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Series B: Physics

Sixty-Two Years of Uncertainty

Historical, Philosophical, and Physical Inquiries into the Foundations of Quantum Mechanics

Edited by

Arthur I. Miller

Cambridge University
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"NICHT SEIN KANN WAS NICHT SEIN DARF," OR THE PREHISTORY OF EPR, 1909-1935:

EINSTEIN'S EARLY WORRIES ABOUT THE QUANTUM MECHANICS OF COMPOSITE SYSTEMS*

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1. INTRODUCTION

The story of Einstein's misgivings about quantum mechanics and about his debate with Bohr has been told many times--by the participants themselves,[1] by their colleagues and contemporaries,[2] and by historians and philosophers of science of later generations.[3] So the question arises: Why tell the story yet again? The answer is that there is more to be said. I will argue that the standard histories have overlooked what was from early on the principal reason for Einstein's reservations about quantum mechanics, namely, the non-separability of the quantum mechanical account of interactions, something ultimately unacceptable to Einstein because it could not be reconciled with the field-theoretic manner of describing interactions.[4] Showing the significance of this issue for Einstein is important not only for the sake of setting right the historical record, but also because it makes Einstein's critique of quantum mechanics far more interesting--from the point of view of the physics involved--than if we see it resting merely on a stubborn old man's nostalgic attachment to classical determinism.

*The quote used in the title is taken from a letter of Wolfgang Pauli to Werner Heisenberg, 15 June 1935 (Pauli 1985, p. 402), in which Pauli takes issue with the EPR argument. Pauli himself took the quote from a poem by Christian Morgenstern, "Die unmögliche Tatsache," reprinted in the collection, "Alle Galgenlieder" (Berlin, 1932), p. 163.

[1]See Bohr 1949 and Einstein 1946.

[2]See, for example, Ehrenfest to Goudsmit, Uhlenbeck, and Dieke, 3 November 1927 (quoted in Bohr 1985, p. 38); see also Rosenfeld 1967.

[3]The accounts by Harvey Brown (1981), Arthur Fine (1979), Clifford Hooker (1972), Max Jammer (1974, 1985), Abraham Pais (1982), and John Stachel (1986) are those most highly to be recommended. Though he is not a historian, Bernard d'Espagnat has written insightfully about the Bohr-Einstein controversy, displaying an especially good understanding of the technical issues involved in Einstein's critique of the quantum theory and his dispute with Bohr; see d'Espagnat 1976, 1981.

[4]To my knowledge, Fine (1986) is the only author who has so far hinted at the importance of this worry in Einstein's thinking about quantum mechanics prior to 1935.

Sixty-Two Years of Uncertainty
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According to the standard accounts, Einstein's critique of the quantum theory first took the form of doubts about its correctness. More specifically, he is supposed to have sought through a series of thought experiments to exhibit violations of the Heisenberg uncertainty relations. Contemporary witnesses and later commentators describe dramatic encounters between Einstein and Bohr at the 1927 and 1930 Solvay meetings, where, one by one, Bohr found the flaws in Einstein arguments, culminating in his stunning refutation of Einstein's "photon box" experiment, a refutation that turned, ironically, upon Bohr's showing how a relativistic correction overlooked by Einstein saves the day for the uncertainty relations. In this version of history, it was only after Bohr had beaten down these attacks on the correctness of the quantum theory that Einstein reformulated his critique in terms of doubts about the theory's completeness, the mature version of this latter critique being found in the 1935 Einstein-Podolsky-Rosen (EPR) paper.

There is, of course, some truth to the standard history, even though it was written by the victors, for Einstein did at one time have doubts about the uncertainty relations. But it is far from being the whole story, and in many crucial ways it is just plain wrong. It is not true that Einstein began to doubt the theory's completeness only after Bohr had parried his attempts to prove it incorrect. Einstein expressed public worries about incompleteness as early as the spring of 1927, and there are hints of such worries earlier still. But more importantly, from a very early date, at least 1925, Einstein was pondering the curious failure of classical assumptions abouut the independence of interacting systems made vivid in the new Bose-Einstein statistics. Earlier still, certainly by 1909, Einstein had recognized that the Planck formula for black-body radiation cannot be derived if one assumes that light quanta behave like the independent molecules in the gases described by classical statistical mechanics. And by spring 1927, Einstein had recognized that quantum mechanics (or at least Schrödinger's wave mechanics) fails to satisfy the kind of separability principle that he regarded as a necessary condition on any adequate physical theory, a condition clearly satisfied by field theories like general relativity.

Einstein did worry as well about the failure of determinism, about the peculiar consequences of indeterminacy, and about the curious nature and role of measurement in quantum mechanics. But these were not, for Einstein, fundamental problems. They were, instead, symptoms corollary to the one basic problem of the quantum mechanical denial of the independence of interacting systems. And the main purpose of the famous series of thought experiments devised by Einstein, at least by the time of the 1930 photon-box thought experiment, was to show that the non-separable quantum theory necessarily yields an incomplete description of physical events if one seeks to apply it to systems assumed to satisfy a strict separability principle.

There is obvious irony in the circumstance that Einstein could not accept the non-separability of the quantum theory, because quantum non-separability is the almost inevitable issue of a line of development initiated by Einstein's recognition that the Planck formula cannot be derived from the assumption of mutually independent light quanta and furthered essentially by Einstein's elaboration in 1924-1925 of Bose-Einstein statistics, where the necessary denial of the independence of interacting systems emerges with special clarity. The history of quantum mechanics up to 1926, which is often described as a search for a way consistently to marry the wave and particle aspects of light quanta and material particles, is, I think, better described as a search for a mathematically consistent and empirically correct way of denying the mutual independence of interacting quantum systems. Particles are naturally imagined as satisfying the separability principle, and hence as being mutually independent. So too the waves familiar to us from hydrodynamics, acoustics, and electrodynamics, but not the kind of "waves" that interfere in the manner necessary to generate the right quantum

statistics, the "waves" that Schrödinger discovered must be located in configuration space, "waves" whose chief virtue is that the "wave" function for a joint system need not be decomposible into separate "wave" functions for the component systems. Einstein opened the line of research that led to Schrödinger's "wave" mechanics, but he could not accept the conclusion, for it was incompatible with his own deep commitment to the separable manner of describing interactions implicit in field theories like general relativity.

The first hints that something is seriously wrong with the standard histories of Einstein's critique of quantum mechanics emerged from a reexamination of the EPR argument initiated by Arthur Fine and since pursued by myself and others. This re-examination revealed that Einstein did not write the EPR paper, did not like the argument it contained, and from the summer of 1935 on espoused a rather different argument for incompleteness, one that turns crucially upon the just-mentioned, characteristically field-theoretic assumption about the independence of interacting systems, the assumption Einstein himself here dubs the "Trennungsprinzip" [separation principle].

Elsewhere I have written at length about Einstein's real argument for the incompleteness of quantum mechanics, about some of the systematic questions raised by the problem of the compatibility of quantum mechanics and field theory, and about Einstein's views on this question after the appearance of the EPR paper in 1935. Here I want to fill in the story for the period before the EPR paper. I am quite deliberate in seeking to do so with the benefit of hindsight, that is to say that, knowing how central the issue of the separability or independence of interacting systems became in Einstein's later discussions of quantum mechanics, I use that insight as a heuristic in trying to understand his earlier struggles with the problem, my working hypothesis being that the worry was similar from early to late.

In what follows, I will first review briefly what I have elsewhere written about Einstein's post-EPR critique of the quantum theory. Then I will turn to a careful retelling of the story of Einstein's worries about quantum mechanics from 1905 to 1935. I will start with Einstein's tantalizing remarks about the failure of separability at the time of his papers on Bose-Einstein statistics. I will then explore the background to these remarks in his earliest papers on the quantum hypothesis, from 1905 to 1909. Returning to the 1920s, I will outline Einstein's growing misgivings about the new quantum mechanics from 1925 to 1927, culminating in his first explicit criticism of the failure of separability in wave mechanics in the spring of 1927. The paper concludes with a review of the history of Einstein's famous Gedankenexperimente critical of quantum mechanics, my aim being to show that from the start his principal goal was to demonstrate how a non-separable quantum mechanics is necessarily incomplete when applied to systems assumed to be separable.

2. EINSTEIN ON LOCALITY AND SEPARABILITY AFTER EPR

The Einstein-Podolsky-Rosen (1935) paper is still commonly taken to represent the definitive statement of Einstein's mature misgivings about the quantum theory. In brief, the argument found there is this. First, a completeness condition is asserted as a necessary condition that must be satisfied by any acceptable scientific theory: "every element of the physical reality must have a counterpart in the physical theory" (EPR 1935, p. 777). Then a sufficient condition for the existence of elements of physical reality (the famous EPR reality criterion) is laid down: "If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity" (Einstein, Podolsky, and Rosen 1935, p. 777). And then, finally, by means of a rather complicated argument, it is shown that in an EPR-type thought experiment involving previously interacting systems, elements of physical reality exist corresponding to both of two conjugate parameters for one of the two interacting systems, since the value of either could have been predicted with certainty and without physically disturbing the system on the basis of measurements carried out on the other system. But quantum mechanics holds that conjugate parameters, like position and linear momentum along a common axis, cannot have simultaneously definite values. Quantum mechanics is, thus, incomplete, since it fails to satisfy the completeness condition.

That is the standard account of Einstein's incompleteness argument. But that account is seriously wrong. Einstein did think quantum mechanics incomplete, but for reasons significantly different from those advanced in the EPR paper. He repudiated the EPR argument within weeks of its publication; and from 1935 on, all of his discussions of incompleteness take a quite different form from that found in the EPR paper. He continued to be concerned with the peculiar way in which quantum mechanics describes interacting systems; but he never invoked the EPR completeness condition, he never invoked the reality criterion, and he never invoked the uncertainty relations. Moreover, what he does say makes far clearer than the EPR paper the connection between his critique of quantum mechanics, on the one hand, and his commitments to field theories and realism, on the other.

Einstein's own incompleteness argument first appears in correspondence with Erwin Schrödinger in June of 1935, barely one month after the publication of the EPR paper; it was repeated and refined in a series of papers and other writings between 1936 and 1949.[5] In outline, it is this. A complete theory assigns one and only one theoretical state to each real state of a physical system.[6] But in EPR-type experiments involving spatio-temporally separated, but previously interacting systems, A and B, quantum mechanics assigns different theoretical states, different "psi-functions," to one and the same real state of A, say, depending upon the kind of measurement we choose to carry out on B. Hence quantum mechanics is incomplete.

The crucial step in the argument involves the proof that system A possesses one and only one real state. This is held to follow from the conjunction of two principles that I (not Einstein himself) call the locality and separability principles. Separability says that spatio-temporally separated systems possess well-defined real states, such that the joint state of the composite system is wholly determined by these two separate states. Locality says that such a real state is unaffected by events in regions of space-time separated from it by a spacelike interval.[7] Einstein argues that both principles apply to the separated systems in the EPR-type experiment (if they are allowed to separate sufficiently before we perform a measurement on B). It follows that system A has its own well-defined real state from the moment the interaction between A and B ceases, and that this real state is unaffected by anything we do in the vicinity of B. But quantum mechanics, again, assigns different states to A depending upon the parameter

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[5]The principal published texts are Einstein 1936, 1946, 1948, and 1949; another important source is Born 1969. For detailed references, see Howard 1985 or 1989.

[6]This is a curious conception of completeness, more akin to what is called in formal semantics "categoricity." For more on the background to the concept of the categoricity or "Eindeutigkeit" of theories in Einstein's work prior to the development of general relativity in 1915, see Howard 1988. A future paper will explore the issue in the years 1915 to 1935.

[7]What Einstein calls the "Trennungsprinzip" in his 1935 correspondence with Schrödinger combines both separability and locality. Einstein does not himself make the distinction clearly until 1946; see Howard 1985.

chosen for measurement on B. Thus, Einstein claims that the incompleteness of quantum mechanics--in the special sense of its assigning different theoretical states to one and the same real state--follows inevitably if we insist upon the principles of locality and separability.

Understanding that this was Einstein's real incompleteness argument is crucial to reconstructing the pre-history of the EPR experiment, and this for two reasons. First, because I want to argue that as early as 1927 and in virtually all of his later thought experiments critical of the quantum theory prior to 1935, it was the problem of non-separability that Einstein was really trying to articulate. And, second, because once we see that this was the real issue, we understand at last why Einstein's commitment to the program of field theories forced him to repudiate quantum mechanics. For as Einstein himself later explained, both locality and separability, but especially the latter, are built into the ontological foundations of field theories. The argument is simple. In a field theory, the fundamental ontology, the reality assumed by the theory, consists of the points of the space-time manifold and fundamental field structures, such as the metric and stress-energy tensors, assumed to be well defined at each point of the manifold.[8] Implicitly, therefore, any field theory assumes (i) that each point of the manifold, and by extension any region of the manifold, possesses its own real state, say that represented by the metric tensor, and (ii) that all interactions are to be described in terms of changes in these separate real states, which is to say that joint states are exhaustively determined by combinations of the relevant separate states, just as the separability principle demands. If this is correct (and I think it is), and if the quantum mechanical account of interactions denies separability, then there can be no reconciliation of the two. Moreover, Einstein had not inconsiderable (if not ultimately compelling) arguments--methodological, epistemological, and metaphysical--for retaining both locality and separability, which helps to explain his dogged commitment to the field theory program as an alternative to quantum mechanics.

For what follows, the point about the explanation of interactions in accordance with the separabilty principle bears elaboration. In one sense, two interacting systems even under a classical description are not independent of one another, since various correlations (if only momentum and energy conservation) are called into being by the interaction. But if the two systems are separable, always possessing well-defined separate states that exhaustively determine any joint properties--as is the case in classical mechanics, electrodynamics, and general relativity--then they are independent in the sense that each possesses its own separate "reality," if you will. And this independence manifests itself in the fact that all of the correlations between them can be explained in terms of their separate states. In the interesting case of statistical correlations of the kind to be considered below, this means that all joint probabilities for measurement outcomes, given the joint state of the two systems, always factorize as the product of separate probabilities for the individual measurement outcomes on the two systems, given, for each system, its own separate state.[9] The non-separability of the quantum mechanical account of interactions manifests itself precisely in the fact that joint probabilities do not thus factorize.

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[8]It is important to note, however, that on Einstein's understanding of a field-theoretic ontology (at least that of general relativity), the points are not given independently of the structures defined upon them. The legacy of his wrangling with the "hole argument" ("Lochbetractung") was his regarding the points of the manifold as being only implicitly defined as the intersections of world lines. For details, see Stachel 1989.

[9]For more detail, see Howard 1989, pp. 239-241.

3. BOSE-EINSTEIN STATISTICS AND THE BOHR-KRAMERS-SLATER THEORY: 1924-1925

The full story of Einstein's struggle with the quantum goes back to 1900, when, as a student, he first read Planck's papers on irreversible radiation processes and began to think about the manner in which light and matter interact. And it was in 1909 that Einstein first asserted in print that the quantum hypothesis is incompatible with classical assumptions about the independence of interacting systems. But I want to start with what was happening at the beginning of 1925, when Einstein for all intents and purposes ceased contributing to the development of the quantum theory, and took on the role of the theory's chief critic.

A few months earlier, in June of 1924, Einstein received from the Bengali physicist Satyendra Nath Bose a letter and an accompanying manuscript with a strikingly new derivation of the Planck radiation law. What was novel in Bose's derivation--Einstein called it "an important advance" (Einstein 1924a, p. 181)--was that it made no explicit use of the wave-theoretical arguments until then standard, proceeding instead on the assumption that a volume filled with light quanta can be treated by methods standard in the kinetic theory of gases, except that a new kind of statistics is required, statistics fundamentally different from classical Boltzmann statistics. Einstein was so impressed that he translated Bose's paper himself and arranged for its publication in the *Zeitschrift für Physik*. Bose's approach made it possible for the first time to understand how, in calculating the probabilities, W, that enter the Boltzmann equation, $S = k \cdot \log(W)$, the quantum approach makes different assumptions about equiprobable cases than are made classically. Not that all of this was immediately apparent. For Einstein wrote to Ehrenfest on 12 July about Bose's paper: "Derivation elegant, but essence remains obscure" (EA 10-089). But the essence was soon to become clearer when Einstein applied Bose's idea not to a photon gas, but to a quantum gas of material particles.

Einstein went on to write three papers on the subject; they represent his last great substantive contribution to quantum mechanics. What is not now realized is that what they showed him about quantum mechanics may have forever dulled his enthusiasm for the topic. The first of these papers was presented to the Prussian Academy on 10 July 1924 (Einstein 1924b), the second, containing the prediction of the low-temperature phase transition since known as "Bose-Einstein condensation," was presented on 8 January 1925 (Einstein 1925a), and the third on 29 January (Einstein 1925b). The significance of all three is limited, for spin was not yet clearly understood, the exclusion principle had yet to be articulated by Pauli, and it would take two more years before the respective roles of Fermi-Dirac and Bose-Einstein statistics were clearly distinguished. But such limitations are not immediately relevant to the story of Einstein's doubts about the quantum theory.

