Sixty-Two Years of Uncertainty
Historical, Philosophical, and Physical Inquiries into the Foundations of Quantum Mechanics

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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.

²See Bohr 1949 and Einstein 1946.
³See, for example, Ehrenfest to Goudsmith, Uhlenbeck, and Dieke, 3 November 1927 (quoted in Bohr 1985, p. 38); see also Rosenfeld 1967.
⁴The accounts by Harvey Brown (1981), Arthur Fine (1979), Clifford Hooker (1972), Max Jammer (1974, 1985), Abraham Pais (1982), and John Stachel (1996) 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.
⁵To 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.
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 flaws in Einstein’s arguments, culminating in a 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. More importantly, from a very early date, at least by 1925, Einstein was pondering the curious failure of classical assumptions about the independence of interacting systems made vivid in the new Bose-Einstein statistics. Earlier still, certainly by 1909, Einstein had realized that the Planck formula for black-body radiation cannot be derived from the assumption that light quanta behave like particles in the gases described by classical statistical mechanics. And by spring 1927, Einstein had recognized that quantum mechanics (or at least Schrödinger’s version of it) fails to satisfy the kind of thought experiment 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, secondary concerns, arising from the 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 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 reasoning that was used 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 decomposable into separate “wave” functions for the subsystems. 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 hint 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 on the just-mentioned, characteristically Einsteinian assumption about the independence of interacting systems, the assumption Einstein himself here dubbs 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 early entanglement ideas and a reconstruction of his argument that the inability of quantum mechanics to describe complete quantum systems is necessarily incomplete when applied to systems assumed to be separable.

2. EINSTEIN ON LOCALITY AND SEPARABILITY AFTER EPR

The Einstein-Podolsky-Rosen (EPR) paper is still commonly taken to represent the definitive statement of Einstein’s mature misgivings about the quantum theory. As such, the argument found the standard 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 violating special relativity, we can predict with certainty the 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 simultaneously have definite values. One of Schrödinger's axioms of 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 than 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 incompleteness 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 republished and refined in a series of papers and other writings between 1936 and 1949. In outline, it is this: A complete theory must assign to every real event a unique state of every physical system. But in EPR-type experiments involving two well-isolated systems, the state of one is determined only by the state of the other. Quantum mechanics assigns different theoretical states to each system, different "psi functions," to one and the same real state of the other, and thus 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. Locality says that a state is unaffected by events in regions of space that are separated from it by a spacelike interval. Separability says 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). It follows that system A has its own well-defined real state from the moment the interaction between A and B ceases, and that a 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 chosen for measurement on B. Thus, Einstein claims that the incompleteness of quantum mechanics—the 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 foundation 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. 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 that 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 unanswerable) arguments—methodological, ontological, 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 separability principle bears elaboration. In one sense, two systems of quantum mechanics 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, in the sense of the weakened well-definedness of its own separability principle, they are independent in the sense that each possesses its own separate reality. 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, the kind we consider here 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 each by itself. In such cases, the quantum mechanical account of interactions manifests itself precisely in the fact that joint probabilities do not thus factorize.

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" ("lochabstraktum") was his regarding the points of the manifold as being only implicitly defined as the intersections of world lines. For details, see Stachel 1989.

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 happened near the beginning of 1925, when Einstein wrote 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, that enter the Boltzmann equation, $\rho = \exp(-\beta E) / Z$, 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 certain statements obscure" (EB 10-009). 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 foretold his enthusiasm for the topic. The first of these papers was presented to the Prussian Academy on 10 July 1924 (Einstein 1924a), 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 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 57 of the second paper is titled: "Comparison of 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 approved 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 1925b, 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—of 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 complexes 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:

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According to Bose the molecules stack together relatively more often than according to the hypothesis of the statistical independence of the molecules. (EB 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 individual 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 "in-
distinguishable," particles. This is meant to suggest that 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 do in classical Boltzmann statistics; hence, cases two and three in the
classical statistics must be regarded as just one case 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 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 one sees 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 some time,
and "Here is particle B," at some later time, t; the particle observed at t
could 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 identifi-
ability of the classical particles, representing the Bose-Einstein particles
by unlabeled dots.) It is equally misleading to speak here of "indistin-
guishability" of particles. For even in Bose-Einstein statistics, we can know that
in case there are different particles, we just cannot tell which is which.

