Revisiting the Einstein-Bohr Dialogue

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Einstein and Bohr – No names loom larger in the history of twentieth-century physics, and rightly so, Albert Einstein and Niels Bohr being the figures most prominently associated with the relativity and quantum revolutions.[1] Their names dominate, likewise, the history of philosophical reactions to the new physics of the twentieth century, Bohr for having identified complementarity as the chief novelty in the quantum description of nature,[2] Einstein for having found vindication in relativity theory for either positivism or realism, depending upon whom one asks.[3] Famous as is each in his own domain, they are famous also, together, for their decades-long disagreement over the future of fundamental physics, their respective embrace and rejection of quantum indeterminacy being only the most widely-known point of contention.

A well-entrenched narrative tells the story of the Einstein-Bohr debate as one in which Einstein's tries, from 1927 through 1930, to prove the quantum theory incorrect via thought experiments exhibiting in-principle violations of the Heisenberg indeterminacy principle, only to have Bohr find the flaw in each, after which Einstein shifts his direction of attack, faulting the quantum theory now not as incorrect, but incomplete. In 1935, the Einstein, Podolsky, and Rosen (EPR) paper, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" (Einstein, Podolsky, Rosen 1935) represents the high-water mark of this critique. It is met by Bohr's deep and devastating reply (Bohr 1935), after which Bohr grows ever more in stature and influence as the sage of Copenhagen, while Einstein slips into senility in Princeton, meddling, perhaps commendably, in the politics of the atomic bomb, but no longer capable of constructive contributions to physics, itself.[4]

That something is seriously wrong with this triumphalist narrative has been remarked upon by various authors for more than twenty years.[5] More than anyone else, however, it was Mara Beller, in her *Quantum Dialogue* (Beller 1999) who forced a reassessment.[6] One need not agree with every detail in Beller's own account of the way in which the community around Bohr in Copenhagen achieved consensus on questions of interpretation in order to appreciate the point that the writing or rewriting of history is, itself, one of the many tools with which communities define themselves and construct consensus. Reason enough always to be just a bit suspicious of any community's telling its own story.

Some aspects of Beller's new history I commend; others I do not. I commend Beller's stressing, like James Cushing before her (Cushing 1994), the role of contingent social and historical circumstance in the achievement of consensus. More than Beller, I find cogency in Bohr's arguments, but there can be no doubt that the victory of a "Copenhagen" point of view on interpretation is explained, in no small measure, by such factors as Bohr's control over financial resources through his institute in Copenhagen, by his personal prestige, and even by a conversational manner that some regarded as persistent and others as bullying. But Beller identifies Bohr as the chief enforcer of a Copenhagen orthodoxy, whereas I think that what later came to be regarded as Copenhagen orthodoxy owed more to Werner Heisenberg than to Bohr. On my reading, Bohr was never a positivist, did not endorse wave-packet collapse, and did not demand ideological conformity among his followers.[7] It was not by compulsion, but by the example of his dogged pursuit of the deep philosophical lessons of the quantum that Bohr created and sustained not a unitary "Copenhagen interpretation," but what Leon Rosenfeld called the "Copenhagen spirit" (Rosenfeld 1957)

That the standard, triumphalist Copenhagen narrative requires significant revision is, however, as noted, a point upon which I wholeheartedly agree with Beller. It needs revision if only to redress the insult to Einstein. The present paper is a contribution to that revision. I argue that the standard history is wrong not only for the kinds of reasons cited by Beller, but also for the reason that it more or less completely misses the real point at issue between Einstein and Bohr. Simply put, both Bohr and Einstein understood early and clearly that the chief novelty of the quantum theory was what we, today, call "entanglement," the non-factorizability of the joint states of previously interacting quantum systems. Bohr embraced entanglement, seeing in it the roots of complementarity. Einstein rejected entanglement as incompatible with the principle of the spatial separability of systems, a principle that he thought not only a necessary feature of any field theory like general relativity but also a necessary condition for the very intelligibility of science. Everything else is derivative, including Bohr's defense of complementarity and Einstein's charge of the incompleteness of quantum mechanics, a charge unsustainable without the assumption of what Einstein termed the "separation principle."

