Truth by fiat: the Copenhagen Interpretation of Quantum Mechanics

Verdade por Decreto: a Interpretação de Copenhagen da Mecânica Quântica

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ABSTRACT The purpose is, first, to review the occurrences which led to the acceptance by the bulk of physicists of the instrumentalist approach to quantum mechanics (QM) as the only one worth considering as truly rendering nature at the atomic level. Formulated soon after the creation of QM it outright refused the local realist interpretation. Next, to bring out the successive waves of dissenting voices, which still go on unabated. Finally, to recall that – in spite of the pervading dominance of the Copenhagen reading of quantum *phenomena* – the manifold questions concerning the interpretation of QM continue to be the subject-matter of forceful interventions claiming that the foundational basis of QM is far from being definitely closed.

Keywords foundations of Quantum Mechanics – Copenhagen Interpretation of Quantum Mechanics – rhetoric of inevitability – Copenhagen-Göttingen monocracy – Marxist determinism and sociology of Physics.

RESUMO *O objetivo é, em primeiro lugar, analisar as ocorrências que levaram à aceitação, pela maioria dos físicos, da abordagem instrumental para a mecânica quântica (MQ) como a única a considerar como verdadeiramente adequada à representação da natureza ao nível atômico. Formulada logo após a criação do MQ, ela recusou completamente a interpretação realista local. Em seguida, realçar as sucessivas vozes discordantes, que continuam a fazer-se ouvir. Finalmente, lembrar que – apesar do quase total domínio da interpretação de Copenhagen – as múltiplas questões envolvendo a interpretação da MQ continuam a ser o assunto de enérgicas intervenções no sentido de que os fundamentos da MQ ainda não estão definitivamente estabelecidos.*

Palavras-chave *fundamentos da Mecânica Quântica – interpretação de Copenhagen da Mecânica Quântica – retórica da inevitabilidade – monocracia de Copenhagen-Göttingen – determinismo marxista e sociologia da Física.*

The Copenhagen canon

The point of view sustained nowadays by the vast majority of physicists on most basic issues related with the interpretation of quantum mechanics (QM) was essentially brought up by Niels Bohr and Werner Heisenberg at the V Congress of Solvay, held in Brussels in October of 1927. Having rapidly been accepted by almost all of its attendees, a depiction of the quantum theory as the new entrancing paradigm – which came to be known as the *Copenhagen Interpretation* – came out at the end of that meeting. Composed of several key elements – quantum jumps, impossibility of space-time causality, indeterminism, complementarity between the particle and wave descriptions, indispensability of classical concepts – the Copenhagen philosophy – with this general acceptance, which went far beyond the strict confines of physics, spreading out into areas like biology and even into domains as far removed as anthropology and psychology – soon ascended to the category of statute-law, endowed with an aura of almost self-evident inevitability.

Ever since the combination of the impressive empirical success of QM with the Copenhagen interpretation, the prevailing position among physicists has purposely (and conveniently) ignored the obnoxious subjacent core questions having to do with the foundations of the theory, considering that the criticisms (particularly those issued out by Einstein) were hopelessly naive at best and sadly mistaken at worst. Willingly admitting that either all matters connected with the foundations of the theory had been, from the outset, unmistakably set to rest by Bohr and his followers, or that – if there remained any (in any case, minor) open points – they definitely didn't belong to hard physics, but, rather, within the confines of metaphysics, the overwhelming majority of physicists have been, from the beginning, solely engaged in applying QM to concrete questions.

Illustrative of the widespread, almost universal consensus attained by the Copenhagen approach to QM is the following episode, reported by J. Bernstein:

> *I once saw Oppenheimer reduce a young physicist nearly to tears by telling him a talk he was delivering on the quantum theory of measurement at the Institute was of no interest, since all the problems had been solved by Bohr and his associates two decades earlier.*[1]

Dissenting voices have not been entirely silenced, however, and authoritative diverging positions have continued sprouting ever since, as exemplified by Murray Gell-Mann lamenting that "the fact that an adequate philosophical presentation [of quantum mechanics] has been so long delayed is no doubt caused by the fact that Niels Bohr brainwashed a whole generation of theorists into thinking that the job was done fifty years ago."[2]

Actually, this ever simplistic version – according to which all the foundational points of QM had been adequately and definitely addressed by Bohr at the V Congress of Solvay – does not fit together with what effectively happened there. As a matter of fact, three of its most prominent participants – Einstein, Schrödinger and de Broglie – remained forever utterly convinced that the outlook proposed by Bohr was wide off the mark of presenting an adequate (and much less definitive) representation of quantum *phenomena*: Einstein never accepted the completeness of the formulation coming out from the Copenhagen-Göttingen axis,[3] and, eight years later, would fire off an attack, known as the EPR argument,[4] which, notwithstanding Bohr's prompt attempts to neutralize it, continues to be argued and commented about ever since: Schrödinger maintained his unwavering belief in a realistic interpretation of his wave-mechanics;[5] de Broglie, after the 1927 Congress of Solvay has abandoned his pilote-wave theory[6] (a simplified version of his early theory of the double solution) converted himself to Bohr's views; however, he went back to his theory of the double solution[7] once David Bohm gave it quite a positive boost with his two introductory articles on hidden variables.[8]

Spreading the faith

After 1927, the *Kopenhagener Geist der Quantentheorie* spread rapidly throughout the international physics community, very soon becoming the official, the authorized, and, in fact, the only acceptable interpretation of QM, unquestionably (and uncritically) endorsed by the overpowering majority of physicists. However – taking into account the diminutive number of its head proponents and, in face of the undisguised dislike, or better, unequivocal disavowal, openly demonstrated by its weighty opponents –, how can such a prompt, such unreserved and generalized acceptance be understood? Here we offer just a first approach to answer this question.

First and foremost, the actual way in which the campaign was fielded and unfurled under Bohr's attentive and relentless guidance counted far more than his undeniable personal influence and authority,[9] the effects of which were definitely amplified by the existence of his institute in Copenhagen. Inaugurated on 3 March 1921, Bohr's *Institut for Teoretisk Fysik*, founded and directed by him, soon became the greatest attractor of young talent, rapidly managing to gather within its walls eager junior theoretical physicists, not only from Europe, but from the USA and Asia as well, many (if not all) of whom were determined to secure a place among the best of their profession.[10] During the third and fourth decades of the last century, Bohr's institution, – being then the acknowledged world's premier site for quantum physics –, effectively served as a springboard for the diffusion, not only of the technical theoretical aspects being developed there, but also of the quantum doctrine as it was fundamentally envisaged by Bohr.

Secondly, closely following the creation of Heisenberg's matrix-mechanics (1925) and of Schrödinger's wave-mechanics (1926), several key collaborators, allies and followers of Bohr's took hold of prime academic positions in Central Europe, which, as a consequence, turned into proficient diffusion centers of the Copenhagen philosophy. Thus, besides Max Born, who was already in charge in Göttingen, that was the case of Heisenberg, who went to the University of Leipzig as director of its institute of theoretical physics; of Wolfgang Pauli, who moved to Zurich (being replaced in Hamburg by Pascual Jordan); of Hendrik Kramers, who left Copenhagen (where he had acted as Bohr's assistant) on being nominated to the University of Utrecht. To and from the Danish capital, as well as to and from all these other centers, flocked the eager catechumen, ready to be first indoctrinated and then appointed as zealous ushers in charge of spreading the Copenhagen philosophy throughout the world. Thus, from Europe, we find, among many others, the names of Felix Bloch, Hendrik Casimir, Charles Darwin, Max Delbrück, Paul Dirac, Ralph Fowler, Oskar Klein, Nevill Mott, Rudolf Peierls, León Rosenfeld, Edward Teller, Viktor Weisskopf and Carl von Weizsäcker; from the Soviet Union, we recall George Gamow, Peter Kapitza and Lev Landau; from the United States, Robert Oppenheimer, John Slater and Richard Tolman; from Japan, Yoshio Nishina.

Thirdly, in spite of the conceptual differences that certainly existed among the three chief promoters of the Copenhagen line – Bohr, Heisenberg and Pauli–,[11] all of them were united together in the defense of its core content, anchored on the – perhaps (at least for some) mystifying concept of complementarity.[12]

Bohr's undeniable prestige and practically unquestioned authority – particularly among those he made sure of repeatedly attracting to his institute, many of whom looked up to him with sincere and unreserved reverence – made the uncritical approval of the quantum philosophy, of which he was the chief proponent, instigator and mentor, a settled question almost from the earliest days of QM. Suffice, for instance, the unsuspected testimony of Rosenfeld, surely one of Bohr's closest and more faithful supporters: "Bohr declared, with intense conviction, that he saw the day when complementarity would be taught in the schools and become part of general education; and better than any religion, he added, a sense of complementarity would afford people the guidance they needed".[13] No wonder, then, that Einstein referred to the "prophet" Bohr and to the religious mysticism of his doctrine of complementarity. In a letter to Schrödinger of 31 May 1928, Einstein openly gives vent to his feelings: "The Heisenberg-Bohr tranquilizing philosophy – or religion? – is so delicately contrived that, for the time being, it provides a gentle pillow for the true believer from which he cannot very easily be aroused. So let him lie there."[14]

The Copenhagen ideology was thence quickly brought up to the status of the only *bona fide* canon, and, as such, suffering badly from any contradiction, even if given out by the likes of Einstein, Schrödinger or de Broglie.