Another common way of characterizing the novelty of Bose-Einstein sta-
tistics is to say that such statistics are appropriate for interacting parti-
cles evincing the wave-like aspect of de Broglie's description of material
matter. As we shall see, it is wrong to credit the idea of material
particles possessing simultaneously a wave aspect 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
unit--if not by de Broglie's later view that a dual nature was both
then massive particles should as well. But otherwise this characterization of
the innovation represented by Bose-Einstein statistics is not incorrect,
in--amending the fact of evading the new statistics in Bose-Einstein statistics can
be regarded as necessary in 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 me-
chanics, Planck (1900) elaborated this suggested way of visualizing the
influence of quantum mechanical interpretation of the statistics
(Schrödinger 1926a). Still, it is striking that Einstein himself did not
merely state 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 de Broglie'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 and noted 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
displacement phenomena can just be explained in terms of a quantum
field 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
(Planck 1924a, p. 93)."

What is interesting here is Einstein's scepticism regarding the reality of matter waves (and even the wave nature of photons), his suggestion that the effects commonly regarded as symptoms of 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 the device of wave-theoretical
representations; there is no artificial, a convenient tool, a vivid
image, for helping us to think clearly about quantum interactions and sta-
tistical 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 also be found in space-like separated systems.

But there is another 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 about-face, he returns back to the main topic of the talk, 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
dispose with the ether in theoretical physics. The idea of ether, 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 1924a, p. 93).

Recall how a continuous field theory like general relativity incorpo-
rates 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. This should be the metric tensor in general relativity. Within this framework, action
is explained in terms of a change in the fundamental parameter being propa-
gated 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 adja-
cent points. What is not allowed is for the value of the fundamental param-
eter at one point to be immediately functionally dependent upon values at
bistant points. It is the restriction to local action so conceived that
Einstein had in mind when he said that all "local-action theories" assume
correct. The General relativity, through its incorporation of the
first-signal principle, is even more restrictive in this regard than classi-
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 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 account for a tendency to ensure 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 process of 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 the Einstein himself stresses, 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 connection 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)

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 is too large. The initial state is left unchanged in the final state.

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, subliminally, 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 on B's state (the virtual field) of B, which in turn can depend on the probability of a transition at A. And the probability of a transition at B can depend on the probability of various transitions at B. On the other hand, the probability of a transition at A is further dependent on 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 causal. By contrast, the second 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 non-causal. But dependence of the latter kind threatens classical models of local interaction, with prohibitions on "distant action"; it was general relativists' 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 theoretical papers, all in the period 1924-1925. They appeared in 28 or 29 May, within days of the paper's German publication.14 What may be a list of objections to the theory prepared for that occasion survives in the Einstein Archive (EA 8-0261) under the title "Einfaches Briefchen gegen Bohr—Kramers." 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 Innbrucker conference.

14Rudolf Ladenburg to Kramers, 8 June 1924, as quoted in Bohr 1984, p. 27, giving the date as 28 May. But Wigner (1980, p. 441) reports that the colloquium took place regularly on Thursdays, which would make the date 29 May.
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 the individual cases. This was only Schrödinger's illusion in an interpretation 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 found in Bose-Einstein statistics. But as we shall see in due course, 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 1922, 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 their statistical considerations about ghost fields and statistical independence of light-field mover-particle fields. And his reasons for objecting to the BKS theory are similar to the reported reasons in Einstein's own ideas along this line; as his letter to Ehrenfest of 31 May 1924 he says of the BKS theory: "This ideas 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 is in instant, both would follow a guiding field. But those guiding fields give only the probabilities of the directions in which the two components, the light quantum and the electron, will be reflected through the collision. It may happen that in one collision the light quantum is strongly deflected, the electron very little. And 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 best determine probabilistically the motions of individual particles or the transitions in individual

states. 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 the individual cases. This was only Schrödinger's illusion in an interpretation 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 found in Bose-Einstein statistics. But as we shall see in due course, 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.

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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 quantify 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 $h\nu / N$" (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 some reflection, W = $W_1 W_2$, 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, $S = k \log W$, as Einstein thought it did in his 1905 paper, 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. 160).

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 Strahlungproblems" (Einstein 1909a).

The context was yet another attempt to understand the relationship between his own light quantum hypothesis, which by then had become universally accepted, 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. The kind 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, $(k\nu / h\nu) N_{\nu}$, were it alone present, would yield fluctuations "as if the radiation consisted of pointlike quanta of energy $h\nu$ moving 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 also include those more 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 essay 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. Shortly after Einstein read Lorentz's influential lecture on the radiation problem delivered to the 1908 International Congress of Mathematicians in Rome (Lorentz 1908a). Lorentz had by this time fully accepted Planck's radiation theory and had even adapted the Rayleigh-Jeans formula (see Lorentz 1908b), but in a letter to Einstein of 6 May 1909 (EA 6-418), 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 relatively small apertures. The best way to understand the fluctuations in the radiation formula. 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 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 ideal.