On this way of revising the history, Bohr still emerges with the better arguments. Here, too, Beller and I disagree. But Einstein's legacy is rehabilitated, his dissent being seen for what it was: principled, well-motivated, based upon deep physical insight, and informed by a sophisticated philosophy of science. Why later apologists for Copenhagen orthodoxy preferred the senile Einstein as Bohr's antagonist is hard to fathom, for Bohr's defense against Einstein's critique makes more sense and is more interesting when read as a reply to a good argument rather than a bad one. Putting a debate about entanglement at center stage has the additional salubrious effect of highlighting a still more general failure of the received history of quantum mechanics in the early twentieth century. It was not just Einstein and Bohr who were arguing about entanglement. Everyone was. That the joint states of previously interacting quantum systems do not factorize and that the quantum mechanical story about multi-particle systems differs, thus, in a fundamental way from the classical story were facts understood by all of the major players, well before Erwin Schrödinger baptized the phenomenon as "entanglement" in 1935, and long before quantum information theory made entanglement the hot, new topic in the foundations literature in the 1990s.[8] Why the standard histories of quantum mechanics have not emphasized this is as puzzling as their penchant for letting caricature take the place of a sympathetic and accurate account of Einstein's critique of the quantum theory.

Einstein, Bohr, and Entanglement before the 1927 Solvay Meeting

Standard histories of the Einstein-Bohr debate have it beginning in earnest during the personal encounter between Einstein and Bohr at the 1927 Solvay meeting. This was an important confrontation, but to understand what Einstein and Bohr were arguing about then and later, one must understand first their prior confrontations with the issue of entanglement. Einstein had been thinking about the problem for over twenty years, Bohr for nearly ten.

From the very beginning, that is, from his 1905 paper on the photon hypothesis, Einstein understood that the emerging quantum theory was likely to incorporate some compromise with the fundamentally classical idea of the mutual independence of interacting systems.[9] The reason was simple and obvious. Recall that Einstein's route to the photon hypothesis was via a thermodynamic analogy. Einstein showed that expression for the entropic behavior of radiation in the Wien (high-frequency) regime exhibited the same functional form as the entropy of a classical, Boltzmann gas.

That analogy sustained the inference that radiation in the Wien regime had a microstucture like that of a Boltzmann gas, in the sense that it consisted of discrete, distinguishable, mutually independent, corpuscle-like carriers of the field energy that Einstein dubbed light quanta. In Einstein's own words: "Monochromatic radiation of low density (within the domain of validity of Wien's radiation formula) behaves from a thermodynamic point of view as if it consisted of mutually independent energy quanta of the magnitude $R\beta\nu/N$" (Einstein 1905, 143). What did it mean to say that light quanta were "mutually independent"? It meant precisely that the Boltzmann formula applied or that entropy is an extensive property of the radiation gas, for the additivity of the entropy of a composite system is equivalent to the factorizability of the joint probabilities for the differently spatially situated component subsystems occupying given cells of phase space, this latter being the normal way of expressing the probabilistic independence of two events. Again, in Einstein's own words:

> If we have two systems S_1 and S_2 that do not interact with each other, we can put
>
> $$S_1 = \varphi_1(W_1),$$
> $$S_2 = \varphi_2(W_2).$$
>
> If these two systems are viewed as a single system of entropy S and probability W, we have
>
> $$S = S_1 + S_2 = \varphi(W)$$
>
> and
>
> $$W = W_1 \cdot W_2.$$
>
> The last relation tells us that the states of the two systems are mutually independent events. (Einstein 1905, 140-141)

But this argument works only in the Wien limit. That such mutual independence is thus shown only to hold in the Wien limit and that it is likely to fail outside of the Wien regime is obvious, however much this obvious fact goes unremarked in much of the secondary literature on

Einstein and the quantum. The simple fact is that, from the very beginning, Einstein understood the limitations of the argument and understood that, in general, the radiation field could not be modeled in simple corpuscular terms.

If there be any doubt that Einstein understood that mutual independence would not obtain outside of the Wien regime, consider what he wrote to Hendrik Lorentz four years later:

> I must have expressed myself unclearly in regard to the light quanta. That is to say, 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 spaces. This would be the most convenient explanation of the Wien end of 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 indeed cannot divide without change of frequency.
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> As I already said, in my opinion one should not think about constructing light out of discrete, mutually independent points. I imagine the situation somewhat as follows: . . . I conceive of the light quantum as a point that is surrounded by a greatly extended vector field, that somehow diminishes with distance. Whether or not when several light quanta are present with mutually overlapping fields one must imagine a simple superposition of the vector fields, that I cannot say. In any case, for the determination of events, one must have equations of motion for the singular points in addition to the differential equations for the vector field. (Einstein to Lorentz, 23 May 1909, Einstein 1993, Doc. 163)

In other words, the idea of wave-particle duality is born out of Einstein's effort to exploit the metaphor of interference for describing the manner in which light quanta outside of the Wien regime do not behave like mutually independent systems.

