How come several physicists working on foundations of quantum mechanics, with ideas far more orthodox than Leggett's, had so many problems in their careers, while Leggett had none? Furthermore, his foundational work was extremely well recognized! For instance, he had a key role regarding the recognition of the foundation of physics as an autonomous field. As he tells, "In 1984, motivated by what seemed to us a particularly foolish paper on Bell's theorem that had appeared in PRL, Anupam and I had written an indignant letter to the then editor of PRL admonishing him to apply the same standards to manuscripts in the area of quantum foundations as those used in other areas of physics. The result (which in retrospect I should have anticipated!) was that I was asked, and agreed, to become the first divisional associate editor (DAE) of PRL for the newly created division of quantum foundations, a post which I held until 1996." Not only was he publicly seen as a researcher on foundations of physics (but not only as such), he also had the role of contributing to the establishment of the field as a part of mainstream physics, at least for the prestigious journal Physical Review Letters (PRL).[25]

The idea that Leggett faced no problems is even more interesting when we understand the situation a little better. The context in which he was involved

[25] (Leggett, 2020)

was post-Bell's inequality tests. After a first period of tests during the 1970's, with almost undeniable confirmation of QM in the early 80's, more specifically in 1982, Alan Aspect had in some sense solved Bell's conundrum. Quantum Mechanics had been verified to an extent that very few loopholes remained, none of them serious enough to put in danger the meaning of the experimental results. Yet, even during the earlier decade, it was quite unfashionable to think of Quantum Mechanics as either wrong or incomplete. The hot topic was to understand the true meaning of nonlocality, and, as Everett's interpretation was becoming important, to understand quantum formalism with universal validity[26].

Yet, for Leggett, none of this was particularly important. He would always recognize that QM had a very strong domain of validity, as he was still applying it to solve problems. However, much in the same way as Albert Einstein in the EPR, he was applying it to find problems in its applications. As we have seen, he had done it before, failing to prove it wrong about the superfluidity of Helium 3 (and being awarded the Nobel for this). Now he chose the more unstudied field of the applicability of QM to describe macroscopic superpositions. The outline of this challenge appeared in his very first article about foundations, in 1980. In it, he asked: "What experimental evidence do we have that quantum mechanics is valid at the

[26] For this context, see Freire (2006) and Osnaghi, Freitas and Freire (2009).

macroscopic level?"[27] Yet, this question, as posed, did not do full justice to what he was arguing. As a former researcher on superfluidity, he of course knew that QM was applicable and in fact worked at the macroscopic level, something that was already widely recognized at the time, even at textbook level. Feynman's chapter on superconductivity in 1965, for instance, was called "The Schrödinger Equation in a Classic Context: A Seminar on Superconductivity"[28]. However, then, the question is a little more subtle. That atoms collectively behaved according to QM, even if sometimes this would happen on a (relatively) large scale, was very well known, but did true macroscopic systems behave according to QM in a linear superposition?

We have to first define what a *true* macroscopic quantum behavior would be. Leggett, then, defined Disconnectivity. In simple terms, disconnectivity describes the quantity of particles effectively interacting collectively to produce the macroscopic quantum effect[29]. For instance, in the He3 superfluidity phenomenon, despite there being many particles involved, the true interaction would be in a cooper pair, which leads to $D = 2$. Whereas in a cat, despite the fact that we are unable to write a density matrix for all the particles in it, all of them (or at least many them) indeed interact together to

[27] (Leggett, 1980)

[28] (Feynman, Leighton, & Sands, 1965)

[29] In his book, Takagi, who collaborated with Leggett, presented a definition as "the maximum number of those democratically-counted degrees of freedom which are involved in an irreducible linear combination". See (Leggett, 1980) for this and the rest of the paragraph and (Takagi, 2002).

form a living (or dead) cat, leading to a higher *D*. So a cat would be a great candidate to perform experiments to test the validity of QM on *true* macroscopic quantum systems, as Schrödinger had already realized. Yet, apart from several problems, it can be quite hard to get a cat inside a box, as they only do what they want to, so the quest for simpler systems, but also with higher *D* would be central for Leggett from now on[30].

Macroscopic quantum tunneling fitted the bill and Leggett focused on it in the following years. What made it so special, more specifically coherence in Squids, is that the time the system takes to be damped and to lose its coherence would be much greater than other candidates, even more because of such low temperatures required for the superconducting device. This would, in principle, allow one to observe quantum coherence at the macroscopic level, or at least infer it from the tunneling effect. If we assume the universal validity of quantum mechanics, this would present no problem at all. The point that Leggett makes is that this is not a trivial assumption. To put it more precisely, there was no evidence that linear quantum mechanics would be applicable to macroscopic systems, and every test so far would not be able to differentiate a pure system from an ensemble. In his words,

[30] In sum, to test QM macroscopically, one needs a system that is a superposition of states describing n particles instead of n superpositions of states describing single particles.

"Clearly the argument as to whether the pure state (2.7)[31] is or is not distinguishable from a mixture is only of relevance if one believes that (2.7) is the correct description in the first place. But this description only follows under the assumption that the linear laws of quantum mechanics can be applied strictly to any physical system, however macroscopic and complex. This assumption is not a trivially obvious one; it would, for example, not necessarily be a priori absurd to postulate that, at a certain level of complexity, nonlinear terms begin to play a role and cause superpositions of the form (2.7) to evolve continuously into one of their branches'' (Leggett, 1980).

While not directly advocating a non-linear approach, Leggett was not at all hiding the fact that we have no secure bases to assume the universal validity of QM and, even more strongly, that it would probably break down once we could perform experiments about macroscopic tunneling on squids. While it was possible to use several examples during the 80's, two of them are a little more striking. The first is from their 1983 article in the *Annals of Physics*. It is rather long, but very revealing: "Finally, it should of course be emphasized that all the calculations of this paper have been carried out within the conventional framework of quantum mechanics, that is, under the assumption that this framework can indeed be extrapolated to the macroscopic scale in

[31] The state 2.7 is $|\text{apparatus}\rangle=\Sigma_i c_i|X_i\rangle$, where X_i is a macroscopically distinct state of the apparatus.

the sense discussed in the Introduction. Should it eventually turn out that for a particular type of physical system quantum tunneling is not observed under the conditions the theory predicts it should be, no doubt the most obvious inference would be the calculations, or the model on which they are based, are wrong; however, an alternative inference, which it would unwise to exclude totally a priori, would be that quantum mechanics cannot in fact be extrapolated in this way."[32] So, by no means was this a secret. It was actually quite clear, as remarked at the end of the article. But, if his motivation was to prove QM wrong, also known as the most successful physical theory that we have ever had, how come he became so important?

This leads us to the second example. In 1983, Leggett was invited to give a course at the Nato Advanced Study Institute. That year, the theme was Percolation, Localization and Superconductivity, and, as the editors Allen Goldman and Stuart Wolf, stated, "the study of MQT [Macroscopic Quantum Tunneling] is the newest subject in this grouping and is of fundamental significance for the quantum theory of measurement", but, at the same time, "the macroscopic quantum tunneling which is closely associated with the concept of quantum noise may determine the ultimate sensitivity of Josephson devices to electromagnetic signals". So while this certainly could be important for foundational studies, it was clear that there could be

[32] (Caldeira & Leggett, 1983).

practical and even technological applications and "a complete understanding of the role of damping in these systems appears to require more experimental and theoretical work", making it clear that this was something rather open for both researchers and students[33]. The first series of lectures in print are from Leggett. In the first line of the introduction, he says that "In discussions of the quantum theory of measurement, a crucial question is whether the usual laws of quantum mechanics can be applied to macroscopic bodies, and in particular, whether it is legitimate to assume the occurrence in nature of linear superpositions of states with macroscopically different properties", adding "that this is not a matter of 'quantum theology' but can be tested, at least indirectly, by experiment". Again, he argues that his "general approach will be to assume that the linear laws of quantum mechanics do apply without modification to macroscopic bodies and to explore the consequence of this assumption. Naturally, if the experiments were to fail to show the predicted results, the assumption might have to be re-examined" (Leggett, 1984).

Yet, while being the core of his program, other physicists did not think along the same lines. To exemplify, we may use the following lecture by Vinay Ambegoakar, "Quantum Dynamics of Superconductors and Tunneling between Superconductors". Initially he wanted to make clear that both he and Leggett were doing, in some sense, the same thing: "Since A.J. Leggett's

[33] See (Goldman & Wolf, 1984) for the quotations above.

lectures at this institute take a very much phenomenological point of view, mine should complete his rather well". Yet, "one matter I leave entirely to Leggett. That is the general question of whether ordinary quantum mechanics describes transitions between macroscopically distinct quantum states in superconducting devices. (...) I would be <u>most</u> surprised if it does not, and it would never occur to me to doubt that it does." He continues pledging loyalty: "What follows is a technical but straightforward application of the quantum mechanical machinery which – basically mysterious though it may be – we have all learned to operate with instructions from Copenhagen. As for Schrödinger's cat, my way out of that conundrum is to remark that, as a reluctant co-owner of one, I know that cats are more devious – for which read complex – than superconductors" (Ambegoakar, 1984). By no means does this indicate any kind of misunderstanding, neither personal nor cognitive. Both thanked each other for their respective lectures and both understood clearly what the other was doing. The fact portrayed here is that the physics community chose to separate Leggett's physics from his "theology". What he was doing was so important that instead of passing it strictly to the foundational domain, at the margins, they embraced it, just *pretending* that his deep philosophical insights did not exist.

Briefly, we may use one more example. In its January edition of 1984, Physics Today presented a report "Physics News in 1983". It had been organized by the American Institute of Physics for the last 15 years, but for the first time it was presented in Physics Today. It was divided by fields, and each field had its contents selected by members of the American Physical Society, i.e., its subdivisions as astrophysics division, fluid dynamics, education, electron and atomic physics etc. In the Condensed matter part, Leggett's work had been chosen as noteworthy, so he wrote a short piece describing his program, presenting similar ideas to those described above and explaining his challenge of QM. However, it is more interesting to see what the editor of this session, Miles Klein, said, dedicating one of four paragraphs to it: "Tunneling is an important manifestation of quantum mechanical behavior and is found in nuclei, molecules, crystals, and many-electron systems such as superconducting junctions and, perhaps, in nonlinear one-dimensional conductors. Tunneling on a macroscopic scale presents, on the one hand conceptual problems associated with the foundations of quantum mechanics and on the other hand useful behavior that may be incorporated into devices such as superconducting transistors. At a finer level, tunneling now allows the production of images of surfaces on an atomic scale". It is therefore clear that the editor was aware of foundational implications that this research might have and, while mentioning them, felt it necessary to emphasize its practical applications and technological improvements[34].

[34] See (Klein, 1984) and (Leggett, 1984).

But if we can account for how a researcher so attached to foundations of QM could become so important and recognized, the issue of why he was ignored by the “mainstream” milieu of foundational researchers remains. It was not that he wanted to keep his distance from them. He published in the *Foundations of Physics*, the “official” journal of this community, and he was, as we have mentioned, the first divisional associate editor for the area of foundations of physics at the *Physical Review Letters*. He joined virtually every single conference on the theme, always presenting his ideas, he wrote an article for a David Bohm’s Festschrift, and he even discussed it in an acclaimed science popularization book published by Oxford University Press, “The Problems of Physics”, dedicating a full chapter to it, “Skeletons in the Cupboard”[35].

While harder to evaluate, there are a few indications of why it was like this. In his “The Problems of Physics”[36], a book dedicated to discussing in layman terms the present situation of physics and the prospects for future research, Leggett argued that “some of the views to be explored in this chapter, particularly towards the end, would probably be characterized by the more

[35] See (Leggett, 1987a) for the book and (Leggett, 1987b) for the article in Bohm’s volume.

[36] The book is part of a series called The Problems of Science. “This group of OPUS books describes the current state of key scientific subjects, with special emphasis on the questions now at the forefront of research”. Aside from Physics, the other volumes were on Biology, Chemistry, Evolution and Mathematics, and the whole series has been reissued under Oxford Classic texts. So, despite the name, this was not a provocative piece, instead just a portrait of current Physics as seen by one of the main theorists of the field.

charitable of my colleagues as heterodox, and by the less charitable quite possibly as crackpot"[37]. So, if his fellow mainstream physicists had no problem with his ideas (maybe they were the more charitable), we can only imagine why his fellow foundational researchers had problems with his thoughts. First, after the most definitive tests of Bell inequalities, "nobody" was going against QM. The more general feeling, even during the 1970's, was that QM was strongly confirmed and those out of synchrony were being left behind. However, it is not completely true that QM was so confirmed. Quite a few names were trying to find loopholes in the experimental tests, but Leggett did not seek support among them: "such loopholes can indeed be found, but however many have been closed (…) a sufficiently ingenious objector will almost certainly find yet more" and "All one can say is that most of these objections seem to most physicists so contrived and *ad hoc* that in any other context they would be dismissed out of hand"[38]. Furthermore, another group becoming important were supporters of the Everett interpretation, also known as many-worlds. Besides the fact that they assumed the universal validity of the linearity of Quantum Mechanics equations, Leggett thought of it as an "exotic solution". To make it clear, "it seems to me that the many-worlds is nothing more than a verbal placebo, which gives the

[37] For this citation and the following ones in this paragraph, see (Leggett, 1987).

