The statistical interpretation: Born, Heisenberg and von Neumann, 1926–27

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1 Introduction

In 1954, Max Born was awarded the Nobel prize for physics 'for his fundamental research in quantum mechanics, especially for his statistical interpretation of the wavefunction'.[1] For the Nobel committee this presumably meant what by then was the standard reading of the wavefunction: a state of a quantum system that determines irreducible probabilities for the results of quantum measurements, as arguably codified in the textbooks by Dirac (1930) and von Neumann (1932). As the latter explicitly writes in his section on 'The statistical interpretation' (1932, Section III.2, p. 109):

> This conception of quantum mechanics, which recognises its statistical statements as the true form of the laws of nature and gives up the principle of causality, is the so-called statistical interpretation. It was formulated by M. Born. It is the only interpretation of quantum mechanics consistently implementable today [...]

Almost the same words recur in the conclusion of a little paper published by Einstein in 1953 in the *Festschrift* for Born's retirement from the University of

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[1]From `https://www.nobelprize.org/prizes/physics/1954/summary/`, consulted 13 August 2020.

Edinburgh: 'the only interpretation of the Schrödinger equation acceptable so far is the statistical interpretation given by Born' (Einstein 1953, pp. 39–40). What Einstein had in mind, however, was something *completely different*. In this paper Einstein took a macroscopic ball (say 1mm in diameter) bouncing between the walls of a box. Quantum mechanically we can describe it using a stationary wave, namely an equal-weight superposition of a wave travelling from left to right and one travelling from right to left. Einstein noted that this description yields the correct statistics, namely a uniform probability distribution for the position of the particle in the box, and probability 1/2 each for momentum directed left or right. But it is clearly not a description of an *individual macroscopic ball*.[2] Hence (Einstein 1953, pp. 39–40):

> the only interpretation of the Schrödinger equation acceptable so far is the statistical interpretation given by Born. This, however, does not yield a real description of the individual system, but only statistical statements for ensembles of systems.

Presumably, Einstein was writing tongue-in-cheek, since Born and he heartily *disagreed* about the issue of the completeness of quantum mechanics. But Einstein's understanding of the term 'statistical interpretation' is far closer to Born's original ideas as presented in 1926 than what von Neumann and presumably the Nobel committee understood. As a matter of fact, the 'statistical interpretation' underwent radical change in a very short time (1926–27) – even though this change seems to have been subsequently largely forgotten (among others by Born himself[3]).

This chapter will sketch this development. I shall distinguish three phases, to be discussed respectively in Sections 2–4:

- June–October 1926: Born's papers on collisions and on the adiabatic theorem (as well as Dirac's 'On the theory of quantum mechanics');
- October 1926–October 1927: Heisenberg's and Jordan's papers on fluctuation phenomena, Dirac's and Jordan's transformation theory, Heisen-

[2] Einstein uses this example also as the basis for his criticism of Bohm's (1952a,b) theory, because although in the latter the individual ball does have a position and a momentum, it is motionless *until* we open the box: this is not the macroscopic behaviour we wish to recover. Cf. Myrvold (2003) and Bacciagaluppi (2016a) for discussions.

[3] See e.g. Beller (1990).

berg's uncertainty paper, and Born and Heisenberg's joint report at the 1927 Solvay conference;

- October–November 1927: von Neumann's 'Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik' ['Probabilistic construction of quantum mechanics'].

As we shall see, it is the first phase that makes intelligible why Einstein could talk of 'the statistical interpretation given by Born' meaning that ψ is an incomplete description of an individual quantum system. The second phase is closer to what we now take to be standard quantum mechanics, in particular with an increased emphasis on the role of measurements. (But there were marked differences in the understanding of the theory among the likes of Dirac, Jordan, and Born and Heisenberg.) By the time von Neumann had finished with it (with Born's blessing), the 'statistical theory' had essentially become what we know from the later textbooks. Von Neumann had also given a complete characterisation of the statistical ensembles in quantum mechanics, in particular showing that there are no ensembles that are dispersion-free for all quantum-mechanical quantities.[4] This last result appears to establish that indeterminism in quantum mechanics is irreducible, and contrasts starkly with what Einstein appears to understand under 'the statistical interpretation given by Born'. The final Section 5 will accordingly review the relation between the statistical interpretation and indeterminism.[5]

2 Phase I: Born

The immediate background for Born's introduction of the statistical interpretation was the conflict between matrix mechanics and wave mechanics. As

[4]That is, for every ensemble there are quantities with a non-trivial distribution of values (non-zero dispersion).

[5]This chapter is a much updated version of a lecture given at the 5th Tübingen Summer School in the History and Philosophy of Science, August 2016. The lecture was itself based on material in Bacciagaluppi and Valentini (2009) and Bacciagaluppi (2008), to which I refer the reader for further details, and on my talk on 'Von Neumann's no-hidden-variables theorem (and Hermann's critique)', given as a joint LogiCIC/LIRa seminar, University of Amsterdam, May 2016. When not otherwise noted, all translations are by myself and all emphases are original.

initially developed, the two theories shared some of the same successes, e.g. the calculation of the hydrogen spectrum (Pauli 1926, Schrödinger 1926). However, they were far from being 'equivalent'.[6] They also employed two fundamentally different physical pictures, suggesting in fact the potential for subsequent empirical disagreement.[7]

In today's terminology, the wave mechanical understanding of the wavefunction ψ was as an 'ontic state', e.g. as literally representing a smeared-out electron. Schrödinger was able to interpret quantisation of energy in terms of the discreteness of eigenoscillations, and hoped to derive other quantum phenomena as arising from his continuous and deterministic wave equation. (For instance, he developed a treatment of radiation phenomena still used today as a semiclassical approach.[8])

The matrix mechanical picture instead assumed that systems were always in stationary states, and randomly performed quantum jumps between them.[9] Matrices described collectively the possible states and the possible transitions (including selection rules), but there was no object providing a description of the state of a system. Radiation intensities (transition probabilities) were determined via correspondence arguments.[10]

Even its proponents, however, thought that there were open problems within matrix mechanics. As Born and Heisenberg retrospectively put it in their Solvay report (Bacciagaluppi and Valentini 2009, p. 383 [translation slightly amended]):

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[6] See the classic discussion by Muller (1997a,b, 1999).

[7] Cf. Bacciagaluppi and Valentini (2009, Section 4.6) and Bacciagaluppi and Crull (2021, Section 3.3).

[8] Cf. e.g. Bacciagaluppi and Valentini (2009, Section 4.4).

[9] This is not in fact stated explicitly but has to be read between the lines. The closest to a smoking gun is presumably Jordan's derivation of the blackbody fluctuation formula at the end of the 'three-man paper' by Born, Heisenberg and Jordan (1926). There Jordan uses a Boltzmannian approach, and modifies the Lorentz–Ehrenfest calculation for an element of a vibrating string. Specifically, he uses matrix variables instead of classical variables, formally obtaining wave and particle terms in the fluctuation formula. In order for this to be in fact a fluctuation formula, however, Jordan must believe that each string element (a part of the system) is jumping between states of different energies. For details, see Bacciagaluppi, Crull and Maroney (2017). Cf. also Duncan and Janssen (2008).

[10] It has recently been shown that the problem of intensities had been a driving concern in the development of matrix mechanics; see the wonderful paper by Blum *et al.* (2017).

> The most noticeable defect of the original matrix mechanics consists in that at first it appears to give information not about actual phenomena, but rather only about possible states and processes. It allows one to calculate the possible stationary states and processes. It allows one to calculate the possible stationary states of a system; further it states the nature of the harmonic oscillation that can manifest itself as a light wave in a quantum jump. But it says nothing about when a given state is present, or when a change is to be expected. The reason for this is clear: matrix mechanics deals only with closed periodic systems, and in these there are indeed no changes. In order to have true processes, as long as one remains in the domain of matrix mechanics, one must direct one's attention to a *part* of the system; this is no longer closed and enters into interaction with the rest of the system. The question is what matrix mechanics can tell us about this.

As discussed by Wessels (1980), aperiodic phenomena had been on Born's mind ever since he had started working on modifying Bohr's theory of the atom in 1921. In particular, Born and Franck (1925a,b) had written two papers together on the dynamical interaction between two atoms leading either to the atoms forming a molecule or coming apart again in a collision. This was followed by the first of Born's papers with Jordan,[11] which among other things contained a section on collisions in which they suggested investigating asymptotic motions, in particular in the context of the Ramsauer effect (Born and Jordan 1925a). After the momentous introduction of the new quantum kinematics by Heisenberg (1925), in their following paper Born and Jordan (1925b) announced further work on aperiodic phenomena, and Born noted the difficulties of doing so – involving something like continuous matrices – in the third section of the 'three-man paper' (Born, Heisenberg and Jordan 1926). His first concrete contribution to the problem was in fact work with Norbert Wiener on an 'operator formalism' that generalised the matrix formalism. They applied it to the free particle, obtaining in a rather contrived way something that looked like classical inertial motion (Born and Wiener 1926a,b).[12]

Born then turned to use *wave mechanics* to treat collisions in two fundamental papers (Born 1926a,b – the first of which was a 'preliminary communication'). The scenario treated by Born is the collision between an electron

[11] Jordan was originally one of Franck's assistants, with whom he co-authored the article on scattering for the *Handbuch der Physik* (Franck and Jordan 1926).

