QUANTUM MECHANICS

Historical Contingency and the Copenhagen Hegemony

JAMES T. CUSHING

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EIGHT

Early Attempts at Causal Theories: A Stillborn Program

In this chapter I begin to trace the origins and eventual fate of the causal quantum-theory program. This begins with de Broglie's theory of phase waves, includes various hydrodynamical and velocity-field (essentially "hidden variables") interpretations, such as those due to Madelung and Einstein, and progresses through de Broglie's theory of the double solution and his (provisional) pilot-wave model, the encounters with the Copenhagen group at the 1927 Solvay congress, the impact of von Neumann's "impossibility" proof, an interpretation by Rosen, Bohm's 1952 paper and, finally, Nelson's 1966 work on a stochastic basis for the Schrödinger equation.

After his fundamental paper on wave mechanics in 1926, Erwin Schrödinger at first attempted to give a *realistic* interpretation to the ψ-function.[1] One objection to this was that, while a real wave might exist in the physical *three*-dimensional space (or possibly in a four-dimensional space-time) in which we exist, it would make little sense to speak of a physical wave existing and propagating in a $3n$-dimensional *configuration* space (where n is the number of particles in the system).[2] Such a ψ must be merely a mathematical construct, useful perhaps for calculating probabilities, but surely not to be assigned actual physical reality.

Max Born in his successful probability interpretation of $|\psi|^2$ did not initially *categorically* rule out the possibility that quantum mechanics, as formulated at that time, might be a statistical theory as a matter of *practical* necessity, rather than as a matter of absolute principle.[3] In an article in *Nature* in early 1927, Born even allowed the possible existence of microscopic atomic coordinates that are averaged over in practice. "Of course, it is not forbidden to believe in the existence of [microscopic] coordinates."[4] Born was at least agnostic on the question of whether, in later terminology, a hidden-variables version of quantum mechanics might indeed exist.

However, Pascual Jordan, in a later issue of the same volume of *Nature*, was on the whole less open to the possibility of a basically classical, continuous, and picturable view of microphenomena.[5] Jordan did not of-

fer proofs or even compelling arguments for foreclosing this *possibility* of continuity, but rather referred to the *opinions* of most scientists and to the *difficulty* of conceiving of such an alternative. Born later came to believe in the impossibility of microscopic coordinates, mainly as a result of Heisenberg's "uncertainty" paper.[6]

The previously dominant worldview of classical physics had been based on continuity and picturability for physical processes, and even Born was not at first hostile toward the possibility of a *largely* classical ontology being compatible with the new quantum theory. How positions hardened against this a priori intuitively appealing understanding of the basic physical phenomena is the question I now address.

8.1 Madelung

In the fall of 1926, Erwin Madelung suggested a hydrodynamical interpretation of quantum mechanics.[7] He began with the Schrödinger equation for the wave function ψ and made a set of mathematical transformations (similar to the type Bohm would make decades later). All of the mathematical details (appendix 1 to this chapter) aside, Madelung suggested interpreting the Schrödinger equation as representing a physical "fluid" (of identical particles of mass m) of density ρ and with a velocity field v. This ideal fluid had no viscosity. One difficulty of this interpretation, since Madelung was considering a fluid consisting of a continuous distribution of *charge*, was that the equations contained a "quantum force" term (to use a later terminology here) that depended only upon the *local* density ρ ($= |\psi|^2$), but not upon the total charge distribution. The physical meaning or significance of the additional ("quantum potential") term in his Newtonian equation of motion will be a recurrent theme of, or actually a problem for, various causal interpretations I discuss. Although Madelung claimed that this model gave an intuitively clear picture of quantum phenomena, it is not wholly evident, at least to a modern reader, just *what* this ideal fluid was, how it represented an atom in some state, or how emission and absorption phenomena were to be envisioned.[8] In spite of conceptual difficulties that remain, it is clear that Madelung was attempting to provide a classical picture or explanation of quantum phenomena.

George Temple, in his 1934 book on quantum mechanics, derived the equations of motion for a charged fluid and then discussed Madelung's theory. He showed how, for certain cases, Madelung's transformation reduced the nonlinear hydrodynamical equations to the Schrödinger equation.[9] He observed that, to prevent radiation from these moving charges, the fluid flow must remain steady and this cannot occur under the influence of electromagnetic forces *alone*. Hence, there was need for the *quantum force* (or *quantum potential U*).[10] That is, Temple attempted to give some motivation for introducing the quantum potential in order to account for the lack of electromagnetic radiation in stationary states.

8.2 De Broglie

The similarity of the Schrödinger equation to classical hydrodynamical equations, as well as the analogy of the Hamilton-Jacobi formulation of classical mechanics to classical wave optics, led de Broglie to his own attempts at a largely classical formulation of quantum mechanics. He suggested two different approaches: the hypothesis of the double solution and the theory of the pilot wave. Neither succeeded and these failures made him "see better the necessity for adopting entirely new ideas which were developed during the course of the same year by Bohr and Heisenberg."[11]

Before I sketch some of the details of de Broglie's attempts, let me indicate the significance of this quotation. The original (French) version of *Physics and Microphysics* (the source of this citation) was published in 1947. At that time, de Broglie was very much in the Copenhagen camp, having been converted (by 1930) after his bitter experience at the 1927 Solvay congress. David Bohm's 1952 paper was to have a profound ("reconversion") effect on de Broglie. However, in 1947, de Broglie still repudiated his former ideas. In a note to the 1955 translation of *Physics and Microphysics,* he acknowledged Bohm's paper (which he had seen only in preprint at the time of these comments) and essentially rejected it as just a version of his pilot-wave theory.[12] Shortly thereafter, de Broglie admitted that Bohm had successfully overcome the original objections to a pilot-wave model. I return to this part of the story later.