As I already said, in my opinion one should not think about constructing light out of discrete, mutually independent points. 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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11See Einstein to Lorentz 13 April 1909 (EA 70-139); Einstein read the 1909 reprinting in the Revue générale 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 Entwicklungsunserer 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 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 quantum cannot be regarded as independent is reflected in his observation that the emergence of what he later termed “force fields” having the character of “plane waves.” He concludes by noting that in introducing the 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 invented with the kind of independence another standardly assumed for the systems of particles to which classical statistical mechanics applies, and, second, his search for a theoretical model of quantum mechanics that would accommodate 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 Einstein himself allowed that the classical manner of counting complexons had to be modified after the manner of Planck’s counting rule, though the theoretical footnotes of the latter remained obscure. It was only quantum papers of Bose and Einstein in 1924-1925 that began to clarify these matters.

There is, however, one individual whose name 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 Mieczyslaw Wolfke, a young Polish physicist who took a degree under O. Lummer at Breslau in 1910 and became a Privatdozent at the ETH in 1913. He moved to the University of Zurich 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 O. Krutkow (1916) 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 Pre-suppositions,” 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. Nonetheless, 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 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 (Wolfke 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 in 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 a light quanta of the same frequency enclosed in a volume v find themselves at an arbitrary moment of time in the volume x of the volume v. The expression for this probability reads:

\[ W = (x/v)^a. \]

This probability may be interpreted as the product of the individual probabilities y/x that an individual one of the light quanta under consideration lies in the subvolume y at an arbitrary moment of time. From the fact that the total probability W is expressed as the product of the individual probabilities y/x, 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 probabilities for each to occupy its own location—just as was assumed in classical statistical mechanics, but it must be denied in order to derive the Planck formula.\footnote{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 with the gas of a different kind. The total entropy of the system is then 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 the whole system. To make the paradox clear, Einstein wrote that “when the gases are of the same kind, they must agree with regard to Y [phase velocity] and Y [frequency]. Moreover . . . it is necessary that Y varies as N, and nearly agrees 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 joint probabilities 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 1902 to the two terms in his expression for fluctuation in radiation pressure in black body radiation. Only now, of course, we are talking about massive particles rather than massless photons. 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 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

\footnote{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 assuming an independence with respect to the instantaneous moments 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 “act independently of one another” (Einstein 1909a, p. 189; emphasis mine).

\footnote{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 “Quantumzellen” 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 Mehr 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 felt came to regard as fundamentally inadequate. And my hypothesis is that his position for regarding the quantum theory, his reason for turning away from active work on it, is 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 conclusion 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 by de Broglie's thesis (de Broglie 1924). It is as if once that they cannot be independent, distinguishable particles of the kind that classical mechanicals, 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 Ramanauer effect (Ramanauer 1920, 1921a, 1921b) as an interference phenomenon he understood (1925). Ramanauer 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 are being studied. 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 Ramanauer's claim as "a most strange" (Born 1921b, 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 Ramanauer's effect could be interpreted quite straightforwardly as a result of interference between the electrons and the atom in the gas.

One important figure had himself been thinking independently about the Ramanauer 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 Ramanauer'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 1925, 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 Ramanauer concerning the penetration of slow electrons through atoms that especially do not fit in with the assumed viewpoint. Encouraged by these results we 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 1925, 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 Ramanauer 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 it. Schrödinger (1925, 1926a) was one of these, while Planck (1925), and particularly de Broglie, was another. It was the physical significance of the denial of independence in Bose-Einstein statistics. How could spatially localized material particles fail to be independent if 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 (1925a), sought to develop consistent wave-theoretical interpretations of the new statistics that would make the only way to understand the non-independence of interacting systems. Indeed, Elsasser himself, in proposing his interpretation of the Ramanauer effect as a wave-like interference phenomenon, wrote of Einstein's "detour through statistics" (Elsasser 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, in-capable of further explanation. On the contrary, the new statistics should 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 in a place where it is possible without a sacrifictum intellectus. (Schrödinger 1926a, pp. 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. Wolfle 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 "other 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 same model of cavity radiation that corresponds to what is still not 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 a new formulation of the Einstein--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 the meaning 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 sign a letter supporting his interpretation of the wave mechanics. This had the effect of creating a controversy over the Faust exclusion principle in the field of wave mechanics (Schrödinger 1926b). 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 in the field of wave mechanics (Schrödinger 1926b), and was investigating the possibility of a spin--1 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 Zeitschrift für Physik between 18 September 1925 and 4 February 1926. Einstein wrote in a letter to Ehrenfest: "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 impressive 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 to忙 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 that "We must still judge whether the same thing in a less complicated way, 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. His wave, his quanta! both realities stand rock solid. Aber der Teufel macht einen VersdrAf (der sich wirklich reinnt). (EA 10-144)"

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

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 radiation as an entirely different system of equations of motion. He has shown that the De Broglie waves play in phase space. The things appear in the Annalen. No such internal framework, 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 ideas." (EA 16-600).

