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Why were Matrix Mechanics and Wave Mechanics considered equivalent?

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Abstract

A recent rethinking of the early history of Quantum Mechanics deemed the late 1920s agreement on the equivalence of Matrix Mechanics and Wave Mechanics, prompted by Schrödinger's 1926 proof, a myth. Schrödinger supposedly failed to prove isomorphism, or even a weaker equivalence ("Schrödinger-equivalence") of the mathematical structures of the two theories; developments in the early 1930s, especially the work of mathematician von Neumann provided sound proof of mathematical equivalence. The alleged agreement about the Copenhagen Interpretation, predicated to a large extent on this equivalence, was deemed a myth as well.

In response, I argue that Schrödinger's proof concerned primarily a domain-specific ontological equivalence, rather than the isomorphism or a weaker mathematical equivalence. It stemmed initially from the agreement of the eigenvalues of Wave Mechanics and energy-states of Bohr's Model that was discovered and published by Schrödinger in his first and second communications of 1926. Schrödinger demonstrated in this proof that the laws of motion arrived at by the method of Matrix Mechanics are satisfied by assigning the auxiliary role to eigenfunctions in the derivation of matrices (while he only outlined the reversed derivation of eigenfunctions from Matrix Mechanics, which was necessary for the proof of both isomorphism and Schrödinger-equivalence of the two theories). This result was intended to demonstrate the domain-specific ontological equivalence of Matrix Mechanics and Wave Mechanics, with respect to the domain of Bohr's atom. And although the mathematical equivalence of the theories did not seem out of the reach of existing theories and methods, Schrödinger never intended to fully explore such a possibility in his proof paper. In a further development of Quantum Mechanics, Bohr's complementarity and Copenhagen Interpretation captured a more substantial convergence of the subsequently revised (in light of the experimental results) Wave and Matrix Mechanics.

I argue that both the equivalence and Copenhagen Interpretation can be deemed myths if one predicates the philosophical and historical analysis on a narrow model of physical theory which

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disregards its historical context, and focuses exclusively on its formal aspects and the exploration of the logical models supposedly implicit in it.

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1. Introduction

Recently, based on a careful scrutiny of the key arguments pursued by physicists at the beginning of the Quantum Revolution, several philosophers have characterized some of the essential agreements between these physicists as unsubstantiated and unjustified.

To cite perhaps the most notable example, in the late 1920s, the community of quantum physicists agreed on the equivalence of the two competing formal accounts of quantum phenomena, namely, the Matrix Mechanics of Heisenberg, Born and Jordan and E. Schrödinger's Wave Mechanics.

Early on, these accounts had been perceived to be substantially different in terms of the mathematical techniques they employed. The Matrix Mechanics was an algebraic approach employing the technique of manipulating matrices. The Wave Mechanics, in contrast, employed differential equations and had a basic partial differential wave equation at its heart.

In addition, the formalisms were initially applied to two distinct sets of experimental results. The Matrix Mechanics was deemed successful in treating the appearance of spectral lines and later was found to be successful (to some extent) in experiments with electron scattering. For the Wave Mechanics, its initial applicability to light interference experiments was extended to include the account of the energy values in experiments with hydrogen atoms.

And finally, the ontological commitments arising from the formalisms were at odds with each other. Although renouncing individuality that characterizes classical particles, Heisenberg's approach was based on the emphasis on *discrete properties* of the observed phenomena, such as the occurrence of spectral lines of different intensities. This, in combination with his view that the postulated micro-physical entities do not necessarily have to be visualizable in space and time, abolished the commitment to spatial continuity in explaining and understanding quantum phenomena. In contrast, Schrödinger perceived *the field-like continuity* of some key micro-physical phenomena (e.g., those related to the double-slit experiments), as they were accounted for by Wave Mechanics, precisely as its main advantage over the old Quantum Mechanics.

It is not easy to determine to what extent each of these contrasting aspects was responsible for the general understanding that the two theories were irreconcilable. Be that as it may, because of this widespread belief, when the argument for their supposed equivalence was first conceptualized and published by Schrödinger in 1926, it was seen as a major breakthrough—it predicated the development of Quantum Mechanics.

Recently, however, Muller (1997a, 1997b, 1999) has deemed this equivalence a myth. Muller argues that the initial agreement concerning the equivalence was based on the misconception that both empirical and mathematical equivalence (i.e., the isomorphism of the mathematical structures of the two theories) were successfully demonstrated, and that only later developments in the early 1930s, especially the work of von Neumann (1932), provided sound proof of the mathematical equivalence, as opposed to the more famous proof provided by Schrödinger or similar attempts by others (Dirac, 1930; Eckart, 1926; Pauli, 1926).

If this re-evaluation tells the true story, it implies that the wide agreement among physicists on the equivalence of two formalisms in the 1920s, on which further developments of the theory were critically predicated, was an unjustified, indeed, an irrational act of faith (or myth, as Muller labels it) on the part of the physics community.

Even the so-called Copenhagen Interpretation of Quantum Mechanics, which has dominated the field since the 1930s, *and which stemmed from the new Quantum Mechanics, largely predicated on the alleged equivalence*, was debunked by the same rethinking of the history of the debate over the foundations of quantum theory (Beller, 1999), and was deemed another myth (Howard, 2004). Thus, presumably, the agreement on the interpretation that argued for the synthesis predicated on both Wave Mechanics and Matrix Mechanics (initially Niels Bohr's interpretation), and which was thought to have had successfully countered the arguments for the exclusive commitment to continuity based on Wave Mechanics on the one hand, and the discontinuity based on Matrix Mechanics on the other, was forced on the community by the Göttingen group (Beller, 1999) and/or constructed as a myth by subsequent deliberate or semi-deliberate misinterpretations of the history (Howard, 2004). In any case, focusing on the agreement on the mathematical equivalence (i.e., equivalence of mathematical structures of the theories) favors such views. If the mathematical equivalence of the two theories was proved in the 1920s, then the physical theory as such did not favor Copenhagen Interpretation over the other two interpretations (Schrödinger's and Heisenberg's), at least not in any straightforward way. But then it becomes rather puzzling how could have such a wide agreement on the Copenhagen Interpretation been justified (if the agreement was reached at all). And if the agreement on the mathematical equivalence was unjustified, as Muller claims, the distinctness of the competing theories could hardly offer a powerful argument for the Copenhagen Interpretation, that won overwhelmingly against the arguments for both wave-mechanical and matrix-mechanical approach to interpreting the theory.

So did the philosophers finally get it right, or have they missed something crucial in their analysis of early Quantum Mechanics? I will argue the latter.

