Hilbert-Style Axiomatic Completion: On von Neumann and Hidden Variables in Quantum Mechanics

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Introduction

Von Neumann's (in)famous proof of the non-existence of hidden variables in quantum mechanics is commonly discussed in the context of work that came much later, namely that of Bohm and Bell. In this context, the goal of specifying a set of axioms is to identify *only* what is essential for *any* quantum theory. Call this axiomatic reconsideration. Thus, given that Bell (rightly) criticized one of von Neumann's assumptions, the story goes that von Neumann made a grave error; even worse, von Neumann thereby *erroneously* claimed to have ruled out hidden variables.

This story is wrong—or, so I argue. However, in the main, I do not disagree either on the historical, physical, or mathematical facts. Indeed, excellent exegetical work has already been done on von Neumann's work in physics [Duncan and Janssen, 2013] [Lacki, 2000] [Rédei, 1996] [Rédei, 2006] [Rédei and Stöltzner, 2006] [Stöltzner, 2001] [Bueno, 2016], including on his no hidden variables proof [Bub, 2011] [Bub, 2010] [Dieks, 2017] [Mermin and Schack, 2018] [Stöltzner, 1999] [Acuña, 2021a] [Acuña, 2021b]. Instead, my disagreement concerns primarily the framing, which lumps von Neumann in with Bohm and Bell (especially the latter). Here I argue that von Neumann was performing an *axiomatic completion* of quantum mechanics, where 'quantum mechanics' refers to a *specific* theory of quantum phenomena rather than, vaguely, to *any* theory of quantum phenomena.[1] This axiomatic completion relied on Hilbert's axiomatic

[1]In what follows I will use 'quantum theory' to refer to what we today call 'quantum mechanics'

method. With this understanding at hand, I re-interpret the history of von Neumann's no hidden variables proof.

The argument proceeds as follows. In §1, I give an overview of the axiomatic endeavors foreshadowed in [Hilbert et al., 1928]. Here I emphasize two features of the Hilbertian axiomatic method: (1) it requires the separation of the facts of an area of knowledge from the formalism and is thereby *provisional* and (2) it is a preeminently *practical* method aimed at *ordering* and *orienting* areas of knowledge. In §2, I describe the history of quantum theory that immediately preceded Hilbert et al. [1928]. Here I focus especially on the influence and status of the transformation theory as developed by Dirac and Jordan. In §3, I re-interpret von Neumann's work in 1927 as the prelude to his axiomatic completion of quantum mechanics, where the latter is understood as the work coming out of the Göttingen—Cambridge tradition. Here my central claim is that his 1927 work aimed to lay the foundation for answering the extant question of whether the transformation theory could be extended. I therefore also briefly discuss two little-known debates, one between von Neumann and Schrödinger and another between Jordan and Heisenberg (and von Neumann), on the status of hidden variables. In §4.1, I show that von Neumann's 1932 book made his use of the axiomatic method—including its character as provisional and relative—explicit; in this sense, *nothing was deeply hidden* concerning his motivations. In §4.2, I briefly revisit the infamous proof of IV.1 and IV.2 to show how it is to be read as an axiomatic completion. Finally, I conclude by discussing the legacy of von Neumann's axiomatic completion of quantum mechanics insofar as it oriented later inquiry.

1 Axiomatic Completions: Provisional and Practical Meta-Mathematics

A common misconception of Hilbertian axiomatics holds that it promoted formalization of scientific and mathematical theories in the service of radical epistemological or metaphysical goals. Wilson [2017, 151], for instance, has repeatedly suggested that the program intended to reveal the basic metaphysics of theories, analogous to later intentions for rational reconstruction. Similarly, Lacki [2000, 315] characterizes Hilbert's interest in axiomatization as residing "in his care for logical clarification and rational

and reserve 'quantum mechanics' for its historical referent, i.e., the cluster of work that grew up in Göttingen and Cambridge.

reconstruction." This comparison is meant to damn Hilbert insofar as rational reconstruction is widely considered a failure. Yet this comparison trades on half-truths about Hilbert's axiomatic method. Indeed, closer inspection reveals that Hilbert's axiomatic method was both provisional and practical in such a way that it contributed essentially to scientific progress, as I will show here for quantum theory.

The core feature of the axiomatic method comes in Hilbert's maxim to "always keep separated the mathematical apparatus from the physical content of the theory" [Lacki, 2000, 313].[2] Concerning quantum theory in particular, we find an expression of this early in [Hilbert et al., 1928], which contrasts the ideal (non-obtaining) way to a quantum theory[3] with the actual way the quantum theory had been delivered. Ideally,

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[2]Lacki seemingly understands this to be synonymous with the imperative to delimit "as close as possible[...]what are the minimal assumptions on which to secure its [quantum theory's] foundations, assumptions which should be sufficiently beyond any doubt so that one could consider them safely as not subject to further revision" [Lacki, 2000, 313]. Note that were the axiomatic method to demand "assumptions sufficiently beyond any doubt," then Hilbert's axiomatization of geometry would have been an abject failure. Here, it will become clear that we need not assume von Neumann was any different from Hilbert on this.

[3]I quote at length here in the footnote as the passage is otherwise unavailable in English; all translations are mine, unless specified [von Neumann, 1963, 105]:

> The way to this theory is as follows: Certain physical demands on these probabilities are suggested by our past experiences and trends, and their satisfaction necessitates certain relations between the probabilities. Secondly, one seeks a simple analytic apparatus in which quantities occur that satisfy precisely the same relations. This analytic apparatus, and with it the operands occurring in it, now undergoes a physical interpretation on the basis of the physical demands. The aim in doing so is to so fully formulate the physical demands that the analytic apparatus is uniquely defined. This way is thus that of an axiomatization, as has been carried out, for example, with geometry. Through the axioms the relations between the elements of geometry, point, line, plane, are characterized and then it is shown that these relations are exactly satisfied by an analytic apparatus, namely the arithmetic equations.
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> In the new quantum mechanics, one formally assigns a mathematical element, which is in the first instance a mere operand, as representatives according to a certain specification of each of the mechanical quantities, but from which one can receive statements about the representatives of other quantities and thus, through back-translating, statements about real physical things.
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> Such representatives are respectively the matrices in the Heisenberg, the q-numbers in the Dirac, and the operators in the Schrödinger theories and their present developments.
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> It is therefore important to note that we examine two wholly different classes of things, namely on one hand the measurable numerical values of physical quantities and on the other their assigned operators, which are calculated with strictly according to the rules of quantum mechanics.
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> The above suggested procedure of axiomatization is not typically followed in physics now, but rather is the way to the erection of a new theory, as here, according to the following principles.

a quantum theory would be arrived at by first characterizing the physical demands on probabilities and the necessary relations among them, and only then identifying the analytic apparatus uniquely defined by these relations. In short, the theory would be an axiomatization of the sort Hilbert carried out for geometry. However, the quantum theory was actually arrived at by first supposing an analytic apparatus and then interpreting that formalism. This was a significant barrier to understanding the theory because "one cannot sharply distinguish between[...] the formalism and its physical interpretation" [von Neumann, 1963, 105].

The axiomatic method's aim to distinguish a formalism from the physical facts motivating it served the larger goal of understanding how a mathematical theory is applied to the world.[4] Though perhaps sounding too philosophical to be of interest to scientists themselves, this larger goal was not exclusive to Hilbert. Einstein, for instance, also wanted to answer the "riddle" of the relationship between mathematical theories and reality; indeed, he even shared Hilbert's belief that the axiomatic method had solved this riddle [Einstein, 1921, 3–4]:

> So far as the propositions of mathematics correspond to reality, they are not certain, and so far as they are certain, they do not correspond to reality. Complete clarity on the situation seems to me to have come into the community's possession only through the method of mathematics known by the name of "Axiomatics." The progress achieved by the axiomatic method consists in the fact that it cleanly separates the logical-formal from the factual or intuitive content; only the logical-formal is the subject of

> More often than not, one supposes an analytic apparatus before one has yet specified a complete system of axioms, and then arrives at the establishment of the basic physical relations only by interpretation of the formalism. It is difficult to comprehend such a theory when one cannot sharply distinguish between these two things, the formalism and its physical interpretation. This divorce should here be made as clearly as possible, when we also, in accordance with the present state of the theory, don't yet wish to found a complete axiomatics. In any case, what is certainly well-situated is the analytic apparatus, which—as purely mathematical—is also capable of no modification. What can, and probably will, be modified about it is the physical interpretation, with which exists a certain freedom and arbitrariness.