What is relevant is a question raised by Ehrenfest. Section §7 of the second paper is titled: "Comparison of the Gas Theory Developed Here with That Which Follows from the Hypothesis of the Mutual Statistical Independence of the Gas Molecules." It begins thus:

> Bose's theory of radiation and my analogous theory of ideal gases have been reproved by Mr. Ehrenfest and other colleagues because in these theories the quanta or molecules are not treated as structures statistically independent of one another, without this circumstance being especially pointed out in our papers. This is entirely correct. If one treats the quanta as being statistically independent of one another in their localization, then one obtains the Wien radiation law; if one treats the gas molecules analogously, then one obtains the classical equation of state for ideal gases, even if one otherwise proceeds exactly as Bose and I have. (Einstein 1925a, p. 5)

After showing how, following Bose's method, one counts the number of "complexions" corresponding to a given macrostate, that is to say how one distributes particles over the cells of phase space, Einstein adds:

> It is easy to see that, according to this way of calculating, the distribution of molecules among the cells is not treated as a statistically independent one. This is connected with the fact that the cases that are here called "complexions" would not be regarded as cases of equal probability according to the hypothesis of the independent distribution of the individual molecules among the cells. Assigning different probability to these "complexions" would not then give the entropy correctly in the case of an actual statistical independence of the molecules. Thus, the formula [for the entropy] indirectly expresses a certain hypothesis about a mutual influence of the molecules--for the time being of a quite mysterious kind--which determines precisely the equal statistical probability of the cases here defined as "complexions." (Einstein 1925a, p. 6)

Exactly what Einstein meant by his comment about the connection between the failure of statistical independence and "a quite mysterious kind" of "mutual influence" of one molecule upon another is spelled out in a letter to Schrödinger of 28 February 1925 (evidently written before Schrödinger had seen Einstein's second gas theory paper):

> In the Bose statistics employed by me, the quanta or molecules are not treated as being *independent of one another*. . . . A complexion is characterized through giving the number of molecules that are present in each individual cell. The number of the complexions so defined should determine the entropy. According to this procedure, the molecules do not appear as being localized independently of one another, but rather they have a preference to sit together with another molecule in the same cell. One can easily picture this in the case of small numbers. [In particular] 2 quanta, 2 cells:

Bose-statistics

	1st cell	2nd cell
1st case	• •	—
2nd case	•	•
3rd case	—	• •

independent molecules

	1st cell	2nd cell
1st case	I II	–
2nd case	I	II
3rd case	II	I
4th case	–	I II

> According to Bose the molecules stack together relatively more often than according to the hypothesis of the statistical independence of the molecules. (EA 22-002)

And in a P.S., Einstein adds that the new statistics are really not in conflict with those employed in his 1916 papers on transition probabilities, where the standard Maxwell-Boltzmann distribution was employed (Einstein 1916a, 1916b), because it is really only in relatively dense gases where the difference between the statistics of independent particles and the Bose-Einstein statistics will be noticeable: "There the interaction between the molecules makes itself felt,– the interaction which, for the present, is accounted for statistically, but whose physical nature remains veiled."

In many modern textbooks and histories of the subject, the principal innovation embodied in Bose-Einstein statistics is described in terms at first glance quite different from those we have just found Einstein using. The new statistics are said to be those appropriate to "identical" or "indistinguishable" particles. What is meant is clear. In the two-particle, two-cell case cited by Einstein we cannot tell which of the two particles is which, that is to say, we cannot keep track of their individual identities, as we can in classical Boltzmann statistics; hence, cases two and three in the classical statistics must be regarded as just one case (case two) in Bose-Einstein statistics, weighted equally with the other two remaining cases. But the "identical particles" vocabulary is misleading, for in the important case two in Bose-Einstein statistics, the two particles are by no means identical: they occupy different cells of phase space and so differ in position or momentum. They are arguably identical in cases one and three, since they occupy the same cell. But these cases have their counterparts in the Boltzmann statistics. The interesting difference appears in just those cases where the particles are not identical. What is important is the fact that we cannot track the individual identities of Bose-Einstein particles. We cannot say, as we could classically, "Here is particle A" at time t_0, and "Here is particle A," at some later time, t_1; the particle observed at t_1 might just as well be particle B. Classically, we can track individual identities, which possibility leads to Boltzmann statistics. (Notice how Einstein uses numerical labels, I and II, to suggest the separate indentifiability of the classical particles, representing the Bose-Einstein particles by unlabeled dots.) It is equally misleading to speak here of "indistinguishable" particles. For even in Bose-Einstein statistics we know that in case two there are different particles, we just cannot tell which is which.

Another common way of characterizing the novelty of Bose-Einstein statistics is to say that such statistics are appropriate for material particles evincing the wave-like aspect shortly before suggested in de Broglie's dissertation (1924). As we shall see, it is wrong to credit the idea of material particles possessing simultaneously a wave-like aspect wholly to de Broglie, since Einstein was well-known even at the time to have toyed with such ideas since at least 1921, motivated by considerations of symmetry and unity--if massless photons have a dual nature as both waves and particles, then massive particles should as well. But otherwise this charactization of the innovation represented by Bose-Einstein statistics is not incorrect, inasmuch as the novel way of counting complexions in Bose-Einstein statistics can be regarded as necessitated by the possibility of interference between the particles (the particles interfere precisely because we cannot tell which is which), such interference being perhaps most easily visualized with wave-theoretical models. Einstein himself pointed to this way of conceiving Bose-Einstein statistics in his second gas theory paper (Einstein 1925a, pp. 9-10); and in an important preliminary to his own development of wave mechanics, Schrödinger later elaborated this suggestion in an attempt to find a plausible wave-theoretical physical interpretation of the statistics (Schrödinger 1926a). Still, it is striking that Einstein himself did not emphasize this way of viewing the new statistics. He preferred to emphasize the fact that the particles are not treated as statistically independent systems and that such a failure of statistical independence is a symptom of a physically mysterious interaction between the particles.

Why did Einstein prefer this way of characterizing what was novel in his new statistics? Of course he understood the connection between his work and deBroglie's ideas, a connection equally obvious to most of his contemporaries. What point was he trying to make by stressing instead the failure of statistical independence and the existence of mysterious interactions? Might his way of characterizing the situation even tell us something about his understanding of the significance of wave-theoretical models?

An important clue to Einstein's thinking is provided in a talk entitled "On the Ether" that Einstein gave to the Schweizerische Naturforschende Gesellschaft in September 1924, after he had received and assimilated Bose's paper. At the end of his talk he turned to Bose's work. After explaining that Bose had replaced the customary wave-theoretical derivations of the Planck radiation law with a derivation employing the methods of statistical mechanics, Einstein remarked: "Then the question obtrudes whether or not diffraction and interference phenomena can just be connected to the quantum theory in such a way that the field-like concepts of the theory merely represent expressions of the interactions between quanta, in which case the field would no longer be ascribed any independent physical reality" (Einstein 1924c, p. 93). What is interesting here, aside from Einstein's scepticism regarding the reality of matter waves (and even the wave nature of photons!), is his suggestion that the effects commonly regarded as symptoms of a system's having a wave-like nature, that is, diffraction and interference, are really better understood as reflecting interactions between quanta. Thus, where others see waves, Einstein sees evidence of the physically mysterious interactions between quantum systems that he believed underlie classically unexpected statistical correlations between such systems. For Einstein, it is quanta, both light quanta and material particles, together with their curious interactions, that are real. The device of wave-theoretical representations is merely an artifice, a convenient tool, a vivid image, for helping us to think clearly about quantum interactions and statistical correlations.

One additional idea that will later loom large for Einstein had not yet come to the fore in his remarks about Bose-Einstein statistics, which is that the kinds of statistical dependence evinced in Bose-Einstein statistics can obtain even between spacelike separated systems or events. But there is other evidence that this problem too was already on Einstein's mind, as the concluding paragraph of the just-quoted talk indicates. For in a seemingly abrupt shift, Einstein turns back to the main topic of the talk, the ether, by which he meant the space-time manifold plus metric, remarking that even if the quantum theory develops into a real theory, "we will not be able to dispense with the ether in theoretical physics, that is, with the continuum endowed with physical properties; for the general theory of relativity, to whose fundamental aspects physicists will indeed always cling, excludes an immediate distant action, but every local-action theory assumes continuous fields, and thus the existence of an 'ether'" (Einstein 1924c, p. 93).

Recall how a continuous field theory like general relativity incorporates the principle of local action. In effect, such a theory treats every point in the field, every point of the space-time manifold in the case of general relativity, as a separable, independent system, possessing its own physical state represented by the fundamental field parameter, which would be the metric tensor in general relativity. Within this framework, action is explained in terms of a change in the fundamental parameter being propagated from point to point across the field, which is to say that the value of the fundamental parameter at any point is always wholly determined by the field equations and by the values of that parameter at all immediately adjacent points. What is not allowed is for the value of the fundamental parameter at one point to be immediately functionally dependent upon values at distant points. It is the restriction to local action so conceived that Einstein had in mind when he said that all "local-action theories" assume continuous fields. General relativity, through its incorporation of the first-signal principle, is even more restrictive in this regard than classical field theories, like Maxwellian electrodynamics, that impose no upper bound on signal velocities. For in general relativity, even the admissible varieties of local action are constrained to occur only between points of the manifold that are timelike separated.

It is important to keep in mind Einstein's basic commitment to the separable field-theoretic ontology and its associated locality constraints, because it helps to understand why the Bose-Einstein statistics would appear puzzling to Einstein. For the field-theoretic way of explaining interactions requires us to assign separate states to spatially separated systems. These states would determine separately the probabilities for each system's behavior, and it would follow that joint probabilities would have to be determined wholly by these separate probabilities, which is to say that the joint probabilities would have to factorize. But that does not happen in Bose-Einstein statistics, which is why Einstein found them so mysterious.

Einstein's gas theory papers were not the first investigations to make acute various questions about the statistical correlations that obtain between interacting systems. In fact, Einstein had been worrying about the general problem of probability relations between interacting systems for a long time. Such concerns had most recently come to the fore in his reaction to the Bohr-Kramers-Slater (BKS) theory (Bohr, Kramers, and Slater 1924). The English version of the BKS paper appeared in April 1924, the German version on 22 May. We remember it today for its use of virtual fields determining the probabilities of individual atomic emissions (and absorptions), and for its suggestion that, in consequence of the merely probabilistic determination of transition events, energy and momentum are conserved only on average, over large numbers of quantum events, and not in individual events. Einstein, of course, opposed the BKS theory because of its abandonment of strict energy-momentum conservation, but that is far from the whole story.

As we will see, there is irony here. Einstein turns out eventually to repudiate quantum mechanics in part because of its denial of the statistical independence of distant systems. But one of the main things that troubled him about the BKS theory was precisely its assumption of the statistical independence of atomic transitions (absorption or emission of energy quanta) in distant systems, or rather its failure to assume correlations sufficient to guarantee strict energy-momentum conservation in individual events.

In the BKS theory, each atom is assumed to be the source of a virtual radiation field with components corresponding to all of that atom's possible transitions. The radiation field serves two purposes. First, it determines the probabilities for emissions and absorptions by the atom from which the field originates, that is to say, the transition probabilities introduced by Einstein in his 1916 quantum theory papers (Einstein 1916a, 1916b). Second, it serves as the vehicle through which that atom communicates with surrounding atoms. It accomplishes this by helping to determine the probabilities for absorption and induced emission in these other atoms, depending upon whether or not it interferes constructively or destructively with the virtual radiation field emanating from each of the latter. But as BKS themselves stress, the correlations engendered by this communication between atoms are quite weak:

> In fact, the occurrence of a certain transition in a given atom will depend on the initial stationary state of this atom itself and on the states of the atoms with which it is in communication through the virtual radiation field, but not on the occurrence of transition processes in the latter atoms. . . . As regards the occurrence of transitions . . . we abandon . . . any attempt at a causal connexion between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories. (Bohr, Kramers, and Slater, p. 165)

Or again,

> By interaction between atoms at greater distances from each other, where according to the classical theory of radiation there would be no question of simultaneous mutual action, we shall assume an independence of the individual transition processes, which stands in striking contrast to the classical claim of conservation of energy and momentum. Thus we assume that an induced transition in an atom is not directly caused by a transition in a distant atom for which the energy difference between the initial and the final stationary state is the same. On the contrary, an atom which has contributed to the induction of a certain transition in a distant atom through the virtual radiation field conjugated with the virtual harmonic oscillator corresponding with one of the possible transitions to other stationary states, may nevertheless itself ultimately perform another of these transitions. (p. 166)

And, finally, they add, in an interesting comment: "But it may be emphasized that the degree of independence of the transition processes assumed here would seem the only consistent way of describing the interaction between radiation and atoms by a theory involving probability considerations" (pp. 166-167). But, of course, this is wrong, as the later development of quantum mechanics was to show.

Consider more carefully the kind of coupling that BKS were assuming. The probability of a transition in a given atom, A, is determined by its associated virtual radiation field. This virtual radiation field can be altered by the effects of a radiation field propagating, subluminally, from another atom, B, and since the virtual field radiating from B is determined by B's current stationary state, the probability of a transition in A can depend upon the state (the virtual field) of B, which is to say that the probability of a transition at A can depend upon the probabilities of various transitions at B. On the other hand, the probability of a transition at A is statistically independent of the actual occurrence of a transition at B. The first kind of dependence is wholly consistent with classical, local, field-theoretic models of interactions, since the changes in A's state (virtual field) induced by B's state (virtual field) are propagated subluminally. But dependence of the latter kind threatens classical models of local interaction, with prohibitions on "distant action"; it was general relativity's exclusion of such "Fernwirkungen" that Einstein cited in late 1924 as the main reason why general relativity would never be abandoned.

Einstein's objections to the BKS theory are recorded in at least three different places. Einstein gave a colloquium on the BKS theory in Berlin on 28 or 29 May, within days of the paper's German publication.[10] What may be a list of objections to the theory prepared for that occasion survives in the Einstein Archive (EA 8-076) under the title "Bedenken inbezug auf Bohr-Cramers." It begins as follows: "1) Strict validity of the energy principle in all known elementary processes. Assumption of the invalidity in distant actions unnatural." A similar list of objections is contained in a letter to Ehrenfest of 31 May 1924 (EA 10-087); it begins in the same vein: "1) Nature appears to adhere strictly to the conservation laws (Frank-Hertz, Stokes's rule). Why should distant actions be excepted?"

Perhaps the most interesting record of Einstein's objections, however, interesting because of its intended audience, is a letter from Pauli to Bohr of 2 October 1924, in which Pauli reports the contents of a conversation about the BKS theory that Pauli had with Einstein during the Innsbruck

[10] Rudolf Ladenburg to Kramers, 8 June 1924, as quoted in Bohr 1984, p. 27, gives the date as 28 May. But Wigner (1980, p. 461) reports that the colloquia took place regularly on Thursdays, which would make the date 29 May.

Naturforscherversammlung in late September (it was Pauli's first meeting with Einstein). The very first of Einstein's objections, as reported by Pauli, is this: "1. By means of fluctuation arguments one can show that, in the case of the statistical independence of the occurrence of elementary processes at spatially distant atoms, a system can, in the course of time, display systematic deviations from the first law, in that, for example, the total kinetic energy of a radiation-filled cavity with perfectly reflecting walls can, in the course of time, assume arbitrarily large values. He finds this dégoûtant (so he says)" (Pauli 1979, p. 164). What Einstein is pointing to in this example, also mentioned in the list of "Bedenken" for his Berlin colloquium and in the cited letter to Ehrenfest, is not the failure of energy and momentum to be conserved in individual events, which comes in objection 2, but rather the existence of systematic deviations from energy conservation even on the average; to Ehrenfest he describes this as a matter of the "constantly increasing Brownian motion" of a "mirror-box" (EA 10-087). In fact, the fluctuations turn out to be significant only in certain limiting cases (Schrödinger 1924), but that is of no consequence here. What is important is the clue that this and the other quoted remarks provide as to Einstein's real reservations about the BKS theory. Specifically, Einstein believed that any adequate quantum theory would have to incorporate at a basic level some kind of strong statistical dependence of spatially-separated systems, in order to secure strict energy-momentum conservation. And what he was searching for with his "mirror-box" thought experiment was a vivid way to show the consequences of the BKS theory's failure to do this.

Spatially separated systems are statistically independent in the BKS theory because it assigns a separate virtual wave field to each (spatially separated) atomic system. In this regard, the BKS theory resembles Einstein's own earlier speculations about "ghost fields" ["Gespensterfelder"] or "guiding fields" ["Führungsfelder"], which he had introduced to try to explain the interference effects between quantum systems, be they light quanta or material particles. And his reasons for objecting to the BKS theory are similar to the reported reason for his never having published his own ideas along this line; in his letter to Ehrenfest of 31 May 1924 he says of the BKS theory: "This idea is an old acquaintance of mine, but one whom I do not regard as a respectable fellow."

Here is how Wigner recalls Einstein's reasoning about this matter in his University of Berlin physics colloquium:

> Yet Einstein, though he was fond of it [the "Führungsfeld" idea], never published it. He realized that it is in conflict with the conservation principles: at a collision of a light quantum and an electron for instance, both would follow a guiding field. But these guiding fields give only the probabilities of the directions in which the two components, the light quantum and the electron, will proceed. Since they follow their directions independently, it may happen that in one collision the light quantum is strongly deflected, the electron very little. In another collision, it may be the other way around. Hence the momentum and the energy conservation laws would be obeyed only statistically--that is, on the average. This Einstein could not accept and hence never took his idea of the guiding field quite seriously. (Wigner 1980, p. 463; emphasis mine)

The dilemma that Einstein faced here was that some kind of wave aspect had to be associated with light quanta and material particles to explain diffraction and interference, wave-like interference even between material particles being suspected by many at least since the discovery of the Ramsauer effect in 1920. And these wave-aspects--call them "ghost fields," "guiding fields," "virtual fields," or whatever--can at best determine probabilistically the motions of individual particles or the transitions in individual

atoms. But as long as the "guiding" or "virtual fields" are assigned separately, one to each particle or atom, one cannot arrange both for the merely probabilistic behavior of individual systems and for correlations between interacting systems sufficient to secure strict energy-momentum conservation in all individual events. As it turned out, it was only Schrödinger's relocation of the wave fields from physical space to configuration space that made possible the assignment of joint wave fields that could give the strong correlations needed to secure strict conservation, and the even stronger correlations evinced in Bose-Einstein statistics. But as we shall see, the price to be paid for Schrödinger's innovation was a degree of non-separability between interacting systems that Einstein found intolerable because inconsistent with the field-theoretic manner of representing interactions.

