Hilbert-Style Axiomatic Completion: The (Not So) Hidden Contextuality of von Neumann's "No Hidden Variables" Theorem

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1 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 or the physical 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]. Instead, my disagreement concerns 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

[1] In what follows I will use 'quantum theory' to refer to what we today call 'quantum mechanics' and reserve 'quantum mechanics' for its historical referent, i.e., the cluster of work that grew up in Göttingen and Cambridge.

relied on Hilbert's axiomatic 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 the first section, I give an overview of the axiomatic endeavors foreshadowed in [Hilbert et al., 1928]. Here I emphasize three features of the Hilbertian axiomatic method: (1) it requires the separation of the *facts* of a given theory from the *formalism*, (2) the formalism is *uniquely* characterized with respect to the theory, and (3) the goal was to *order* the area of knowledge and *orient* its further research. As such, the method's results were *provisional* and *relative*. In the second section, I describe the history of quantum theory that immediately preceded (Hilbert, Neumann, and Nordheim 1928). Here I focus on the influence and status of the transformation theory as developed by Dirac and Jordan. In the third section, I re-interpret von Neumann's work in 1927 as the 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 insofar as von Neumann very likely already had his "no hidden variables" proof in 1927, he had thus demonstrated that the Hilbert space formalism was the *unique* representation of quantum mechanics. I also introduce a little-known debate between von Neumann and Schrödinger on the status of hidden variables. In the fourth section, 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 the fifth section, 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.

2 The Axiomatic Method: Separating Facts from Formalism

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 reconstruction." Yet this view trades on half-truths. Closer inspection reveals a richer and more grounded program in which Hilbert is not so naive, von Neumann not so facetious, and the labor not so fruitless.

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 in the following passage from [Hilbert et al., 1928], which remarks on the ideal (non-obtaining) way to a quantum theory[3] (italics mine; page references are to [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 linear equations.
>
> 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

[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]. In a follow-up to this paper, I argue that this conflates two axiomatic spirits, those of axiomatic reconsideration and axiomatic completion, and that the former is an anachronism during this period. 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 because the passage is not readily available in English. All translations are mine, unless specified.

> other quantities and thus, through back-translating, statements about real physical things.
>
> 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.
>
> *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.*
>
> 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.
>
> 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.

The bigger picture here is rather clear: while quantum mechanics took the non-ideal path to its current position, during which the physical facts[4] and formalism were not clearly separated, an ideal erection of the theory would cleanly separate the two. Indeed, though the axiomatic method and this separation maxim are today

[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.

associated with Hilbert, they were more routinely recognized in the day. For instance, this is the feature of the axiomatic method that Einstein's "Geometrie und Erfahrung" identifies as the solution to the "riddle" of how mathematics—whose objects are purely imaginary—can apply to actual objects [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 mathematics according to the axiomatic method, but not the intuitive or other content connected to the logical-formal.

Thus, in broad strokes, the axiomatic method is for separating the mathematical (logical-formal) from the non-mathematical (intuitive) content.

This tells us not only *what* the axiomatic method is for but also *whom* it is for: the meta-mathematician. We can spell this out using the example of Hilbert's axiomatization of geometry, to which Hilbert referred above.[5] In brief, a more-or-less agreed upon set of propositions was taken to constitute Euclidean geometry at the end of the 19th Century. However, it was unclear just what assumptions were 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:[6] while it was clear

[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, 202?]. 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 aspects of the axiomatic method, 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."

that the proposition was true on our "intuitive" conception of Euclidean geometry, its formal relation to the other axioms had not yet been clarified. Enter Hilbert, the meta-mathematician. Taking the more-or-less agreed upon propositions (constituting Euclidean geometry) as the targets for recovery—but eschewing the "intuitive" moorings Euclidean geometry had accumulated in its practical go of it!—Hilbert characterized the axioms necessary for recovering these target propositions. Crudely put, where the mathematician asks what propositions *a given set of axioms* suffice to prove, the meta-mathematician turns this around via the axiomatic method to ask what axioms are necessary to prove a given set of propositions. Thus, the axiomatic method is primarily for the meta-mathematician.

Before considering quantum mechanics, I want to highlight a couple of features of the axiomatic method using Hilbert's Grundlagen der Geometrie as an example. These features are easiest to draw out from Hilbert's "Axiomatische Denken" [Hilbert, 1917]. Firstly, the method is *relational* in two senses. On the one hand, the method is relational in that the formal theory is given relative to a more-or-less comprehensive field of knowledge, which is itself constituted by facts. The facts of such a field of knowledge admit of an ordering that [Hilbert, 1917, 405]

> is effected in each case with the help of a certain truss [Fachwerk][7] of concepts in such a manner that to each individual subject of a field of knowledge corresponds a concept from this truss and to each fact within the field a logical relation between the concepts. The truss of concepts is none other than the theory of the field of knowledge.

Thus the conceptual truss plays a crucial role in the axiomatic method. Nevertheless, Hilbert is at pains to stress that the theory is *not* the same as the field of knowledge;

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.

[7] 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.)

rather, we quickly gather, as here concerning consistency and independence, that the theory plays a precise, practical role insofar as it *represents* that 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.

To put it crudely and anachronistically, Hilbert understands theories as "lossy" representations of fields of knowledge. In putting it this way, I mean to stress that Hilbert was not so naïve to think that the "ladder" of facts is "kicked away" once a theory has been given. Rather, he recognized that one has inevitably made non-trivial choices in how to represent these facts, regardless of how well-informed such choices are.

Concerning Hilbert's *Grundlagen*, the picture is something like the following. First, the facts are the "more-or-less agreed upon propositions" that were taken to constitute Euclidean geometry. Insofar as it is in this set of propositions, this includes Archimedes' Axiom. These facts admit of an ordering, given in Hilbert's *Grundlagen* by the choice of axioms. This ordering is effected "with the help of [the] truss of concepts" that inform the groupings Hilbert gives to the axioms (namely, connection, order, parallels, congruence, and continuity). By aligning the subjects of Euclidean geometry with concepts and its logical relations among those concepts, this truss of concepts constitutes a *theory* of Euclidean geometry. Hilbert then examines the independence and consistency of the various groups of axioms.