If we premise our analysis of actual scientific practice on models of scientific knowledge, such as that of P. Suppes (1957, 1960), used in the above-outlined analysis of the equivalence case, we reduce the conceptual and historical analysis to the aspects of scientific knowledge having to do with the mathematical-logical analysis of the formalisms (such as Matrix Mechanics and Wave Mechanics), which, although indispensable in some aspects of scientific practice, may not be necessary in others. Such a narrowly focused analysis is bound to miss some key aspects of the physicists' arguments, embedded as they are in historical and philosophical contexts, which must be unraveled if one is to do justice to the physicists' thinking.

With respect to equivalence, I will argue that although it is true that Schrödinger failed to provide a full-fledged proof of mathematical equivalence, for the reasons that Muller points out, his paper contained only a preliminary attempt to do so.

Judging by its structure, its content, and the historical context in which it appeared, *Schrödinger's proof concerned a domain-specific ontological equivalence*, *the domain being Bohr's atom*. Bohr's complementarity and Copenhagen Interpretation captured a more substantial convergence of, the subsequently revised (in light of the experimental results), theories. Furthermore, even the mathematical equivalence of the theories did not seem out of the reach of existing theories and methods, although Schrödinger never intended to fully explore such a possibility in his proof paper.

2. The alleged myth of the equivalence

Muller (1997a, p. 36) argues, "The Equivalence Myth is that Matrix Mechanics and Wave Mechanics were mathematically and empirically equivalent at the time when the equivalence proofs appeared and that Schrödinger (and Eckart) demonstrated their equivalence" (although Schrödinger's proof was more elaborate and influential than Eckart's). Thus, the argument goes, the myth perpetrated by Schrödinger's (1926a) contemporaries was that he attempted to prove the mathematical equivalence of Matrix Mechanics and Wave Mechanics (the *explananas* of Schrödinger's overall argument), in order to explain their allegedly established empirical equivalence (*explanandum*) (Muller, 1997a, p. 36). Yet, Muller argues, contrary to the widespread belief at the time (and subsequently), Wave Mechanics and Matrix Mechanics were neither proven mathematically equivalent by Schrödinger, nor were they empirically equivalent.

The incorrect view that Wave Mechanics and Matrix Mechanics were empirically equivalent, Muller argues, stems from an overlooked fact that the two could and should have been treated as empirically distinct in light of the available knowledge. That the electron charge densities were smeared and that this "made it conceivable to perform an *experimentum crucis* by charge density measurements" (Muller, 1997a, p. 38) was overlooked. Moreover, the empirical agreement between Wave Mechanics and Matrix Mechanics hinted at by Schrödinger (1926a) on the first page of his paper concerns two cases that are insufficient as evidence of the purported empirical equivalence. The first case was a rather tenuously relevant (to the empirical equivalence thesis) case of coinciding energy values for the hydrogen atom and "the few toy systems" (Muller, 1997a, p. 49), and the second was the quantization of orbital angular momentum. I will say more about both cases shortly.

Muller's notion of *mathematical equivalence*, supposedly the main goal of the proof, is certainly much stronger than an equivalence that although employing mathematical techniques, *has no explicit goal* of arriving at a conclusion about the logical structures of the theories (e.g., empirical equivalence).[1] Thus Muller initially argues that since "the essence of a physical theory lies in the mathematical structures it employs, to describe physical systems, the equivalence proof, including part of Schrödinger's intentions, can legitimately be construed as an attempt to demonstrate the isomorphism between the mathematical structures of Matrix Mechanics and Wave Mechanics" (Muller, 1997a, p. 38).

While he never explicitly claims that isomorphism of Matrix Mechanics and Wave Mechanics was Schrödinger's main goal, his reconstruction of the proof, where "Matrix Mechanics and Wave Mechanics such as they were around March 1926 are thus tailored in

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[1] Schrödinger's statements about "mathematical equivalence" are ambiguous. See footnote 10.

structural terms" (1997a, p. 38), indicates that the proof's goal could not be much different.

As a matter of fact, Muller considers a "softer" kind of mathematical equivalence than the one that required a proof of the full-fledged isomorphism, one which also concerns the equivalence of the mathematical structures of Matrix Mechanics and Wave Mechanics. He labels it "Schrödinger-equivalence" or simply "S-equivalence". He subsequently presents a weakened version (Muller, 1999), namely "the proper S-equivalence", which is allegedly closer to Schrödinger's original intention. Yet proving this sort of equivalence is as demanding as that of proving isomorphism. More precisely, isomorphism, S-equivalence as well as proper S-equivalence, require the *bidirectional derivation* (i.e., derivation of matrices from eigenfunctions and *vice versa*), and as I will argue, this was not indispensable to the main goal of Schrödinger's proof paper.

There are three different reasons for the supposed failure of the mathematical equivalence. The first reason is that the absence of a state-space in Matrix Mechanics prevented *the direct mutual translation of sentences of Wave Mechanics and Matrix Mechanics*. A related second reason is that the language of Matrix Mechanics could not refer to space, charge-matter densities, or eigenvibrations,[2] "because Matrix Mechanics did not satisfy (in the rigorous model-theoretic sense) any sentence containing terms or predicates referring to these notions" (Muller, 1997a, p. 39). The most substantial reason, however, was the failure of an attempted (Muller believes) proof of S-equivalence. This failure was due to the unjustified assumptions regarding the so-called "the problem of the moments" of a function (Muller, 1997b) (allegedly sidestepped by von Neumann's proof Muller, 1999).

Muller's reconstruction of both the S-equivalence and proper S-equivalence prompts him to explicate his view of Schrödinger's (unachieved) goal: "[*A*]nyone who claims that today Matrix Mechanics and Wave Mechanics, as we currently know these theories, are 'equivalent' *in Schrödinger's intended sense* (*proper S-equivalence*) [italics by the author], or in some reasonably wakened sense … is not stating a mathematically established fact, but is perpetuating the Equivalence Myth of Quantum Mechanics" (Muller, 1999, p. 545).

Thus, although the *explanandum* of Schrödinger's overall argument was a limited empirical agreement of the two theories (rather than empirical equivalence, as the physics community allegedly believed), judging by Muller's reconstruction of the proof, *proving mathematical equivalence* (*whether isomorphism*, *S-equivalence or proper S-equivalence*) *was Schrödinger's central goal* (i.e., it served as the *explanans*), *and was perceived as such by the physics community at the time*.

3. The empirical evidence in early Quantum Mechanics

But was there really a myth of *empirical equivalence*? And was it perceived as an explanandum of Schrödinger's overall argument?

