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The statistical interpretation according to Born and Heisenberg

Guido Bacciagaluppi*

Abstract

At the 1927 Solvay conference Born and Heisenberg presented a joint report on quantum mechanics. I suggest that the significance of this report lies in that it contains a ‘final’ formulation of the statistical interpretation of quantum mechanics that goes beyond Born’s original proposal. In particular, this formulation imports elements from Heisenberg’s work as well as from the transformation theory of Dirac and Jordan. I suggest further a reading of Born and Heisenberg’s position in which the wave function is an effective notion. This can make sense of a remarkable aspect of their presentation, namely the fact that the ‘quantum mechanics’ of Born and Heisenberg apparently lacks wave function collapse.

1 Introduction

The fifth Solvay conference of 1927 saw the presentation of (and confrontation between) three fundamental approaches to quantum theory: de Broglie’s pilot-wave theory, Schrödinger’s wave mechanics, and ‘quantum mechanics’ (i.e. matrix mechanics and its further developments), the latter presented to the conference in a joint report by Born and Heisenberg.

A thorough examination of the conference proceedings reveals substantial amounts of material that are either little known or generally misrepresented. Such an examination is given in a forthcoming book on the 1927 Solvay conference (Bacciagaluppi and Valentini, 2008), which also includes a complete

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English version of the proceedings, based on the original-language materials where available.¹

In this paper, I wish to focus on the report by Born and Heisenberg, arguing that it contains a version of the statistical interpretation of quantum mechanics that goes well beyond that elaborated by Born in his papers on collisions and in his paper on the adiabatic theorem (Born, 1926a,b,c). In particular, the report offers an interpretation of the interference of probabilities, which appears to be related to Heisenberg's ideas as developed in his uncertainty paper (Heisenberg, 1927).

I shall further propose a reading of Born and Heisenberg's position in which the wave function has no fundamental status, in a way related to Heisenberg's paper on fluctuations (Heisenberg, 1926). Born and Heisenberg's report should thus indeed be seen as presenting an approach that is fundamentally different from both de Broglie's pilot-wave theory and Schrödinger's wave mechanics.

Finally, I suggest that the proposed reading makes sense of an aspect of Born and Heisenberg's presentation (and of the discussions) that is especially puzzling from the point of view of a modern reader, namely the almost total absence of the 'collapse of the wave function' or 'reduction of the wave packet'.

Much of the material presented below is based on Bacciagaluppi and Valentini (2008),² including parts of the book that are joint work or even principally the work of my coauthor (the latter especially in section 3). However, the perspectives on this material adopted in the paper and in the book

¹Quotations below from the proceedings of the conference are based on this English edition; page references are to the corresponding passages of the on-line draft available at <http://xxx.arxiv.org/abs/quant-ph/0609184>.

²The report itself and the discussion following it are translated and annotated on pp. 408–447. Born and Heisenberg's views are analysed and discussed principally in chapters 3 and 6. Among the topics discussed in this paper, the main ones treated in the book are the following. Born and Heisenberg's treatment of interference is discussed in section 6.1.2 (pp. 172–177). The derivation of transition probabilities in Born's collision papers and in Heisenberg's fluctuations paper are discussed, respectively, in section 3.4.3 (pp. 107–108) and 3.4.4 (pp. 109–111). Phase randomisation in measurement is discussed in detail on pp. 173–177. Extensive presentations and analyses of Born's discussion of the cloud chamber and of the exchange between Heisenberg and Dirac are given, respectively, in sections 6.2 (pp. 177–182) and 6.3 (pp. 182–189). Finally, Einstein's alternative hidden-variables proposal (with Heisenberg's comments) is discussed in detail in section 11.3 (pp. 259–265).

are very different. The idea of a ‘definitive’ version of the statistical interpretation merging elements from Born’s and Heisenberg’s work is hardly mentioned in the book. Furthermore, the presentation in the book is uncommittal about the views on collapse and on the status of the wave function held by Born and Heisenberg. This paper instead attempts to put forward one particular reading (not because it is unequivocally supported by the evidence, but as a proposal for making sense of the material that will need further evaluation).

It is useful therefore to spell out at least some of the differences between the treatment of the material in this paper and in Bacciagaluppi and Valentini (2008). First of all, as emphasised already, here I suggest that the report is a new stage of development of the statistical interpretation. This is something that is left largely implicit in the discussion in the book. Here I suggest that Born and Heisenberg present a single coherent position. The treatment in the book allows for possible differences in opinion between the two authors (emphasising for instance the possible relation between Born’s discussion of the cloud chamber and the guiding-field ideas in his collision papers). Here I try to make explicit links between Born and Heisenberg’s implicit notion of state in their treatment of transition probabilities on the one hand, and Born’s treatment of the cloud chamber on the other; I also hint at the possibility that Pauli had such a link in mind. Neither suggestion is made in the book. Last but not least, I suggest here that Born and Heisenberg did not believe in the reality of the wave function. This is mentioned in the book only as one tentative possibility among others.

Bacciagaluppi and Valentini (2008) refrains on purpose from drawing conclusions from the material that might have been premature. This paper hopes to be a first step in drawing further conclusions. Indeed, while the interpretation of quantum theory seems as highly controversial again today as it was in 1927, from the vantage point of eighty years of philosophy of quantum physics a more dispassionate evaluation of the sources in the interpretation debate should be possible. I wish to thank Antony Valentini for discussion and comments during the preparation of this paper, although of course all deviations from and additions to the presentation of the material as given in the book are my sole responsibility.

2 The statistical interpretation in Born and Heisenberg's report

The report by Born and Heisenberg on ‘quantum mechanics’ is surprisingly difficult for the modern reader. This is partly because Born and Heisenberg are describing various stages of development of the theory that are quite different from today’s quantum mechanics. At the same time, the interpretation of the theory also appears to have undergone important modifications, in particular regarding the notion of the state of a system (see Bacciagaluppi and Valentini, 2008, section 3.4).