Schrödinger's first wave-mechanics paper had appeared in the Annalen 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 appeared 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 spaces 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 as one need not hardly consider them as a real attempt to be accepted on its own merits. The field in a n-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 clearly formulated than that of Heisenberg: namely, Schrödinger's hard to place an indication 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 course, one would like to obtain a clearer understanding 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 ones. The Heisenberg--Dirac theories compel my admiration, but to me they don't smell like reality." (Herrmann 1968, p. 103). On 28 August he wrote to Ehrenfest: 

"Admirably—but 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 waves in 4-dimensional coordinate space are not understandable, 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. His waves, his quanta! both realities stand rock solid. Aber der Teufel macht einen Vers drauf (der sich wirklich reinnt). (EA 10-144)"

14See 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.
Schrödinger’s theory make a great impression, and yet I do not know whether it is comprehensible to anything more than the old quantum rules, i.e., a question of something corresponding to an aspect of the real 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 (carnal ray). There was controversy about whether the experimental design was sufficient to yield an unambiguous decision. 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. 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 were 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. 

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, I have had doubts and I would like to bring them to your attention. I have been told that you believe that there may be a need to abandon the concept of a wave function (which you are so brave as to say: ‘true genius’). But this worries me, because your two systems that are not at all coupled with one another, and which I have a possible value of the energy of the first according to quantum mechanics, with which I have a certain value of the total system composed of the two. But I do not see how your equation

\[ \text{div grad } \psi = \frac{g}{\hbar} (6 - g) \]

should express this property.

So that you will see what I mean, I set down another equation that would satisfy this requirement:

\[ \text{div grad } \psi = \text{const} \]

14See, for example, Schrödinger to Einstein, 23 April 1926 (EA 22-014), and Einstein to Schrödinger, 24 April (EA 22-018); see also Jone 1926, and Bohr to Einstein, 13 April 1927 (Bohr 1985, pp. 418-421).

15See Einstein 1926b; see also Einstein to Ehrenfest, 18 June 1926 (EA 10-130), and 28 August (EA 10-144), as well as Einstein to Lorentz, 22 June 1926 (EA 16-407).

16In 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 group 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
\[
\begin{align*}
\text{div} \ \text{grad} \ \varphi_1 &= \frac{B_1}{h^2} - \frac{E_1}{h^2} \quad (\text{valid for the phase space of the 1st system}) \\
\text{div} \ \text{grad} \ \varphi_2 &= \frac{B_2}{h^2} - \frac{E_2}{h^2} \quad \varphi_3 = 0 \quad (\text{2nd})
\end{align*}
\]

have as a consequence
\[
\text{div} \ \text{grad} \ (\varphi_1 \varphi_2 + (q^2 + R^2)(\varphi_1 \varphi_2)) = 0 \quad (\text{valid in combined } q\text{-space})
\]

One requires for the proof only to multiply the equations with \( \varphi_1 \) or \( \varphi_2 \) and then add. \( \varphi_1 \varphi_2 \) 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-O12)

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 (RA 22-016) pointing out the same thing. But an slip 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” (RA 22-018). Clearly, Einstein had not yet seen Schrödinger’s proof of the equivalence of wave and matrix mechanics (Schrödinger 1926).

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 to the individual wave equation, \( \psi_1 \) and \( \psi_2 \), for two different systems, then \( \psi_1 \psi_2 \) 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 \( \psi_1 \psi_2 \) is any solution of a joint wave equation for a joint system, then it ought to be equivalent to a product, \( \psi_1 \psi_2 \), 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 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 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 Grossman 1927) and November (Einstein 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 over-determined set of field equations having failed (Einstein 1923). Thus, in summarizing the results of the first paper, Einstein and Grossman 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 Grossmann 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 Heisenberg’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 depth 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. (RA 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 apparently be derived relativistically. But it appears that absolutely nothing ‘unclassical’ is to be obtained thereby” (RA 10-168). 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 prett\(y\), even if there is still a slight anagram 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” (RA 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 astrophysical differentials could be linked to quantum mechanics; and I do not regard it as being at all out of the question” (RA 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

\[^{19}\]The first mention of the problem of motion that I can find in Einstein’s correspondence from this period is in a letter to Born 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.