The determined strategy of the Copenhagen faithful – with their insistence that their interpretation of QM more than being objective, coherent and most natural, was also the only one physically adequate – is what Mara Beller calls the "rhetoric of inevitability," by which consistency passes for inevitability:

(…) [t]he founders and followers of the Copenhagen interpretation advocated their philosophy of physics not only as a possible interpretation but as the only feasible one. Attempts at basically different approaches, albeit by such prominent scientists as Einstein, Schrödinger, Landé, and Bohm, were dismissed and ridiculed.[15]

In almost every field of human activity, the rule most commonly and easily followed by its practioners – once a general blueprint for its adequate functioning is declared as concluded by the acknowledged establishment – consists of proceeding with one's perceived job without unduly hesitations or digressions. In this manner, the main thought entertained by the eager generation of young physicists entering the field in the first half of the 1930s was to acquire as much and as fast as was at all feasible the technical expertise necessary to apply the new quantum theory to the countless natural *phenomena* at their disposal; the fruits of such capability being the indispensable publications strictly required for forging out a proper academic career.

Notwithstanding the pre-eminence of the few leading contenders in the first debates over the epistemological and ontological set up of quantum theory – all of whom pertaining to the restrict group of the prime creators of the new microphysics –, the generality of physicists kept largely aloof from the whole affair. Something that is easily understood considering that the strenuous perusal of philosophical matters is inimical to the production of expedite quantitative results related with concrete physical systems. Actually, until the appearance of John Bell's mathematical results in the 1960s, the consensual attitude among physicists was that any questions concerning interpretations aiming at bringing about the meaning of QM had nothing to do with hard physical issues, hinging instead on ontological aspects (e.g., matter as waves and/or particles) and on epistemological points (e.g., the statute of indeterminism or incompleteness). As it has been proclaimed: "The controversy has finally reached a point where it can no longer be decided by any further experimental observations; it henceforth belongs to the philosophy of science rather than to the domain of physical science proper."[16]

With the rather sudden ascendance of the United States to the status of top global superpower, its science (obviously an all important contributing factor for that) most naturally acquired an equally domineering standing. American physics – which had undergone an unprecedented growth between the two world wars, benefiting in great measure from the influx of several of the best European scientists, and from the enormous impulse it got during and after the second of those wars – followed a pragmatic line, consonant with the general outlook of the society to which it was linked up, valuing more applications related with technological innovations than arcane ponderings about fundamental issues. Illustrative of this are the words of Willis Lamb, who, in 1969, after having received the Nobel Prize in physics, proclaimed that

I have taught graduate courses in quantum mechanics for over 20 years at Columbia, Stanford, Oxford and Yale, and for almost all of them have dealt with measurement in the following manner. On beginning the lectures I told the students `You must first learn the rules of calculation in quantum mechanics, and then I will tell you about the theory of measurement and discuss the meaning of the subject.' Almost invariably, the time allotted to the course ran out before I had to fulfill my promise.[17]

New dissenting voices

The forceful message issuing from all the leading centers of quantum physics during the 1930s was wont to proclaiming in no uncertain terms that the physical conceptual foundations of QM – as rendered by the formulation of Copenhagen – were not only totally exempt from all interpretative doubts, but that they had attained from the outset the status of a logically consistent complete structure; the only legitimate remaining task, therefore, consisted of applying this canon to the myriad concrete cases in waiting. Philosophers – if so disposed – were entitled to peruse over metaphysical points, bearing in mind, however, that there was absolutely nothing lacking or amiss as far as the physics of the theory´s foundations were concerned. One consequence of this being that all young (or even not so young) physicists willing or merely inclined to peering over the conceptual basis of QM were frowned upon by their peers and their superiors, thereby being often constrained to forgo a career in physics and pursue instead one in philosophy or history of science. The end result of all this, within the ranks of international physics, is the overall consensus about which 'philosophers speculate, physicists calculate.'

In spite of that environment, largely hostile to any further analysis of the conceptual basis of QM, dissenting voices to the Copenhagen paradigm kept being heard. The early unyielding refusal by Einstein and a few others to endorsing the official doctrine, – so hastily put in place as the only one admissible, worth accepting, or even deserving considering, – propitiated the later sprouting of scores of further dissenters. In the late 1940s and early 1950s, Soviet physicists and philosophers directed strong criticisms against Bohr's epistemology, based on the concept of complementarity, blaming that concept of introducing idealism in physics and of going against the dialectical-materialism. A serious challenge to the orthodoxy was raised in 1952 by David Bohm, who presented a brand new ontological interpretation of QM, built over the concept of hidden variables.[18] Then, five years later, Hugh Everett published his theory of the relative state (also known as the theory of many-worlds), reclaiming – albeit in a rather different sense – Schrödinger's original idea that the wave function encompasses a reality of its own. Next, came Wigner's reanalysis, in the early sixties, of the whole measuring process with his contentious inclusion of human conscience. And, encouraged by Bohm's results, John Bell initiated his seminal work with two articles, the first in 1964[19] and the second in 1966,[20] where – besides criticizing Bohr's and von Newmann's positions on the measurement process – namely the latter's "proof" that dispersion free states, and hence hidden variables, are impossible –, he reexamined the possibility of including hidden variables in QM, reviving the debate about locality in quantum physics, a subject already addressed long before by Einstein at the V Congress of Solvay in 1927 and to which he returned to eight years later with his influential and unsettling EPR article.

The expression "interpretation of Copenhagen" seems to have been coined by Heisenberg in his contribution to the commemorative volume celebrating Bohr's 70th birthday in 1955,[21] apparently in an attempt to make believable a single and unified version of QM.[22] However, the expression was immediately criticized by Léon Rosenfeld, one of Bohr's most heedful partisans, as he felt it could give occasion to the possibility of other interpretations of QM, different from the one constructed by his correligionaries. In fact, what was commonly held was the completeness of QM, together with the widespread sharing of the so-called "Spirit of Copenhagen", essentially comprising the concepts of indeterminism, complementarity and the full wave function description of single systems, but, at the same time, with many people disagreeing with several tenets of the official view such as – besides its overall subjectivism –, the collapse of the wave function, or the observer's privileged role.

Indeed, far from being the only existing understanding of QM, the Copenhagen vision has had, from the very beginning, the company of an ever-increasing number of adversary competing views: theories of hidden variables; theory of many-worlds; dynamic models of spontaneous collapse of the wave function; consistent or decoherent stories; interpretations in which the wave function describes only the knowledge or the information one may have of the state's system, but not the system's own intrinsic reality; stochastic interpretations; model of the quantum state diffusion; model based on multiple Hilbert spaces; models of the wave function collapse based upon irreversibility; gravitationally induced collapse of the wave function.[23]

In addition to all this opposition – and in parallel with it –, there continues to be investigation adhering to the Copenhagen line on the possibility of an operationalistic interpretation of QM, built upon a new axiomatic for the theory's structure.[24]

Even if the great majority of physicists persist in not taking too much interest in questioning the established orthodoxy, this wealth of differing approaches – besides being a clear indication that there is more interpretative freedom nowadays, at least among those disposed with a philosophical persuasion –, points to the increasing relevance that the questioning of the foundations of QM has acquired over the years.

The canon of Copenhagen and its heralds

The first announcement of the complementarity ideology – tying it in with Heisenberg's uncertainty relations –, was made by Bohr at the Como Conference of September 1927 and published next year in the proceedings of the V Congress of Solvay. This text – which can be regarded as an authentic manifesto of the Copenhagen's canon –, soon enough commandeered the manner by which physicists viewed quantum theory. In any case, the overwhelming majority of them did not unduly encumbered themselves with any of the theory's subtleties, focused as most of them were (and still are) in extracting tangible results from applying the theory to specific concrete situations. Under Bohr's guidance – and with a small coterie of paladins composed of Heisenberg, Pauli, Born, Jordan, Kramers and Rosenfeld in tandem –, the Copenhagen doctrine of QM, impervious to the disapproval issued by a small bunch of obnoxious die-hards, rapidly spread out, easily imposing itself as "an epistemology of universal validity, a dialectic sharp enough to cut conundrums about the nature of life and the freedom of the will."[25]

The central point of Bohr's talk at Como had to do with his belief that the mathematical formalism of QM authorized only "complementary" descriptions of mutually exclusive classical concepts for the same quantum object (an electron or a photon, say). Explicit examples of empirically mutually exclusive pairs-equivalent to the wave/particle classical concepts being superposition/individuality and causality/space-time. "This romantic and even mystical manner of speaking"[26] was Bohr's way of tackling the intricacies of the interaction between light and matter. Thus, for example, the wave representation allowing the propagation of light throughout space-time to be followed is thoroughly incapable of rendering a description of the energy (or momentum) transfer during a quantum collision (between, say, a photon and an atom). Therefore, since the depiction of both the experimental set up and the respective measurements will always have to be represented in terms of classical concepts, a complete description of a quantum event without recourse to complementary descriptions will never be possible. In other words: The total relinquishment of any description of the quantum domain which does not take into account the complementarity postulate is utterly commanded. In this fashion, the grounds for the introduction in physics of a "doctrine of renunciation" are installed – with "renunciation" becoming a trade term of the school of Copenhagen.[27] One just has to listen to Heisenberg:

> *For the atom of modern physics all qualities are derivative; in general it has no material characteristics... As Bohr has emphasized, it is no longer right to assert: the qualities of a body are reduced to geometry. On the contrary: knowledge of the color of a body only becomes possible by renouncing knowledge of the atomic and electronic motions in the body, while knowledge of electronic motions in turn forces renunciation of knowledge of color, energy, and temperature.*[28]

The idea of renunciation would take several different forms throughout the years, one of which being in terms of some of the so-called "theorems of impossibility", such as the theorem of von Neumann, which attempted to show the impossibility of building alternative interpretations, based on hidden variables theories, to the Copenhagen interpretation of QM. That is, the idea that complementarity already encompasses the seeds of the subsequent developments brought out by the "proofs of impossibility".