It is known which sections of the report were drafted by Born and which by Heisenberg. In particular, the section most relevant to our concerns — that on the ‘Physical interpretation’ of the theory — was drafted by Born, who also prepared the final version of the paper, although Heisenberg made some further small changes.³ As we shall see, the interpretation presented merges crucially elements of Born’s and Heisenberg’s work, and (at least for the purposes of this paper) we shall consider the interpretational views as set forth in the report (and in the discussions reported below) as expressing a common voice. This is also supported by Born’s remark to Lorentz that Heisenberg and he were ‘of one and the same opinion on all essential questions’.⁴

2.1 The statistical interpretation

Until the 1927 report, the most explicit presentation of the statistical interpretation of quantum theory was that given in Born’s paper on the adiabatic theorem (1926c). The picture presented by Born is as follows. Particles exist, at least during periods in which systems evolve freely (say, between 0 and t). At the same time, they are accompanied by de Broglie-Schrödinger waves ψ . Regardless of the form of these waves, during a period of free evolution a system is always in a stationary state. When the waves ψ are developed in the basis of eigenstates $\psi_n(x)$ of energy, say

$$\psi(x, 0) = \sum_n c_n \psi_n(x) , \tag{1}$$

³Born to Lorentz, 29 August 1927, AHQP-LTZ-11 (in German). Cf. Bacciagaluppi and Valentini (2008, section 3.2).

⁴Born to Lorentz, *loc. cit.*; quoted with the kind permission of Prof. Gustav Born.

they yield the probabilities for the occurrence of the stationary states, the ‘state probabilities’ being given by $|c_n|^2$. During periods, say from t to T , in which an external force is applied (or the system interacts with another system) there may be no anschaulich representation of the processes taking place. As regards the particles, the only thing that can be said is that ‘quantum jumps’ occur, in that after the external influence has ceased the system is generally in a different stationary state. The evolution of the state probabilities instead is well-defined and determined by the Schrödinger equation, in the sense that the state probabilities at time T are given by the corresponding expression $|C_n|^2$ of the coefficients of $\psi(x, T)$.

For the case in which $\psi(x, 0) = \psi_n(x)$, Born determines explicitly these coefficients, call them b_{nm} , in terms of the time-dependent external potential; thus,

$$\psi(x, T) = \sum_m b_{nm} \psi_m(x) . \quad (2)$$

Given the interpretation of the quantities $|b_{nm}|^2$ as state probabilities, in this case they are also the ‘transition probabilities’ for the jump from the initial state, which by assumption is $\psi_n(x)$ at time t , to the final state $\psi_m(x)$ at time T .

Finally, for the general case of an initial superposition (1), Born states that the state probabilities $|C_n|^2$ have the form

$$|C_n|^2 = \left| \sum_m c_m b_{mn} \right|^2 , \quad (3)$$

noting that (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 expression should be simply $\sum_m |c_m|^2 |b_{mn}|^2$

(with a footnote to Dirac (1926) as also pointing out this fact⁵). He also remarks that, as he will show later on, the quantum jumps become independent in the case of an external perturbation by “‘natural” light’.

As it appears in Born’s adiabatic paper, the statistical interpretation is quite different both from the familiar textbook interpretations and from the

⁵Cf. especially pp. 674 and 677 of Dirac’s paper.

interpretation we shall find in Born and Heisenberg's Solvay report. For instance, the requirement that the state of an isolated system be always a stationary state is unfamiliar, to say the least. (As we shall see, it is eventually relaxed in Born and Heisenberg's report.)

For now let us focus on Born's remark about quantum jumps not being independent. This terminology appears to presuppose a probability space in which the elementary events do not correspond to single systems performing quantum jumps, but to N -tuples of systems all performing quantum jumps between t and T .⁶ (The analogous case in classical statistical mechanics is the treatment of gases of interacting rather than non-interacting particles.)

If this is the correct way of understanding Born's statistical interpretation of the wave function (at least as proposed in 1926), then Einstein may well have had Born's view in mind when at the 1927 Solvay conference he criticised what he labelled 'conception I' of the wave function (p. 487):⁷

The de Broglie-Schrödinger waves do not correspond to a single electron, but to a cloud of electrons extended in space. The theory gives no information about individual processes, but only about the ensemble of an infinity of elementary processes.

According to Einstein, it is only the alternative 'conception II', in which the wave function is a complete description of an individual system (and which he also goes on to criticise), that enables one to derive the conservation laws, the results of the Bothe-Geiger experiments and the straight tracks of α -particles in a cloud chamber. Note that the last example is taken up by Born in the general discussion (see below section 3.2).

Be it as it may, Born's paper on the adiabatic theorem lacks a separate discussion of interference; and this is the crucial point where the report by Born and Heisenberg goes further than Born's paper. Born and Heisenberg (p. 423) consider an atom that is initially in a superposition of energy states $\psi_n(x)$, with coefficients $c_n(0) = |c_n(0)| e^{i\gamma_n}$ and eigenvalues E_n . The

⁶Born's discussion of natural light later in the paper only reinforces this impression. Born assumes that due to the irregular temporal course of the external perturbation, the b_{nm} will fluctuate independently.

⁷For an alternative interpretation of Einstein's comments, see Bacciagaluppi and Valentini (2008, p. 225).

Schrödinger equation induces a time evolution

$$c_n(t) = \sum_m S_{nm}(t) c_m(0) . \quad (4)$$

In the special case where $c_m(0) = \delta_{mk}$ for some k , we have $|c_n(t)|^2 = |S_{nk}(t)|^2$, and Born and Heisenberg interpret $|S_{nk}(t)|^2$ as a transition probability. They also draw the conclusion that ‘the $|c_n(t)|^2$ must be the state probabilities’ (p. 424). Thus far the discussion is reminiscent of Born’s treatment, and Born and Heisenberg in fact quote Born’s paper on the adiabatic principle in support of this interpretation.

At this point, however, Born and Heisenberg recognise a ‘difficulty of principle’ (p. 424), which is precisely that for an initial superposition of energy states the final probability distribution is given by

$$|c_n(t)|^2 = \left| \sum_m S_{nm}(t) c_m(0) \right|^2 , \quad (5)$$

as opposed to

$$|c_n(t)|^2 = \sum_m |S_{nm}(t)|^2 |c_m(0)|^2 . \quad (6)$$

This ‘theorem of the interference of probabilities’ in Born and Heisenberg’s words appears to contradict what ‘one might suppose from the usual probability calculus’ (p. 424).