[12] See also Bacciagaluppi and Valentini (2009, Section 3.4) and Bacciagaluppi (2008, Section 2.2).

initially described by a plane wave (i.e. a stationary state) and an atom initially also in a stationary state. Born determined the asymptotic behaviour of the combined system solving the time-independent Schrödinger equation by perturbative methods. For the case of an inelastic collision, the result is what one now calls an *entangled* wavefunction that is the superposition of a series of stationary states of the atom and a continuum of outgoing plane waves. Born thought that these components must clearly be the *possible* final states after the collision, and he identified the (mod-squared) expansion coefficients as the corresponding *probabilities*.[13] Since the initial state is a stationary state of the atom and a 'uniform rectilinear motion [*sic*]' of the electron, these probabilities are also equal to the transition probabilities, i.e. the probabilities for quantum jumps from the initial state to one of the possible final states.[14] As also first emphasised by Wessels (1980),[15] in these papers Born's 'statistical interpretation' concerns exclusively the distribution of the stationary states of the atom and of the free electron.

Importantly, the corresponding probabilities are not probabilities for 'finding' a system in a certain stationary state upon measurement: the atom and the electron (at least when the interaction is completed) are assumed to *be* in a stationary state. Thus, in line with Einstein's understanding, the Schrödinger wavefunction is *not* a complete description of a physical system, because in general it does not specify the actual state of a system. Note that nowhere in these papers does Born use the word 'state' to refer to anything but a stationary state. On the other hand, a description in terms of the actual stationary states need not be *causally* complete. Indeed, Born himself considers it plausible that the transition probabilities are an expression of

[13]The correct expression appears in the preliminary communication only in a footnote added in proof. Presumably independently, Dirac (1926, Section 5) also identified squared amplitudes in the energy basis expansion (the sum of which he noted was conserved) as the frequencies of the various stationary states in an ensemble. The problem treated by Dirac was that of an atom subject to an external perturbation, in particular an external electromagnetic field, and he rederived the Einstein coefficients in a special case (cf. Darrigol 1992, p. 333).

[14]For details, see e.g. Wessels (1980), Beller (1990) and Bacciagaluppi and Valentini (2009, Section 3.4).

[15]About earlier work she notes: 'Hund (1967, pp. 56–157) and Konno (1978, pp. 141–144) provide the only historical accounts of Born's work in which Born's original statistical interpretation is stated clearly enough to distinguish it from the interpretation now commonly attributed to Born' (Wessels 1980, p. 197).

genuine indeterminism, and he uses the picture of the wavefunction as a deterministically evolving 'guiding field'. But, as Beller (1990) emphasises, at this stage Born is open-minded about determinism being still possible in principle.

Born's proposed interpretation appears fully explicitly in a later paper from the same year, 'Das Adiabatenprinzip in der Quantenmechanik' ['The adiabatic principle in quantum mechanics'] (Born 1926c), in which he investigates and discusses his approach further (much more so than in the better-known collision papers). It turns out to be quite surprising:[16]

- Particles exist (at least when evolving freely).
- They are accompanied by de Broglie–Schrödinger waves.
- During free evolution, systems are always in stationary states.
- If at time $t = 0$ the wavefunction of a system is

  $$\psi(x,0) = \sum_m c_m \psi_m(x) \ , \tag{1}$$

  where the $\psi_m(x)$ are eigenfunctions of energy, then the expressions $|c_m|^2$ are the probabilities for the occurrence of the corresponding stationary states. Born calls them 'state probabilities' (he again reserves the term 'state' for stationary states).
- Consider the application of an external force (or an interaction): no 'anschaulich' representation of what takes place may be possible, but

[16] For more details see Bacciagaluppi (2008, Section 2.1). This paper is not discussed explicitly by Wessels (1980), but it is covered in Beller (1990). Despite Beller's excellent discussion of the wave and particle pictures in Born's work, I find it difficult to assess in what precise sense Born thinks of particles: it is quite possible that at least for the case of free particles (or even stationary orbits?) he envisages a picture of well-defined geometric trajectories, to be given up during collisions. Beller also points out how initially Born appears to have been enthusiastic about Schrödinger's idea of describing quantum jumps as a continuous process. I would like to emphasise, however, that already in the first collision paper the final (entangled) wavefunction is strictly incompatible with it. Born's views on quantum mechanics from this period are also presented informally in two near-identical papers (Born 1927a,b), which were expanded versions of a talk given by Born in August 1926.

'quantum jumps' occur, in the sense that after the external intervention ceases, the system will generally be in a different stationary state.

- The evolution of the state probabilities is nevertheless well-defined and is determined for arbitrary times $t = T$ by the solution of the Schrödinger equation,

$$\psi(x,T) = \sum_n C_n \psi_n(x) \ . \tag{2}$$

- For the case that $\psi(x,0) = \psi_m(x)$ for some m, the solution can be determined explicitly (in terms of the external potential). Writing for this case

$$\psi(x,T) = \sum_n b_{mn} \psi_n(x) \ , \tag{3}$$

  the expressions $|b_{mn}|^2$ are then the transition probabilities for the quantum jump from $\psi_m(x)$ at $t = 0$ to $\psi_n(x)$ at $t = T$. (In the 'adiabatic limit' they tend to δ_{mn}, thus arguably recovering classical behaviour.)

- In the general case $\psi(x,0) = \sum_m c_m \psi_m(x)$, the state probabilities at time $t = T$ have the form

$$|C_n|^2 = |\sum_m c_m b_{mn}|^2 \ . \tag{4}$$

What Born says about this general case is intriguing (Born 1926c, p. 174):

> The quantum jumps between two states labelled by m and n thus do *not* occur as independent events; for in that case the above expansion should be simply $\sum_m |c_m|^2 |b_{mn}|^2$.

This suggests an inaccurate understanding of interference, as due to *different electrons* not jumping independently (which seems only confirmed by Born's discussion of 'natural light' later in the paper).[17] Understanding interference was to play an important role in the next phase of development of the statistical interpretation.

[17] In footnotes on pp. 174 and 180, Born references Dirac (1926) as also pointing out that the quantum jumps are not independent (and become so if one averages over the phases), and generally noting that their points of view are in agreement. Dirac writes explicitly: 'One cannot take spontaneous emission into account without a more elaborate theory

3 Phase II: from Born to Born & Heisenberg

Born's collision papers managed to make *both* Heisenberg and Schrödinger furious. Heisenberg, because Born had successfully solved an open problem in matrix mechanics using wave mechanics; Schrödinger, because at the same time Born had reinterpreted wave mechanics 'in Heisenberg's sense'.[18]

Soon, however, Heisenberg (1927a) (following an idea suggested by Pauli in a letter of 19 October 1926) as well as Jordan (1927a) translated Born's results into the language of matrix mechanics – precisely by looking at what matrix mechanics could say about 'parts of systems'. Specifically, Heisenberg and Jordan considered two weakly coupled atoms with energy differences in common (all or one, respectively). Because of the coupling, the energy matrices of the two atoms are no longer diagonal in the energy basis of the composite system, and thus are time-dependent, exhibiting a slow 'exchange of energy'. The main aim of the papers is to demonstrate how this result is in fact equivalent to the picture of matching quantum jumps in the two atoms. Born had calculated the transition probabilities for the quantum jumps in terms of the coefficients of the final wavefunction, and Heisenberg and Jordan now calculated them in terms of the elements of the transformation matrix between the two energy bases. This arguably provided a rigorous calculation of the transition probabilities by purely matrix mechanical means. In the same letter of 19 October Pauli also suggested that the statistical interpretation be applied to other quantities, specifically that the modulus squared of both $\psi(\mathbf{x})$ and

involving the positions of the various atoms *and the interference of their individual emissions*' (Dirac 1926, p. 677, emphasis added). Further evidence for an early understanding of interference as a many-particle effect can be taken from comments by Einstein at the 1927 Solvay conference, where he distinguishes between two conceptions of the wavefunction (both of which he finds problematic): one as pertaining to single electrons and one in which '[t]he de Broglie–Schrödinger waves do not correspond to a single electron, but to a cloud of electrons extended in space' (Bacciagaluppi and Valentini 2009, p. 441).