The promulgation of the Copenhagen philosophy, officialised from the V Congress of Solvay on – unable as it was to prevail upon the uncompromising stand of the realists' stalwarts, such as Lorentz, Planck, Einstein, von Laue, Schrödinger, Landé and de Broglie (ordered by age) –, forced Bohr's partisans to take up an active and determined stance with a view to promoting the dissemination of the recently established creed, simultaneously with paring the way for its acceptance.

A most relevant contribution for this was, undoubtedly, Heisenberg's series of lectures on QM, proffered by him at the University of Chicago in 1929, and published the following year under the title of *Physikalische Prinzipien der Quantentheorie*.[29] At the end of the preface, Heisenberg had no qualms in declaring that his book's goal would be attained if it could contribute for the diffusion of the *Kopenhagener Geist der Quantumtheorie*, which, according to him, oriented the entire development of modern atomic physics. Translated into several languages and having seen several re-editions in Europe and in the United States, the book was definitely a major boost in speeding-up the diffusion of the Copenhagen doctrine.

Bohr's most significant contribution in that matter consisted of the publication of his Como talk (which had effectively initiated the whole affair), and of three of his essays, collected in the small book *Atomic theory and the description of nature,* published in Danish (1929), German (1931), French (1932) and English (1934), and which he personally sent to chosen individuals from all over the world. Also relevant are the two additional epistemological Bohr's books, both collecting seven essays: *Atomic physics and human knowledge* – published in 1958, where he develops his views on QM and its applications, being one of which the Discussion with Einstein on epistemological problems in atomic physics –, and *Essays 1958-1962 on atomic physics and human knowledge*, published in 1963.

An unexpected bombshell

In view of this picture of utmost generalized approval of the prevailing standpoint – so efficiently promoted as an almost revealed truth –, it is not surprising that the 1935 article by Einstein, Podolsky and Rosen, generally known as the EPR paper or argument,[30] was received by Bohr and company as an entirely unexpected bombshell: "This onslaught came down upon us as a bolt from the blue. Its effect on Bohr was remarkable."[31] Dirac deemed it necessary to start all anew,[32] seeing that Einstein had sufficiently demonstrated a serious enough intrinsic failure of the theory as it stood. (This in spite of Einstein's avowed displeasure with the form – as given essentially by Podolsky – the published article had taken.) Pauli was simply furious.[33] Rosenfeld questioned who, besides Bohr himself, would be up to putting up a defense of the beleaguered bulwarks. Indeed, Einstein's alternative of "objective reality" brought forth by EPR – implying as it did a "counter-proposal to complementarity"[34] – was immediately understood as an enormously imposing threat to the recently entrenched Copenhagen way of thinking.

So it was that, upon having taken notice of Rosenfeld's report on the EPR, Bohr immediately set aside everything he had in hand in order to be able to devote his full attention not only to ward off Einstein's damaging blow, but – even more pressing – to attempt to neutralize its nefarious effects and consequences, with a view at making it look merely as a ineffective and innocuous off-target shot.[35] In fact, it had to be Bohr to come forward with an authoritative answer to the hard and deep difficulties raised by the EPR. And answer he promptly did, putting up an exhaustive defense of his complementarity principle and introducing the novel argument of the absolute inseparableness between the quantum system being observed and the respective measuring apparatus.

For the collectivity of physicists and, in particular, for its younger members (almost all of whom, almost certainly, never bothered to grasp either Einstein's reasoning or Bohr's counterarguments), Bohr's reply to the EPR was taken as a welcome affidavit granting one and all the right to assuredly engage, without any further qualms, the foreseen myriad of appetizing quantum riches in waiting. Moreover, very rare were those willing to devoting time and effort to the admittedly thankless job of painstakingly pondering over the subtleties of the argumentation of such acknowledged towering figures as Einstein and Bohr. On the one hand there was Bohr – the prescient champion of the future – forthcoming and benign, graciously opening up the alluring gates to the bountiful new territories of the quantum. On the other hand stood Einstein – the unequalled star of yore, yet now, well past his prime, the somewhat unexpected defender of the old ways –, increasingly seen by one and all as an obstinate, cranky old curmudgeon, repeatedly insisting in raising seemingly gratuitous stumbling blocks with his unduly and ungodly encumbrances. It was not hard to foretell who would come out on top.

Efficient strategies and tactics for winning over hearts and minds

An efficient and well-tested manner of having a particular point of view adopted by others consists in consistently caricaturing all contrary ways of thinking. These tactics which are likely to be quite effectual when applied to social and political issues –, ought not work so well in the case of the natural sciences, given the presumed objective and 'neutral' innate qualities of their subject matter. And yet, as Thomas Kuhn has reminded us,[36] science is a human concern and, as such, it is prone to the conditionings, limits and qualifications proper to man.

Besides, the heart core of the Copenhageners did not put below them belittling and even disparaging anyone willing to voice any opposing views, either totally ignoring them or, on the contrary, by openly branding them as worthless minor players, hopelessly unable to grasp the subtleness and delicacy of Bohr's reasoning.[37] And, there, precisely, lies the whole crux of the matter: How could a paramount, most cherished viewpoint of Bohr's – given his own weight and his ever vigilant, attentive and fiercely loyal Praetorian Guard – be ignored? A good example of this is Heisenberg not wasting any time in accusing Schrödinger in 1927 of not having even an elementary understanding of the dispersion mechanism of a wave packet, in spite of the latter's recognition, well before Heisenberg himself did, of its unavoidable dispersion.

The determined campaign launched in favor of the Copenhagen interpretation evolved in a two-pronged thrust: Together with promoting the revered Dane to the status of herald, highest patron and indisputable supreme master of quantum theory, classifying the bulk of their opponents as mere minor players; and, for those few who could not be lightly dismissed as irrelevant contenders, more carefully crafted methods were put in practice.[38] Thus, while for someone like Einstein or Schrödinger, the well-tried technique of cheapening their sophisticated and robust arguments was put in practice, for the likes of David Bohm or Alfred Landé, unmerciful demotion of their merits and expertise was reserved.

As for Landé, after having published his book *Quantum mechanics* in 1951 – in which he proposed to make clear the physical meaning of the theory, and in which he professed to be a follower of the Copenhagen School (though more in accordance with Heisenberg than with Bohr) –, he soon recanted his faith. Becoming one of the most outspoken critics of the orthodoxy, he attacked above all the dogmatic status it attributed to the wave-particle duality. Rosenfeld, always available and willing, promptly confronted Landé – in a unfavorable recension of his book *Foundations of quantum mechanics,* published in 1955 –, expressing extreme disapproval and stating characteristically that he was "making a muddle of a perfectly clear situation."[39]

Endeavoring to make sure that only very few would take the trouble of looking up the original works with their indispensable lines of argumentation, these simplifications, besides distorting, most times than not, the reasoning and the claims as they were in reality proffered, effectively hid the main body of their all important core points. The end result being, then, that it was far easier to counteract those misconstrued presentations: the rebuttal by the defenders of the orthodoxy was in fact not aimed directly against the proclaimed target, but towards a much more amenable, and, therefore, much more convenient version.