Einstein had himself believed for some time that an adequate quantum theory would have to incorporate some kind of strong coupling between distant systems in order to secure strict energy-momentum conservation. Many other physicists were still not sure about this matter as late as fall 1924, when Pauli wrote to Bohr, in the above-cited letter (2 October 1924): "And if you were to ask me what I believe about the statistical dependence or independence of quantum processes in spatially distant atoms, then I must answer honestly: I do not know. The Geiger experiment, which I hear is already being started, will indeed quite soon decide this question experimentally. It suits me equally well if it turns out one way or the other" (Pauli 1979, p. 165). But the mentioned Bothe-Geiger experiment (Bothe and Geiger 1924, 1925a, 1925b) and the Compton-Simon experiment (Compton and Simon 1925a, 1925b, 1925c) were soon to persuade most everyone that energy and momentum are strictly conserved in individual atomic events. Writing to Einstein on 9 January 1925, Ehrenfest put the matter thus: "If Bothe and Geiger find a 'statistical independence' of electron and scattered light quantum, that proves nothing. But if they find a dependence, that is a triumph for Einstein over Bohr. -- This time (by way of exception!) I believe firmly in you and would thus be pleased if dependence were made evident" (quoted from Bohr 1984, p. 77). However, the issue had already been decided, as Einstein explained to Lorentz on 16 December 1924: "Geiger and Bothe have carried out an experiment that speaks in favor of strict light quanta and against the views that Bohr-Cramers-Slater have recently developed. They showed that in the Compton effect the deflected radiation and the electron thrown out toward the other side are events statistically dependent upon one another. But, nevertheless, the energy-momentum principle appears to hold strictly and not only statistically" (EA 16-575).

Of course the statistical dependence demonstrated by Bothe-Geiger and Compton-Simon does not involve the kind of correlation that surfaces in Bose-Einstein statistics. One can explain energy-momentum conservation quite naturally in terms of a model positing distinguishable particles, systems whose separate identities can be tracked throughout their interactions, which is precisely how Einstein preferred to think of his light quanta and material particles. In more modern language, the correlations evinced in the Bothe-Geiger and Compton-Simon experiments can be explained in terms of common causes; there is here no threat of non-locality or non-separability.

But while Einstein preferred to think of light quanta and material particles as independent, distinguishable systems, he really already knew better. For one thing, there was the obvious problem that a simple corpuscular model is powerless to explain interference and diffraction, which is part of what drove Einstein to the unsuccessful "Führungsfeld" idea in the first place. And, more importantly, Einstein's own earlier work on the quantum hypothesis, in particular, his efforts to understand the relationship between his light quantum hypothesis and Planck's radiation law, had already taught him that light quanta do not, in fact, behave like the independent particles of classical statistical mechanics. In other words, already at the time of the BKS theory, Einstein had good reason to expect that an adequate quantum theory would require correlations between interacting systems beyond those needed to secure strict energy-momentum conservation.

4. EINSTEIN'S EARLIEST REMARKS ON THE INDEPENDENCE OF QUANTA: 1905-1914

Recall that what primarily distinguished Einstein's point of view from Planck's in 1905 is that, whereas Planck wanted to quantize only the process of a resonator's absorbing or emitting energy, Einstein wanted to introduce light quanta or photons as carriers of that energy even between elementary events of emission or absorption. That is to say, Einstein wanted to quantize the electromagnetic radiation field itself, arguing that Maxwell's equations should be regarded as describing merely the average behavior of a large number of light quanta (Einstein 1905, p. 132; 1906, p. 203). But these light quanta are not yet the photons or light quanta of the mature quantum mechanics of the late 1920s, and this for one crucial reason. Remember the following oft-quoted remark from Einstein's 1905 paper: "Monochromatic radiation of low density (within the domain of validity of Wien's radiation formula) behaves from a thermodynamic point of view as if it consisted of _mutually independent_ energy quanta of the magnitude $R\beta\nu/N$" (Einstein 1905, p. 143; emphasis mine). I have deliberately emphasized the words whose import we usually do not appreciate when reading this passage.

In what sense did Einstein mean these quanta to be independent of one another? He was quite explicit on this point. The quanta are independent in the sense that the joint probability for two of them occupying specific cells in phase space is the product of the separate probabilities. After writing the relation, $W = W_1 \cdot W_2$ ("W" standing for probability, "Wahrscheinlichkeit"), Einstein comments: "The last relation says that the states of the two systems are mutually independent events" (Einstein 1905, p. 141). He had a good reason for postulating such independence. If one defines entropy according to Boltzmann's principle, $S = k \cdot \log(W)$, as Einstein thought one must, then the factorizability of the probability is a necessary and sufficient condition for the additivity of the entropy, itself a necessary condition in Einstein's eyes (Einstein 1905, p. 140).

Einstein never retreated from his belief in the existence of photons, but by 1909 it had become clear to him (if it was not already clear in 1905) that quanta conceived as independent particles, the quanta of 1905, are not the whole story about radiation. An explicit statement of this point first found its way into print in March 1909 in Einstein's masterful survey paper, "Zum gegenwärtigen Stand des Strahlungsproblems" (Einstein 1909a).

The context was yet another attempt to understand the relationship between his own light quantum hypothesis, which by itself was found to yield a formula for black-body radiation valid only in the Wien regime (ν/T large), and the kind of energy quantization implicit in Planck's radiation law. The method was that of fluctuation arguments, an approach that had served Einstein well in the past. He first asked what would be the mean-square fluctuations in the energy of a radiation-filled cavity, and, second, what would be the mean-square fluctuations in the radiation pressure, as manifested by fluctuations in the motion of a mirror suspended in the cavity. Both calculations led directly from Planck's radiation formula to a similar result, namely, an expression for the fluctuations that can be divided into two terms, the first of which Einstein interprets as arising from mutually independent light quanta, the second from interference effects of the kind to be expected were the radiation completely described by Maxwell's electrodynamics. Thus, with regard to the expression for energy fluctuations, Einstein says that this first term, $(R/Nk)\,\nu h\eta$, were it alone present, would yield

fluctuations "as if the radiation consisted of pointlike quanta of energy $h\nu$ that move independently of one another" (Einstein 1909a, p. 189). And about the expression for radiation pressure fluctuations, he says: "According to the current theory [Maxwell's electrodynamics], the expression must reduce to the second term (fluctuations due to interference). If only the first term were present, then the fluctuations in radiation pressure could be completely explained through the assumption that the radiation consists of slightly extended complexes of energy $h\nu$ that move independently of one another" (Einstein 1909a, p. 190). A complete account of cavity radiation entails, however, the presence of both terms. And so it follows that a complete theory cannot assume only mutually independent light quanta; it must allow for some means whereby localized, pointlike quanta can, mysteriously, interfere with one another.

At the time Einstein wrote this survey paper (received 23 January 1909, published 15 March), he was still rather sanguine about the prospects for finding a theoretical model of radiation embodying both the existence of quanta and the possibility of their interfering, this without departing significantly from existing theoretical conceptions. Near the end of the paper he says that what is apparently needed is "a _modification_ of our current theories," not "a complete _abandonment_ of them" (Einstein 1909a, p. 192). But he was clearly struggling to understand how localized quanta could possibly interfere with one another.

This issue came to the fore in an exchange of letters between Einstein and Lorentz in May of 1909, shortly after Einstein read Lorentz's influential lecture on the radiation problem delivered to the 1908 International Congress of Mathematicians in Rome (Lorentz 1908a).[11] Lorentz had by this time reluctantly accepted Planck's radiation formula, instead of his preferred Rayleigh-Jeans formula (see Lorentz 1908b), but in a letter to Einstein of 6 May 1909 (EA 16-418), he pressed Einstein to explain how localized, mutually independent quanta could explain interference and diffraction. Einstein replied on 23 May, speaking first to the question of independence: "I am not at all of the opinion that one should think of light as being composed of mutually independent quanta localized in relativiely small spaces. This would be the most convenient explanation of the Wien end of the radiation formlula. But already the division of a light ray at the surface of refractive media absolutely prohibits this view. A light ray divides, but a light quantum indeed cannot divide without change of frequency" (EA 16-419). Then he goes on to suggest how he really views the situation, introducing for the first time (as far as I can determine) the progenitor of his later "ghost" or "guiding" field idea:

> As I already said, in my opinion one should not think about constructing light out of discrete, mutually independent points. I imagine the situation somewhat as follows: . . . I conceive of the light quantum as a point that is surrounded by a greatly extended vector field, that somehow diminishes with distance. Whether or not when several light quanta are present with mutually overlapping fields one must imagine a simple superposition of the vector fields, that I cannot say. In any case, for the determination of events, one must have equations of motion for the singular points in addition to the differential equations for the vector field. (EA 16-419)

The point is, of course, that these vector fields will mediate the interactions among light quanta.

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[11]See Einstein to Lorentz 13 April 1909 (EA 70-139); Einstein read the 1909 reprinting in the _Revue génerale des sciences_ (Lorentz 1909).

Einstein's vector field idea first made its way into print in his second great survey paper of 1909, this his lecture "Über die Entwickelung unserer Anschauungen über das Wesen und die Konstitution der Strahlung" (Einstein 1909b), delivered to the Salzburg Naturforscherversammlung on 21 September. Einstein first reviews the radiation pressure fluctuation argument from the previous paper, and the interpretation of the two terms in the resulting expression for the fluctuations as quantum and interference terms respectively. But his growing realization that the quanta cannot be regarded as independent is reflected in his observation that the view of quanta as localized particles moving through space and being reflected independently of one another--the model that is the focus of his 1905 light quantum hypothesis paper--is "the crudest visualization of the light quantum hypothesis" (Einstein 1909b, p. 498). Einstein then introduces the vector field idea broached in the letter to Lorentz, but with the difference that the fields are here portrayed as "force fields" having the character of "plane waves." He concludes by noting that in introducing this idea, not yet an exact theory, he "only wanted to make it clear . . . that the two structural characteristics (undulatory structure and quantum structure), both of which should belong to radiation according to Planck's formula, are not to be viewed as irreconcilable with one another" (Einstein 1909b, p. 500).

The customary gloss on this last remark is that it is an anticipation of the notion of wave-particle duality. That is true, but it puts the emphasis in the wrong place. As we have seen, what was really going on here was, first, Einstein's coming to grips with the fact that photons or light quanta cannot be invested with the kind of independence from one another standardly assumed for the systems of particles to which classical statistical mechanics applies, and, second, his search for a theoretical model of quanta that would accomodate this lack of independence without compromising the principle that, at root, radiation has an atomistic structure.

Between 1909 and 1925, many investigations were inspired by Einstein's writings on light quanta, the principal aim being to understand more clearly the difference between Einstein's conception of independent light quanta and Planck's conception of quantized resonators. Several people theorized that the independence assumption had to be modified, and the conviction slowly gained force that the classical manner of counting complexions had to be modified after the manner of Planck's counting rule, though the theoretical foundations of the latter remained obscure. It was really only the papers of Bose and Einstein in 1924-1925 that began to clarify these matters. There is, however, one individual whose now almost entirely forgotten work on light quanta is of special interest because of the unexpected light it throws on Einstein's thinking about the independence problem during the 1910s. This is Mieczysław Wolfke, a young Polish physicist who took a degree under Otto Lummer at Breslau in 1910 and became a Privatdozent at the ETH in 1913. He moved to the University of Zurich, again as Dozent, in 1914, where he remained until assuming a professorship at the Warsaw Polytechnic in 1922. He was thus a colleague of Einstein's in Zurich for about eighteen months in 1913-1914; that relationship is important for our story.

Starting in late 1913, Wolfke published a series of papers developing a derivation of the Planck radiation formula starting from the assumption of what he termed "light atoms," which were conceived as being in some respects similar to Einstein's light quanta (Wolfke 1913a, 1913b, 1914a). Pressed by G. Krutkow (1914) to explain the difference between "light atoms" and "light quanta," especially to explain why Einstein's mutually independent light quanta lead to Wien's law whereas Wolfke's "light atoms" lead to Planck's law, Wolfke published in March of 1914 in the Physikalische Zeitschrift a short paper elaborating the different independence assumptions made by him and by Einstein. The crucial § 3 of his paper, entitled "The Decisive Presuppositions," reads as follows:

> Mr. Einstein has personally drawn my attention to the difference in principle between the Einsteinian light quantum theory and the foregoing argument [deriving Planck's law from light atoms].
>
> The definition of the independence of the light atoms from one another that one presupposes in the probability considerations is alone decisive for the derived radiation formulas.
>
> In the above derivation . . . of the Planck radiation formula only this general assumption is used, namely that the light atoms are mutually independent with regard to their existence, in other words, it is assumed that the probability for the existence of a light atom of a specific frequency is independent of how many atoms of the same frequency are simultaneously present in the volume under consideration.
>
> Nevertheless, in my derivation no limiting assumptions were established regarding the spatial distribution of the light atoms.
>
> However, in opposition to this, the Einsteinian light quantum theory presupposes the special case that light atoms are also spatially independent of one another, i.e, that the probability for a specific position of a light atom is independent of the simultaneous position of the other light atoms of the same frequency.
>
> In consequence of this, the Einsteinian light quantum theory leads to the Wien radiation law, which, as is well known, can be regarded as a special case of the Planck radiation formula. (Wolfke 1914b, p. 309)

How much of this is Einstein and how much Wolfke is hard to say; such evidence must by handled with care. But certainly nothing in the foregoing analysis is inconsistent with what Einstein had earlier said.

The assumption that Wolfke was accurately reporting Einstein's views is strengthened by Wolfke's reply to Krutkow's further demand that he give a more formal characterization of the two kinds of independence (Krutkow 1914b). For Wolfke adverts precisely to Einstein's 1905 characterization of independence, namely, the factorizability of the associated probabilities:

> In fact the Einsteinian light quanta behave like the individual, mutually independent molecules of a gas . . . . However, the spatial independence of the Einsteinian light quanta comes out even more clearly from Einstein's argument itself. From the Wien radiation formula Einstein calculates the probability W that all n light quanta of the same frequency enclosed in a volume v_0 find themselves at an arbitrary moment of time in the subvolume v of the volume v_0. The expression for this probability reads:
>
> $$W = (v/v_0)^n.$$
>
> This probability may be interpreted as the product of the individual probabilities v/v_0 that an individual one of the light quanta under consideration lies in the subvolume v at an arbitrary moment of time. From the fact that the total probability W is expressed as the product of the individual probabilities v/v_0, one recognizes that it is a matter of individual mutually independent events. Thus we see that, according to Einstein's view, the fact that a light quantum lies in a specific subvolume is independent of the position of the other light quanta. (Wolfke 1914c, pp. 463-464)

What Einstein is represented as asserting is a more careful analysis of the type of independence that must be denied to light quanta in an adequate quantum theory. Both Einstein's original 1905 light quanta and the kind of quanta that would have to be assumed to derive the Planck radiation law are held to be independent from the point of view of their existence, which is to say that the probability for the existence of a light quantum of some specific frequency is independent of the number of other light quanta of that frequency already in existence. But Einstein's quanta are independent of one another also in the spatial sense, which is to say that the joint probability for two quanta of the same frequency to occupy specific locations is factorizable as the product of separate probabilties for each to occupy its own location. It is the same kind of spatial independence assumed in classical statistical mechanics, but it must be denied in order to derive the Planck formula.[12]

In a later paper, Wolfke interpreted this failure of spatial independence as a matter of the quanta of a specific frequency tending to join together in complexes that he called "light molecules," separate light quanta being designated "light atoms" (Wolfke 1921). The analogy is of course strained, but it is interesting when one recalls how in 1925 Einstein characterized the novelty of the Bose-Einstein statistics by saying that the molecules "have a preference to sit together with another molecule in the same cell" of phase space and tend "to stack together" (Einstein to Schrödinger, 28 February 1925, EA 22-002).

Another path back to Einstein's 1924-1925 gas theory papers also leads through Einstein's characterization of the independence of light quanta in terms of the factorizability of joint probabilities and the associated additivity of entropies for composite systems. For all that the additivity principle was accorded fundamental importance by most of those who attended to the foundations of statistical mechanics, there was a puzzle about additivity that had been known to physicists since the publication in 1902 of Gibbs's Elementary Principles in Statistical Mechanics. In the final paragraph, Gibbs enunciated the paradox that was to come to be known by his name (Gibbs 1902, pp. 206-207). He considered a chamber divided into two halves by an impermeable barrier, each half filled by a gas; the entropy of the whole system is the sum of the entropies of the two components. When the barrier is removed, allowing the gases to mix, the total entropy will increase if the two gases are different in kind, whereas the total entropy will stay the same if the two gases are of the same kind. But that should not happen if the additivity principle is universally valid, because in both cases the previously separated volumes diffuse throughout the whole chamber in the same way, which should lead to an increase in the entropy of each previously separated component; and then if the entropies of these components still add in the normal way, the total entropy after mixing in both cases should go up. That it does not when the originally separated volumes are identical in kind must be connected in some way to a failure of the additivity principle in the case of indistinguishable particles.