[38] He kept on saying that "Whether one believes that the a priori arguments in favour of local objectivity are so compelling that it is legitimate to grasp even at such straws to save it must of course remain a matter of taste." (Leggett, 1987)

superficial impression of solving the problem at the cost of totally devaluing the concepts central to it, in particular, the concept of 'reality'". And, finally, "I believe that our descendants two hundred years from now will have difficulty understanding how a distinguished group of scientists of the late twentieth century, albeit still a minority, could ever for a moment embraced a solution which is such manifest philosophical nonsense". Given the fact that a quite large part of the debate about foundational issues happened at informal forums, such as popularization books, as still happens nowadays, these words were of very special importance, and even more so when we realize that such non-technical texts are a very important connection among scientists and philosophers.

Finally, it is also worth mentioning the emergence of information. While Leggett never paid any attention to this, Information and Decoherence became intimately connected during the 90's and later, as one can see from the name of the field itself: Quantum information. As Leggett himself claimed recently, "Of course, in retrospect what I was seeing was the first stirrings of the quantum-information revolution that was to sweep through physics at the end of the twentieth century—certainly this was one of the most profound developments in my time, though one in which I did not really participate directly.[39]" Furthermore, the way the field developed, around practical

[39] Leggett, 2020.

applications of old quantum foundational challenges, strongly supported the universal validity of quantum theory. Not only were earlier experiments that supported quantum mechanics being performed in several new ways, such as the Bell inequalities experiments using distances from hundreds of meters to a few kilometers and even further to more than a thousand kilometers in space, but also new experiments emerged, like the same Bell inequalities but with more than two entangled particles, larger and larger entangled systems, measurements on individual systems and so on, every single one of them further confirming quantum mechanics, each time with a greater degree of precision. Proving quantum mechanics wrong became the pursuit of few and from the 90's on it was driven off the agenda altogether.

As the quantum information field practically swallowed the foundations of quantum mechanics, almost no one was still betting that quantum mechanics could be wrong[40]. And if this was not enough, several names that had also been involved in the decoherence approach became quite important in the information field, such as Zurek, Zeh and David Deutsch. This led people to forget about the importance Leggett had in the development of decoherence and also defined how the earlier proponents of this approach were to become the recognized fathers of decoherence.

[40] After all the experimental results, this would also affect Leggett's thought. In 2013, he claimed that "When I first started thinking seriously about this, way back around 1980, I quite seriously hoped that when you got to the level of the so-called 'flux qubit' (...) by that time something else might have happened", something else than the confirmation of Quantum Mechanics on yet another level. He, then, adds, "Right now, it looks as if quantum mechanics is working fine at that level." (Burton, 2020) The general feeling was affected by the new Zeitgeist, and so was Leggett. Still, his hopes would not change, just the feeling of when it would happen. In 1999, he concluded an article in Physics Today asking: "Whither quantum mechanics in the next millennium? We do not know, of course, but here are two reasonable guesses for the short term. First, irrespective of whether or not "quantum computation" becomes a reality, the exploitation of the weird properties of entangled states is only in its infancy. Second, experimental work related to the measurement paradox will become progressively more sophisticated and eventually advance into the areas of the brain and of consciousness. This, of course, assumes that physicists will maintain their current faith in quantum mechanics as a complete description of physical reality. This is something on which I would personally bet only at even odds for the year 2100, and bet heavily against as regards the year 3000!" (Leggett, 1999). Again, in 2014, he claimed that "In 50 years, I think there will have been a major revolution in cosmology and I think there's a small but non-zero chance that we will have pushed quantum mechanics in the direction of macroscopic world to the point it will fail and break down." When pushed a little further to whether or not, ultimately, he believed it would definitely break down, he still answered positively (Burton, 2020). So, as we argued, if even Leggett could not see QM breaking in the short term, others would naturally doubt this possibility to an even further level.

Philosophers themselves also seemed to have problems with Leggett's quite unique style on foundations of quantum mechanics. From the very beginning of the debates, most discussions and examples centered on extremely simple systems and many idealized situations. And, ever since, when we look at foundational debates, most cases are extremely simple. We may take several examples. David Bohm's proposal, certainly one of the most technical of all of them, was actually closer to classical mechanics, and its applications were somewhat basic. Everett's formulation was just plain linear quantum mechanics, without any more complicated applications[41]. Bell's inequalities, albeit slightly more complicated when it got to more specific models that in fact became experiments, in its general idea were quite simple, not much more complicated than EPR. Schrödinger's cat and almost every other example could ultimately fit in the general scheme of $|\Psi\rangle = c(|\uparrow\rangle + |\downarrow\rangle)$, i.e. very simple two-level systems[42]. At the same time, Leggett's foundational physics involved understanding extremely technical aspects of low temperature physics and detailed applications of QM in not so simplified systems, as Leggett was eager to get these models into the laboratory. What we are trying to argue here is not that all philosophers did not have enough training and knowledge in physics to understand his work, as some certainly

[41] Or, in some sense, any applications at all.

[42] This is in fact so strong that a book dedicated to teaching foundational matters was written almost entirely about two-level systems, reducing many problems on Hilbert space to regular two dimensional vector space (Hughes, 1992).

did, but that this, among the other reasons presented, would be one more reason not to pay enough attention to him. It is possible to illustrate the philosopher's attitude with one strong example. In 1987, Leslie Ballentine published one of the most comprehensive resource letters about foundations of QM after Bell's inequalities, i.e. for the previous 20 years. In it, he covered virtually everything that was being debated at the time, except for Leggett's program. There was just a single mention of him about a very small letter that Leggett (with A. Garg[43]) sent to a debate over an article claiming that Bell's experiments did not test local hidden variable theories, with no mention at all of his own program. Ballentine knew therefore, at least at some level, that Leggett was paying attention to the foundational field. He, like several others, was just not paying any attention at all to him. (Ballentine, 1987).

[43] Anupam Garg collaborated with Leggett in the derivation of the so-called Leggett-Garg inequalities. These inequalities bear some similarity with Bell's inequalities, but instead of photons, they were derived with squids in order to test whether two conditions can still be maintained alongside quantum mechanics: 1- Macroscopic realism and 2-noninvasive measurability at the macroscopic level. Since their development, in 1985, there have been several experiments ruling out both conditions. Yet, it still remains an important source of foundational experiments, just like Bell's inequalities. See (Leggett and Garg, 1985) and (Formaggio et al, 2016) for one extreme case of violation of the inequalities.

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It’s a matter of style

It's a matter of style

Previously, we have dedicated a large part of our argument to how Leggett's own style shaped the reception his research had from physicists and why his career did not suffer any drawbacks. After all, he had won the nobel prize and had very prominent positions throughout his career. The easy explanation will credit all his recognition to the several extremely important results he achieved with his research, in different fields of physics. But, as we have argued, we believe that those research results are *a consequence of his style of research* in pursuit of his quite unique research program on the foundation of physics. Yet, we did not explain what we mean by style. Nor we compared with other researchers on foundations of physics to make it more clear what we meant. So, while not a logical necessity nor a practical one, as we all have a pragmatic linguistic comprehension of this word, having a clearer meaning of what we meant by it is not only useful for the sake of our general argument. It is also worth because we deeply believe that this category of analysis is a central concept[1] that is usually overlooked, or, at least, underapplied in the field of science studies.

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[1] "Even the practice of the natural sciences (...) shows stylistic aspects. Branches of science traverse periods in which theorising is dominated by a particular style. The task for historians and philosophers of science is to proceed beyond recognising styles of theorising in the historical record, and provide some account of how these styles become entrenched in scientific practice." See (Eck, C.; McAllister, J. and Vall, R., 1995, p. 14).

The main reason for this, we conjecture, has to do with one extremely hard problem: What does it mean when we say style? As we shall see, almost everyone uses it in its own sense, whether they talk about it in Science or in Art[2]. And, again, if we return to a pragmatic use, we know how to apply it in quite different meanings. One can talk about an American style of practicing physics in opposition to an European one in the context prior to the world wars. In this sense, style has to do with a regional dimension. Going a little further, we can speak of the style of French physics in opposition to an England one after Isaac Newton. If we want to avoid such general contexts, we can think about the different styles Niels Bohr students had when compared to Arnold Sommerfeld ones. Or, we can get even more personal comparing Albert Einstein, Niels Bohr and Erwin Schrödinger different styles tackling the earlier foundational problems of quantum theory. We can think of the different styles experimental physicists have when thinking about a problem compared to the therothetical, or the computational one. We can

[2] Despite, clearly, this not being a study inside the field of art, it is just not possible to think of any idea of style with no reference to the studies in aesthetics. As Svetlana Alpers, an art historian, has argued, "To ask an art historian to speak on the subject of style is to expect something from the horse's mouth. Even when the topic is not set, colleagues in the other humanistic disciplines assembled (...) for a qualifying examination will turn to the art historian as the acknowledged bearer of, definer of, style. 'How could you describe the style of the baroque lyric in France', or 'Could you comment on the development of German baroque drama?' The questions are put to the student, but the professor of French or German looks across at the art historian for confirmation. We know the answers for it is we who set, who validated, the questions". (Alpers, 1987, p.137)

also compare the different styles of pure physicists, applied physicists, mathematical physicists and biophysicists etc. dealing with a similar problem. While it is clearly possible to disagree with several of the categories used in the examples above, we all understand what they mean and the comparison among them. Despite all of them being real situations, I deliberately choose to not offer any references to them because the main point is not to agree with how the concept of style was used by specific studies. The point is that such use of the word has meaning, albeit a very different one in each case[3]. It would be quite easy to expand this list with examples from the history of physics and even easier with art.

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[3] The idea that there is both meaning and sense to use style outside art has been also recognized in the art studies, not only as something that may happen, but as an important development of the studies regarding the concept of style. For Lang, "Arguably the most important recent developments in the study of style have come from just those attempts to establish a role for style that refer it well beyond the artworld or even the conventional domain of rhetoric, the two sources that have historically provided the objects of stylistic analysis. The concept of style had, of course, been applied metonymically before — 'style' itself originates as a metonymy — but its extension to culture and human behavior generally, as by such writers as Erving Goffman, Mary Douglas, and Clifford Geertz, has markedly enlarged the range of these applications. Thus, too, in a relatively short time, we have come to take for granted the use of stylistics analysis to break into certain forms of discourse which had previously seemed inaccessible even for the rhetorical or aesthetics traditions that took the role of style most seriously: thus the discussion of 'styles' of science in connection with the work of Thomas Kuhn and Ian Hacking, for example, as well as the applications of stylistic categories not only to historical, philosophical and sociological discourse (...), but also — style itself can hardly be indifferent to the distinction — to what forms of discourse were discourses *on*." (Lang, 1987, postface, p. 16)

This work is divided into three parts. In the first part, we will discuss a few examples of how the term style was used and look into a few tentative definitions of the term, presenting, in the end, the idea that we use specifically. In the second part, we will apply it by comparing the different styles researchers on the foundations of physics developed during their careers. Finally, we will discuss why we believe our approach is beneficial, as it presents style as a dimension that no other concept in science studies currently approaches, and what kind of information we could gain by using it in this sense.

What is style?

Earlier debates in art

Style, itself, is not a new word. It has several origins, mainly meaning something related to a tool used to write, draw or even sculpt. In fact, this dimension of the word is still in use today with the original form of the word in Latin, stylus. In the most contemporary use of it, stylus is the pen used in digital surfaces, such as a drawing tablet or a touch screen. More than a mere curiosity, this is important because it brings one part of the meaning of stylus that we do not so often think about. The idea is that style is a tool used to make something, not only a way to identify the result. So, in the general sense, style has to do both with the way something is constructed and how it is presented. The path that led to this general meaning was that the name of the instrument began to describe not only it, but the act of using it, and, later, the results.[4]

We will not try to do a broader presentation of how style came to be used in the contemporary language, searching every single language for its first uses. It is sufficient, for us, to know that, at least in the French language, aspects of the contemporary usage of style has its roots during the middle ages:

> *Le moyen âge voit apparaître les sens de « manière de s'exprimer » (oralement), de «manière d'être », « de vivre », « d'agir »,«d'employer », « de combattre », de « manière de procéder » (en justice), et même de « manière de penser », « opinion »*[5].

The idea of a science of style, stylistic, would appear a little later, during the XIX century[6]. So, the current use of an idea of style is almost a century and a half years old. This is in agreement with the report made by F.