Such was indeed the case of Bohr's tactical answer to the EPR, in which he notoriously not only did not correctly discuss the actual argumentation as originally given,[40] but omitted also its central tenet, namely the incompleteness of QM,[41] addressing only its intrinsic non-locality. Another instance, was the 1953 controversy between Schrödinger and Born, in which the latter accused the former of trying to develop a wave ontology through the elimination of the concept of corpuscule of matter,[42] when, in fact, Schrödinger's article of the previous year[43] – explicitly heeding quantum jumps – did not discard quantum discontinuities, as the Copenhagen line misguidedly claimed, but sustained instead that the discontinuities could be formally deduced through a wave-theoretical scheme.[44]

Perhaps the most efficient strategy pursued in guaranteeing the lonely rule of the Copenhagen monocracy consisted in discrediting their opponents, classifying them as dogmatic conservatives, while at the same time positing themselves as intellectually revolutionary (and as such the only ones truly open to the uniquely exquisite intricacies of quantum theory). Thus, Heisenberg referring to Einstein's obdurate antagonism to the finality and unavoidability of the Copenhagen interpretation branded it as "dogmatic" or alternatively "metaphysical realism", in contrast to the "practical" realism adopted by him and his co-religionists. As if escape from metaphysics were indeed at all possible: "Of course, Einstein's philosophy of science was based on metaphysical presuppositions" – Max Jammer once made clear –, "just as was that of Heisenberg, of Bohr, or any other of the great masters in QM. For, as E. A. Butt once put it: '(...) the only way to avoid becoming a metaphysician is to say nothing'."[45]

The same Heisenberg who likewise assuredly dismissed in non uncertain terms all those unwilling to adhere to the imposed canon, choosing, instead, as David Bohm did, to advance an alternative that included hidden parameters:

When such strange hopes were expressed, Bohr used to say that they were similar in structure to the sentence: 'We may hope that it will later turn out that sometimes 2x2=5, for this would be of great advantage for our finances'.[46]

In fact, for Heisenberg, any approaches other than the one of which he was a major architect "are always distressing, for they mislead us into continually occupying ourselves with the inevitable cracks in the old bottles instead of rejoicing over the new wine."[47]

If one stops to consider it, it is quite striking the speedy and frictionless manner by which the physics collectivity willingly embraced and endorsed the view of the quantum established by the Copenhagen-Göttingen clan[48] and how far it was prepared to go to, not only in promoting it, but also in its promptness in fighting off all vestigial memories of any differing propositions. And yet, at the outset of QM, the radically revolutionary ways carried out by the new physics was far from being immediately grasped. Neither Heisenberg's inaugural paper of the Summer of 1925 – with the significantly bland and cautious title of Quantum theoretical reinterpretation of the kinematical and mechanical relations[49] – which gave birth to QM, nor the *Drei Männer Arbeit* by him, Max Born and Pascual Jordan,[50] which prompted followed it and where the matrix characteristics of the new scheme were made evident – seemed at first to entail what turned out to be eventually seen as arguably the most profound conceptual revolution ever undertaken in the whole history of physics. Rather, due to the , at the time, quite abstruse features of matrix mechanics, the three authors chose to emphasize the parallelisms of the new-born theory with well-established classical ideas, such as the similarity between the new quantum dynamical equations and the classical canonical ones, or the identical calculational methods used in perturbation theory.[51]

An unexpected and unwelcome new view of the quantum: enters Schrödinger

The possibility of drawing out any physical meaning out of the perceived resemblance between matrix mechanics and classical mechanics would be abruptly called in question, however, by the entirely unexpected (and, initially, quite unwelcome) arrival of Schrödinger's wave formulation in early 1926, seemingly much more in tune with classical conceptions. In response to this unforetold rival theory – understood at first as a threat to be, the sooner the better, if not neutralized, at least contained –, appearing as a continuous and causal formulation of quanta *phenomena*, prompted the defenders of Copenhagen-Göttingen approach to QM to fire off a barrage of innovative "revolutionary" ideas, aiming expressly at replacing these untimely and unseemly tenets of causality and continuity. (This being a compelling example of how the choice of certain elements of a theory over others may set off the emergence of a new conceptual paradigm.) In a 1963 interview with Thomas Kuhn, Heisenberg would describe his initial attitude towards Schrödinger's wave theory:

[T]he actual psychological situation for myself was that I felt, after we had written our Drei Männer Arbeit, that at that time, the mathematical scheme did make quite definite statements about how to calculate energy levels and to calculate amplitudes and intensities and so on. So I felt that from now on it is just a problem of working things out, if one wanted to get the correct interpretation also for collision problems or for whatever else is to be found in atomic physics. Therefore, when Schrödinger's things came out, I found it very interesting. (…) But I was so much afraid that by means of the Schrödinger mathematical scheme, a new interpretation of the thing would be brought in. Just because the interpretation was not perfectly clear at the time, I was very much afraid that now entirely wrong ideas could enter into the thing and actually have entered. Schrödinger, as you know, wanted to throw all the quantum jumps away and to say that there is no quantization, it's just all wave pictures and so on.[52]

In answer to Kuhn's question on whether quantum theory would have evolved along quite different lines if the equivalence between the matrix and wave formulations had but been shown only much later on, Heisenberg commented that that would have been possible for a while, but that – once people would have realized that the classical terminology of waves and particles should continue to be used, albeit with the added restriction of complementarity – the same conclusion would have been arrived at in the end. According to Heisenberg,

> *We didn't want to go back to the old line, and that was a disappointment with Schrödinger. ... I felt, "Now Schrödinger puts us back into a state of mind which we have already overcome, and which has certainly to be forgotten."*[53]

What is manifestly clear from these statements is that the arrival of Schrödinger's unwelcome formulation made imperative the urgent building up of a coherent, closed and complete (preferably definitive as well) interpretation of the ontological content of quantum theory, with the proviso that, above all, that most important task ought to come out exclusively from the very few practioners officiating along the Copenhagen-Göttingen philosophy. Indeed, springing forth from the only *bona-fide* source – the closed-knit community operating under Bohr's guidance, uniquely endowed with the seal of legitimacy – the proclaimed interpretation of all the fundamental issues of quantum theory raised to the acknowledged pressing need. And yet...

The marxist opposition to Copenhagen and hidden variables

After the V Solvay Congress of 1927, the Copenhagen school of quantum mechanics, based on the philosophy of complementarity, has became quite popular among the majority of physicists and remained almost unchallenged, during the decades of 1930 and of 1940 – despite of the unexpected bombshell caused by the EPR paper of 1935, already alluded above. However, the situation began to change in the early 1950s.

Although the majority of physicists keep themselves, up to today committed to the Copenhagen doctrine, the orthodoxy has never been fully immune either from strong criticisms or from the recurring appearance of other alternatives. In fact, the contestation has been on the increase (especially in the last quarter of a century), with the erosion of the monocracy originating in two major challenges, one external, during the 1950s, and the other, with more internal traits, during the following decade. As Jammer points out, during the early 1950s "the almost unchallenged monocracy of the Copenhagen school in the philosophy of quantum mechanics began to be disputed."[54]

First, the publishing in 1949 of Schilpp's volume,[55] dedicated to Einstein's 70th birthday, propitiated the resumption of the Einstein-Bohr debate of two decades earlier, this time, however, followed by a much larger and more diversified audience, making it then possible that Einstein's old misgivings (in particular about the complementarity principle and the completeness of QM)[56] acquired a renewed and amplified visibility. Also, in an extended series of seminars at Dublin's Institute for Advanced Studies, Schrödinger presented a new reformulation of his interpretation of QM.[57] Set on a realistic ontology of the wave function, it differed markedly from the one he had first introduced in his ground-breaking paper of 1926.

Secondly, several dedicated Copenhagener's – among them the Belgium physicist Léon Rosenfeld, a devoted Marxist, who had been for a while one of Bohr's assistants – had extended the applicability of complementarity to domains other than physics, such as biology, psychology and sociology. Since, however, compliance with complementarity meant eschewing determinism, this was readily perceived as a direct challenge to the Marxist credo.

Well aware of it, Pauli, – as caustic as ever –, observed that "Catholics and Communists depended on determinism to buttress their eschatological faiths, the former in the heaven to come, the latter in the terrestrial paradise."[58] Actually, the condemnation by Soviet physicists and philosophers of the Copenhagen interpretation for its implicit abhorrence of Dialectic Materialism, linked to its incorporation in physics of subjective materialism, had begun as early as the 1930s and had reached a peak in the first half of the 1950s.[59] In effect, following a speech made by one of Stalin's aides during a meeting of the Communist's Party Central Committee in 1947, the Copenhagen philosophy was officially outlawed in the Soviet Union from then on: In the sequence of the formal pronouncement of the Soviet's supreme political body, came the utterance of its supreme scientific authority, which would remain in place for a dozen years.