[4]I refer to 'physical facts' rather than 'physical interpretation' throughout to avoid conflation with our modern notion of philosophical interpretation, which relies on a pseudo-model-theoretic understanding of theory–world relations that was likely unavailable at the time Eder and Schiemer [2018] and, at any rate, is not compatible with Hilbert's above description of his axiomatization of geometry. Besides, 'physical facts' better conforms to Hilbert's account (especially "Axiomatische Denken" [Hilbert, 1917]) as well as the broader physics community's account (e.g., "Geometrie und Erfahrung" [Einstein, 1921]) of axiomatization.

mathematics according to the axiomatic method, but not the intuitive or other content connected to the logical-formal.

Yet, to answer to this larger goal of understanding how mathematics relates to the world, the axiomatic method cannot proceed in the usual mathematical way.

The usual method of mathematics—of determining the consequences of given propositions—is insufficient for addressing its relationship to reality. Instead, addressing the latter is the domain of *meta*-mathematics. The distinction can be spelled out using the example of Hilbert's axiomatization of geometry.[5] By the end of the 19th Century, Euclidean geometry had come to be identified as the combination of a few key propositions (e.g., Pascal's and Desargue's theorems). However, it was unclear which of the many "physically intuitive" basic assumptions on offer were strictly necessary to derive these propositions. In particular, it was asked whether Archimedes' Axiom—i.e., for any previously given line segment CD, every line segment AB can be repeated such that the length of that segment exceeds CD—was necessary for Desargue's theorem:[6] while it was believed that Archimedes' Axiom was "physically intuitive," owing especially to the acceptance of of its apparent consequences which seemed to conform to reality, it was unclear whether it was strictly necessary for Euclidean geometry. Enter Hilbert, the meta-mathematician. Taking Euclidean geometry (Pascal's and Desargue's theorems) as the target, Hilbert demonstrated which axioms were necessary for its recovery. He did this by removing the merely-empirical content of Euclidean geometry, which involved translating geometric statements into a formalism whose necessary assumptions had already been elucidated (the arithmetic equations); with the formalism's structure

[5] The account of Hilbert's axiomatic method that follows is my own. However, it is similar in important respects to especially [Baldwin, 2018, chap. 9], [Detlefsen, 2014], [Peckhaus, 2003], and [Corry, 2004], as well as [Hallett, 1990][Hallett, 1994][Hallett, 2008], [Sieg, 2014], and [Wilson, 2022]. For entry into interpreting Hilbert's *Grundlagen der Geometrie*, see [Giovannini, 2016] and [Eder and Schiemer, 2018]. For an account that emphasizes more of the foundationalist aspect of the axiomatic method as used in science, based on Hilbert's work related to general relativity, see [Brading and Ryckman, 2008][Brading and Ryckman, 2018], as well as [Brading, 2014] and the editors' remarks in [Sauer and Majer, 2009].

[6] I discuss Archimedes' Axiom here to avoid some of the messiness of the history of the parallel postulate. However, I take it that the same point applies to the parallel postulate—e.g., [Eder and Schiemer, 2018, 66–7] (italics mine): "...lacking the precision of an exact axiomatization and a methodologically clean understanding of what is at stake when we ask ourselves about the independence of the axiom of parallels, these results [i.e., non-Euclidean geometries] were still hotly debated among philosophers. This is certainly due in part to the empirical content people associated with geometry and the fact that matters of logical consequence were mixed up with matters of empirical truth." Analogous to the body text, then, my point is that axiomatizing the full set of empirical truths (as then understood) allowed for the eventual trimming of the axiom of parallels precisely because the axiomatization laid bare its logical consequences in the context of the other axioms.

already clear, the axiom candidates for Euclidean geometry could be translated into the formalism's language and their relationship precisely characterized. This done, Hilbert was able to show that Archimedes' Axiom is necessary for Desargue's theorem—hence Euclidean geometry—since it is independent of the other axioms. Thus, crudely put, where the mathematician asks what propositions *a given set of axioms* suffice to prove (perhaps adding the occasional axiom), the meta-mathematician turns this around (using the axiomatic method) to ask which axioms are necessary to prove *a given set of propositions*.

Thus, the meta-mathematician can address the relationship of mathematics to reality in a way that the mathematician alone cannot: if one identifies theorems central to an area of knowledge, the axiomatic method allows the meta-mathematician to (eventually) identify the necessary physical assumptions. As we see from Hilbert's axiomatization of geometry, one must execute three major steps. First, identify the central theorems (Pascal's and Desargue's theorems) and concepts (connection, order, parallels, congruence, continuity) of the area of knowledge (Euclidean geometry); this constitutes the theory of the area of knowledge. Second, identify a formalism whose axiomatic structure is clear (or can be made clear) and into which the theory of the area of knowledge can be translated (the formalism of arithmetic equations). Third, determine the necessity of candidate axioms (Archimedes' axiom) for the identified theorem(s) (Desargue's theorem) by leveraging knowledge of the axiomatic structure of the formalism (arithmetic equations). Call the consideration of a candidate axiom's relation to the other axioms within the formalism its *uniqueness question*: are the other axioms sufficient for deciding the structure of the formalism w.r.t. the candidate axiom? In other words, is the candidate axiom a consequence of the others in the formalism? If it is a consequence of the others, then the formalism is unique for the theory; if it is not, we say that the formalism is not unique for the theory.[7] Answering all extant uniqueness questions constitutes an *axiomatic completion* of the theory of the area of knowledge in the sense that theory's axiomatic structure has been completely determined.

Two features of axiomatic completions are relevant for what follows. These are especially clear in Hilbert's "Axiomatische Denken" Hilbert [1917]. First, axiomatic

[7]The uniqueness question for Archimedes' axiom told us that the formalism of the arithmetic equations was not unique insofar as it was independent of them. Within the formalism, this amounted to the assumption of an additional property attributable to the real numbers over and above the real-number-like properties had by the arithmetic equations.

completions generate provisional representations of reality insofar as axiomatic completions rely on several fallible steps. On the one hand, the theory of a field of knowledge consists in the identification of central theorems and an ordering of the field's facts "with the help of a certain truss [Fachwerk] of concepts"[8] [Hilbert, 1917, 405]. As happened with Euclidean geometry, the theorems believed true or concepts thought appropriate could change; thus, any theory built upon them must be provisional. On the other hand, which uniqueness questions arise and—should the formalism not prove unique—how to respond to them is not straightforward. Were a new uniqueness question arise, or new insight into an axiom candidate gained, then the theory of the area of knowledge may require adjustment to maintain its faithfulness as a representation of the area of knowledge.

Second, axiomatic completions are practical: they are a tool meant to generate helpful representations of a field of knowledge. Indeed, the axiomatic method demands that uniqueness questions be answered precisely because this serves the practical goal of orienting and ordering a field of knowledge [Hilbert, 1917, 407](bold added):

> Should the theory of a field of knowledge—i.e., the truss of concepts whose goal is to represent it—serve its express purpose of **orienting** and **ordering**, then it surely must meet two standards especially: *firstly* it should provide a survey of the dependence resp. independence of the propositions of the theory and secondly a guarantee of the lack of contradictions among the propositions of the theory. In particular the axioms for each theory are to be examined from these two perspectives.

So practical is the goal that even *mutually inconsistent* axiomatizations are an acceptable—indeed, laudable—outcome of axiomatic completions. Consider, as does Hilbert, the field of Lagrangian mechanics. Hilbert presents the combination of Boltzmann's axiomatization *and* Hertz's axiomatization of Lagrangian mechanics as "a deeper layer

[8]While typically translated as "framework," I think its meaning is better captured by "truss." In English-language philosophy these days, "framework" and "system" are often taken to be synonymous, hence we typically assume that frameworks are fairly fleshed-out or robust affairs. But I think this is a mistaken assumption in German, and particularly here: if we instead understand "Fachwerk" as more like "truss"—as a first-pass support, upon which a more robust framework is built—then we may fairly assume that a "Fachwerkes der Begriffe" is more of a stepping-stone on the way to a system of axioms, meaning that concepts (in their informal state) go through a vetting process before being precisified in an axiomatic system. My intention in drawing this distinction is to highlight that the concepts are not necessarily the invention of the (mathematical) theoretician, but rather are often provided by the scientists or other experts of the area in question, and they are merely codified more rigorously by the mathematician. (Obviously, rigor in the spirit of Hilbertian axiomatics.)

in the advancing axiomatization of mechanics" [Hilbert, 1917, 408], despite Hertz's complete eschewal and Boltzmann's complete reliance on a notion of (Lagrangian) force as a truss-concept.[9] Insofar as they are both legitimate representations of the field according to Hilbert (for contributing to the formation of a deeper layer), these representations must be earning their legitimacy through their orienting-and-ordering service to the field. Put simply: axiomatic completions are successful just so far as they order and orient a field of knowledge and its mathematical investigation.