It is hard to argue for the existence of such a myth without assuming an oversimplified perception of the nature of available empirical evidence on the part of the physics community in the early days of Quantum Mechanics. If there was a myth of the empirical equivalence then Schrödinger's (1926a, p. 45) expression concerning the agreement "with each other" of Wave Mechanics and Matrix Mechanics "with regard to the known facts",

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[2] See the explanation of eigenvibrations on p. 18.

employed at the beginning of his proof paper, was perceived by the physics community as the claim of the full-blown empirical agreement.[3] This is a convenient characterization if one aims at constructing the full-fledged empirical equivalence as an explanandum of Schrödinger's (supposed) overall explanation. But it is too easy to demonstrate its failure, as Muller (1997a, p. 54) himself does in part by pointing to the incapability of Wave Mechanics to account for the line intensities.

The expression could also reflect the view that there was some compelling agreement between the two that was not firmly established. The community of physicist likely believed this. But this was not the only, nor perhaps the decisive motive for devising the proof. Schrödinger certainly never committed himself to a strong view of empirical equivalence, and it is actually very unlikely that anybody else believed in full-blown empirical equivalence, given what we know about the relevant developments at the time.

As a matter of fact, some experiments were considered crucial at the time, as they were conceived and performed to decide between the opposing views of micro-physical systems. A set of such experiments concerned the problem of smeared charge densities (the supposed lack of which is cited by Muller as evidence of the alleged unjustified agreement on the empirical equivalence).

Schrödinger's early wave-mechanical treatment of the atom as a charge cloud (instead of an electron as a particle, orbiting around the nucleus—Bohr's early model) did not at first accurately account for radiation of the atom (while Bohr's model did), given that only certain energy states were observed in spectroscopic experiments. The electric density of the cloud differed from place to place but remained permanent. Thus, in order to account for the radiation in corresponding energy states of the atom, Schrödinger introduced the idea of vibrations of the charge cloud in two or more different modes with different frequencies (i.e., the eigenvibrations accounted for by eigenvalues of the wave equation). As a consequence, the radiation is emitted in the form of *wave-packets* of only certain energies, corresponding to Bohr's frequency conditions. Since Schrödinger assumed that the classical electromagnetic theory accounts for the atom radiation, a number of different radiations could be emitted by the atom as the wave-packet of certain energy expands in space.[4] In the introduction to his proof, Schrödinger (1926a, p. 45) refers to the case of the oscillator, a special case of this Wave Mechanics treatment of radiation.[5]

The consequences of Schrödinger's theory, which contradicted Bohr's early view of radiation, were probed experimentally by a series of crucial experiments (Bothe & Geiger, 1926; Compton & Simon, 1925).[6] Thus, at the time of writing the proof paper, Schrödinger, as well as others, knew that despite the initial agreement of his theory with Bohr's results with respect to the energy states and radiation, the issue could be addressed

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[3] It turns out, as I will argue later on, that in order to properly analyze this Schrödinger's statement, the passage should be read in its entirety. "Considering the extraordinary differences between the starting-points and the concepts of Heisenberg's quantum mechanics and of the theory which has been designated "undulatory" or "physical" mechanics, and has lately been described here, it is very strange that these two new theories agree *with one another* with regard to the known facts, where they differ from the old quantum theory. I refer, in particular, to the peculiar "half-integralness" which arises in connection with the oscillator and the rotator" (1926a, p. 45).

[4] Perhaps surprisingly, this assumption was not at odds with what Bohr believed shortly before Schrödinger developed his own view, as Bohr was advocating the Bohr–Kramers–Slater theory that made a very similar assumption about the way the atom radiated energy.

[5] See footnote 3.

[6] See also Stuewer (1975) and Perovic (2006).

further by directly probing "individual radiation processes" that would, in turn, indirectly test the plausibility of the assumption about the vibrations of the atom. Schrödinger was cautioned but was not entirely convinced until 1927 (Mehra & Rechenberg, 1982, p. 138) that the results of these new experiments unequivocally demonstrated the discontinuous nature of matter—energy micro-interactions, as Bohr had claimed. Thus, the issue had been addressed experimentally but remained unresolved at the time of the appearance of the proof.

Nor could the experiments concerning the related issue of quantization of the orbital angular momentum (referred to as the 'rotator case' by Schrödinger at the beginning of the paper (1927a, p. 45)) have contributed to the presumed (by Muller) agreement on the empirical equivalence. By introducing the quantized angular momentum of electron, Bohr's model predicted correctly the spectral lines (i.e., Balmer lines) that corresponded to the allowed rotational frequencies of the electron. Heisenberg started with the discrete values of the spectral lines and developed matrices accounting for them. Schrödinger (1926b, p. 30) admitted that his Wave Mechanics was not capable of accounting for Balmer lines as straightforwardly as Matrix Mechanics did. Yet he presumed this to be a mere technical advantage (Schrödinger 1926a, p. 57), and the equivalence proof set out to demonstrate this. Schrödinger doubted (and offered his reasons in the proof for this doubt) that this particular success of the approach based on Matrix Mechanics necessarily reflected its substantial (epistemological or ontological) advantage, as it was not clear whether the spectral lines indicated the nature of *individual* corpuscular-like interactions of radiation with the matter (i.e., with the spectroscope), or whether they were the consequence of the way wave-packets, not individual corpuscles, interacted with the matter. This issue was also addressed by the above-mentioned scattering experiments and earlier by Ramsauer's (1921) experiments.

The experiments in these two cases, although perceived to be crucial by both the experimentalists and those interested in their theoretical implications, did not immediately prompt discarding either approach, if for no other reason than the physicists were simply unsure at the time, how exactly to apply the newly developed formalisms to particular experiments (Heisenberg in Mehra & Rechenberg, 1982, p. 151). Even a superficial look at the correspondence among them shows that they continued discussing the application of the formalisms and the meaning of such application well into the late 1920s.

Also, it is unlikely that the physicists perceived the coinciding energy values for the hydrogen atom and "a few toy systems", as Muller calls them, as an evidence of the empirical equivalence of Matrix Mechanics and Wave Mechanics. In fact, these values were only indirectly relevant to the relation between Matrix Mechanics and Wave Mechanics, as the "toy systems" were based on Bohr's model of the atom. Schrödinger's (1926b, 1926c) initial major interest concerned the agreement between energy values arrived at by Wave Mechanics and those predicted by Bohr's theory (Jammer, 1989, p. 275). This agreement prompted Schrödinger to think about the connection with Matrix Mechanics. Therefore, the initial agreement between Bohr's model and Schrödinger's Wave Mechanics, that I will discuss shortly, is an essential element of the motivation for the proof.