[18] In Born's own words: 'the fundamental ideas of the matrix form of the theory initiated by Heisenberg [...] have grown directly out of the natural description of atomic processes in terms of "quantum jumps" and emphasise the classic-geometrically incomprehensible nature of these phenomena. It is settled that both forms of the theory arrive at the same results for stationary states; the question is only how one should treat non-stationary processes. Here, Schrödinger's formalism turns out to be substantially more convenient, provided one interprets it in Heisenberg's sense. I therefore wish to advocate a merging of both points of view, in which each fulfils a very particular role' (Born 1926c, p. 168).

its Fourier transform $\varphi(\mathbf{p})$ should be interpreted as probability densities for position and momentum.[19]

The next step was the introduction of transition probabilities also between quantities other than energy. The formal tools for this were developed, again independently of each other, mainly by Dirac (1927) and by Jordan (1927b,c) with the so-called transformation theory. In Dirac's treatment, one considers arbitrary conjugate quantities ξ and η and some arbitrary quantity $g(\xi, \eta)$. If one assumes an ensemble in which ξ has a definite value and η is uniformly distributed, then Dirac's results yield the frequency of the values of g in the ensemble. The main tool that Dirac developed was the theory of the transformation matrices between the energy representation of g and its ξ-representation. Heisenberg's paper on fluctuation phenomena (Heisenberg 1927a) was explicitly cited as a special example, as was Born's work on collisions. Jordan (1927b, pp. 810–811) credits Pauli with the suggestion that there should be well-defined transition amplitudes between any two physical quantities. His theory is developed along these lines, including an axiomatisation of quantum mechanics using probability amplitudes as the basic notion.[20]

Dirac's paper was actually called 'The physical interpretation of the quantum dynamics', and while this was not quite spelled out, Dirac (1927, p. 641) concluded with the suggestion of

> [...] a point of view for regarding quantum phenomena rather different from the usual ones. One can suppose that the initial state of a system deter-

[19]Cf. Pauli to Heisenberg, 19 October 1926 (Pauli 1979, pp. 340–349). Heisenberg's paper was received shortly afterwards on 6 November 1926. For further details, see Bacciagaluppi and Valentini (2009, Section 3.4.4). Pauli's suggestion of $|\psi(\mathbf{x})|^2$ as position density also appeared in a footnote in a paper on gas theory and paramagnetism (Pauli 1927, p. 83), but of the further correspondence between Heisenberg and Pauli in this period only Heisenberg's letters have survived. Note that Heisenberg (1927a) talks about the two atoms exchanging a 'sound quantum', which suggests a connection with Jordan's treatment of the vibrating string in Born, Heisenberg and Jordan (1926) and the related discussion of the analogy between the radiation field and excitations of a lattice (see Bacciagaluppi, Crull and Maroney 2017).

[20]For Dirac's transformation theory, see Darrigol (1992, pp. 337–345). For a detailed treatment of Jordan, including the involvement of Pauli, see Duncan and Janssen (2009). London (1926), too, made contributions to transformation theory, as also acknowledged by Jordan. On these see Lacki (2004) and again Duncan and Janssen (2009).

> mines definitely the state of the system at any subsequent time. If, however, one describes the state of the system at an arbitrary time by giving numerical values to the co-ordinates and momenta, then one cannot actually set up a one-one correspondence between the values of these co-ordinates and momenta initially and their values at a subsequent time. All the same one can obtain a good deal of information (of the nature of averages) about the values at the subsequent time considered as functions of the initial values. The notion of probabilities does not enter into the ultimate description of mechanical processes: only when one is given some information that involves a probability (e.g., that all points in η-space are equally probable for representing the system) can one deduce results that involve probabilities.

Born was delighted with Jordan's work, writing to Wentzel on 13 December 1926:[21]

> Mr Jordan has now carried out a huge generalisation of the idea that the amplitude of the Schrödinger function is related to the state probabilities. Pauli had considered such a generalisation, but Jordan had the idea of the mathematical trick it requires. It now seems to turn out that using this conception quantum mechanics can manage with the usual space and usual time, but that a general probabilistic kinematics takes the place of the causal laws.

Similarly, Heisenberg was hugely impressed with Dirac's results, but still wondered about a satisfactory interpretation.[22] And this is when Heisenberg introduced uncertainty.

The uncertainty paper (Heisenberg 1927b) is often quite correctly read in the context of the debate with Schrödinger about *Anschaulichkeit* (about the applicability of spatiotemporal pictures in quantum mechanics). But it is equally relevant to the development of the statistical interpretation. The idea that physical quantities are well-defined under the appropriate measurement conditions goes along with the idea that quantum probabilities refer to finding certain values *upon measurement*. In this context, Heisenberg disagrees with Jordan about the need to revise probabilistic notions to accommodate interference: no such revision is required because the different probabilities (in Born's notation above, $|\sum_m c_m b_{mn}|^2$ and $\sum_m |c_m|^2 |b_{mn}|^2$) refer to different measurements (Heisenberg 1927b, pp. 183–184). And what the statistical

[21] AHQP-66, Section 3-044, as mentioned by Wessels (1980, p. 197).
[22] See Heisenberg to Pauli, 23 November 1926 (Pauli 1979, pp. 357–360).

interpretation allows us to do is 'conclude through certain statistical rules from one experiment to the possible results of another experiment' (p. 184). Now probabilities pertain to arbitrary quantum mechanical quantities and stationary states are no longer privileged (pp. 190–191). Finally – adding a twist to Dirac's suggestion about how probabilities enter quantum mechanics – Heisenberg takes uncertainty to be fundamental: the law of causality becomes inapplicable because it is impossible in principle to know the present in all its determining data (p. 197). According to Beller this is 'the beginning of a real commitment to indeterministic philosophy, as opposed to physicists' earlier tentative employment of statistical considerations' (Beller 1990, p. 583).[23]

These various strands were then brought together at the fifth Solvay conference of October 1927 in a joint report by Born and Heisenberg, where they set out what for them was the definitive view of the statistical interpretation.[24] Born and Heisenberg largely follow Born's discussion in the adiabatic paper, but give a significantly different treatment of the 'theorem of the interference of probabilities', emphasising the use of 'usual probabilities' along the lines of Heisenberg's uncertainty paper.[25] Much like Heisenberg's take on the law of causality in the uncertainty paper, the law of total probability

[23]Heisenberg's fuller argument for excluding a deeper causal picture is briefly hinted at on pp. 188–189, and was a version of the argument later made famous by Feynman using the two-slit experiment. In that familiar version, interference shows that a particle cannot have a trajectory through one slit, because it would have to know whether the other slit is open or closed. More generally for Heisenberg any 'hidden variables' would suppress interference effects. For details see e.g. Bacciagaluppi and Crull (2009, esp. Section 3.2) and Bacciagaluppi and Crull (2021, Chapters 4 and 14). On the two-slit experiment, see also the remarks by Weizsäcker in his letter to Grete Hermann of 17 December 1933 (Herrmann 2019, pp. 439–444).

[24]The report and ensuing discussion are translated in Bacciagaluppi and Valentini (2009, pp. 372–401 and 402–405). For commentary, see Bacciagaluppi and Valentini (2009, Sections 3.4.6 and 6.1.2).

[25]I now see this reading of what Born and Heisenberg mean with 'usual probabilities' as more natural than the one I suggested in Bacciagaluppi (2008, p. 274). The emphasis on usual probabilities is also an implicit criticism of Jordan's choice of probability amplitudes as the basic notion for his axiomatisation of quantum mechanics. Explicitly, on p. 392 of their report Born and Heisenberg note that amplitudes are not directly observable, and point out that von Neumann's recent work (Hilbert, von Neumann and Nordheim 1928, von Neumann 1927a) suffers from no such drawbacks (nor from problems with Dirac's δ-functions).

is not refuted but its antecedent is not satisfied.[26]

On this reading interference applies to individual systems just as well as to ensembles. Furthermore, compared to Born's case of collisions, an electron is no longer assumed to always be in a stationary state, but only when its energy has in fact been measured. Indeed, the analysis is generalised to any observable that has been measured. In this sense, what Born and Heisenberg present as the statistical interpretation is significantly closer to what we now know as standard quantum mechanics.[27]

But close is no cigar. The physical picture in Born and Heisenberg's report seems to be that what is 'out there' are on the one hand values of measured quantities, on the other transition probabilities; several aspects in the report suggest that they still do not take ψ as the 'state' of the system. Indeed, in Born and Heisenberg's eyes, the work by Heisenberg, Jordan and Dirac shows that matrix mechanics can stand on its own feet. Schrödinger waves may be a useful tool for calculating transition probabilities, but the latter can be defined directly in terms of transformation matrices. Note that, even though the interpretational section of the report was drafted by Born, these aspects suggest Heisenberg's hand.[28]

While the report also includes the idea of uncertainty as grounding the statistical aspects of the theory, it is remarkably silent on the 'reduction of the wave packet', which had also been introduced in the uncertainty paper by Heisen-

[26]Again, if quantities had hidden values, they would perform the transitions and interference would be destroyed. Such ideas also seem related to Heisenberg's work on the S-matrix of the 1940s, to his ideas on potentialities of the 1950s and to other scattered remarks even later.