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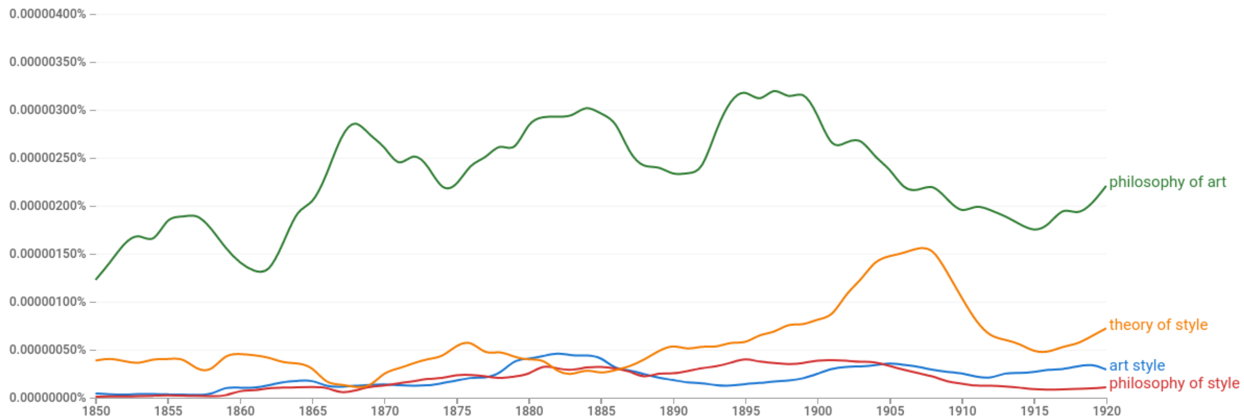
[4] "Le nom de l'instrument ayant servi à désigner l'activité elle-même, puis son résultat". See (Sempaux, 1961, p.739).

[5] *Ibidem*, p. 740.

[6] "*Qu'on l'appelât « Science » (4) ou « Théorie du style » (5), la discipline nouvelle se bornait à recueillir « une multitude d'observations sur le style, souvent fort importantes, [qui] ne rentrent pas strictement dans le cadre de la grammaire h (6), de manière à donner à l'élève des règles du bien écrire inspirées des meilleurs auteurs.*" *Ibidem*, p. 743.

Wellington Ruckstuhl, an american sculptor and art scholar. In 1916 he told that:

> Forty years ago I sat in Harding's picture store in St. Louis looking at two landscapes. Two men entered:
> "Say, Jim! I like that picture."
> "So do I. But I prefer that one."
> 'Why ?"
> "Well, it has a certain style."
> I said to myself: "Style? style?-what does he mean by style?" I had never heard the word used before, except in relation to women's hats.[7]



Relative frequency of appearance per year, from 1850 to 1920.
Source: Google Ngram

As we may see from the graph above, the idea of art style was not yet established as a main source of analysis into art. During this period, although

[7] (Ruckstuhl, 1916, p.172)

an idea of style was already present, the main way to deal with it was as a theory of style, with philosophy of style and art style close to each other. For instance, Ruckstuhl, in his previously mentioned work, was mainly focused on trying to define style, in opposition to what he considered something less important, the manner. He went searching for a definition and "being confronted with such words as Manner, Individual, Universal, Epochal Style, etc., I was kept a long time from finding the desired formula or definition that would cover the case and prove on analysis to be invulnerable." He then presented his definition as "Manner has nothing to do with style, because it is altogether a matter of surface technical execution, while style is a matter of fundamental composition or arrangement." In more details, style in art is "A MATTER OF FUNDAMENTAL COMPOSITION, OF THE ARRANGEMENT OF LINES, MASSES AND COLOR; OF WORDS, OF SOUNDS AND OF MOVEMENTS" . So, style comprehends a grand scheme of interpretation, of creation and execution, and manner is used for more specific details of the artwork. This indicates that while style is a general trait, manner is an individual one. Yet, in the same article, he would argue that while style was a universal trait, such as "Greek, Renaissance, Spanish, French, Dutch", it could also be both universal and individual.

> But a style may be a universal style and yet be individual. Example: the styles of Rembrandt, Holbein, Hals, Titian, Raphael, Giorgione were all universal styles, and yet they

> all are so different that one can easily see their individuality.[8]

While both his definition and thoughts were not particularly important to the long lasting study of style[9], they present a tension that is permanent, a main point to which we will focus, the *tension of whether style is an individual or a collective characteristic.*

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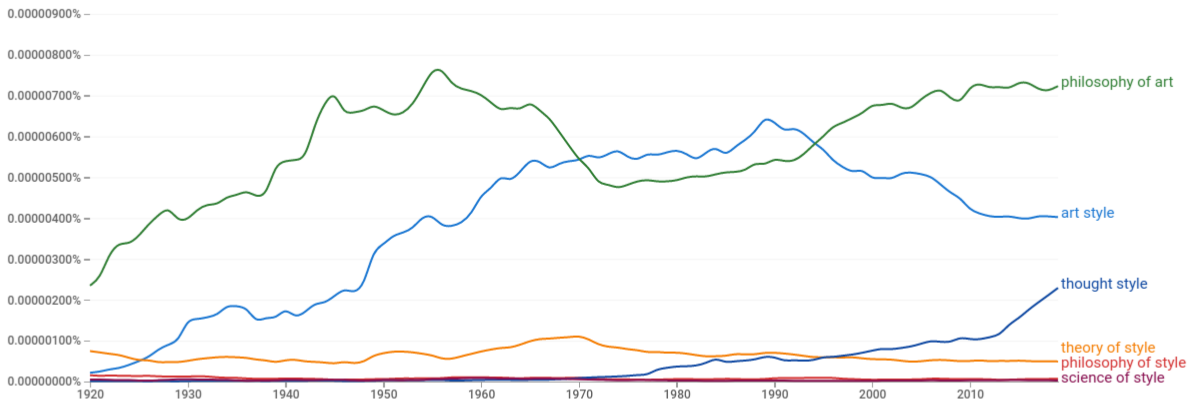
[8] This and all the quotes before are from (Ruckstuhl, 1916).

[9] While not particularly important for our analysis, it is worth mentioning that the topic was the source of a controversy about its meaning. In 1885, Geo Lambdin, in his article named *Style*, set himself to "find out what the word 'style' really means". And, in doing so, reached the conclusion that considered manner broader than style: "But we must keep in mind the distinction between the manner of a school and the work of an individual" so "In this sense style is the part of the man by which he is known to the world, as is his physiognomy; it is not his thought, which is his own and incommunicable without a language, nor is it his manner, which he may share with many of his school; but it is the mode of the expression of his thought". In 1915, Edward Morris, made a presidential address and published it, titled "Science of Style". His use of style was a rather narrow one, with style being part of linguistics, and closer to its origins related to the metonymy of stylus. Still, it is interesting to see how, for him and several others, it was needed a science of written style to understand literature, poetry and other forms of written art and communication, showing how the word style had not yet acquired a more contemporary meaning. In one last example, from 1899, Leigh Hunt, already talking about art, goes in a different direction pointing that style is an individual trait. For him "Yet when we conjure up the magic word style , we bring hand in hand with its, its familiar, its other self, individuality. It may be a bad individuality, it may be a ludicrous individuality, or it may be a grand individuality, and then the style is such, too." See (Lambdin, 1885), (Morris, 1915) and (Hunt, 1899).

And a late debate

As it is possible to see in the graph, from 1920 on style began to be a central concept when dealing with art. For our needs, it is not necessary to follow such evolution and we can focus directly on some of its uses in the science studies. But before presenting those studies, we want to discuss a little more on how the concept is used in the art field.



Relative frequency of appearance per year, from 1920 to 2019.
Source: Google Ngram

In the Oxford Handbook of Aesthetics, Stephanie Ross begins her chapter, Style in art, with a defense of the deeper meaning that can be attributed to Style. She claims that we usually, everyday, think about style as a shallower part of our domain of experience. “In everyday contexts, we often contrast style with substance. Style pertains to surface appearance, or to a way of doing things.” She presents several examples where we believe the idea of style represents the surface of our experience “We notice the style of someone who dresses well, or unusually, or of someone who navigates trying social situations with ease and grace. Style can also be appropriated from other classes or cultures; a recent newspaper series, ‘How Race is Lived in America’, discussed white teenagers taking on the hip-hop style. In all these cases, style seems somewhat trivial, its singleminded pursuit morally questionable, since those cultivating style may be neglecting ‘deeper’, more important concerns.” Then, we reach the major point. Style, at least in art, is part of something deeper. “In the arts, style is of greater moment. Knowing the style of a work of art is a prerequisite to correct understanding and appreciation of it. Only after first placing a work in the correct style category can we answer interpretive questions about its tone, its representational and expressive content, its overall meaning.”[10]

There is a reason why her essay begins tackling this very specific question. Style, both historically and philosophically, has been a major category in art studies, or in aesthetics. It is one of the main tools used in order to understand several aspects in the studies of art, ranging all the way from what

[10] (Ross, 2009)

is art to how art is made and what is the feeling of experiencing art[11]. While none of those can be reduced to style, style is embedded, and, as one may see, those three problems are considered the major issues in aesthetics studies[12]. So, far from being a secondary issue, style is almost a necessary category.

There are other questions regarding style before we move on. For instance, is style something that may be attributed to natural phenomena? Can style be used to describe a beach, a wave crashing? Or, in the same sense, is it possible to talk about the style of particle decaying (although not a very common topic among artists)? This debate is, in its essence, a debate of whether style can happen as something without intention.[13] If intention is something essential to style, any use of style regarding inanimate objects or natural phenomena is a metaphorical one, and, as such, can be used in a fruitful manner, yet doesn't contribute to the understanding of what style is. And, in the same sense, we learn that style is not accidental. It is something that comes from an

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[11] "The aforementioned notions of styles seem to serve a wide range of art-critical, art-historical and art-theoretical functions. In particular, style seems to play *identificatory, interpretive, evaluative* and *explanatory* roles in our artistic practices." (Meskin, 2013, p. 442)

[12] "One may usefully think of the field of philosophical aesthetics as having three foci, through each of which it might be adequately conceived. One focus involves a certain kind of practice or activity or object—the practice of art, or the activities of making and appreciating art, or those manifold objects that are works of art. A second focus involves a certain kind of property, feature, or aspect of things—namely, one that is aesthetic, such as beauty or grace or dynamism. And a third focus involves a certain kind of attitude, perception, or experience—one that, once again, could be labelled aesthetic." (Levinson, 2009)

[13] (Meskin, 2013)

intentional will. That needs to be thought through, developed, perfected with experience and practice[14].

Is it possible to identify specific traits that are connected to style? The way the brush touches the canvas, the theme, the colors, the expression? There is a list of things we could choose beforehand to identify and look for the style of the work? Or is style something more elusive, that is not connected to anything specific and has to be examined in each particular case? While framed like this, is not easy to see, but this debate is related to whether style is something unique to an artist or a more general characteristic, because if it is something general, we would need to find specific characteristics of the works to assess the general style, as we don't have someone specific to look for reasons as to why those works were made as they were. But, if style goes for something more particular, in the end the style would reside on why and how someone completed the work of art and it would appear singularly for each artist, even if the same traits could be used to assess style on several different artists. The important thing is that while some traits would be the same, the set of traits would always be something unique[15].

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[14] Another direct consequence of style being willful is that the control someone has regarding its own style can be thought as a direct consequence of its mastery. Therefore, style could be used to assess quality, or lack of, even if we could debate the meaning of what quality means.

[15] See (Ross, 2009) section 4.

But, is it possible to say that style, despite being so currently employed, is necessary as a concept? In his edited volume "The concept of style", Berel Lang, on the postface for the second edition, talking about the debates that emerged with the publication of the book, concludes that "At some point, then, it becomes pertinent, even necessary, to ask whether, in the analysis of style, such resistances to a systematic foundation may not be rooted in the concept of style itself."[16] Alpers, in the same volume, for instance, believes it is better to use other words with somewhat more specific meanings than style, with its particular history and sense. For her, "One might prefer, as I have tried in my own writing and teaching, to avoid its terminology altogether". The problem arises because of the subjects related to style: "Yet the issue (can it really be called a concept?) of style touches on some essential phenomena and — call it *style* or, as I shall suggest, by another name — one surely must deal with them." Her argument closes with the connection with style and normative accounts of art. While not a (major) problem when dealing with past European art, contemporary art with its broadening concept of what is art cannot fit into the supposed norms, which brings the problem of how to apply style if the objects in which it used to be applied no longer exist in the same format. Without a specific answer on how to replace the answers style used to provide, she left us with more questions than answers. For instance, "In turning away from style as historical ordering to the mode of making, how

[16] See (Lang, 1987, postface, p. 14)

do we then account for continuity, for the fact that art (the arts) has a history?"[17].