Born and Heisenberg then make a remarkable statement (pp. 424–425):

.... it should be noted that this ‘interference’ does not represent a contradiction with the rules of the probability calculus, that is, with the assumption that the $|S_{nk}|^2$ are quite usual probabilities. In fact, [(6)] follows from the concept of probability when and only when the relative number, that is, the probability $|c_n|^2$ of the atoms in the state n , has been *established* beforehand *experimentally*. In this case the phases γ_n are unknown in principle, so that [(5)] then naturally goes over to [(6)]

We shall return in the next section to Born and Heisenberg’s characterisation of the role of the experiment. What they are saying about the probability calculus is that the expressions $|S_{nk}|^2$ denote ‘usual’ transition probabilities *irrespective* of whether they appear in (5) or in (6). Instead, the reason

for the failure of (6) to hold in general is that the expressions $|c_m|^2$ are not always state probabilities, because the state probabilities themselves are not always well-defined (Bacciagaluppi and Valentini, 2008, pp. 175–176). If the state probabilities are well-defined (namely if the energy has been measured, in general non-selectively), then one can calculate them at future times using (6). The truth of this conditional statement, however, is not affected if the state probabilities in fact are not always well-defined.

This, now, is analogous to Heisenberg’s famous discussion of the ‘law of causality’ in his uncertainty paper: the law is again a conditional statement, which remains true although the state of the system is defined in fact only to within the accuracy given by the uncertainty principle. In Heisenberg’s own words: ‘... in the sharp formulation of the law of causality, “If we know the present exactly, we can calculate the future”, it is not the consequent that is wrong, but the antecedent. We cannot in principle get to know the present in all determining data’ (Heisenberg, 1927, p. 197).⁸

What Born and Heisenberg mean by ‘usual’ transition probabilities is evidently not the idea of conditional probabilities defined as quotients of the absolute probabilities, since for them the latter are not always well-defined. Instead they must mean some kind of potentialities, some probabilistic ‘field of force’, existing independently of the presence of a ‘test particle’.

Regarding the ‘state’ of the system, the picture they have in mind seems to be similar to that in Born’s papers: namely, that the actual state of the atom is a state of definite energy. The difference to the earlier picture is that now the stationary states exist or have a well-defined distribution only upon measurement (although the question of why this should be so is not explicitly addressed). Instead, the wave function merely defines a statistical distribution over the stationary states.

The step to considering arbitrary observables, and not just the energy, as having definite values only upon measurement is now very easy.⁹ In order to extend the above picture to the general case, one has to generalise Born and Heisenberg’s notion of transition probability to the case in which two different observables are measured at the beginning and the end of a given time interval. Here Born and Heisenberg are not very explicit. What they

⁸On Heisenberg’s treatment of the ‘law of causality’, see also Beller (1999, pp. 110–113).

⁹Again, Heisenberg’s uncertainty paper (Heisenberg, 1927, pp. 190–191), as well as his correspondence with Pauli (Heisenberg to Pauli, 23 February 1927, in Pauli, 1979, pp. 376–382) both mention explicitly the loss of a privileged status for stationary states.

actually do in the report is to define ‘relative state probabilities’, i.e. equal-time conditional probabilities for values of one quantity given the value of another, in terms of the projections of the eigenvectors (‘principal axes’) of one quantity onto the eigenvectors of the other. (In modern terminology, it is of course these expressions that are called ‘transition probabilities’.) In this they follow Dirac’s (1927) and Jordan’s (1927b,c) development of the transformation theory, which Heisenberg understood as generalising the ideas of his paper on fluctuations (Heisenberg, 1926).¹⁰

2.2 Transition probabilities and the status of the wave function

In Born’s work as presented above, the statistical interpretation is an interpretation of Schrödinger’s theory, albeit ‘in Heisenberg’s sense’ (Born, 1926c, p. 168). As we shall see now, instead, Born and Heisenberg in the report do not start directly with the Schrödinger equation. I shall suggest that in Born and Heisenberg’s view, although they may be very useful tools both for calculational purposes and for understanding interference, the wave function and the Schrödinger equation are only effective notions.

Section II of the report, on the ‘physical interpretation’ of quantum mechanics, begins with the following statement (p. 420):

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 of a system; further it makes a statement about 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

¹⁰Heisenberg to Pauli, 23 November 1926: ‘Here [in Copenhagen] we have also been thinking more about the question of the meaning of the transformation function S and Dirac has achieved an extraordinarily broad generalisation of this assumption from my note on fluctuations’ (in Pauli, 1979, p. 357).

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 raised here, the question to be addressed is how to incorporate into matrix mechanics the (actual) state of a system, and the time development of such a state.

Two methods for introducing change into matrix mechanics are then presented. First of all, following Heisenberg's paper on fluctuation phenomena (Heisenberg, 1926), Born and Heisenberg consider the matrix mechanical description of two coupled systems in resonance. This they interpret in terms of quantum jumps between the energy levels of the two systems, and they give an explicit expression for the corresponding transition probabilities. It is only after this matrix mechanical discussion that Born and Heisenberg introduce the time-dependent Schrödinger equation as a way for describing time dependence. From this, Born and Heisenberg then derive transition probabilities following Born's adiabatic paper (1926c), as described above.

Already in the collision papers Born had aimed precisely at including into matrix mechanics a description of the transitions between stationary states (Born, 1926a,b). Born had managed to describe the asymptotic behaviour of the combined system of electron and atom solving by perturbation methods the time-independent Schrödinger equation, yielding a superposition of components associated to various, generally inelastic, collisions in which energy is conserved. Interpreting statistically the coefficients in the expansion, and since the incoming asymptotic wave function corresponds to a fully determined stationary state and 'uniform rectilinear motion',¹¹ one obtains the probabilities for quantum jumps from the given 'initial' state to the given 'final' state, i.e. the desired transition probabilities.¹²

At first Born may have thought that wave mechanical methods were indispensable for this purpose.¹³ To Heisenberg's delight, however, Pauli was

¹¹This is, indeed, Born's terminology (1926a, p. 864; 1926b, p. 806). In this context, cf. also the discussion of Born and Wiener (1926) in Bacciagaluppi and Valentini (2008, section 3.4.1).