[27]Quirkily enough, some kind of intermediate position resurfaces in Born's Waynflete lectures of 1948. There Born states that systems are indeed in stationary states and perform quantum jumps between them, but *which* systems this applies to depends on the subjective choice of how we distinguish between a system and its environment (Born 1949, pp. 99–101).

[28]Dirac's position was very different. As emphasised by Darrigol (1992, p. 344), Dirac (1927) does not yet contain any expression for quantum states, but we have seen that in his conclusion Dirac does refer to the idea of a deterministically evolving 'state'. Possibly, by describing his point of view as 'rather different from the usual ones', he wishes to distance himself from both Heisenberg and Schrödinger. See also Dirac's unusually extensive remarks on the interpretation of the theory at the fifth Solvay conference (Bacciagaluppi and Valentini 2009, pp. 446–448).

berg (1927b, p. 186). Already in that paper Heisenberg (1927b, pp. 183–184) had suggested that measuring the energy in an atomic beam passing through successive Stern–Gerlach magnets would randomise the phases in a superposition of energy states. This seems to suggest an unorthodox form of the collapse postulate, in which superpositions are maintained but with randomised phases. Such a form of the collapse is discussed explicitly in Heisenberg's Chicago lectures (Heisenberg 1930, p. 60), and would seem to be inconsistent with the repeatability of measurements; but it is inconsistent with the repeatability of measurements *only if* we assume that ψ is the 'state' of a quantum system and determines probabilities for measurement results. It is unobjectionable if we assume that what determines the probabilities of the results are actual values and transition probabilities, and that ψ is just a bookkeeping device that can be changed if and when convenient.[29]

Additional support for this reading may perhaps be found in Born's remarks on the reduction of the wave packet in the General Discussion at the Solvay conference (Bacciagaluppi and Valentini 2009, pp. 437–439), where, prompted by Einstein, Born gave a simplified treatment of α-particle tracks in a cloud chamber. Born cites remarks by Pauli to the effect that reduction can be dispensed with if one includes a description also of the apparatus (i.e. the cloud chamber), but does not go into details, except perhaps with his remark towards the end to the effect that

> [t]o the reduction of the wave packet corresponds the choice of one of the two directions of propagation $+x_0, -x_0$, which one must take as soon as it is established that one of the two points 1 and 2 is hit, that is to say, that the trajectory of the packet has received a kink.

If this is read as an alternative to the description in terms of reduction, Born is saying that *instead* of collapsing the wavefunction, one can keep the original wavefunction but take a definite value for the direction of propagation of the α-particle. In his own remarks Heisenberg also objected to Dirac's suggestion that collapse was a natural process (Bacciagaluppi and Valentini 2009, pp. 449–450).

[29]For further details see Bacciagaluppi and Valentini (2009, Sections 3.2, 6.2, 6.3 and 11.3), and Bacciagaluppi (2008, Section 3).

4 Phase III: von Neumann

In the very same General Discussion at the Solvay conference Born announced a new paper by von Neumann (Bacciagaluppi and Valentini 2009, p. 448):[30]

> I should like to point out, with regard to the considerations of Mr Dirac, that they seem closely related to the ideas expressed in a paper by my collaborator J. von Neumann, which will appear shortly. The author of this paper shows that quantum mechanics can be built up using the ordinary probability calculus, starting from a small number of formal hypotheses; the probability amplitudes and the law of their composition do not really play a role there.

This paper is clearly 'Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik' (von Neumann 1927b), which was presented by Born himself in the session of 11 November 1927 of the Göttingen Academy of Sciences. It had been preceded by two other papers, one by Hilbert, von Neumann and Nordheim (1928), which still built on Jordan's approach, and one by von Neumann alone, in which he developed the Hilbert-space formalism of quantum mechanics (von Neumann 1927a).[31] Then in the November paper von Neumann went on to treat explicitly the statistical aspects of quantum mechanics. The paper is of fundamental importance in the development of the statistical interpretation and less well-known than it ought to be, so it is useful to summarise it in some detail. But the main points can be stated briefly as follows.

Von Neumann is concerned with 'statistical theories', i.e. theories about ensembles of systems. In such theories, measurements of physical quantities generally lead to non-trivial distributions of values for any given ensemble, and a theory makes predictions about values and their distributions. What characterises a *non*-statistical theory are the algebraic relations between its physical quantities; and the crucial difference between classical mechanics and quantum mechanics as non-statistical theories is that the former has a commutative algebra of observables and the latter a non-commutative one. Von Neumann shows that the statistical aspects of quantum mechanics need

[30] The final remark again appears to favourably contrast von Neumann's approach to Jordan's.

[31] Cf. also Lacki (2000), Duncan and Janssen (2012) and Mitsch (forthcoming).

not be postulated *ad hoc*: quantum mechanics as a statistical theory can be completely and rigorously derived purely by imposing 'usual' probabilistic axioms on expectation values for ensembles of systems whose physical quantities are characterised by the non-commutative algebra of matrix mechanics. These probabilistic axioms are positivity and linearity (on the latter we shall have much more to say). The resulting theory indeed completely determines the possible values and value distributions for the physical quantities. Its object are ensembles, characterised by (pure or mixed) states, which can be specified by results of measurements. Thus von Neumann's version of the statistical interpretation now includes both the notion of a quantum state, and that of the collapse of the state upon measurement. This is essentially modern quantum mechanics. Furthermore, while in classical mechanics as a statistical theory the pure states (those that can no longer be decomposed into other states) are such that all physical quantities have 'sharp' distributions (characterised by a single value), for each pure state in quantum mechanics there are quantities that do not have a sharp distribution. Thus the incompatibility of physical quantities leads inexorably to the indeterministic character of quantum mechanics. Readers interested specifically in the relation between statistical interpretation and indeterminism may skip directly to the final section of this chapter. The rest of this section spells out the details of von Neumann's paper (followed by a few brief comments).

In Section I, von Neumann distinguishes between the 'wave theory' of quantum mechanics and the 'statistical theory' initiated by Born, Pauli and London, by which he means the transformation theory of Jordan and Dirac. Von Neumann's topic is the statistical theory, which according to him typically answers the following questions:

(i) what values can a physical quantity have?

(ii) what are the *a priori* probabilities for these values?

(iii) what are the probabilities given results of other measurements?

One can consider a statistical theory also in a classical context, in which however, non-trivial probabilities can be in principle eliminated by performing sufficiently many measurements. This is not so in quantum mechanics, where pairs of measurements are generally mutually incompatible, as emphasised by Dirac (1926) and by Heisenberg (1927b). On the other hand, according to von Neumann, the usual presentation of the statistical quantum mechanics 'rather dogmatically' *postulates* certain quantities to be probabilities (in wave mechanical language the weights in decompositions of the wavefunction), and one then checks the empirical correctness of the predictions. Furthermore, the relation between the theory as usually presented and the theory of probability remains at best unclear. So for instance Jordan (1927b) proposes a theory based on probability amplitudes, but Heisenberg (1927b) dissents. Von Neumann's aim in this paper is to provide a systematic *derivation* of statistical quantum mechanics from the usual theory of probability with a few supplementary assumptions.

Section II describes the basic assumptions of the derivation. Von Neumann considers a physical system $\mathfrak{S}$ and quantities $\mathfrak{a}, \mathfrak{b}, \ldots$ on the system. A function $f(\mathfrak{a})$ of a quantity $\mathfrak{a}$ is always well-defined, namely as the quantity that for each value x of $\mathfrak{a}$ takes the value $f(x)$. A statistical theory concerns *ensembles* of such systems, which are interpreted epistemically (or at least, von Neumann says they represent 'knowledge' about a system – with inverted commas in the original). Measurements of physical quantities lead to a distribution of results for each given ensemble. There should be a maximally disordered ensemble in which all 'states' of the system are equally probable (no 'knowledge' about the system – yielding *a priori* distributions for measurement results) and ensembles arising from it through measurements (yielding distributions conditional on certain previous measurement results). Each ensemble is in fact characterised by the expectation values for all quantities. Von Neumann wishes to describe quantum mechanics as such a statistical theory, keeping as close as possible to the ordinary notions of probability and expectation value.[32]

One needs to make a crucial distinction between quantities that can or cannot be measured simultaneously (von Neumann remarks that if two quantities are compatible they are both functions of a third quantity[33]). A function $f(\mathfrak{a}, \mathfrak{b})$ of two compatible quantities is always well-defined, while in general

[32]Two further assumptions – not needed to characterise ensembles and states – are introduced later and make precise what von Neumann understands by 'measurement': repeatabilty in Section VI and non-contextuality in Section VII.