Over the next decade, many people in addition to Einstein struggled to understand what kind of statistics was appropriate for quantum systems behaving in such a non-classical manner. An especially illuminating moment in that history is recorded in a series of papers by Mieczysław Wolfke, who was Einstein's colleague in Zurich for a short time in 1913-1914. Wolfke was seeking to understand exactly what kind of independence among light quanta was assumed in Einstein's 1905 argument, and his remarks are interesting in part because they are based on direct communications from Einstein, himself. In the last of four papers, Wolfke wrote:

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

But understanding the independence of photons in the Wien limit is not the same thing as understanding the manner in which that independence fails outside of the Wien regime. It took another ten years for Einstein to solve the quantum statistics puzzle thanks to the fortuitous intervention of the Indian physicist, Satyendra Nath Bose (1924).

How deeply the problem of quantum statistics weighed upon Einstein's soul before 1924 is clear from a far-too-little-known argument adduced by Einstein in 1920 for the purpose of opposing(!) a field ontology of trackable, mutually independent carriers of field energy. The setting is the lecture, "Äther und Relativitätstheorie," delivered in May, 1920 as the inaugural address for Einstein's visiting professorship in Leiden, home to Lorentz, whom Einstein held in high esteem. In what is partly a gesture of respect for Lorentz, Einstein evinces surprising sympathy for the concept of the ether, arguing that, in many respects, the space-time of general relativity has taken over the role of the electromagnetic ether, at least inasmuch as it is the abode of field energy. To questions about the microstructure of fields, Einstein gives an interesting answer:

> The special theory of relativity does not compel us to deny the aether. We may assume the existence of an ether; only we must give up ascribing a definite state of motion to it, i.e., we must by abstraction take from it the last mechanical characteristic which Lorentz had still left it. . . .
>
> Think of waves on the surface of water. Here we can describe two entirely different things. Either we may follow how the undulatory surface forming the boundary between water and air alters in the course of time; or else–with the help of small floats, for instance–we can follow how the position of the individual particles of water alters in the course of time. If the existence of such floats for tracking the motion of the particles of a fluid were a fundamental impossibility in physics–if, in fact, nothing else whatever were discernible than the shape of the space occupied by the water as it varies in time, we should have no ground for the assumption that water consists of movable particles. But all the same we could characterize it as a medium.
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> We have something like this in the electromagnetic field. For we may picture the field to ourselves as consisting of lines of force. If we wish to interpret these lines of force to ourselves as something material in the ordinary sense, we are tempted to interpret the dynamic processes as motions of these lines of force, such that each individual line of force is tracked through the course of time. It is well known, however, that this way of regarding the electromagnetic field leads to contradictions.
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> Generalizing we must say this: There may be supposed to be extended physical objects to which the idea of motion cannot be applied. They may not be thought of as consisting of particles that allow themselves to be individually tracked through time. In Minkowski's idiom this is expressed as follows: Not every extended structure in the four-dimensional world can be regarded as composed of world-threads. The special theory of relativity forbids us to assume the ether to consist of particles that can be tracked through time, but the hypothesis of the ether in itself is not in conflict with the special theory of relativity. Only we must be on our guard against ascribing a state of motion to the ether. (Einstein 1920, 9-10)

Savor the irony. Here is Einstein, the inventor of the photon hypothesis, arguing against modeling the electromagnetic field as being composed of distinguishable and, thus, trackable field quanta, and this because such a model is inherently non-relativistic. One easily imagines the relief Einstein felt when, prodded by Bose, he realized in 1924 that he could rescue a quantum field ontology by denying the distinguishability of quanta. Surely we see here another important reason for Einstein's prompt and warm embrace of Bose, along with the fact that Bose's new statistics yielded an elegant, first-principles derivation of the Planck formula for black body radiation.