Kubler will draw some similar criticism. "*Style* is a word of which everyday use has deteriorated in our time to the level of banality. It is now a word to avoid, along with *déclassé* words, words without nuance, words gray with fatigue." He follows his argument showing how the study of style was becoming more and more scarce. While this may actually be true, as the data from ngram does not specify the precise use of style that was gaining momentum and it might not be style in visual arts, could be more focused on literature studies, for instance, this trend would later be reverted, but still this does not deny the fact that during this period several art scholars dealt with the concept of style, not always in a very productive manner. Kubler's proposal will then not be to disregard style, as his colleague, Alpers, in the same book proposed, but to find the sort of specific use that the word could accommodate. "In short, style is taxonomic and extensional rather than a term suited to duration." It is interesting to note the solution he, then, proposes. By understanding that Style has different meanings regarding different fields, such as architecture, visual arts and literature, he suggests

[17] Incidentally, it is interesting to see that, in the same way style is borrowed from art studies to science studies, she borrows Thomas Kuhn's paradigm to think about art. "Do we account for this by saying that they are particularly in touch with themselves, or by saying that they are, like the aging scientists described by Thomas Kuhn, simply out of touch with the current paradigm of style?" For this quote and the others in the preceding paragraph, see (Alpers, 1987).

using Kant’s manifold to deal with this multiplicity, this disagreement. “This manifold comprises the disagreements of among technicists and connoisseurs, formalists and iconographers, historians and semiologists”, and, as a problem,

> may be resolved by within the principle of complementarity as formulated by Niels Bohr. He said that ‘the integrity of human cultures presents features the account of which implies a typically complementarity mode of description’. By this, he meant that clarity requires an ‘exhaustive overlay of different descriptions that incorporate apparently contradictory notions.[18]

[18] As weird as it might seem to an historian of quantum mechanics, such use of Bohr’s complementarity, in art studies, should not surprise, as this idea might have some of its roots inside of art. As is claimed by Schinckus, the simultaneity present in the cubist movement is one of the influences that lead to the concept of Complementarity. While we do not want to strongly commit to this vision, the idea that there is a connection is out there, whether strictly correct or not. Still, Bohr himself believed that his complementarity had further application beyond quantum mechanics or science itself, and that his principle was one that was part of the general knowledge. “The aim of our argumentation is to emphasize that all experience, whether in science, philosophy, or art, which may be helpful to mankind, must be capable of being communicated by human means of expression, and it is on this basis that we shall approach the question of unity of knowledge." For the quote above, see (Kubler, 1987, p. 168). For Bohr’s quotes, including the ones in Kubler and for a general presentation of how Bohr thought about the domain of applicability of complementarity, see (Holton, 1988). And for Bohr and Cubism, see (Schinckus, 2016).

Having presented these general accounts of the problems the concept of style both faces and generates, we can now focus on two more specific cases of authors that developed their own approach to style in art. We will first present the ideas of Arthur Danto, following with the taxonomic and psychological theory of Richard Wollhein.

Arthur Danto was a key name in the field of aesthetics. As Noëll Carroll puts, "Perhaps no other aesthetician of his generation has evolved as complete a philosophy of art as has Arthur Danto"[19], with his theories that tackled the question of what is art that made him one of the most prominent names of philosophy of art since the 1960's. If his importance were not enough, for our purposes we have an additional reason to look into his work. Danto began his career as a philosopher of science and his 1964 article, "The artworld", now a contemporary classic that originated his work on aesthetics, was heavily influenced by the ideas of Thomas Kuhn's "The structure of

[19] (Carroll, 1995, p. 251)

scientific revolutions”[20]. It begins by facing one problem that, in its essence, is quite similar to the demarcation problem. If, in philosophy of science, we need to separate what is science from what is not, in art it is necessary to understand from whatever things that exist in the world, which of those are art and which aren’t. And, in the same sense that philosophers of science try to solve this problem with theories of science, Danto, then, also proposes a new theory of art. In order to understand what is art, Danto inserts the artist

[20] This influence is recognized by the author himself, although later, and is presented in Caroline Jones' study on the influence of Kuhn’s structure on the analysis of the modernist paradigm and the artworld. The way she describes Danto’s intentions are valuable: “Danto's "The Artworld" thus paralleled the sense of science Kuhn aimed to produce (without then citing Kuhn or acknowledging his importance). The community of practitioners they evoked was at once both profoundly historical and sociological; but history and sociology were merely means to an end. Kuhn's ultimate goal (and of course Danto's) was philosophy. Both aimed for an understanding of science (or art) that would also make important contributions to epistemology.” In footnote 23, she claims, and we agree, that this connection must be further studied. Still, the way she presents the paradigm shift and the artworld change as both being completely incommensurate (“In effect, Danto was articulating two separate paradigms, incommensurate to the extent that the possessor of the former (imitation= Renaissance "window") could not comprehend the language of the latter (reality=Warhol's Brillo Box)”) does not seems to be the case as, in Danto’s artworld, there is a clear connection from latter art to earlier. In this sense, the incommensurability would be in only one direction, not in both, as reality could understand imitation as art, but not the other way around. Another major difference between Danto's and Kuhn’s artworld and paradigm is that while the artworld evolution is cumulative, with the latter artworld engulfing the earlier one, paradigm evolution is revolutionary and this was thought exactly to fight the cumulative vision of science. One may still call the evolution of the artworld revolutionary, as certainly was the one after Andy Warhol and the pop art revolution, but the meaning of revolution used in both cases is completely different, one may even say incommensurable. See (Jones, 2000, p. 501-2 and note 23).

into the art. His intention is part of the final result. So, if someone does not understand or even know the intentions involved in this particular piece of art, one may not understand something being art.

> The answer, unpopular as it is likely to be to purists of every variety, lies in the fact that this artist has returned to the physicality of paint through an atmosphere compounded of artistic theories and the history of recent and remote painting, elements of which he is trying to refine out of his own work; and as a consequence of this his work belongs in this atmosphere and is part of this history.[21]

So, to answer the question of what is art, Danto inserts the artworld: “To see something as art requires something the eye cannot decry-an atmosphere of artistic theory, a knowledge of the history of art: an artworld.” Although having a different purpose and function from paradigms, the artworld is also responsible for presenting the meaning for its objects. In the same sense that a paradigm is what makes scientific words and terms meaningful, changing the meaning when the paradigm changes, it is the artworld that conferes the statute of art to something.[22] With no artworld, objects of art would be meaningless. “What in the end makes the difference between a Brillo box and

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[21] (Danto, 1964, p. 579)

[22] Carroll defines the artworld as “an atmosphere of ideas and theories and a backdrop of historical development that provide the conceptual resources that enable not only an audience to recognize something as art, but which provide the artist with the mutual understandings that permit her to presume that there will be an audience out there prepared to recognize what she intends to communicate.” See (Carroll, 1995, p. 251).

a work of art consisting of a Brillo Box is a certain theory of art."[23] So, as the world evolves, so evolves the artworld, bringing together things that *were already art* to things that *had just become art*. In the earlier artworld, there were a number of traits that were part of a work of art that could be used to identify its style. In the new artworld, not only the older elements of style should remain, but new elements now are also part of the complex set of things that we can think of as style. The main point here is that style is not a set of traits that remains over time. They are always connected to the artworld and will increase as the artworld develops[24]. In this sense, we cannot think of style as something outside specific theoretical models, visions of the world. They are intrinsically part of how we see art. "It is this retroactive enrichment

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[23] Andy Warhol's first major work of art, the one that shocked the art community in New York, was his facsimile reproduction of the Brillo Boxes one could find in the market's store houses. But, while the ones in the market were made of cardboard, his boxes were made of plywood, yet they were completely identical to the naked eye. For the citations and a much better explanation of Warhol and his brillo boxes, see (Danto, 1964). See also (Danto, 1981 and 2013).

[24] Carroll, explaining Danto, quotes T.S. Elliot: "No poet, no artist of any art, has his complete meaning alone. His significance, his appreciation is the appreciation of his relation to the dead poets and artists. You cannot value him alone; you must set him, for contrast and comparison, among the dead. I mean this as a principle of aesthetic, not merely historical criticism. The necessity that he shall conform, that he shall cohere, is not one-sided; what happens when a new work of art is created is something that happens simultaneously to all the works of art which preceded it. The existing monuments form an ideal order among themselves, which is modified by the introduction of the new (the really new) work of art among them. The existing order is complete before the new work arrives; for order to persist after the supervention of novelty, the whole existing order must be, if ever so slightly, altered; and so the relations, proportions, values of each work of art toward the whole readjusted." See (Carroll, 1995, p. 254-5).

of the entities in the artworld that makes it possible to discuss Raphael and De Kooning together, or Lichtenstein and Michelangelo."

Richard Wollhein, just like Danto, was one of the leading philosophers for the studies in aesthetics in the second half of the XXth century. His 1965 essay, "Minimal Art", coined the use of minimal to describe art, something that was extremely important during the 1960's. In 1968, a few years after being invited by Danto to write a chapter about philosophy of art to the never published *The Harper Guide to Philosophy*, he published it as an expanded separate essay and became his most influential book, *Art and its objects*. On the matter of style, there were two major works[25]. In them, he proposes his idea of style to solve one major question: Is style something individual or collective? Or, in other words, in which dimension are we able to find it?[26] His answer goes in the direction that we can find style in any dimension we look for, but it won't have the same meaning in all of them:

> The starting-point of my considerations of pictorial style calls for the deployment of two of the distinctions I have reviewed: that between individual style and general style, and that between a generative conception of style and a

[25] (Wollheim 1965, 1980, 1987 and 1995).

[26] "Concerning artistic style, attention has focused on the distinction between individual and period style, on the psychological reality of style, on the interplay between style and representational objective, and on the role that cognizance of style plays in aesthetic appreciation" (Levinson, 2009)

> merely taxonomical conception of style. (The word 'merely' here is all important)[27]

As for general style, Wollheim claims that there are a few different modes of it. One would be a universal style, a style that could be applied to very broad movements, such as classicism, naturalism, cubism etc. We also have styles related to the period of the work. Neoclassicism, art nouveau, XVIIIth century naturalism. We may go on and think about national styles, such as French, American, Dutch and, naturally, combined with periods, and even movements, such as Italian Renaissance or German Romanticism. We may go even further in the direction of the specifics and think about styles of schools and specific places. The Bauhaus school of design and visual arts is, maybe, the best example in this sense, but there are many others. And, in the least generality possible, we have the style of a person as copied and inspired by others. For that, the main example would be da Vinci and its several followers, which is something that is quite different from the specific style of da Vinci himself. So, it should be clear that, despite both referring to the same level of generality, they function in very different dimensions. When we think about the style of da Vinci, as the specific features that are found in his own works and can be used both to identify him and to understand his works, we are saying, tacitly, that da Vinci *had its own style*. But, when we say the style of da Vinci referring to other painters that used to follow the way and the

[27] (Wollheim, 1995, p. 40)

themes he used to paint, we are, again, tacitally inferring that those painters do not possess a style of their own. In fact, it could be said that they are painting *a la da Vinci*.

So, we reach a point that is central in his argument. While individual style does have a psychological reality, and have a generative function allowing one to explain the works, general style can only be used to classify works that share similarities, therefore the nature of style is different, serving a taxonomic function, not an explanatory one. For him, the way to describe general style would be adequate "(1) it picks out all the interesting/ significant/ distinctive elements of a painter's work, *and* (2) it groups them in the most convenient available way into stylistics features" while the way to describe individual style is adequate if "(1) it picks out those elements of a painter's work which are dependent upon process or operations characteristic of his acting as a painter, *and* (2) it groups these elements into stylistic features accordingly, that is, according to the process or operations that they are dependent upon." To make things a little simpler, one is related to elements, while the other is to processes. Since he favors the individual dimension of style, let's look how he explains what processes are:

> What is a process constitutive of style? *What is a style-process*? A *style-process* can be divided up into three different items or aspects. The first item in any such process is a *schema* or *universal* under which some part of the pictorial resources available to the painter are

> brought by him. Secondly, there is a *rule* or *instruction* for placing, or otherwise operating on, that part of the pictorial resources which the schema picks out. Thirdly, there is an *acquired disposition* to act on the rule, where this disposition is, generally, not just psychological but psychophysical.[28]

Style, as it happens in the individual case, goes beyond a set of attributes that can be identified in the work. It is a way of presenting the work, of making it, but it is there even before the work is done, when it still is just a concept. The style is both part of where the work comes from and how the work comes to be. Part of what it is to be a great artist is to have control over all this process, not being driven by it, but driving it to the direction the artist wants. This leads us to another way of understanding the difference of the two modes of styles:

> And the crucial consideration here is that style is something formed, not learned. Indeed, it may be just here that we find an important difference between individual style (...) and general style. It may well be that general style is learned, not formed, and correspondingly that the general style in which a painter works is to be explained by external factors, including existing conventions.

Wollheim understands that his proposal is not completely aligned to the most common usage of style and that it would be necessary a research

[28] For the citations, see (Wollheim, 1987, p. 190-1)

program in order to identify those stylistic features in the way he proposes. From the guidance he sets, we chose the following idea to conclude:

> The first maxim is that stylistic features should be expected to be identified on a very abstract level indeed. By this I mean that different elements instantiating the same stylistic feature may exhibit gross diversities as far as their physical configuration is concerned, and, again, comparatively minute differences in physical configuration might suffice to make different stylistic elements instantiations of different stylistic features. This was something that, for instance, Giovanni Morelli totally failed to take account of.[29]

The name of Morelli, here, is rather important, as it shows the kind of contrast that Wollheim proposes from mere details to constitutive elements. As Carlo Ginzburg has taught us, Morelli's method of identifying works of art was based on finding the little details: "In each case, infinitesimal traces permit the comprehension of a deeper, otherwise unattainable reality: traces, *more* precisely, symptoms (in the case of Freud), clues (in the case of Sherlock Holmes), pictorial marks (in the case of Morelli". But, as Ginzburg himself notes, this method contributes very little to none in order to understand art (or human sciences):

> The quantitative and antianthropocentric orientation of natural sciences from Galileo on forced an unpleasant dilemma on the humane sciences: either assume a lax

[29] (Wollheim, 1987, p. 199).