[33]Strangely, he also states that this is a feature specific to quantum mechanics (cf. his footnote 8 on p. 248).

it is meaningless to talk of functions of incompatible quantities. On the other hand, it is always possible to define the *sum* of two incompatible quantities if one assumes expectation values to be additive: $\mathfrak{a}+\mathfrak{b}$ is the quantity with

$$\mathsf{E}(\mathfrak{a}+\mathfrak{b}) = \mathsf{E}(\mathfrak{a}) + \mathsf{E}(\mathfrak{b}) \tag{5}$$

for all ensembles.[34] The definition can be trivially extended to arbitrary linear combinations of two quantities or even countably many quantities when the corresponding series converges. Therefore, even in a case like that of quantum mechanics, expectation functionals $\mathsf{E}(\mathfrak{a})$ can be required to satisfy:

A. For any convergent real linear combination $\alpha\mathfrak{a}+\beta\mathfrak{b}+\gamma\mathfrak{c}+\ldots$,

$$\mathsf{E}(\alpha\mathfrak{a}+\beta\mathfrak{b}+\gamma\mathfrak{c}+\ldots) = \alpha\mathsf{E}(\mathfrak{a}) + \beta\mathsf{E}(\mathfrak{b}) + \gamma\mathsf{E}(\mathfrak{c}) + \ldots$$

Furthermore, it is clear that if a quantity $\mathfrak{a}$ takes only positive values, its expectation value in any ensemble will also be positive:

B. For $\mathfrak{a} \geq 0$, $\mathsf{E}(\mathfrak{a}) \geq 0$.

These are both features of expectation values in ordinary probability theory: linearity (extended by continuity to countable linear combinations) and positivity. However, von Neumann explicitly leaves out normalisation (remarking that it is more important to have well-defined 'relative probabilities').[35]

After these 'conceptual' assumptions, von Neumann then introduces a 'formal' assumption: namely that physical quantities in quantum mechanics correspond bijectively to diagonalisable ('normal') linear operators on a separable Hilbert space.[36] For von Neumann's purposes the relevant aspects of this correspondence are the following:

[34]Note that von Neumann is thereby also taking physical quantities to be characterised by their expectation values in all ensembles.

[35]Note that von Neumann was writing before the now standard axiomatisation of probability theory by Kolmogorov.

[36]The spectral theorem for self-adjoint operators was proved in full only in von Neumann (1929), but von Neumann (1927a, Sections IX and X) already contained partial results.

C. Let $S, T, \ldots, \alpha S + \beta T + \ldots$ be diagonalisable. If $S, T, \ldots$ correspond to $\mathfrak{a}, \mathfrak{b} \ldots$ then $\alpha S + \beta T + \ldots$ corresponds to $\alpha\mathfrak{a} + \beta\mathfrak{b} + \ldots$

D. Let S be diagonalisable, and $f(x)$ be a real function. If S corresponds to $\mathfrak{a}$, then $f(S)$ corresponds to $f(\mathfrak{a})$.[37]

That is, the correspondence is such that the functional relations between the physical quantities are represented by the functional relations between the operators. In particular, the *sum* of two operators corresponds to the sum defined via (5). This, von Neumann notes, conforms to actual practice, but he is fully aware that it is a very radical feature of quantum mechanics. In footnote 9 on p. 249 he takes the example of the mutually incompatible quantities $\mathbf{Q}^2$, $\mathbf{P}^2$ and $\mathbf{Q}^2 + \mathbf{P}^2$: even though the first two can take continuous values and $\mathbf{Q}^2 + \mathbf{P}^2$ takes only discrete values, the quantum-mechanical expectation values *are* in fact linear.

Note that **C.** and **D.** are part and parcel of matrix mechanics (in Hilbert-space formulation) seen as a *non-statistical* theory. They simply express the algebraic structure of the physical quantities of the theory. **A.** and **B.** instead are assumptions of the 'usual probability theory'. On the basis of these assumptions, von Neumann will answer questions (i), (ii) and (iii): what values physical quantities take in quantum mechanics, what the maximally disordered state is, and which other states arise from it through which measurements – in particular recovering the usual probabilistic interpretation of the wavefunction. The rest of the paper carries this out.

Sections III and IV are headed 'General form of the expectation values. States'. (They are the ones containing his characterisation of quantum mechanical ensembles and what later became known as von Neumann's 'impossibility theorem' for hidden variables.) In Section III von Neumann shows from **A.**–**D.** that expectation functionals $\mathsf{E}(S)$ on the diagonalisable operators S are of the form

$$\mathrm{Tr}(SU) \tag{6}$$

for some positive linear symmetric operator U, and that any such operator defines an expectation functional – a central result that is remarkably easy

[37]Such a function is to be understood along the lines of von Neumann (1927a, Section XIV).

to prove. In Section IV he then defines pure ensembles as ones for which the operator U has no non-trivial decomposition $\eta U^* + \vartheta U^{**}$ (with $\eta, \vartheta > 0$), and shows that (up to an overall positive factor) they are given by the one-dimensional projection operators P_φ with φ a normalised vector in the Hilbert space. Any two φ related by a phase factor define the same pure ensemble; the corresponding expectation values are normalised; the operator S corresponding to a quantity $\mathfrak{a}$ is simply the operator that yields $\mathsf{E}(\mathfrak{a})$ for all pure ensembles via $(\varphi, S\varphi)$ (in von Neumann's notation: $Q(\varphi, S\varphi)$); and $\mathsf{E}(\mathfrak{a})$ is dispersion-free if and only if φ is an eigenvector of S – in which case the value of S is the corresponding eigenvalue. It follows that (unlike in classical mechanics) for each pure ensemble there are physical quantities with non-zero dispersion. Note that von Neumann equates 'pure' ensembles with 'homogeneous' ones ('i.e. those in which all systems [...] are in the same state', p. 255). In this sense, while ensembles in general are arguably interpreted epistemically, pure ensembles are ones in which all systems are in the same ontic state ('Thereby we shall have found all states in which the system $\mathfrak{S}$ can be', p. 255). Von Neumann has thus recovered the *statistical interpretation of the wavefunction*, as well as introduced the notion of a *quantum state* and the *eigenstate–eigenvalue link* that defines which values observables have in which states and thus answering question (i) above.

Sections V and VI are titled 'Measurements and states'. Although von Neumann has abstractly characterised ensembles and states in Section IV, operationally they are defined in terms of results of measurements, so he needs to establish how they are related to measurements. In Section V he shows that quantities $\mathfrak{a}_1, \ldots, \mathfrak{a}_m$ are jointly measurable if and only if they correspond to operators $S_1, \ldots, S_m$ all elements of whose resolutions of the identity commute.[38] If the measurements only fix the value of some quantity $\mathfrak{a}_j$ to a certain interval I_j, then only the corresponding projection operator $E_j(I_j)$ in the resolution of the identity of S_j needs to commute with the elements of the resolutions of the identity of the other operators. Von Neumann also shows that the product of two compatible quantities $\mathfrak{a}_1, \mathfrak{a}_2$ corresponds to the product $S_1 S_2$. Then in Section VI, he considers a measurement that fixes the values of $\mathfrak{a}_1, \ldots, \mathfrak{a}_m$ to within the intervals $I_1, \ldots, I_m$ from the (yet to be determined) 'maximally disordered' ensemble. Assuming that measurements are *repeatable*, he shows that the positive operator U characterising the re-

[38] Resolutions of the identity had been introduced in von Neumann (1927a, Section IX).

sulting ensemble commutes with the projector $E := E_1(I_1)E_2(I_2)\dots E_m(I_m)$ and in fact $U = EU = UE$. In the special case in which E is one-dimensional, U is thus the corresponding projection operator P_φ. In modern terminology, von Neumann has derived that in a maximal measurement the state *collapses* to an eigenstate of the measured observable.