¹²Note that Born considers indeed two conceptually distinct objects: on the one hand the stationary states of the atom and the electron, on the other hand the wave function that defines the probability distribution over the stationary states. He reserves the word 'state' only for the stationary states.

¹³Cf. Born to Schrödinger, 16 May 1927: 'the simple possibility of treating with it

able to sketch how one could reinterpret Born's results in terms of matrix elements.¹⁴ A few days later, Heisenberg sent Pauli the manuscript of his paper on fluctuation phenomena (Heisenberg, 1926), in which he developed considerations similar to Pauli's ones in the context of the example of two atoms in resonance. Indeed, starting from a closed system (thus stationary from the point of view of matrix mechanics) and focussing on the description of the subsystems, Heisenberg was able to derive explicit expressions for the transition probabilities within matrix mechanics proper, without having to introduce the wave function as an external aid. A very similar result was derived at the same time by Jordan (1927a), using two systems with a single energy difference in common.

Born's collision papers and the papers by Heisenberg and by Jordan can be all understood as seeking to obtain 'information about actual phenomena', by 'direct[ing] one's attention to a *part* of the system'. In this context, the fact that it is Heisenberg's setting rather than Born's which is chosen in the report suggests that Born and Heisenberg indeed intend to make the point that matrix mechanics can account for time-dependent phenomena without the aid of wave mechanics.

It is in this sense, I suggest, that one should read the following remark made by Born and Heisenberg between their introduction of the time-dependent Schrödinger equation and their discussion of transition probabilities and interference (p. 423):

Essentially, the introduction of time as a numerical variable reduces to thinking of the system under consideration as coupled to another one and neglecting the reaction on the latter, but this formalism is very convenient and leads to a further development of the statistical view.

In particular, I suggest that in Born and Heisenberg's view one should not simply interpret a time-dependent external potential in the Schrödinger equation (as used in the adiabatic paper for instance) as a substitute for the

aperiodic processes (collisions) made me first believe that your conception was superior' (quoted in Mehra and Rechenberg, 2000, p. 135).

¹⁴See Pauli to Heisenberg, 19 October 1926, in Pauli (1979, pp. 340–349), and Heisenberg's reply: 'Your calculations have given me again great hope, because they show that Born's somewhat dogmatic viewpoint of the probability waves is only one of many possible schemes' (Heisenberg to Pauli, 28 October 1926, in Pauli, 1979, p. 350).

full Schrödinger equation of the combined system, but that the Schrödinger equation itself arises from considering only subsystems.¹⁵

This reading is further supported by Born and Heisenberg’s remarks on generalising transition probabilities to the case of an arbitrary observable, which are now couched in terms that bypass wave functions entirely (pp. 428–429):

Alongside the concept of the relative state probability $|\varphi(q', Q')|^2$, there also occurs the concept of transition probability, namely, whenever one considers a system as depending on an external parameter, be it time or any property of a weakly coupled external system. Then the system of principal axes of any quantity becomes dependent on this parameter; it experiences a rotation, represented by an orthogonal transformation $S(q', q'')$, in which the parameter enters The quantities $|S(q', q'')|^2$ are the ‘transition probabilities’; in general, however, they are not independent, instead the ‘transition amplitudes’ are composed according to the interference rule.

In part, reference to wave functions here is eliminated through a switch to the Heisenberg picture. One should note, however, that Born and Heisenberg manage to eliminate reference to the wave function completely *only* because they consider exclusively maximal observables. In the more general case of non-maximal (i.e. coarse-grained) observables,¹⁶ transition probabilities (whether in their sense or in the modern sense) depend also on the quantum state.

The overall picture one glimpses from these aspects of Born and Heisenberg’s remarks is that what exists are just transition probabilities and measured values (although, as mentioned already, it is not explained why measurement should play such a special role).

As regards the transition probabilities, the $|S_{nk}|^2$ defined by Born and Heisenberg are *independent* of the actual wave function. They can be calculated using the formalism of wave functions, namely as the coefficients in

¹⁵Cf. also the derivation of time-dependent transition probabilities in Heisenberg (1930, pp. 148–150).

¹⁶And of course in the most general case of observables as positive-operator-valued measures (POVMs), for which see e.g. Peres (1993, pp. 282–289).

(4) for the case in which the initial wave function is the k th eigenstate of energy, but they are taken as the correct transition probabilities even when the initial wave function is arbitrary.

By way of contrast, one could take Bell's (1987) discrete and stochastic version of de Broglie's pilot-wave theory. In a theory of this type, given a choice of preferred observable ('beable' in Bell's terminology), the $|c_n|^2$ are indeed always state probabilities, and one constructs appropriate transition probabilities that are generally different from Born and Heisenberg's $|S_{nk}|^2$, thereby explicitly retaining the validity of the standard formula (6). Evidently, Bell's transition probabilities must depend on the actual wave function of the system, which thus acts as a pilot wave, as in de Broglie's theory. Born and Heisenberg instead choose to give up the $|c_n|^2$ as state probabilities and to keep the transition probabilities independent of the actual wave function (which is thus not a pilot wave in any sense).

In general, wave functions themselves can usefully represent statistical information about measured values, but one need not consider wave functions as describing the real state of the system (*contra* Schrödinger). In this sense, they appear to resemble more the Liouville distributions of classical mechanics, a comparison suggested also by some of Born and Heisenberg's remarks (p. 433):¹⁷

For some simple mechanical systems the quantum mechanical spreading of the wave packet agrees with the spreading of the system trajectories that would occur in the classical theory if the initial conditions were known only with the precision restriction [given by the uncertainty principle]. But in general the statistical laws of the spreading of a 'packet' for the classical and the quantum theory are different

As Darrigol (1992, p. 344) has emphasised, there is no notion of state vector either in Dirac's paper on the transformation theory (Dirac, 1927). (The well-known bras and kets do not appear yet.) The main result of Dirac's paper is to determine the conditional probability density for one observable given a value for a different observable, a result that Dirac illustrates by discussing precisely Heisenberg's example of transition probabilities in resonant atoms and Born's collision problem. As we shall see in section 3.3,

¹⁷Note also that in his discussion of the cloud chamber, Born once refers to the wave packet as a 'probability packet' (p. 483).

however, by the time of the Solvay conference Dirac's and Born and Heisenberg's views had diverged, both with regard to whether the wave function should describe 'the state of the world', and with regard to the notion of the collapse of the wave function.