This brings us to the way in which, on the other hand, the method is *relational* in the more traditional mathematical sense: independence and consistency results are relative to some prior mathematical theory. This is essentially a response to a problem I skirted a moment ago when introducing Hilbert's *Grundlagen*. There I noted the importance of eschewing the "intuitive" moorings a set of facts has accumulated in its practical go of it. This is far easier said than done, however. In the *Grundlagen*, as Hilbert et al. said above, Hilbert used the linear equations as an analytic apparatus that represented precisely the relations arising in his theory of geometry. By translating unresolved geometric questions into the better-understood language of linear equations, Hilbert was able to answer these questions in the latter and then translate their answers back into geometry. Doing this he, for instance, reduced the question of the consistency of the geometric axioms to the question of the consistency of the theory of the real numbers.

That said, there is an extra trick here, which is that the analytic apparatus needs to be uniquely specified with respect to the physical facts as codified by the truss of concepts. This is only implicit in "Axiomatische Denken," but it is explicit in [Hilbert et al., 1928]. There they say that the axiomatic (ideal) way to a (quantum) theory is to specify the physical demands on probabilities, then develop an analytic apparatus "in which quantities occur that satisfy precisely the same relations," and finally "interpret" the apparatus on the basis of the physical demands. One might hastily conclude that the relationship between apparatus and interpretation is the one familiar to us, namely one where the hope is to have so specified the mathematics that all interpretations are equivalent in some model-theoretic sense. Precisely the opposite is desired here, however: "The aim in doing so [interpreting the analytic apparatus] is to so fully formulate the physical demands that the *analytic apparatus* is uniquely defined" (italics added). And this should make sense. If a set of physical demands admits of multiple formalizations, then claims to have separated the factual from the formal in the area are dubious. In other words, Hilbert's axiomatization of geometry needed a 1-1, invertible mapping between the languages of geometry and analysis in order to effect the translational strategy, i.e, replacing questions of geometrical truth with arithmetical truth. With this in hand, Hilbert can confidently claim to have represented the truths of geometry as truths of arithmetic. So, the axiomatic method applied to the area being investigated demands a, presumably better understood, analytic apparatus to which the area's theory corresponds uniquely.

The upshot is that the axiomatization of an area is *relative* both to that area's set of facts and to an analytic apparatus, and the latter relation demands a unique correspondence between the theory of the area and the analytic apparatus. This brings us to the second feature of the axiomatic method. This is that it is a continual process whose results are always *provisional*, especially insofar as they are non-mathematical. Yes, fundamental propositions of the theory can be viewed as axioms of the area of knowledge [Hilbert, 1917, 406]:

> The fundamental propositions can be seen from an initial standpoint as the axioms of the individual field of knowledge: the continuing development

> of the individual field of knowledge is then founded merely on the further logical development of the already-presented framework of concepts. This standpoint is prevailing especially in pure mathematics, and the tremendous developments of geometry, arithmetic, function theory, and the whole of analysis we owe to the corresponding method of working.

Temptation would have us conclude that axioms are not subject to revision, and above Hilbert et al. did say that the *analytic apparatus* of quantum mechanics was "well-situated" and "capable of no modification." But this is not a very interesting claim: indeed, formalisms *are* certain in that they never *require* modification—but only so far as they are mathematical, as Einstein said above. And while we hear this, too, in "Axiomatische Denken", he at the same time stresses that axioms are always provisional owing to their function [Hilbert, 1917, 407] (italics mine):

> Consequently, in the mentioned cases, the problem of the grounds of an individual field of knowledge had then found a solution; however, this was only provisional. In fact the need asserted itself in the individual sciences mentioned to in turn ground the propositions looked at as axioms which lay at the foundation. Thus one reached "Proofs" of the linearity of the equation for the plane[...], of the law of entropy and the proposition of the existence of roots of an equation.
>
> But the critical examination of these "Proofs" affords recognition that they are not themselves proofs, but rather at those depths merely make possible the tracing back to certain deeper-lying propositions, which henceforth are to be seen for their part as axioms in place of the propositions to be proven. Thus originate the axioms actually so-called today in geometry, arithmetic, statistics, mechanics, radiation theory or thermodynamics. These axioms form a deeper-lying layer of axioms with regard to the one layer, as they have been characterized through the propositions just mentioned as lying at the ground in individual fields of knowledge. The procedure of the axiomatic method, as it is characterized here, hence comes to a deepening of the foundations of the same individual field of knowledge, of course to the extent such is necessary with each framework, when one elaborates the same field wishing to go higher and yet vouch for his security.

But recall that the primary benefit of axiomatization is separation of factual and formal content. Thus, if the axiomatic method is indefinitely applicable—in the sense that axioms so-called today could be replaced tomorrow—it is not hard to see that "going deeper" could rule today's so-called formal content as factual tomorrow, or perhaps even vice versa. Indeed, if this were not possible, it is difficult to see how a deepening of the foundations—to which the procedure of the axiomatic method comes—could vouch for the security of one who wishes to elaborate on a formalism: there would be no "insecurities" (=hidden factual content) to remove. We gather, thus, that Hilbert does not consider axioms as beyond revision, *even in pure mathematics*.[8] Rather, there is an expectation that deeper layers will be found when the need for them arises, and this inevitably will lead to the demotion of axioms today-so-called in favor of deeper-lying ones.

All told, then, axiomatization is a *relational* and *provisional* method for separating between the factual and formal content of an area of knowledge. It is relational first in the sense that it is relative to a chosen set of facts from that area of knowledge and, at the same time, a truss of concepts from which one constructs a theory of that area. Second, it is relational in the sense that independence and consistency results for the theory are relative to an analytic apparatus that is uniquely picked out by the strictures of that area's theory. The method is provisional in the sense that the axioms it produces to represent of an area of knowledge are subject to revision as our understanding of that area changes.[9] Finally, an axiomatization aims to *orient* and *order* our inquiry in an area of knowledge, especially our mathematical inquiry, by focusing our attention on what is central to its theory.

[8]However, this does not mean that revising a theory involves denouncing the previous one. In such a case, we have simply shifted our interests, and the previous theory is still perfectly acceptable as a mathematical theory. (This line on Hilbert gets trickier to defend when it comes to elementary arithmetic or logic, but this needn't concern us here.)