Finally, Sections VII and VIII are titled 'Statistical relations between different measurements'. Von Neumann returns to the question of what ensembles arise from the maximally disordered ensemble through measurements that fix values of physical quantities only to within given intervals. He now makes a *non-contextuality* assumption: that the ensemble arising from such a measurement is independent of which other measurements are carried out compatibly with it. In other words (and in modern terminology), that such a measurement of a non-maximal observable can be implemented by measuring an arbitrary maximal observable compatible with it and then coarse-graining over the finer results.[39] Under this assumption, von Neumann shows that U is a mixture of projections P_{φ_p} for which $\{\varphi_p\}$ spans the range of E and that the corresponding weights are all equal, so that (up to a proportionality factor) $U = E$.[40] In Section VIII he then uses this result to answer questions (iii) and (ii) above. Starting from an initial maximally disordered state, for two successive measurements that fix the values of two sets of quantities $\mathfrak{a}_1, \dots, \mathfrak{a}_m$ and $\mathfrak{b}_1, \dots, \mathfrak{b}_n$ to certain intervals (defining two not necessarily commuting projection operators E, F), he expresses the (unnormalised) *transition probability* between the corresponding results as $\mathrm{Tr}(EF)$ – an expression he had proposed and discussed in von Neumann (1927a, Sections XII–XIV) as generalising the transition probabilities of the usual transformation theory. He also determines the 'maximally disordered ensemble' as the identity operator

[39]From von Neumann's book (1932, Chapter V.1) it is clear that this is in fact the only way he conceptualises the measurement of a non-maximal observable. This was later criticised by Lüders (1950), who introduced the now standard form of the collapse postulate for the case of non-maximal observables.

[40]It may be surprising that while in Section III von Neumann characterises ensembles as arbitrary unnormalised density matrices, here in Section VII he is claiming that ensembles arising through non-maximal measurements are always (multi-dimensional) projection operators. But note that he is attempting to characterise only ensembles that arise from an initial state that is 'maximally disordered'. Von Neumann does not have the general notion of a measurement as a completely positive map, but he can certainly conceive of preparing more general ensembles by *mixing* different ensembles of the form $U = E$. (He explicitly mentions mixing in Section VIII.)

$U = \mathbf{1}$, noting that it indeed assigns the same (relative) probability 1 to all pure states (even though a notion of 'volume' on the unit sphere of a separable Hilbert space may be ill-defined[41]), and that it can be prepared also by mixing in equal proportions the elements of an arbitrary complete orthonormal system. Ensembles of the form $U = E$ can be analogously prepared by mixing in equal proportions the elements of an arbitrary orthonormal system spanning the range of E, showing that decompositions of mixed states are non-unique. (For von Neumann this shows that the only homogeneous ensembles, i.e. the only true 'states', are pure.) Finally, if the range of E is finite-dimensional, then the statistical distributions defined by $U = E$ can be normalised by dividing by the dimension, while if it is infinite-dimensional one obtains indeed only relative distributions.

Section IX is a 'Summary'. Von Neumann states that he has uniquely derived the usual statistical quantum mechanics from the 'usual probability theory', from the fundamental assumptions that measurements in general disturb a system, that certain pairs of measurements are incompatible, and that nevertheless measurements are repeatable, and from the formal assumption that in quantum mechanics physical quantities and their functional relations are represented by operators and their functional relations. He remarks that, since states always evolve deterministically according to the Schrödinger equation, the statistical and 'acausal' character of quantum mechanics arises specifically through the in-principle limitations of measurements pointed out by Heisenberg (1927b).

Von Neumann's statistical quantum mechanics is now arguably the theory as we know it, and not just because he uses a Hilbert-space formulation (albeit only infinite-dimensional). Unlike the theory as presented by Born and Heisenberg at the 1927 Solvay conference – which included recognisable transition probabilities but no recognisable notions of states or collapse – von Neumann's statistical theory includes a characterisation of both pure and mixed states, respectively as wavefunctions and (unnormalised) density operators; it includes the general form of the Born rule for expectation values $\mathrm{Tr}(US)$ and a generalised form of transition probabilities $\mathrm{Tr}(EF)$; and it includes a recognisable version of the collapse postulate for the general case of maximal measurements and in a special case of non-maximal measurements.

[41] Cf. the note added in proof on p. 256, where von Neumann compares his approach advantageously to that of Weyl (1927).

The physical picture behind von Neumann's formulation of the theory is also fairly clear. He appears to believe that a physical system is always in some pure state. These states are represented by Schrödinger's wavefunctions, but can be equivalently characterised in terms of the values of a maximal set of commuting observables. Measurements are interventions on a system that in general irreducibly disturb it, forcing the system into one of the eigenstates of the measured observable (cf. von Neumann's footnote 33 on p. 264). Ensembles, represented by generally unnormalised density operators, are understood epistemically. *A priori* probabilities corresponding to no knowledge are represented by the identity operator. Homogeneous ensembles are identified with pure states. Unlike the case of classical statistical mechanics, these are not dispersion-free.

One could comment extensively on all of these points. Because of entanglement, quantum systems are not always in pure states.[42] Measurements are only given a phenomenological description.[43] The requirement of a well-defined notion of *a priori* probabilities resurfaces in von Neumann's later work on rings of operators.[44] One should not identify homogeneous ensembles and pure states.[45] But we want to conclude by focusing on the last point, the lack of dispersion-free states, and specifically on whether and how von Neumann can be understood as having shown that 'the statistical interpretation [...] is the only interpretation of quantum mechanics consistently implementable today'.

[42]The EPR state is an example where the subsystems are described by the maximally mixed state, but – precisely if quantum mechanics is complete – not by any specific decomposition into either position or momentum eigenstates.

[43]Not only is there no 'measurement problem' yet, but also no attempt to model the process of measurement. Von Neumann was going to discuss the latter in Chapter VI of his book, but it is arguably Pauli who did the most interesting early work on modelling the process of measurement in Section A.9 of his 1933 handbook article (Pauli 1933). On Pauli's interest in the measurement process, cf. above Born's remarks on the cloud chamber, Pauli's letter to Bohr of 17 October 1927 (Pauli 1979, pp. 411–413) and the discussion in Bacciagaluppi and Crull (2021, Section 5.5).

[44]As discussed by Rédei (1996), it was a motivation for von Neumann in the mid-1930s to think of type-II factors as a possible replacement for Hilbert space in the formulation of quantum mechanics.

[45]If we define a homogeneous ensemble as one in which systems are all in the same state, then subsystems of an EPR pair are indeed always in the same state, even though they are described by the identity matrix which is non-uniquely decomposable into pure states.

5 Statistical interpretation and indeterminism

The statistical interpretation of quantum mechanics was connected from the start to the issue of indeterminism. We saw how Born had opted for a middle course between the discontinuity of matrix mechanics and the continuity of wave mechanics, with the latter determining the probabilities for the former. As discussed in detail by Beller (1990), we also saw how Born nevertheless was only tentatively taking the quantum probabilities as an expression of indeterminism, and that it was maybe Heisenberg in the uncertainty paper who first claimed that the statistical interpretation was an expression of irreducible indeterminism. The explicit references to Heisenberg in von Neumann's paper indicate that von Neumann understood his 'probabilistic construction' of quantum mechanics as endorsing Heisenberg on this point. Specifically, I would like to suggest that von Neumann saw himself as providing a rigorous proof of Heisenberg's intuition that irreducible probabilities follow from the *incompatibility* of quantum mechanical quantities.[46] Indeed, von Neumann's distinction between classical and quantum mechanics is precisely that at the non-statistical level the former is commutative and the latter is not, and that at the statistical level the probabilities in classical mechanics can always be eliminated (which makes clear they are merely epistemic) but that this is impossible in quantum mechanics.

The emergence of this crucial distinction can be seen implicitly also in the previous development of the statistical interpretation. In Born's 1926 papers, the statistical distributions are over the stationary states of essentially non-interacting systems, thus over the values of a set of *commuting* quantities. In Heisenberg and Jordan's fluctuation papers, we first explicitly see probabilities arising through consideration of *non*-commuting matrices, namely

[46] Wigner (1970, note 1) states that von Neumann's main argument against hidden variables was based on the impossibility of preparing dispersion-free states when considering successive measurements of incompatible observables. This argument is in fact presented in von Neumann's book, but von Neumann nevertheless raises the question of whether dispersion-free states exist at all (von Neumann 1932, Section IV.1). The argument and Wigner's remarks are briefly discussed by Jammer (1974, pp. 266–267). Thanks to Chris Mitsch for correspondence on this point, and see Mitsch (forthcoming) for further discussion.

the Hamiltonians with and without an interaction term. This is generalised in the transformation theory to arbitrary pairs of non-commuting quantities, and taken in the uncertainty paper as the very reason why probabilities are needed. Finally, von Neumann proves that the non-commutative algebra of matrix mechanics leads to pure states that always involve non-trivial probabilities.