Curiously, Gibbs's own reaction to this paradox is rarely noted. It was to infer that the paradox forces us for most purposes to use statistical measures of entropy and other thermodynamic quantities calculated on the basis of what he called the "generic phase," rather than the "specific phase" (Gibbs 1902, p. 207). Gibbs's conclusion is pertinent to the later history of the paradox in quantum mechanics because of the way he defines "generic phase." The "specific phase" is the phase as we normally conceive it in classical statistical mechanics--a point in the standardly defined $6n$-dimensional phase space for a system of n particles. The "generic phase" is defined, in effect, as an equivalence class of specific phases differing only through exchanging the positions of otherwise indistinguishible particles. This is almost Bose-Einstein statistics, except for the way Gibbs weighted the points (cells) in his "generic phase" space, namely, as the sum of the weights of the specific phases related to the given generic phase by exchange of indistinguishable particles.[13] But that the rules for counting phases or cells must be modified in the case of indistinguishable particles was clearly recognized by Gibbs.

Various authors puzzled over the Gibbs paradox in succeeding years. The realization that its solution is somehow connected to the curious statistics of indistinguishable particles began to emerge with an important study by Planck (1922). But it was really only in Einstein's gas theory papers of 1924 and 1925 that the problem found its definitive solution. At the end of the first of these papers, Einstein raises the problem (Einstein 1924b, p. 267), and then in the second paper he offers this solution:

> These considerations thow light upon the paradox that was pointed to at the end of first paper. In order for two wave trains to interfere noticeably, they must agree with regard to V [phase velocity] and ν [frequency]. Moreover . . . it is necessary that v as well as m nearly agree for both gases. The wavefields associated with two gases of noticeably different molecular mass thus cannot noticeably interfere with one another. From this one can conclude that, according to the theory presented here, the entropy of a gas mixture is additively composed out of those of the components of the mixture, exactly as in the classical theory, at least as long as the molecular weights of the components diverge from one another somewhat. (Einstein 1925a, p. 10)

The solution, in other words, is that the additivity associated with the factorizability of probabilities, and hence with the classical conception of the independence of interacting systems, fails precisely in those cases where interference is possible, interference being the other symptom of the failure of independence.

Einstein's solution of the Gibbs paradox comes at the end of a section largely devoted to a calculation of the mean square fluctuation in the number of particles with energies falling within a given infinitesimal range. The resulting expression is a sum of two terms that Einstein interprets in a manner analogous to the interpretation he gave in 1909 to the two terms in his expression for fluctuations in radiation pressure in black-body radiation, only now, of course, we are talking about massive particles rather than massless photons. Thus the first term is said to represent the fluctuations that would arise were the particles composing the gas statistically independent of one another. The second is the interference term (Einstein 1925a, p. 9). So, with regard to their relative independence and their capacity to interfere, material particles behave just like photons; or in the less helpful if more standard gloss on of this result, wave-particle duality is extended finally to material particles as well as to photons.

5. THE TURNING AWAY: 1925-1927

The publication of Einstein's three gas theory papers marked the end of his substantive contributions to the development of quantum theory. From

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[12] Since we are concerned with the probability of a system's occupying a given cell of phase space, Einstein must by the same logic be asssuming an independence with respect to the instantaneous momenta or velocities of the systems in question. Remember Einstein's glossing the necessary independence assumption in 1909 as the assumption that the quanta of energy "move independently of one another" (Einstein 1909a, p. 189; emphasis mine).

[13] Apart from the vocabulary, Gibbs's distinction between generic and specific phase is virtually identical to the distinction introduced in Planck 1925 between so-called "Quantenzellen" and "Urzellen," except, of course, that Planck knew, in effect, how to weight his "quantum cells" properly, even if he did not know why this is the proper weighting. See Mehra and Rechenberg 1982, p. 616, for further discussion.

this time on, with but few exceptions, Einstein's time and energy were devoted mainly to the search for a unified field theory that would accommodate empirically well-established quantum phenomena within the field-theoretic framework, but not by incorporating wholesale the formal apparatus of the developing quantum theory, an apparatus that Einstein gradually came to regard as fundamentally inadequate. And my hypothesis is that his reason for so regarding the quantum theory, his reason for turning away from active work on it and turning back to unified field theory, was his finally coming to grips with the fact that the quantum theory's way of describing interacting systems is incompatible with the assumptions of separability, locality, and independence that are a necessary part of the field-theoretic approach as he understood it. The clear articulation of this insight was to take most of the rest of his life; but it was clear enough already in 1925 to turn Einstein away from further substantive work on the quantum theory.

In the history of the development of modern quantum mechanics, events began to move rapidly in the spring of 1925. The paper containing Pauli's enunciation of the exclusion principle (Pauli 1925) was published in March. Two months later the first results of the Bothe-Geiger experiments were announced (Bothe and Geiger 1925a), a complete account coming in June (Bothe and Geiger 1925b). The Bothe-Geiger experiment (and the Compton-Simon experiment, the results of which were published in September--Compton and Simon 1925c) convinced most physicists that the particle-like light quanta Einstein had advocated for years would have to be taken seriously, and that Bohr, Kramers, and Slater were wrong in asserting the statistical independence of transition processes in distant atoms. But at the same time, Einstein was arguing in his gas theory papers that material particles as well as light quanta exhibit wave-like interference effects, of the kind recently predicted in de Broglie's thesis (de Broglie 1924), and hence that they cannot be independent, distinguishable particles of the kind posited in classical mechanics, though Einstein himself may have wanted them to be that way. So Einstein was arguing that both light and matter have wave- and particle-like properties. How was this situation to be understood?

What convinced many physicists that a wave-like character of material particles would have to be taken just as seriously as the particle-like character of light was Walter Elsasser's wave-theoretical interpretation of the Ramsauer effect (Ramsauer 1920, 1921a, 1921b) as an interference phenomenon (Elsasser 1925). Ramsauer claimed to have demonstrated experimentally that the mean-free path of electrons passing through certain noble gases goes to infinity (the scattering cross-section goes to zero) as the velocity of the electrons declines. In effect, the atoms of the gas become invisible to the electrons. Remember, these are material particles that Ramsauer was studying. The result was so shocking that many physicists literally did not believe it. Born's reaction is typical. In a letter to Einstein of 29 November 1921 he characterized Ramsauer's claim as "simply insane" (Born 1969, p. 93). But in his note published in July of 1925, in which he cites Einstein's gas theory papers and de Broglie's dissertation, Elsasser showed that the Ramsauer effect could be interpreted quite straightforwardly as a result of interference between the electrons and the atoms in the gas.

One important figure had himself been thinking independently about the Ramsauer effect in much the same way as Elsasser. Here is what Bohr wrote to Hans Geiger on 21 April 1925 in response to Geiger's report of a new experiment by Bothe refuting another implication of the BKS theory (Einstein's reaction to this experiment is discussed below):

> I was quite prepared to learn that our proposed point of view about the independence of the quantum process in separated atoms would turn out to be wrong. . . . Not only were Einstein's objections very disquieting; but recently I have also felt that an explanation of collision phenomena, especially Ramsauer's results on the penetration of slow electrons through atoms, presents difficulties to our ordinary space-time description of nature similar in kind to the those presented by the simultaneous understanding of interference phenomena and a coupling of changes of state of separated atoms by radiation. In general, I believe that these difficulties exclude the retention of the ordinary space-time description of phenomena to such an extent that, in spite of the existence of coupling, conclusions about a possible corpuscular nature of radiation lack a sufficient basis. (Bohr 1984, p. 79)

On the same day Bohr wrote much the same thing to James Franck (director of the institute where Elsasser worked in Göttingen): "It is, in particular, the results of Ramsauer concerning the penetration of slow electrons through atoms that apparently do not fit in with the assumed viewpoint. In fact, these results may pose difficulties for our customary spatio-temporal description of nature that are similar in kind to a coupling of changes of state in separated atoms through radiation. But then there is no more reason to doubt such a coupling and the conservation laws generally" (Bohr 1984, p. 350). What Bohr means here by "customary space-time description" and similar terms is precisely a description like that afforded by classical field theories or classical mechanics, where spatially separated systems are assumed to be separable. What is important is that the Ramsauer effect was seen as evidence for distant correlations between material particles of the kind then commonly represented by wave-theoretical interference.

Many physicists were impressed by Einstein's gas theory papers but puzzled about the new statistics, which they struggled to understand and reinterpret (see, for example, Planck 1925, Schrödinger 1925). Most puzzling was the physical significance of the denial of independence in Bose-Einstein statistics. How could spatially localized material particles fail to be independent? How could they interfere with one another? Encouraged by Elsasser's note and by Einstein's own nod toward de Broglie, several of these thinkers, most notably Landé (1925) and Schrödinger (1926a), sought to develop consistent wave-theoretical interpretations of the new statistics, thinking this the only way to understand the non-independence of interacting systems. Indeed, Elsasser himself, in proposing his interpretation of the Ramsauer effect as a wave-like interference phenomenon, wrote of Einstein's "detour through statistics" (Elasasser 1925, p. 711).

Schrödinger's paper is an important first step toward the development of wave mechanics. It begins as follows:

> In the new gas theory recently developed by A. Einstein, this surely counts, in general, as the essential point, namely, that an entirely new kind of statistics, the so-called Bose statistics, are to be applied to the movements of gas molecules. One's natural instinct rightly resists viewing this new statistics as something primary, incapable of further explanation. On the contrary, there seems to be disguised within it the assumption of a certain dependence of the gas molecules upon one another, or an interaction between them, which nevertheless in this form can only be analyzed with difficulty.
>
> One may expect that a deeper insight into the real essence of the theory would be obtained if we were able to leave as it was the old statistical method, which has been tested in experience and is logically well founded, and were to undertake a change in the foundations <u>in a place</u> where it is possible without a <u>sacrificium intellectus</u>. (Schrödinger 1926a, p. 95)

Schrödinger goes on to observe that what yields Einstein's gas theory is the application to molecules of the kind of statistics which, applied to "light atoms" (cf. Wolfke 1914b, 1921), gives the Planck formula. But he notes that we can derive the latter using the "natural" statistics if only we apply these statistics to the "ether resonators," that is to the degrees of freedom of radiation. Schrödinger then suggests that the same trick will work with gas molecules, if we simply interchange the concepts, "manifold of energy states" and "manifold of carriers of these states." And he comments:

> Thus one must simply fashion our model of the gas after the <u>same</u> model of cavity radiation that corresponds to what is <u>still not</u> the extreme light quantum idea; then the natural statistics--basically the convenient Planck method for summing states--will lead to the Einstein gas theory. That means nothing else than taking seriously the de Broglie-Einstein undulation theory of moving corpuscles, according to which the latter are nothing more than a kind of "foamy crest" on wave radiation that constitutes the underlying basis of everything. (Schrödinger 1926a, p. 95)

The idea is, as Einstein himself had suggested, to try to understand the failure of independence in Bose-Einstein statistics by means of the wave-theoretical conception of interference. But what this means is that one of the primary motivations behind Schrödinger's development of wave mechanics was the desire to explain the curious statistics of interacting systems, and in particular the failure of probabilities to factorize, that had come to the fore in the Bose-Einstein statistics.

Einstein followed all of these developments closely, as one can best tell from his correspondence with Ehrenfest and Schrödinger. Indeed, the collaboration between Einstein and Schrödinger was such that Schrödinger tried unsuccessfully to persuade Einstein to be listed as coauthor of a paper on degeneracy in a Bose-Einstein gas (Schrödinger 1926b).[14] Through most of 1925 and into early 1926, Einstein was primarily concerned with such investigations of the new Bose-Einstein statistics. He was also following the controversy over the Pauli exclusion principle and Fermi-Dirac statistics for spin-$\frac{1}{2}$ particles like the electron, being at first quite sceptical.

Late in 1925, however, Einstein also turned his attention to the new matrix formalism. The fundamental papers of Heisenberg, Born, and Jordan appeared in the <u>Zeitschrift für Physik</u> between 18 September 1925 and 4 February 1926. From the start, Einstein was critical of this approach. Thus, in a letter to Michele Besso of 25 December 1925, he wrote: "The most interesting thing that theory has yielded recently is the Heisenberg-Born-Jordan theory of quantum states. A real multiplication sorcery in which infinite determinants (matrices) take the place of cartesian coordinates. Most ingenious and so greatly complicated as sufficiently to protect it against a proof of incorrectness" (Speziali 1972, pp. 215-216). In a letter to Ehrenfest of 12 February 1926, he wrote: "I have still busied myself much with Heisenberg-Born. Though with all manner of admiration for the idea, I incline more and more to the view that it is incorrect" (EA 10-130). On 13 March he said much the same in a letter to Lorentz: "Though with all manner of admiration for the spirit that resides in these works, my instinct struggles against this way of conceiving things" (EA 16-594). One month later, on 12 April, he offered this more detailed criticism in a letter to Ehrenfest and praises Schrödinger's wave mechanics by comparison:

> The Born-Heisenberg thing will certainly not be right. It appears not to be possible to arrange uniquely the correspondence of a matrix

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[14]See Schrödinger to Einstein, 5 November 1925 (EA 22-005); Einstein to Schrödinger, 14 November (EA 22-009); and Schrödinger to Einstein, 4 December (EA 22-010). Einstein did present the paper at the 7 January meeting of the Berlin Academy, in whose proceedings it was published.

> function to an ordinary one. Nevertheless, a mechanical problem is supposed to correspond uniquely to a matrix problem. On the other hand, Schrödinger has constructed a highly ingenious theory of quantum states of an entirely different kind, in which he lets the De Broglie waves play in phase space. The things appear in the Annalen. No such infernal machine, but a clear idea and -- "compelling" in its application. (EA 10-135)

And the initial enthusiasm for Schrödinger is echoed in a letter to Lorentz on the same day: "Schrödinger has, in press, a theory of quantum states, a truly ingenious carrying out of de Broglie's idea" (EA 16-600).

Schrödinger's first wave-mechanics paper had appeared in the <u>Annalen</u> on 13 March. Given the background to it explored above, namely, Schrödinger's employment of Einstein's own ideas on the wave nature of material particles in an attempt to understand the physical meaning of Einstein's new statistics, it is not surprising that Einstein was initially favorably inclined toward wave mechanics. But Einstein's enthusiasm was short lived. Schrödinger's proof of the equivalence of wave and matrix mechanics was published on 4 May. It may be coincidence, but the first hint of doubt about wave mechanics on Einstein's part appears at just this time. Thus, a few days earlier, on 1 May, he wrote to Lorentz: "Schrödinger's conception of the quantum rules makes a great impression on me; it seems to me to be a bit of reality, however unclear the sense of waves in n-dimensional q-space remains" (EA 16-604). By 18 June, the doubts about Schrödinger's approach began to crystallize, as Einstein explained in another letter to Ehrenfest: "Schrödinger's works are wonderful--but even so one nevertheless hardly comes closer to a real understanding. The field in a many-dimensional coordinate space does not smell like something real" (EA 10-138). And four days later he voiced similar doubts to Lorentz: "The method of Schrödinger seems indeed more correctly conceived than that of Heisenberg, and yet it is hard to place a function in coordinate space and view it as an equivalent for a motion. But if one could succeed in doing something similar in four-dimensional space, then it would be more satisfying" (EA 16-607).

Throughout the summer and fall of 1926, Einstein continued to worry about the significance of waves in coordinate space. On 21 August he wrote to Sommerfeld: "Of the new attempts to obtain a deeper formulation of the quantum laws, that by Schrödinger pleases me most. If only the undulatory fields introduced there could be transplanted from the n-dimensional coordinate space to the 3 or 4 dimensional! The Heisenberg-Dirac theories compel my admiriation, but to me they don't smell like reality" (Hermann 1968, p. 108). On 28 August he wrote to Ehrenfest:

> Admiringly--mistrustfully I stand opposed to quantum mechanics. I do not understand Dirac at all in points of detail (Compton effect). . . . Schrödinger is, in the beginning, very captivating. But the <u>waves in n-dimensional</u> coordinate space are indigestible, as well as the absence of any understanding of the frequency of the emitted light. I have already written to you that the canal ray experiments have turned out entirely in the sense of the undulatory theory. Hic waves, hic quanta! both realities stand rock solid. Aber der Teufel macht einen Vers darauf (der sich <u>wirklich</u> reimt). (EA 10-144)[15]

And as late as 29 November the same combination of admiration and growing distrust is still evident in a letter to Sommerfeld: "The successes of

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[15]This last remark is untranslatable without loss of meaning. Literally, it means: "But the devil makes a poem about this (that <u>really</u> rhymes). Figuratively it means something like, "only the devil can make sense of it."

Schrödinger's theory make a great impression, and yet I do not know whether it is question of anything more than the old quantum rules, i.e., a question of something corresponding to an aspect of the real events. Has one really come closer to a solution of the riddle?" (EA 21-356). But surely Einstein's most famous remark from this period is the one found in his letter to Born of 4 December: "Quantum mechanics very much commands attention. But an inner voice says that that is still not the real thing. The theory delivers much, but it hardly brings us closer to the secret of the old one. In any case, I am convinced that He does not play dice. Waves in 3n-dimensional space, whose velocities are regulated by potential energy (e.g., rubber bands) . . ." (Born 1969, pp. 129-130). Remember that it was Born himself who earlier in the summer of 1926 had first introduced the probabilistic interepretation of the Schrödinger wave function (Born 1926a, 1926b).