[9]Likewise for consistency and independence results, since we may also revise what we consider the canonical translation. Historically, this just seems less likely to change. That said, I suspect this is the better way to understand Hilbert's "proof" of the Continuum Hypothesis against the backdrop of the later work of Gödel and Cohen: Hilbert, thinking syntactically, assumed his L-like structure exhausted the truths of set theory; Gödel and Cohen, thinking semantically, showed that this assumption was non-trivial.

3 The (Pre-)History of Hilbert, von Neumann, Nordheim (1928)

As [Hilbert et al., 1928] 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 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] summarizes the distinctions as follows:

> One reason for the failure of the mathematical equivalence is the fact that whereas matrix mechanics could in principle describe the evolution of physical systems over time (by means of the Born-Jordan equation), but limited itself unnecessarily to periodic phenomena, wave mechanics could not—Schrödinger's time-dependent wave-equation dates from 3 months later than his equivalence proof. Other reasons for the failure of mathematical equivalence are: the absence in matrix mechanics of a state space but its presence in wave mechanics (the space of wave-functions); the fact that Euclidean space and a set of charge-matter densities, both prominently present in wave mechanics, had no matrix-mechanical counterparts; and the fact that matrix mechanics produced the first theory of a quantised electromagnetic field by means of matrix-valued fields, whereas Schrödinger emphasised there was no need to tinker with the classical Maxwell equations in wave mechanics.

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.

[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 et al., 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]).

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 by Mehra and Rechenberg):

> Relation to Heisenberg. I am convinced, along with Geheimrat Sommerfeld, that an intimate relation exists. It must, however, lie rather deeply, because Weil [sic], who has studied Heisenberg's theory very thoroughly and has developed it further himself, says upon reading the first manuscript that he is unable to find the connection. Consequently, I have given up looking any further myself....Now I firmly hope, of course, 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 momentarily.) 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 certain respect, wave mechanics was equivalent to matrix mechanics.[12]

Two things are important here. First, it bears repeating that Schrödinger was not only *convinced* almost immediately that his wave mechanics could absorb matrix mechanics, but he strongly *desired* its absorption. Second, he made good on this claim (he thought) precisely by *expanding* wave mechanics to *make* it equivalent [Muller, 1997a, 54–5]:

> In his second founding paper Schrödinger had confessed that wave mechanics could not calculate the intensity of spectral lines, something which matrix mechanics in principle was able to do. But from his own proof [of equivalence] Schrödinger learned how to express spectral-line intensities in wave mechanics, by looking at the [relevant] matrix-mechanical formula (6). . . . So besides extending wave mechanics by adding the canonical

[12]See [Perovic, 2008] on the goal of Schrödinger's proof. As there noted [Perovic, 2008, 459], von Neumann takes Schrödinger to have demonstrated the mathematical equivalence of the two theories, contrary to what Perovic claims Schrödinger was after.

> wave operators (Postulate W1) whilst in the process of proving equivalence, Schrödinger was here extending wave mechanics once more by another brand new postulate. Schrödinger was not just attempting to prove the equivalence of matrix mechanics and extant wave mechanics, but he was also *expanding* wave mechanics on the spot to *make* it equivalent to matrix mechanics.

These are important to stress because they complicate any claim that Schrödinger was coerced or otherwise unduly influenced into accepting the quantum hegemony. Unknowingly or not, his *choice* to make wave mechanics equivalent to matrix mechanics—even in so limited a setting as Bohr's model of the atom—set him on the path to hegemony.[13] In what follows, I will stress Schrödinger's own choices and views to this end.

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: 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 from transformation theory.

Second, the transformation theory replaced the morass of interpretation-adjacent

[13]For one, as Muller notes, Schrödinger is now committed to the energy basis as physically preferred because the postulate he provides—roughly, that the spectral-line intensities in wave mechanics are proportional to those of matrix mechanics—fails for every other basis.

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 a shortcut to get their operator calculus to display the correct (discrete) behavior when necessary:

> In the foregoing we have proceeded as if all variables would vary in a continuous domain, while physically the cases of interest are just those in which conditions are quantized, and therefore the variables also have to run through discontinuous ranges of values. However, for the moment, our proceeding is throughout quite consistent and comprehensive without these last discontinuous cases since we have expressly introduced improper functions $\delta(x - y)$ whose occurrence has no other meaning than that the corresponding variables are only able to take certain discrete values.

Others, notably Dirac, had done this as well. This was a problem:

> However, from the mathematical standpoint, the way of calculating covered, especially when one tries to treat such questions as the above regarding the statistical weights, is rather unsatisfying, since one is never sure to what extent the operations appearing are really to be permitted.

Indeed, as we glean from this last remark—which refers us to von Neumann's [von Neumann, 1927c]—it was not *only* a problem 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 (and how) his formalism was to be interpreted statistically (translation from Duncan and Janssen):

> Pauli considers the following generalization: Let q, β be two Hermitian quantum-mechanical quantities, which for convenience we assume to be continuous. Then there is always a function $\phi(q, \beta)$, such that $|\phi(q_0, \beta_0)|^2 dq$ measures the (conditional) probability that, for a given value β_0 of β, the quantity q has a value in the interval $q_0, q_0 + dq$. Pauli calls this function the probability amplitude.

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].[14] The centrality of the statistical interpretation is clear in Hilbert et al., too. 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]:

> With this it is recognized that the physical interpretation of the eigenfunctions of Pauli given in the introduction is a special case of the general theory; for now $|\psi_n(x)|^2$ is the probability that the position coordinate of the system has a value between x and $x + dx$ when the system finds itself in the n-th state.

One immediately wonders, however, which states are allowable in this apparatus, i.e., whether there are states that remove this statistical character from the transformation theory. Consequently, the transformation theory quickly led to questions of the completeness (specifically, w.r.t. determinism) of quantum mechanics.

[14] However, also see fn. 252 of [Mehra and Rechenberg, 2001, 210].

Thus, the situation was this as von Neumann began his own work on quantum mechanics. First, 'quantum mechanics' meant the transformation theory. Given that Schrödinger not only desired but effected an equivalence between matrix and wave mechanics, one presumes he would have accepted the arrival of the transformation theory, as well, at least to some degree. Second, there appeared to be a need for a theory of the Dirac delta and its ilk.[15] Third, Born's statistical interpretation was assumed to interpret the transformation theory, and there immediately came the question of the completeness of quantum mechanics. It was understood that a theory of the Dirac delta might shed light on this question.