All these considerations were on going while Schrödinger and others were devising their proofs. More importantly, neither Schrödinger nor anybody else was certain whether or to what extent either of the two formalisms fully accounted for the observed properties of micro-physical processes, *nor whether either was indispensable*. As Jammer (1989, p. 210) puts it, Matrix Mechanics and Wave Mechanics were “designed to cover the same range of experiences” but it was not firmly established in 1926 that either did so.

In any case, there was considerable agreement between Wave Mechanics and Matrix Mechanics in accounting for the known experimentally derived facts, and this prompted the question about the possibility of a substantial equivalence (both empirical and mathematical). This, in turn, encouraged the construction of new crucial experiments, pushing the limits of the applicability of existing formalisms to them. Given this, the use of the phrase “the two new theories agree with one another with regard to the known facts” was a conditional statement—as both the continuation of the sentence (“where they [Wave Mechanics and Matrix Mechanics] differ from the old quantum theory”), and the subsequent sentence (which tempers the claim by revealing a clearly theoretical consideration behind the mention of the factual connection)[7] indicate.[8] The intention was much more tenuous than the full-fledged empirical evidence demands. It was unlikely that the motivation for the proof (i.e., the explanandum) was reduced to the meaning of the relevant phrase in the paper treated independently from the context of both the proof paper and the experimental and theoretical knowledge of the time. What Schrödinger and others had in mind was not a ‘myth’ of full-fledged empirical equivalence that should be explained. Rather, Schrödinger wished to show (and others commended his intention as well as his result) that the factual state of affairs indicated the possibility of domain-specific equivalence, stemming from the agreement of eigenvalues and Bohr’s energy levels, which could be revealed by fairly simple manipulations of both methods.

4. Was Schrödinger’s proof, a proof of mathematical equivalence?

It is tempting to define the goal of Schrödinger’s proof as a single goal.

Although there might be a single most important goal, the text reveals the complexity and hierarchy of Schrödinger’s intentions.[9]

In a passage that precedes the actual proof, Schrödinger (1926a, p. 46) states that “[i]n what follows the very intimate *inner connection* between Heisenberg’s Quantum Mechanics and my Wave Mechanics will be disclosed”. He continues, “From the formal mathematical standpoint, one might well speak of the *identity* of the two theories” and concludes the paragraph by saying, “The train of thought in the proof is as follows”.

An initial reading of this passage might suggest that the author is about to provide a full-blown proof of mathematical equivalence and that one should judge the effort based on this assumption. Even if Schrödinger’s intentions were different, or at least diverse in terms of the proof’s goals, the mention of the equivalence from “the mathematical standpoint” might urge one to accept such a narrow interpretation. It is possible, however, and as I will argue, quite likely that a rather different key goal is referred to by another phrase used in the passage, namely, the reference to “the intimate connection” between Matrix Mechanics and Wave Mechanics, and that the *subsequent* phrase, “mathematical standpoint”, refers to a distinct issue treated separately in the proof.

We should certainly not rely on this one passage. It does not help, though, that another passage that mentions the goals and the nature of the proof is also quite ambiguous

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[7] See footnote 18.

[8] See the entire Schrödinger’s sentence in the footnote 3.

[9] A similar complexity can be revealed in other proofs devised at the time as well.

(Schrödinger, 1926a, pp. 57–58).[10] Nor does it help that Schrödinger's attitude with regards to proving the equivalence appears to change significantly over time. In a letter to Wien, dated March 1926, he writes that "both representations are—from the purely mathematical point of view—totally equivalent" (Mehra & Rechenberg, 1982, p. 640). Yet in his second Communication, he states that Matrix Mechanics and Wave Mechanics "will supplement each other" (Schrödinger, 1926c, p. 30)[11] pointing out the advantages of each over the other, rather than noting their similarities. Moreover, as Jammer (1989, p. 273) points out, the physical and mathematical equivalences that Schrödinger (1926a, p. 58) mentions, are quite possibly distinct, although we can hardly determine, based on the text of the proof alone, whether or to what extent Schrödinger believed this and what exactly such a view would imply.

A textual analysis of the relevant passages that explicitly state the goals of the proof, although necessary, can go only so far. In order to determine, first, what the real intentions, and possible achievements, of the proof were, and second, how they were perceived by others, we must judge the text within the historical context in which it was written.

The proof was not motivated by empirical considerations alone. Possibly more important was agreement with Bohr's model of the atom. It prompted articulation of the key step in the proof: The construction of matrices based on the eigenfunctions. As Gibbins says, "Schrödinger in 1926 proved the two theories … equivalent", albeit ontologically, not empirically,[12] "*at least as far as the stationary, or stable-orbit, values for dynamical variables were concerned*" (Gibbins, 1987, p. 24).

Actually, both Matrix Mechanics and Wave Mechanics were constructed against the background of Bohr's model and were attempts to improve and, finally, to replace it. While Bohr's model had been changing since its inception, the importance of stationary (permitted) energy states in understanding quantum phenomena remained intact.[13] And as we will see, it became clear to what extent this core of the model remained insightful once Matrix Mechanics and Wave Mechanics were fully developed and the proofs of their equivalence devised.

Bohr's correspondence rules would have made the best available guidelines for the construction of Matrix Mechanics in its early phase.[14] And it would not be surprising if Matrix Mechanics were envisioned as nothing more than an improved version of Bohr's method. In any case, from Heisenberg's point of view, after he developed Matrix

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[10] Schrödinger's adamant statement that "they are completely equivalent from the mathematical point of view, and it can only be a question of the subordinate point of convenience of calculation" (p. 57) certainly overstates the case as the passages that follow this sentence suggest a much more moderate discussion of the formalisms and only a preliminary discussion of mathematical equivalence. In this context, it is hard to see how the phrase could be interpreted to refer to the isomorphism or S-equivalence.

[11] "I am distinctly hopeful that these two advances will not fight against one another, but on the contrary, just because of the extraordinary difference between the starting-points and between methods, that they will supplement one another and that the one will make progress where the other fails" (Schrödinger, 1926c, p. 30).

[12] See footnote 20 for the reasons why I label this equivalence "ontological".

[13] According to Bohr's (1913) early model electrons of the atom can occupy only certain orbital states characterized by appropriate energy levels.

[14] One could argue that Heisenberg's use of a method of quantizing the classical equation of motion in this early phase might not be sufficient evidence for the influence of Bohr's correspondence principle; it is not clear what exactly was the correspondence principle, nor it is clear whether Heisenberg's argument could be read as a correspondence argument. See Darrigol (1992).