Accepting Bose's new statistics, however, meant facing up to the fact that the failure of the mutual independence of quanta outside of the Wien limit, suspected by Einstein since at least 1909, was going be a deep and pervasive feature of a still not yet established quantum mechanics. Einstein explained this point in his second paper on the extension of bosonic statistics to material particles:

> Bose's theory of radiation and my analogous theory of ideal gases have been reproved by Mr. Ehrenfest and other colleagues because in these theories the quanta or molecules are not treated as structures statistically independent of one another, without this circumstance being especially pointed out in our papers. This is entirely correct. If one treats the quanta as being statistically independent of one another in their localization, then one obtains the Wien radiation law; if one treats the gas molecules analogously, then one obtains the classical equation of state for ideal gases, even if one otherwise proceeds exactly as Bose and I have. . . . It is easy to see that, according to this way of calculating [Bose-Einstein statistics], the distribution of molecules among the cells is not treated as a statistically independent one. This is connected with the fact that the cases that are here called "complexions" would not be regarded as cases of equal probability according to the hypothesis of the independent distribution of the individual molecules among the cells. Assigning different probability to these "complexions" would not then give the entropy correctly in the case of an actual statistical independence of the molecules. Thus, the formula [for the entropy] indirectly expresses a certain hypothesis about a mutual influence of the molecules–for the time being of a quite mysterious kind–which determines precisely the equal statistical probability of the cases here defined as "complexions." (Einstein 1925, 5-6)

Einstein made the same point with equal clarity in a letter to Schrödinger in February of 1925:

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

Bose-statistics

	1st cell	2nd cell
1st case	●●	–
2nd case	●	
3rd case	–	●●

independent molecules

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

> According to Bose the molecules stack together relatively more often than according to the hypothesis of the statistical independence of the molecules. (Einstein to Schrödinger, 28 February 1925, EA 22-002)

That the Schroödinger who later coined the term "entanglement" was the recipient of this letter is noteworthy, for when one reads Schrödinger's work in developing wave mechanics over the next several months through the lens of his ongoing correspondence with Einstein, it is hard to avoid the conclusion that one of Schrödinger's principal motivations was to find a wave-mechanical formalism capable of expressing precisely the curious failure of the mutual independence of quantum mechanical systems revealed in the new quantum statistics, just as Einstein, in 1909, deployed the metaphor of wave interference to model the failure of mutual independence outside of the Wien regime. The key move of writing the n-particle wave function in 3n-dimensional configuration space, rather than 3-space, is necessary because only thus does one have access to a state space rich enough to comprise non-factorizable joint state functions. The price paid, famously, and much to Schrödinger's regret, was the loss of visualizability, but that was precisely the price required by entanglement, as both Bohr and Einstein would quickly realize.

On Einstein's side there followed two years of intense theoretical and experimental work probing the subtleties of the new quantum mechanics of multi-particle systems. With advice from Einstein, the Berlin experimentalist and master of the technology of coincidence counting, Walther Bothe, pursued a kind of proto-Bell experimental program, investigating a variety of different correlation phenomena (see, for example, Bothe 1926). On the theoretical front, Einstein's efforts culminated in May of 1927–a few months before the first of the famous clashes with Bohr at the Solvay meeting–in a failed attempt to provide his own hidden variables model of Schrödinger's new wave mechanics. Ironically, Einstein's model failed precisely because it included the very entanglement Einstein was hoping to avoid. Though it reached the proof stage, Einstein's lecture to the Berlin Academy was never published because Einstein could not find a way around the entanglement. He explained in a "Note Added in Proof":

> I have found that the schema does not satisfy a general requirement that must be imposed on a general law of motion for systems.
>
> Consider, in particular, a system Σ that consists of two energetically independent subsystems, Σ_1 and Σ_2; this means that the potential energy as well as the kinetic energy is additively composed of two parts, the first of which contains quantities referring only to Σ_1, the second quantities referring only to Σ_2. It is then well known that
>
> $$\Psi = \Psi_1 \cdot \Psi_2 ,$$
>
> where Ψ_1 depends only on the coordinates of Σ_1, Ψ_2 only on the coordinates of Σ_2. In this case we must demand that the motions of the composite system be combinations of possible motions of the subsystems.
>
> The indicated scheme [Einstein's hidden variables model] does not satisfy this requirement. In particular, let μ be an index belonging to a coordinate of Σ_1, v an index belonging to a coordinate of Σ_2. Then $\Psi_{\mu v}$ does not vanish. (Einstein 1927a)

Here, then, we have Einstein on the eve of the 1927 Solvay meeting. For twenty-two years he had known that the full story of the quantum would involve some fundamental compromise with the classical notion of the mutual independence of interacting systems. In 1924, Bose showed him that this failure of mutual independence was not incidental but an essential feature of the quantum realm, the deep fact underlying the Planck formula. In 1925, Schrödinger developed the wave mechanical formalism that relates the new quantum statistics to the symmetry properties of the two-particle wave function. In the spring of 1927, Einstein's attempt at a hidden variables model of wave mechanics is abandoned because it, too, evinces the very entanglement whose presence in wave mechanics Einstein was seeking to evade. It was only a few months earlier, in a letter to Max Born of 4 December 1926, that Einstein had complained that God "does not play dice" with the world (Born 1969, 129-130). But that entanglement, not indeterminacy, was the chief source of Einstein's misgivings about quantum mechanics should now be clear. Indeterminacy was but a symptom; entanglement was the underlying disease.