De Broglie's main goal was to unify the wave and particle dualism into a *single* coherent picture or model. His hope was to treat the "particle" as a (mathematical) singularity in the center of an extended wave. As outlined in 1927, this *extended,* continuous part of the wave would "sense" the environment (obstacles, slits, etc.) and thus vary the motion of the singularity accordingly. He wanted two related wave solutions, one for the singularity and the other for the continuous wave. This latter would account for the statistical behavior of a collection of particles. A classical conception of actually existing entities in a continuous space-time background was to underlie his theory or worldview.[13] De Broglie's solution to the wave-particle duality was a synthesis of wave *and* particle, versus the wave *or* particle of the (eventual) Copenhagen interpretation. For him, the (continuous, extended) wave aspect was to be represented by the function ψ and the singularity by a function u.

His basic motivation had been Planck's fundamental relation for light ($\varepsilon = h\nu$) which united a particle aspect (localized energy ε) and a wave aspect (the frequency ν).[14] In his Nobel Prize acceptance speech in 1929, de Broglie recalled that he had been dissatisfied with Planck's relation because it defined the energy of a light corpuscle by a relation that contains a frequency ν. De Broglie felt that a purely corpuscular theory should not contain a frequency. On the other hand, the stable motions of the electrons in the atom are characterized by whole numbers that are typically associated in physics with interference and standing waves. This suggested to him "that electrons themselves could not be represented as simple corpuscles either, but that a periodicity had also to be assigned to them, too."[15]

In early 1927 de Broglie attempted to generalize these intuitive arguments (cf. appendix 2 to this chapter) into his "principle of the double solution." To every continuous solution $\psi = Re^{i\phi/\hbar}$ of the wave equation there was to correspond a singularity solution $u = fe^{i\phi/\hbar}$ having the same phase ϕ as ψ, but whose amplitude f represented a moving singularity.[16] On the basis of this principle, he was able to show that the velocity v of the singularity in u (the "particle") was to be determined by the "guidance formula" ($v = \nabla\phi/m$).[17] It was this phase ϕ, rather than the amplitude of ψ, that determined the motion of the singularity representing the particle.[18] Only for the case of a *free* particle was de Broglie able to carry through these pilot-wave ideas explicitly.[19] He also suggested that $\rho = |\psi|^2$ represented the probability of finding a particle ("singularity") at a point in space.[20] The ψ-wave gave statistical information about the behavior of an ensemble of particles (or, equivalently, probabilistic information about the behavior of a single particle whose initial location—the "hidden variable"—is unknown or uncertain). At the end of his 1927 paper on this double-solution theory, de Broglie observed that one might simply postulate the existence of two distinct realities, particle and wave, with the motion of the particle determined by the phase of the wave. He considered such a move to be not really satisfactory and only provisional.[21] His pilot-wave theory is mathematically the same as Madelung's formalism.[22]

It should be evident that de Broglie's style of doing physics was an intuitive one in which general insights played the major role. He was not a formalist possessing great mathematical power. The existence and nature of the singular solution u to the wave equation for the general case of motion in the presence of a force field proved quite complex mathematically. In later years de Broglie himself suggested that a *nonlinear* equation may be required for u.[23] In the face of these severe mathematical difficulties, de Broglie decided, as an interim measure, to accept, or simply *postulate,* the existence of a particle accompanied by its phase wave ψ.[24] He presented this hybrid model, his pilot-wave theory, at the fifth Solvay congress in October of 1927. I have already indicated why Schrödinger was not enthusiastic about this theory and what Pauli's reaction to it was. Although I have also alluded to what I believe was a factor in Einstein's dismissing de Broglie's theory, I now consider that in more detail.

8.3 Einstein

A fascinating (unpublished) manuscript in the Einstein Archives is titled "Does Schrödinger's Wave Mechanics Determine the Motion of a System Completely or Only in the Sense of Statistics?"[25] In it, he tells us that to each solution of the wave equation there corresponds the motion of an *individual* system that is determined unambiguously and uniquely. In 1927 Einstein wrote to Born about this work and indicated that it would soon be published.[26]

Conceptually, but not in its mathematical details, this theory was very much in the spirit of Madelung's hydrodynamical model. In an addendum to this manuscript, Einstein mentioned that Walther Bothe had pointed out a difficultly with this scheme for compound systems whose overall state may be represented by a *single* product of the wave functions of each of the subsystems. Such a system is made up of *independent* (i.e., noninteracting) subsystems. It turns out that the motions of the compound system will not be simply combinations of motions for the subsystems, as Einstein required on physical grounds. This showed that Einstein's particular recipe for determining the "flow lines" was not tenable (not, of course, that there could not exist another one that would be tenable). Einstein suggested that it might be possible to overcome this difficulty, but nothing specific followed. The fact that this paper, originally presented orally at a meeting of the Prussian Academy in early 1927, was never published indicates that this "entanglement" problem remained grounds, for him, to reject this particular "classical" attempt at interpreting quantum mechanics.[27] Years later, Born commented that he himself could no longer "remember it now; like so many similar attempts by other authors, it has disappeared without trace."[28]

The general, at least conceptual, similarities among this attempt by Einstein, the Madelung hydrodynamical model, and the de Broglie pilot-wave theory probably account for Einstein's lack of interest in de Broglie's 1927 Solvay congress presentation. All three of these attempts suffered from apparently bizarre, physically unacceptable properties: Madelung's (of which Einstein surely already knew) had a peculiar "inner" force of

the continuum (cf. eqs. [8.14] and [8.15] in appendix 1 to this chapter), Einstein's own had the entanglement feature I have just discussed, and de Broglie's pilot-wave theory would be made to seem incoherent by Pauli's objection at the 1927 Solvay congress itself. Whether it was these strange features or the general nonlocality of quantum theory that left Einstein cool toward de Broglie's pilot-wave presentation is unclear.[29] We do know that by the spring of 1927 (*prior* to this Solvay conference) Einstein was already critical of quantum mechanics (in either the matrix or the wave formulation) because, among spatially separated systems, there existed correlations that seemed to violate a principle of action by contact.[30]