In that same year of 1947, at the yearly Meeting of the Soviet's Union Academy of Science, simultaneous with the proscription of complementarity from Soviet physics, it was decreed that – in order to make it compliant with Dialectical Marxism – the only acceptable interpretation of QM, from then on, would be that in which the "real objectivity" of atomic particles was tacitly assumed. Thereupon – and as a direct consequence of this –, a profusion of works condemning the orthodox standpoint, signed by such physicists as A. Al. Maksimov and D. I. Blokhintzev, soon began coming to light. Particularly influential in this respect were some papers of 1952 by Blokhintzev (translated before long into German and French), very critical of the idealistic and positivist conception of QM, and where, for the first time, the "School of Copenhagen," which he qualified as "reactionary," is mentioned.[60] In cause here is not so much a dispute concerning physical arguments, but a confrontation between two antagonistic theories of knowledge: Marxist materialism on one side and the immaterialism promoted by the subjectivist idealism of Copenhagen on the other.[61]

Thirdly, the appearance of hidden-variable theories – specifically Bohm's causal interpretation of QM – as viable alternatives to complementarity was perceived as a direct threat against this dearly held notion of Bohr's. Bohm, in 1952, proposed a causal hidden-variable theory in which particles occupied well-defined positions, albeit not completely determined, in practice. With this, Bohm put in question not only von Neumann's 1932 "proof" of the impossibility of the existence of any hidden-variable theories,[62] but cast doubt upon complementarity as well. Still, whereas Bohm's scheme exhibited exactly the same empirical results as non-relativistic QM, not only its theoretical framework was quite different, but its subjacent interpretation was far removed, moreover, from the entrenched monocracy. Opposed to the argument made by James Cushing – who claims that the causal theory of Bohm was studiously ignored by physicists with the lone exception of the Broglie[63] and, for a short while, of Mario Bunge –, Olival Freire states that "the causal interpretation did not pass unnoticed", although "most notices were unfavorable reception".[64] Actually, in spite of the two eldest Copenhageners Bohr and Born, admitting that – as a result of the choices made – the divide between ordinary QM and hidden-variables versions had an epistemological basis, the younger champions of the orthodoxy held that Bohm's and de Broglie's results were mere metaphysical constructs, devoid of any physical content or meaning. In sum, the reception of the Bohm's causal theory caused a "reaction of the old guard".

The opposition to Bohm's heresy was most naturally headed by the trio composed by Heisenberg, Pauli and Rosenfeld, all of whom, as forefront stalwarts of the positivist creed, did not shy away from including more than a hint of derision in their forthright rebuttal of the new theory. So, Heisenberg equated hidden-variables with "fictitious entities," considering the entire theory no more than an "ideological superstructure" with a language utterly unable – contrarily to the Copenhagen construct – of stating anything useful about physics.[65] For his part, Pauli – who already had, a quarter of a century before (in the V Congress of Solvay), undisguisedly manifested his distaste with de Broglie's proposition –, now directed his disapproval not only against the pilot-wave conception and Bohm's particular causal theory, but also against the general proposition of hidden-variables, dismissing it as mere "artificial metaphysics."[66] Going further, Pauli called upon von Neumann's "proof" against hidden-variables theories, deriding the non-locality of Bohm's formulation for being unable to furnish any intrinsic properties of individual elementary particles.[67]

The most radical defender of Bohr's epistemology was, however, Léon Rosenfeld who went a long way at singling out complementarity as an essential, and indispensable component of QM, stemming directly from experience, and deeming the proponents of determinism as traders of rationality for metaphysics: "the physicist who still tries to cling to it [the determinism], who refuses to surrender to the evidence of complementarity, abandons the rational attitude of the scientist taking, whether wanting it or not, that of the metaphysician;"[68] comparing Bohm (for his proposal of a causal formulation as an alternative to complementarity) to a tourist in a foreign land; branding Landé (for his creation of an interpretation of QM exclusively in terms of particles); and others, as dilettantes, responsible for imparting confusion to quantum theory.[69] And, reaching even farther out, connecting complementarity with Marxist ideology.[70]

Unequivocal evidence of how removed from objective neutrality (tacitly taken for granted whenever hard science is concerned) Rosenfeld was willing to position himself was his attempt at combining Bohr's epistemology with Marxism.[71] Indeed, in his defense of Bohr (and of his concept of complementarity) from the attacks fired off by Soviet physicists, Rosenfeld deftly put together a dialectical-materialistic rendering of complementarity, which he vouched as being "the first example of a precise dialectical scheme, whose formal structure has been accurately analyzed by the logicians."[72]

Still, notwithstanding their total abhorrence of Bohm's work, neither Heisenberg, nor Pauli or Born were prepared to endorse this kind of unabashed admixture of physics with politics. In a letter of 13 May 1954 to Heisenberg, Pauli expressed his satisfaction for having been able, in his capacity as editor of the volume honoring Bohr's 70th birthday, to thwart Rosenfeld's unduly attempt at promoting marxist-materialistic ideology in his contribution to that volume.[73] Max Born, in turn, went to the trouble of sending Rosenfeld a ten page typed text with the title *Dialectical Materialism and Modern Physics*, whose express purpose was to state in no uncertain terms that Dialectical Materialism could not be justified by complementarity.[74]

All this serves as a reminder of how – like any other human activity – both the production of science as well as the course it undertakes are not immune to the contingent social, political, and economical environment prevailing at any time and place. In fact, the practice of science neither takes place in aseptic conditions – entirely devoid of any contamination drawn from several different cultural influences and fashions –, nor does it correlate with a neutral production mode. Besides being necessarily influenced – and to a large extent determined –, by the actual manifold agendas of concerned individuals, the practice of science is unavoidably conditioned by scientific trends, ideological constraints, power plays, financial means, and so forth.

The history of the creation and empowerment of Copenhagen's hegemonic monocracy corroborates the thesis that the scientific enterprise – as in fact any human initiative – is highly dependent on a myriad of factors; and that its immensely varied outcomes reach far beyond its strict confines.

Assailing the monocracy from within

Around the mid twentieth century, a growing contestation to the claim that complementarity was intrinsic to QM commenced to be insistently heard. But, whereas until then, the major opposition to complementarity had been invariably raised by outsiders – expressly through hidden-variable proposals – now the criticism, coming as it did from within the loyalist ranks, cut closer to the bone. Springing from the very orthodoxy, the new heresy – with its worrisome questioning of the canonical views on quantum measurement – opened up a broad and permanent breach in the long-standing interpretative monopoly held by the Copenhagen school.[75]

Many were, then, the "quantum dissidents"[76] who, at the time, set free from the bonds imposed by the orthodoxy and dared to address the admittedly hazardous confines of fundamental matters in quantum theory – such as quantum measurement, non-locality and decoherence –, viewed by those obnoxious heretics as not even close to being adequately settled yet.[77] And if for some, like Wigner, Jauch or Margenau whose positions were not dependent on whether or not they upheld unconventional and/or controversial ideas, there were many more others – particularly among those in the early stages of their careers – for whom the inherent risk was unmistakably much higher. "[In the Universities] there are fewer corners where a creative person can ... pursue risky and original ideas. ... This also makes it harder for young scientists to buck the mainstream and devote themselves to the invention of new research programs. In our attempts to make unbiased evaluations of our peers' work, we professors tend almost reflexively to reward those who agree with us and penalize those who disagree."[78] Such was the glaring case of Hugh Everett III, who in 1957 introduced in his Ph.D. dissertation[79] (under John Wheeler in Princeton) his "relative state" theory through a thorough heterodox treatment of the measurement process, which did not contemplate any collapse whatsoever of the wave function. Everett's proposal resulted from his dissatisfaction with what he viewed as an artificial dichotomy brought about by von Newmann's interpretation of the dynamics of quantum processes: On one hand the state vector describing the system's physical state – incorporating (through a linear superposition of eigenstates) all possible eigenvalues of a given physical quantity – and with the state vector obeying a deterministic, continuous time-evolution set by Schrödinger's equation; on the other hand, the non-deterministic, irreversible discrete transition, occurring during a measurement, to only one of the allowed eigenstates – the so-called "collapse" or "reduction" of the wave-function –, precisely that one corresponding to the measured eigenvalue. Incapable of shining any light on the proceedings leading from a microscopic superposition of quantum states to a macroscopic description of the outcome of a classical measurement, this principle – named by von Neumann the "projection postulate" – is thus irrevocably endowed with a ad hoc nature.[80]

In his treatment, Everett considered the states of a subsystem of a composite system as being related to each other, that is, as being *relative states* interacting with each other, as it happens during a measuring process. This formulation (which fully agrees with observation), although comprehending a deterministic theory in which the states evolve according to Schrödinger's equation, does not entail any collapse of the wave function. Moreover, since the probabilistic interpretation derives from the formalism, its use is entirely justified, making, therefore, Everett's creation – combining von Neumann's treatment for the measuring device with Bohm's realism –, in his own words, a *metatheory* for the standard quantum theoretical formulation. Everett claimed further that his formal scheme could be applied to all quantum theories for which the principle of superposition applies, and that, as such, it comprises an appropriate setting for the quantization of gravitation.[81]

It was not at all surprising, then, that – in spite of Wheeler's initial efforts[82] at convincing Bohr's of the merits of his student's approach, Everett's relative states proposal was promptly rejected by the Copenhagen cohort, the same as Bohm's work had been a few years before.

Much more serious than Everett's was, however, Wigner's challenge, brought about some years later with his decision to carefully re-examine the subject of quantum measurement, an issue Wigner clearly decided as not quite settled yet. Following the line taken thirty years before by his old friend von Neumann in his book *Mathematical Foundations of Quantum Mechanics*,[83] Wigner admitted an active role for human conscience on all measurement processes.[84] A role which he averred as only definitely secured once the information coming out from a given measurement enters, and is registered in the observer's mind.