4 Von Neumann's Axiomatic Completion of Quantum Mechanics—In 1927

It was von Neumann's aim to answer the completeness question for quantum mechanics using the axiomatic method. Two problems stood in his way, however. First, a theory of the Dirac delta apparently still lay in the way of a definitive answer to the completeness question. Second, it was not clear what, precisely, was being assumed in quantum mechanics. I discuss these in turn, giving only brief attention to the former. However, as an interlude, I introduce a debate between von Neumann and Schrödinger that I place sometime in 1927 or early 1928.

First, 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 [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: $\delta\left(x\right)=0$,for $x\neq 0$, $\int_{-\infty}^{\infty}\delta\left(x\right)dx=1$.
>
> . . . But a common deficiency of all these methods is that they introduce in-

[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. [I recommend the latter reference on recommendation of the former; I have not yet found a copy of this work to review it myself.]

> 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.
>
> 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 unitary and rigorous way.

Two things matter from this work. First, von Neumann placed quantum mechanics on a rigorous mathematical footing. In so doing, he entirely avoided the Dirac delta function and, hence, showed that it was irrelevant to the completeness question for quantum mechanics. That is, quantum mechanics now had a *unitary* formalism. Second, he understood the core of quantum mechanics to consist in the solving of eigenvalue problems, much like Schrödinger did wave mechanics (in certain moods).

Before discussing von Neumann's "Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik," I want to introduce a little-known debate that occurred between Schrödinger and von Neumann; with it on the table, we can discuss the details of von Neumann's induction. The topic was the completeness of quantum mechanics. The background was this: in early 1927, there was general agreement among the Göttingen theorists and Dirac that quantum mechanics was essentially statistical [Mehra and Rechenberg, 2001, 210–1]. Moreover, despite some internal disagreement about whether this reflected something about the quantum domain per se, there was high confidence that the mathematical formalism was faithful to reality. In Jordan, this led to a sharp criticism of Schrödinger's *Abhandlungen zur Wellenmechanik* [Schrödinger, 1927a] for its reliance on "guiding principles" opposed by "the majority of physicists" (cited and translated in Mehra and Rechenberg, 211). While ending with an apology for the "unfriendly tone," Jordan did not back down from his characterization in a follow-up letter to Schrödinger (16 May 1927; translated in Mehra and Rechenberg, 212):

> It seemed to me that I only reproduced your own views by stating that your interpretation stands in harsh contrast to the fundamental assumptions of Bohr. Now it is correct that all quantum-mechanical theoreticians—Bohr, Born, Heisenberg, Pauli, Dirac, Wentzel, Oppenheimer, Gordon, von Neumann—are convinced that the fundamental assumptions of Bohr must

> be upheld without exception. Therefore, I do not believe that I exaggerated when I stated that the majority of physicists take a standpoint different from yours.

Here we see the front end of the divide we later see between the quantum mechanical theoreticians on one hand and the determinists, including Einstein, Schrödinger, Planck, and von Laue, on the other. Note that von Neumann made the list as a quantum mechanical theoretician. In this sense, then, von Neumann is no different from his fellow quantum mechanical theoreticians.

Von Neumann's debate with Schrödinger reveals the extent to which he was thinking physically and, indeed, thinking about extensions of quantum mechanics. Wigner relates the events in his discussion of Bell's inequality (Wigner 1970, 1009):[16]

> The discussion of Von Neumann, most commonly quoted, is that contained in his book [...], Secs. IV.1 and IV.2. As an old friend of Von Neumann, and in order to preserve historical accuracy, the present writer may be permitted the observation that the proof contained in this book was not the one which was principally responsible for Von Neumann's conviction of the inadequacy of hidden variable theories. Rather, Von Neumann often discussed the measurement of the spin component of a spin-$\frac{1}{2}$ particle in various directions. Clearly, the probabilities for the two possible outcomes of a single such measurement can be easily accounted for by hidden variables [...]. However, Von Neumann felt that this is not the case for many consecutive measurements of the spin component in various different directions. The outcome of the first such measurement restricts the range of values which the hidden parameters must have had before that first measurement was undertaken. The restriction will be present also after the measurement so that the probability distribution of the hidden variables characterizing

[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 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].

> the spin will be different for particles for which the measurement gave a positive result from that of the particles for which the measurement gave a negative result. The range of the hidden variables will be further restricted in the particles for which a second measurement of the spin component, in a different direction, also gave a positive result. A great number of consecutive measurements will select particles the hidden variables of which are all so closely alike that the spin component has, with a high probability, a definite sign in all directions. However, according to quantum mechanical theory, no such state is possible. Schrödinger raised the objection against this argument that the measurement of a spin component in one direction, while possibly specifying some hidden variables, may restore a random distribution of some other hidden variables. It is this writer's impression that Von Neumann did not accept Schrödinger's objection. His point was that the objection presupposed hidden variables in the apparatus used for the measurement. Von Neumann's argument needs to assume only two apparata, with perpendicular magnetic fields, and a succession of measurements alternating between the two apparata. Eventually, even the hidden variables of both apparata will be fixed by the outcomes of many subsequent measurements of the spin component in their respective directions so that the whole system's hidden variables will be fixed. Von Neumann did not publish this apparent refutation of Schrödinger's objection.

Thus, von Neumann was convinced by a *physical* argument that quantum mechanics could not be extended with hidden variables.

Obviously, non-trivial assumptions are being made here, both by von Neumann and by Schrödinger. For ease of understanding, we can put these in our own terms. First, the hidden variables von Neumann has in mind are such that their measurement reveals a property the system already had; this is clear as Wigner says the outcome of a subsequent measurement "restricts the range of values which the hidden parameters must have had before that first measurement was undertaken." Second, the purpose of a would-be hidden variable theory, according to von Neumann, is to "complete" the state description of a system so as to return determinism; we gather this from the general setup, where the goal is to narrow in on initial conditions that can predict future measurements. (In our terms, we only care about effectively-measurable, non-contextual quantities.) Of course, if one is taking quantum mechanics' probability relations for granted—call this assumption (*Uncertainty*)—this is not possible in combination with the first assumption. A detailed version of this line of thinking echoes Heisenberg re: the indeterminacy relations [Beller, 1985, 346–8]. Now we come to Schrödinger's objection and von Neumann's reply. From our vantage point, we see Schrödinger as gesturing toward the contextuality response, i.e., that while measurement may reveal aspects of the system, the apparatus itself influences measurement, too. To a Bohmian, this therefore looks like just the right response.