8.4 Kennard, Rosen, Fürth

In 1928, Earle Kennard published in *The Physical Review* a discussion of the formalism and application of Schrödinger's wave mechanics. His opening sentence had a sense of finality on the interpretation question.[31] Early in the paper Kennard cited Madelung's work and arrived at a "Newtonian" equation of motion and stated:[32]

> *Thus each element of the probability moves in the Cartesian space of each particle as that particle would move according to Newton's laws under the classical force plus a "quantum force"* given by the h-term in (Newton's second law of motion. eq. [8.14]).
>
> The motion here considered occurs in a space of *n* dimensions. We can also, however, replace the *n*-dimensional packet by *n* separate packets, one for each particle, all moving in the same ordinary space.[33]

He went on to point out that, in spite of this similarity with Newton's second law of motion, there is a profound difference since the motion of any one particle depends, in general, on the instantaneous location and velocities of all the other particles as well.[34] Here, again, is nonlocality.

I now step somewhat out of the time sequence of events and mention another example of a hidden-variables interpretation because it nicely focuses on those central features of such theories that most found objectionable on physical grounds. In a somewhat obscure journal, Nathan Rosen in 1945 published a paper whose purpose was to explore "the extent to which classical concepts can be carried over into the quantum theory and the conditions under which they conflict with the formalism of this theory."[35] He cited Madelung's and Kennard's papers and obtained the same equations of motion as they did. With regard to the dynamical equation of motion (Newton's second law, as modified by the quantum potential), Rosen observed "that the motion of each particle of the ensemble depends on the density with which all the members of the ensemble [of *possible* representatives of the *actual* particle under consideration] are distributed, so that effectively we have an interaction among the [virtual] particles."[36] The basic point that Rosen is making here is that the quantum potential U depends upon $\rho = |\psi|^2$, the density of particles in an ensemble in which only *one* particle (the *actual* one) is real and the rest are merely *possible*. This is worse than mere nonlocality among distant, actually existing particles and, interpreted as Rosen does, calls for a bizarre influence of possible systems on actual ones. This would scarcely help a realistic interpretation of the phenomena. On the other hand, Bohm's 1952 paper made a virtue of the quantum potential by interpreting it in terms of a nonlocal influence of the (actual) environment upon the (actual) particle under consideration—all very realistic.

There was also an early precursor to the Brownian-motion type of stochastic mechanics that I return to in the next chapter. Schrödinger noted the *formal* (mathematical) similarity between the diffusion equation and his own wave equation, but he saw the *physical* differences between these two cases as more significant than the formal resemblances.[37] In 1933, Reinhold Fürth, citing an earlier review by Schrödinger on the interpretation of quantum mechanics, discussed this formal analogy between the equations for the position probability of a classical statistical mechanical system and quantum mechanics.[38] Unsharp observables resulted and there were inherent (practical) limitations on the accuracy of measurements, from which "Heisenberg"-like inequalities followed. A relation was established between diffusion equations for real density functions and the Schrödinger equation for complex functions and this was related to reversible and irreversible natural processes. Fürth examined some specific cases and then argued/conjectured that in general an "uncertainty" relation $\Delta x \Delta v \approx D$ held, where D is the diffusion coefficient in the diffusion equation.[39] (In section 9.3.2 we shall see that in 1966 Edward Nelson obtained a similar result and made the identification $D = \hbar/2m$.)[40] Fürth also referred to previous literature on the inherent limitations that Brownian motion placed on the accuracy of measurements.[41] There was considerable discussion of such limitations in the years 1925 to 1935.[42]

Similar stochastic-mechanics approaches to quantum phenomena would recur several times after the Second World War.[43] In section 9.3.2 I discuss this program at some length. I mention these early attempts here to indicate not only that such an approach was *possible* just after the 1927 Solvay congress, but also that *in fact* it was considered. However, there soon appeared to be a reason to believe that all such attempts *must*, ultimately, fail.

8.5 Von Neumann's "proof"

We have seen how, by 1927–1928, the issue of the Copenhagen versus a causal interpretation had been essentially decided. The story has included both "external" factors and rational arguments that were taken at the time as convincing for choosing Copenhagen. Even by September of 1927, *prior* to the October Solvay congress, Bohr's Como lecture had to a large extent solidified the matrix of what would become the Copenhagen interpretation. This general acceptance of the Copenhagen interpretation effectively precluded any consideration of causal interpretations. With the appearance in 1932 of von Neumann's *Mathematical Foundations of Quantum Mechanics*, however, there appeared to be a logically irrefutable proof that any type of hidden-variables theory that gave *all* of the same predictions as standard quantum mechanics was *impossible*.

8.5.1 The impact of the theorem

In spite of the considerable mystique that has surrounded von Neumann's theorem in recent decades, that proof was probably *not* the decisive reason that hidden-variables theories were not actively pursued in the 1930s. Most people already *believed* (for reasons that, in retrospect, appear considerably less convincing now than they did at the time) that such extensions were ruled out on physical or experimental grounds. Although the proof was cited by proponents of the Copenhagen interpretation, that was usually done as a nod to mathematical purity or as a put-down "clincher," rather than as *the* central element in a refuting argument.[44] Of course, it is not possible to tell what effect that theorem *may* have had in diverting people from seriously pursuing hidden-variables theories.