2 Quantum Phenomena Get a Theory and a Formalism (Sort of)

Von Neumann was introduced to quantum theory via Hilbert. The problems faced then, and Hilbert's way of understanding and approaching them, significantly shaped von Neumann's early work in the area (up to and including his book). Von Neumann's first serious contact with quantum theory was in Hilbert's 1926–7 Winter term lectures, which, through his own and Nordheim's efforts, became [Hilbert et al., 1928]. Here I summarize this work and the developments immediately preceding it, showing that the field was in the midst of acquiring a truss of concepts (the statistical viewpoint of quantum mechanics) and central theorem (Born rule), which would later combine to form a theory of quantum phenomena, as well as a formalism (transformation theory). Along the way, there developed a uniqueness question regarding the statistical interpretation of the formalism.

As this work was being written—in early 1927—quantum theory faced several problems. In the previous two years, there had arisen not one, but two[10] calculational techniques for predicting quantum phenomena: matrix (quantum) and wave (undulatory) mechanics.[11] Each had met with some predictive success. However, the two calculational techniques appeared fundamentally different on their face. In fact, the two theories were then known to differ in rather significant ways, both mathematically and

[9] See Eisenthal [2021] for a recent discussion of Hertz's treatment of the notion of force.

[10] Really, four, but I will follow the usual convention of ignoring Born and Wiener's operator mechanics and Dirac's q-numbers.

[11] The usual list of Göttingen matrix mechanics publications includes [Heisenberg, 1925] [Heisenberg, 1926] [Born and Jordan, 1925] [Born et al., 1926]; English translations for three of these can be found in [van der Waerden, 1967]. The usual list for wave mechanics includes Schrödinger's four "Quantisierung als Eigenwertproblem" papers and [Schrödinger, 1926], which can all be found in [Schrödinger, 1927b] (English translation: [Schrödinger, 1927a]).

physically. To put it mildly, quantum theory was a mess. In arguing that the two were not, in fact, equivalent, Muller [1997a, 38] [Muller, 1997b] [Muller, 1999] points out that: wave mechanics could not describe the evolution of physical systems at all, and matrix could do so only for periodic phenomena; matrix mechanics lacked a state space; the Euclidean space and charge-matter densities of wave mechanics had no correlate in matrix mechanics; and matrix mechanics quantized the electromagnetic field, while Schrödinger deemed this unnecessary. None of these differences were hidden from view, and their most significant difference—the apparent discreteness of matrix mechanics versus the apparent continuity of wave mechanics—was often discussed.

Yet despite these differences, the two calculational techniques led to the same answers in a number of elementary problems. This was considered a promising development by many. Schrödinger himself, in addition to Sommerfeld, was quickly convinced that wave mechanics was equivalent (or at least, made matrix mechanics superfluous) [Mehra and Rechenberg, 1987, 638–9]. As he wrote in a 22 February, 1926, letter to Wien (translation in Mehra and Rechenberg), he was "convinced, along with Geheimrat Sommerfeld, that an intimate relation exists," and, despite not being able to find the relation himself, expressed his firm hope "that the matrix method, after its valuable results have been absorbed by the eigenvalue theory, will disappear again." (Note, too, that Schrödinger here refers to his theory as an eigenvalue theory. This will be important in §3.) Despite telling Wien that he had given up looking for the connection, we know he did not. Not long after—somewhere between one and four weeks later—Schrödinger managed to prove that, in a limited sense, wave mechanics was the same as matrix mechanics.[12]

In the wake of matrix and wave mechanics there arose the transformation theory, developed by Jordan, Dirac, and London. The transformation theory brought with it three things that are significant here. First, it resolved lingering questions concerning the relationship of the various quantum calculi: they were all equivalent, as far as the transformation theory was concerned. Mehra and Rechenberg, in fact, conclude their discussion of the transformation theory by quoting what Oskar Klein later told Kuhn:

[12]See [Perovic, 2008] on the goal of Schrödinger's proof. As there noted [Perovic, 2008, 459], von Neumann seems to take Schrödinger to have demonstrated the mathematical equivalence of the two theories, contrary to what Perovic claims Schrödinger was after. However, note that von Neumann later clarifies (I.4, fn. 35) that Schrödinger had *not* established full equivalence of the two spaces of functions. Also, see [Muller, 1997a, 54–5] for an argument that, in actuality, what Schrödinger had done was *expand* wave mechanics to *make* it equivalent to matrix mechanics [Muller, 1997a, 54–5], thereby setting himself on the path to quantum mechanical hegemony.

the transformation theories of Jordan and Dirac "were regarded as the end of the fight between matrix and wave mechanics, because they covered the whole thing and showed that they were just different points of view" [Mehra and Rechenberg, 2001, 89]. Unsurprisingly, the language physicists used changed, too, so that 'quantum mechanics' came to refer not just to matrix mechanics but also to those calculi captured in the transformation theories, as well as the transformation theories themselves. This is seen already in the early presentations of transformation theory by [Jordan, 1927, 810] and [Dirac, 1927, 621], each of whom: uses 'quantum mechanics' to refer loosely to the various calculi (but clearly not intending to capture any unconceived alternative "theories" of quantum phenomena); refer to Heisenberg's quantum mechanics instead as 'matrix mechanics'; and call wave mechanics, for instance, a "representation" (resp. for Jordan, "Form"). Thus, the theory of *quantum mechanics* is the one arising specifically through the transformation theory.

Second, the transformation theory replaced the morass of interpretation-adjacent mathematical questions plaguing the various forms of quantum mechanics with essentially one. Where before matrix and wave mechanics faced related but distinguishable questions about the validity of their calculi's methods, transformation theory faced instead the single question of the domain of validity of Dirac's delta function. This is reflected in [Hilbert et al., 1928] [von Neumann, 1963, 105], wherein the "formulation of Jordan's and Dirac's ideas," they say, "[becomes] substantially simpler and therefore more transparent and more easily understandable." One presumes that it is this simplicity, transparency, and understandability that makes the analytic apparatus, as they said at the outset, "well-situated" and "capable of no modification" in its capacity as pure mathematics [von Neumann, 1963, 106]. That is, the apparatus they present is *rigorous enough* that they accepted it as broadly correct. Yet it was not *entirely* without problems. Throughout their paper, Hilbert et al. had used Dirac's delta function as a shortcut to get their operator calculus to display the correct (discrete) behavior when necessary. Others, notably Dirac, had done this as well. They considered this a problem "since one is never sure to what extent the operations appearing are really to be permitted." Indeed, it was not *only* a problem of mathematical rigor in their mind because it impugned even their treatment of statistical weights[13]: if the rigorous method departs significantly from that using the Dirac delta, this might also expand the allowable states via an expansion of the solutions to the eigenvalue problem.

[13] Here they refer to von Neumann's [von Neumann, 1927c] as addressing the question of mathematical rigor.

This brings us to the last change wrought by the transformation theory, namely, bringing Born's [Born, 1926a] [Born, 1926b] statistical interpretation of Schrödinger's wave function to new heights of importance through its generalization. Jordan [1927, 811], for instance, apparently drawing on ideas from Pauli [Duncan and Janssen, 2009, 20–1], made it quite explicit that his formalism was to be interpreted statistically using a probability amplitude function applied to two Hermitian quantum-mechanical quantities. The centrality of the statistical interpretation is clear in Hilbert et al., too.[14] They begin the paper as follows [von Neumann, 1963, 105]:

> The basic physical idea of the whole theory consists in bringing to light the general probability relations in patches of rigorous functional relationships in ordinary mechanics.
>
> The nature of these relationships is best explained through a particularly important example. If the value W_n of the energy of the system is known, and namely equal to the n-th eigenvalue of the quantized system, then following Pauli the probability density that the system coordinate has a value between x and $x + dx$ is given by $|\psi_n(x)|^2$, where ψ_n is the eigenfunction associated with the eigenvalue W_n.