> scientific system in order to attain noteworthy results, or assume a meticulous, scientific one to achieve results of scant significance.[30]

So, the main point that distances a style based approach and Morelli's one is that while the latter searches for those elements that appears unconsciously in the works, the infinitesimal details, which may or may not be useful to identify authorship, in the case of style we look precisely for those elements that appear consciously, that the author specifically, with all its control, chooses to place in its work. With this in mind, we can now turn to some attempts of using style in science.

[30] (Ginzburg, 1989, p. 101 and p. 124).

Examples of Style on the foundations of Physics:

In order to understand a little better our approach to style, we will examine a few aspects of three physicists' style that performed research on foundations of quantum mechanics. This will not be a general examination of their style. There are so many details and topics to be examined in such endeavor that this can only be done in a work that is directly focused on such. As this is beyond the scope of this work, what we will focus on is one specific dimension that is distinctive in these cases and that allows us to compare their different approaches on a similar characteristic: how to tackle the problems on foundations of quantum mechanics?

We shall begin with Anthony Leggett, since he is also the subject of another part of this thesis. As we have shown, Leggett held a very deep dissatisfaction with the general framework of quantum theory. The nature of this dissatisfaction is not relevant here, but the way he chooses to further study it. Just a few years after completing his Ph.D. he decided that he would be completely focused on foundations of QM. His quest would be to find situations in which QM would show to be wrong or, to use his words, it would break down. The idea that this is something that could be found, that there are problems or contexts that QM would either break down, be incomplete or, even worse, completely absurd was present since the beginning of the theory. For the first example, the breakdown, we have the famous thought experiments that Albert Einstein presented to Niels Bohr during the 1927 Solvay conference, mainly trying to find situations that were incompatible with a main aspect of the theory, the indetermination principle. As an example of the second, we have Einstein again, this time with Boris Podolski and Nathan Rosen, and the extremely influential work on nonlocality as a necessary (and, in their opinion, wrong) consequence of the theory[31], while an example of the third is Erwin Schrödinger's article on the simultaneously dead and alive cat being part of a quantum experiment.

Leggett would follow the path of the first example, trying to show QM wrong. But, instead of using thought experiments, his work would focus on looking for very extreme situations in which the applicability of the theory was neither obvious nor guaranteed. It was expected that the theory could work on those cases, but it was not a necessary assumption. Those situations were so different than those usually found that it made them a very good candidate to model, using quantum mechanics, and to perform the experiments to test those predictions, but with the expectative that the experimental results would not agree with the predictions.

To do this kind of questioning, it was necessary to apply the theory into those very extreme situations. This had one impórtant consequence: Since those problems were so extreme, the endeavour to model them was a rather

[31] To be precise, they claim that because quantum theory cannot agree with the requirement of locality, the theory can't be considered complete and argue that a different one, this time complete, is possible to be achieved.

difficult one. They were very far away from the usually simple systems, dual levels, that were routinely used on foundations of quantum mechanics. Also, another important consequence of this approach was that this kind of research was highly valued not only by those interested on foundational issues, but also by a more general public. To summarize, the elements that we are calling Leggett's style here are: 1. Using extremely difficult problems to test QM; 2. Those problems needed to allow for experimental tests in order to evaluate their results and compare them with QM; and, 3. Choosing and solving problems that were not *only* immediately important for foundations of QM, but also to the more general audience of physicists.

The second example that we have is John Archibald Wheeler. Wheeler would be mainly identified with his research on General Relativity, but, before that, he had done very important applications of QM, including a classical work with Niels Bohr on the liquid droplet model in 1939. Before retiring from Princeton University, he didn't complete any research on foundational issues, but he did advise one with his Ph.D. student Hugh Everett on such issues. After leaving Princeton and moving to his own institute associated with the University of Texas, Austin, his research would mainly be focused on the foundation of QM.

In his case, we can identify a general style of his approach: He liked to use what we could call bare theories, with the least possible assumptions over the basic mathematical and physical structure of the theories. In the case of general relativity, for instance, he liked to apply the equations and see where they would lead[32]. With this, he proposed the existence of geons, wormholes, black holes, and other structures that were a consequence of the mathematical derivation from the theory, but whose existence were not identified (and some aren't still today).

With the delayed choice experiment, Wheeler sought[33] to show that Bohr's idea of phenomenon was in perfect agreement with the theory, despite being profoundly anti-intuitive, as it confronts an idea of reality independent of the experimental context. We can take, for instance, the description made, after the experiments were performed, by someone as insuspect as one can be about it, Tony Leggett: "In fact, there is no real paradox here (or in any of the other delayed-choice experiments); a consistent application of the quantum measurement axioms predicts precisely the experimentally observed results."

[32] This kind of approach is also not unique, especially in the context of general relativity. The idea that it could be possible to use theoretically possible physical structures, such as wormholes, to travel in time in the direction of the past, or even to other universes, is one example of this kind of approach, that takes the consequence of the theory without additional assumptions to exclude this kind of result. As Michel Paty has argued, this specific consequence, traveling to the past, should be dismissed in principle and, therefore, the use of additional assumptions would be necessary. While we certainly agree with Paty, we don't want to discuss more deeply whether this kind of approach is a good one or not. In the case of time travel it certainly isn't, but other kinds of consequences in QM, such as nonlocality and entanglement, were result of, one way or another, this same type of approach. At the same time, in the context of QM it was developed Superselection Rules exactly as a way to address the results that were in principle allowed in QM but were shown to be incompatible with experimental results from classical physics.

[33] "We search here, not for new experiments or new predictions, but for new insight." See (Wheeler, 1978).

and “What the “delayed-choice” experiments really illustrate, in a spectacular way, is the pitfalls of applying the projection postulate at too early a stage in the game, while nothing has been registered at the macroscopic level and there is still a possibility of mutual interference of the possible alternatives”[34]. So, in the conflict of an external (to the theory) notion of reality and an internal consequence, Wheeler naturally chose the latter. It should be said that while the experiment he proposed could, in principle, have its results conflicting with QM, and that it actually serves to rule out some kind of realistic theories, Wheeler never had any doubt on its future results. Even before any possibility of actual realization, he discussed the consequences of the results in agreement with the theory.

> Not one of the seven delayed choice experiments has yet been done. There can hardly be one that the student of physics would not like to see done. In none is any justification whatsoever evident for doubting the obvious predictions.[35]

In a more general way, Wheeler believed in a reality that was *created* by the theory:

> «Fabricate form?» Do you suggest that even the 4-dimensional spacetime manifold is only a fabrication, only a theory – irreplaceable convenience though that theory is?

[34] See (Leggett, 2009, 164-5).
[35] See (Wheeler, 78, p. 40).

> Yes! Compare space-time with cloth. Each it is useful under everyday circumstances to call a manifold. Yet each is exactly then not a manifold when it comes to an end, whether in the selvedge made by the loom, or in the geodesic terminations made by one of the «gates of times» – big bang or big crunch or black hole. Nowhere more clearly than in the ending of space-time are we warned that time is not an ultimate category in the description of Nature.[36]

With such belief, the path of following a theory to its deepest consequences is not unexpected. In the case of Hugh Everett's work, it is possible to see his influence on how the text was completely rewritten from the first version to the published one[37]. One main reason for its development was to remove the dual dynamics of quantum systems, one linear and regular and the other, the measurement, abrupt and non-linear, so, as to speak, to have just the bare quantum dynamics without observations as a special case within the theory. He also tries to remove the ad hoc Born's rule, that specifies the probability of a specific result in a measurement. In his assessment of this interpretation, he argues:

> Observations are treated as a special case of normal interactions that occur within a system, not as a new and different kind of process that takes place from without. The conventional mathematical formulation with its

[36] See (Wheeler, 1983, p. 204).

[37] For Everett's thesis history and the central role Wheeler had in it, see (Osnaghi, Freitas and Freire, 2009).

> well-known postulates about probabilities of observations is derived as a consequence of the new or "meta" quantum mechanics.

and

> Instead of founding quantum mechanics upon classical physics, the "relative state" formulation uses a completely different kind of model for physics. This new model has a character all of its own; is conceptually self-contained; defines its own possibilities for interpretation; and does not require for its formulation any reference to classical concepts. It is difficult to make clear how decisively the "relative state" formulation drops classical concepts. One's initial unhappiness at this step can be matched but few times in history: when Newton described gravity by anything so preposterous as action at a distance; when Maxwell described anything as natural as action at a distance in terms as unnatural as field theory; when Einstein denied a privileged character to any coordinate system, and the whole foundations of physical measurement at first sight seemed to collapse.[38]

So, in this new formulation, by Everett, with his sponsorship and signature, no new problems of applications were being solved, no new experiments were to be proposed. All the effort went only in the direction of eliminating the dependences quantum theory had on external assumptions. So, in summary, his style of approach foundational problems wouldn't be directed to solve any new problems, or even to propose experiments to test quantum mechanics,

[38] See (Wheeler, 1957).

since even when he proposed them, they were only to confirm it even further and to examine the consequences of applying the formalism, to see where it would take us and what it says about reality. When he, with Everett, tried to reformulate the general framework of application of the formalism, it was in the direction of eliminating additional elements in the formalism, to think what would come of the theory if it were the only thing to be used.

The last one is David Bohm[39]. With an intention that shares a lot of similarities with Leggett, his approach would be quite different. He also believed that the standard formulation of QM was faulty, but not in the sense that it would be in conflict with experimental results. He thought the results were correct, but the ontological load of the theory was unsatisfactory, so he developed a new realist theory in terms of non-local hidden variable that, despite having trajectories in the space, could replicate the same experimental results of QM, at least in the small domain that it was originally applied. As Freire describes,

> Bohm used these models to carry out detailed calculations of a number of different problems, for instance, stationary states, transitions between stationary states (including scattering problems), the Einstein-Podolsky-Rosen Gedankenexperiment, and photoelectric and Compton effects. To achieve results compatible with those from quantum mechanics, Bohm

[39] For an in depth analysis of David Bohm's history and approach to QM, see (Freire, 2019).

> modeled light as electromagnetic waves. In all these problems he found the results predicted by the usual mathematical formalism of quantum theory.[40]

There was also the hope that the usual formulation of QM wouldn't be able to describe intra-nuclear phenomena with precision, but this remained as a bet. So, just like Leggett, Bohm could see possible candidates to show QM limit of applicability, but differently, he chose not to pursue them, focusing instead on already known applications. It was, naturally, a very sound choice, as it could show the logical viability of his proposal, but we need to understand that this was a choice, not a necessity or, in other sense, a matter of style.

In a sense, they all were trying to do something quite similar. They wanted to understand better the foundations of physics, more specifically the foundations of ṕhysicis in the domain that is usually associated with QM. Two of them wanted to show it wrong, while the other wanted to deepen its consequences. From the two that thought the theory's foundations were wrong, one chooses to look for extreme situations to find experimental results while the other focuses on showing the viability of an alternative theory. It is possible to say that the specific objects that they were dealing with were different, which, then, entailed them to follow different paths, but this same object is also part of their styles, their unique approach into the field.

[40] See (Freire, 2019, p. 67).

As is well known, Leggett, Wheeler and Bohm were among the most important theoretical physicists in the XXth century. They all made long lasting contributions to the field, but the recognition they received were very different. In one sense, Leggett was the most recognized one, as he received the 2003 Nobel prize and held very prestigious positions throughout his career. In other, he was virtually unknown inside the field of foundations of QM, despite his long lasting contributions. When Wheeler organized, together with Wojciech Zurek the 1983 volume on Quantum Theory and Measurement, selecting the most important works on the field, Leggett was already a very prestigious researcher, but none of his works made it into the book. Outside the foundational field, his works were fueling several new approaches, experiments and dissertations. Wheeler's delayed choice took some time to become important in the field and it was mainly through the experimental implications it had[41]. Bohm's work was heavily debated, but it was not before the development of Bohmian Mechanics that its applications became a major topic outside the field of foundations[42].

While certainly more work is needed to understand the specifics of styles here involved, one could argue, for instance, that their reception was tied,

[41] As Joan Bromberg commented "Another ten pages sketched out six other arrangements that might be modified into delayed-choice experiments. I read this article, therefore, as an appeal to experimentalists to carry out such trials." (Bromberg, 2008, p. 327)

[42] For a major review on Bohmian Mechanics and its applications, see (Benseny et al, 2014).

among other aspects, to the style of research, to how they presented their objects and how they developed them. Still, there are several ways to understand those dynamics, even without any mention of style. There are historical contingencies, political movements, economic influences, and even fashion trends, just to name a few. What we claim, here, is not that those approaches are wrong. Is that it should be clear that we may need to analyze more dimensions in order to understand even better those dynamics, and that style as a variable might be important in several of the outcomes within the field of science.

Conclusions

With the concept of style being in the domain of the individual, we may expand the scope of tools that history, sociology and anthropology of sciences apply in its studies. By using this category, we are able to expand the set of variables that we examine when performing such studies. This may become important as when we deal with sciences, we are mainly still attached to the cognitive aspects of its results, the debate among them and the major social conditions and aspects around them, on how those social conditions influence the dynamics and the results of science[43]. On the other hand, with the notion of style, we can begin to examine several other aspects of scientific practice, in a way very similar to how style allows one to examine very specific details of an artwork.