Should any of this rule out the very possibility of fundamental determinism? Max Born appears to have been the first to explicitly draw stronger conclusions from von Neumann's result. In a little-known paper of 1929 he wrote:[47]

> Although the new theory seems thus well established in experience, one can still pose the question of whether *in the future*, through extension or refinement, it might not be made *deterministic again*. In this regard one must note: it can be shown in a mathematically exact way that the established formalism of quantum mechanics allows for *no* such completion. If thus one wants to retain the hope that determinism will return someday, then one must consider the present theory to be *contentually false*; specific statements of this theory would have to be refuted experimentally. Therefore, in order to convert the adherents of the statistical theory, the determinist should not protest but rather test.

But it is von Neumann himself in his 1932 book who discusses the issue most explicitly. In the 'Introduction' section he announces his purpose with these words (von Neumann 1932, pp. 2–3):

> In analysing the fundamental questions it will be shown in particular how the statistical formulas of quantum mechanics can be derived from some qualitative fundamental assumptions. Further, the question is discussed in detail of whether it is possible to trace the statistical character of quantum mechanics back to an ambiguity (i.e. incompleteness) in our description of nature: this explanation would best fit the general principle according to which all probabilistic statements arise from the incompleteness of our knowledge. This explanation 'by hidden parameters', as well as another related one that ascribes the 'hidden parameters' to the observer and not to the observed system, has in fact been proposed several times. However, it turns out that this cannot be achieved in a satisfactory way, or more precisely: such an explanation is incompatible with certain qualitative fundamental postulates of quantum mechanics.

[47] Born (1929, pp. 117–118), as translated in Crull and Bacciagaluppi (2017, p. 223).

Von Neumann then sets up the question in Section III.2 ('The statistical interpretation'), discussing hidden parameters relating to the system in Section IV.1 ('Fundamental basis of the statistical theory') and Section IV.2 ('Proof of the statistical formulas'), and hidden parameters relating to the apparatus at the beginning of Section VI.3 ('Discussion of the measurement process'). In the later literature, these two discussions have become known respectively as von Neumann's 'impossibility theorem' for hidden variables (or some such name) and his 'insolubility theorem' for the measurement problem of quantum mechanics. The latter is the proof that measurement statistics cannot be reproduced by assuming a mixed state for the apparatus and a unitary interaction between system and apparatus (cf. Brown 1986). For von Neumann, the two are parallel cases in the sense that they attempt to explain the quantum mechanical statistics by assuming respectively that the state of the system or the state of the apparatus are not homogeneous.[48]

Section IV.2 is the one that interests us most. After presenting his results of 1927, von Neumann returns to the question of whether the quantum statistics could be explained by assuming that the homogeneous states are in fact mixtures of dispersion-free states characterised by 'hidden parameters'. He states that this is impossible for two reasons that at first may seem to beg the question: homogeneous states would not be homogeneous after all, and dispersion-free states have just been shown not to exist. But then he adds (von Neumann 1932, p. 171):

> It should be noted that here we did not need to discuss the further details of the mechanism of the 'hidden parameters': the established results of quantum mechanics can in no way be rederived with their help, it is even impossible that the same physical quantities with the same functional relations exist (i.e. that [**D.**] and [**C.**] hold), if other variables ('hidden parameters') should exist in addition to the wavefunction.
>
> [...] the relations assumed by quantum mechanics [...] would have to fail already for [...] known quantities. Thus it is not at all a question of interpretation of quantum mechanics, as often assumed, rather the latter ought to be objectively false in order for a behaviour of elementary processes to be possible that is other than statistical.

[48]Mitsch (forthcoming) following Wigner (1970, note 1) suggests that Schrödinger was in fact the originator of some of the proposals von Neumann is criticising (during the years they spent together in Berlin). On measurements in quantum mechanics, see also Schrödinger to von Neumann, 25 December 1929 (Meyenn 2011, pp. 474–476).

Recall that the original purpose of von Neumann's 1927 paper was to provide a rigorous derivation of the statistical aspects of quantum mechanics from its algebraic structure together with general assumptions from the theory of probability. What von Neumann is saying here is that the existence of hidden variables would imply that in quantum mechanics we have *misidentified* the algebraic structure of the physical quantities. As Dieks (2017, Section 4) emphasises, von Neumann's fundamental assumption is that classical kinematics should be replaced by the new quantum kinematics. Now this assumption may turn out to be false, but for von Neumann all the evidence was in favour of it: the new kinematics had already been securely established through the matrix mechanics of Heisenberg (1925), Born and Jordan (1925b) and Born, Heisenberg and Jordan (1926), well before Born introduced the first elements of the 'statistical interpretation' in his papers of 1926.

Compare this with Bohm's (1952a,b) theory – which does what von Neumann was reputed to have shown to be impossible: provide a deterministic underpinning ('causal interpretation') of quantum mechanics. It does so precisely by rejecting the quantum kinematics. The true algebra of physical quantities of a system in Bohm's theory is the commutative algebra generated by the position operators, and for these quantities (and all 'hidden states') von Neumann's assumptions straightforwardly hold, including the assumption of linearity. Other operators simply do not correspond to single physical quantities. As Bohm himself remarked discussing von Neumann's theorem, 'the so-called "observables" are [...] not properties belonging to the observed system alone' (1952b, p. 187). In any measurement one indeed measures some physical quantity on the system – but (except for functions of position) that quantity will depend on the details of the experimental context. Different quantities are then mistakenly identified with each other because they happen to have the same statistics in all ensembles characterised by quantum states. According to Bub (2010), the significance of von Neumann's theorem for the hidden variables debate is that it establishes this kind of contextuality.[49]

[49] Bohm himself (1952b, pp. 187–188) inaccurately suggests that the experimental results depend on the hidden variables of both system and measuring device, but no apparatus hidden variables are needed. What is crucial is that the same hidden variables for the system lead to different results depending on a measurement context – quantum observables are 'contextual observables'. For analyses of measurement contextuality in the Bohm theory, see Pagonis and Clifton (1995) and Barrett (1999, Section 5.2). See also the analogy between the Bohm theory and the 'firefly box' in Bacciagaluppi (2016b, Section 6).

The significance of von Neumann's result for the hidden variables debate has been traditionally framed in the context of Bell's (1966) criticism. Bell pointed out that if one wishes to construct a theory in which hidden states determine without dispersion the values of quantum mechanical observables, then it is unreasonable to require of the hidden states that

$$\mathsf{E}(\alpha S + \beta T) = \alpha \mathsf{E}(S) + \beta \mathsf{E}(T) \tag{7}$$

(the conjunction of von Neumann's assumptions **A.** and **D.**): dispersion-free values of quantum observables must equal eigenvalues, and eigenvalues of non-commuting operators in general do not behave additively.[50]

In recent years the criticism of von Neumann has become somewhat more strident. In a later interview, Bell even characterised assumption (7) as 'silly' and 'foolish',[51] with a number of authors following suit (e.g. Mermin 1993). Recent work by Bub (2010), Dieks (2017) and Mitsch (forthcoming) instead variously counters this assessment by restoring von Neumann's work to its historical context.[52]

[50]It has also been appreciated recently that Grete Hermann had already criticised von Neumann's reputed argument against hidden variables thirty years earlier (see e.g. Seevinck 2017). Hermann's criticism, too, centres on the linearity assumption, but unlike Bell she is aware that von Neumann fully realises how non-trivial it is (recall von Neumann's own example of $\mathbf{Q}^2$, $\mathbf{P}^2$ and $\mathbf{Q}^2 + \mathbf{P}^2$). Instead she believes that von Neumann smuggles it in illicitly. But her overall assessment of the significance of von Neumann's result is precisely that the existence of hidden variables would imply that quantum mechanics has misidentified the algebraic structure of the physical quantities it describes. For details see Dieks (2017, Section 5). The criticism first appears in a manuscript from 1933 (Dirac Archives, DRAC 3/11) in which Hermann defends the possibility of deterministically completing quantum mechanics (and which contains the above passage from Born (1929) as an epigraph). Hermann's criticism was then published as Section 7 ('The circle in Neumann's proof') in Hermann (1935), an extended analysis of quantum mechanics in which instead Hermann provides an alternative neo-Kantian argument for the completeness of the theory closely related to Bohr's complementarity. Both Hermann's 1933 manuscript and her 1935 essay are translated in Crull and Bacciagaluppi (2017, Chapters 14 and 15) – with extensive discussions – and have been (re-)published in German in Herrmann (2019, pp. 185–203 and 205–258). See also the extremely interesting correspondence from 1968 between Hermann and Jammer (where Hermann retracts the specific claim of circularity) published in Herrmann (2019, pp. 599–610).

[51]*Omni* magazine (May 1988, p. 88), quoted in Mermin (1993).