Mechanics along with Born and Jordan, Bohr's method turned out to be a useful, albeit rough, first approximation. Matrix Mechanics emerged as a fully independent method once Heisenberg joined efforts with Born and Jordan (Born, Heisenberg, & Jordan, 1926; Jammer, 1989, p. 221).[15] Commenting on this, Lorentz optimistically notes in 1927, "The fact that the coordinates, the potential energy, etc., are now represented by matrices shows that these magnitudes have lost their original meaning, and that a tremendous step has been taken towards increasing abstraction" (Lorentz in D'Abro, 1951, p. 851).

Pauli's application of Matrix Mechanics to the hydrogen atom illustrated the independence of the method in a similar fashion (Mehra & Rechenberg, 1982, pp. 656–657). Yet Pauli realized that the fundamental assumptions concerning quantum phenomena, as approached from the point of view of Matrix Mechanics, are in agreement with Bohr's model and that, in this sense, the two might not be as different as they are in terms of methodology.

An insight concerning the relation of Wave Mechanics and Bohr's model, very similar to that of Pauli's concerning Matrix Mechanics, motivated Schrödinger to write the proof paper. In order to understand this, it is critical to take into account that the agreement of Wave Mechanics and Bohr's model (i.e., its core concerning stationary states) precedes the agreement of Wave Mechanics and Matrix Mechanics.

If one replaces the parameter E in Schrödinger's equation: $\Delta\psi + 8\pi^2 m_o/h^2\,[E - E_{pot}(x,y,z)]\,\psi = 0$, with one of the so-called eigenvalues, E_n, the equation will have a solution (thus becoming one of the eigenfunctions for a given eigenvalue).[16] The solution determines the amplitude of the de Broglie wave (stemming from his compromise between corpuscular mechanics and the theory involving continuity), while the eigenvalue (i.e., the energy) determines the frequency of the wave—that is, the chosen eigenvalue and the corresponding eigenfunction determine the mode of (eigen) vibration.

Now, Schrödinger's (1926b, 1926c) solution of the hydrogen atom eigenvalue equation of his first and second communication resulted unexpectedly in Bohr's energy levels. Or more precisely, as Bohr, who understood the importance of the insight, stated in 1927, "The proper vibrations of the Schrödinger wave-equation have been found to furnish a representation of electricity, suited to represent the electrostatic properties of the atom in a stationary state" (Bohr, 1985, Vol. 6, p. 96).[17]

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[15] As a matter of fact, in his transitional paper Heisenberg (1925) did not use matrices but rather linear arrays of Fourier expansion. Born and Jordan built Matrix Mechanics based on Heisenberg's paper (Heisenberg, 1925).

[16] Mathematically speaking, if a differential equation (such as Schrödinger's equation) contains an undetermined parameter, and it admits solutions only when particular values (eigenvalues or proper values) are assigned to the parameter, the solutions of the equation are called eigenfunctions.

[17] In (1926b, p. 8) Schrödinger starts from the wave mechanical assumptions and derives the expression—$E_l = m(e^2)^2/2K^2{}_l$ where "the well known Bohr energy-levels, corresponding to the Balmer lines, are obtained, if the constant K, introduced in for reasons of dimensions, we give the value $K = h/(2\pi)$, from which comes $-E_l = 2\pi^2 m(e^2)^2/h^2{}_l^2$". In (1926c, pp. 27–28), at the end of the discussion of the case of the rotator, Schrödinger generalizes the expression of an earlier derived wave function ($\mathrm{div}\,\mathrm{grad}\,\psi - (1/u^2)\ddot{\psi}$) in the following way: "For it is possible to generalize by replacing $\mathrm{div}\,\mathrm{grad}\,\psi$ by $f(q_k)\,\mathrm{div}\{[1/f(q_k)]\,\mathrm{grad}\,\psi\}$, where f may be an arbitrary function of the q's, which must depend in some plausible way on E, $V(q_k)$, and the coefficients of the line elements". Later on, he comments on the agreement between energy values in Bohr's theory and eigenvalues (discussed on p. 26), emphasizing the advantage of his approach: "…the quantum levels are *at once* defined as the *proper values* of Eq. (18) [wave equation], which *carries in itself its natural boundary conditions*" (p. 29). The entire argument for the advantage of the wave-mechanical approach in the second Communication was predicated on this agreement.

This insight made a great impression on Schrödinger. The newly discovered agreement raised a deeper question concerning an apparently discontinuous nature of the system imposed on an essentially continuous approach of Wave Mechanics by quantum conditions. Others were equally impressed: Wentzel immediately set out to examine this agreement with a new Wave Mechanics approximation method (Jammer, 1989, pp. 275–276).

Wave Mechanics had already emerged as methodologically independent from Bohr's account, and Schrödinger states this explicitly in the first section of the proof: "…we have a continuous field-like process in configuration space, which is governed by a single partial differential equation, derived from a principle of action. This principle and this differential equation replace the equations of motion and the quantum conditions of the older 'classical quantum theory'"(1926a, p. 45). *However, in light of this newly obtained agreement, it was not obvious that Wave Mechanics's independence, like that of Matrix Mechanics, was not merely a methodological independence.*

Schrödinger was well aware of all this, and it guided the development of the equivalence proof. Thus, the central issue was not the relation between logical structures of the formalisms. Rather, given that *Wave Mechanics and Bohr's model agreed with respect to the eigenvalues and stationary energy states, the main question it addressed was whether Wave Mechanics and Matrix Mechanics agreed with respect to eigenvalues and, thus, to stationary states as well.* A successful demonstration was expected to prove the non-*ad hoc* nature of Wave Mechanics's assumptions. The paper also aimed at demonstrating epistemological significance, doubted (by Heisenberg and others in the Göttingen school, and perhaps Schrödinger himself at first) because of its inapplicability to the spectral line intensities.

5. The proof of the domain-specific ontological equivalence—as far as eigenvalues/stationary states go

Although the above-stated central goal of Schrödinger's proof may seem disappointingly modest, one should bear in mind that the importance of elucidating the nature of the "intimate connection" between Matrix Mechanics and Wave Mechanics was only superficially apparent at the time, and might have been insignificant, as the independence of the two theories could have turned out to be more fundamental.[18] In any case, Schrödinger's expression of the "intimate connection" between Matrix Mechanics and Wave Mechanics, rather than his reference to the equivalence of the two "from the mathematical standpoint", indicates the central goal of the proof. That Schrödinger "compared Wave Mechanics and Matrix Mechanics", as M. Bitbol (1996, p. 68) labels the endeavor, was far more important than his attempted proof of isomorphism of the two.