Von Neumann's proof further confirmed de Broglie's position against his own causal theory.[45] While that may be, de Broglie had *already* been converted to Copenhagen. Similarly, in a letter of 2 July 1935 to Pauli on the recent EPR paper and Bohr's response to it, Heisenberg included a long addendum titled "Is a Deterministic Completion of Quantum Mechanics Possible?"[46] Although Heisenberg does refer approvingly to von Neumann's book there, he does not single out the "impossibility" proof specifically as a key element in his lengthy discussion of why such a completion is not possible.

In 1936 Jordan spoke out against causality in atomic phenomena, but as was characteristic of him, much more categorically than many of his colleagues. In his popular lectures on physics in this century, Jordan discussed the example of individual photons passing through a polarizer and then told his reader that a "denial of the classical concept of causality is not to be understood as a temporary imperfection of our knowledge, but is inherent in the nature of the thing—again showing how incorrect our previous, classical concepts were."[47] In his more technical treatise on quantum mechanics, Jordan handled this same example in virtually the same way.[48]

In a 1939 conference report in *New Theories in Physics,* Bohr referred to von Neumann's proof, but only as the most clear and elegant demonstration of the already well known fact "that the fundamental superposition principle of quantum mechanics logically excludes the possibility of avoiding the non-causal feature of the formalism by any conceivable introduction of additional variables."[49] While he reported on von Neumann's own discussion of the "impossibility" proof at the conference, Bohr also claimed that this result is already evident from more elementary considerations.[50]

As time passed, the theorem gained in importance. Born's Waynflete lectures at Oxford in 1948 drew up a case against a causal interpretation of quantum mechanics and von Neumann's result was cited as being one of the more important elements in the argument. Born first used historical precedent to argue against the likelihood of a reversion to a primitive conception, such as determinism, and offered his opinion that the attendant mathematical difficulties could not be overcome. He then cited von Neumann's *Mathematische Grundlagen der Quantenmechanik* and assured his audience "that the formalism of quantum mechanics is uniquely determined by [a few plausible] axioms; in particular, no concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one [so that] . . . if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different."[51]

The *limitations* of the theorem were rarely stressed. For instance, Pauli in 1948 referred to "von Neumann's well known proof that the consequences of quantum mechanics cannot be amended by additional statements on the distribution of values of observables, based on the fixing of values of some hidden parameters, without changing some consequences of the present quantum mechanics."[52] Even many years later, in a special 1958 issue of the *Bulletin of the American Mathematical Society* dedicated to the memory of John von Neumann, as reflective a theoretical physicist as Léon van Hove would state that "von Neumann could show that hidden parameters with this property [of reinstating causality] cannot exist if the basic structure of quantum theory is retained."[53] Van Hove does imply that von Neumann may have allowed the possibility of certain types of hidden-variables theories as long as the formalism of quantum mechanics was *modified.*[54]

As I now show, von Neumann was able to obtain his result only with

the assumption that one of ordinary quantum theory's rules for statistical ensembles could be *extended* to the dispersion-free ensembles of any hidden-variables theory. This assumption turns out to be unwarranted (although it was *many* years before John Bell pointed this out with great clarity in the mid-1960s).

8.5.2 *The actual theorem*

The hope central to the class of causal interpretations I consider is that there exists (at least *in principle*) a set of hidden variables (say, the actual microscopic coordinates of the particles) that, if known or controllable by the experimenter, would *completely* and *uniquely* determine the motion of individual microsystems. The results of experiments would be determined on an event-by-event basis. In practice, this hope would hold, these hidden variables are unknown and must be averaged over to obtain predictions for an ensemble of particles. I denote these assumed hidden variables collectively by the symbol λ (i.e., λ may stand for an entire set $\lambda_1, \lambda_2, \lambda_3, \ldots$).[55] There should then be subensembles such that the value of the observable in question should have a *definite* value for all members of the subensembles (i.e., the measured values of this observable should be *dispersion free*). Von Neumann's proof shows that there will necessarily be situations in which such dispersion-free states cannot exist, provided one is to maintain *all* of the predictions (for experimental outcomes) of the standard formalism of quantum mechanics.[56] This, it turns out, is really equivalent to the question of the *completeness* of the Copenhagen interpretation of quantum mechanics.

The problem as actually posed by von Neumann involves the possible decomposition of an ensemble *E* into subensembles.[57] He put this as follows:

> We could attempt to maintain the fiction that each dispersing ensemble can be divided into two (or more) parts, different from each other and from it, without a change of its elements. . . . This is the question: is it really possible to represent each ensemble $[E_1, E_2, \ldots, E_N]$, in which there is a quantity $\mathfrak{R}$ with dispersion, by the superposition of two (or more) ensembles different from one another and from it?[58]

After the details of his proof, he concluded that all ensembles have dispersions.[59] He told his reader that "the present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible."[60] Von Neumann denied the possibility of fine-grained versus coarse-grained knowledge being the source of the quantum-mechanical dispersions that would be produced in an averaging process.