It is tempting to read von Neumann as begging the question against Schrödinger, even as winning him over with rhetoric rather than substance. I think this is too quick, however, and ultimately the episode deserves more scrutiny than I can manage here. Nevertheless, I will make a few observations. First, Schrödinger was already doubting the prospects of his interpretation in late 1926 in light of recent experimental work, so it would not have been von Neumann alone who convinced him; relatedly, it is clear from his subsequent writings that Schrödinger was not one to back down in the face of social pressure. Second, Schrödinger's (and de Broglie's) hope had been for a genuine *matter wave* theory, not a pilot-wave-in-configuration-space theory. Indeed, de Broglie abandoned his approach when it became clear to him this goal was unattainable. Finally, Schrödinger himself sometimes appeared to agree with the quantum mechanical theoreticians—for instance in his 5 May, 1928, letter to Bohr (translation from [Bohr, 1985, 46–8]):

> One further remark: If you want to describe a system, e.g., a mass point by specifying its p and q, then you find that this description is only possible with a limited degree of accuracy. This seems to me very interesting as a limitation in the applicability of the old concepts of experience. But it seems to me imperative to demand the introduction of *new* concepts, with respect to which this limitation *no longer* applies.

So far so good for a Bohmian (modulo the final remark implying he meant to circumvent the uncertainty relations altogether). Except he continues:

> Because what is in principle unobservable should not at all be contained in our conceptual scheme, it should not be possible to represent it within the latter. In the *adequate* conceptual scheme it should no longer appear as if our possibilities of experience were limited through unfavorable circumstances.

Putting aside Schrödinger's reaction to this disagreement for the moment, what this event demonstrates is that von Neumann, among others, believed that quantum mechanics was incompatible with hidden variables on more-or-less intuitive grounds. However, it remained unclear whether the statistical interpretation derived *from* the transformation theory or was merely *assumed* of it. In this way, the situation was analogous to the status of Archimedes' Axiom prior to Hilbert's axiomatization of geometry.

This brings us to the second problem von Neumann faced in 1927. On the Hilbertian understanding, axiomatic theories are given relative to an agreed upon set of propositions, and they are constructed with the help of concepts that set out some or other propositions as fundamental. When Hilbert et al. began the work we find in their joint paper, the scope of quantum theories and especially the concepts involved were unclear. However, with the transformation theory and its statistical interpretation at hand, quantum theorizing was finding its footing on quantum mechanics. Despite this, the foundational concepts remained underspecified, particularly the status of the statistical interpretation. Indeed, as far as von Neumann was concerned, the statistical interpretation had merely been assumed [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., his Hilbert space formulation of it. That is, what did everyone 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

[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.

Note that the two features of the axiomatic method I highlighted earlier are present here. First, the induction is transparently *provisional*—"to be attempted" is weaker even than the more typical academic-ese of "given." This should not be a surprise, either, for in this context *any* attempt is an improvement upon merely assuming the statistical interpretation. Second, he is transparent about the assumptions that he is making. More importantly, he is providing an inductive structure *for* quantum mechanics—that is, he intends to get back quantum mechanics with whatever assumptions he lands on. These assumptions are made *relative* to quantum mechanics. In addition, he makes it clear that his analysis is relative to—that is, uses—the Hilbert space formalism. These are cardinal sins for Bell-style axiomatics; hence, he is working toward an axiomatic completion, not providing an axiomatic reconsideration.

Not surprisingly, the assumptions he makes end up looking much like those he assumed in his disagreement with Schrödinger. Summarizing the (non-probabilistic) assumptions qualitatively, he says in closing [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 is assuming (*Uncertainty*) Heisenberg's understanding of the uncertainty relations in 1 and 2;[18] (*Quantities*) the quantum mechanical way of representing quantities, which restricts one to just those that are effectively measurable; and (*Probability*) the ordinary probability theory.

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 $f(\mathfrak{R})$.

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). Further, von Neumann defines dispersion-free and pure states:[19]

α An $Exp(\mathfrak{R})$ is *dispersion-free* if $Exp(\mathfrak{R}^2) = Exp(\mathfrak{R})^2$

β An $Exp(R)$ is *pure* if $Exp(\mathfrak{R}) = \alpha Exp'(\mathfrak{R}) + \beta Exp''(\mathfrak{R}), \alpha, \beta > 0, \alpha + \beta = 1$ implies $Exp(\mathfrak{R}) = Exp'(\mathfrak{R}) = Exp''(\mathfrak{R})$.

[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].

[19] Von Neumann does not label these definitions as he does in his book. Nevertheless, I use these labels for ease of referring back to them.

Von Neumann's assumptions A., B. and definitions α, β are not exceptional here. A. was a common assumption for probability at the time, and it was well-fitted to the quantum mechanical view. 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.[20] One might also be worried that α ruled out an important class of hidden variable theories. However, recall from above that the *point* of a hidden variable theory was understood, by seemingly all parties, to be the identification of observable variables returning determinism. But these are just those that would give dispersion-free states. Indeed, Schrödinger himself seemed to accept A. and α;[21] however, given other remarks from the time, it is not clear he held them consistently or reflected on their significance deeply.

Thus we see in his debate with Schrödinger, in his mathematical founding, and in his inductive founding that von Neumann is unabashedly assuming quantum mechanics (or in the latter what is the same, the assumptions that give rise to quantum mechanics). In his debate with Schrödinger, we saw expressed the general presumption that quantum mechanics *per se* can contain no hidden variables. Likewise, we have seen that the transformation theory was taken even by von Neumann himself to be the more-or-less final form of quantum mechanics—though with the caveat that the lack of a theory of the Dirac delta function mucked up a proper understanding of its kinematic completeness. In his mathematical foundations, von Neumann showed that such a theory of the Dirac delta was unnecessary, for the transformation theory could be reinvented from the ground up without reference to it. Indeed, what von Neumann showed was that exactly what one needed to solve the central problem of

[20] Duncan and Janssen [2013, 247] (rightly) point out that "[w]hile it may be reasonable to impose condition (A) on directly measurable quantities, it is questionable whether this is also reasonable for hidden variables.". However, this misses that von Neumann, by intending only to capture quantum mechanics as it existed, *meant* to treat only directly measurable quantities.