Bohr, too, had been thinking about what we now call entanglement for a long time by the fall of 1927. Since not long after Carl Ramsauer discovered in 1920 the counterintuitive fact that the scattering cross-section of electrons passing through certain noble gases goes to zero as the electron velocity is reduced (see Ramsauer 1920, 1921a, 1921b), Bohr suspected that a variety of non-classical phenomena would find their explanation in violations of classical assumptions about the mutual independence of interacting systems, or, in Bohr's own idiom, in failures of "ordinary space-time description." It was hard to imagine how slower electrons could more readily penetrate the gas if the scattering of the electron by, say, an argon atom were modeled as a classical hard-sphere collision. Clinton Davisson and Lester Germer's later discovery of interference effects in electron diffraction from nickel crystals (Davisson and Germer 1927), usually remembered as the first experimental proof of the wave nature of the electron, in fact just reinforced an already well-founded impression of the highly non-classical character of electron scattering.

Worries about the failure of "ordinary space-time description" were among the main reasons for Bohr's well-known skepticism about the photon hypothesis, for remember that Einstein's pre-1924, pre-Bose photons were wrongly assumed to be mutually independent, corpuscle-like carriers of field energy behaving entropically like the mutually independent molecules of a Boltzmann gas. But even Bohr was slow to grasp the fundamental character of entanglement. He was helped by the disproof of the Bohr-Kramers-Slater (BKS) theory in 1924.

A lineal descendent of Einstein's 1909 idea of wave-particle duality, the BKS theory sought to explain phenomena such as the absorption and emission of radiation by associating with each elementary structure, such as valence electrons, a virtual field with component probability amplitudes corresponding to each possible transition that the system could undergo. But it was still a "classical" theory in the sense that it explained interactions–as in the emission and absorption of a photon by two electromagnetically interacting atoms–as the result of the conjoint but still separate doings of the virtual fields associated with each interacting system. This is why, though energy and momentum were conserved on average, the BKS theory predicted violations of energy and momentum conservation in individual atomic events, because the separate actions of the two virtual fields implied the statistical independence of, say, photon emission by one atom and subsequent photon absorption by the other atom.

The Compton-Simon and Bothe-Geiger experiments proved strict energy and momentum conservation in individual atomic events (Compton and Simon 1925a, 1925b, Bothe and Geiger 1924, 1925) and so refuted the BKS theory. But the standard histories fail to emphasize the main implication drawn by Bohr and others from this failure. What Bohr found important was precisely the proof that coupled atomic events are not thus statistically independent of one another, that strict energy and momentum conservation thus implies the failure of "ordinary space-time description." Here is how he put the point in a letter to Geiger in April of 1925, where the immediate subject was a different experiment of Bothe's (Bothe 1926) that yielded evidence against another aspect of the BKS theory:

> I was quite prepared to learn that our proposed point of view about the independence of the quantum processes in separated atoms would turn out to be wrong. . . . Not only were Einstein's objections very disquieting; but recently I have also felt that an explanation of collision phenomena, especially Ramsauer's results on the penetration of slow electrons through atoms, presents difficulties to our ordinary space-time description of nature similar in kind to the those presented by the simultaneous understanding of interference phenomena and a coupling of changes of state of separated atoms by radiation. In general, I believe that these difficulties exclude the retention of the ordinary space-time description of phenomena to such an extent that, in spite of the existence of coupling, conclusions about a possible corpuscular nature of radiation lack a sufficient basis. (Bohr to Geiger, 21 April 1925; Bohr 1984, 79)

On the same day he made a similar point in a letter to James Franck:

> It is, in particular, the results of Ramsauer concerning the penetration of slow electrons through atoms that apparently do not fit in with the assumed viewpoint. In fact, these results may pose difficulties for our customary spatio-temporal description of nature that are similar in kind to a coupling of changes of state in separated atoms through radiation. But then there is no more reason to doubt such a coupling and the conservation laws generally. (Bohr to Franck, 21 April 1925; Bohr 1984, 350)