The very *structure of the proof* is best explained if the proof were intended to offer further insight into the agreement between Bohr's model and Wave Mechanics, by constructing suitable matrices from eigenfunctions, thereby demonstrating the "intimate connection" between Wave Mechanics and Matrix Mechanics, and thus, indirectly

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[18] Already in (1926c) while discussing the rotator case, Schrödinger notes the agreement between Matrix Mechanics and Wave Mechanics, with respect to the quantum energy levels: "Considering next the proper values, we get … $E_n = (2n+1)/2\, h\nu_o$; $n = 0,1,2,3,\ldots$. Thus as quantum levels appear so-called "half-integral" multiples of the "quantum of energy" peculiar to the oscillator, *i.e.* the *odd* multiples of $h\nu_o/2$. The intervals between the levels, which alone are important for the radiation, are the same in the former theory. It is remarkable that our quantum levels are *exactly* those of Heisenberg's theory" (p. 31).

showing the significance of their agreement with Bohr's (revised) model.[19] Although considerable empirical agreement between Matrix Mechanics and Wave Mechanics motivated Schrödinger's argument, the equivalence was demonstrated with respect to a feature of Bohr's model (stationary states) of the atom. This model certainly in turn interpreted experimental results, which made it widely accepted, but the equivalence itself was a result that concerned this model (i.e., sub-atomic constitution the model accounted for), and not experimental results more directly.[20]

Schrödinger's paper can be divided into four parts—the introduction, which I have just discussed, and the three parts of the actual proof.[21]

Part 1 of the proof establishes the preliminary connection between Matrix Mechanics and Wave Mechanics. Very early on, Schrödinger emphasizes the limitations placed on his attempt (i.e., quantum conditions). And he explicates the background conditions of the Matrix Mechanics that originate from Bohr's model (i.e., with stationary states and the correspondence rules). He states: "I will first show how to each function of the position and momentum-co-ordinates there may be related a matrix in such a manner, that these matrices, in every case, satisfy the formal calculating rules of Born and Heisenberg (among which I also reckon the so-called 'quantum condition' or 'interchange rule')" (1926a, p. 46). (Briefly stated, the idea was that the interchange rules—that were, initially at least, a condition that stemmed from Bohr's model—correspond to the analysis of the linear differential operators used in Wave Mechanics.)

Thus, since Born–Heisenberg's matrix relation $\mathbf{pq}\text{–}\mathbf{qp} = (h/2\pi i)\mathbf{1}$ corresponds to the Wave Mechanics relation $[(h/2\pi)(\partial/\partial q)]\ q\psi\ -q[(h/2\pi i)(\partial/\partial q)]\ \psi = (h/2\pi i)\ \psi$, a differential operator $F[(h/2\pi i)(\partial/\partial q),q]$ can be associated with the function of momentum and position $F = F(p,q)$. If the phase velocity functions, $u_k = u_k(q)$, in the configuration space of the position q form a complete orthonormal set, then an equation $F_{jk} = \int u^*{}_j[F,u_k]\,\mathrm{d}q$, can be derived that determines the elements of the matrix F_{jk}. Thus, as this argument goes, in this very particular sense, any equation of Wave Mechanics can be consistently translated into an equation of Matrix Mechanics.

Part 2 addresses the pressing issue of whether it is possible to establish the "inner connection" between Matrix Mechanics and Wave Mechanics and, hence, the agreement of both with Bohr's model. This part of the text is the key to the proof, as Schrödinger and others saw it, as it provides the *unidirectional argument for the ontological equivalence* as far Bohr's atom goes—by constructing suitable matrices from eigenfunctions.

Relying on the insights of Part 1, Schrödinger replaces the u_i of the $u_k = u_k\ (q)$ with the eigenfunctions of his wave equation. Thus, he obtains an operator function: $[H,\psi] = E\psi$. The operator's eigenvalues E_k satisfy the equation $[H,\psi_k] = E_k\psi_k$. As it turns out, solving this equation is equivalent to diagonalizing the matrix H.[22]

In *the final and decisive step* of Part 2, Schrödinger demonstrates that the matrices constructed in accordance with the elements of matrix F_{jk} given by the above-stated equation, with the help of some auxiliary theorems, satisfy the Born–Jordan–Heisenberg laws of motion. More precisely, the Heisenberg–Born–Jordan laws of motion

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[19] It was almost certainly understood by others this way, as I will argue shortly.

[20] This is why I label this equivalence "domain-specific" and "ontological" rather than "empirical".

[21] Parts 1 and 2 do not correspond exactly to the original paragraphs of the paper, whereas Part 3 pretty much corresponds to the last paragraph.

[22] In other words, the H turned out to be diagonal with respect to the specified basis (diagonalization of a matrix is a particular orthogonal transformation of the so-called quadratic form, i.e., its rotation).

(Born et al., 1926)—the laws initially derived purely from Matrix Mechanics point of view (Jammer, 1989, p. 221)—are satisfied by (as Schrödinger characterizes the decisive step in the Introduction) "assigning the auxiliary role to a definite orthogonal system, namely to the system of *proper functions* [Schrödinger's italics] of that partial differential equation which forms the basis of my Wave Mechanics" (1926a, p. 46).

The first indication that Schrödinger believes that the main goal was already achieved in Part 2 with the construction of matrices from eigenfunctions, is his claim at the beginning of Part 3 that he "might reasonably have used the singular" when speaking of Matrix Mechanics and Wave Mechanics. *Yet if we believe that providing a proof of the isomorphism between Matrix Mechanics and Wave Mechanics was the central goal of the proof, Part* 3 *of the text must be at least as essential as Part* 2, as it tries (and ultimately fails) to establish the reciprocal equivalence required by such a goal.

Unlike the pressing issue dealt with in Part 2, the issue addressed in Part 3 is an 'academic' (in a pejorative sense of the word) one of isomorphism (or S-equivalence) requiring the proof of reciprocal equivalence. Schrödinger states that "the equivalence actually exists, and it also exists conversely". But he never fully demonstrates this, nor does he make an outstanding effort to do so. Instead, he provides a vague idea of how one might proceed in proving this sort of mathematical equivalence.[23] More precisely, as Muller (1997a, p. 56) correctly pointed out, Schrödinger does not prove the bijectivity of the Schrödinger–Eckart mapping, necessary for isomorphism.[24]

However, Muller (1997a, p. 55) misses the bigger picture when he predicates his reconstruction of the proof on its (the proof's) reduction to the model of mathematical equivalence that seems implicit only in Part 3; this is also true of his brief discussion of the possibility of Schrödinger's proof being a proof of ontological equivalence in Muller (1997b). He leaves out the agreement with Bohr's model of the atom, not realizing that the failure of Part 3 concerning the reciprocal equivalence is not alarming, as it is irrelevant to the central goal. (This is why Muller puzzles over Schrödinger commenting on the subject of bijectivity and reciprocal equivalence in a footnote Muller, 1997a, p. 52.)