But while this understanding begins as an example, it ends as an instance of the general theory [von Neumann, 1963, 131]. The mathematicians, however—especially Jordan and von Neumann—wondered which states are allowable in this apparatus, i.e., whether there are states that remove this merely-statistical character from the transformation theory. More precisely, the question was whether these states were excluded by fiat as an additional quantum-mechanical assumption (Jordan's contention), or rather that they were excluded already by the assumptions necessary for building up the transformation theory (Heisenberg and, I suggest here, von Neumann's contention)[Beller, 1985, 346–7]. Consequently, the transformation theory led to a uniqueness question concerning merely-statistical predictions in the transformation theory, at least for some.

Thus, the situation was this as von Neumann began his own work on quantum mechanics. First, 'quantum mechanics' meant the concepts built upon the transformation theory. Second, there appeared to be a need for a theory of the Dirac delta and its

[14] Further, the transformation theory—and Dirac's and Jordan's thoughts thereupon—seemingly had an influence on Heisenberg's articulation of the uncertainty principle in his [Heisenberg, 1927] [Beller, 1985]. However, also see fn. 252 of [Mehra and Rechenberg, 2001, 210].

ilk.[15] Third, Born's statistical interpretation was assumed to interpret the transformation theory, and, for Jordan and von Neumann especially, there immediately came the question of the uniqueness question concerning merely-statistical predictions in the transformation theory. It was understood by them that a theory of the Dirac delta might shed light on this question.

3 Von Neumann's Prelude to the Axiomatic Completion of Quantum Mechanics

One of von Neumann's aims primary goals in 1927 was to answer the uniqueness question for quantum mechanics. However, preliminary work on both the mathematical and physical front was required before an axiomatic completion could be executed.

On the mathematical front, the transformation theory was still plagued by the "unrigorous" Dirac delta. More immediately, the occurrence of the Dirac delta in the transformation theory meant that the latter was not yet fully formed mathematically. This von Neumann meant to tackle in his "Mathematische Begründung der Quantenmechanik" [von Neumann, 1927c][von Neumann, 1963, 153]:

> [In the transformation theory] [i]t is impossible to avoid including the improper eigenfunctions (see §IX); such as, e.g., $\delta(x)$ first used by Dirac, which is supposed to have the following (absurd) properties: $\delta(x) = 0$,for $x \neq 0$, $\int_{-\infty}^{\infty} \delta(x)\,dx = 1$.
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> ...But a common deficiency of all these methods is that they introduce in-principle unobservable and physically meaningless elements into the calculation[...]. Although the probabilities appearing as final results are invariant, it is unsatisfactory and unclear why the detour through the non-observable and non-invariant is necessary.
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> In the present paper we try to give a method to remedy these shortcomings, and, as we believe, to summarize the statistical standpoint in quantum mechanics in a uniform and rigorous way.

[15] Gimeno et al. [2020] argue that, in fact, it was reluctance to use (functions like) the Dirac delta function that doomed Born and Wiener's operator calculus because, without it, they could not solve the problem of linear motion, which was its intended purpose. See [Peters, 2004] for more on the status of (functions like) the Dirac delta at the time.

Two things matter from this work. First, von Neumann placed the transformation theory on a rigorous mathematical footing. In so doing, he entirely avoided the Dirac delta function and, hence, showed that it was irrelevant to the predictive formula of quantum mechanics (the Born rule, occurring as the trace rule in von Neumann). That is, not only did quantum mechanics now have a *uniform* formalism, the Dirac delta was demonstrably irrelevant to the uniqueness question. Second, the Hilbert space formalism afforded a rigorous demonstration of the centrality in quantum mechanics of solving eigenvalue problems. This latter was important insofar as it was on the basis of a non-rigorous analogy between the eigenvalue problems in each of matrix and wave mechanics, the two dominant pre-transformation theory formalisms, that the two were believed to be equivalent. With these two accomplishments on the books, the axiomatic method now had a proposition it could target for recovery: the trace rule.

Yet on the physical front, it was still not clear what the "truss of concepts" for quantum mechanics was, i.e., which concepts and assumptions were considered essential in applications of quantum mechanics. This had two parts. First, the diversity of mathematical approaches and sheer novelty of quantum mechanics made it unclear which physical principles and methods of reasoning were being relied upon. In particular, did quantum mechanics assume, as an additional assumption, the exclusion of hidden variables? Or, rather, was this a consequence of more basic quantum mechanical assumptions? While this consideration does not show up in von Neumann's 1927 works, there is reason to believe it was on his mind. We know that in the early weeks of 1927, there was disagreement among the Göttingen and Cambridge theorists regarding the completeness of the quantum mechanical formalism, which played into the debate regarding whether quantum mechanics could be deterministic. Jordan, for one, apparently thought that the states of quantum mechanics could be further specified so that the theory would be determinate [Beller, 1985, 346–7]. Von Neumann would have been aware of this as he wrote his 1927 papers. Moreover, as Wigner recounts later, von Neumann engaged in a similar debate with Schrödinger around the same time, concluding opposite Jordan and Schrödinger [Wigner, 1970, 1009].[16] Thus, there

[16] Several facts point to this argument having occurred sometime between von Neumann's arrival in Berlin and the writing of his book. First, von Neumann appears not to have communicated with Schrödinger prior to Berlin, as he asked Weyl to describe his work to Schrödinger in an effort to win the assistantship to Schrödinger (Letter of 27 June, 1927). Besides, von Neumann's communications with Weyl suggest that von Neumann was not sufficiently familiar with quantum theory prior his time in Göttingen. Thus, the argument did not precede his move to Berlin in Summer 1927. Second, the argument seems to have taken place in person: for one, Wigner seems to have intimate knowledge

was doubt enough to justify investigating what was the truss of concepts for quantum mechanics and, moreover, the status of hidden variables with respect to that truss. In this way, the situation was analogous to the status of Archimedes' Axiom prior to Hilbert's axiomatization of geometry.

Second, the relationship between the quantum mechanical truss of concepts—whatever they were—and the transformation theory was unclear. Nevertheless, von Neumann's identification of the Born rule (in the form of the trace rule) as the central proposition of the transformation theory—and, hence, any quantum mechanical theory—served as a helpful constraint on what they were and how they could manifest in basic assumptions. Thus, the question on the physical front of what the basic assumptions of quantum mechanics were became: which assumptions sufficed for deriving the trace rule?

It was this question which von Neumann began to tackle in his "Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik" [von Neumann, 1927a][von Neumann, 1963, 209]:[17]

> The method commonly used in statistical quantum mechanics was essentially deductive: the absolute square of certain expansion coefficients of the wave function, or of the wave function itself, was equated quite dogmatically with probability, and agreement with experience was subsequently verified. However, a systematic derivation of quantum mechanics from facts of experience or basic assumptions of probability theory, i.e., an inductive foundation, was not given. Also the relation to ordinary probability was an insufficiently clarified one: the validity of its basic laws (addition and multiplication law of probability) was not sufficiently discussed.

Thus, to effect an axiomatic completion, von Neumann yet needed to identify the basic assumptions that give rise to quantum mechanics, i.e., the trace rule. That is, what must the quantum mechanists agree *on*?

> In the present work such an inductive structure is to be attempted. We make the assumption of the unconditional validity of ordinary probability

of both sides; for another, there is no record of the discussion anywhere in von Neumann's surviving documents, whereas we would expect one had it taken place in writing after von Neumann's move to the U.S. Finally, the argument is conceptually of a piece with discussions and inquiries we know were happening at the time [Bacciagaluppi and Crull, 2009] [Bacciagaluppi and Valentini, 2009].

[17]This paper of von Neumann's was submitted to the *Proceedings* by Born about a month after the 5th Solvay Conference.

> theory. It turns out that this is not only compatible with quantum mechanics, but also (in combination with less far-reaching factual and formal assumptions—compare the summary in §IX, 1-3) sufficient for its unambiguous derivation. Indeed, we will be able to establish the entire 'time-independent' quantum mechanics on this basis.