Given the frequent complaints about the alleged opacity of Bohr's idiolect, it is worth pausing a moment to make sure that we understand what Bohr means by the failure of the space-time mode of description. Note the manner in which Bohr associates the failure of space-time description with the failure of both corpuscular models of radiation–the photon hypothesis–and the assumed statistical independence of coupled changes of state. The associations are the same that Einstein had been making since 1905, when he argued that a corpuscular model of the radiation field means precisely the assumption of the statistical independence of two spatially separated photons occupying given cells of phase space. How are corpuscularity and statistical independence related, in turn, to space-time description? For both Bohr and Einstein, it was because the assumption of a difference in spatial location was thought to be sufficient for securing the mutual independence of thus differently situated systems. But corpuscular models are not alone in being thus linked to space-time description and the assumed statistical independence of coupled changes of state. Wave models can likewise be seen as providing a space-time description and undergirding the assumed statistical independence of changes of state, as long as separate wave structures are associated with spatially separated systems, as in Einstein's early speculations about wave-particle duality and in the BKS theory. Schrödinger's introduction of entangled n-particle wave functions written not in 3-space but in 3n-dimensional configuration space offends against space-time description because it denies the mutual independence of spatially separated systems that is a fundamental feature of a space-time description.

Bohr's thinking about quantum foundations finally began to coalesce in the summer of 1927, stimulated by his less than wholly positive reaction to the Heisenberg indeterminacy principle (Heisenberg 1927). His new ideas, specifically the concept of complementarity, were premiered in his lecture to the Volta Congress at Como, Italy in September. By this time Bohr had assimilated the fact that, in the quantum theory, coupled systems do not evince the mutual independence that classical physics ordains for spatially separated systems. The important new idea is that "observers," too, are physical systems, and that when an "observed object" is coupled with an "observer" in measurement, as it must be if the measurement is to engender the object-observer correlations necessary for the measurement's being a measurement, then both observer and object lose their mutual independence, if, that is, we treat the measurement interaction like any other physical interaction in the quantum domain.

That instrument and object form an entangled pair is the premise from which Bohr derives complementarity:

> Now, the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation. . . .
>
> This situation has far-reaching consequences. On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. 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, symbolizing the idealization of observation and definition respectively. (Bohr 1928, 54-55)

These few words have given rise to much confusion in the secondary literature, even though Bohr clearly sought, in vain, to avoid one possible confusion by speaking not of the "observer" as a subjective knower but of the "agency of observation," the point being, one presumes, to emphasize the physical nature of measurement interactions. Should Bohr also have foreseen that another misreading of these words would turn them into an endorsement of anti-realism, a crucial premise in the all-too-common, if mistaken, arguments that Bohr's complementarity interpretation of quantum mechanics is a species of positivism? Bohr does not say, here, that there is no quantum reality. What he says is that, in virtue of what we now call entanglement, we cannot ascribe an *independent* reality to either the observer or the observed.

This is not the place for an extended discussion of Bohr on complementarity, but note two points.[10] First, many authors, Beller included, argue that the challenge of EPR, which employs indirect, not direct measurements, forced a dramatic revision in Bohr's conception of complementarity. It is said that Bohr stopped talking about the complementarity between space-time description and the claims of causality introduced in the Como lecture and began to speak, instead, of complementarity as a relationship between incompatible observables, like position and momentum. That is simply false. In the published version of the Como lecture, itself, a few pages after the just-quoted remark, Bohr wrote:

> According to the quantum theory a general reciprocal relation exists between the maximum sharpness of definition of the space-time and energy-momentum vectors associated with the individuals. This circumstance may be regarded as a simple symbolical expression for the complementary nature of the space-time description and the claims of causality. (Bohr 1928, 60)

Can there be a more categorical assertion of the essential identity of the two versions of complementarity? The difference is no more than that between a conceptual and a formal characterization of one and the same notion.

Of course there is the possibility of a complementary relationship between parameters other than position and momentum or energy and time. Nothing precludes that. The mentioned instantiations of complementarity are in the foreground in 1927 for two reasons: (a) the fundamental role of position and momentum in defining the state concept of classical particle mechanics (state $=_{df}$ point in phase space), and the comparably fundamental roles of time and energy as, respectively, the fundamental dynamical variable in classical mechanics and the associated conserved quantity; (b) the fact that the state concept, the dynamics, and the conservation laws in classical particle mechanics all work together in harmony, as they must, whereas the various parts of the classical mode of description cannot be combined consistently in the quantum theory.