After the turn of the year, the doubts about wave mechanics finally hardened into conviction. Thus Einstein wrote on 11 January 1927 to Ehrenfest: "My heart is not warmed by the Schrödinger business--it is noncausal and altogether too primitive" (EA 10-152). And the same hardening of Einstein's opinion is clear from a letter to Lorentz of 16 February 1927: "The quantum theory has been completely Schrödingerized and has much practical success from that. But this can nevertheless not be the description of a real process. It is a mystery" (EA 16-611). Within two months this conviction was to evolve into a sharp and penetrating critique.

What was Einstein doing during this period that might help to explain his growing disenchantment with the new quantum mechanics of Heisenberg, Dirac, Born, Jordan, and Schrödinger, a theory whose development owed so much to the stimulus of Einstein's own investigations, including most recently his gas theory papers? On the one hand, Einstein--always the good empiricist--was paying careful attention to new experimental developments that might shed light on the quantum puzzles, especially those probing distant correlations and wave-like interference phenomena. Thus, for example, in the above-cited 13 March letter to Lorentz he reports with delight the results of an experiment by Bothe (Bothe 1926) yielding another refutation of the BKS theory and a vindication of his own light quantum hypothesis.

In Bothe's experiment, a piece of copper foil is weakly irradiated with x-rays, producing flouresence radiation (approximately 2 events per second), nearly all of which is captured in two oppositely situated Geiger counters perpendicular to the incident x-rays. According to the BKS theory, the radiation emitted from an atom is represented by a nearly spherical wave emanating from the atom; the wave determines the probability of subsequent absorptions in all parts of space reached by the wave. There would therefore be a nonvanishing probability that radiation will be absorbed simultaneously in each chamber, that is to say a small but significant probability that coincidences will be detected. But according to Einstein's light quantum hypothesis, emission is a directed process; if the emitted quantum is absorbed in one chamber, there is no chance of a simultaneous absorption in the other chamber. Thus, no coincidences should be detected, except those few arising accidentally from nearly simultaneous emissions. What Bothe found was a coincidence rate far lower than would be expected on the BKS theory, a rate very close to that predicted by the light quantum hypothesis. Einstein reported the results to Lorentz as Bothe described them: "He found complete statistical independence of the absorption events" (EA 16-594). But as Einstein was surely aware, the result could as well be described as showing a strong statistical dependence between events in the two chambers, a perfect correlation between detection in one chamber and non-detection in the other.

On 16 March, three days after reporting to Lorentz the results of the Bothe experiment, and three days after Schrödinger's first wave mechanics paper (Schrödinger 1926c) appeared in the Annalen, Einstein submitted a note

to Die Naturwissenschaften (Einstein 1926a) in which he himself proposed another experiment. Something about the old quantum theory that had long troubled Einstein was the fact that the frequency of radiation emitted from an atom is not related to any intrinsic periodicity of the atom itself, such as periodic mechanical motions of charge carriers (electrons) within the atom, as it should be according to classical Maxwellian electrodynamics. Instead, the frequency is related only to the difference in energy between two stationary states. But then, so Einstein seems to have reasoned, radiation of identical frequency emitted by different atoms should not cohere. Einstein's experiment was designed to exhibit such coherence, if it existed, in the transverse radiation emitted by separate atoms in an atomic beam (canal ray). There was controversy about whether the experimental design was sufficient to yield an unambiguous decision,[16] but when the findings were finally reported (Rupp 1926), Einstein at least took them as confirming coherence, even if he was not entirely happy with that result, preferring, as he did, a world of independent light quanta and particles.[17] Here then we have another experiment, this one initiated by Einstein himself, which interested Einstein primarily for the light it shed on the peculiarities of the quantum mechanical account of interacting systems. In this case it was a matter of the peculiar interference exhibited by systems that would have to be regarded, from a classical point of view, as wholly independent. Ironically, Einstein was yet again contributing to showing that the world may not be the way he wanted it to be.[18]

There is other evidence that Einstein was brooding about the strange quantum mechanics of composite systems in the spring of 1926, and this specifically in connection with Schrödinger's wave mechanics. One month after his proposal for the canal ray interference experiment, on 16 April, Einstein wrote to Schrödinger that the new theory showed "true genius," but that he was troubled by one feature of it:

> With justified enthusiasm, Herr Planck has shown me your theory, which I too have then studied with the greatest interest. In the course of this study, one doubt has occurred to me, which you can hopefully banish for me. If I have two systems that are not at all coupled with one another, and E_1 is a possible value of the energy of the first according to quantum mechanics, with E_2 such a value for the second system, then $E_1 + E_2 = E$ must be such a value for the total system composed of the two. But I do not see how your equation
>
> $$\mathrm{div\ grad}\ \phi + \frac{E^2}{h^2(E - \Phi)}\ \phi = 0$$
>
> should express this property.
>
> So that you will see what I mean, I set down another equation that would satisfy this requirement:
>
> $$\mathrm{div\ grad}\ \phi + \frac{E - \Phi}{h^2}\ \phi = 0$$

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[16] See, for example, Schrödinger to Einstein, 23 April 1926 (EA 22-014), and Einstein to Schrödinger, 26 April (EA 22-018); see also Joos 1926, and Bohr to Einstein, 13 April 1927 (Bohr 1985, pp. 418-421).

[17] See Einstein 1926b; see also Einstein to Ehrenfest, 18 June 1926 (EA 10-138), and 28 August (EA 10-144), as well as Einstein to Lorentz, 22 June 1926 (EA 16-607).

[18] In his letter to Ehrenfest of 24 November 1926 (EA 10-148), Einstein alludes to yet another experiment that must have been concerned with the same cluster of problems: "Our experiment on the Compton effect is still not ready, but it will certainly succeed." I have been able to determine neither the details of the experiment, nor who Einstein's collaborator was.

Then the two equations

$$\text{div grad } \phi_1 + \frac{E_1 - \Phi_1}{h^2} \phi_1 = 0 \text{ (valid for the phase space of the 1st system)}$$

$$\text{div grad } \phi_2 + \frac{E_2 - \Phi_2}{h^2} \phi_2 = 0 \text{ (" " " " " " " 2nd ")}$$

have as a consequence

$$\text{div grad } (\phi_1 \phi_2) + \frac{(E_1 + \Phi_1) - (E^2 + \Phi_2)}{h^2} (\phi_1 \phi_2) = 0 \quad \text{(valid in combined q-space)}$$

One requires for the proof only to multiply the equations with ϕ_1 or ϕ_2 and then add. $\phi_1\phi_2$ would thus be a solution to the equation for the combined system belonging to the energy value $E_1 + E_2$.

I have tried in vain to establish a relation of this kind for your equation. (EA 22-012)

In fact, Einstein had misremembered the Schrödinger equation, which was precisely the one Einstein proposed as possessing the desired additivity property. Einstein noted the error himself in a postcard to Schrödinger of 22 April (EA 22-013), which must have crossed in the mail Schrödinger's letter of 23 April (EA 22-014) pointing out the same thing. But as slips of memory go, this one is interesting for what it reveals about Einstein's concerns. And that the description of composite systems __was__ his concern is made evident in his next postcard to Schrödinger, dated 26 April, after receipt of Schrödinger's letter of the 23rd. After again apologizing for his error, Einstein writes: "I am convinced that you have found a decisive advance with your formulation of the quantum condition, just as I am convinced that the Heisenberg-Born path is off the track. There the same condition of system-additivity is __not__ fulfilled" (EA 22-018). Clearly, Einstein had not yet seen Schrödinger's proof of the equivalence of wave and matrix mechanics (Schrödinger 1926e).

It would take Einstein another year to pinpoint the difficulty with wave mechanics that he was just beginning to sniff out. Here in the spring of 1926 he was arguing that if there exist two solutions of the Schrödinger equation, ϕ_1 and ϕ_2, for two different systems, then $\phi_1 \cdot \phi_2$ should also be a solution for the joint system, as it indeed is. What he was to argue in the spring of 1927 was that if ϕ is any solution of the Schrödinger equation for a joint system, then it ought to be equivalent to a product, $\phi_1 \cdot \phi_2$, of separate solutions for the separate subsystems, which is generally not the case.

His growing doubts about quantum mechanics are apparently what led Einstein in late summer 1926 to turn his attention back to a problem connected with general relativity that he had neglected since 1916. The problem was that of deriving the equations of motion for a test particle from the general relativistic field equations.[19] Einstein himself had dealt with the issue in an approximate way in 1916, showing that particles follow geodesic paths (at least if the particle's own gravitational field is neglected), and it had in the meantime been investigated by a number of others, including Hermann Weyl and Arthur Eddington, who established exact results. Curiously, however, Einstein seems to have been largely unaware of this work when

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[19] The first mention of the problem of motion that I can find in Einstein's correspondence from this period is in a letter to Besso of 11 August 1927, apparently written while Einstein was visiting in Zurich. It may thus be the case that his interest in returning to the problem was stimulated by conversations with Schrödinger, who was himself then teaching at the University of Zurich.

he tackled the problem in 1926 as if it were still an open matter. He eventually submitted two papers on the subject to the Berlin Academy--in January (Einstein and Grommer 1927) and November (Einstein 1927d) 1927--which largely reproduce the results of the earlier work.[20]

Exactly what Einstein hoped to achieve is not clear, though he seems to have hoped that deriving the equations of motion for elementary particles from general relativity would lead to progress in the quantum theory, an earlier attempt to tie gravitation to quantum mechanics by means of an overdetermined set of field equations having failed (Einstein 1923). Thus, in summarizing the results of the first paper, Einstein and Grommer write: "The progress achieved here lies in the fact that it is shown for the first time that a field theory can include a theory of the mechanical behavior of discontinuities. This can be of significance for the theory of matter, or the quantum theory" (Einstein and Grommer 1927, p. 13). And in a letter to Weyl of 26 April 1927 (this in reply to Weyl's letter of 3 February complaining about Einstein's neglect of Weyl's own earlier work on the subject), Einstein writes:

> I attach so much importance to the whole issue because it would be very important to know whether or not the field equations are to be seen as refuted by the facts of the quanta. One is indeed naturally inclined to believe this and most do believe it. But until now still nothing appears to me to have been proved about this.
>
> The new results in the quantum domain are really impressive. But in the depths of my soul I cannot reconcile myself to this head-in-the-sand conception of the half-causal and half-geometrical. I still believe in a synthesis of the quantum and wave conceptions, which I feel is the only thing that can bring about a definitive solution. (EA 24-088)

More specifically, Einstein seems from the start to have hoped that some of the characteristically non-classical features of the quantum theory might result; at least so it seems from slightly despairing negative remarks, like this in his letter to Ehrenfest of 24 November 1926: "The equations of motion of singularities can really be derived relativistically. But it appears that absolutely nothing 'unclassical' is to be obtained thereby" (EA 10-148). The despair changed to hope early in 1927, after the publication of the first paper on the equations of motion. Thus, on 11 January 1927, he writes to Ehrenfest: "The problem of motion has become pretty, even if there is still a slight snag in it. In any case, it is interesting that the field equations can determine the motion of singularities. I even think that this will once again determine the development of quantum mechanics, but the way there is still not to be perceived" (EA 10-152). And on 5 May he writes, again to Ehrenfest: "I published the paper on the relativistic dynamics of the singular point indeed a long time ago. But the dynamical case still has not been taken care of correctly. I have now come to the point where I believe that results emerge here that deviate from the classical laws of motion. The method has also become clear and certain. If only I would calculate better! . . . It would be wonderful if the accustomed differential equations would lead to quantum mechanics; and I do not regard it as being at all out of the question" (EA 10-162). The hope was not realized, but as late as November 1927, when he published his second note on the problem of motion, Einstein continued to regard the question as an open one.

Einstein was looking to general relativity to provide equations of motion for elementary particles, because he thought it one of the principal

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[20] For the history of this problem, see Havas 1989, upon which I have relied extensively.

shortcomings of Schrödinger's wave mechanics that it failed to do so. In effect, what was lacking in the new quantum mechanics, as was soon made vivid by Heisenberg's enunciation of the uncertainty relations (Heisenberg 1927), was the concept of a world-line or a trajectory, establishing which is the aim of the problem of motion. Thus, the criticism of Schrödinger in the above-quoted letter of 11 January 1927 to Ehrenfest concludes: "I do not believe that kinematics must be discarded." Moreover, given his preference for a view of nature in which the separability of interacting systems is assumed, Einstein would naturally look to field theories, like general relativity, to supply the want in quantum mechanics.

Exactly how a field theory might accomplish the end of saving the notion of independent systems while reproducing the empirically established quantum facts was a question Einstein did not prejudge. He seems ready to consider a number of alternatives, but all within the larger framework of the field-theoretic way of individuating systems. So it is no accident, for example, that in mid-1926 and early 1927 he begins to show renewed interest in, and later genuine enthusiasm for five-dimensional Kaluza-Klein theories, a subject he had touched upon four years before (Einstein and Grommer 1923), and which had been revived by Oskar Klein in April 1926 as a way of trying to unify quantum mechanics and general relativity (Klein 1926). In a letter to Ehrenfest of 18 June 1926, for example, Einstein concludes a paragraph criticizing Schrödinger, with the remark: "I am curious about what Herr Klein has found; give him my best" (EA 10-138); and on 3 September, after the July publication of Klein's paper, he remarks to Ehrenfest: "Klein's paper is beautiful and impressive" (Pais 1982, p. 333). On 16 February 1927, at about the time of two short notes of his own on the Kaluza-Klein theory (Einstein 1927a), he commented in a letter to Lorentz: "It turns out that the unification of gravitation and Maxwell's theory by means of the five-dimensional theory (Kaluza-Klein-Fock) is accomplished in a completely satisfactory way" (EA 16-611). And on 5 May he writes Ehrenfest: "The last paper by O. Klein pleased me very much; he really appears to be a level-headed fellow" (EA 10-162). By early the following year, Einstein had given up entirely the hope that equations of motion derived from general relativity would solve the quantum problem, looking now exclusively to Kaluza-Klein theories, as he explained to Ehrenfest on 21 January 1928: "I think I told you that the derivation of the law of motion according to the rel. theory has finally succeeded. But it simply comes out classically. I think that Kaluza-Klein have correctly indicated the way to advance further. Long live the 5th dimension" (EA 10-173; as quoted in Havas 1989, p. 249).

On 23 February 1927, shortly after the presentation of his first paper on Kaluza-Klein theories, Einstein gave a talk at the University of Berlin under the title, "Theoretisches und Experimentelles zur Frage der Lichtentstehung" (Einstein 1927b). The only significance of the talk is that it gives us yet another clue to the issues that were claiming Einstein's attention in the spring of 1927. He singles out for attention a recent experiment of Bothe's that is, in a way, a progenitor of the two-slit diffraction experiment (Bothe 1927). Bothe arranged for radiation from a single x-ray source to be divided into two beams, each of which impinges on a paraffin block, P_1 and P_2, respectively. Part of the scattered radiation from each block is then allowed to scatter a second time from a third paraffin block, S, placed midway between the other two, and one then measures the magnitude of the Compton effect in the resulting twice-scattered radiation. Bothe's declared aim was to use the experiment to decide between two different ways in which light quanta may be associated with a wave field. In the first, the entire energy and momentum of the field is taken to be concentrated in a single "super" light quantum. In the second, each quantum has its normal energy, $h\nu$, and momentum, $h\nu/c$, the individual quanta being associated with partial waves, the total wave field being regarded as a product of the activity of many quanta. Bothe argued that the first conception would lead to

an anomalous Compton effect, whereas the second, because of constructive interference between the two coherent partial waves incident upon S, would yield the same Compton effect as if S were irradiated by only a single beam. The results decisively favored the second point of view.

Einstein's report of these results is interesting because of how it differs from Bothe's. Einstein says merely that the results support the view that "light has a particle-like character, and is thus corpuscular" (Einstein 1927b, p. 546). This is true, inasmuch as Bothe claimed to have demostrated that energy and momentum are associated with light quanta after the fashion of Einstein's own corpuscular conception of the quanta. But Bothe went on to point out that the interference crucial to the experiment's outcome is incompatible with a radically corpuscularian conception of light: "It thus turns out that the spatio-temporal localization of the quanta does not go so far as to permit one to speak, generally, of a continuous 'motion.' In this we glimpse the principal result of the investigation" (Bothe 1927, p. 342). And in order to make this point even more vividly, Bothe published a schematic diagram of the experiment, to help show that a simple conception of light quanta as strictly localized particles cannot explain the alternating light and dark bands in a typical interference pattern. Of course, Einstein too understood that interference phenomena ruled out a radical corpuscularian conception of light. Indeed, he immediately follows his characterization of the Bothe experiment with the remark: "But other characteristics of light, the geometrical characteristics and the interference phenomena, cannot be explained by the quantum conception" (Einstein 1927b, p. 546), and he ended his talk thus: "What nature demands of us is not a quantum theory or a wave theory, instead nature demands of us a synthesis of both conceptions, which, to be sure, until now still exceeds the powers of thought of the physicists" (Einstein 1927b, p. 546). But Einstein's not mentioning that Bothe's experiment itself dramatically revealed this very duality suggests that his instinctive sympathies still lay with the radical corpuscularian view.

It is against this background that we must assess what is assuredly Einstein's most interesting critical comment on the quantum theory from this period. At the meeting of the Berlin Academy on 5 May 1927, Einstein presented a paper entitled "Does Schrödinger's Wave Mechanics Determine the Motion of a System Completely or Only in the Statistical Sense?" ["Bestimmt Schrödinger's Wellenmechanik die Bewegung eines Systems vollständig oder nur im Sinne der Statistik?"]. As word of the talk spread, it evidently aroused considerable interest, as witness Heisenberg's letter to Einstein of 19 May, where he writes: "In a roundabout way, through Born, Jordan, I learned that you had written a paper in which you put forward the same points that you advanced in the recent discussion, namely, that it would still be possible to know the paths of corpuscles more exactly than I would like. Now I naturally have a burning interest in this. . . . I do not know whether you would find it very immodest if I might ask you for any proofs of this work?" (EA 12-173). And, at least initially, Einstein thought he had established a secure result, writing to Ehrenfest on 5 May: "I have also now carried out a little investigation concerning the Schrödinger business, in which I show that, in a completely unambiguous way, one can associate definite movements with the solutions, something which makes any statistical interpretation unnecessay" (EA 10-162). For reasons that we will explore shortly, the work was never published, but a manuscript version survives in the Einstein Archive (EA 2-100).