In general terms, the constructing of matrices from eigenfunctions in Part 2 becomes meaningful in itself, independently of the reciprocal connection, in light of the final ontological goal of providing a plausible big picture (i.e., Bohr's model). There might be an

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[23] Muller's view of S-equivalence as the central goal of the paper is incorrect—Schrödinger ended the proof *vis à vis* eigenvalues in Part 2, contrary to what Muller believes. The "moments problem" of a function issue referred to in Part 3, has to do with the preliminary discussion of either the full-fledged proof of the isomorphism or S-equivalence (and an attempt to argue for epistemological advantage of Wave Mechanics). Thus, Schrödinger promises "[t]he functions can be constructed from the numerically given matrices" (p. 58). If so, "the functions do not form, as it were, an *arbitrary* and *special* "fleshly clothing" for the bare matrix skeleton, provided to pander to the need of the intuitiveness". In order to show this, he invokes the totality of the "moments" of a function. According to Muller, both this kind of equivalence as well as a weaker version of it, which Muller labels proper S-equivalence and discusses in the Addendum (Muller, 1999) to his two-part paper, cannot be proved. Although he might be correct, both the strong and the weak version of S-equivalence require that the moment mapping is bijective—the requirement invoked only in Part 3 of Schrödinger's proof, after the main goal was (presumably) achieved.

[24] Moreover, Part 3 seems to have a further, arguably more important, ontological rather than logical, goal of demonstrating that Wave Mechanics was more than merely an *ad hoc* convenient tool, a sort of a shorthand for the superior Matrix Mechanics, as it was, in Schrödinger's view, perceived by Heisenberg and others (Bitbol, 1996, p. 68).

alternative explanation of the proof's goal,[25] but such an explanation would have to take into account that Schrödinger (and others in their proofs) insisted on the derivation in Part 2 as central. Also, the insistence on the derivation in this direction made sense especially because Matrix Mechanics was not suitable to account for single states.[26]

Moreover, the mathematical equivalence of Matrix Mechanics and Wave Mechanics would have made sense as the *explanans* and as the key, and perhaps, the only goal of the proof, only if a full-blown empirical equivalence was established. Otherwise, given that the ontological and methodological status of Wave Mechanics and Matrix Mechanics was tentative, the more pressing issue of the relation between Matrix Mechanics, Wave Mechanics and Bohr's model could have been resolved with a "softer" derivation (or, rather, the "construction" of matrices from eigenfunctions[27])—the kind of derivation devised in Part 2.

The key to the proof, then, is its purported demonstration of the formalisms as essential only through their coherence with Bohr's model. It is not clear why Schrödinger might have insisted on a more demanding and what, at the time, seemed a rather academic and esoteric issue, namely, the mathematical equivalence of possibly dispensable formalisms. Taken in historical context, the more tangible demonstration was more desirable, especially because establishing Bohr's model as an acceptable "big picture" did not require bi-directional derivation to prove isomorphism. Although ambiguous in his statement of the central goal of the proof, then, Schrödinger likely gave priority to the ontological goal.

A debate with Bohr that immediately resulted in doubts and later led to even more devastating doubts concerning the applicability of Wave Mechanics, took place around the time of writing the proof paper. As expressed in a letter to Wien shortly before the debate, Schrödinger's optimism was diminishing (also reflected in his second Communication—1926c). This ultimately led him to refocus and to use the soft derivation in Part 2.

Were this not the case, it would be hard to explain the closing passage in the introductory section of the proof, where Schrödinger apparently gets his priorities straight. Although he explicitly emphasizes the construction of matrices from eigenfunctions at the beginning of the paragraph, he only vaguely hints at offering only "a short preliminary sketch" (1926a, p. 47) of a derivation in the opposite direction as well as the relativistic context of the wave-equation in the last section of the paper. Does this mean that Schrödinger was not keen on the (supposed) main goal of his proof? Or could the passage indicate that he perceived the issue as rather academic?

His characterization of the reversed "construction" would be even more surprising if one believed that Part 3 was the key to the proof. Schrödinger (1926a, p. 58) tentatively says, "The following *supplement* [Schrödinger's italics] to the proof of equivalence given above is interesting", before going on to discuss the possibility of the construction of Wave Mechanics from Matrix Mechanics and its implications for the epistemological status of Wave Mechanics.[28]

The assertive tone and the insistence on the exclusiveness and superiority of Wave Mechanics over both old quantum theory and Heisenberg's approach, very explicit in his

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[25] E.g., the equivalence could have be seen as a precursor to the relativistic version of Schrödinger's account, especially because, otherwise, his brief discussion of this issue at the very end of last paragraph seems inserted. Even so, this might not be a competing but rather supplementary goal of the proof.

[26] See quote of Bohr's account on p. 18.

[27] "Construction" is a better word choice than "derivation" in this case, given that the latter might indicate the purely logical nature of the proof. Schrödinger himself uses this notion.

[28] See footnote 23 for the continuation of Schrödinger's discussion.

first Communication (Schrödinger, 1926b), and somewhat toned down in the second (Schrödinger, 1926c), does not characterize the proof paper. In fact, quite the contrary: The tone of the proof paper is defensive. In Part 3, Schrödinger rather cautiously argues that Wave Mechanics may have the same epistemological significance as Matrix Mechanics does, and, judging by the above-cited passage, treats this portion of the paper as secondary. That Schrödinger set out to, first and foremost, demonstrate the significance of Wave Mechanics (motivated by its agreement with Bohr's model), a significance which was doubted because of its failure to account for the spectral line intensities, is in keeping with such a tone.

Even if, despite the above-presented indications to the contrary, Schrödinger were at first undecided as to the main goal of the proof, soon after publishing it and his four Communications, he and the quantum physics community embraced it and its limited ontological goal.