3 Measurements and effective collapse

It is remarkable that the reduction of the wave packet is totally absent from Born and Heisenberg's report, although this concept had been famously introduced by Heisenberg himself in the uncertainty paper (Heisenberg, 1927, p. 186). In this section we shall discuss what appears to take the place of reduction in Born and Heisenberg's report, then we shall focus on the two places in the conference proceedings where the reduction of the wave packet appears explicitly: Born's treatment of the cloud chamber in his main discussion contribution (pp. 483–486) and the intriguing exchange between Dirac and Heisenberg (pp. 494–497), both appearing in the general discussion at the end of the conference.

3.1 Measurement and phase randomisation

What is Born and Heisenberg's description of measurement? In the report, measurement appears only in the discussion of interference, namely, as we have seen, as the source for its suppression. This suppression of interference is achieved neither by applying the 'reduction of the wave packet' (i.e. not by collapsing the wave function onto the eigenstates of the measured observable) nor through entanglement of the measured system with the measuring apparatus (a simple form of what we would now call decoherence). The latter would in fact presuppose a quantum mechanical treatment of the interaction between the two, which was uncharacteristic for the time.

Instead, Born and Heisenberg appear to take measurement as introducing a randomisation of the phase in the wave function (Bacciagaluppi and Valentini, 2008, p. 173–177): indeed, they consider the case in which (p. 425):

.... the relative number, that is, the probability $|c_n|^2$ of the atoms in the state n , has been *established* beforehand *experimentally*. In this case the phases γ_n are unknown in principle,

so that [(5)] then naturally goes over to [(6)]

At this point Born and Heisenberg add a reference to Heisenberg's uncertainty paper, which indeed contains a more detailed version of essentially the same claim (see also below section 3.3). There, Heisenberg considers a Stern-Gerlach atomic beam passing through two successive regions of field inhomogeneous in the direction of the beam (so as to induce transitions between energy states without separating the beam into components). If the input beam is in a definite energy state then the beam emerging from the first region will be in a superposition. The probability distribution for energy emerging from the second region will then contain interference — as in (5), where the 'initial' superposition (1) is now the state emerging from the first region. Heisenberg asserts that, if the energy of an atom is actually measured between the two regions, then because of the resulting perturbation 'the "phase" of the atom changes by amounts that are in principle uncontrollable' (Heisenberg, 1927, pp. 183–184), and averaging over the unknown phases in the final superposition yields a non-interfering result.

This is clearly not the same as applying the collapse postulate. Indeed, if one applied the usual 'Dirac-von Neumann' postulate, after the measurement the atoms would be in eigenstates of energy, and the non-interfering result would be obtained by averaging over the different energy values.

The difference between the two descriptions is masked by the fact that the averages are the same, i.e. a statistical mixture of states of the superposed form (1), with randomly-distributed phases γ_n , is indeed statistically equivalent to a mixture of energy states $\psi_n(x)$ with weights $|c_n(0)|^2$, because the corresponding density operators are the same. But for the subensembles selected on the basis of the measurement results (i.e. for the subensembles with definite values for the energy), the density operators are clearly different.

In the standard collapse case, indeed, the selected subensemble is homogeneous and described by a pure state $\psi_n(x)$. In the case of phase randomisation, taken literally, the subensembles selected on the basis of the measurement results are instead described by the *same* mixture of superposed states with randomly-distributed phases γ_n . If we take the state of the system (in the modern sense, i.e. the density operator) as determining the probabilities for the results of future measurements, we ought to conclude that in the case of phase randomisation an immediate repetition

of the measurement will generally not yield the same result as the original measurement, and that any value could occur as a possible result.

However, if our reading above of Born and Heisenberg's discussion of the probability calculus is correct, the quantum state in the modern sense is *not* what determines the result of a subsequent measurement. While each atom in, say, the k th subensemble has a wave function of the form (1) with some unknown phases in the coefficients, we also know that it has the energy value E_k , because the energy has been measured and the atom has been selected precisely on the basis of this energy value. But now, according to Born and Heisenberg, the transition probabilities $|S_{nk}|^2$ are independent of the actual wave function of the atom, so that if the atom is known to have the energy E_k , the statistical distribution of the energy values upon repetition of the measurement is simply given by (6) with $c_m(0) = \delta_{mk}$. If the repetition takes place immediately after the first measurement, the transition probabilities $|S_{nk}|^2$ will tend to δ_{nk} , so that indeed the first result will be confirmed (Bacciagaluppi and Valentini, 2008, p. 175–176).

One might dispute that the description of measurements as randomising the phases should be taken literally: it might be simply a rather sloppy way of talking about the decoherence induced by the measurement (encountered sometimes even today in discussions of decoherence in general).¹⁸ However, the fact that Born and Heisenberg during the conference (and Heisenberg in the uncertainty paper) appear to use both the description of measurements in terms of phase randomisation and that in terms of reduction of the wave packet as equally good alternatives, may indicate that neither should be taken literally. The wave function can be chosen one way or another, depending on what is more convenient 'for practical purposes'.

3.2 Born's discussion of the cloud chamber

In his discussion of the cloud chamber, Born attributes to Einstein the question of how one can account for the approximately straight particle track revealed by a cloud chamber, even if the emission of an α -particle is undi-

¹⁸My thanks to Antony Valentini for pointing out that a description of measurement in terms of phase randomisation appears also in Bohm's textbook on quantum mechanics (Bohm, 1951, pp. 122, 600–602).

rected, so that the emitted wave function is approximately spherical.¹⁹ Born asserts that to answer it (p. 483):

.... one must appeal to the notion of ‘reduction of the probability packet’ developed by Heisenberg. The description of the emission by a spherical wave is valid only for as long as one does not observe ionisation; as soon as such ionisation is shown by the appearance of cloud droplets, in order to describe what happens afterwards one must ‘reduce’ the wave packet in the immediate vicinity of the drops. One thus obtains a wave packet in the form of a ray, which corresponds to the corpuscular character of the phenomenon.