Long-established as a distinguished and reputed member of the theoretical physics community, and, moreover, a recent recipient of the physics Nobel Prize for 1963, Wigner certainly could not be easily dismissed. On the contrary, as a prominent representative of the Princeton School,[85] his reputation compelled to a good amount of discretion when dealing with any scientific utterances he might offer. May as it be, exception made by the very few who, at the time cared about foundational questions in QM, Wigner's attempts at elucidating the worrisome points raised by the measuring process went practically unnoticed, but certainly not – of course – by the ever alert guardians of the orthodoxy. Hence, it was that Wigner found himself at the receiving end of Rosenfeld's harsh criticisms. Impervious to Wigner's high standing, Rosenfeld's diatribes, put forward in rather acidic and contemptuous tones, persisted unabated well until the end of the 1960s.

During the 1960s the disputed controversy between Wigner (together with Jauch, Yanase, Shimony and Moldauer) and Rosenfeld (alongside with the Italians Daneri, Loinger and Prosperi) about measurement problem, led to the breakdown of the monocracy of Copenhagen.[86] The year of 1970 is a turning point in the research on the foundations of QM, particularly with the *Enrico Fermi Summer School*, held in Varenna, and with the beginning of the publication of the journal *Foundations of Physics.* In that Summer School, Wigner supported the proposals of H. D. Zeh for the measurement problem, which, about twenty years later, would lead to the theoretical and, subsequently, experimental characterization of the physical phenomenon of "decoherence", the later being a subject of current research in physics.[87]

The research on the foundations of QM continued to flourish in the 1970s and 1980s, now directed towards the measurement problem, the Bell's theorem – motivated by the EPR article and by Bohm's work on a causal interpretation of QM, and his insistence on non-local realistic hidden variables theories – and towards its experimental tests (which initiated the so-called "experimental metaphysics" [88]), using the quantum entanglement phenomenon of pairs of particles, to which were crucial the works of John Bell, John Clauser, Abner Shimony, Michael Horne, Richard Holt, and the group of Alain Aspect, among others. From the late 1980s on, Daniel Greenberger, Michael Horne, Anton Zeilinger, Jian-Wei Pan, Serge Haroche, Nicolas Gisin and others, by means of advanced experimental techniques of parametric down conversion for producing pairs of photons and of multi-photon entanglement, performed several fundamental tests of QM versus local realism and developed new domains of research such as quantum information, quantum cryptography and quantum computation, areas that could only be opened by the breakdown of the hegemony of the Copenhagen interpretation.[89]

The *status quo* lives on

The microscopic quantum world – that confine of the molecule, of the atom, and of its constituents – entailed for its description a mathematical language so uncommon and exclusive, and, hence, so distant from the one used for describing macroscopic *phenomena*, that the physical interpretation of microscopic events ought, likewise, to be formulated in terms quite apart from the ones used for the classical description. Entirely new physical attributes, related with quantum objects – like "spin," "strangeness," "charm," "color," and many others, without any correspondence whatsoever with the classical world – bringing with them unfamiliar mathematical structures like matrices and non-commuting algebras or even new constructions – so to speak made to order – such as Hilbert spaces and distributions.

And yet, the real weirdness drawn from these new concepts and algorithms derives from the fact that they did not coadunate with any visualization whatsoever of the quantum objects that they purported to represent. (An electron *cannot* be depicted as a spinning tiny ball, nor as a wave more or less localized in space.) The analysis of the double-slit experiment in sufficient detail propitiates the full appreciation of Richard Feynman's terse pronunciation: "Nobody understands quantum mechanics" (which, coming from Feynman, surely cannot be lightly dismissed). By this, what Feynman90 meant of course was that what nobody really understands is the Copenhagen interpretation of QM, and not the mathematical theory itself. For sure, no one (in any case not Einstein, for a fact) can understand a description which touts its non-locality; a description which depicts the wave-particle duality as absolutely necessary for a complete representation of quantum *phenomena*; a description which superposes undefined possible states until the making of a measurement (an observation) "squashes" the superposition, extracting from it only one "projected" state; a description having as a fundamental entity a "state" function (in general, a complex entity), devoid of any intrinsic physical meaning, reserved for its square modulus (a real valued function), with the meaning of a probability for localizing the "particle"/ "wave" in a certain point of space at a certain time. It is one thing to be able to adroitly employ the most refined mathematical techniques related with the observed behavior of microscopic *phenomena*; it is quite another matter having an understanding beyond whatever the mathematics strictly asserts. But, once this limitation is accepted, it automatically brings with it another hard difficulty: How will it be at all possible to bridge the microscopic and macroscopic domains of natural events, when the latter one is the only one directly accessible to human experience? Bridge absolutely needful, however, in order that the observations, the measurements, made by us – us, irrevocably classical entities – may have a meaning.

And that was precisely what the creators of the Copenhagen interpretation proposed to achieve, making sure that their vision would offer a picture as close as it was at all feasible to the classical description, thus ensuring that the new microscopic physics – alongside with its exclusive quantum features – would retain the familiar macroscopic concepts of "wave," "particle," "mass," "charge," and so forth.

Altering a firmly settled, well-reputed epistemological position in any of the natural sciences (physics certainly being no exception) proves always to be a rather strenuous enterprise as it necessarily goes against the entrenched beliefs held by the prevailing *status quo*. A notorious case of the present time being the forceful attempts to impose string theory (or rather theories) as the definite and final word concerning Nature at its most fundamental level, encompassing not only the ultimate explanation of elementary particles, but the long sought quantization of gravitation. However, notwithstanding these supremely ambitious claims, very few from outside the committed ranks of string theorists have adhered to this belief yet, the vast majority of physicists keeping instead a prudent and mistrustful distance[91] from strings. And yet, once having firmly secured for themselves a number of key positions in physics departments at several high-profile American research institutions, the closely-knitted string community began employing the well-tested politics of advertising the string ideal as an already settled truth, even though this truth has proved to be ever more elusive with the passing of time. "More than one friend has advised me that, `The community [of string theorists] has decided string theory is right and there is nothing you can do about it. You can't fight sociology'."[92]

In fact, as a social endeavor, physics is a rigidly stratified hierarchical structure, subjected to an effective pecking order, transversally pervading the global community. In the informed words of Lee Smolin, the sociology of science, referring to the entire practice of science, also "refers to the influence that older, established scientists have over the careers of younger scientists. We scientists feel uncomfortable talking about it, because it forces us to confront the possibility that the organization of science may not be entirely objective and rational."[93] Also, as stated by an anthropologist, who – during the early days of functioning of the Large Hadron Collider (LHC) at CERN – studied how scientists form opinions, make technical decisions and circulate knowledge, "[p]hysicists are professionally contemptuous."[94]

The questioning goes on

The doctrine created by and emanated from the Copenhagen-Göttingen axis – unreservedly taken for granted by almost the totality of practicing physicists over the last eight decades as a body of work in which each and every statement is fully adequate for a complete description of the quantum realm – has been forcefully and repeatedly questioned ever since its inception.

As already mentioned here, the first major breach of this exclusive point of view was owed largely to Einstein's efforts, followed much later on by Bohm's causal and non-local quantum theoretical interpretation, and, still later, by Bell's theorems. Bell's seminal results initiated a new era with the possibility of, for the first time, envisaging experimentation apt to contrast the statistical predictions of QM with those of hidden-variables theories, bringing about in this way the so-called 'experimental metaphysics'. Yet – and in spite of continuing relevant work going on ever since –, the fact remains that physicists keep ignoring and/or negating any contrary views to the Copenhagen dogma up to the present day.

As it is hopefully sufficiently clear from the above, it has not been implied nor even suggested here that the ruling orthodoxy offers a deceptive or inaccurate, unsound or misleading reading of quantum *phenomena*; neither its claim of handing out a description of microscopic behavior concurrent with observation has been denied here. Rather, the manifest purpose here has been simply to point out the hurry which dictated its arrival, following in the steps of the formal appearance of the new quantum ideas of Heisenberg and Schrödinger. Then, showing how this picture, created by just a handful of men, all of whom acting under the guidance of Niels Bohr instantly acquired the seal of inevitability, almost as if it were a new set of infallible truths. And, finally, recalling that the Interpretation of Copenhagen as, of course, all concepts devised to build a comprehensible vision of the physical world (like space, time, force, field, gravitation, etc.) is an abstract construct of the human mind, and, as such is necessarily prone to be altered or suppressed. Even the proposals of the greatest physicists were quite naturally not immune to doubts, hesitations, fruitless dead-ends, and plain mistakes.[95]

The long established Copenhagen paradigm stands unshaken in large measure, regardless of any enduring uneasiness. And yet, manifold doubts persist in disturbing, up to our days, some of the most reputed and respected representative figures of the new quantum order. Suffice it here to mention just two examples. The Italian Emilio Segrè recalled that "in his last years, Fermi seemed less convinced that the current interpretation of QM is the last word on the subject."[96] Segrè goes on to observe that "[t]his resistance against quantum theory was apparent mainly in people older than Fermi and his group, because the younger physicists either understood or believed the theories, and, in any case, learned to use them, even if they had not completely assimilated them."[97]

In a conference held in Jerusalem in 1979, in honor of Einstein's centenary, Dirac proffered the following warning: "It seems clear that the present QM is not in its final form. Some further changes will be needed, just about as drastic as the changes made in passing from Bohr's orbit theory to quantum mechanics ... It might very well be that the new quantum mechanics will have determinism in the way that Einstein wanted."[98]

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1 Quoted in BARRETT, Jeffrey A. *The Quantum Mechanics of minds and worlds*. Oxford: Oxford University Press, 1999. p. 15. In the same page, immediately after this quotation, Barrett states: "It is curious that Bohr and his Copenhagen interpretation of quantum mechanics could have led to such consensus when there is good reason to suppose that few physicists ever really understood what Bohr's position was. Indeed, it is difficult to find a single, unambiguous statement anywhere in the literature of what the Copenhagen interpretation was. It was the von Newmann-Dirac formulation that made it into the textbooks, and so this was presumably the formulation that in fact held the allegiance of most physicists."