Naturally, it is not possible to draw a complete list of what could be examined this way, but one can think of several examples that may (or may not, as in the case of art, which parts of one's artistic practice that are part of its style and relevant to understand it can only be identifiable in concrete

[43] This still remains as such because of the lingering dependency science studies have on the notion of truth. When we break with such dependency, we begin to be free to understand several other aspects that are not closely related to such a notion. While a complete discussion of this lies outside of the scope of the present work, it should be clear that the whole panorama of science studies is somewhat complicated. In the majority of studies, science is dealt with as a social process inside a society, but at the same time, when the field sleeps at night, the belief of science as related to the *truth* is still there.

cases) help to understand better science. We could include the use of language, the argumentation, the kind of articles, whether reviews, letters or other, the journals' choice for publication (and, for such, we would need to understand a lot better the difference among the practice of different journals, something that should have been done by now due to the extreme importance journals have on contemporary science), the type of problems tackled and the general approach to them, the impact training has, with the role of textbooks and other kinds of training practices. We can still think of the type of funding, on how the individual deals with choosing and searching for funding sources, of the choice of positions, the different universities and research centers, the nation and specific locations inside, with regard to the individual, not the collective (while understanding that both need to be examined). The list could certainly go on with more and more examples being added. The point here is not to think about what could be studied, even more because several of the examples before have been dealt in specific cases. The point is that we don't really have a way to connect and think how those different aspects act together to affect the practice of science. As such, a sophisticated notion of style in science needs to be developed, understanding beforehand that in this endeavour, the same kind of problems that arise in art studies are bound to arise here again. And, for that, it may be necessary to develop, as one may say, a science of style that is not only connected to the artistic domains of expression, a science of style that happens to understand style as being present in several different expressions of the human experience, with the sciences included among them.

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The specificity of art and science

The specificity of art and science: A brief discussion on the specificity of art, art studies, and history of art vs the specificity of science, science studies, and the history of science and also their common ground and entanglement

Specificities - Art and science are different things. This is not a very advanced statement, a rather trivial one, actually. It is not a necessary one also, as they could be extremely similar in principle. Still, they are not. It's easy to see how they can be different. For that, we can take very specific examples of each field and think about them. Let's take, for instance, one of the most famous works of art: the starry night, by Vincent van Gogh. It is pure art as pure as art can be. It was, at first, not considered art, but this has been long gone (this will be important later). In the same sense, we can think of theoretical physics. As theoretical physics is an extremely vast subject, we can take a specific part of it, let's say, superstrings. It is also as pure as theoretical physics can be and, although not in the same manner, it also suffered the criticism that it might not be physics. As is something a little younger than the starry night, these critics are not long gone and, although in a very limited sense, it still is possible to discuss whether superstrings is a part of physics, but a more precise description of the debate would be whether it is good or bad physics, or, in another way, if it could be useful at all. So, if not good physics, it mostly is physics and, as such, science.

The starry night is, clearly, not science. It would be possible to use it in science in several ways, ranging from the study of mental illness to the way paint dry, but those issues are not in the essence of this work of art. We do know extensively about van Gogh's life and the context in which the starry night was produced to be completely sure that it has nothing to do with the practice of science. Even the most fluid concept of science would have difficulty framing this case as being part of science.

Superstring theories also are not usually identified with art. This happens despite being one of the several physical theories that uses aesthetics arguments on its behalf. Superstring is said to be elegant and simple (and, for physicists and mathematicians, simplicity is beautiful)[1]. But the physicists that are involved in superstring theories are not trying to reach any kind of art and its results, currently, are not being accepted to be presented in art museums.

[1] A famous book that talks about superstring theory is called "The elegant universe", by Brian Greene, one of the main theorists in the field. While certainly the community around superstring theories do find elegance an important value, not all physicists agree. Albert Einstein, for instance, said "I adhered scrupulously to the precept of that brilliant theoretical physicist L. Boltzmann, according to whom matters of elegance ought to be left to the tailor and to the cobbler." See Einstein, A. (1920). *Relativity: the special and the general Theory.* New York: Henry Holt and Company.

Again, this conclusion is not unexpected, since we begin arguing exactly this, that science and art are different. The main point here is that there will always be several cases in which those two fields remain separated.

There can be another way to separate these two fields, that does not focus on specific works of those two fields. This will be through a not so obvious path, we will think about the way those two fields are practiced. This time, we will begin with science.

We are, as we all know, years away from the time in which it was believed that science is science because it was created through the scientific method[2]. Instead, without facing every single detail, we now have the scientific institution, an abstract concept that connects several groups. It is it, with its real institutions, such as publishers, funding agencies, research institutes, universities, societies, academies, etc, and its members, the scientists, that reserves to itself the power to identify something as science. Usually, it claims that science is what is produced by its members, through its institutions and made public through its

[2]This sentence is both true and not true at the same time. While Paul Feyerabend may be the one that fought more intensively against this idea (as it is clear by the title of his most famous book, "Against Method"), since the 1960's studies of science have been showing the idea of a unique method is plainly wrong and, therefore, this is an idea that has virtually disappeared from professional studies. At the same time, scientists and its efforts to popularize its results do remain using the idea of a scientific method being behind what is singular about science itself.

own means of publishing. The people that have access to those means are those who were certified by those institutions, usually through graduation courses, and that might later be called scientists. While this is a circular definition of how science is currently practiced, science being what is done by scientists who, at the same time, are the ones who are part of science, it is a solution that deals with the problem that what someone does totally outside of the scientific institution is not a part of science currently[3].

We can take an example. Let's say Albert Einstein went to the university and presented his Ph.D. thesis on statistical mechanics, learned about the problems of electromagnetism at the time, wrote his paper about the electrodynamics of moving bodies, which is unconnected to his thesis, but never published it. By doing so, the paper

[3] Although not exactly identical, this description of science owes a lot to the Bourdieusian view on science. It is an institutional theory of what science is and may not be adequate to earlier periods of science, but it solves so many problems at the very little expense of objectivity and truth. Of course, while these two values remain highly regarded, especially among scientists, the true challenge to them is the Duhem-Quine problem. Then, again, the solution to conciliate a theory that is about institutional practice with the everyday feeling of the scientists is that they can believe that their theories are both true and objective, and as long as everyone agrees, everything may work as if they were so, despite no logical or epistemological background to support it. See Bourdieu, P. (1975). The specificity of the scientific field and the social conditions of the progress of reason, *Social Science Information*, 14(6), 19-47, Quine, W. (1951). Main Trends in Recent Philosophy: Two Dogmas of Empiricism, *The Philosophical Review,* 60(1), 20-43, and Stanford, K. (2017), Underdetermination of Scientific Theory, in *The Stanford Encyclopedia of Philosophy*, <https://plato.stanford.edu/archives/win2017/entries/scientific-underdetermination/>.

would be the same as the one he actually wrote and published. The only difference is not publishing it. So, it would be produced through whatever those methods that are considered scientific and would face a subject that is thought to be scientific, by someone with all the credentials that are thought to be scientific. It would, nevertheless, not become science, as science today is only those objects that go through scientific paths of validation and those methods are, basically, making it go public, even if it is through means that are not so strict such as preprints on electronic databases as they are, by all means, both public and part of science.

We may return to art. Art, just as science, is extremely dependent on the institutional dimension of its practice. Bourdieu has already pointed, in his works about science, that much of what was being discussed there should also be understood examining the field of Art (and also other intellectual fields). Also, in the same way we say science meaning both the abstract entity and the mundane institution in which science is practiced, we can say art, but also artworld. And, again, this artworld is composed of so many institutes and artists, but also by the belief and understanding of what art is at these specific periods of time, as the space for divergence in this matter is way bigger than the one allowed inside science, with very different outcomes. This, for instance, leads to the idea of a marginal art that does not completely fit in the artworld or the institutions, but an idea of a marginal science, with the same meaning, is nothing but a countersense.[4]

We may go further and reach the level in which we find the artist, the individual entity. While commonly produced inside the artworld, through its courses, academies, collectives, etc, the artist may not need those. Family, for instance, is usually a path to enter the artworld without any institution, or we may say formal institution, to credit for its entrance. Some of the most important artists of the last 200 hundred years weren't formed inside those formal institutions, while not a single major scientist arose outside institutions in the same period[5]. But, even if not formed inside the formal institutional artworld, this artist may still reach it somehow. There are several ways to accomplish this, but they are not important here, as we want to think about those who don't, as obviously those who do reach it created recognized art. So, we have the individual artist who, somehow, stands outside the institutionalized world of art. He is still able to create art, even if no one ever sees it, beside the artist himself, as whatever he produces still has all that is

[4] There is a deeper meaning to this as marginal art is thought to be art, even if not yet recognized as such, for several reasons, and marginal science is thought as not being science and hardly may ever change such outcome, albeit still possible.

[5] Still, so many of these important scientists come from families that are already part of science. So, it is not the role that family has in its descendents choices that is different between those two fields, but how the formal institutions play a different role on the entrance in each field.

needed to be recognized as art. Of course the discussion about the quality of the art this specific artist produces is a different one. It will possibly be bad art, but nonetheless art[6].

So, we have then a major difference among those two fields. While art, at least in principle, may have an existence that can be identified individually, even more so in the contemporary period, Science cannot, as its practice is, necessarily, social. While there were years and years of work trying to develop the logical characteristics that could define science, an endeavour that did not seem very successful, this problem grows enormously when we expand the notion of science beyond the natural sciences and include all those fields that have a similar social practice. If it is quite hard to find a common place among physics, biology and geology, when we add linguistics, economy, anthropology, and architecture, this search seems quite impossible to be completed. On the other hand, it is possible to find a similar logical ground that unifies

[6] This point is based on Arthur Danto. His ontology of art changes from his first article in the 1960's through his classic "Transfiguration of the commonplace" all the way to his last book from 2013, What art is, but remains very close to the idea that art becomes art through the meaning that is embodied in it, may it be through the artist or through other means. His reflections emerge from the problem posed by the work of Andy Warhol, in which an work of art may be visually indistinguishable from an everyday object, then begging the issue of why one of them is art and the other isn't (or, in other sense, is a different work of art)? See Danto, A. (1964). The artworld. *The Journal of Philosophy*, 61(19), 571-584, (1981). *The transfiguration of the commonplace*. Cambridge: Harvard University Press and (2013). *What is art?*. New Haven: Yale University Press.

classical forms of art, such as literature, painting, dance, and theater, with newer forms of art such as video-games, design, comic books, etc that it is not based on the social practices, as, contrary to science, the several types of art works are produced in extremely different social configurations. Just as an example, it would be very hard to find a social common place between a dance company and a producer of video-games. Still, both may produce art that has a logical identity.

Common Ground - While different, science and art can have objects and social practices that are part of both domains at the same time. The way to understand this is not, as we did before, looking through the more obvious cases, the more obvious situations that are thought as being either science or art, but to find objects and contexts in which both are the objectives of the authors, who will be, at the same time, artists and scientists.

We can begin our first example with the classical work from Euclides da Cunha, Os Sertões, from 1902[7]. Documenting the military battle against Antônio Conselheiro, Cunha created a book that merges the scientific prose of his time with an extremely skilled and poetic

[7] In English, Rebellion in the backlands, published by the Chicago University Press.

description of a central moment in Brazilian history[8]. As we are over a century further away from its publication, it's easy to recognize it as a classic of literature, as its place as such is undisputed, with very beautiful editions still being published by all sorts of publishers, but not as much to see its scientifics contributions. As a way to keep this essay short, we will trust the words of the sociologist Gilberto Freyre and literary critic and writer Antônio Candido. Freyre, describing Cunha's work, claimed that:

> "On the description of the backlands, the scientist would make mistakes on details of geography, geology, botany, anthropology; the sociologist, in minuteness of explanations and social diagnostics of the countryside people. But to redeem itself from the mistakes of the technique, there was in Euclides da Cunha the poet, the prophet, the artist full of genial intuitions. The Euclides that discovered in the landscape and the man from the backlands values far beyond the right and wrong of science grammar."[9]

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[8] This point has been developed by José Carlos Barreto de Santana in his book on science and art in Euclides da Cunha, focusing mainly on the scientific dimension of the Os Sertões, since it is the least studied dimension. See Santana, J. (2001). *Ciência e Arte: Euclides da Cunha e as Ciências Naturais.* São Paulo: Hucitec.

[9] "Na descrição dos sertões, o cientista erraria em detalhes de geografia, de geologia, de botânica, de antropologia; o sociólogo, em pormenores de explicação e de diagnóstico sociais do povo sertanejo. Mas para o redimir dos erros de técnica, havia em Euclides da Cunha o poeta, o profeta, o artista cheio de intuições geniais. O

Even if Freyre showed criticism about the science in it, that only serves to place Cunha inside this very same science. The main argument, of course, is not that the science is bad, it is that the literature, the art, is so important that this makes the mistakes unimportant. If not the best science, it still is science. Cândido would also connect the scientist and the artist:

> "We will only comprehend him, for, if we put him beyond sociology - because somehow he subverts the social relations discriminated by science, giving them a figure and a quality that, without drowning the observation realism, they belong before to the category of vision."[10]

The dual dimension of Cunha's book, the artistic and the scientific, was completely clear for both of them. Nowadays, this idea is somewhat strange, but at the time, because of the way sociology professionalized itself during the XIXth century, distancing itself from literature, the idea of a sociologic work also being part of the literary world was plainly common. "Sociology's precarious situation as a kind of 'third culture'

Euclides que descobrira na paisagem e no homem dos sertões valores para além do certo e do errado da gramática da ciência." Our translation.