But then Born goes on to consider if wave packet reduction can be avoided by treating the atoms of the cloud chamber, along with the α -particle, as a single system described by quantum theory, a suggestion that he attributes to Pauli. The latter had made this suggestion also in a letter to Bohr one week before the beginning of the Solvay conference.²⁰

This is precisely a point that was not quite satisfactory in Heisenberg [(1927)]; there the ‘reduction of the packets’ seemed a little mystical. Now in fact it should be stressed that such reductions are not necessary in the first place if one includes *in* the system all means of measurement. But in order to describe at all observational results theoretically, one has to ask what one can say alone about a *part* of the whole system. And then from the complete solution one sees immediately that, in many cases (of course not always), leaving out the means of observation can be formally replaced by such reductions.

Born’s own opinion is as follows (p. 483):

Mr Pauli has asked me if it is not possible to describe the process without the reduction of wave packets, by resorting to a multi-dimensional space whose number of dimensions is three times the

¹⁹Cf. Einstein’s main contribution to the general discussion (pp. 486–488), and above, section 2.1.

²⁰Pauli to Bohr, 17 October 1927, in Pauli (1979, p. 411).

number of all the particles present This is in fact possible and can even be represented in a very anschaulich manner [d'une manière fort intuitive] by means of an appropriate simplification, but this does not lead us further as regards the fundamental questions. Nevertheless, I should like to present this case here as an example of the multi-dimensional treatment of such problems.

Both Born and Pauli thus seem to think that the reduction of the wave packet is a dispensable element in the description of measurements.²¹ However, Born's subsequent discussion remains somewhat unclear about why this should be so. From the above quotation, it appears that the discussion is intended mainly as an illustration of the use of configuration-space wave functions (a point reiterated by Born at the end of his discussion). Born, indeed, merely presents a multi-dimensional treatment of the problem, simplified in that all motions are in one dimension and the cloud chamber is represented by only two atoms. Only in the end does Born remark that (p. 486):

To 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 [atoms] 1 and 2 is hit

Now, provided this remark is at all relevant to the question of whether wave packet reduction is unnecessary, it should be read as an alternative to the description by means of reduction. That is, one should be able to leave the wave packet uncollapsed and choose *instead* a direction of propagation for the α -particle, either because this is truly what happens upon measurement, or because the two descriptions are equivalent at least 'for all practical purposes', in which case presumably neither is to be taken literally.

Incidentally, the atoms in the cloud chamber are described by Born on the same footing as the α -particle, making this perhaps the first example of explicit inclusion of a measuring apparatus in the quantum mechanical description. Note that the fact that the Schrödinger equation was not applied to the measurement interaction means that there was no awareness at

²¹Note that also Pauli's remarks to Heisenberg about transition probabilities and Born and Heisenberg's treatment thereof, discussed in section 2.2, crucially make reference to 'what one can say alone about a *part* of the whole system'. Pauli's suggestion to Born and his remarks to Heisenberg may in fact be related.

the 1927 Solvay conference of the ‘measurement problem’, in the sense of macroscopic superpositions arising from the measurement interaction. For instance, also in Bohr’s famous exchanges with Einstein between the sessions of the conference (Bohr, 1949), Bohr applies only the uncertainty principle to the apparatus, and certainly not the Schrödinger equation, so that no macroscopic superpositions are considered. As regards Born’s example of the cloud chamber, it could have been used in principle to raise this problem. However, if the reading of Born and Heisenberg’s position suggested here is correct, it is not surprising that Born did not see the resulting macroscopic superposition as a problem, since the ‘state’ of the α -particle (under the given conditions) would correspond indeed to its direction of motion.

3.3 The exchange between Heisenberg and Dirac

Born’s remarks on the collapse of the wave function should be contrasted with Dirac’s remarks on the same topic, also in the general discussion (pp. 494–495):

According to quantum mechanics the state of the world at any time is describable by a wave function ψ , which normally varies according to a causal law, so that its initial value determines its value at any later time. It may however happen that at a certain time t_1 , ψ can be expanded in the form

$$\psi = \sum_n c_n \psi_n ,$$

where the ψ_n ’s are wave functions of such a nature that they cannot interfere with one another at any time subsequent to t_1 . If such is the case, then the world at times later than t_1 will be described not by ψ but by one of the ψ_n ’s. The particular ψ_n that it shall be must be regarded as chosen by nature.

This, according to Dirac (p. 495) is ‘an irrevocable choice of nature, which must affect the whole of the future course of events’. Dirac thus appears both to take the wave function to be a real physical object, and to take the collapse of the wave function to be a real physical process, connected with lack of interference (an interesting point both from today’s perspective and for the exchange with Heisenberg). But Dirac goes further, and recognises

that there are circumstances where the choice made by nature cannot have occurred at the point where it might have been expected. Dirac considers at some length the specific example of the scattering of an electron, concluding with the following observation (pp. 495–496):

If, now, one arranged a mirror to reflect the electron wave scattered in one direction d_1 so as to make it interfere with the electron wave scattered in another direction d_2 , one would not be able to distinguish between the case when the electron is scattered in the direction d_2 and when it is scattered in the direction d_1 and reflected back into d_2 . One would then not be able to trace back the chain of causal events so far, and one would not be able to say that nature had chosen a direction as soon as the collision occurred, but only [that] at a later time nature chose where the electron should appear. The interference between the ψ_n 's compels nature to postpone her choice.

In Dirac's manuscript of this discussion contribution,²² a cancelled version of the last sentence begins with 'Thus a possibility of interference', while another cancelled version begins with 'Thus the existence of interference'. Possibly, Dirac hesitated here because he saw that in principle the mirror could always be added by the experimenter after the scattering had taken place. Thus, there would be no cases in which interference could be ruled out as impossible, making this an unrealisable criterion for the occurrence of collapse.