Einstein's idea for associating definite movements with any solution of the Schrödinger equation was the following. Given any solution, Ψ, for definite total energy E and potential energy Φ, it is possible to express div grad Ψ as a sum of n terms Ψ_{ab}, to each of which we can associate a definite "direction" in n-dimensional configuration space; the n "directions" will be defined separately for the value of Ψ at each point in configuration space. Following Schrödinger, Einstein styled div grad Ψ a "metric" in configuration space, calling the Ψ_{ab} the "tensor of Ψ-curvature" and div grad Ψ the scalar of this tensor. He then showed that the total kinetic energy of the system can also be expressed as a sum of terms, one corresponding to each of the "directions" in configuration space, and that, having thus decomposed the kinetic energy, one can associate with each term in the expression for the total kinetic energy a "velocity" in the corresponding "direction." Whether one can make physical sense out of these "velocities" is not clear; but an answer to the question is not essential to our story.

What is essential is the reason Einstein himself gave for abandoning this effort. The copy in the Archive has attached to it an extra sheet headed "Nachtrag zur Korrektur" ["Added in Proof"]. Pais reports "that the paper was in print when Einstein requested by telephone that it be withdrawn" (Pais 1982, p. 444). One might guess that the addition helps to explain the withdrawal:

> Added in proof. Herr Bothe has in the meantime calculated the example of the anisotropic, two-dimensional resonator according to the schema indicated here and thereby found results that are surely to be rejected from a physical standpoint. Stimulated by this, I have found that the schema does not satisfy a general requirement that must be imposed on a general law of motion for systems.
>
> Consider, in particular, a system Σ that consists of two energetically independent subsystems, Σ_1 and Σ_2; this means that the potential energy as well as the kinetic energy is additively composed of two parts, the first of which contains quantities referring only to Σ_1, the second quantities referring only to Σ_2. It is then well known that
>
> $$\Psi = \Psi_1 \cdot \Psi_2 ,$$
>
> where Ψ_1 depends only on the coordinates of Σ_1, Ψ_2 only on the coordinates of Σ_2. In this case we must demand that the motions of the composite system be combinations of possible motions of the subsystems.
>
> The indicated scheme does not satisfy this requirement. In particular, let μ be an index belonging to a coordinate of Σ_1, ν an index belonging to a coordinate of Σ_2. Then $\Psi_{\mu\nu}$ does not vanish. . . .
>
> Herr Grommer has pointed out that this objection could be taken care of by means of a modification of the stated schema, in which we employ not the scalar Ψ itself, but rather the scalar $\lg \Psi$ for the definition of the principal directions. The elaboration of this idea should occasion no difficulty, but it will only be presented when it has been shown to work in specific examples. (EA 2-100)

My guess is that the article was withdrawn when Einstein realized that Grommer's suggested route around the non-separability problem failed to work.

This is a crucial text for my argument that the separability problem was all along at the forefront of Einstein's worries about the shortcomings of quantum mechanics. He says here, simply, that separability is a necessary condition on any theory aiming to describe the motions of physical systems. And while the specific instance of non-separability discussed in the "Nachtrag" concerns Einstein's own refinement of Schrödinger's wave mechanics, he surely realized that exactly the same problem infects Schrödinger's original theory. Remember that just one year earlier he had wrongly criticized that theory for failing to satisfy the converse condition (that the product, $\phi_1 \cdot \phi_2$, of any two solutions for the separate systems, ϕ_1 and ϕ_2, should also be a solution for the joint system). Now he has found what is, for him, the right criticism. For Schrödinger's theory fails to satisfy the requirement that any solution of the Schrödinger equation for the composite system be expressible as a product of solutions for the separate subsystems. Quantum mechanics implies interference effects between the two subsystems that make no sense from the point of view of the classical model of independent particles. This is the same problem that first presented itself to Einstein in his 1909 papers on radiation and surfaced again in the curious quantum statistics for material particles in Einstein's 1924-1925 gas theory papers. And it is the same problem that lay behind Einstein's own argument for the incompleteness of quantum mechanics first elaborated in his correspondence with Schrödinger in the summer of 1935, right after the publication of the EPR paper, where the separability problem had been obscured.

6. THE 1927 SOLVAY MEETING

Most of us know about the sequence of thought experiments by which Einstein sought to convince Bohr and others of the inadequacies of quantum mechanics through Bohr's account of his dispute with Einstein (Bohr 1949). On the whole, it is an accurate account, confirmed in some points of detail by other contemporary evidence. But it is not the whole story, and on at least one crucial point--the aim of Einstein's famous "photon-box" thought experiment, it is seriously in error, as are other standard accounts deriving in part from it, such as Jammer's history of Einstein's objections to quantum mechanics (Jammer 1974, 1985). And, even more importantly, most readers of Bohr's review article are unlikely to realize that non-separability was the main issue over which Bohr and Einstein were really arguing. Einstein, of course, understood perfectly well what the issue was (the "problem" is EPR):

> Of the "orthodox" quantum theoreticians whose position I know, Niels Bohr's seems to me to come nearest to doing justice to the problem. Translated into my own way of putting it, he argues as follows:
>
> If the partial systems A and B form a total system which is described by its ψ-function $\psi(AB)$, there is no reason why any mutually independent existence (state of reality) should be ascribed to the partial systems A and B viewed separately, <u>not even if the partial systems are spatially separated from each other at the particular time under consideration</u>. The assertion that, in this latter case, the real situation of B could not be (directly) influenced by any measurement taken on A is, therefore, within the framework of quantum theory, unfounded and (as the paradox shows) unacceptable. (Einstein 1949, pp. 681-682)

What I want now to do is to review the history of Einstein's <u>Gedankenexperimente</u> with the explicit aim of showing how, through it all, non-separability was the real issue that Einstein was trying to bring to the fore.

The first of the famous <u>Gedankenexperimente</u> dates from the 1927 Solvay meeting, held in Brussels from 24 through 29 October. As reported by Bohr (1949, pp. 211-218), Ehrenfest (letter to Goudsmit, Uhlenbeck, and Dieke, 3 November 1927, reprinted in Bohr 1985, pp. 415-418), and others (for example, Heisenberg 1967, pp. 107-108), most of the interesting discussion between Bohr and Einstein took place outside of the organized conference sessions, at breakfast and during walks between the hotel and the meeting. What exactly Bohr and Einstein discussed is not as clear as it might be, because the records left by them differ in crucial ways. They agree in placing at the center of those discussions the precursor of what we now call the single-slit diffraction experiment. But what that experiment was supposed to show, and what else the dicussion touched upon is not clear.

Consider first the published version of Einstein's contribution to the general discussion, which seems to have taken place on Friday, 28 October, just before the close of the conference (Einstein 1927c). Since the discussion took place near the end of the conference, and since Einstein submitted the written version of his remarks a month later,[21] and thus with ample time for reflection, it seems safe to assume that this version represents in some sense the culmination of Einstein's thinking on that occasion.

After sketching the single-slit experiment, Einstein remarks that there are two ways of interpreting the quantum theory in such a context. What he calls "Interpretation I" is essentially the ensemble interpretation that he insisted in later years is the only tenable way of understanding the quantum theory. On this view, the wave function is associated with a large collection of similar systems; in the present case Einstein says it refers to an "electron cloud" corresponding to "an infinity of elementary processes" (p. 101). "Interpretation II," presumably that favored by defenders of the theory like Heisenberg and Schrödinger, regards the quantum theory as "a complete theory of the individual processes" (p. 101), meaning that a wave function is associated with each individual electron, providing a maximally complete description of its behavior. Having remarked that it is only "Interpretation II" that permits us to explain energy-momentum conservation in individual events and thus results such as those found in the Bothe-Geiger experiment, Einstein says that he nevertheless wants to make some criticisms of this interpretation:

> The scattered wave moving towards P does not present any preferred direction. If $|\psi|^2$ was simply considered as the probability that a definite particle is situated at a certain place at a definite instant, it might happen that <u>one and the same</u> elementary process would act <u>at two or more</u> places of the screen. But the interpretation according to which $|\psi|^2$ expresses the probability that <u>this</u> particle is situated at a certain place presupposes a very particular mechanism of action at a distance which would prevent the wave continuously distributed in space from acting at <u>two</u> places of the screen. In my opinion one can only counter this objection in the way that one does not only describe the process by the Schrödinger wave, but at the same time one localizes the particle during the propagation. <u>I think that de Broglie is right in searching in this direction.</u> If one works exclusively with the Schrödinger waves, <u>interpretation</u> II of $|\psi|^2$ in my opinion implies a contradiction with the relativity postulate.
>
> I would still like briefly to indicate two arguments which seem to me to speak against viewpoint II. One is essentially connected with a multidimensional representation (configuration space) because only this representation makes possible the interpretation of $|\psi|^2$ belonging to interpretation II. Now, it seems to me that there are objections of principle against this multidimensional representation. In fact, in this representation two configurations of a system which only differ by the permutation of two particles of the same kind are represented by two different points (of configuration space), which is not in agreement with the new statistical results. Secondly, the peculiarity of the forces of acting only at small <u>spatial</u> distances finds a less natural expression in the configuration space than in the space of three or four dimensions. (Einstein 1927c, pp. 102-103)

This text should be better known, if only for Einstein's having here raised, for the first time that I know, the problem of the non-relativistic character of wave-packet collapse. (This criticism echoes the problem of distant

[21] The German manuscript of a fragment of Einstein's remarks (EA 16-617), included in a letter of Einstein to Lorentz (the conference organizer) of 21 November 1927 (EA 16-615), carries a notation in an unknown hand in the upper left-hand corner: "Allg. Disk Freitag" ("Gen[eral] Disc[ussion] Friday").

correlations between events of detection and non-detection explored in Bothe's 1926 experiment, the one whose results Einstein excitedly reported to Lorentz. And on at least one earlier occasion, Einstein had worried that matrix mechanics might not be generally covariant, though for entirely different reasons; see Einstein to Ehrenfest, 12 February 1926, EA 10-130). But what interests me more is his next criticism, the one having to do with the new statistics.

Recall Einstein's letter to Schrödinger of 28 February 1925, written right after publication of his second and third gas theory papers, where he so clearly explained to Schrödinger both the structure of the two-particle state space presupposed by Bose-Einstein statistics and how this structure is related to the failure of statistical independence between two Bose-Einstein particles. Though he did not say so explicitly--there was no need for one master of statistics to remark on such a triviality to another master--one could have made the point about the failure of independence by noting that the two-particle state space is <u>not</u> simply a product of the two one-particle state spaces, as would be the case with classical, two-particle Boltzmann statistics. What Einstein is now saying is that something is fundamentally wrong with Schrödinger's employment of configuration space, because the two-particle configuration space <u>is</u> the product of the two one-particle configuration spaces, contrary to what must be the case in order to derive Bose-Einstein statistics. Of course Einstein is mistaken here, but not about the structure of the two-particle configuration space. His error is his not understanding that the <u>state space</u> of Schrödinger's (and Heisenberg's) quantum mechanics is <u>not</u> configuration space, but instead a rather differently structured Hilbert space (in the now-standard representation), and that in the two-particle Hilbert space, only a single ray (vector), and hence a single quantum state, is associated with the two mentioned configurations, so that the derivation of the novel statistics proceeds without difficulty. But the fact that Einstein was thus mistaken is less important for our purposes than the fact that he was still brooding about the quantum mechanics of composite systems.

One thing puzzles me about Einstein's thinking at this time. Earlier in 1927, in May, he had identified non-separability as a principal failing, from his point of view, of Schrödinger's wave mechanics. But now, in October, he is still committed, apparently, to the new statistics he had helped to introduce, and clearly aware that the novelty of these statistics is connected to the failure of traditional assumptions about the statistical independence of systems, which is to say the failure of the probabilities to factorize. From Born's statistical interpretation of the wave function, with which Einstein was well-acquainted, it is but a short step to making the connection between the non-factorizability of the probabilities in Bose-Einstein and Fermi-Dirac statistics and the non-separability of two-particle wave functions. Why Einstein seems not yet to have made that connection is a mystery to me, all the more so since the derivation of the new statistics from wave-mechanical fundamentals had already been accomplished by Dirac (1926).

Surprisingly, virtually none of Einstein's published objections to the quantum theory at the 1927 Solvay meeting are reported in Bohr's well-known account (Bohr 1949), and this in spite of the fact that Bohr footnotes that publication (Bohr 1949, p. 212). Bohr does allude to the wave-packet collapse problem: "The apparent difficulty, in this description, which Einstein felt so acutely, is the fact that, if in the experiment the electron is recorded at one point A of the plate, then it is out of the question of ever observing an effect of this electron at another point (B), although the laws of ordinary wave propagation offer no room for a correlation between two such events" (Bohr 1949, pp. 212-213). But he quickly moves on to emphasize a different cluster of issues:

> Einstein's attitude gave rise to ardent discussions within a small circle, in which Ehrenfest . . . took part in a most active and helpful way. . . . The discussions . . . centered on the question of whether the quantum-mechanical description exhausted the possibilities of accounting for observable phenomena or, as Einstein maintained, the analysis could be carried further and, especially, of whether a fuller description of the phenomena could be obtained by bringing into consideration the detailed balance of energy and momentum in individual processes. (Bohr 1949, p. 213)

And then, while claiming to "explain the trend of Einstein's arguments," Bohr goes on to introduce the familiar refinements--a movable diaphragm and the addition of a second diaphragm with two slits--all by way of elaborating his own complementarity interpretation that precludes measurements more accurate than those permitted by the uncertainty relations on the grounds that the requisite experimental arrangements would be mutually exclusive.

We have no detailed independent record of the 1927 discussions between Einstein and Bohr, so for all we know this may well be an accurate account. It is true that Einstein had doubts about the uncertainty relations, and it is true that he held out hope for a more complete fundamental theory. Moreover, the only piece of contemporary evidence of which I know, namely, Ehrenfest's letter to Goudsmit, Uhlenbeck, and Dieke of 3 November 1927, largely confirms Bohr's account:

> It was delightful for me to be present during the conversations between Bohr and Einstein. Like a game of chess. Einstein all the time with new examples. In a certain sense a sort of Perpetuum Mobile of the second kind to break the UNCERTAINTY RELATIONS. Bohr from out of the philosophical smoke clouds constantly searching for the tools to crush one example after another. Einstein like a jack-in-the-box: jumping out fresh every morning. Oh, that was priceless. But I am almost without reservation pro Bohr and contra Einstein. (Quoted in Bohr 1985, p. 38)

Still, my instincts tell me that something is not right about the Bohr (and Ehrenfest) account of the Bohr-Einstein discussion; at the very least they put the emphasis in the wrong place.

There is no reason to doubt that Einstein offered <u>Gedankenexperimente</u> aiming (at least in part) to exhibit violations of the uncertainty relations; it would have been the kind of intellectual game that Einstein so enjoyed. Moreover, he had a special reason to dispute the uncertainty relations with Bohr in particular, because six months earlier, in a letter to Einstein of 13 April, Bohr had deployed the uncertainty relations in disputing Einstein's interpretation of the Rupp experiment as favoring, unambiguously, a wave-like conception of radiation (see Bohr 1985, pp. 418-421). But if the uncertainly relations <u>really</u> were the main sticking point <u>for Einstein</u>, why did Einstein not say so in the published version of his remarks, or anywhere else for that matter in correspondence or in print in the weeks and months following the Solvay meeting? My guess is that it is because any doubts Einstein had about the validity of the uncertainty relations were secondary to his deeper worries about the way quantum mechanics describes composite (interacting) systems.[22]

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[22] Harvey Brown has also noted that Einstein's published remarks at the 1927 Solvay meeting are not directed toward questioning the uncertainty relations, as Bohr claims, and that these remarks instead anticipate the 1935 EPR argument; see Brown 1981, p. 61.

One clue that supports my interpretation is provided by Bohr's own account of the discussion. As Bohr tells it, the general drift of the discussion between himself and Einstein during the week of the 1927 Solvay meeting was toward an ever more careful consideration of the "detailed balance of energy and momentum in individual processes," meaning, as he explains, an ever more careful consideration of the interaction between the electron and the diaphragm. Einstein evidently argued that by measuring the recoil momentum of the diaphragm, one could predict accurately the lateral component of the electron's momentum, and that when this information was combined with the particle's position, as defined by the aperture in the diaphragm, one could thus predict the precise position at which the electron would hit the screen, whereas the quantum theory yields just probabilities for its hitting various points of the screen. Bohr replied with the standard complementarity argument, namely, that in order to measure the diaphragm's recoil momentum one would have to detach it from its mount so that it could move freely in the lateral direction, but that in doing so one thereby loses all precise knowledge of the particle's position when it passes through the slit, since the slit's location is now indefinite.