Von Neumann was able to produce a mathematical contradiction only by assuming that, even for dispersion-free subensembles, the expectation value of the sum of two (noncommuting) operators is simply the sum of the expectation values of each separately (as *is* the case in quantum mechanics). This was widely accepted as establishing that general dispersion-free states cannot exist so that a hidden-variables extension of quantum mechanics is impossible *in principle*. With the aid of hindsight provided by later work, we can see today that what has actually been established is that the sum rule is inconsistent with a hidden-variables extension.[61] Another way to parse this is that such a hidden-variables extension requires a *more* complete specification of the state of a microsystem than that possible with the quantum-mechanical state vector ψ.[62]

While some philosophers seem to have been explicitly aware of this logical point (even in the 1930s), this caveat was not emphasized by physicists in most references to von Neumann's proof.[63] The sociological and psychological reasons for the largely uncritical acceptance by the physics community of the implications of von Neumann's proof have been studied in detail.[64] Von Neumann had tremendous intellectual prestige among scientists, including such leaders in quantum theory as Wigner.[65] He had been David Hilbert's favorite when, as a young man, von Neumann attempted an axiomatization of mathematics in the 1920s. That project, like his no-hidden-variables proof a decade or so later, nearly succeeded. It was shut down by Gödel's incompleteness theorem. These were both brilliant, but ultimately failed, undertakings. It was only with Bell's work in the mid-1960s that this proof's irrelevance for many types of hidden-variables theories became widely understood.[66]

In Bohm's theory, how a microsystem behaves depends upon its environment (i.e., an observed value is contextual). That makes it evident why a set of hidden variables of the microsystem *alone* could not fix definite values for outcomes of incompatible measurements. The quantum potentials (or, equivalently, the wave functions) for these different experimental arrangements would be different. This contextuality allows Bohm's theory to escape von Neumann's theorem.[67]

We see that von Neumann's theorem is fine as a mathematical theorem (i.e., as a correct exercise in deductive logic, *given* his axioms), but that it is simply irrelevant to a large class of hidden-variables theories (one of which is Bohm's). The theorem just does not imply everything it was taken to.

Appendix 1 Madelung's derivation

Madelung accepted Schrödinger's wave equation

$$-\frac{\hbar^2}{2m}\nabla^2\psi + V\psi = i\hbar\frac{\partial\psi}{\partial t} \tag{8.1}$$

as his starting point and made the substitution[68]

$$\psi = R\exp(i\,S/\hbar)\,. \tag{8.2}$$

If this is substituted into the Schrödinger equation and real and imaginary parts are separated, two equations result (just as, of course, happened in Bohm's theory in appendix 1 to chapter 4):

$$\frac{\partial R}{\partial t} = -\frac{1}{2m}[R\,\nabla^2 S + 2\nabla R\cdot\nabla S] \tag{8.3}$$

$$\frac{\partial S}{\partial t} = -\left[\frac{(\nabla S)^2}{2m} + V - \frac{\hbar^2}{2m}\frac{\nabla^2 R}{R}\right]. \tag{8.4}$$

If eq. (8.3) is multiplied by R, the result can readily be rewritten as

$$\frac{\partial}{\partial t}(R^2) = -\frac{1}{m}\nabla\cdot(R^2\,\nabla S)\,. \tag{8.5}$$

If we identify

$$\rho = R^2 \tag{8.6}$$

as a fluid density and the phase

$$\phi = S \tag{8.7}$$

as a velocity potential so that the velocity field v of this fluid is given as[69]

$$v = \frac{1}{m}\nabla\phi = \frac{1}{m}\nabla S\,, \tag{8.8}$$

then eq. (8.5) becomes the continuity equation

$$\nabla\cdot(\rho v) + \frac{\partial\rho}{\partial t} = 0 \tag{8.9}$$

expressing the conservation of mass of the fluid. So, eq. (8.3) is basically the continuity equation. The other separation equation, eq. (8.4), becomes, with the use of eq. (8.8),

$$\frac{\partial\phi}{\partial t} + \frac{1}{2m}(\nabla\phi)^2 = -V + \frac{\hbar^2}{2m}\frac{\nabla^2 R}{R}. \qquad (8.10)$$

If we take the gradient of eq. (8.10), divide by m and use eq. (8.8), we obtain from the left-hand side of eq. (8.10)

$$\frac{\partial v}{\partial t} + \frac{1}{2}\nabla(v^2) \equiv \frac{dv}{dt}. \qquad (8.11)$$

This is just the acceleration of a volume element of fluid as it moves along a flow line defined by $v(x, t)$ since, for *any* $f(x, t)$ along such a flow line [where $x = x(t)$],

$$\frac{df}{dt} = \sum_{j=1}^{3}\frac{\partial f}{\partial x_j}\frac{dx_j}{dt} + \frac{\partial f}{\partial t} = v\cdot\nabla f + \frac{\partial f}{\partial t}. \qquad (8.12)$$

If we use eq. (8.8), as $v_j = \partial\phi/\partial x_j$ and set $f = v_k$ in eq. (8.12), we find[70]

$$\begin{aligned}\frac{dv_k}{dt} &= \sum_{j=1}^{3} v_j\frac{\partial v_k}{\partial x_j} + \frac{\partial v_k}{\partial t} = \sum_{j=1}^{3} v_j\frac{\partial v_j}{\partial x_k} + \frac{\partial v_k}{\partial t}\\ &= \frac{1}{2}\frac{\partial v^2}{\partial x_k} + \frac{\partial v_k}{\partial t}.\end{aligned} \qquad (8.13)$$

Therefore, the gradient of eq. (8.10) (multiplied by $1/m$) is just

$$\frac{dv}{dt} = -\frac{1}{m}\nabla V + \nabla\left(\frac{\hbar^2}{2m^2}\frac{\nabla^2 R}{R}\right). \qquad (8.14)$$

This last "force" term can be written as $-\nabla U$, where

$$U \equiv -\frac{\hbar^2}{2m}\frac{\nabla^2 R}{R} \qquad (8.15)$$

(later called a "quantum potential" by de Broglie).[71]