[21] For instance, Schrödinger strongly endorsed von Neumann's "Beweis des Ergodensatzes und des H-Theorems in der neuen Mechanik" [von Neumann, 1929] in a December 1929 letter to the latter, saying "The idea of linking the actual operators of quantum mechanics with real measurements contained a dissonance which has now been fundamentally resolved. By using these new concepts extensively it will be possible to achieve a real mapping of the real measurements to the scheme of quantum mechanics, and then the same scheme will be satisfying" (AHQP, Dec 1929 letter). This work explicitly assumed Heisenberg's view of the uncertainty relations and the so-called quantum principle, that quantities correspond to Hermitian operators on a Hilbert space. These are just (*Quantity*) and (*Uncertainty*), and presumably (*Probability*) was also assumed. Additionally, Schrödinger effectively accepted this as late as 1935, where he assumed would-be hidden states are dispersion-free and respect the functional relations between Hermitian operators. See [Bacciagaluppi and Crull, 2009].

quantum mechanics—the eigenvalue problem, around which Schrödinger's wave mechanics was built—was the Hilbert space formalism. The sole remaining question, then, was whether von Neumann's Hilbert space formalism *truly* captured quantum mechanics by ruling out hidden variables: if the Hilbert space formalism is the unique representation of quantum mechanics, then hidden variables should be impossible.

There things stood in 1927, and it was not until 1932 that there came a proof to this effect. Except, this matter was essentially settled in 1927—all of the required tools were there! The essential ingredients in his later proof are the trace formula $Exp(R) = Tr(UR)$ and the definition α of dispersion-free expectation values; once those are in place, the proof is trivial, involving some quick definition-chasing. Anyone reading the paper should have seen this, and it is highly unlikely that von Neumann did not see this himself. So why no proof until 1932?

5 A Textbook for Mathematicians

It is my contention that the proof did not show up until 1932 because there was no need to publish it until then. An important fact in this regard is that von Neumann's book was part of Courant's series of textbooks for lay-mathematicians, *Foundations* [Grundlehre] *of Mathematical Science* (subtitled *in Stand-Alone Presentations with Special Consideration for the Fields of Application*). While von Neumann's book's occurrence as part this series is naturally prominent in the front-matter of the German (Springer) publication, this fact is entirely obscured in subsequent printings. Nonetheless, this tells us that the audience was not expected to be familiar with the area, meaning that results needed to be especially explicit. Meanwhile, the 1927 works were published in the *Göttingen Nachricten* whose readers could be expected to be reasonably familiar with the techniques von Neumann borrowed from the likes of Schmidt, Courant, Toeplitz, Hellinger, and Hilbert. In the rest of this section, I will briefly characterize the book from the standpoint of the axiomatic method. In so doing, it will be clear that von Neumann was using Hilbert's axiomatic method.

One of the primary aims of von Neumann's book [von Neumann, 1932][von Neumann, 1955] was to determine whether the Hilbert space formalism—with the "induction" assumptions serving as its basis—could countenance hidden variables. We see hints of this already at the very beginning of the preface [von Neumann, 1955, vii]:[22]

[22] 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 unitary [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.[23]

Fitting for an axiomatization, von Neumann wants a "unitary" 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 existence of hidden variables. Firstly, von Neumann foreshadows the irrelevance of the Dirac delta fiction for developing quantum mechanics [von Neumann, 1955, ix]:

> The method of Dirac, mentioned above, (and this is overlooked today in a great part of quantum mechanical literature, because of the clarity and elegance of the theory) in no way satisfies the requirements of mathematical rigor—not even if these are reduced in a natural and proper fashion to the extent common elsewhere in theoretical physics. For example, the method adheres to the fiction that each self-adjoint operator can be put in diagonal form. In the case of those operators for which this is not actually the case, this requires the introduction of "improper" functions with self-contradictory properties. The insertion of such a mathematical "fiction" is frequently necessary in Dirac's approach, even though the problem at

[23]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 'unitary' 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.

> hand is merely one of calculating numerically the result of a clearly defined experiment. There would be no objection here if these concepts, which cannot be incorporated into the present day framework of analysis, were intrinsically necessary for the physical theory. Thus, as Newtonian mechanics first brought about the development of the infinitesimal calculus, which, in its original form, was undoubtedly not self-consistent, so quantum mechanics might suggest a new structure for our "analysis of infinitely many variables"—i.e., the mathematical technique would have to be changed, and not the physical theory. But this is by no means the case. It should rather be pointed out that the quantum mechanical "Transformation theory" can be established in a manner which is just as clear and unitary [einheitliche], but which is also without mathematical objections. It should be emphasized that the correct structure 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.

As noted above, this dissolves the lingering questions surrounding the transformation theory, and, at the same time, paves the way for determining uniqueness. Not surprisingly, then, von Neumann secondly addresses the axiomatically-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]:

> 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 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 (Uncertainty) are being assumed.) As they occur in the book, these are the arguments of IV.1—2 and VI, respectively.[24]

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]:

> 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.

[24] 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. I address the latter argument elsewhere.

Thus in the main, chapter II redescribes von Neumann's work on the Hilbert space formalism, which began with [von Neumann, 1927c]. 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][25] 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.
>
> Until a more precise analysis of the statements of quantum mechanics will enable us to test [prüfen][26] 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*—which 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

[25] 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.

[26] 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.

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 to a mathematical formalism (Hilbert space formalism) and *provisional* insofar as quantum mechanics itself is provisional.

6 *No* Hidden Variables for Quantum Mechanics

Let us now consider IV.1—2 in this light. By this point, the contents should not surprise us: von Neumann will assume the "inductive" basis of quantum mechanics from his [von Neumann, 1927a]—the "qualitative basic assumptions"—and examine the possibility of hidden variables. Indeed, this is precisely what happens. I sketch von Neumann's examination in this section.