2 BARRETT, op. cit., p. 15-16.

3 In a letter to Schrödinger of August 1939, Einstein proclaimed that he was "convinced as ever that the wave representation of matter is an incomplete representation of the state of affairs, no matter how practically useful it has proved itself to be. The prettiest way to show this is by your example with the cat" (Schrödinger's `smeared-out' cat, which was included in his paper in *Naturwissenschaften*, v. 23, p. 8, 1935; translated in PRZIBRAM, K. (Ed.). *Letters on wave mechanics: Schrödinger. Planck. Einstein. Lorentz*. New York: Philosophical Library, 1967. p. 35. Translation and Introduction from the German by Martin J. Klein.

4 EINSTEIN, A.; PODOLSKY, B.; ROSEN, N. Can Quantum-Mechanical description of physical reality be considered complete? *Physical Review*, v. 47, p. 777-780, 1935. Reprinted in WHEELER, John A.; ZUREK, Wojciech H. (Ed.), *Quantum Theory and measurement*. Princeton: Princeton University Press, 1983. p. 138-141.

5 "The conception of a world that really exists is based on there being a far-reaching common experience of many individuals, in fact of all individuals who come into the same or similar situation with respect to the object concerned." Letter of Schrödinger to Einstein of 18 November 1950. In: PRZIBRAM, op. cit., p. 37.

6 DE BROGLIE, Louis. Sur la possibilité de rélier les phénomènes d'interference et de diffraction à la théorie des quantas de lumière. *Comptes Rendus*, v. 183, p. 447-448, 1926; id., La structure de la matière et du rayonnement et la mécanique ondulatoire'. *Comptes Rendus*, v. 184, p. 273-274, 1927; id., La mécanique ondulatoire et la structure atomique de la matière et du rayonnement. *Journal de Physique et du Radium*, v. 8, p. 225-241, 1927. All these three articles are reprinted in DE BROGLIE, Louis. *La physique quantique restera-t-elle indéterministe?* Paris: Gauthier-Villar, 1953. p. 25-27; 27-29; 225-241, respectively.

7 DE BROGLIE, Louis. *Une tentative d'interpretation causale et non lineaire de la mecanique ondulatoire: la Théorie de la Double Solution*. Paris: Gauthier-Villars, 1956. p. 85-89; id. Vue d'ensemble sur mes travaux scientifiques. In: GEORGE, André (Ed.). *Louis de Broglie: physicien et penseur*. Paris: Éditions Albin Michel, 1953. p. 464-471. Volume dedicated to de Broglie on his sixtieth birthday in 1952.

8 JAMMER, Max. *The philosophy of quantum mechanics*: The interpretations of quantum mechanics in historical perspective. New York: John Wiley & Sons, 1974. p. 44-47.

9 Dirac's testimony is quite elucidative in this respect: "I was greatly impressed" – wrote him in a letter to Margrethe Bohr at the time of her husband's death – "by the wisdom that Niels showed, not only in physics but in all branches of human thought. He was the wisest man I knew, and I did my best to absorb some of the wisdom he imparted." FARMELO, Graham. *The strangest man*: The hidden life of Paul Dirac, quantum genius. London: Faber and Faber Ltd., 2009. p. 371.

10 "Bohr's Institute" – wrote George Gamow, one of its visitors from the Soviet Union – "fast became the world's center of quantum physics, in such a fashion that, paraphrasing the old Romans, `all roads led to Blegamsvej 17'." GAMOW, George. *Thirty years that shook physics: The story of quantum theory*. New York: Dover Publications, 1966. p. 51.

11 In fact, initially, these three composed the entire group of mentors: "I could say that at that time [October 1927, at the V Congress of Solvay] it was practically Bohr, Pauli and myself. Perhaps just the three of us. That very soon spread out." Heisenberg interview by T. S. Kuhn, 19 February, 1963, NBA.

12 Einstein remained for ever inimical to "Bohr's principle of complementarity, the sharp formulation of which, moreover, I have been unable to achieve

despite much effort I have expended on it." EINSTEIN, Albert. Remarks to the essays appearing in this collective volume. In: SCHILPP, Paul A. (Ed.), *Albert Einstein*: philosopher-scientist. 3. ed. Illinois: Open Court, [1949/1969]. p. 674. Schrödinger did not appreciate it either: "Bohr wants to complement away [*wegkomplementieren*] all difficulties.". PAIS, Abraham. *Niels Bohr's times, in Physics, Philosophy and Polity*. Oxford: Clarendon Press, 1991. p. 425. As for Dirac, in an interview he gave in 1969, he observed that the principle of complementarity "always seemed to me a bit vague." And, always true to himself: "It wasn't something which you could formulate by an equation." FARMELO, op. cit., p. 128.

13 HEILBRON, John L. The earliest missionaries of the Copenhagen spirit. *Revue d'Histoire des Sciences*, v. 38, p. 195-230, 1985. Quoted from p. 223.

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17 WHEELER; ZUREK, op.cit., p. xix.

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37 BARRETT, op. cit., p. 15.

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44 BELLER, op. cit., p. 277-278.

45 JAMMER, Max. *Albert Einstein, historical and cultural perspectives: The centennial symposium in Jerusalem*. Princeton: Geral Holton and Yehuda Elkana, Princeton University Press, 1982. p. 68.

46 HEISENBERG, Werner. *Physics and Philosophy: The revolution in modern science*. New York: Harper & Row, 1958. Republished by Prometheus Books, Amherst, New York, 1999, p. 132.

47 Ibid., p. 139.

48 Jeffrey Barrett manifested his surprise with Bohr's interpretation of QM having so fast such widespread consensus when "there are good reasons for supposing that few physicists have really understood which was Bohr's position. In fact, it is difficult to find in the literature a single unambiguous statement over what is the Copenhagen interpretation." BARRETT, op. cit., p. 15.

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51 Significantly, the authors shied away from drawing out any deterministic conclusions from the new theory.

52 CUSHING, James T. *Quantum mechanics: Historical contingency and the Copenhagen hegemony*. Chicago and London: The University of Chicago Press, 1994. p. 114-115.

53 Ibid., p. 116.

54 JAMMER, op. cit., 1974, p. 250.

55 SCHILPP, Paul A. (Ed.). *Albert Einstein*: Philosopher-Scientist. 3. ed. La Salle, Illinois: Open Court, 1949-1969.

56 OSNAGHI; FREITAS; FREIRE JR., op. cit., p. 101; CAMILLERI, op. cit., p. 34-37, 45; PATY, M. The nature of Einstein's objections to the Copenhagen interpretation of quantum mechanics. *Foundations of Physics*, v. 25, n.1, p. 183-204, 1995; JAMMER, M., op. cit., 1974, p. 250. See also the references in the notes 40 and 41 of this article.

57 BITBOL, Michel. *Schrödinger's Philosophy of Quantum Mechanics*. Boston Studies in the Philosophy of Science. Dordrecht: Kluwer Academic Publishers, 1996, especially the first four chapters and page ix of the Introduction. See also OSNAGHI; FREITAS; FREIRE JR., op. cit., p. 101.

58 FREIRE JR., op. cit., p. 20.

59 JACOBSEN, Anja Skaar, *Léon Rosenfeld*: Physics, Philosophy and Politics in the Twentieth Century. Hackensack, NJ and London: World Scientific, 2012, especially chap. 6, p. 255; CAMILLERI, op. cit., p. 34-40; FREIRE JR., op. cit., p. 20-28. KOJEVNIKOV, Alexi B. *Stalin's great science: The times and adventures of soviet physicists*. London: Imperial College Press, 2004. The chap. 9 "Modernist science, ideological passions" is particularly recommended, especially p. 225ff; GRAHAM, Loren R. *Science, Philosophy, and human behavior in the soviet union*. New York: Columbia University Press, 1987, particularly chap. ten, p. 320-353; Id., *Science and Philosophy in the soviet union*. New York: Alfred A. Knopf, 1972, particularly chap. III, p. 69-110.