[10] "Só o compreenderemos, pois, se o colocarmos além da sociologia - porque de algum modo subverte as relações sociais normalmente discriminadas pela ciência, dando-lhes um vulto e uma qualidade que, sem afogar o realismo da observação, pertencem antes à categoria da visão." Our translation.

between the natural sciences on the one hand and literature and the humanities on the other was exacerbated by the fact that the intellectual traditions of the Enlightenment and the counter-Enlightenment struggled with one another over its destiny."[11] Walter Lepenies work shows exactly how sociology grew among this tension in three different contexts, France, England and Germany. Several literary classical authors, such as Gustave Flaubert and Honoré de Balzac, do stand exactly in this same intersection of art and science. They do help to see this kind of superposition, although it should be mentioned that, in this specific field of sociology, this is no longer the case. Sociology went on to become a fine example of *scientific objectivity*[12], while even the more sophisticated realistic works of literature, such as the magnificent books from Svetlana Aleksiévitch, Nobel prize of literature in 2015, does not fall inside the world of science. But sociology and literature are not the only interface.

[11] The quotes from Freyre and Cândido were from texts published in 1943 and 1952, both reunited in one of the several commemorative editions of Os sertões. This specific edition is from Editora Ubu. Cunha, E. (2019) *Os sertões.* São Paulo: Ubu. The last quote is from Lepenies, W. (1988). *Between Literature and Science: The rise of sociology.* New York: Cambridge University Press. The quote is at page 7.

[12] Read it with all the possible cautions about the notion of objectivity in sciences.

Design and Architecture are examples that live in this dual dimension[13]. Design, for instance, historically, has evolved on the tension between crafting beautifully and designing objects that address several constraints such as usability, mass reproducibility, social function and several others. The most famous school of design, Bauhaus, had several major artists among its masters, such as Paul Klee, Wassily Kadinsky, László Moholy-Nagy and Josef Albers. At the same time, "this total rejection of art as an explanation for form reflected Bauhaus ideology of the time, whereby art was to be absorbed into handicraft and engineering. The word 'art' was to [be] deleted from the dictionary"[14]. This duality seems to be well recognized since its time and up to today. Theo van Doesburg, one of the most prominent names of De Stijl movement, when considering becoming a master there, asked "When I first became acquainted with the aims of the Bauhaus, I was not only amazed but enthusiastic. Where else in the world was it possible to satisfy the new desire for a systematic art education, a desire which had

[13] Before we begin, it should be noted that both subjects are more easily classified inside technoscience than directly into science. While this is true, this does not, by any means, exclude the scientific dimensions that are an intrinsic part of every technoscience field. It would be very hard to argue that medicine, pedagogy, the several dimensions of engineering, pharmacy and even chemistry does not have a true scientific dimension. At the same time, it is quite easy to show that they do not share the same artistic dimension of both Design and Architecture.

[14] Droste, M. (2011). *Bauhaus*. Berlin: Taschen, page 84. It should be noted that, as is the tradition of art books, Taschen published this edition in a Hardcover with a well designed frontispiece to be shown in coffee tables to exhibit artistic culture.

begun to assert itself in all countries in the fields of art, science and technics?[15]" About a century later, *Nature* magazine, on the 100th anniversary of Bauhaus, covered it claiming that "There, mathematical principles and engineering rigour were applied to fine art, craft and architecture. The school pioneered a splendid amalgamation of science and art.[16]"

On one hand, Design schools remain typically associated and even based inside art schools. On the other hand, the theoretical and the experimental work, with the discussion about its practice happens through conferences and peer reviewed articles published on journals indexed and ranked by its impact factor. Certainly, this description

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[15] 1928 quote from Theo van Doesburg published in the catalog of the 1938 Bauhaus exhibition at the MoMA (Museum of Modern Art, NY, USA), p. 93 Unfortunately, it does not say from where the quote is, only that it was from the press. Bayer, H., Gropius, W. and Gropius, I. (1938). *Bauhaus, 1919-1928*. New York: The Museum of Modern Art.

[16] Weber, N. (2019). The Bauhaus at 100: science by design. *Nature*, 572, 174-175. The author, Nicholas Fox Weber was a student of Josef Albers, former student and master at Bauhaus, who emigrated to the USA. He runs the Josef and Anni Albers foundation.

seems familiar to anyone involved with science[17]. Architecture does share a lot of the description made above.

> “a sober view of the state of architecture at the beginning of the 21st century reveals a pluralistic and diverse scene, one where some architects clearly practice as visual artists (these are the so called “star-architects”), others practice in a corporate context much like engineers (these are technical production firms with names like SOM, RDGB, and BNIM), and a few have become more socially active and engaged than ever (these are firms that see themselves as socio-environmental activists)”[18]

Modernist architecture, for instance, greatly in debt to Le Corbursier, paid an extremely significant attention to the technical dimension and

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[17] The debate about the nature of Design is actually way more complicated than what this discussion shows. There is a permanent tension about the technical side of Design, more if we think about the industrial Design, with the artistic side. A fine example of such are the several books by the famous Italian designer Bruno Munari in which he argues in favor of the artistic side of the profession. Three examples are Design as Art, Artist and Designer and Fantasy. Of course this is only a necessity if this is not completely agreed upon. There is not a single book written supporting the idea of Physics as science. See Munari, B. (1971). *Design as Art.* London: Penguin Books, (2004). *Artista e Designer*. Lisboa: Edições 70, and (2018). *Fantasia*. Lisboa: Edições 70.

[18] See Kroes, P. *et al*. (2008). Design in Engineering and Architecture: Towards an Integrated Philosophical Understanding *in* Vermaas, P. *et al. Philosophy and Design: From Engineering to Architecture*. New York: Springer. for the quote at page 7 and part of the following argument. See also Cohen, J. (2012). *The Future of Architecture Since 1889*. London: Phaidon.

its problems. In Brazil, with Lúcio Costa, Oscar Niemeyer and several others, there were important developments related to the specific use of *pilotis* in shape of V and Y, skeletons with reinforced concrete, and shells abundantly. Along with a series of changes and innovations related to the concept itself of architecture, this produced buildings that not only reshaped how they were used, how they integrate the city in which they are a part of, but also the aesthetic of the constructions and the technical engineering necessary for it. This, of course, is just a specific example, in one movement of a group of architects, of the kind of phenomena that is dealt by architecture that has to deal with so many different dimensions of knowledge, ranging clearly from art to several different fields of the sciences[19].

Entanglement - While there will be objects that are easily part of both fields, art and science, such as design objects, architecture and essays about the human experience, the interesting cases are the ones that are not located on this more recognizable interface of the fields. For instance, if we are to take the experimental setup for, let's say, the Bell

[19] The kind of innovation Niemeyer set in the field of architecture is such that the manner he used shells and *pilotis* can be found in several constructions around the world by architects directly influenced by him. At the same time, several of his constructions, mainly because of their visuals, are considered historical heritage. His influence was so huge that he became the first architect from an ex-European colony to design a permanent residential building in Europe.

inequalities' tests with the use of crystals with parametric downward conversion as a work of art for some specific reason, not the result of the experiment, not the meaning of the result, but the experimental setup *per se*, we can begin to see the interface of science and art happening in different places than we are used to[20]. We may go even further to more extreme examples and think about the source code for the program that was used with the data emitted by the Large Hadron Collider sensors to calculate the interactions in order to identify the kind of reaction that were used as proof of the existence of the Higgs' boson[21]. Since art can be anything (but not everything), this rather arbitrary example shows us how difficult it is to identify the specificity of those fields. We can, actually, take one more example of this kind of problem, but now using another hypothetical situation, in this case, an object of art, created as such, but being part of science.

Let's imagine that, as so many enter design schools, or architecture schools, with the intention of creating objects of art, someone does the

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[20] This has been done before. Several old scientific instruments are now part of the collection of art museums, and the most easily found are clocks and compasses. One may see, for instance, the collection of the MET, the metropolitan museum of New York. Photography taken by scientific telescopes are also commonly found in art museums and they are results of experiments, but also presented with aesthetic and artistic meaning.

[21] In 2012, the MoMA announced the acquisition of more than a dozen video-games, with their source-codes, as part of the permanent collection of the museum. So, the idea of a source-code being part of art is not so far fetched as it may seem at first.

same and joins a physics school. Understanding that in order to make good art inside this field, one also needs to be very good on the field itself, so our imaginary student joins a prestigious undergraduate school and, later, follows with a prestigious Ph.D. in any area of physics. But, still, it needs more training and now is in a post-doc at a prestigious institute of research. In order to reach all those positions, one has been doing the highest kind of physics one can make, but let's not forget the reasons behind it all. After the post-doc and several years of excellence, one now obtains a tenure-track position as assistant professor in a prestigious university and begins obtaining its own funding. After around seven to ten years, one finally receives its tenure and has the liberty and the funding to do whatever one wants to. The thing is, the art that one wanted to do is exactly the same it has been doing all those years, as it wanted to make physics as a form of art. Now we don't need to discuss whether or not his art, contained in several articles, presentations in conferences, and book chapters is to be considered good art (or even immediately recognized as art, as the institutions of artworld sometimes take way more time than is reasonable to accept something as art). The objective to make art is concluded and this new piece of art is now done. It is important to note that it was not necessary, as part of the intention, the desire to make any contributions to science, in fact it was even being done in opposition to this idea, but still it was a side effect of this project[22]. We then have a class of objects that is part of both science and art simultaneously.

If the whole example seems exaggerated, and of course it is, it also isn't so far away from two examples: the Sokal affair and the Bogdanov affair. The intentions of Sokal were to question the lack of rationality of a specific field of society. In essence, this is extremely similar to what Marcel Duchamp did when he presented his fountain to the Society of Independent Artists. They had a policy of accepting any work of art to the exhibition, just as long as the artist had paid the fee. So, he chose an absurd object to show the problem in this specific rationality[23]. So Sokal's article could very much be a work of art. In fact, it may even be someday. But, as it was not his intention, it is not yet. The thing is that he used the structure of the field itself in order to present his criticism. And if this can be done as a prank, it certainly can be done as art.

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[22] The idea of someone trying to do one thing and end up with the exact opposite is presented in chapter one, where Leggett spent a lot of his career trying to show limitations in quantum theory but, instead, demonstrated that it had an even larger domain of application than it was shown before.

[23] Just to be clear, I do not claim that what Sokal has done has the same importance of what Duchamp did. Duchamp changed the art in the twentieth century with his criticism, while Sokal only ended up writing a very naive book. He also became strongly attached to the science wars, but much like Archduke Franz Ferdinand, everything was bound to happen anyway. For the book, see: Sokal, A.; Bricmont, J. (2003). *Intellectual Impostures: Postmodern philosophers' abuse of science*. London: Profile Books.

The Bogdanov affair is a few steps further than Sokal's. While Sokal was eager to show what he did, the reason behind this affair remains undisclosed. They, the twin brothers Igor and Grichka Bogdanov, in fact were trained in the scientific field they wrote papers, having awarded their Ph.D. in physics and mathematics with works that remain controversial from the University of Burgundy. The articles, from their dissertations, were published in peer reviewed scientific journals, including the traditional journals *Annals of Physics* and *Nuovo Cimento*.

From the article in *Annals of Physics*, it is possible to read that:

> “In such a context, the KMS state of the (pre)spacetime may be considered as a transition phase from the *Euclidean topological phase* (**β**=0) to the *Lorentzian physical phase*, beyond the Planck scale”.

From Grichka Ph.D. dissertation:

> “We demonstrate that the lorentzian signature of the space-time metric (+++–) is not fixed at the Planck scale and shows 'quantum fluctuation' between the lorentzian and euclidean (+++±) forms until the 0 scale where it becomes euclidean (++++).”