First, von Neumann makes plain his "basic, qualitative" assumptions. He begins by characterizing the kinds of quantities and relations thereof being considered [von Neumann, 1955, 297]:

> Let us forget the whole of quantum mechanics but retain the following. Suppose a system $\mathfrak{S}$ is given, which is characterized for the experimenter by the enumeration of all the effectively measurable quantities in it and their functional relations with one another. With each quantity we include the directions as to how it is to be measured—and how its value is to be read or calculated from the indicator positions on the measuring instruments. If $\mathfrak{R}$ is a quantity and $f(x)$ any function, then the quantity $f(\mathfrak{R})$ is defined as follows: To measure $f(\mathfrak{R})$, we measure $\mathfrak{R}$ and find the value a (for $\mathfrak{R}$). Then $f(\mathfrak{R})$ has the value $f(a)$. As we see, all quantities $f(\mathfrak{R})$ ($\mathfrak{R}$ fixed, $f(x)$ an arbitrary function) are measured simultaneously with $\mathfrak{R}$. This is a first example of simultaneously measurable quantities. In general, we call two (or more) quantities $\mathfrak{R}$, $\mathfrak{S}$ simultaneously measurable if there is an arrangement which measures both simultaneously in the same system—except that their respective values are to be calculated in different ways from the readings. (In classical mechanics, as is well-known, all quantities are simultaneously measurable, but this is not the case in quantum mechanics, as we have seen in III.3.) For such quantities, and a function $f(x, y)$ of two variables, we can also define the quantity $f(\mathfrak{R}, \mathfrak{S})$. This is measured if we measure $\mathfrak{R}, \mathfrak{S}$ simultaneously—if the values a, b are found

> for these, then the value of $f(\mathfrak{R}, \mathfrak{S})$ is $f(a, b)$. But it should be realized that it is completely meaningless to try to form $f(\mathfrak{R}, \mathfrak{S})$ if $\mathfrak{R}, \mathfrak{S}$ are not simultaneously measurable: there is no way of giving the corresponding measuring arrangement.

Von Neumann then 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 the uncertainty relations to be general and what are essentially responsible for the intractability of a hidden variable theory (i.e., (*Uncertainty*)). Discussing the attempt to identify hidden variables through successive measurements, von Neumann says that measurement changes the systems such that no progress is made [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 1927c), namely (*Probability*), (*Uncertainty*), and (*Quantities*). (This last is a bit obscured, but it comes in the fact that the quantities are effectively measurable.) Nothing is new so far, even if the discussion is longer.

Von Neumann then turns to a discussion of hidden variables, beginning by summarizing the intuition one gathers from quantum mechanics (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 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. 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*), (*Uncertainty*), and (*Quantities*) above. Here we should recall that the axiomatic goal is to so precisely define the mathematical formalism that it is the unique characterization of the (informal) theory; if this has been done, then the mathematical formalism should *agree* with any determinations that the (informal) theory makes. In this case, then, a successful axiomatization of quantum mechanics would mean that the Hilbert space formulation agrees with (informal) quantum mechanics that hidden variables are not possible. As in [von Neumann, 1927a], von Neumann thinks of these assumptions as coming in two types: there are the assumptions of probability and then assumptions specific to quantum mechanics. First, he considers the assumptions of probability, the first several being:

A. If a quantity $\mathfrak{R}$ is always 1, then its expectation is 1, i.e., $Exp(\mathfrak{R}) = 1$;

B. for each $\mathfrak{R}$ and each real number a, $Exp(a\mathfrak{R}) = aExp(\mathfrak{R})$;

C. if $\mathfrak{R}$ is non-negative by nature, then $Exp(\mathfrak{R}) \geq 0$;

D. if the quantities $\mathfrak{R}, \mathfrak{S}, ...$ are simultaneously measurable, then $Exp(\mathfrak{R}+\mathfrak{S}+\cdots) = Exp(\mathfrak{R}) + Exp(\mathfrak{S}) + \cdots$.

A.–C. are obviously trivial, and as von Neumann notes, D. is a theorem of probability. He also notes that it is formulated only for simultaneously measurable $\mathfrak{R}, \mathfrak{S}, \ldots$ "since otherwise $\mathfrak{R} + \mathfrak{S} + \ldots$ is meaningless" [von Neumann, 1955, 308–9]. Yet he continues:

> But the algorithm of quantum mechanics contains still another operation, which goes beyond the one just discussed: namely, the addition of two arbitrary quantities, which are not necessarily simultaneously observable. This operation depends on the fact that for two Hermitian operators, R, S, the sum $R + S$ is also an Hermitian operator, even if the R, S do not commute, while, for example, the product RS is again Hermitian only in the event of commutativity (cf. II.5). In each state ϕ the expectation values behave additively: $(R\phi, \phi) + (S\phi, \phi) = ((R + S)\phi, \phi)$ (cf. E2., III.1). The same holds for several summands. We now incorporate this fact into our general set-up (at this point not yet specialized to quantum mechanics):
>
> E. if $\mathfrak{R}, \mathfrak{S}, ...$ are arbitrary quantities, then there is an additional quantity $\mathfrak{R} + \mathfrak{S} + \cdots$ (which does not depend on the choice of the $Exp(\mathfrak{R})$-function), such that $Exp(\mathfrak{R} + \mathfrak{S} + \cdots) = Exp(\mathfrak{R}) + Exp(\mathfrak{S}) + \cdots$.
>
> If $\mathfrak{R}, \mathfrak{S}$ are simultaneously measurable, this must be the ordinary sum (by D.). But in general the sum is characterized by E. only in an implicit way, and it shows no way to construct from the measurement directions for $\mathfrak{R}, \mathfrak{S}, \ldots$ such directions for $\mathfrak{R} + \mathfrak{S} + \cdots$.

But this, combined with D., is just A. in [von Neumann, 1927a]. 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 (*Uncertainty*)—*is* being assumed.

Having formalized the probabilistic aspects of the question, in light of (*Probability*), (*Quantities*), and (*Uncertainty*) just as before,[27] von Neumann then characterizes 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

[27] Strictly speaking, von Neumann does not use (*Uncertainty*) but a generic indeterminacy relation so as to "not specialize to quantum mechanics." It is important to remember that quantum mechanists believed that Heisenberg's indeterminacy relationship arose in a (nearly) strictly classical fashion, so that *any* physical theory (where energy is discrete) must give rise to a measurement-induced indeterminacy relation. In a sense, this is where the (over)confidence in the generality of Heisenberg's uncertainty relation originates. See, e.g., von Neumann's III.4.