\[^{20}\]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. The idea of what was lacking in the new quantum mechanics, as was soon made vivid by Heisenberg's quantization of the uncertainty principle (Heisenberg, 1927), was the concept of a wave-like or a trajectory-like establishment which is the aim of the problem of motion. Thus, the criticism of Schrödinger in his now famous letter to Einstein of 11 January 1927 to Ehrenfest concludes: "If do not believe that kinematics must be discarded. Einstein's view is that the ether is not to be considered a number of alternatives, but all within the larger framework of the field-theoretic way of forming systems. 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" (Pail, 1982, p. 333). On 16 February 1927, at the time of the now short notes of his own on the Kaluza-Klein theory (Einstein 1927a), he commented in a letter to Lorentz: "Kaluza-Klein's attempt to unify the unification of gravitation and Maxwell's theory by means of the five-dimensional theory (Kaluza-Klein-Poincaré) is accomplished in a completely satisfactory way" (EA 16-611). And on 5 May he writes the last paper of Klein pleased me very much; he really appears to be a level-headed young man" (EA 10-162). By early the following year, Einstein had given up entirely the hope that equations of motion derived relativistically 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 recently my intended lines of attack according to the Poincaré theory. The final result comes up classically. It is correct Kaluza-Klein have correctly indicated the way to advance further. Long live the 5th dimension" (EA 10-173) as quoted in Havaš 1989, p. 269).

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 "Theoretical Physics and Experiment: An Experimental and Theoretical Contrast" (Einstein 1927b). The one significant feature 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 controversial experiment (Bothe 1927). He then arranged the radiation from a single X-ray source into two beams, each of which impinged on a paraffin block, Ps and P2, respectively. The paraffin block is then allowed to scatter a second time from a third paraffin block, 5, placed midway between the other two, and one then measures the magnitude of the Compton effect in the resulting inverse-scatter beam. Both'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, hν, and momentum, hν/c, the individual quanta being associated with partial waves, the total wave field being regarded as a product of the activity of many quanta. Both 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 many of his results upon measurement. Einstein is quite sure that the view that "light has a particle-like character, and is thus corpuscular" (Einstein 1927b, p. 546). This is true, inasmuch as Bohr claimed to have demonstrated that energy and momentum are associated with light quanta after the fashion of Einstein's own corpuscular conception of the quanta. But both Einstein wanted 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 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, both 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 classical corpuscularian conception of light. Indeed, he immediately follows his characterization of the Bohr 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 concepts" (EA 10-162). I agree with the thought of the physicien" (Einstein 1927b, p. 546). But Einstein's not mentioning that Bothe's experiment itself dramatically revealed this very due to 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 Reality of the Quantum World? Or Only in the Statistical Sense?" ("Die WettSchirg. der Wellenmechanik die Bewegung eines Systems vollständiger oder nur im Sinne der Statistik?). As word of the talk spread, it evidently aroused considerable interest, as witness Einstein's letter to Heisenberg's department 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 your talk and 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). A short time later, he thought he had a result, writing to Ehrenfest on 5 May: "I have also now carried out a little investigation regarding the Schrödinger business, in which I show that, in a completely unambiguous way, one can associate definite results with the solutions, something which makes any statistical interpretation unnecessary" (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 2-160).

Einstein's ideas for associating definite movements with any solution of the Schrödinger equation was the following. Given any solution, y, for definite total energy E and potential energy V, it is possible to express div grad V as a sum of n terms V (n), to each of which we can associate a definite "direction" in a-dimensional configuration space; the n "directions" will be
defined separately for the value of $\Psi$ at each point in configuration space. Following Schrödinger, Einstein styled div grad $\Psi$ a "metric" in configuration space, calling the $\Psi_{\alpha\beta}$ the "tensor of $\Psi$-curvature" and div grad $\Psi$ 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 $\Psi_{\alpha\beta}$, in configuration space, and that, having thus decomposed the kinetic energy, he can associate with each term an answer to the effect that the total kinetic energy is 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"). The paper reports "that the paper was in print when Einstein requested by telephone that it be withdrawn" (Paasi 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 $\psi_i$, the scheme indicated here and thereby found results that are surely to be rejected from a physical standpoint. Stimulated by this, I have found that the scheme does not satisfy a general requirement that must be imposed on a general law of motion for systems.

Consider, in particular, a system $B$ that consists of two energetically independent subsystems, $E_1$ and $E_2$; 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 $E_1$, the second quantities referring only to $E_2$. It is then well known that

$$\Psi = \Psi_1 + \Psi_2,$$

where $\Psi_1$ depends only on the coordinates of $E_1$, $\Psi_2$ only on the coordinates of $E_2$. In this case we must demand that the symplectic composite system be combinations of possible motions of the subsystems. The indicated scheme does not satisfy this requirement. In particular, there is no index belonging to a coordinate of $E_2$. Then $\Psi_{12}$ does not vanish. ...Herr Grossmann has pointed out that this objection could be taken care of by means of a modification of the stated scheme, in which we employ not the scalar $\Psi$ itself, but rather the scalar $\Psi_{12}$ 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 Grossmann's suggested route around the non-separability problem failed to work.