Bohr's highlighting the connection between Heisenberg indeterminacy and the complementarity of space-time description and causality gives us the clue we need to understand the latter. A bit of formal hindsight helps as well. Space-time description is the easy part. A space-time description in classical particle mechanics models the behavior of a physical system by tracking the time evolution of the system in 3-space or, equivalently, the evolution of its state in phase space. If we are dealing with interacting systems, *A* and *B*–the really crucial case–then it is the separate evolution of each in space that concerns us, the interaction handled by a potential via which one calculates the separable effects of *A* on *B* and *B* on *A*. But Bohr (and Einstein) already knew that such "pictures"–yes, literally, pictures–do not work in the quantum theory, where interacting systems are entangled.

The "claims of causality" is the harder part, at least at first glance. Bohr's talk of "causality" is confusing, because he does not mean the univocal time evolution of physical states. What Bohr does mean is indicated by his associating the "claims of causality" with the energy-momentum vector. Energy and momentum are the fundamental conserved quantities of classical particle mechanics. A "causal" description, in Bohr's use of the term, is a description according to which energy and momentum are conserved.

The failure of the BKS theory had taught Bohr how the two harmonious aspects of a classical mechanical description come apart in the quantum realm, for, as we just saw, violations of strict energy and momentum conservation are the famous untoward consequence of the BKS theory's still assigning separate states (including the virtual fields) to spatially separated but interacting systems.

In the quantum domain, a space-time description implies no energy and momentum conservation in individual events; the claims of causality are violated. Salvaging energy and momentum conservation in individual events requires the right, strict correlations between interacting systems: a gain in energy here means a loss of energy there, and a loss of momentum there means a gain of energy here. As was already becoming clear to Einstein and Bohr in 1927, getting the correlations right means introducing entangled joint multi-particle states, as Schrödinger had done in wave mechanics.

The 1927 and 1930 Solvay Encounters

The 1927 Solvay meeting took place in October, one month after the Volta Congress in Como and a few months after Einstein had abandoned his attempted hidden variables model of Schrödinger's wave mechanics. In addition to bits and pieces of contemporary correspondence, the main sources of information about the encounter there between Einstein and Bohr are the published proceedings of the meeting (Solvay 1927) and Bohr's memoir of his discussions with Einstein, written some twenty years after the event (Bohr 1949). Witnesses agree that the most interesting conversations took place off the record, at breakfast and dinner, often lasting well into the night. One would think Bohr's memoir the better source, therefore, but for the fact that it differs so dramatically from the published record.

Bohr's memoir and the published record agree that the one-slit diffraction experiment was of central concern. According to Bohr, the point at issue was whether or not the one-slit experiment could be manipulated to yield violations of Heisenberg indeterminacy:

> The discussions . . . centered on the question of whether the quantum-mechanical description exhausted the possibilities of accounting for observable phenomena or, as Einstein maintained, the analysis could be carried further and, especially, of whether a fuller

> description of the phenomena could be obtained by bringing into consideration the detailed balance of energy and momentum in individual processes. (Bohr 1949, 213)

While claiming to explain "the trend of Einstein's arguments," Bohr recalls his introducing his own long-since-familiar refinements, such as a movable diaphragm and a second diaphragm containing two slits, all for the purpose of countering Einstein's efforts to prove that one can make precise determinations of both the position and the momentum of the particle traversing the apparatus.

The published record of Einstein's one contribution to public discussion shows Einstein thinking about something very different. Einstein says that, in application to the one-slit diffraction experiment, quantum mechanics can be given either an ensemble interpretation ("Interpretation I") or an interpretation ("Interpretation II") according which the wave function represents the objective state of an individual quantum system. Einstein offers three objections to "Interpretation II." The first is that it violates relativity because, by fixing the probability that this one specific particle is located at one specific place, the second interpretation "presupposes a very particular mechanism of action at a distance which would prevent the wave continuously distributed in space from acting at *two* places of the screen" (Einstein 1927b, 102). Crudely put, the idea is that if the particle has a high probability of being here, then, simultaneously (the crucial point), it has a high probability of not being there.