And from the article in Czechoslovak Journal of Physics:

> “We draw from the above that whatever the orientation, the plane of oscillation of Foucault's pendulum is necessarily aligned with the initial

> singularity marking the origin of physical space S3, that of Euclidean space E4 (described by the family of instantons Ibeta of whatever radius beta), and, finally, that of Lorentzian space-time M4."[24]

The main thing to show with these citations is that it is extremely hard to point out whether or not those make any sense or not. They do share a grammar and vocabulary with the most legitimate works of the field and, while difficult to evaluate if one has formal training in physics and mathematics, it is nearly impossible without this background. Even those with speciality in the very narrow and advanced field that they talk about have not reached an agreement, when they were published, about the nature of those works: if they are real and an example of low quality

[24] While none of the quotes has any meaning, it is interesting to understand better what is said in the last one. John Baez, professor of mathematics at the University of California, Riverside, and a specialist in topology and quantum gravity, explains that what they propose is that the direction in which Foucault's Pendulum is oscillating is aligned to the place where the big bang happened. Yet, the big bang did not happen in any place as the big bang created space and time itself. So, if we are to point at the big bang, it's everywhere as space and time are still today in expansion. Baez was actually involved in the affair as he was one of the first physicists to point out that their works had no meaning. He also makes the best description of the whole affair to date. For his comments, see Baez, J. (2010). *The Bogdanoff Affair*. Available at https://math.ucr.edu/home/baez/bogdanov.html See also how *Nature* covered the affair: Butler, D. (2002). Theses spark twin dilemma for physicists. *Nature* 420, 5. The first quote is from Bogdanoff, G. and Bogdanoff, I. (2002). Spacetime Metric and the KMS Condition at the Planck Scale, Annals of Physics, 296, p. 90-97. The second is from the nature article and the third from Baez's essay. See also the discussion in Glattfelder, J. (2019). Philosophy and Science: What Can I Know?. In: *Information—Consciousness—Reality*. Berlim: Springer.

production, just like so many others, if they are real and a nice contribution to the field or if they are, like Sokal's, a hoax[25].

"The strangely moralizing quality of the hoaxer reveals a latent teacher at every turn – as well as an artist[26]", so we begin to see how there are so many ways of making art through science, literally through science, in such ways that they may even become indistinguishable from each other in those very specific cases. The full amount of possibilities that the artistic fields allows one to create art together with how contemporary science validates its works through social dynamics is what supports this possibility. In cinema and video-games, for instance, there is a choice in their production, in their objectives, if they are to become wider audience products, mainly to achieve money, or if they will be part of the artistic production. Sometimes, they can even achieve both[27]. Scientists, then, should begin paying more attention to those possibilities, since they are already quite present, while artists need to, *in fact*, examine new dimensions for the artistic world.

[25] But, since, it has become clear that their work has no sense at all, just like Sokal's, full of jargon and without sense. The question of the hoax still remains.

[26] Fleming, C. and O'Carroll, J. (2010) The Art of the Hoax, Parallax, 16(4),p. 45-59, p.46.

[27] Such as Super Mario Bros. and several of Woody Allen's films that go way beyond the cult cinema circuit.

Consequences - Pierre Bourdieu had already pointed out the similarity of both fields. In his sociology, centered on the symbolic economy of prestige, he developed a system that identifies a dynamic that is almost identical in all intellectual fields, with the details of the dynamics changing, but not the general framework, so paving an easy transition from his studies on art to his studies on science[28].

If he was more concerned with the general dynamics, we are, here, more concerned with the tiny similarities (and tiny differences) of the fields. On the points in which the social system of both fields work together, but not only those. There are also dynamics that are bound to happen inside science that are typically found in art and vice-versa, despite their different natures in principle. And while this article is focused on the proximity of art and science, this is valid in a very broad sense. As both are cultural products of specific societies, they share a common ground that generates, simultaneously, identical and different

[28] His main work on art is "The rules of Art", and several of his essays on the subject are organized on the volume "The field of cultural production". On science, his production is more concise. The first and most commonly used work is "The specificity of the scientific field and the social conditions of the progress of reason", and, more recently, his last course at the *École Normale Supérieure* that was published in English as "Science of Science and Reflexivity". Bourdieu, P. (1995). *The Rules of Art*. Stanford: Stanford University Press; (1993). *The Field of Cultural Production*. New York: Columbia University Press; (1975). The specificity of the scientific field and the social conditions of the progress of reason. *Social Science Information*, 14(6), p. 19-47; and (2004). *Science of Science and Reflexivity*. Chicago: Chicago University Press.

practices with all the parts of this very specific society. So, the general lesson is that the tools that we need to understand each field, each dynamic, each single case, may be located in places that we usually do not look for. In another work, we showed how style could easily be borrowed from art to science in order to explain certain dynamics. In a work in progress, we focus on how propaganda, in the same political sense that is usually applied, also do apply in science[29]. Even if we do understand how the objects in which those concepts apply can be so extremely different, they also retain a type of identity that allows for this kind of fluidity.

This kind of *ressemblance*, which bears on identifying a common dynamic for different aspects of our society is not a new proposal also. When anthropologists stopped only going to distant societies and started studying their own places of origin, in what would be known as urban anthropology, they employed the very same idea that the tools used for other societies could be applied in other contexts as well, such as Gilberto Velho's ethnography of the "Edifício Estrela" in Copacabana,

[29] This is, in fact, extremely similar to how Paul Feyerabend used it in Against Method, despite the fact that he did not try to generalize it, and to Olival Freire's use of dissidence to explain the field of foundations of quantum mechanics. See Feyerabend, P. (1988). *Against Method (3rd. ed.)*, New York: Verso. and Freire Jr., O. (2015). *The quantum dissidents: Rebuilding the Foundations of Quantum Mechanics (1950-1990)*. Berlin: Springer.

Rio de Janeiro, or Roberto da Matta's work on carnival in the same city[30]. This, in some sense, could also be similar to what Bruno Latour (with Steve Woolgar) did in his study in anthropology of science at the Salk Institute, but him, instead of understanding science as another cultural dimension of a specific society, he approached it as a exotic product, just as exotic as any distant civilization[31]:

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[30] Velho, G. (1975). *Utopia Urbana*. Rio de Janeiro: Jorge Zahar; DaMatta, R. (1991). *Carnivals, Rogues, and Heroes: An Interpretation of the Brazilian Dilemma*. Notre Dame: University of Notre Dame Press.

[31] Despite our description, they would not agree with it: "Thirdly, our use of 'anthropology' denotes the importance of bracketing our familiarity with the object of our study. By this we mean that we regard it as instructive to apprehend as strange those aspects of scientific activity which are readily taken for granted. It is evident that the uncritical acceptance of the concepts and terminology used by some scientists has had the effect of enhancing rather than reducing the mystery which surrounds the doing of science. Paradoxically, our utilisation of the notion of anthropological strangeness is intended to dissolve rather than reaffirm the exoticism with which science is sometimes associated. This approach, together with our desire to avoid adopting the distinction between 'technical' and 'social,' leads us to what might be regarded as a particularly irreverent approach to the analysis of science. We take the apparent superiority of the members of our laboratory in technical matters to be insignificant, in the sense that we do not regard prior cognition (or in the case of an ex-participant, prior socialisation) as a necessary prerequisite for understanding scientists' work. This is similar to an anthropologist's refusal to bow before the knowledge of a primitive sorcerer. For us, the dangers of 'going native' outweigh the possible advantages of ease of access and rapid establishment of rapport with participants. Scientists in our laboratory constitute a tribe whose daily manipulation and production of objects is in danger of being misunderstood, if accorded the high status with which its outputs are sometimes greeted by the outside world." We keep our position as the sense in which they use "exotic" here is in opposition to what can be understood, intelligible. It is the same sense in which anthropology used to explain foreign "tribes" in opposition to the scientific fairs that portrayed them as exotic animals from distant lands.

> "We envisaged a research procedure analogous with that of an intrepid explorer of the Ivory Coast, who, having studied the belief system or material production of "savage minds" by living with tribesmen, sharing their hardships and almost becoming one of them, eventually returns with a body of observations"[32]

Anthropology, in fact, showed them to be non-exotic, but at the price of remaining still *exotic*, different. Science, even if still including Latour himself, was still exotic to the rest of the society, not a cultural product similar to religion, for instance. If the following quote seems to question this description: "Our particular use of an anthropological perspective on science also entails a degree of reflexivity not normally evident in many studies of science. By reflexivity we mean to refer to the realisation that observers of scientific activity are engaged in methods which are essentially similar to those of the practitioners which they study", on the postscript, about 7 years after the first edition, they write: "Professor Latour's knowledge of science was non-existent; his mastery of English was very poor; and he was completely unaware of the existence of the social studies of science. Apart from (or perhaps even because of) this last feature, he was thus in the classic position of the ethnographer sent to a completely foreign environment." Either he was performing a reflexive study, understanding that his own methods were, somehow, similar to the ones of the group he was studying or he knew nothing about science when the study began. You can't have both at the same time. And, again, they refer to science as a "completely foreign environment", which we call exotic.

It should be emphasized that Latour radically changed this view and approach in his later works. Latour, B. and Woolgar, S. (1986). *Laboratory Life: The construction of scientific facts*. Princeton: Princeton University Press. For the above, see pages 29, 30 and 273.

[32] This may explain why his works, despite being very successful, were much more appreciated and discussed in the Science Studies, which lacked this kind of research, than in the field of anthropology itself. " Moreover, in the corridors of anthropology departments we've registered an overall curiosity as to what 'this Latour character is all about'. Yet, for all of this interest, he is certainly not an obligatory reference in anthropology (in comparison, for instance, Bourdieu has reached the status of what might well be called a hegemonic figure)." Berliner, D., Legrain, L., & van de Port, M.

This is the exact opposite of what Velho and da Matta did. Velho, while a master student, performed ethnography in the building that he was living with his wife, and da Matta, after a successful career with indigenous societies on the north of the country, switched to study the same Carnival that he experienced since he was a child, so nothing could be less exotic than both endeavours.

While different, neither science nor art can be thought of as exotic. In some senses, they are, indeed, unique, but in so many ways they can be so close to each other (and, generally, to all society), that it shouldn't be strange to use concepts of one field on another or even study both together, as close phenomena with some similar characteristics[33].

(2013). Bruno Latour and the anthropology of the moderns. *Social Anthropology*, 21(4), 435–447.

[33] One more time, this idea is certainly not strange to the program proposed by Bourdieu, as he proposes a structure that allows one to study it all under the same theoretical approach. But this is not unique to him. Anthropological studies in general do connect quite well, even if we do not agree with their conclusions, several aspects of society that, at first, appear to be different. In the classical study on Balinese's cockfight, Clifford Geertz proposes to understand their society through the thick description of the cockfights. da Matta, in a similar sense, tries to explain aspects of Brazillian society through the dynamics of the Carnival. Geertz, C. (1973). Notes on the Balinese Cockfight *in The Interpretation of Cultures*. New York: Basic Books.

The Bohr and Everrett-Wheeler dialogue

The Bohr and Wheeler-Everett dialogue on the foundations of quantum mechanics and power relations in science[1]

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 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 20th 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 a 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 of social relations within science during the 20th century.

[1] This text was published with Olival Freire Jr. and Iolanda Faria, available at https://doi.org/10.24117/2526-2270.2020.i8.04

Quantum mechanics was established in 1925–1927, but physicists never reached an agreement about the meaning of its formalism and about 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, being 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 research. 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 the 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 acceptation 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.[2]

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

[2] 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).

summarize the historical events that will be analyzed in the following section. In section 4, we re-read Everett's case in the 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, p. 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, inseparably, political conflicts: so that a survey on power in the scientific field could perfectly well consist of apparently epistemological questions alone.* (Bourdieu, 1975, p. 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, p.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, p. 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, p. 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 realize that Bohm's interpretation represented an even greater challenge to dominant positions in the field of physics—the challenge to 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.[3]

[3] 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)

Case study: a summary of the Everet-Wheeler-Bohr dialogue[4]

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, Willian Boone, for his doctorate selection. Boone depicted Everett as a true genius and added: "This is once-in-a-lifetime recommendation for I think it is most unlikely that I shall ever again encouter a student I can give such complete and unreserved support."[5] 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 reading the letter of recommendation is that Everett was a brilliant student. Due to this academic record, Everett received a National Science Foundation graduate scholarship[6]. Even though it did not grant him a free pass into the graduate program entrance exams, it certainly helped his admission. The scholarship, in

[4] 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).

[5] Letter from William Boone to Hugh Taylor, Dean of Graduate Studies, April 17, 1953. Alumni File of Hugh Everett III, Seeley G. Mud Manuscript Library, Princeton.

[6] *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.

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 attractive 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 in 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 advising another student on the foundations of Quantum Theory.

In 1954, Everett's ideas about Quantum Theory were not fully developed yet; that 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 in a less technical way, 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."[7] 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

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[7] "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.

1913 atomic model. After 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, that 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 reasoning, 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 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 to 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 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 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 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 it 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.”[8] 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

[8] Wheeler letter to A. G. Shenstone, May 28, 1956. Wheeler Papers, Box Di, series # 2, American Philosophical Society, Philadelphia, PA.

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 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 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.[9] He also received a job offer from Johns Hopkins University, also with a 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.

[9] In the American academic system, *tenure* is equivalent to the stability of the employment contract in the institution.

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 in 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 of 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[10].

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 in addition 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

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

have the opportunity to make the decision himself.[11] 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.[12] 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

[11] 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.

[12] 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.

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 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.[13] What was considered as a

[13] 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.

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:[14]

> *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*

[14] *Still more quantum mechanics.*: 1971, *Physics Today*, 24 (10), p.11. [Letters by G.L. Trigg, M. Hammerton, R. Hobart Ellis Jr., R. Goldston, H. Schmidt].

> *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.*

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 out 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 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, despite the fact that 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 of 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.[15] 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.

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 monocracy.

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

[15] 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.

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 a classification as strictly externalist does not allows 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.[16]

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 a careful dissection of the internal dynamics of the scientific dispute. The 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. An 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

[16] 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.

(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 scientific field's analysis.

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