Here we begin to see what the basic assumptions for quantum mechanics are: ordinary probability theory, along with some "less far-reaching factual and formal assumptions." Call the former (*Probability*). It is at the close of the paper that von Neumann finally makes the latter assumptions clear. Summarizing the latter (non-probabilistic) assumptions qualitatively, he says [von Neumann, 1963, 234]:

> The goal of the preceding work was to show that quantum mechanics is not only compatible with ordinary probability theory, but rather that under its presupposition—and some plausible factual assumptions—even the only possible solution. The underlying assumptions were the following:
>
> 1. Each measurement changes the measured object, and therefore two measurements always interfere with each other—unless one can replace both with one.
> 2. However, the change caused by one measurement is such that the measurement remains valid, i.e., if you repeat it immediately afterwards, you will find the same result.
> 3. The physical quantities are—in following a few simple formal rules—to be written as functional operators.

He quickly follows these with: "Note, by the way, that the statistical, "acausal" nature of quantum mechanics is due solely to the (principal!) inadequacy of measurement (cf. the work of Heisenberg cited in notes 2 and 4)." Thus, von Neumann appears already to disagree with Jordan, and agree with Heisenberg, that the existence of experimentally incompatible physical quantities (*Incompatibility*) is responsible for the acausal nature of quantum mechanics.[18] In 3, he is assuming the quantum mechanical

[18] In the introduction to [von Neumann, 1927b], von Neumann introduced the same assumptions, saying "1. Corresponds to the explanation given by Heisenberg for the a-causal behavior of quantum physics; 2. expresses that the theory nonetheless gives the appearance of a kind of causality" [von Neumann, 1963, 236]. Translation by Duncan and Janssen [Duncan and Janssen, 2013, 248].

way of representing quantities (*Quantities*), which restricts one to just those that are experimentally measurable.[19]

We should characterize (*Quantities*) and (*Probability*) further. These assumptions show up in §II, "basic assumptions." Let $\{\mathfrak{S}_1, \mathfrak{S}_2, \mathfrak{S}_3, ...\}$ be an ensemble of copies of the system $\mathfrak{S}$. Given that the goal is to recover quantum mechanics, von Neumann aimed for an expression of the expectation value $Exp(\mathfrak{R})$ in the ensemble of some quantity $\mathfrak{R}$ of the system. The assumption (*Probability*) amounted to:

A. *Linearity.* $Exp(\alpha\mathfrak{R} + \beta\mathfrak{S} +) = \alpha Exp(\mathfrak{R}) + \beta Exp(\mathfrak{S}) + \cdots$, $(\alpha, \beta$ real).

B. *Positive-definiteness.* If the quantity $\mathfrak{R}$ is always positive, then $Exp(\mathfrak{R}) \geq 0$.

while (*Quantities*) amounted to:

C. *Linearity of operator assignment to quantities.* If the operators $R, S, \ldots$ represent the quantities $\mathfrak{R}, \mathfrak{S}, \ldots$, then $\alpha R, \beta S, \ldots$ represents the quantity $\alpha\mathfrak{R}, \beta\mathfrak{S}, \ldots$.

D. If the operator R represents the quantity $\mathfrak{R}$, then $f(R)$ represents the quantity $f(\mathfrak{R})$.

Thus, (*Quantities*) captures the central focus in quantum mechanics on the functional relationship among experimentally measurable quantities. As [Duncan and Janssen, 2013, 213] note, assumptions A. and B. do not show up in §IX. Rightly, I am suggesting, they presume this is because they are "part of ordinary probability theory." Indeed, they also note that assumptions 1 and 2 (above quote) do not appear in A.–D.; this is because these assumptions are captured through their correspondence with operators (1 through commutative properties of operators and 2 through idempotency of projection operators corresponding to the measurement being made).

Thus, von Neumann had clearly accomplished his *mathematical* goal of demonstrating that ordinary probability theory (*Probability*) (in conjunction with (*Quantities*) and (*Incompatibility*)) was "sufficient" for the "unambiguous derivation" of the trace rule. Yet one point should be emphasized here, following Acuña: the immediate goal of his "Aufbau" was to identify the basic probabilistic-theoretical assumptions sufficient for

[19] Strictly speaking, however, note that in this work (*Quantities*) appears to be a formal, not factual, assumption. In his book, by contrast, the assumption appears less formal for the way it is introduced; see §4.2. Since I take the axiomatic completion of the book to be the central thread in his early work, I distinguish (*Incompatibility*) from (*Quantities*) throughout the following; see fn. 27.

deriving the trace rule Acuña [2021a] Acuña [2021b].[20] As such, A. (above) is legitimate insofar as it was a common assumption for probability at the time, and it was well-fitted to the transformation theory's emphasis on the functional relations among quantities (see [Acuña, 2021b, 12–13]). While it is not stated so explicitly in von Mises, whose work von Neumann was familiar with and later cited, expectation values naturally behave linearly in his *Kollektiv* approach. And as von Neumann hastens to add in a footnote, this also held for non-commuting quantities.

Nevertheless, it is less clear that von Neumann achieved his *meta-mathematical* goal of showing that quantum mechanics is "the only possible solution." That is, von Neumann did not explicitly show that Heisenberg's quantum mechanical states—i.e., those free of hidden variables that would return determinism—are the only states consistent with the trace rule. Were this not to be the case, then Heisenberg's countenancing of only those states would be a *necessary* additional assumption for his version of quantum mechanics. Von Neumann began work on this question by defining what it meant for an ensemble to be homogeneous. However, he only demonstrated that the homogeneous states corresponded to the unit vectors in Hilbert space w.r.t. the trace rule—he did not show explicitly, as he did in his book, that these states were necessarily dispersive. One could still hope that, in general, there were dispersion-free, homogeneous states compatible with the trace rule.

4 Axiomatic Completion: Von Neumann's 1932 Mathematical Textbook

4.1 Introduction and Chapters I–III

One of the primary aims of von Neumann's book [von Neumann, 1932][von Neumann, 1955] was to determine whether the Hilbert space form of the transformation theory—with the basic assumptions of (*Probability*), (*Quantities*), and (*Incompatibility*) serving as its basis—could countenance hidden variables. That is, he aimed to answer the uniqueness question for merely-statistical predictions in the transformation theory, providing thereby an axiomatic completion of quantum mechanics. We see hints of this already at the very beginning of the preface [von Neumann, 1955, vii]:[21]

[20] I do not follow Acuña [2021b, 12] in taking this to mean that hidden variables "were not considered at all in the original formulation of the theorem."

[21] Page numbers will refer to the English translation's original 1955 printing.

> The object of this book is to present the new quantum mechanics in a uniform [einheitliche] representation which, so far as it is possible and useful, is mathematically unobjectionable [einwandfreie].[...]Therefore the principal emphasis shall be placed on the general and fundamental questions which have arisen in connection with this theory. In particular, the difficult problems of interpretation, many of which are even now not fully resolved, will be investigated in detail. In this context the relation of quantum mechanics to statistics and to the classical statistical mechanics is of special importance.[22]

Fitting for an axiomatization, von Neumann wants a "uniform" representation that is "mathematically unobjectionable"—that is, it is a *unique* and *formal* (=fact-free) mathematical representation. Further, it is a representation and investigation of *quantum mechanics*, as it then existed, and not the more nebulous idea of a "generic" theory of quantum phenomena. It is also clearly provisional, at least in the sense that it does not claim to resolve every problem of interpretation.

The rest of the preface then focuses predominately on the two issues we have already encountered, namely the Dirac delta and the uniqueness question. Firstly, von Neumann observes that "the correct structure [of the transformation theory] need not consist in a mathematical refinement and explanation of the Dirac method, but rather that it requires a procedure differing from the very beginning, namely, the reliance on the Hilbert theory of operators" [von Neumann, 1955, ix]. This is all the more justified since the Hilbert theory of operators is sufficient for "the problem at hand" in quantum mechanics "of calculating numerically the result of a clearly defined experiment" (ibid.). This problem is soluble in the Hilbert theory of operators using the trace rule.

As noted above, this paved the way for determining uniqueness by solidifying the trace rule as an essential proposition for quantum mechanics. Not surprisingly, then, von Neumann secondly addresses the metamathematically-fundamental question of the uniqueness of the mathematical representation of the quantum-mechanical view. This begins by noting the inductive foundation of quantum mechanics [von Neumann, 1955, ix–x]:

[22] I have provided the original German words in brackets where I depart from Beyer's translation. While I do not disagree with Beyer's translation, I nevertheless think the terms I use better capture the intended meaning in contemporary (philosophical) English. 'Einheitliche' translated as 'uniform' better emphasizes the singleness implied, while I translate 'einwandfrei' more colloquially as 'unobjectionable' to avoid unintended association with the superficial rigor of later sufferers of Theory T syndrome; at any rate, Beyer translates the latter this way on page ix.