This is a crucial text for my argument that the separability problem was not along the forefront of Einstein's worries about the shortcomings of quantum mechanical treatments. 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 the theory for failing to satisfy the converse condition (that the product $\Psi \Psi^*$, of any two solutions for the separate systems, $\Psi_1$ and $\Psi_2$, 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 1925 quantum theory of material particles in Einstein's 1926-1927 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 EPJ 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 knock Bohr and others of the inadequacy 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, at least on one crucial point—the aims of Einstein's famous "phantom-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 articles 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 EPJ).

Of the "orthodox" quantum theorists whose position I know, Niels Bohr's seems to me to come nearest to doing justice to the problem. Transcribed in my own way of putting it, he argued as follows:

If the partial systems $A$ and $B$ form a total system which is described by its $\psi$-function $\Psi(AB)$, there is no reason why any mutually independent existence (states of reality) should be attributed to its parts viewed separately, not even if the partial systems are spatially separated from each other at the particular time under consideration. The assertion, that in this latter case an event situated on $A$ is, therefore, within the framework of quantum theory, unaffected (and the paradox shows) unacceptable. (Einstein 1949, pp. 661-662)

What I want now to do is to review the history of Einstein's Gedankenexperiment 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 Gedankenexperiments dates from the 1927 Solvay meeting, held in Brussels from 24 through 29 October. As reported by Bohr (1911), no notes from the session and Bohr's (1911) 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 discussion 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, 1) 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 "classical interpretation," whereas "interpretation II," presumably favored by defenders of the theory like Heisenberg and Schrödinger, regards the quantum theory as "a complete theory of the individual electron" (p. 101). According to interpretation II, the 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 Bohr-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 \(|\psi|\) was simply considered as the probability that a definite particle is situated at a certain place at a definite instant, it might happen that one and the same elementary process would act at two or more places of the screen. But the interpretation according to which \(|\psi|\) expresses the probability that this particle is situated at a certain place presupposes a very particular Hilbert space, only a single, a priori determined distance which would prevent the wave continuously distributed in space from acting at two places of the screen. In my opinion one can only counter this objection if one does not only describe the process by the Schrödinger wave, but at the same time one localizes the particle during the propagation. I think that de Broglie is right in preaching this in this direction. If one works exclusively with the Schrödinger wave, interpretation II of \(|\psi|\) in my opinion implies a contradiction with the relativistic 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 \(|\psi|\) belonging to individual particles. Now, it seems to me that there are objections on principle against this multidimensional representation of particles, 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 in agreement with the new statistical results. Secondly, the peculiarity of the forces of acting only at small spatial intervals 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

1) The German manuscript of a fragment of Einstein's remarks (RA 16-617), included in a letter of Einstein to Lorentz (the conference organizer) of 21 November 1927 (RA 16-615), carries a notation in an unknown hand in the upper left-hand corner: "Alg. Disk Freitag" ("General Discussion on Friday").
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 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 the screen. Ehrenfest’s letter to Goudsmit, Uhlenbeck, and Dyke 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 up another, and sometimes he was successful: juggling 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’s) 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 Gedankenexperimenten 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 uncertainty relations really were the main sticking point for Einstein, 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.  

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 manipulating the 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. Bohr replied with the standard complementarity argument, namely, that in order to measure the electron’s 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 diaphragm, Bohr remarks, in words reminiscent of his reply to EP: “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 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 inadequacy 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 transformation had already shown that any 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 individual parts from his general relativity. This meant that 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” independently (even by their own states in his insistence on strict energy-momentum conservation in individual events that Einstein knew from his general relativity 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 was his insistence that these systems be represented as localized, 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). 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 symmetrical expression for the complementary nature of the space-time description and the claim of causality" (Bohr 1927, p. 583). 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 pretends that two systems, 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 relations for, say, the x-axis in connection with two interacting systems, the case made in the EPR paper. The total linear impulse in the EPR paper, that is, 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 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 localization; if you choose the latter, you get localization, but no momentum conservation.

Thus, the reason Einstein cannot get both localization 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 the scientist who became that month's "hero of causality" (Bohr 1927, p. 583). 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 result is of interest from the point of view of the general question whether or not field theory stands in contradiction to 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 is essential to me, for the time being, still to be imperative, this in spite of the fact that through command of the numerical relationships has been accomplished, in another way, by quantum mechanics. (Einstein 1927a, 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.