What this means is that as the discussion between Bohr and Einstein progressed, what may have begun as doubts about the implications of the uncertainty relation for the description of the individual electron evolved into a discussion of the quantum mechanical two-body problem, the two bodies being the electron and the diaphragm. In Bohr's own words: "As regards the quantum-mechanical description, we have to deal here with a two-body system consisting of the diaphragm as well as of the particle" (Bohr 1949, p. 216). And later on, after describing how the situation is made even more vivid by consideration the two-slit diffraction experiment, Bohr remarks, in words reminiscent of his reply to EPR: "We . . . are just faced with the impossibility, in the analysis of quantum effects, of drawing any sharp separation between an independent behaviour of atomic objects and their interaction with the measuring instruments which serve to define the conditions under which the phenomena occur" (Bohr 1949, p. 218). There is a very good reason why the discussion may have taken these turns: first from a consideration of the adequacy of the wave function as a description of an individual electron to the validity of the uncertainty relations, and then from the uncertainty relations to the quantum mechanical two-body problem and non-separability.

Remember that Einstein had been worrying since at least the early 1920s about how to reconcile a probabilistic description of individual systems with the conservation laws. Einstein's 1916 papers on radiative transformations had shown him that an element of chance would likely have to enter an adequate future quantum theory; and Einstein knew as well that some kind of wave-like character had to be associated with both light quanta and material particles to explain interference effects. Einstein's "ghost fields" or "guiding fields" accomplished both ends. But recall Wigner's report of Einstein's own reasons for never having pushed the idea of "ghost fields" or "guiding fields". It was that if each of two interacting systems is guided <u>independently</u> (in the statistical sense) by a separate "guiding field" one cannot guarantee energy-momentum conservation, and it was his insistence on strict energy-momentum conservation in individual events that determined his opposition to the BKS theory. This problem was solved by Schrödinger's shifting the wave function from physical space to configuration space, but at the price of non-separability, a failure of independence that Einstein knew from his gas theory papers had to be part of the quantum theory but that he still found too bitter a pill to swallow when confronted by it in wave mechanics. Another expression of Einstein's desire that the behavior of systems such as electrons be determined independently by their own states is his insistence that these systems be represented as <u>localized</u>, particle-like systems. As we saw above, the desire for localization was strengthened by Einstein's worry that a non-relativistic action-at-a-distance would be implied by our taking the wave function itself as real. And remember that, as early as 1924, Einstein was arguing that the wave-like aspects of quantum systems should be seen not as something real, but merely as a convenient device for representing interactions between systems.

So what Einstein wanted was an ontology of (1) independently controlled, localized systems, but also (2) systems that satisfy strict energy-momentum conservation. Classical mechanics and classical field theories, including general relativity, manage to reconcile these two desiderata. Quantum mechanics does not.

One can imagine Bohr responding to Einstein's published 1927 Solvay discussion remarks by emphasizing just this point. Einstein said that "Interpretation II," which associates the wave function with individual systems rather than ensembles, is the only acceptable interpretation because it is necessary in order to secure strict energy-momentum conservation. But then he insists that we understand the system to which the wave function is associated to be strictly localized. Bohr would have pointed out that, according to quantum mechanics, one cannot have both. He had already argued this very point forcefully in his address at the Volta Congress in Como one month earlier: "The very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description" (Bohr 1927, p. 580).[23] And: "According to the quantum theory a general reciprocal relation exists between the maximum sharpness of definition of the space-time and energy-momentum vectors associated with the individuals. This circumstance may be regarded as a simple symbolical expression for the complementary nature of the space-time description and the claims of causality" (Bohr 1927, p. 582). The key to understanding passages like this is realizing that when Bohr talks about "space-time coordination" or "space-time description," he means the kind of description in which quantum systems such as electrons are regarded as localized, the kind of description Einstein preferred, the kind of description that strongly suggests, if it does not actually entail, the separability of such localized systems. And when Bohr talks about the "claims of causality," he means--as he explains himself--strict conservation of sharply defined energy and momentum.

The discussion would have turned from the uncertainty relations to the quantum mechanics of interacting systems, focussing on the interaction between the electron and the diaphragm, because uncertainty intrudes only when one severs conceptually the physical link between the two interacting systems, that is to say, only when one pretends that two really non-separable systems are separable. Consider the position-momentum uncertainty relationship for, say, the x-axis in connection with two interacting systems, the case made famous in the EPR paper. The total linear momentum after the interaction, $p_1 + p_2$, and the relative separation, $x_1 - x_2$, are compatible observables; both can be defined with arbitrary sharpness. It is only the individual momenta and positions, p_1, x_1 and p_2, x_2, that are incompatible, subject to the uncertainty relations. It is only the pure case, non-factorizable joint state that contains all of the correlations necessary to preserve the link between the positions and momenta of the two systems. If

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[23] This is quoted from the version published in Nature in April of 1928, but the progenitor of this specific remark can be found in a manuscript dated as early as 12-13 October, and it is otherwise wholly consistent with the argument of even the earlierst suriving manuscripts of the Como talk. For more on the history of the various manuscripts, see Bohr 1985.

one pretends that the two systems are separable, that means employing a mixture over factorized joint states, not a pure case. Such a mixture can preserve the momentum correlations or the position correlations, but not both. If you choose the former, you get strict momentum conservation, but no spatial localizability; if you choose the latter, you get localizability, but no momentum conservation.

Thus, the reason Einstein cannot get both localizability and energy-momentum conservation is because of the non-separability of interacting systems in quantum mechanics. The point deserves emphasis. Non-separability is the basic phenomenon that distinguishes quantum physics from classical physics; uncertainty is merely a symptom. Uncertainty intrudes only when one pretends to describe the properties of an independent system, there being no really independent systems. As Bohr himself was wont to say, "isolated material particles are abstractions, their properties on the quantum theory being definable and observable only through their interaction with other systems" (Bohr 1927, p. 581). In a separable universe, there need be no uncertainty. So even where uncertainty seems to be the issue, quantum nonseparability is the real heart of the matter.

Whether or not the discussion between Einstein and Bohr actually proceeded in this fashion is impossible to say. I offer this scenario as a reconstruction that at least reconciles the otherwise rather different seeming records published by Einstein and Bohr. But I do think it a plausible scenario, one that helps us to make better sense of the later history of Einstein's objections to the quantum theory. And one further piece of documentary evidence strengthens my conviction that worries about non-separability really lay behind the October 1927 controversy with Bohr. Remember that, for Einstein, it is field theories that provide the clearest embodiment of a separable ontology, each point of the underlying manifold being regarded as endowed with its own, separate, well-defined state, say in the form of a metric tensor. Just one month after the Solvay meeting, in late November, Einstein returned again to the problem of motion in general relativity, the problem he had begun exploring with Grommer late in 1926, taking up now specifically the equation of motion for elementary particles like the electron. His second note on this subject was presented to the Berlin academy on 24 November. In the introduction, he says the following about the results of his investigations:

> This result is of interest from the point of view of the general question whether or not field theory stands in contradiction with the postulates of the quantum theory. The majority of physicists are indeed today convinced that the facts of the quanta rule out the validity of a field theory in the customary sense of the word. But this conviction is not grounded in a sufficient knowledge of the consequences of the field theory. For that reason, the further tracing of the consequences of the field theory with regard to the motion of singularities seems to me, for the time being, still to be imperative, this in spite of the fact that a thorough command of the numerical relationships has been accomplished, in another way, by quantum mechanics. (Einstein 1927d, p. 235)

The whole point of trying to derive the equation of motion from field-theoretic first principles is to show that the motion of a particle is wholly determined by the values of the fundamental field parameters, such as the metric tensor, at points of the manifold immediately adjacent to the particle's trajectory. That is to say that successfully deriving such an equation of motion for elementary particles would rule out any non-separability between interacting particles, since the field would mediate the interaction in a purely local fashion.

7. THE PHOTON-BOX AND BEYOND: 1930-1935

After the 1927 Solvay meeting, Einstein's interest in quantum mechanics dropped off markedly. He devoted himself ever more single-mindedly to developing a unified field theory, hoping, but always in vain, to find thereby a deeper field-theoretic foundation for quantum mechanics. Characteristic of his resigned attitude during this time is a remark in a letter to Ehrenfest of 23 August 1928: "I believe less than ever in the essentially statistical nature of events and have resolved to apply the tiny capacity for work that is still given to me according to my own taste, in a manner independent of the contemporary goings on" (EA 10-186). What little he had to say about quantum mechanics was confined to attempts to articulate yet more clearly why he did not think it to be the final word in fundamental physics.

His next major contribution along these lines, the famous "photon-box" Gedankenexperiment, came at the 1930 Solvay meeting, held in Brussels the week of 20-25 October. Our only record is found in Bohr's later recollection (Bohr 1949) and in manuscript notes for a talk Bohr gave at the University of Bristol a year later, on 5 October 1931 (Bohr 1931). Einstein's only recorded comment in the proceedings of the conference, an inconsequential remark after a talk by Pierre Weiss (Solvay 1930, p. 360), has nothing to do with foundational problems.

The details are well-known. A radiation-filled cavity has in its side a shutter controlled by a clock. The shutter opens for an instant at a definite time, allowing the escape of one photon. Weighing the box before and after the release, we can determine the energy of the emitted photon with arbitrary accuracy, and when we combine this result with the known time of emission, we supposedly have a violation of the energy-time uncertainty relation. Bohr's ironic refutation of the experiment is also well known. He pointed out that the weighing requires that the box be accelerated in a gravitational field, and that this affects the rate of the clock just enough to secure agreement with the uncertainty relations. The irony, of course, is that relativity is here invoked to save quantum mechanics.

On the face of it, this is merely another attempt to find a violation of the uncertainty relations, which is indeed all it might be. But there is evidence that, here again, Einstein's real aim may well have been to bring out the peculiariities, from a classical point of view, of the quantum mechanical account of interactions. The evidence is a letter from Ehrenfest to Bohr of 9 July 1931, written immediately after Ehrenfest had visited Einstein in Berlin. According to Jammer (Jammer 1974, pp. 171-172; 1985, pp. 134-135), from whom most of us have learned about the letter, Ehrenfest reported that Einstein no longer wanted to use the photon-box thought experiment to disprove the uncertainty relations, but "for a totally different purpose" (Jammer 1985, p. 134), the implication being that disproving the uncertainty relations had been the original intention behind the photon-box thought experiment. But Jammer has misread the letter. What Ehrenfest really wrote to Bohr is this: "He said to me that, for a very long time already, he absolutely no longer doubted the uncertainty relations, and that he thus, e.g., had BY NO MEANS invented the 'weighable light-flash box' (let us call it simply L-F-box) 'contra uncertainty relation,' but for a totally different purpose" (BSC-AHQP).[24] Einstein may have wanted to dispute the

[24] "Er sagte mir, dass er schon sehr lange absolut nicht mehr an die Unsicherheitsrelation zweifelt und dass er also z.B. den 'waegbaren Lichtblitz-Kasten' (lass ihn kurz L-W-Kasten heissen) DURCHAUS nicht 'contra Unsicherheits-Relation' ausgedacht hat, sondern fuer einen ganz anderen Zweck."

uncertainty relations in 1927, but as we see by his own testimony to Ehrenfest, that was not his purpose at the time of the 1930 Solvay meeting.

Ehrenfest goes on to explain Einstein's real intention. What Einstein wanted, says Ehrenfest, is a "machine" that emits a projectile in such a way that, after the projectile has been emitted, an inspection of the machine will enable the experimenter to predict either the value of the projectile's magnitude A or the value of its magnitude B, these values then being measurable when the projectile returns after a relatively long time, it having been reflected at some location sufficiently distant (½ light-year) to insure that there will be a spacelike separation between the projectile and the machine at the time we inspect the machine. According to Ehrenfest: "It is thus, for Einstein, beyond discussion and beyond doubt, that, because of the uncertainty relation, one must naturally choose between the either and the or. But the [experimentor] can choose between them AFTER the projectile is already finally under way" (BSC-AHQP).[25] And: "It is interesting to get clear about the fact that the projectile, which is already flying around isolated 'for itself,' must be prepared to satsify very different 'non-commutative' predictions, 'without knowing as yet' which of these predictions one will make (and test)" (BSC-AHQP).[26] The photon-box turns out to satisfy all of the requirements for such a "machine," the two quantities, A and B, being respectively, the time of the photon's return and its energy or color (wavelength).

Jammer is quite right that we see here all of the ingredients of Einstein's later incompleteness arguments, but Jammer's interpretation is skewed by his taking the published EPR argument as a correct guide to Einstein's views, rather than the quite different version first presented in Einstein's correspondence with Schrödinger from the summer of 1935, the version featuring the separation principle. Since we know that separability was the main issue in 1935, we should look for it here in 1931. It's not hard to find.

In fact, the logic of the 1931 version of the photon-box Gedankenexperiment is almost exactly that of Einstein's own 1935 incompleteness argument. The whole point of placing the reflector ½ light-year away is to assure a spacelike separation between the inspection of the photon-box and the projectile. The argument works as a criticism of the quantum theory only if one assumes that the projectile, when thus separated from the box, is, in virtue of that separation and its therefore possessing its own independent reality, wholly unaffected by what we do to the box when we inspect it. But quantum mechanics makes a different assumption. It says that, if we weigh the box, we can predict the color of the returning photon exactly, but that its time of return will be indefinite, whereas if we check the clock, we can predict the time of the photon's return exactly, its color now being indefinite. In other words, quantum mechanics says that the state we ascribe to the photon depends crucially on what we do to the box. What Einstein is thus arguing is that classical assumptions about the separability of previously interacting systems lead to different results than the quantum mechanical account of interactions, and that, if we adhere to these

[25] "Es steht also fuer Einstein ausser Discussion und ausser Zweifel, dass man, wegen der Unsicherheitsrelation natuerlich zwischen dem entweder und oder waehlen muss. Aber der Frager kann dazwischen waehlen, NACHDEM das Projectil endgueltig schon unterwegs ist."

[26] "Es ist interessant sich deutlich zu machen, dass das Projectil, das da schon isoliert 'fuer sich selber' herumfliegt darauf vorbereitet sein muss sehr verschiedenen 'nichtcommutativen' Prophezeihungen zu genuegen, 'ohne noch zu wissen' welche dieser Prophezeihungen man machen (und pruefen) wird."

assumptions of separability, as Einstein, the champion of field theories, clearly thought we must, then the quantum theory must be judged incomplete.

Essentially the same Gedankenexperiment reported to Bohr by Ehrenfest was presented by Einstein himself at a colloquium in Berlin on 4 November 1931 (Einstein 1931). The published report leaves the aim of the experiment unclear, but it is interesting because Einstein is said to have stressed, himself, that the two measurements on the box--the weighing or the reading of the clock--cannot both be performed, just as Ehrenfest had reported to Bohr, indicating again that disputing the uncertainty relation is not Einstein's aim. In Bohr's account of his controversy with Einstein, the demonstration that the two parameters cannot be measured simultaneously was precisely his (Bohr's) triumph over Einstein at the 1930 Solvay meeting. But how could Einstein so easily accomodate this point, if it were really such a devasting critique of his original idea. The answer is that Einstein never intended to assert that both measurements could be performed simultaneously on the box, or at least that such a possibility was never a crucial part of the experiment. Bohr, Jammer, and others have taken it to be crucial only because they wrongly believed that the uncertainty relations, rather than non-separability, was Einstein's real target.

That Bohr was still not clear about the real point of Einstein's argument is evident from the fact that in his 5 October 1931 talk at the University of Bristol (Bohr 1931), three months after Ehrenfest's 9 July letter informing him of how Einstein wanted to use the photon-box experiment, Bohr gave a quite different account of the experiment, essentially the same as in his later recollections (Bohr 1949), presenting it as an objection to the uncertainty relations. If Bohr misunderstood Einstein in this way in 1931, how do we know that he was not guilty of exactly the same misunderstanding in October 1930 and in his later recollections?[27]

Einstein spent three months (11 December to 4 March) in the United States in late 1930 and early 1931, mostly at Cal Tech. He evidently spent some of this time talking with his Cal Tech colleagues Richard C. Tolman and Boris Podolsky about his objections to quantum mechanics. On 26 February 1931 they submitted to the Physical Review a note entitled "Knowledge of Past and Future in Quantum Mechanics" (Einstein, Tolman, and Podolsky 1931), which Einstein himself apparently credited primarily to Tolman (see Einstein 1931, p. 23). The stated aim is to show that, contrary to what some had claimed (see, for example, Heisenberg 1930, p. 20), the past behavior of a particle cannot be known any more precisely than its future behavior; but the ETP Gedankenexperiment (to coin a designation) is of interest for our story because it involves a modification of the photon-box arrangement that permits the study of correlations not between the box and the emitted photon, but between two particles both emitted from the box.

The box is now fitted with two holes opened by the same shutter. One of the two emitted particles travels directly to an observer at O; the other follows a different trajectory, reflected toward O at a great distance from the box. Weighing the box before and after the release of the particles allows us to determine their total energy. ETP argue that measuring the time

[27] Several other authors have questioned the cogency of Jammer's account of the photon-box thought experiment, arguing as I do (but without having examined Ehrenfest's letter to Bohr of 9 July 1931) that a proof of incompleteness was the real aim. See Hooker 1972, p. 78; Hoffmann 1979, pp. 187, 190; Fine 1979, p. 157; and Brown 1981, pp. 67-69. I highly recommend Harvey Brown's account of the matter for its careful consideration of technical matters, and I thank Brown for drawing my attention to the Hooker and Hoffmann references.

of arrival of the first particle at O, along with its momentum, would enable one to calculate the time when the shutter opened and thus to predict both the time of arrival and the energy of the second particle, contrary to the limitations of the uncertainty relation. That being ruled out by the quantum theory, ETP conclude that it must not be possible to measure both the time of the first particle's arrival and its pre-arrival momentum. This may have been how Tolman meant to use the arrangement. But it obviously lends itself to other uses that may have been of more interest to Einstein. Thus, if the particles are once again taken to be photons (whose velocity is a known constant), then one has the option of measuring either the time of arrival of the first photon or its energy and thus predicting either the time of arrival of the second photon, or its energy (color). But the geometry of the experiment (the great distance of the reflector from O) insures that measurements performed on the first photon cannot affect the second, if we assume separability and locality, and thus that both the time of the second photon's arrival at O and its energy correspond to independently real properties of the photon. Once again, the assumption of separability leads to results in conflict with the quantum theory.