> In the analysis of the fundamental questions, it will be shown how the statistical formulas of quantum mechanics can be derived from a few qualitative, basic assumptions.

The connection is then made to the uniqueness question:

> Furthermore, there will be a detailed discussion of the problem as to whether it is possible to trace the statistical character of quantum mechanics to an ambiguity (i.e., incompleteness) in our description of nature. Indeed, such an interpretation would be a natural concomitant of the general principle that each probability statement arises from the incompleteness of our knowledge. This explanation "by hidden parameters," as well as another, related to it, which ascribes the "hidden parameter" to the observer and not to the observed system, has been proposed more than once. However, it will appear that this can scarcely succeed in a satisfactory way, or more precisely, such an explanation is incompatible with certain qualitative fundamental postulates of quantum mechanics.

These two explanations more-or-less directly correspond to Jordan's and Schrödinger's hopes, as captured in Wigner's recollection of the debate with von Neumann: von Neumann showed that "according to quantum mechanical theory, no such state [where the spin component has, with a high probability, a definite sign in all directions] is possible"; Schrödinger objected, claiming (essentially) that hidden variables could exist in the measuring apparatus; and von Neumann then showed that the measuring apparatus is no different in kind from the measured system, in the sense that quantum mechanics still applies, hence hidden variables fare no better if posited there. (Again, note that (*Probability*), (*Quantities*), and (*Incompatibility*) are being assumed.) As they occur in the book, these are the arguments of IV.1—2 and VI, respectively.[23]

Before addressing the hidden variables question, von Neumann first recapitulates his earlier work. In chapter I, we receive a summary of the equivalence work that preceded his own, as well as an explanation for its inadequacy for addressing the uniqueness problem and, thereby, for characterizing the "really essential elements of quantum mechanics" [von Neumann, 1955, 33]. This culminates in the following characterization of the goals of chapter II [von Neumann, 1955, 33]:

[23] I only discuss the argument of IV.1—2 here because the argument of VI is also significantly shaped by other contemporaries of von Neumann, particularly Szilard, Bohr, and Heisenberg.

We wish then to describe the abstract Hilbert space, and then to prove rigorously the following points:

1. That the abstract Hilbert space is characterized uniquely by the properties specified, i.e., that it admits of no essentially different realizations.
2. That its properties belong to FZ as well as $F\Omega$. (In this case the properties discussed only qualitatively in I.4 will be analyzed rigorously.) When this is accomplished, we shall employ the mathematical equipment thus obtained to shape the structure of quantum mechanics.

Thus in the main, chapter II redescribes von Neumann's work on the Hilbert space formalism, which began with [von Neumann, 1927c]; here, von Neumann is especially careful to note the necessary and sufficient conditions for central results. In chapter III, von Neumann then describes and expands upon the "induction" of quantum mechanics from [von Neumann, 1927a]. In each chapter, especially the latter, it is emphasized throughout that the mathematical formalism is ultimately in service to the quantum mechanical understanding and subject to revision according as the latter itself changes (see, e.g., pp. 133, fn. 86; 211—12; 213—14 with 221—23; 237—38). This is made especially clear in III.2 when von Neumann foreshadows the discussion of hidden variables in IV.1—2 [von Neumann, 1955, 210]:

> Whether or not an explanation of this type, by means of hidden parameters, is possible for quantum mechanics, is a much discussed question. The view that it will sometime be answered in the affirmative has at present prominent representatives. If it were correct, it would brand the present rendering [Form][24] of the theory as provisional, since then the description would be essentially incomplete.
>
> We shall show later (IV.2) that an introduction of hidden parameters is certainly not possible without a basic change in the present theory. For the present, let us re-emphasize only these two things: The ϕ has an entirely different appearance and role from the $q_1, ..., q_k, p_1, ..., p_k$ complex in classical mechanics and the time dependence of ϕ is causal and not statistical: ϕ_{t_0} determines all ϕ_t uniquely, as we saw above.

[24] I depart from Beyer's translation of 'Form' as 'form' to emphasize that it would be the mathematical form of the theory (quantum mechanics), i.e., the Hilbert space formalism, that is provisional.

> Until a more precise analysis of the statements of quantum mechanics will enable us to test [prüfen][25] objectively the possibility of the introduction of hidden parameters (which is carried out in the place quoted above), we shall abandon this possible explanation.

Here von Neumann has made it clear (1) that the question is whether *quantum mechanics*—whose formalism, the transformation theory, is mathematically rendered in the Hilbert space formalism—can accommodate hidden variables, and (2) that, contrary to Schrödinger's and others' (e.g., Jordan) expectations, the wave function's evolution is fundamentally unlike that of classical position and momentum in the Hamiltonian schema. In all of this, then, he has made it clear that his axiomatization is relative to a set of propositions (quantum mechanics) and hence *provisional* insofar as quantum mechanics itself is provisional.

4.2 Chapter IV, Sections 1 and 2

Let us now consider IV.1—2 in this light. By this point, the contents should not surprise us: von Neumann will derive the trace rule from the "inductive" basis of quantum mechanics, as in his [von Neumann, 1927a], and then determine the consistency of dispersion-free states with the trace rule. Indeed, this is precisely what happens.

To begin, von Neumann makes plain his "basic, qualitative" assumptions. He first characterizes the kinds of quantities and relations thereof being considered, i.e., (*Quantities*) [von Neumann, 1955, 297]. Von Neumann supposes that systems are characterized by the enumeration of "all the effectively measurable quantities in it and their functional relations with one another." He also clarifies what this means for simultaneously measurable quantities. Then, von Neumann elaborates on non-simultaneously measurable quantities, saying that "their appearance in elementary processes was always to be suspected" and "their presence has now become a certainty" [von Neumann, 1955, 300–1]. Going farther still, he makes it clear that he is taking Heisenberg's uncertainty relations to be general and what are essentially responsible for the intractability of a hidden variable theory (i.e., (*Incompatibility*)).[26] Indeed, as before, von Neumann

[25] Beyer translated 'zu prüfen' as 'to prove', which in typical English implies von Neumann meant to "objectively prove a possibility"; this is certainly not what von Neumann meant, and the more common translation as 'to test' or 'to examine' is more appropriate, regardless.

[26] However, note that for the purposes of his proof von Neumann only assumes there exist incompatible quantities, not Heisenberg's particular understanding of the uncertainty relations; he is careful not to specialize the setup to quantum mechanics too early, as we gather upon his introduction of E.

draws from this the obvious consequence that dispersing ensembles cannot effectively be resolved into those without dispersion [von Neumann, 1955, 304–5]:

> That is, we do not get ahead: Each step destroys the results of the preceding one, and no further repetition of successive measurements can bring order into this confusion. In the atom we are at the boundary of the physical world, where each measurement is an interference of the same order of magnitude as the object measured, and therefore affects it basically. Thus the uncertainty relations are at the root of these difficulties.

The assumptions, then, are just what were present in von Neumann's [von Neumann, 1927a], namely (*Probability*), (*Incompatibility*), and (*Quantities*).[27] Nothing is new so far, even if the discussion is longer.

Yet still, the question remains whether hidden variables are *consistent* with the predictive formulas of quantum mechanics (i.e., the trace rule). This becomes the focus of the rest of the section [von Neumann, 1955, 305]:

> Therefore we have no method which would make it always possible to resolve further the dispersing ensembles (without a change of their elements) or to penetrate to those homogeneous ensembles which no longer have dispersion. The last ones are the ensembles we are accustomed to consider to be composed of individual particles, all identical, and all determined causally. Nevertheless, we could attempt to maintain the fiction that each dispersing ensemble can be divided into two (or more) parts, different from each other and from it, without a change in its elements. That is, the division would be such that the superposition of two resolved ensembles would again produce the original ensemble. As we see, the attempt to interpret causality as an equality definition led to a question of fact which

[von Neumann, 1955, 309].