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

Likewise, the section ends unremarkably as concerns the status of hidden variables: they cannot be added to the Hilbert space formalism. Unclimactically, then, the uniqueness question has been answered, and in the negative: quantum mechanics (i.e., (*Probability*), (*Quantities*), and (*Uncertainty*)) cannot be extended to include hidden variables, hence insofar as Schrödinger accepted (*Probability*), (*Quantities*), and (*Uncertainty*), he could not have found hidden variables.

Finally, von Neumann turns to discuss the implications of this result. Summarizing the technical meaning, he says [von Neumann, 1955, 323]:

> We have derived all these results from the purely qualitative conditions A'., B'., α), β), I., II.
>
> Hence, within the limits of our conditions, the decision is made and it is against causality; because all ensembles have dispersions, even the homogeneous.

But as we know, A'., B'., α), β), I., II. are just the formalization of (*Probability*), (*Quantities*), and (*Uncertainty*), i.e., quantum mechanics. Rephrasing, then, von Neumann continues [von Neumann, 1955, 324](italics mine):

[28] Note also that von Neumann says these rules "are valid" in such a case, rather than that such rules "are true" or something of the sort: he is signaling that (*Quantities*) has already been assumed.

> It should be noted that we need not go any further into the mechanism of the "hidden parameters," since we now know that the established results of quantum mechanics can never be re-derived with their help. In fact, we have even ascertained that it is impossible that the same physical quantities exist with the same function connections (i.e., that I., II. hold), if other variables (i.e., "hidden parameters") should exist in addition to the wave function.
>
> Nor would it help if there existed other, as yet undiscovered, physical quantities, in addition to those represented by the operators in quantum mechanics, because the relations assumed by quantum mechanics (i.e., I., II.) would have to fail already for the by now known quantities, those that we discussed above. *It is therefore not, as is often assumed, a question of re-interpretation of quantum mechanics,–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.*

This is unambiguously correct. Von Neumann proved that quantum mechanics is uniquely (w.r.t. its kinematical structure) characterized by the Hilbert space formalism, i.e., A'., B'., α), β), I., II.. It follows from this that quantum mechanics does not admit of any re-interpretation. Hence, for another description than the statistical one to be possible—namely, a hidden-variable description—one of quantum mechanics' assumptions must be false. And von Neumann does not consider this impossible [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.[29]

At the same time that this answers the question of extending quantum mechanics with hidden variables, it also achieves the goals of the axiomatic method. First, von Neumann has ordered the facts of quantum mechanics. It is straightforward how. Second, he oriented our future research. This orientation has not been sufficiently appreciated to date. I briefly discuss this in the conclusion.

Before moving on, I want to reply to a possible concern. The concern runs as follows. I have here characterized von Neumann's use of the axiomatic method as successful for providing an axiomatic completion of quantum mechanics. As we all know, quantum mechanics possesses not one but *two* dynamics, a non-linear one for measurement contexts and a linear one for everything else. In this way, one may say that 'measurement' is the switch on which the entire theory's consistency turns: absent an account of measurement, the theory is logically inconsistent because it gives *two* answers to any given question. Thus, the concern continues, I must be saying von Neumann's axiomatization of quantum mechanics was successful *despite being inconsistent* since he did not provide a complete theory of measurement. But this is ridiculous!

While it appears straightforward, this argument makes a non-trivial assumption in order to ensure a conflict between the two dynamics arises somewhere. The assumption is that fundamental theories are universally valid, hence that in virtue of being fundamental—whatever that means—quantum mechanics must be universally valid. Undoubtedly, this assumption is typical in philosophy of science these days. Yet not only does von Neumann nowhere commit himself to the universal validity of quantum mechanics, I am unaware of his committing to *any* physical theory being universally valid, even tentatively. Indeed, committing to an axiomatized theory as universally valid would be anathema to the axiomatic method. Recall from §2 that an axiomatization is a "lossy" representation of the area of knowledge it means to order; this is just how the philosophy of mathematics guiding the axiomatization shakes out. Understood this way, one cannot demand that an axiomatization provide a complete "picture" of the world (to use Hertz's term). However, without this demand there is no reason

[29]Note that because the "formal theory"—the Hilbert space theory—was explicitly constructed to be quantum mechanical, and because he has now shown that it is the *unique* such formal theory, the ambiguity here between 'the formal theory of quantum mechanics' and 'quantum mechanics' is justified.

to suppose that, absent a complete theory of measurement, the two dynamics *must* conflict for von Neumann. Thus, since von Neumann didn't intend to give a complete picture of the world according to quantum mechanics, criticizing him for lacking one is about us, not him.

7 Conclusion: Orienting for Our Future

In conclusion I discuss the goal of the axiomatic method, particularly what it means to *orient* future research. To then orient ourselves, I wish to 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 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 axiomatization 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, as I claim, it changed the places folks looked for hidden variable interpretations and dampened curiosity concerning the physical significance of the Dirac delta. However, at the same time it tells us where we might fruitfully focus attention: (*Probability*), (*Quantities*), and (*Uncertainty*). 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):[30]

> 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.

This is the path of Bohm, de Broglie, Bell, and others, and the essential feature is that *physical* or *epistemological* considerations related to (*Probability*), (*Quantities*), and (*Uncertainty*) 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 quantum theory.[31]

However, the axiomatic method is *also* about enriching mathematics. Thus, on the other hand, von Neumann's axiomatization uses physical facts to tell something to the mathematician, namely, that attention should be focused on algebras of non-commuting operators, orthomodular lattices, quantum logics, and the like. This has proven fruitful in mathematics, as von Neumann himself ensured. And 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. This is the axiomatic reconsideration path that Misra, Gleason, Jauch, Piron, and others have traveled [Misra, 1967][32]:

> The alternative left to us is to proceed axiomatically 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

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

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

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

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.

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 distinct but related futures. Just as Hilbert had wanted, von Neumann effectively summarized and clarified where we *had 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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