60 CAMILLERI, op. cit., p. 34-37.

61 Outside of the Soviet Union, a few physicists adept of Marxism – in particular David Bohm in the United States, Jean-Paul Vigier in France and Mario Bunge in Canada – also explicitly opposed the Copenhagen orthodoxy. See, for instance, FREIRE JR., op. cit., p. 21.

62 Concerning von Neumann's "proof" see, for instance, VON NEUMANN John. *Mathematical foundations of quantum mechanics*. Princeton: Princeton University Press, 1955. Translated from the German original edition by BEYER, Robert T. *Mathematische grundlagen der quantenmechanik* of 1932. The formal proof occupies pp. 305-24, which is followed by several pages of commentary. The problem of "hidden parameters" is placed in the preface (p. x), and on p. 209.

63 Emboldened by Bohm's results, de Broglie promptly recovered and developed his ideas of old about hidden-variables. See DE BROGLIE, op. cit., 1953.

64 FREIRE JR., op. cit., p. 18-19; Id., The historical roots of Foundations of quantum physics' as a field of research (1950-1970). *Foundations of Physics*, v. 34, p. 1741-1760, 2004. For a description of the reactions to Bohm's causal theory, see, for example, ch. 9 of CUSHING, op. cit., p. 144-173, where further developments of Bohm's theory, made by D. Dürr, S. Goldstein, N. Zanghi and A. Valentini are mentioned.

65 HEISENBERG, op. cit., 1999, p. 131-135.

66 PAULI, Wolfgang. Remarques sur le probleme des parametres caches dans la mecanique quantique et sur la theorie de l'onde pilote. In: GEORGE, André (Ed.), op. cit., 1953, p. 33; CUSHING, op. cit., p. 149.

67 PAULI, op. cit., 1953, p. 38-39; CUSHING, op. cit., p. 150.

68 Translated from the original French: "le physicien qui essaie encore de s'y cramponner, qui refuse de se rendre à l'évidence de la complémentarité, abandonne l'attitude rationnelle de l'homme de science pour prendre, qu'il le veuille ou non, celle du métaphysicien". ROSENFELD, Léon. L'Évidence de la complémentarité. In: GEORGE, op. cit., 1953, p. 51-52.

69 ROSENFELD, op. cit., 1953, p. 55-56; FREIRE JR., Olival. O debate sobre a imagem da Ciência: A propósito das ideias e da acção de E.P. Wigner. In: SANTOS, Boaventura Sousa (Org.). *Conhecimento prudente para uma vida decente*: um discurso sobre as ciências revisitado. Porto: Edições Afrontamento, 2003. p. 485.

70 See ROSENFELD, op. cit., 1953, p. 50-56. Rosenfeld was connected to the Western brand of Marxism rather than the Soviet variety and was convinced that complementarity was a remarkable dialectical achievement.

71 JACOBSEN, op. cit., p. 5, 96, 139, and especially chap. 6.

72 ROSENFELD, op. cit., 1953, p. 64; CAMILLERI, op. cit., p. 31-32; FREIRE JR., op. cit., 2005, p. 21.

73 In his letter, Pauli – ever corrosive – nicknamed Rosenfeld as $\sqrt{Trostsky\ x\ Bohr}$.

74 Born's text was eventually published in FREIRE, Olival; LEHNER, Christoph. 'Dialectical materialism and modern Physics', an unpublished text by Max Born. *Notes and Records of the Royal Society*, v. 64, p. 155-162, 2010; CAMILLERI, op. cit., p. 43-44.

75 This new and varied front of heterodox ideas integrated, among others, the original offerings of H. Everett, B. De Witt and N. Graham; the corpuscular proposal of A. Landé; and the statistical interpretation of K. Popper and L. Ballentine. On the other hand, among those addressing novel views about quantum measurement were E. Wigner, J. Jauch, H. Margenau, B. D' Espagnat, A. Shimony, G. Ludwig, J. Bub, H. Zeh, H. Araki and M. Yanase.

76 FREIRE JR., Olival. Quantum dissidents: Research on the foundations of quantum theory circa 1970. *Studies in History and Philosophy of Modern Physics*, v. 40, p. 280, 2009.

77 During the 1970s, a small group of quantum dissidents actively defied the generally accepted line according to which all fundamental issues having to do with quantum theory had already been conclusively answered by the founding fathers of the theory.

78 SMOLIN, Lee. *The trouble with Physics*: The rise of String Theory, the fall of a science and what comes next. Boston: Houghton Mifflin Company, 2006. p. 264-265.

79 EVERETT III, Hugh. `Relative state' formulation of quantum mechanics. *Review of Modern Physics*, v. 29, p. 454-62, 1957. Reprinted in DEWITT, Bryce S.; GRAHAM, Neill (Ed.). *The many-worlds interpretation of quantum mechanics*. Princeton: Princeton University Press, 1973. p. 141-149, and in WHEELER; ZUREK (Ed.). op. cit., p. 315-323.

80 But, whereas Bohr's answer to this was that the outcomes of measurements ought to be expressed in terms of the language of classical physics, von Neumann held that the treatment of the measuring apparatus should be quantal.

81 EVERETT, op. cit., 1957-1973, p. 149.

82 OSNAGHI; FREITAS; FREIRE, op. cit., p. 97. After failing in persuading Bohr – a former mentor of his, and under whose spell he had remained –, Wheeler kept a prudent distance from the results of the thesis he had supervised.

83 VON NEUMANN, op. cit., 1955.

84 FREIRE JR., op. cit., 2003, p. 483-495.

85 Home and Whitaker, based on Ballentine, distinguish between two types of *orthodox interpretations* of QM: i) different versions of the Copenhagen Interpretation (also called "Copenhagen School," represented by Bohr, Heisenberg, Pauli, and Rosenfeld, and ii) the "Princeton School," represented by von Neumann and Wigner. See, for example, BALLENTINE, Leslie E. The statistical interpretation of quantum mechanics. *Review of Modern Physics*, v. 42, p. 360, 1970; and HOME, David; WHITAKER, M. A. B. Ensemble interpretations of quantum mechanics. A modern perspective. *Physics Reports*, v. 210, p. 243-247, 1992. FREIRE JR., op. cit., 2003, p. 491-493, shows that Wigner himself designates his own interpretation as *orthodox interpretation*.

86 FREIRE JR., op. cit., 2003, p. 495-500.

87 JOOS, Eric; ZEH, H. Dieter; KIEFER, C.; GIULINI, D.; KUPSCH, J.; STAMATESCU, I.-O. *Decoherence and the appearance of a classical world in quantum theory*. 2. ed. Berlin: Springer-Verlag, 2003.

88 COHEN, Robert S.; HORNE, Michael; STACHEL, John (Ed.). *Experimental metaphysics*: Quantum mechanical studies for Abner Shimony. Dordrecht, The Netherlands: Kluwer Academic Publishers, 1997. v. 1.

89 BERTLMANN, R. A.; ZEILINGER, A. *Quantum [un]speakables*: From Bell to quantum information. Berlin: Springer-Verlag, 2002; EVANS, James; THORNDIKE, Alan S. (Ed.). *Quantum Mechanics at the crossroads*: New perspectives from History, Philosophy and Physics. Berlin: Springer-Verlag, 2007; FREIRE JR. Quantum dissidents, 2009, p. 280-289; NIEUWENHUIZEN, Theo M.; ŠPIČKA, Václav; MEHMANI, Bahar; AGHDAMI, Maryam J.; KHRENNIKOV, Andrei Yu. (Ed.). *Beyond the quantum*. New Jersey: World Scientific, 2007; ADENIER, G.; KHRENNIKOV, Andrei Yu; LAHTI, Pekka; MAN'KO, Vladimir I.; NIEUWENHUIZEN, Theo M. (Ed.). *Quantum theory*: Reconsideration of Foundations-4. AIP Conference Proceedings. Melville, New York: American Institute of Physics, 2007. v. 962.

90 During the Second World War, Feynman was engaged on the Manhattan Project at Los Alamos. During his stay there at the time, Bohr singled out Feynman to try out his ideas because he was the only one who would not be in awe of him.

91 Even some dedicated former string theorists, like Lee Smolin, have formally recanted string's contentions. See SMOLIN, op. cit.

92 SMOLIN, op. cit., p. 267.

93 Ibid., p. 267.

94 MERALI, Zeeya. The large human collider. *Nature*, v. 464, p. 483, 2010.

95 Just recall Einstein's back and forth dealing with his "cosmic" term which he belatedly added to his original field equation of the general theory of relativity, or his failed quest for an apt unified theory; or still Bohr' totally wrong turn with his BKS-theory with its violation of strict energy and momentum conservation.

96 SEGRÈ, Emilio. Biographical introduction. In: CRONIN, James W. (Ed.). *Fermi remembered*. Chicago: The University of Chicago Press, 2004. p. 22.

97 Ibid., p. 22.

98 DIRAC, Paul A. M. The early years of relativity. In: HOLTON, G.; ELKANA, Y. (Ed.). *Albert Einstein, historical and cultural perspectives*. Princeton: Princeton University Press, 1982. p. 85.

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