Precisely this point was made by Heisenberg, shortly afterwards in the discussion (p. 497):

I do not agree with Mr Dirac when he says that, in the described experiment, nature makes a choice. Even if you place yourself very far away from your scattering material, and if you measure after a very long time, you are able to obtain interference by taking two mirrors. If nature had made a choice, it would be difficult to imagine how the interference is produced. I should rather say, as I did in my last paper [(Heisenberg, 1927)], that the *observer himself* makes the choice, because it is only at the moment when the observation is made that the 'choice' has

²²AHQP-36, section 10.

become a physical reality and that the phase relationship in the waves, the power of interference, is destroyed.

Note the striking resemblance between what is said here by Heisenberg and what is said (more understatedly) by Born in his treatment of the cloud chamber. Born talks about the ‘choice of one of the two directions of propagation’, a choice which is taken not when one of the two atoms is hit, but when it is ‘established’ that it is hit (when the ionisation is ‘shown’ by the appearance of the cloud droplets); Heisenberg (who of course is also following Dirac’s terminology) talks of a ‘choice’ of which path is taken by the electron, a choice which becomes physically real ‘only at the moment when the observation is made’. But Heisenberg goes further than Born here, suggesting that what happens upon observation is that ‘the phase relationship in the waves, the power of interference, is destroyed’, i.e. that the effect of measurement is phase randomisation rather than collapse.

4 Born and Heisenberg on ‘hidden variables’

To conclude, we shall now have a brief look at the views on what one would now call ‘hidden variables’ (in particular in the context of guiding fields) expressed at the time by Born and by Heisenberg, mostly before the Solvay conference. Indeed, the idea of observables having values that are not strictly linked to the wave function of the system (no ‘eigenstate-eigenvalue link’) might strike one as typical of hidden variables theories. This is precisely what happens in pilot-wave theories of the Bell type, as mentioned in section 3.1 above. Unsurprisingly, however, the views on the subject expressed by Born and by Heisenberg are quite negative.

4.1 Born on the practical irrelevance of microcoordinates

Consider Born’s second paper on collisions (Born, 1926b). In this paper Born makes an explicit link between his work and guiding-field ideas, saying that while in the context of optics one ought to wait until the development of a proper quantum electrodynamics, in the context of the quantum mechanics of material particles the guiding field idea could be applied already, using the de Broglie-Schrödinger waves as guiding fields; these, however, determine the trajectories merely probabilistically (p. 804). In the concluding remarks

of the paper, Born comments explicitly on whether this picture is to be regarded as fundamentally indeterministic (pp. 826–827):

In my preliminary communication [(Born, 1926a)] I laid very particular stress on this indeterminism, since it seems to me to correspond perfectly to the practice of the experimenter. But of course it is open to anyone who will not rest content therewith to assume that there are further parameters not yet introduced in the theory that determine an individual event. In classical mechanics these are the ‘phases’ of the motion, e.g. the coordinates of the particles at a certain instant. It seemed to me unlikely at first that one could freely include quantities in the new theory that correspond to these phases; but Mr Frenkel²³ has informed me that perhaps this in fact can be done. Be it as it may, this possibility would change nothing in the practical indeterminism of collision processes, since indeed one cannot give the values of the phases; it must lead, besides, to the same formulas as the ‘phaseless’ theory proposed here.

Thus, Born took it that a ‘completion’ of quantum mechanics through the introduction of further parameters into the theory would have no practical consequences, an opinion echoed in Born and Heisenberg’s report immediately after their introduction of transition probabilities (p. 422):

While the determinateness of an individual process is assumed by classical physics, practically in fact it plays no role, because the microcoordinates that determine exactly an atomic process can never all be given; therefore by averaging they are eliminated from the formulas, which thereby become statistical statements. It has become apparent that quantum mechanics represents a merging of mechanics and statistics, in which the unobservable microcoordinates are eliminated.

At the Solvay conference the idea of quantum mechanics as eliminating microscopic coordinates from the description of motions is mentioned by Born also in discussing Schrödinger’s treatment of the Compton effect (p. 371; cf.

²³This is presumably Y. I. Frenkel, who at the time was in Germany on a Rockefeller scholarship. Born had supported Frenkel’s application. (See Frenkel, 1996, p. 72).

also p. 444). It may have been an important element of Born's intuition, and appears also in Born's reaction to the EPR paper (Einstein, Podolsky and Rosen, 1935).²⁴

4.2 Heisenberg and Einstein on hidden variables

The above statements by Born may not rule out unequivocally the possibility of thinking of the wave function as a guiding field (more so perhaps his statements in the adiabatic paper on the Unanschaulichkeit of the quantum jump). Heisenberg's statements on the subject instead indicate both that he understood the principles behind pilot-wave theories and that he rejected them decidedly.

Heisenberg's views are contained in a letter to Einstein about the latter's own unpublished hidden-variables proposal (cf. Pais, 1982, p. 444). In May 1927, Einstein had proposed what in retrospect appears to be an alternative version of pilot-wave theory, with particle trajectories determined by the many-body wave function, but in a way different from that of de Broglie's theory. This theory was described in a paper entitled 'Does Schrödinger's wave mechanics determine the motion of a system completely or only in the sense of statistics?',²⁵ which was presented on 5 May 1927 at a meeting of the Prussian Academy of Sciences. On the same day Einstein wrote to Ehrenfest that '... in a completely unambiguous way, one can associate definite movements with the solutions [of the Schrödinger equation]' (quoted in Howard, 1990, p. 89). However, on 21 May, before the paper appeared in print, Einstein withdrew it from publication. The paper remained unpublished, but its contents are nevertheless known from the manuscript version in the Einstein archive — see also Belousek (1996) and Holland (2005).

Heisenberg had heard about Einstein's theory through Born and Jordan, and on 19 May — just two days before Einstein withdrew the paper — wrote to Einstein enquiring about it. On 10 June 1927, Heisenberg wrote to Einstein again, this time with detailed comments and arguments against what Einstein was (or had been) proposing. I shall now briefly summarise this second letter.²⁶

²⁴See Born to Schrödinger, 28 June 1935, AHQP-92, section 2 (in German).

²⁵'Bestimmt Schrödingers Wellenmechanik die Bewegung des Systems vollständig oder nur im Sinne der Statistik?', Albert Einstein Archive 2-100.00; currently available on-line at <http://www.alberteinstein.info/db/ViewDetails.do?DocumentID=34338> .