Appendix 2 De Broglie's guidance argument

De Broglie's basic 1923 argument was the following.[72] Consider a particle of rest mass m_0 and denote its rest frame by S'.[73] In this frame let there be a periodic phenomenon associated with the particle and characterized by the frequency ν_0

$$E_0 \equiv m_0c^2 = h\nu_0 \qquad (8.16)$$

and represented by the "wave"

$$\psi_0 = A_0\exp(-2\pi i\nu_0 t_0). \qquad (8.17)$$

Notice that ψ_0 does *not* depend upon the spatial coordinates in frame S'. There is a frame S in which an observer (at rest) sees this particle moving with a velocity $v = \beta c$ along the positive x-axis. The corresponding phase wave ψ as seen by S is

$$\psi(x, t) = A_0\exp\left[-2\pi i\nu_0\gamma\left(t - \frac{\beta x}{c}\right)\right], \qquad (8.18)$$

since the time coordinate t_0 in S' is related to the space-time coordinates (x, t) in S via the Lorentz transformation

$$t_0 = \gamma\left(t - \frac{\beta}{c}x\right), \qquad (8.19)$$

$$\gamma = \frac{1}{\sqrt{1-\beta^2}}. \qquad (8.20)$$

Equation (8.18) represents (in frame S) a phase wave

$$\psi(x, t) = A_0\exp\left[-2\pi i\nu\left(t - \frac{x}{V}\right)\right] \equiv A_0e^{i\phi/\hbar}, \qquad (8.21)$$

$$\nu = \gamma\nu_0, \qquad (8.22)$$

$$V = \frac{c^2}{v}. \qquad (8.23)$$

Here ν is the frequency of this phase wave and V is its (phase) velocity.[74] The quantity $\phi/\hbar$ is the phase of ψ. The frequency ν is just what one would have expected from

$$h\nu = E = mc^2, \qquad (8.24)$$

$$m = \gamma m_0, \qquad (8.25)$$

which is the analogue of eq. (8.16) once the relativistic value for m in frame S is used (in place of m_0 for S'). For reference later note that since

$$\phi = 2\pi\hbar\nu\left(\frac{x}{V} - t\right), \qquad (8.26)$$

it follows that

$$\frac{1}{m}\frac{\partial\phi}{\partial x} = \frac{2\pi\hbar\nu}{mV} = \frac{h}{m}\frac{\gamma\nu_0}{V} = \frac{h\nu_0}{m_0}\frac{v}{c^2} = v, \qquad (8.27)$$

which is just the velocity of the particle. That is, the gradient of the *phase* of ψ gives the *velocity* of m. Equation (8.27) is the same as eq. (8.8).

Furthermore, de Broglie observed that the phase of the periodic phenomenon associated with the particle in frame S' remained everywhere the same as the phase of ψ in S. To see this consider a collection of "clocks" in S' each having a period τ_0 in that frame. One of these is to be associated with the particle of rest mass m_0. The frequency

$$\nu_0 = \frac{1}{\tau_0}, \tag{8.28}$$

originally defined in S', becomes for S

$$\nu_S \equiv \frac{1}{\tau} = \frac{\nu_0}{\gamma} \tag{8.29}$$

because of the time dilation effect

$$\tau = \gamma\, \tau_0 . \tag{8.30}$$

Now, de Broglie reasoned, if S follows the "particle" along for a time dt (and, as it travels, by definition, $dx = v\, dt$), the observed *change* of phase (for S) of this "intrinsic" phenomenon will be just

$$-\, 2\pi\hbar\nu_S dt = -\, 2\pi\hbar \frac{\nu_0}{\gamma}\, dt . \tag{8.31}$$

However, if $\phi_1 = \phi(x_1, 0)$ was the phase of ψ at the initial position x_1 of m at $t = 0$, then the phase of ψ at the new location of m a time dt later will be (cf. eq. [8.21])

$$\begin{aligned} \phi_2 = \phi(x_1 + dx, dt) &= \phi_1 + 2\pi\hbar\nu \left(\frac{\beta}{c}\, dx - dt\right) \\ &= \phi_1 - 2\pi\hbar\nu(1 - \beta^2)\, dt = \phi_1 - 2\pi\hbar\frac{\nu_0}{\gamma}\, dt . \end{aligned} \tag{8.32}$$

The "intrinsic" phase of the "particle" (of mass m) moving with velocity v in S remains *fixed* relative to the phase wave ψ moving with (phase) velocity V in S. In other words, the phases of the "particle" and of the wave ψ are *always* equal to each other at the location of the particle as it moves along its trajectory in frame S. Starting from such a congruence, one can also argue back to the (by now familiar) guidance condition[75]

$$v = \frac{1}{m} \nabla\phi . \tag{8.33}$$

Appendix 3 Einstein's 1927 hidden-variables theory

Einstein's basic idea was that the time-independent Schrödinger equation

$$\frac{\hbar^2}{2m} \nabla^2\psi + (E - V)\, \psi = 0 \tag{8.34}$$

can be used to find the kinetic energy $K = E - V$ for any given wave function solution ψ defined on an n-dimensional configuration space.[76] He used the quantum-mechanical expression for the kinetic energy

$$K = -\frac{\hbar^2}{2m} \frac{\nabla^2\psi}{\psi} \tag{8.35}$$

to define an equivalent kinetic energy in point-particle mechanics as

$$K = \frac{1}{2}\, m\, v^2 = \frac{1}{2}\, m\, g_{\mu\nu}\dot{q}_\mu \dot{q}_\nu , \tag{8.36}$$

where $g_{\mu\nu}$ is the metric tensor for the configuration space and $\dot{q}_\mu$ is the velocity component of the particle.[77] These $\dot{q}_\mu$ are functions of the configuration-space coordinates (that is, they define a velocity field, the tangents to which are the "flow lines" or possible particle trajectories). Specifically, having set