[27] While (*Incompatibility*) may appear to be a consequence of (*Quantities*), given that the latter is typically glossed as "quantities are represented by Hermitian operators," this isn't quite right. Rather, (*Quantities*) concerns the behavior of physical quantities *in general*, with (*Incompatibility*) a *specific* constraint placed on quantum quantities. Von Neumann introduces Hermitian operators (at the beginning of IV.2) as the mathematical representatives of physical quantities precisely *because* physical quantities thus represented respect (*Incompatibility*) and (*Quantities*). The typical gloss therefore misunderstands the given mathematical definition of physical quantities (in IV.2) as prior to the constraints on any satisfactory definition (in IV.1, i.e., (*Incompatibility*) and (*Quantities*)), making (*Incompatibility*) appear redundant.

can and must be answered, and which might conceivably be answered negatively. This is the question: is it really possible to represent each ensemble $[S_1, ..., S_N]$, in which there is a quantity $\mathfrak{R}$ with dispersion, by the superposition of two (or more) ensembles different from one another and from it?

Von Neumann then formalizes the question using the tools of probability theory and his earlier definitions of dispersion-free and homogeneous ensembles. In brief, the question is whether there can exist *dispersion-free expectation functions* in quantum mechanics, i.e., whether an ensemble can ever be characterized in a way that all of its variables exhibit no dispersion in the expectation value for their subsequent measurement.

Finally, von Neumann formally characterizes the informal assumptions (*Probability*), (*Incompatibility*), and (*Quantities*) above. Von Neumann's formal characterization of the informal assumptions are essentially the same as in [von Neumann, 1927a]. This includes the infamous **B'.** (A. in [von Neumann, 1927a]):

- if $\mathfrak{R}, \mathfrak{S}, ...$ are arbitrary quantities, and $a, b, \ldots$ are real numbers, then $Exp(a\mathfrak{R} + b\mathfrak{S} + \cdots) = aExp(\mathfrak{R}) + bExp(\mathfrak{S}) + \cdots$.

As *many* later commentators have remarked, this is to assume that any would-be hidden variables must behave as if they are quantum mechanical quantities (e.g., [Misra, 1967] [Bell, 1966] [Mermin and Schack, 2018]). One would only assume this if one had already assumed quantum mechanics was true! Yet as I have said, this is exactly right: quantum mechanics—namely, (*Probability*), (*Quantities*), and (*Incompatibility*)—*is* being assumed. Moreover, as Acuña [2021b, 15–6] stresses, **B'.** is used to derive the trace rule, which applies to any two physical quantities—*including* incompatible quantities. In fact, "without **B'.**, the possibility of dispersion-free states would lead us to the fact that the legitimate quantity $f(\mathfrak{R}, \mathfrak{S}) = \mathfrak{R} + \mathfrak{S}$ cannot be captured by the Hilbert space formalism" (ibid.). Thus, if the Hilbert space form of the transformation theory is taken as adequate for representing all effectively measurable quantities—which it is being so taken—then **B'.** is well-motivated. Indeed, von Neumann qualifies at the end of Section 1 that by "reason of A'., B'. and α', β' we are now in position to make a decision on the question of causality, *as soon as we know the physical quantities in S as well as the functional relationships between them*" (emphasis added).

Von Neumann begins the second section by characterizing the relationship quantities will have to the Hilbert space formalism. Yet this, too, is straightforward as this was the entire point of Chapter II and, indeed, von Neumann had already assumed these in the guise of F* and L* in III.5 for his discussion of properties.[28] Thus, IV.2 begins unremarkably [von Neumann, 1955, 313–14]:

> There corresponds to each physical quantity of a quantum mechanical system, a unique hypermaximal Hermitian operator, as we know (cf., for example, the discussion in III.5.), and it is convenient to assume that this correspondence is one-to-one—that is, that actually each hypermaximal operator corresponds to a physical quantity. (We also made occasional use of this in III.3.) In such a case the following rules are valid[29] (cf. F., L. in III.5, as well as the discussion at the end of IV.1.):
>
> I. If the quantity $\mathfrak{R}$ has the operator R, then the quantity $f(\mathfrak{R})$ has the operator $f(R)$.
>
> II. If the quantities $\mathfrak{R}, \mathfrak{S}, \ldots$ have the operators $R, S, \ldots$, then the quantity $\mathfrak{R} + \mathfrak{S} + \cdots$ has the operator $R + S + \cdots$. (The simultaneous measurability of $\mathfrak{R}, \mathfrak{S}, \ldots$ is not assumed, cf. the discussion on this point above.)

Note that it is "convenient" for von Neumann to assume the correspondence between quantities and operators is one-to-one because he takes it as obvious that the operator calculus represents legitimate functional relationships among quantities; in particular, if experimentally incompatible quantities $\mathfrak{R}$ and $\mathfrak{S}$ have the operators R and S and $R+S = Q$, then Q *should* represent a quantity $\mathfrak{Q}$ (and **B'.** says to do so in the obvious way, i.e., $\mathfrak{Q} = \mathfrak{R} + \mathfrak{S}$).

Von Neumann then quickly derives the trace rule from I, II, A'. and B'. before moving on to consider its consequences. These consequences concern the existence of homogeneous and dispersion-free states consistent with the trace rule. Von Neumann begins by showing that dispersion-free ensembles cannot be represented as states in the Hilbert space formalism. The proof is a simple reductio: if we assume that such an ensemble exists for a quantity $\mathcal{R}$ represented by the operator R, then U must be the zero or identity operator, neither of which is possible [30] Then, von Neumann recapitulates

[28] These correspond to his C. and D. in [von Neumann, 1927a].

[29] Note also that von Neumann says these rules "are valid" in such a case, rather than simply asserting that such rules "are true": he is signaling that (*Quantities*) has already been assumed.

[30] Again, note that this proof does not use B'.—see [Acuña, 2021b, §4] and Acuña [2021a] on the relation of von Neumann's theorem to related theorems, e.g., Gleason's theorem and the theorem of

his proof that the homogeneous states are the unit vectors in the Hilbert space. Thus, there is no chance of penetrating to those homogeneous ensembles without dispersion—"within the limits defined by our conditions, the decision is made, and it is against causality, because all ensembles—even homogeneous ensembles—have dispersion" [von Neumann, 1955, 324].[31]

This answers the uniqueness question in the positive: quantum mechanics (i.e., (*Probability*), (*Quantities*), and (*Incompatibility*)) already rules out hidden variables through the trace rule, meaning that the transformation theory is the unique representation of quantum mechanics. This means Heisenberg's assumption of quantum mechanical states was not necessary, and it also means that Jordan was wrong to believe there were dispersion-free states among the homogeneous states in the transformation theory. Thus, the statistical interpretation of the transformation theory is final: the maximal-information states (homogeneous states) are necessarily dispersive (statistical).

Finally, conceiving of von Neumann's axiomatization as a Hilbert-style axiomatization sheds further light on an odd remark von Neumann makes concerning his theorem. The Hilbert-style axiomatization conception fits naturally with von Neumann's presentations of his theorem as conditional. We can phrase this conception as: if one assumes (*Probability*), (*Quantities*), and (*Incompatibility*) (which are satisfactory insofar as they are sufficient for the trace rule), then the transformation theory is uniquely interpreted as statistical. Call this conditional statement *Uniqueness*. Nevertheless, there is one place von Neumann seems to suggest the theorem is not conditional. After several rephrasings comporting with the conditional structure of *Uniqueness*, von Neumann concludes that "the present system of quantum mechanics would have to be *objectively false*, in order that another description of the elementary processes than the statistical one be possible" [von Neumann, 1955, 324](italics mine).

Von Neumann's claim that quantum mechanics would have to be objectively false strongly is odd because it suggests he did not see his theorem as conditional. Indeed, the suggestion has served as the primary evidence for anti-conditional interpretations of his theorem. Further, the most prominent explanation of the phrase by conditional

Kochen–Specker and Bell.

[31]At this point, von Neumann seems to imply (fn. 172) that even the proof that there are no dispersion-free ensembles is from his "Wahrscheinlichkeitstheoretischer." Strictly speaking this is false, as neither the proof nor the definition of dispersion-free ensembles is contained therein. However, von Neumann does observe—as a property of the trace rule!—that "there are quantities in each state whose distribution function is not sharp" [von Neumann, 1963, 222].

interpreters—that he merely meant that I and II would have to be false [Bub, 2010] and [Dieks, 2017]—is unsatisfactory, since emphasizing the objectiveness of I and II would be an odd flight of rhetoric for von Neumann. After all, as we saw above, these capture a correspondence assumed in part for mere convenience! Moreover, the passage is clearly saying *quantum mechanics* would have to be false [Acuña, 2021b, 18], not I and II (whose role the prior sentence had already discussed). For this reason, Acuña suggests a middle ground between the anti-conditional and Bub and Dieks interpretations of the remark. According to Acuña, von Neumann *did* make a mistake, albeit an understandable one: "he concluded that a theory in which [physical quantities are not represented by Hermitian operators] would lead to empirical divergences with respect to Hilbert space quantum theory, so that the predictive success of the latter discards hidden variable theories from the outset—not for being impossible, but for being objectively false" [Acuña, 2021b, 19]. In short, he suggests that von Neumann believed the Born rule could not be recovered without representing physical quantities as Hermitian operators; yet Bohm's theory does just this, so von Neumann was wrong.