²⁶Heisenberg to Einstein, 19 May and 10 June 1927, Albert Einstein Archive 12-173.00

Evidently, Einstein had not sent the withdrawn paper in reply to the original enquiry, for Heisenberg mentions he has learnt nothing new, but Heisenberg says he would like to write again why he believes indeterminism is '*necessary*, not just consistently possible'. If he has understood his viewpoint correctly, Einstein thinks that, while all experiments will agree with the statistical quantum theory, nevertheless in the future one will be able to talk also about definite particle trajectories. Heisenberg's main objection is now as follows.

Consider free electrons with a constant and low velocity, 'so slow, that the de Broglie wavelength is very large compared to the size of the particle, i.e. the force fields of the particle should be practically zero on distances of the order of the de Broglie wavelength'. Such electrons strike a grating with spacing comparable to their de Broglie wavelength. Heisenberg remarks that, in Einstein's theory, the electrons will be scattered in discrete spatial directions. Now, if the initial position of a particle were known one could calculate where the particle will hit the grating and 'set up some obstacle that reflects the particle in some *arbitrary* direction, quite independently of the other parts of the grating'. This could be done, if the forces between the particle and the obstacle act indeed only at short range, small with respect to the spacing of the grating. Heisenberg then continues:

In reality the electron is reflected independently of the obstacle in question in the definite discrete directions. One could only escape this if one sets the motion of the particle again in direct relation to the behaviour of the waves. But this means that one assumes that the size of the particle, that is, its interaction forces, depend on the velocity. Thereby one actually gives up the word 'particle' and loses in my opinion the understanding for why in the Schrödinger equation or in the matrix Hamiltonian function always appears the simple potential energy e^2/r . If you use the word 'particle' so liberally, I take it to be very well possible that one can define also particle trajectories. But the great simplicity that in the statistical quantum theory consists in that the motion of the particles takes place classically, insofar as one can talk of motion at all, in my opinion is lost.

Heisenberg then notes that Einstein seems willing to sacrifice this simplicity

and 12-174.00 (both in German). Passages from the letter of 10 June are quoted with the kind permission of Prof. Helmut Rechenberg of the Werner Heisenberg Archive.

for the sake of maintaining causality. However, even Einstein's approach would not be able to change the fact that many experiments would be determined only statistically: 'Rather we could only console ourselves with the fact that, while for us because of the uncertainty relation $p_1q_1 \sim h$ the principle of causality would be meaningless, the good Lord in fact would know in addition the position of the particle and thereby could preserve the validity of the causal law'. Heisenberg concludes the objection by saying that he finds it 'actually not attractive [eigentlich doch nicht schön] to want to describe physically more than the connection between experiments'.

Note that Heisenberg's objection is not that the theory does not predict the usual scattering pattern in the practically unrealisable case in which one manipulates the trajectory of a particle with known initial position. Rather, his gedankenexperiment serves to establish the point that, even in the normal case (in which the initial position of the particle is unknown), the direction in which a 'particle' is scattered must depend only on the local features of the grating, thus contradicting the normal experimental results. The only way to have the direction of scattering depend on the features of the grating other than where the particle hits it, is to make the trajectory of the particle depend on the associated wave rather than on particle-like short-range interaction behaviour.

It is striking that Heisenberg's objection concerning the electron and the grating shows that he thought that a trajectory-based deterministic theory of quantum phenomena is possible. It is equally striking that Heisenberg appears to have thought that such a theory is nevertheless unacceptable on what would seem to be aesthetic grounds (or grounds of *Anschaulichkeit*), because it gives up both the usual concept of particle and the mathematical simplicity of quantum mechanics. This objection appears to have remained a mainstay of Heisenberg's negative views on hidden variables. Indeed, Heisenberg repeated it also in his own draft reply to the EPR paper (Heisenberg, 1985, p. 416).²⁷

²⁷My thanks to Elise Crull for directing my attention to this passage in Heisenberg's draft.

5 Conclusion

In this paper, I have suggested that Born and Heisenberg's report at the 1927 Solvay conference is significant because it presents a more mature and definitive version of the statistical interpretation of quantum mechanics. The key point about this suggestion is that the interpretation in the report merges elements of Born's interpretational work of 1926 and of Heisenberg's work on fluctuations and in the uncertainty paper. I have also proposed a specific reading of Born and Heisenberg's position (thereby continuing where the analysis of Bacciagaluppi and Valentini, 2008, leaves off). The key intuition behind this proposal is that Born and Heisenberg did not take the wave function to be a real entity.

Of course, it is well-known that Heisenberg at least was strongly antagonistic to Schrödinger's introduction of wave functions and to his attempts to interpret them as giving an anschaulich picture of quantum systems. While Born's work of 1926 can be put in relation with ideas on guiding fields, I suggest that, at least come 1927, Born and Heisenberg's conception of the wave function was thoroughly statistical, i.e. more analogous to a classical Liouville distribution, thus making also the collapse of the wave function a matter of convenience of description.

Born and Heisenberg's own words give the impression that they considered the presentation in their report to be indeed a final formulation of the theory and interpretation of quantum mechanics (pp. 409, 437):²⁸

Quantum mechanics is meant as a theory that is in this sense anschaulich and complete for the micromechanical processes ([Heisenberg, 1927]) There seems thus to be no empirical argument against accepting fundamental indeterminism for the microcosm.

.... we consider [quantum mechanics] to be a closed theory [geschlossene Theorie], whose fundamental physical and mathematical assumptions are no longer susceptible of any modification.

Even as these views were being expressed, there remained significant dif-

²⁸One can recognise Heisenberg's pen in these passages, which were in fact drafted by him (Born to Lorentz, *loc. cit.*, note 3). Cf. also Heisenberg's later writings on the concept of 'closed theories', e.g. Heisenberg (1948).

ferences of opinion even within the ‘Göttingen-Copenhagen’ camp (as seen in the exchange between Dirac and Heisenberg). Moreover, with its lack of collapse and perhaps even of fundamental wave functions, the interpretation presented was itself quite different from what might be assumed today to have been the ‘statistical interpretation’ of quantum theory.

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