$$\nabla^2\psi = g^{\mu\nu}\psi_{\mu\nu} , \tag{8.37}$$

where $\psi_{\mu\nu}$ (which Einstein termed "the tensor of ψ-curvature") is the covariant derivative, he then sought a "unit" vector A^μ

$$g_{\mu\nu}A^\mu A^\nu = 1 \tag{8.38}$$

that would render

$$\psi_{\mu\nu}A^\mu A^\nu \equiv \psi_A \tag{8.39}$$

an extremum. This is the normal curvature of the differential geometry of surfaces.[78] A hermitian quadratic form like eq. (8.39) is rendered an extremum by those vectors A^μ that are the solution to the eigenvalue problem[79]

$$(\psi_{\mu\nu} - \lambda g_{\mu\nu})A^\nu = 0 . \tag{8.40}$$

In terms of these A^μ and their eigenvalues $\lambda_{(\alpha)}$, Einstein was able to give an expression for *uniquely* assigning the $\dot{q}_\mu$ in terms of a given ψ. (The details of the recipe need not concern us here.)[80]

The essence of Bothe's objection is that the (covariant) derivative $\psi_{\mu\nu}$ for such a product wave function $\psi = \psi_1\psi_2$ is not zero when μ is an index for referring to the first subsystem and ν one for the second subsystem. That is why the motions of the compound system will not be simply combinations of motions for the subsystems, as Einstein demanded that they be on physical grounds.[81]

Appendix 4 Von Neumann's unwarranted assumption

To illustrate what is at issue in von Neumann's theorem, let me consider some physical observable A for which there are possible measurement outcomes a_k, $k = 1, 2, 3, \ldots$. The idea is that for some particular value λ_0, every single observation (of a specified type) would with certainty yield a value $a(\lambda_0)$ (i.e., a definite and fixed one of the a_k). As usual, the average value $\langle A \rangle$ for a set of N observations is defined as

$$\langle A \rangle = \frac{1}{N} \sum_{j=1}^{N} a_j \,. \tag{8.41}$$

If the hidden variables λ are distributed according to some density function $\rho(\lambda)$, then the ensemble average is obtained as

$$\langle A \rangle = \int \rho\,(\lambda)\, \langle A \rangle_\lambda \, d\lambda = \int \rho\,(\lambda)\, a(\lambda)\, d\lambda \,. \tag{8.42}$$

It is these $\langle A \rangle$ that would have to agree with the predictions of standard quantum mechanics, while the $\langle A \rangle_\lambda$ might remain inaccessible in practice.

For λ restricted to the *fixed* λ_0, we would have $a_j = a(\lambda_0)$ for *all* j so that

$$\langle A \rangle_{\lambda_0} = \frac{1}{N} \sum_{j=1}^{N} a_j(\lambda_0) = a(\lambda_0) \,. \tag{8.43}$$

Similarly, $\langle A^2 \rangle_{\lambda_0}$ would have the value

$$\langle A^2 \rangle_{\lambda_0} \equiv \frac{1}{N} \sum_{j=1}^{N} a_j^2 = a^2\,(\lambda_0) \,. \tag{8.44}$$

Now the *dispersion* of A (here restricted to the subensemble λ_0) is defined as

$$\Delta A_{\lambda_0} \equiv \sqrt{\langle A^2 \rangle_{\lambda_0} - \langle A \rangle^2_{\lambda_0}} \tag{8.45}$$

and is generally taken as a measure of the speed or "scatter" of the individual values of a_j observed. However, if the system is in a *dispersion-free* state specified by the λ_0, then

$$\Delta A_{\lambda_0} = 0 \,. \tag{8.46}$$

Suppose we consider two noncommuting hermitian operators representing observables A and B and define another operator C as

$$C = A + B. \tag{8.47}$$

If λ_0 represents an in-principle possible dispersion-free state of an ensemble, then

$$\langle A \rangle = a(\lambda_0),\ \langle B \rangle = b(\lambda_0),\ \langle C \rangle = c(\lambda_0) \,. \tag{8.48}$$

If—and this is von Neumann's critical assumption—the same linearity rule for expectation values that holds in standard quantum mechanics

$$\langle C \rangle = \langle A \rangle + \langle B \rangle \tag{8.49}$$

can be extended to dispersion-free ensembles then, for this λ_0,[82]

$$c(\lambda_0) = a(\lambda_0) + b(\lambda_0) \,. \tag{8.50}$$

However, there are many simple counterexamples to eq. (8.50).[83] For instance, begin with the spin operator $\boldsymbol{\sigma}$

$$\boldsymbol{\sigma} = \sigma_1 \hat{\boldsymbol{i}} + \sigma_2 \hat{\boldsymbol{j}} + \sigma_3 \hat{\boldsymbol{k}} \,, \tag{8.51}$$

where $\hat{\boldsymbol{i}}$, $\hat{\boldsymbol{j}}$, and $\hat{\boldsymbol{k}}$ are unit vectors along the x-, y-, and z-axes respectively, and the σ_i are the 2×2 Pauli spin matrices. With the choices

$$A = \sigma_1,\ B = \sigma_2,\ C = \boldsymbol{\sigma}\cdot\boldsymbol{n} \,, \tag{8.52}$$

where $\boldsymbol{n} = (1, 1, 0)$, we have $C = A + B$. Since all of the Pauli matrices have eigenvalues $+1$ and -1 *only*, it follows, in this case, that $a(\lambda_0) = +1$ *or* -1, $b(\lambda_0) = +1$ *or* -1, and $c(\lambda_0) = +\sqrt{2}$ *or* $-\sqrt{2}$. Equation (8.50) would then require that

$$\pm\sqrt{2} = \pm 1 \pm 1 \,, \tag{8.53}$$

which is impossible for *any* choice of the signs in eq. (8.53).