[52]As mentioned, Bub (2010) argues that von Neumann's theorem shows that hidden variables theories cannot be constructed on the model of classical statistical mechanics but have to be contextual, and Dieks (2017) that von Neumann's crucial assumption is

Let us be clear. There are *no* mistakes in von Neumann's mathematics: taking self-adjoint operators and their functional relations as representing physical quantities and their functional relations, it follows that expectation values are represented by density operators, and that no such expectation values are dispersion-free for all physical quantities. And it is equally clear that the result does *not* rule out deterministic hidden variables theories: Bohm's theory is an unequivocal example of such a theory. It is also presumably the case that many physicists mistakenly did take von Neumann's theorem to rule out hidden variables theories (as many physicists nowadays mistakenly take Bell's theorem to rule them out!).[53] The question is whether von Neumann's theorem in fact establishes something interesting about hidden variables and what von Neumann himself thought it established.[54]

In terms of the distinction standard today between the 'completeness' and 'correctness' of quantum mechanics, if one theoretically completes quantum mechanics by assuming the existence of Bohmian corpuscles, quantum mechanics does nevertheless remain empirically correct. To insist that this would make quantum mechanics 'objectively wrong' may seem disingenuous. Knowledge of the Bohm theory, however, gives us the benefit of hindsight.

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indeed the new quantum kinematics. Mitsch (forthcoming) argues that the 'uniqueness' of the statistical quantum mechanics is to be seen in the context of Hilbert's axiomatic method, which aimed in particular to define a unique formal apparatus capturing a given physical content and was thus understood as both provisional and relative.

[53]Note that (with few exceptions, most notably Born), the other 'founding fathers' of quantum mechanics rarely referred to von Neumann's theorem when arguing for the completeness of quantum mechanics. This can seen for instance in their reactions to the EPR paper (see Bacciagaluppi and Crull 2021). Bohr's arguments were based on complementarity, Heisenberg's (as mentioned) were based on interference. Pauli did not refer explicitly to von Neumann but (*pace* Bell's later criticism) he did take the example of $\mathbf{Q}^2$, $\mathbf{P}^2$ and $\mathbf{Q}^2 + \mathbf{P}^2$ as showing the impossibility of hidden variables: if $\mathbf{Q}$ and $\mathbf{P}$ had continuously distributed dispersion-free values, so would $\mathbf{Q}^2 + \mathbf{P}^2$. As first discussed by Fine (1994), in his first paper on entanglement Schrödinger (1935) took up this example, and presented it as part of a dilemma: the EPR argument showed that all quantum mechanical observables had (non-contextual!) values, but these could not obey any obvious functional relations.

[54]Note that replying on Bohr's behalf to Hermann's 1933 manuscript, Weizsäcker in his letter to Hermann of 17 December 1933 agrees that determinism is still viable, but wishes to argue that the self-imposed restrictions of quantum theory are not arbitrary (Herrmann 2019, p. 440). Also Jammer (1974, pp. 272–275, esp. fn 45) in discussing Hermann's criticism remarks perceptively on what von Neumann's result does or does not show.

The historically accurate question may be too difficult ever to decide, but it is whether the likes of Born and von Neumann considered the possibility (however implausible) that a theory with a different algebraic structure might be able to deliver the same empirical predictions as quantum mechanics, or whether they tacitly assumed that any such theory would automatically contradict also the empirical predictions of quantum mechanics. If the former, they would have been mistaken in discounting that possibility, but perfectly rational in judging it implausible. If the latter, they would indeed have been drawing an unwarranted conclusion from von Neumann's theorem.[55]

In later years, von Neumann does not seem to have returned much to the issue of 'hidden parameters'. He discusses it in print in discussion comments on Bohr's talk at the 1938 Warsaw conference – a rare occasion where Bohr actually cites von Neumann's theorem as clarifying how 'the fundamental principle of superposition of quantum mechanics [*sic*] logically excludes the possibility of avoiding the non-causal character of the formalism by any kind of conceivable introduction of additional variables' (Bohr 1939, p. 17), before going on to a more detailed discussion of complementarity as 'providing a direct generalisation of the very ideal of causality' (p. 28). In his comments, von Neumann characterises a physical system in terms of its measurable physical quantities and their algebraic relations, he then considers ensem-

[55]Note that the situation is subtle even in the Bohm theory. If one considers ensembles with arbitrary position distributions, then the theory will indeed violate the empirical predictions of quantum mechanics (for instance, it will violate the no-signalling theorem). A theory with different algebraic structure does lead to different empirical predictions. But if one considers special 'equilibrium' ensembles in which positions are distributed according to the Born distribution $|\psi(\mathbf{x})|^2$, the empirical predictions of the theory become the same as those of standard quantum mechanics (in particular the self-adjoint operators emerge as contextual observables). Not only, the theoretical regime defined by these ensembles is stable in the sense that – whether they evolve freely or are subjected to any (quantum) measurements – these ensembles will always remain 'equilibrium' ensembles. De Broglie in 1927 already knew that the $|\psi(\mathbf{x})|^2$-distribution was stable under time evolution (Bacciagaluppi and Valentini 2009, p. 70) but did not have Bohm's (1952b) measurement theory for quantum observables other than position. The only author before Bohm who seriously considered the distinction between physical quantities and quantum operators – and even the idea of hidden variables in 'disequilibrium' violating the empirical predictions of quantum mechanics – appears to have been Grete Hermann; cf. Section 5 of her 1933 manuscript and her letter to Heisenberg of 9 February 1934 (Herrmann 2019, Letter 7, pp. 457–461). For a detailed discussion of the Bohmian equilibrium regime see e.g. Dürr, Goldstein and Zanghì (1992). For Bohmian disequilibrium see e.g. Valentini (2002a,b).

bles, and defines a 'purely' statistical theory as one in which pure states are not dispersion-free (not 'causal'). Given that measurable quantities in quantum mechanics are given by self-adjoint operators, from the 'obvious properties' of expectation values one easily derives the general trace formula (6), from which it follows that pure states in quantum mechanics are not causal. Quantum mechanics is thus the first example of a 'properly' statistical theory, unlike classical statistical mechanics (which can in fact be completed via 'hidden parameters'). Von Neumann concludes that 'no causal explanation of quantum mechanics is possible unless one sacrifices some part of the theory as it exists today' (Bohr 1939, p. 38). Again, this short exposition can be given the nuanced readings discussed above. Dieks (2017, Section 6) also reports that after Bohm's (1952a,b) papers appeared von Neumann seems to have agreed that Bohm's theory reproduced all the predictions of quantum mechanics, without objecting to it on the basis of his earlier results.

A final remark that seems revealing about what von Neumann thought his theorem showed was published after von Neumann's death by Léon Van Hove (who had worked for a few years at the Institute for Advanced Studies at Princeton) in a short paper on 'Von Neumann's contributions to quantum theory' that was part of an issue of the *Bulletin of the American Mathematical Society* in memory of von Neumann (Van Hove 1958, p. 98):

> Von Neumann devoted in his book considerable attention to a point which had not been discussed in the 1927 papers and which was later the subject of much controversy. It is the question of the possible existence of 'hidden variables', the consideration of which would eliminate the noncausal element involved in the measuring process. Von Neumann could show that hidden parameters with this property cannot exist if the basic structure of quantum theory is retained. Although he mentioned the latter restriction explicitly, his result was often quoted without due reference to it, a fact which sometimes gave rise to unjustified criticism in the many discussions devoted through the years to the possibility of an entirely deterministic reformulation of quantum theory.

I wish to conclude by suggesting a more dispassionate assessment of von Neumann's result in the light of another theme in the foundations of quantum mechanics, namely the difference between classical and quantum probability. Von Neumann's choice of linearity of expectation values as a probabilistic axiom may strike a modern reader as relying on superfluous algebraic structure (as opposed to Boolean structure), but von Neumann's paper is a formalisation of quantum mechanics as a probability theory for incompatible quantities, and his impossibility theorem is the first proof establishing the fundamental difference between classical and quantum probability.[56]

Acknowledgements

My heartfelt thanks go to Olival Freire for support and encouragement and for the wonderful way in which he has put together this volume, and to Christian Joas for his extremely helpful and detailed comments (as Heisenberg wrote, 'Wissenschaft entsteht im Gespräch'). I am very grateful to Sonja Smets and Soroush Rafiee Rad for the opportunity of presenting parts of this material in Amsterdam, to Marco Giovanelli for that of presenting in Tübingen, and to the audiences of those presentations. I have discussed related materials with more people than I can remember, but for recent discussions and correspondence I wish to thank especially Chris Mitsch (also for a draft of his paper) and Damon Moley.

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[56]Cf. the classic discussion by Pitowsky (1994), or for an introductory overview see again Bacciagaluppi (2016b).

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