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 changed attitude during this time is a remark in a letter to Ehrenfest of 23 August 1928: “I believe less and less 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 tastes, in a manner independent of the contemporary going on” (RA 10). Einstein’s ballad to 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 “photo-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 his 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 a more fundamental problem, because, as Einstein later himself admitted, “Einstein’s real aim may well have been to bring out the peculiarities, 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 Jammes (Jammes 1974, p. 143, 135–135, 134–135), from whom most of us have learned about the letter, Ehrenfest reported that good attitude toward the photon-box thought experiment to disprove the uncertainty relations. But the original intention behind the photon-box thought experiment, Ehrenfest believes, was a totally different purpose” (Jammes 1985, p. 134), the implication being that deviations from the uncertainty relations had been the original intention behind the photon-box thought experiment. But Jammes 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 ‘weigbare light-flash box’ (let us call it simply L-F-box) ‘contra uncertainty relations’ but ‘for a totally different purpose’ (NSC-ARQF).”

Einstein may have wanted to dispute the 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 uncertainty relation or the result of the measurement then being measurable when the projectile returns after a relatively long time, it having been reflected at some location sufficiently distant (1 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/or or the two. The experimenter may choose only after the projectile is already finally under way” (NSC-ARQF).

And: “It is interesting to get even about the fact that the projectile, which is already flying around isolated ‘for itself,’ must be prepared to satisfy very different ‘non-commutative’ predictions, without knowing as yet which of these predictions one will make (and test)” (NSC-ARQF).

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).

Jammes is quite right that we see here all of the ingredients of Einstein’s incompleteness arguments, but Jammes’ 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. In 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 A light-year away is to provide a spacelike separation between the inspection of the photon-box and the projectile. The argument works as a critique of the quantum theory only if one assumes that the projectile, when then separated from the box, is, in virtue of that separation and its therefore possessing its own individuality, 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 colors 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 tells us that the state we ascribe to the projectile 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 assumptions, our discussion is unsound.”
assumptions of separability, as Einstein, the champion of field theories, clearly thought we must, than 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 in 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 and subsequently 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, or its energy, and Bohr himself, Bohr's 'triumph over Einstein at the 1930 Solvay meeting. How could Einstein so easily accommodate this point, if it were really such a desperate attack on his critical 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, Jammes, 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. By 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?  

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 Bohr's Poldasky about objects 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 Poldasky 1931), which Einstein himself apparently credited primarily to Tolman (see Einstein 1931, p. 23). The stated aim is to show that, contrary to what was 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 emitted particles travels directly to an observer at 0; the other follows a different trajectory, reflected toward 0 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 of arrival of the first particle at 0, along with its momentum, would enable one to calculate the time when the shutter opened and thus 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 here 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 leaves it to use on at least of more interest to Einstein. Thus, if two 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 e same properties of the photon's arrival at 0 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 by Einstein himself ever given of the history of these Gedankenexperiment 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 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, S1 and S2, is then reflected back. If the reflection is exactly off all the beams are recombined at a second half-silvered mirror O-O' producing two final beams, S3 and S4, each of which enters a detector. Consider a beam S of such low intensity that just one photo passes through the apparatus. Einstein says that if the mirrors M1 and M2 are fixed, preventing us from determining, by the mirrors' recoil, which path a given photon travels, the reflected beams S1 and S2 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 S3. But if the mirrors M1 and M2 are movable, so that we can say which path each photon takes, the two reflected beams S1 and S2 are coherent, and there is no interference at all, and equal numbers of photons show up, on average, in each detector.

Einstein's analysis of the experiment is somewhat confused. In his first, undated letter, Epstein points out that Einstein 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 degrees of freedom, the mirror, ψ for the quantum). There is then no ψ(q,t) as all as no complete observation of the mirror is at hand. Then, in terms of the theory, one cannot at all ask how ψ(q,t) is constituted as a function of the time" (RA 10-581).

He considers two previously interacting particles "described as completely as possible in the sense of the quantum theory by ψ(q,q',t) (RA 10-581). After a sufficiently long time, the particles have separated and we are in a state that seems each individual particle corresponds to a real state of affairs" (RA 10-581). To learn something about q, we perform a measurement on q, and we can arrange the measurement so that 'the ψ(q,t) resulting
from this measurement and from the $\Psi(x,y,t)$ has either a sharp position (for a given value of the time), or a sharp momentum (Ref. 34, 36), 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. (Ref. 10-581). In this case, an influence (and Einstein thinks that this view is correct), it would mean the existence of superluminal effects, against which Einstein's "physical instinct" struggles, so that 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, even of incompleteness argument. In this there is nothing new. What is significant is what follows in the next letter.