The only direct account that Einstein himself ever gave of the history of these Gedankenexperimente was in an exchange of letters with the Cal Tech physicist, Paul S. Epstein in the latter part of 1945, following the publication of an article by Epstein, "The Reality Problem in Quantum Mechanics," in the June issue of the American Journal of Physics (Epstein 1945). Epstein had introduced his own thought experiment--a variation on the two-slit diffraction experiment--to illustrate the central point of the EPR argument. A beam of light, S, is split by a half-silvered mirror N-N', and each resulting beam, S_1 and S_2, is then reflected again by a perfect mirror, M_1 and M_2, respectively, after which the beams are recombined at a second half-silvered mirror O-O' producing two final beams, S_3 and S_4, each of which enters a detector. Consider a beam S of such low intensity that just one photon at a time passes through the apparatus. Epstein says that if the mirrors M_1 and M_2 are fixed, preventing us from determining, by the mirrors' recoil, which path a given photon travels, the reflected beams S_1 and S_2 are coherent and interfere at the second half-silvered mirror O-O', so that by suitably adjusting the geometry of the arrangement we can make all of the emerging photons go into one detector, say that corresponding to S_3. But if mirrors M_1 and M_2 are movable, so that we can tell which path each photon travels, the beams, S_1 and S_2, are incoherent, there is no interference at O-O', and equal numbers of photons show up, on average, in each detector.

Epstein's analysis of the experiment is somewhat confused. In his first, undated letter, Einstein points out that Epstein had spoken glibly of the ψ-function of one of the photons, ignoring its interaction with the mirror. Einstein explains that Epstein has ignored the non-separability of the joint photon-mirror system: "Now I do not understand the following in your treatment of the mirror example with a light quantum. If a mirror is movable (laterally), then the total system is a system with two types of coordinates (e.g. Q for the mirror, q for the quantum). There is then no $\psi(q,t)$ at all, as long as no 'complete' observation of the mirror is at hand. Then, in terms of the theory, one cannot at all ask how $\psi(q,t)$ is constituted as a function of the time t" (EA 10-581). But then, instead of continuing with an analysis of Epstein's experiment, Einstein sketchs his own preferred way of viewing the matter.

He considers two previously interacting particles "described as completely as possible in the sense of the quantum theory by $\psi(Q,q,t)$" (EA 10-581). After a sufficiently long time, the particles have separated, and now we ask "in what sense each individual particle corresponds to a real state of affairs" (EA 10-581). To learn something about q, we perform a measurement on Q, and we can arrange the measurement so that "the $\psi(q,t)$ resulting from this measurement and from the $\psi(Q,q,t)$ has <u>either</u> a sharp position (for a given value of the time), <u>or</u> a sharp momentum" (EA 10-581). In order to decide what the "real state of affairs" is with regard to the second particle, we ask whether the measurement on the first particle has a real, physical influence on the second particle. If there were such an influence (and Einstein thinks Epstein prefers this view), it would mean the existence of superliminal effects, against which Einstein's "physical instinct" struggles, and it would be difficult to see how such effects could be incorporated in the quantum theory. But if the measurement on the first particle has no physical influence on the second particle, "then all of the determinations for the second particle that result from the possible measurements on the first particle must be true of the second particle, if no measurement at all were performed on the first particle" (10-581). It follows that quantum mechanics is incomplete. We recognize here yet another statement of Einstein's own (non-EPR) post-1935 incompleteness argument. In this there is nothing new. What is significant is what follows in the next letter.

Epstein responded on 5 November 1945 (EA 10-582), confessing that he never really understood the EPR paper, that he had in the meantime restudied it, but was still confused by some of the calculations in it. Einstein answered on 10 November (EA 10-583). He begins by declining to discuss the mathematical questions, saying that Schrödinger had settled them in a thorough treatment shortly after the publication of the EPR paper (Schrödinger 1935, 1936). He says, then, that it may be better if he shows Epstein how he himself first arrived at the incompleteness argument: "I myself first came upon the argument starting from a simple thought experiment. I think it would be best for us if I exhibited this to you" (EA 10-583). The arrangement is the following.

A photon-box can move freely in the x-direction. An observer rides with the box and has at his or her disposal various instruments, including a clock for timing the opening of the shutter and tools with which to measure the box's position. Before starting the experiment, the observer allows the box to come to rest, something that can be determined by means of light emitted from the box being reflected from a distant wall. Of course, knowing that the box is at rest, that is, that it has zero momentum in the x-direction means that its position is unknown. Now the experimenter opens and closes the shutter at a definite time, allowing one photon to emerge. At this point, the experimenter has an option to measure one of two things. He or she can <u>either</u> anchor the box to the reference frame, permitting a precise measurement of the box's position, and thus a prediction of the exact time when the emitted photon will be received at some distant location S, which means, says Einstein, a "sharp determination of the position of the photon." <u>Or</u> the experimenter can make a new measurement of the box's recoil momentum, in which case he or she can predict exactly the energy or color of the emitted photon. What does the experiment show? Einstein says:

> As soon as it has left the box B, the light quantum represents a certain "real state of affairs," about whose nature we must seek to construct an <u>interpretation</u>, which is naturally in a certain sense arbitrary.
>
> This interpretation depends essentially upon the question: should we assume that the subsequent measurement we make on B <u>physically</u> influences the fleeing light quantum, that is to say, the "real state of affairs" characterized by the light quantum?
>
> Were that kind of a physical effect from B on the fleeing light quantum to occur, it would be an action at a distance, that propagates with superluminal velocity. Such an assumption is of course logically possible, but it is so very repugnant to my physical instinct, that I am not in a position to take it seriously—entirely apart from the fact that we cannot form any clear idea of the structure of such a process.

> Thus I feel myself forced to the view that the real state of affairs corresponding to the light quantum is independent of what is subsequently measured on B. But from that it follows: Every characteristic of the light quantum that can be obtained from a subsequent measurement on B exists even if this measurement is not performed. Accordingly, the light quantum has a definite localization and a definite color.
>
> Naturally one cannot do justice to this by means of a wave function. Thus I incline to the opinion that the wave function does not (completely) describe what is real, but only a to us empirically accessible maximal knowledge regarding that which really exists. . . . This is what I mean when I advance the view that quantum mechanics gives an <u>incomplete description</u> of the real state of affairs. . . .
>
> If one is of the view that a theory of the character of quantum mechanics is definitive for physics, then one must either completely renounce the spatio-temporal localization of the real, or replace the idea of a real state of affairs with the notion of the probabilities for the results of all conceivable measurements. I think that this is the view that most physicists currently have in mind. But I do not believe that this will prove to be the correct path for the long run. (EA 10-583)

Here we have all of the ingredients of Einstein's own post-1935 incompleteness argument, including, most importantly, the separability principle in the form of the assumption that an independent real state of affairs is associated with the light quantum from the moment it leaves the box.

By Einstein's own account, this <u>Gedankenexperiment</u> and, presumably, the indicated interpretation of it, was the starting point from which the incompleteness argument developed. However, Einstein does not say exactly when the experiment first occurred to him, so we cannot insert it at a definite place in our chronology of Einstein <u>Gedankenexperimente</u> on the basis of any direct evidence. Can indirect arguments be brought to bear? If the Bohr-Jammer account is correct, according to which sometime in the summer of 1931 Einstein changed his mind about how to deploy the photon-box thought experiment, then the "Epstein" experiment had to come later. But Ehrenfest's 9 July 1931 letter to Bohr shows that Einstein did not change his mind about the use to which the experiment was to be put, that he intended it from the start as showing the incompleteness of the quantum theory. If that is the case, then the "Epstein" version of the photon-box arguably came first, as Einstein says. And from one point of view, the 1930 Solvay photon-box is sufficiently simpler than the "Epstein" photon-box that it may be regarded as a refinement of the latter; for simply weighing the box is a lot easier than performing the complicated series of momentum measurements sketched in the letter to Epstein.

If the "Epstein" photon-box <u>Gedankenexperiment</u> goes first in the chronology then, we must revise our understanding of how the 1930 Solvay photon-box was to be deployed in line with the analysis given in Ehrenfest's letter to Bohr. The logic of the argument would have been the same as that in the "Epstein" photon-box. After the photon is emitted, the experimenter can measure either the energy of the box, by a second weighing, or the time of emission, and depending upon which measurement he or she makes, a different prediction can be made about the photon. All that Bohr's famous critique concerns is the question, inessential from Einstein's point of view, whether or not the two measurements on the box can be carried out simultaneously.

From this point on, the tendency of Einstein's thinking is clear. He wanted to show the quantum theory to be incomplete, and he wanted to do this by showing that the assumption of separability (plus the assumption of locality), is incompatible with the claim that the theory gives a complete description of individual systems. All that changes are the details of the Gedankenexperimente intended to demonstrate this.

The first of these new experiments dates from April of 1932. When Einstein returned from his third annual visit to the United States, his ship lay over for three days in Rotterdam, where Ehrenfest came from Leyden to visit him on 4 April (see Jammer 1985, pp. 135-136). The next day, Einstein wrote to Ehrenfest: "Yesterday you nudged me into modifying the 'box-experiment' in such a way that it would employ concepts less foreign to the wave theorists. I do this in the following, where I employ only such idealizations that I know will appear unobjectionable to you" (EA 10-231). The experiment is a modified Compton scattering experiment, in which the scattered photon is assumed to move along the same axis that the scattering mass m is free to move along, the photon being reflected at a distant mirror back to an experimenter who sits near the mass m. Einstein assumes that the mass m is initially at rest (zero momentum) which means that its location is indeterminate. He then argues that when the scattered photon returns to the experimenter, he or she can measure either the photon's momentum, enabling the experimenter to deduce the momentum of the mass m, or the photon's time of arrival, enabling the experimentor to deduce the time when the initial scattering occurred and thus the precise position of m right after the scattering. (There is an obvious error here.) The important point is that we can thus deduce either the position or the momentum of m, without in any way disturbing m itself. Einstein's conclusion comes as no surprise: "Thus, without any experiment on m, it is possible to predict, according to a free choice, either the momentum or the position of m with in principle arbitrary accuracy. This is the reason why I feel myself motivated to attribute objective reality to both. It is to be sure not logically necessary, that I concede" (EA 10-231).

The last documented stage in the development of Einstein's Gedankenexperimente can be dated to sometime during the spring or summer of 1933, when Einstein was staying in Le Coq sur Mer, Belgium after his return to Europe, in late March, in the wake of Hitler's Machtergreifung, and before his final departure for the United States in early September. Léon Rosenfeld was then a lecturer at the University of Liège, and had just finished his famous joint paper with Bohr on the measurability of field quantities in quantum electrodynamics (Bohr and Rosenfeld 1933). He gave a lecture on the topic in Brussels, which Einstein attended. After the talk, Einstein approached Rosenfeld wanting to discuss not the topic of the lecture but the general problem of completeness, about which he said he still felt a certain "uneasiness" ["Unbehagen"]. Rosenfeld quotes Einstein as follows:

> What would you say of the following situation? Suppose two particles are set in motion towards each other with the same, very large, momentum, and that they interact with each other for a very short time when they pass at known positions. Consider now an observer who gets hold of one of the particles, far away from the region of interaction, and measures its momentum; then, from the conditions of the experiment, he will obviously be able to deduce the momentum of the other particle. If, however, he chooses to measure the position of the first particle, he will be able to tell where the other particle is. This is a perfectly correct and straightforward deduction from the principles of quantum mechanics; but is it not very paradoxical? How can the final state of the second particle be influenced by a measurement performed on the first, after all physical interaction has ceased between them? (Rosenfeld 1967, pp. 127-128)

Through the haze of Rosenfeld's again not unbiased recollection we can recognize here the same logic that is by now quite familiar, but elaborated

against a Gedankenexperiment that is growing ever more refined, toward the conceptual purity of the EPR-type experiment.

8. CONCLUSION

After 1935, Einstein's reasons for thinking quantum mechanics incomplete were intimately connected to his firm belief in the separability principle. It is the separability principle that licenses the crucial inference that the undisturbed system in EPR-type Gedankenexperimente has its own unique separate state independently of any measurements we might carry out on the other system. And if, according to the quantum theory, measurements on the other system lead us to ascribe different states (different psi-functions) to the undisturbed system depending upon the kind of measurement we perform on the first system, then it follows that quantum mechanics does not yield a complete description of undisturbed system. But, of course, as Bohr pointed out, quantum mechanics denies the separability of previously interacting systems. Einstein understood quite well that it was this disagreement over separability that stood between him and Bohr, as evidenced by his remark to Schrödinger in the summer of 1935: "One cannot get at the talmudist [Bohr] if one does not make use of a supplementary principle: the 'separation principle'" (Einstein to Schrödinger, 19 June 1935, EA 22-047). But Einstein was committed to separability because of his deeper commitment to field theories, and their associated way of describing interactions in purely local terms, a description that rests fundamentally on the assumption that every system, indeed, every point of the space-time manifold, has its own separate state in the form of well-defined values of the fundamental field parameters like the metric tensor.

In this paper I have been arguing that Einstein's worries over the way quantum mechanics describes interacting systems did not begin in 1935. On the contrary, I have shown that the puzzling behavior of interacting quantum systems had been at the forefront of Einstein's concern from at least 1909 and that these worries began to crystallize in the mid-1920s into the belief that, because it regards interacting systems as non-separable, quantum mechanics would be fundamental inadequate. And I have argued, finally, that the real aim of the famous series of Gedankenexperimente starting at the 1927 Solvay meeting was to bring out precisely this feature of the quantum theory and to exhibit the, to Einstein, unacceptable consequences to which it leads. That is to say, I argued that Einstein's concern over the uncertainty relations and the breakdown of strict causality in quantum mechanics was secondary to his deeper concern over the quantum mechanical account of interactions.

One important test of this reconstruction of Einstein's views would be to determine whether or not Einstein's contemporaries understood his reservations about the quantum theory in this manner. Let me show that this was the case by quoting from just two letters written right after the publication of the EPR paper.

The first is a letter from Pauli to Heisenberg of 15 June 1935, in which Pauli prodded Heisenberg into composing a "pedagogical" reply to EPR, a reply that, unfortunately, was never published.[28] Pauli writes:

[28] For the text of Heisenberg's reply, "Ist eine deterministische Ergänzung der Quantenmechanik möglich?", see Pauli 1985, pp. 409-418; Heisenberg enclosed a copy with his letter to Pauli of 2 Juli 1935. Another copy, in typescript, is in the Einstein Archive, EA 5-207.

Einstein has again expressed himself publicly on quantum mechanics, indeed in the 15 May issue of Physical Review (together with Podolsky and Rosen--no good company, by the way). As is well known, every time that happens it is a catastrophe. "Weil, so schließt er messerscharf--nicht sein kann was nicht sein darf" (Morgenstern). . . .

He now understands this much, that one cannot simultaneously measure two quantities corresponding to non-commuting operators and that one cannot simultaneously ascribe numerical values to them. But where he runs into trouble in this connection is the way in which, in quantum mechanics, two systems are joined to form a composite system. . . .

A pedagogical reply to [this] train of thought must, I believe, clarify the following concepts. The difference between the following statements:

a) Two systems 1 and 2 are not in interaction with one another (= absence of any interaction energy). . . .

b) The composite system is in a state where the subsystems 1 and 2 are independent. (Decomposition of the eigenfunction into a product.) . . . .

Quite independently of Einstein, it appears to me that, in providing a systematic foundation for quantum mechanics, one should start more from the composition and separation of systems than has until now (with Dirac, e.g.) been the case. -- This is indeed--as Einstein has correctly felt--a very fundamental point in quantum mechanics, which has, moreover, a direct connection with your reflections about the cut and the possibility of its being shifted to an arbitrary place. . . .

NB Perhaps I have devoted so much effort to these matters, which are trivialities for us, because a short time ago I received an invitation to Princeton for the next winter semester. It would be fun to go. I will by all means make the Morgenstern motto popular there. (Pauli 1985, pp. 402-404)

Notice that Pauli uses the past perfect tense: "as Einstein has correctly felt" ["wie Einstein richtig gefühlt hat"]. What he is characterizing here is not what he has just learned from the EPR paper, in which the issue of separability is anyway almost totally obscured (see Einstein to Schrödinger, 19 June 1935, EA 22-047); instead, he is describing the view that he has long associated with Einstein.

The second letter is from Schrödinger to Pauli, sometime between 1 July and 9 July 1935. It was prompted by Arnold Berliner's having asked Schrödinger to write a reply to EPR for Die Naturwissenschaften and Berliner's having told Schrödinger that Pauli was quite agitated about the matter. Schrödinger portrays Einstein's fundamental view of the nature of reality, the view lying behind the incompleteness argument, as follows: "He has a model of that which is real consisting of a map with little flags. To every real thing there must correspond on the map a little flag, and vice versa" (Pauli 1985, p. 406). This is Schrödinger's marvelously vivid way of characterizing Einstein's view of the fundamental ontology of field theories, the ontology which gives the most radical possible expression to the separability principle. Schrödinger has rightly discerned that it is this fundamental commitment that animated Einstein's opposition to quantum mechanics.

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