Acuña's interpretation of von Neumann's remark is plausible. Nevertheless, the Hilbert-style axiomatization conception of von Neumann's work suggests a precisification of Bub's and Diek's interpretation worth considering. If *Uniqueness* is the correct (conditional) interpretation of von Neumann's theorem, then Bub and Dieks are wrong to think of I and II as assumptions. Rather, these are *valid* given the quantum-mechanical assumptions—just as von Neumann says—and in particular given the assumption of *Quantities*. This latter assumption was introduced in his book at the beginning of §1 through a characterization of experimental measurement; this sort of characterization might reasonably be considered objective, particularly given the detailed story von Neumann tells about measurement in Chapter III. In this characterization, von Neumann is clearly conceiving of the measurement of a physical quantity as a measurement of a physical property of the observed system alone. Thus, the suggestion is that when von Neumann said quantum mechanics would have to be objectively false, what he meant was that measurements would not be of the observed system alone.[32]

[32]While I agree with Acuña that there is likely not enough textual evidence to settle this controversy definitively, the suggestion given here has several advantages worth noting. First, it comports with the axiomatic completion reading insofar as it highlights the elucidation of the axioms of quantum mechanics and their interrelationship as central rather than the derivation of the trace rule, which is merely a constraint on the axioms. Indeed, von Neumann almost immediately began searching for generalizations of the trace rule derived from the Hilbert space formalism. Second, von Neumann

Conclusion: Orienting for Our Future

Von Neumann's final word on causality highlights well the components of the axiomatic method I began this work with [von Neumann, 1955, 327–8]:

> The question of causality could be put to a true test only in the atom, in the elementary processes themselves, and here everything in the present state of our knowledge militates against it. The only formal theory existing at the present time which orders and summarizes our experiences in this area in a half-way satisfactory manner, i.e., quantum mechanics, is in compelling logical contradiction with causality. Of course it would be an exaggeration to maintain that causality has thereby been done away with: quantum mechanics has, in its present form, several serious lacunae, and it may even be that it is false, although this latter possibility is highly unlikely, in the face of its startling capacity in the qualitative explanation of general problems, and in the quantitative calculation of special ones. In spite of the fact that quantum mechanics agrees well with experiment, and that it has opened up for us a qualitatively new side of the world, one can never say of the theory that it has been proved by experience, but only that it is the best known summarization of experience.

First, the theory axiomatized—quantum mechanics—was considered *provisional* insofar as it was merely "the best known summarization." Second, von Neumann's axiomatization *ordered* the facts of quantum mechanics. Finally, the axiomatization *oriented* later research. Yet this orientation has not been sufficiently appreciated to date; in closing, I briefly sketch how it oriented later research.

First, I should clarify what it means for an axiomatization to orient research. To orient ourselves, I begin again with Hilbert's "Axiomatische Denken." There we glimpse—however flowery its expression may be—Hilbert's true aim, of enriching mathematics through the sciences and vice versa [Hilbert, 1917, 405]:

> As in the life of the peoples the individual persons can only prosper when all of the neighboring peoples do well, and as it commands the interest of the

seems to have foreseen the possibility of (*Quantities*) being denied (see Wigner [1970] on his debate with Schrödinger)—but just thought such denials were obviously wrong-headed (as did the other quantum mechanists). Third, this is precisely where Bohm put pressure on quantum mechanics and von Neumann, not I and II *per se*.

> states that order prevails not only within each individual state, but also that the relations among the states themselves must be well-ordered, so too is it in the life of the sciences. The significant representatives of mathematical thought, in proper recognition of this, have always demonstrated great interest in the laws and the arrangement in the neighboring sciences and, above all, cultivated the relations to the neighboring sciences, especially to the great kingdoms of physics and epistemology, always to the benefit of mathematics itself. I believe the nature of these relations and the basis of their fruitfulness becomes most plain if I describe to you the one general method of research that appears to be more and more effective in the new mathematics: I mean the axiomatic method.

Not least because von Neumann often said as much himself [von Neumann, 1954], I think this connection should be minded as we consider what it means to orient an area of inquiry.

In the case of von Neumann's axiomatic completion of quantum mechanics, I think the relationship is this. On the one hand, his axiomatization used the tools of mathematics to tell something to the physicist, namely, that quantum mechanics cannot be extended with hidden variables. This is useful for it changed the places folks (e.g., Bohm) looked for hidden variable interpretations and dampened any lingering concern about the "fit" of the formalism to its theoretical underpinnings (e.g., Jordan). However, at the same time it tells us where we might fruitfully focus attention: (*Probability*), (*Quantities*), and (*Incompatibility*). Indeed, this is what has since taken place, and whenever such attention has borne fruit, von Neumann's proof seems to be mentioned as the inspiration. To pick but one (unexceptional) example Misra [1967]:[33]

> The only justification [of von Neumann's A'., B'., α), β), I., II.] is the a posteriori one that they lead to the usual formalism of quantum [theory]. Such a justification, which is sufficient from an empirical point of view, has little compelling force in the context of the hidden-variable problem. For one is now concerned with the possibility of generalizing the usual formalism of quantum [theory] and the mere fact that a set of postulates leads to the usual formalism cannot be a sufficient recommendation for these postulates. . . . The alternative left to us is to proceed axiomatically

[33] I have "translated" Misra's 'quantum mechanics' as 'quantum theory' for the sake of consistency with the foregoing.

> in the spirit of von Neumann. Only, one must now start with less stringent postulates than those assumed by VON NEUMANN. The aim of such an axiomatic approach is to isolate the weakest possible assumptions which must be violated for having hidden variables. Once such assumptions have been isolated, one can then decide if and how they can be altered so as to allow hidden variables.

This is the axiomatic reconsideration path taken by Bohm, de Broglie, Bell, and others, and the essential feature is that *physical* or *epistemological* considerations related to (*Probability*), (*Quantities*), and (*Incompatibility*) predominate. Thus, in a first sense, von Neumann's axiomatic completion of quantum mechanics has *oriented* by focusing our attention on the physical and epistemological considerations that underwrite the usual quantum formalism.[34]

However, the axiomatic method is *also* about enriching mathematics. Thus, on the other hand, von Neumann's axiomatization used physical facts to tell something to the mathematician, namely, that attention should be focused on operator algebras, non-commutative geometry, orthomodular lattices, quantum logics, and the like. This has proven fruitful in mathematics, as von Neumann himself ensured. However, it also quickly wrapped back around to physics, where the study of Hilbert spaces and C* algebras gave way, in particular, to sharpenings of von Neumann's "no hidden variables" theorem (mentioned above). This is the path taken immediately by von Neumann himself (as well as early co-authors in, e.g., Jordan and Wigner), but also later by Haag, Wightman, and others.[35]. Indeed, von Neumann's work was crucial for later epochal work, like Schwartz's on distributions [Schwartz, 1951] and Wigner's on irreducible representations of the Poincaré group [Wigner, 1939]. What is common on this approach is that *mathematical* considerations related to A'., B'., α), β), I., II. predominate. Thus in a second sense, von Neumann's axiomatization of quantum mechanics has *oriented* by focusing our attention on the mathematical considerations to which quantum theory gives rise.

In the end, then, von Neumann's use of the axiomatic method—his axiomatic completion of quantum mechanics—oriented us toward two related futures. Just as Hilbert would have wanted, von Neumann effectively summarized and clarified where we *had*

[34] And, I might add, he focused our attention on these considerations in a much more precise way than, say, Bohr did.

[35] See Landsman [2019] for an introduction to the history of functional analysis and quantum theory after 1932.

been—in physics as well as in mathematics—in an effort to identify where we *could go*. The relationship between physics and mathematics, not to mention the fields themselves, has been the better for it.

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