Einstein responded to 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 fills arrived at the incompleteness argument: "I myself first came upon the argument starting from a simple thought on August. 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 disposal various instruments, including a classical shutter and tools with which to measure the box's position. Before starting the experiment, the observer places the shutter in a far distant fragment of the universe. Now the shutter is opened, and the photon emits light. What is the position of the photon? The result is not measurable in the same manner as the box. It is a quantum mechanical state, and the question of its position is not determinable. The photon can be measured, but the position of the photon is not determinable.

As soon as it has left the box, the light quantum represents a certain "real state of affairs," about which nature we must seek to construct an interpretation, which is naturally in a certain sense arbitrary.

The interpretation depends essentially upon the question: should we assume that the subsequent measurements make or do not influence the height of the quantum, that is to say, the "real state of affairs" characteristic by the light quantum?

Hence a physical effect from B on the height of the 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 contrary to my physical instinct, that I cannot 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 subsequent measurement on B is independent even if this measurement is not performed. Accordingly, the light quantum has a definite localization and a definite color.

I 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 say that quantum mechanics gives an incomplete description of the real state of affairs. . . .

If one is of the view that a theory of the character of quantum mechanics is required for physics, then one must either complete the spatio-temporal localization of the real, or replace the notion 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 Gedankenexperiment and, presumably, the 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 the appropriate place in the chronology of the above events. But Eichenerfest's 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 that there could be no complete quantum theory. If that is the case, then the "Epstein" version of the photon-box argument came first, as Einstein says. And from one point of view, the 1930 Solvay photon box may be regarded as a refinement of the letter; 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 gedankenexperiment 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 measures the position or the box, by a second watch, depending on which position measurement he or she makes, a different prediction can be made about the photon. All that Bohr's famous critique concerns is the question, essential 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 de-
scription of individual systems. All that changes are the details of the Gedankenexperiment 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 and wrote to Ehrenfest: "Yesterday you nudged me into modifying the 'box' experiment in such a way that it would employ contemproary quantum theory 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 Compton scattered photon is assumed to move along the same axis that the scattering mass is free to move along, the photon being reflected at a distant mirror back to an experimenter who sits near the mass. Einstein assumes that the mass is initially at rest (zero momentum) which means that the position of the photon is nondeterminable. 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 position of the mass, or the photon's time of arrival, enabling the experimenter to deduce the time when the initial scattering occurred and thus the precise position of the mass at the time of the scattering. (There is an obvious error here.) The important point is that we can only deduce either the momentum or the position of the mass, without in any way disturbing it itself. Einstein's conclusion comes as no surprise: "Thus, without any experiment on its, it is impossible to predict, according to a free choice, either the momentum or the position of the photon itself 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 Gedankenexperiment can be dated to sometime in the spring or summer of 1933, when on the 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 Liége, and had just published his famous collection of papers on Einsteinian field quantities in quantum mechanics (Rosenfeld 1933). He gave a lecture on the topic of the Gedankenexperiment at the time, which Einstein attended. After the talk, as Rosenfeld records, Einstein approached him to discuss the topic of the lecture but "too short warned me, about which he said he still felt a certain 'un-American'." 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 on 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 both particles 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 recall, we can recognize here the same logic that is 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 that licenses the crucial inference that the undisturbed system in EPR-type Gedankenexperiments 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 realist Bohr if one does not make use of a supplementary principle: the 'separability 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, even in the United States, was that these worries began to crystallize in the mid-1920s into the belief that, because it regards interacting systems as non-separable, quantum mechanics is thus fundamentally inadequate. And I have argued, finally, that the real aim of the famous series of Gedankenexperiments starting at the 1927 Solvay meeting was to bring out precisely this feature of the quantum theory and to exhibit this, to Einstein, unacceptable consequences to which at the 1935 Solvay meeting, I argued that Einstein's "incomplete" theory allowed for strict reality 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 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.34 Pauli writes:

34For the text of Heisenberg's reply, "Ist eine deterministische Ergänzung der Quantenmechanik möglich?", see Pauli 1935, pp. 409-418; Heisenberg enclosed a copy with his letter to Pauli of 2 July 1935. Another copy, in typewritter, 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. "Well, so schliessIch nicht sein kann—ich nicht sein darf" (Morgenstern). . .

Now he understands this not at all. He cannot simultaneously measure two quantities corresponding to non-commuting operators and that one cannot simultaneously ascribe numerical values to them. But where he is in 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—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.

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 in characteristic 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, 17 September 1935, RA 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 in his fundamental view 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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REFERENCES


Krutkow, G., 1914b, "Bemerkung zu Herrn Wolfses Obersicht: "Welche Strahlungs-