Thus, two years after the publication of his seminal work, in his correspondence with others, he continued to discuss the application of the Wave Mechanics and its meaning. Moreover, judging from the following excerpt from Bohr's 1928 letter to Schrödinger, the key issue was still the nature of the agreement of Wave Mechanics and Matrix Mechanics with Bohr's (revised) model. Bohr is still concerned with an (implicit) assumption of Matrix Mechanics regarding stationary states as a limitation on the applicability of Wave Mechanics:

> In the interpretation of experiments by means of the concept of stationary states, we are indeed always dealing with such properties of an atomic system as dependent on phase relations over a large number of consecutive periods. The definition and applicability of the eigensolutions of the wave equation are of course based on this very circumstance (emphasis added; Bohr's letter to Schrödinger (May 23, 1928), in Bohr, 1985, Vol. 6, p. 49).

In his letter to Pauli, dated June 8 1926 (Pauli, 1979, p. 328), in which he discusses visualizibility of Wave Mechanics, Heisenberg stated the following: "[T]he great accomplishment of Schrödinger's theory is the calculation of matrix elements". Although Heisenberg's sense of epistemological superiority of Matrix Mechanics over Wave Mechanics may have contributed to his attitude, the remark seems to show his understanding of the limited goal of the proof. If Heisenberg believed that anything like mathematical equivalence was proved, it would be rather puzzling to praise the success of what (he would have believed) was only the first step in such a proof.[29]

It is also important to compare Schrödinger's effort with similar efforts by others. For instance, in his letter to Jordan (12 April 1926), Pauli talks about "a rather deep connection between the Göttingen mechanics and the Einstein–de Broglie radiation field" (Mehra & Rechenberg, 1982, p. 656). He thinks he has found "a quite simple and general way [to] construct matrices satisfying the equations of the Göttingen mechanics",

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[29] One might also argue that once Heisenberg thought the mathematical equivalence was proved, the only open question, addressed in his remark, was which of the two theories provided better interpretation and understanding (in terms of the "visualization") of relevant micro-physical phenomena. Yet this approach can hardly make sense of his emphasis on the calculation of matrices from Wave Mechanics. Alternatively, Heisenberg might have been concerned with such a question but did not believe that the mathematical equivalence was proved: thus, he could be pondering here whether the proved limited equivalence can help address the question (although it is not clear at all how it could).

a description of the proof's goal which is analogous to the moderate goal of Schrödinger's proof. It is also striking to what extent the use of Bohr's model was critical in constructing Pauli's proof and the proofs of others.[30]

6. Conclusion

Thus, the 1920s agreement on equivalence appears to be an agreement on a 'myth' only if we leave out the ontological goal of providing a coherent overall model of the atom, and focus solely on the purely formal goal. However, only at a later stage of development was the proof worked out in the terms which Muller's historical and conceptual analysis takes to be central to the 1920s agreement. And although the equivalence of the 1920s was perhaps more provisional than that of the 1930s, it was justified by virtue of its ontological aim.

It is not at all clear, however, that the proof of the equivalence provided by von Neumann in the 1930s could have settled the issue at the time of the appearance of Schrödinger's proof, given the tentative standing of the formalisms. As Hanson notes, "von Neumann's theory was a splendid achievement. But it was also a precisely defined mathematical model, based on certain arbitrary, but very clearly stated assumptions concerning quantum theory and its physical interpretation" (Hanson, 1963, p. 124). Moreover, the well-known scattering phenomena could not be formulated in a satisfying way within the ramifications of his approach at the time.[31]

In the stage of the development of Quantum Mechanics at which the first set of equivalence proofs was provided, the community of quantum physicists was keen on severe experimental testing of the corpuscular and wave mechanical hypothesis concerning the microphysical processes and their implications. Only after the experiments were judged to have provided satisfying results with respect to the available theoretical accounts (Bohr's model, Matrix Mechanics and Wave Mechanics) did the development of the theory enter the next stage, where an answer to the question of mathematical equivalence of the two formalisms became significant.

Later commentators understood Schrödinger's proof in the same spirit as that of von Neumann (and Muller is right in claiming this) because of the changing tide in quantum physics. The second stage of the quantum revolution had already begun, and physicists concentrated their efforts on the formal aspects of research, grounded on firmly established experimental results. But we should not confuse the subsequent equivocation with the actual understanding of the goals in the 1920s quantum physics community. It is a mistake to judge these two stages of the development of quantum theory by a criterion that applies only to the second stage.

Assuming that the proof was justifiably perceived as a breakthrough, what is the moral of the story? How did this affect the understanding of quantum phenomena at the time? What was the importance of the proof, given its domain-specific ontological goal?

Here, Pauli's attitude regarding the results of his own proof are informative. After presenting the relation between Matrix Mechanics and Wave Mechanics in his letter to Jordan, he concluded that, "from the point of view of Quantum Mechanics the

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[30] See Mehra & Rechenberg (1982, p. 657) on Pauli's proof, D'Abro (1951, p. 874) on Dirac's, Mehra & Rechenberg (1982, p. 150) on Heisenberg's & Scott (1967, p. 57) on the proof of Eckart's.

[31] Jammer (1989, p. 335) also points out some of the difficulties with von Neumann's approach.

contradistinction between 'point' and 'set of waves' fades away in favor of something more general'' (Mehra & Rechenberg, 1982, p. 657). This is strikingly similar to the complementarity view devised by Bohr in response to the same developments.

Also, although at the time there was still a lack of the agreement on the full-fledged empirical equivalence, the proof demonstrated that the two approaches added up to a coherent account of the atom—at least as far as the known facts went.

In order to appreciate the relevance of this point, it is important to understand that interpretations, formalisms, and the relevant experiments were closely related aspects of the same endeavor. Disentangling them by introducing rigid distinctions might misguide us in our attempts to reconstruct the relevant views and arguments. Both the development of Quantum Mechanics and its interpretation were closely dependent on the experimental results: The view of the interpretation(s) arising from the theory, and the theory arising from the experiments, is misleading. It is more accurate to say that all three components informed each other.

Thus, the roots of what has become the Copenhagen Interpretation might be found, to a great extent, in the domain-specific ontological equivalence of Matrix Mechanics and Wave Mechanics, not in the manufacturing of consent among physicists and philosophers. If we leave out Bohr's model as the background to the proof (s) and concentrate on the equivalence as a purely mathematico-logical issue, the loose agreement represented by the Copenhagen Interpretation seems to have been enforced whether or not the mathematical equivalence was actually proved.[32] If, however, we take into consideration both Bohr's and Schrödinger's accounts yet another notion of equivalence comes to the foreground, one which helps us make sense of Schrödinger's rather complex goals for his equivalence paper and its reception at the time. But it might also force us to rethink the nature of the gradual convergence of the community of physicists on Bohr's interpretation, as it might have been predicated on the proof of this modest and perhaps mathematically less tractable equivalence.

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[32] See p. 4.

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