Perhaps a detailed physical model that actually accomplishes what the theorem is often taken as forbidding will be helpful for the reader.[84] For the "spin" operator[85]

$$\hat{\sigma} \equiv \frac{1}{\sqrt{2}}\,(\sigma_x + \sigma_y) = \frac{1}{\sqrt{2}} \begin{pmatrix} 0 & 1-i \\ 1+i & 0 \end{pmatrix} \tag{8.54}$$

one easily verifies that the eigenvalues of $\hat{\sigma}$ are ± 1 and that the corresponding eigenvectors are

$$\chi_+ = \begin{pmatrix} \frac{1}{\sqrt{2}} \\ \frac{1+i}{2} \end{pmatrix}, \chi_- = \begin{pmatrix} \frac{1}{\sqrt{2}} \\ \frac{-1-i}{2} \end{pmatrix}. \quad (8.55)$$

Since direct calculation shows that

$$\langle\chi_+|\hat{\sigma}|\chi_+\rangle = 1\,, \quad (8.56a)$$

$$\langle\chi_+|\sigma_x|\chi_+\rangle = \frac{1}{\sqrt{2}} = \langle\chi_+|\sigma_y|\chi_+\rangle\,, \quad (8.56b)$$

we see that

$$\langle\hat{\sigma}\rangle_{\chi+} \equiv 1 = \frac{1}{\sqrt{2}}\left(\frac{1}{\sqrt{2}} + \frac{1}{\sqrt{2}}\right) = \frac{1}{\sqrt{2}}(\langle\sigma_x\rangle_{\chi+} + \langle\sigma_y\rangle_{\chi+}) \quad (8.57)$$

with a similar relation (except for an overall minus sign) holding for the state χ_-. This is an example in which three operators A, B, and C are such that

$$A = B + C,\ [B, C] \neq 0\,, \quad (8.58)$$

but still

$$\overline{\langle A\rangle} = \overline{\langle B\rangle} + \overline{\langle C\rangle}\,. \quad (8.59)$$

Here the averages are to be taken over an *entire* ensemble (not just over one "pure" subensemble).

But, *need* this be so for dispersion-free (hidden-variables) states? It is, of course, true in quantum mechanics that there cannot be a simultaneous complete set of eigenstates $\{\psi_i\}$ of noncommuting hermitian operators, since the conditions

$$A = A^\dagger,\ B = B^\dagger,\ A\psi_i = \alpha\psi_i,\ B\psi_i = \beta\psi_i \quad (8.60a)$$

on such a set are necessary and sufficient for

$$[A, B] = 0\,. \quad (8.60b)$$

However, one *can* give a more complete, dispersion-free state description for the example of eq. (8.54). Consider an "electron" moving in a straight line and let its spin variable be $\boldsymbol{\lambda}$ and the direction along which this spin will be measured be denoted by the vector $\boldsymbol{a}$ (where both $\boldsymbol{\lambda}$ and $\boldsymbol{a}$ lie in a plane perpendicular to the direction of motion). The *observed* value of the spin is to be assigned according to the rule

$$\text{sgn}\,(\boldsymbol{\lambda}\cdot\boldsymbol{a}) > 0 \Rightarrow +1 \quad (8.61a)$$

$$\text{sgn}\,(\boldsymbol{\lambda}\cdot\boldsymbol{a}) < 0 \Rightarrow -1 \quad (8.61b)$$

Then, *by construction*, the "observed" values in any *fixed* state $\boldsymbol{\lambda}$ of the subensemble of those $\boldsymbol{\lambda}$ lying between the x- and y-axes of figure 8.1 are[86]

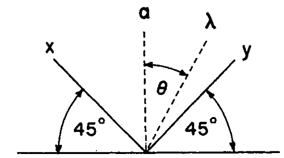

Figure 8.1 The "hidden variable" λ

$$\langle\hat{\sigma}\rangle_\lambda = 1,\ \langle\sigma_x\rangle_\lambda = 1,\ \langle\sigma_y\rangle_\lambda = 1\,,$$

so that

$$\langle\hat{\sigma}\rangle_\lambda \neq \frac{1}{\sqrt{2}}(\langle\sigma_x\rangle_\lambda + \langle\sigma_y\rangle_\lambda)\,. \quad (8.62)$$

Therefore, these dispersion-free states do *not* satisfy the sum rule. It is also evident that

$$\overline{\langle\hat{\sigma}\rangle}\ \text{(averaged over } \textit{all}\ \boldsymbol{\lambda} \text{ in the upper half plane)} = +1. \quad (8.63)$$

Now assume that the "hidden variable" $\boldsymbol{\lambda}$ is distributed over the *half* plane above the horizontal axis of figure 8.1 according to the "probability" $P(\boldsymbol{\lambda})$[87]

$$P\,(\boldsymbol{\lambda}) = \frac{1}{2}\cos\,(\theta)\,. \quad (8.64)$$

Then direct calculation yields

$$\overline{\langle\sigma_x\rangle} = \frac{1}{2}\int_{-\pi/2}^{\pi/4} \cos\,(\theta)\,d\theta - \frac{1}{2}\int_{\pi/4}^{\pi/2} \cos\,(\theta)\,d\theta = \frac{1}{\sqrt{2}} = \overline{\langle\sigma_y\rangle}\,, \quad (8.65)$$

so that

$$\overline{\langle\hat{\sigma}\rangle} = \frac{1}{\sqrt{2}}(\overline{\langle\sigma_x\rangle} + \overline{\langle\sigma_y\rangle})\,. \quad (8.66)$$

This *local* hidden-variable model reproduces the *statistical* predictions of quantum mechanics.