More interesting for and relevant to our purposes is the second objection:

> [It] is essentially connected with a multidimensional representation (configuration space) because only this representation makes possible the interpretation of $|\Psi|^2$ belonging to interpretation II. Now, it seems to me that there are objections of principle against this multidimensional representation. In fact, in this representation two configurations of a system which only differ by the permutation of two particles of the same kind are represented by two different points (of configuration space), which is not in agreement with the new statistical results. (Einstein 1927b, 103)

Einstein's objection fails because he confuses points in the two-particle configuration space with states in the state space (a Hilbert space) in which the two-particle wave functions live. But the objection is instructive, nonetheless, because it shows that Einstein was still ruminating on the consequences for quantum mechanics of the failure of classical assumptions about the mutual independence of spatially disjoint systems.

Why Bohr's later memoir omits this objection from Einstein is not clear. The lapse of time and a lapse of memory could explain it just as well as could the hypothesis of a conscious and deliberate rewriting of history to reinforce the idea of a Copenhagen hegemony. But time lapses and memory lapses are less persuasive in explaining why Bohr's recollection of the still more famous encounter with Einstein at the 1930 Solvay meeting directly contradicts contemporary documentary evidence.

At center stage in the Einstein-Bohr encounter at the 1930 Solvay meeting was Einstein's well-known photon box thought experiment. A box containing a photon has an opening covered by a shutter that is activated by a timer attached to a clock inside the box by means of which we could accurately time the emission of the photon from the box. The whole box is suspended by a spring by means of which arrangement we could weigh the box both before and after the photon's emission with whatever accuracy we desire, thus determining the photon's energy via the mass-energy equivalence relation. As Bohr tells the story, Einstein introduced the photon-box thought experiment for the purpose, yet again, of exhibiting violations of Heisenberg indeterminacy. Simply perform both measurements: weigh the box to fix the emitted photon's energy and open the box to check the clock and fix the time of emission. Bohr tells us that, at first, Einstein had him completely stumped. He could find no flaw in Einstein's reasoning. Only in the wee hours of the morning did it come to him. Ironically, general relativity would save quantum mechanics, specifically the general relativistic effect of a gravitational field on clock rates. A quick calculation showed Bohr that the change in the box's mass when the photon is emitted changes, in turn, its vertical location in the earth's gravitational field, and that the effect of the latter change on the rate of the clock in the box induces precisely the uncertainty in the clock's rate needed to insure satisfaction of the Heisenberg indeterminacy principle (Bohr 1949, 227-228). Bohr uses general relativity against Einstein to save quantum mechanics! A wonderful story. But is it true?

Einstein seems to have thought that they were arguing about something else. We know this from a letter that Paul Ehrenfest wrote to Bohr in July 1931, after a visit with Einstein in Berlin. Ehrenfest and Einstein seem to have had a long and thorough chat about the debate with Bohr at the previous fall's Solvay meeting. Ehrenfest reports to Bohr a most surprising comment from Einstein:

> He [Einstein] said to me that, for a very long time already, he absolutely no longer doubted the uncertainty relations, and that he thus, e.g., had BY NO MEANS invented the "weighable light-flash box" (let us call it simply L-F-box) "contra uncertainty relation," but for a totally different purpose. (Ehrenfest to Bohr, 9 July 1931, Bohr Scientific Correspondence, Archive for History of Quantum Physics. As quoted in Howard 1990a, 98)

What was that totally different purpose? It was nothing other than an anticipation of Einstein's later argument for the incompleteness of quantum mechanics.[11]

As Ehrenfest explains to Bohr, Einstein's idea was this. Let the photon leave the box and be reflected back from a great time and distance, say one-half light year. At about the time when the photon is reflected, we can either weigh the box or check the clock, making possible our predicting either the exact time of the photon's return or its energy (literally, its color), which is to say that, depending upon which measurement we choose, we ascribe a different theoretical state to the photon, one with definite energy, one entainling a definite time of arrival. Crucial is the fact that the event of performing the measurement on the box–weighing it the second time or checking the clock–is space-like separated from the event of the photon’s distant reflection, because then our choice of a measurement to perform can have no effect on the real state of affairs of the photon, meaning that the photon’s real state of affairs when it returns will be one and the same, regardless of the measurement we performed on the box. This is all just quantum mechanics, in Einstein’s view. But then quantum mechanics has associated two different theoretical states with one real state of affairs, which is possible only if the quantum theory’s state descriptions are incomplete.

If you are unfamiliar with the recent revisionist literature on Einstein’s incompleteness critique of quantum mechanics and are, therefore, saying to yourself that you do not recognize in the immediately foregoing a precursor to the EPR argument, you are right. But stay tuned, for we will shortly see that Einstein did not write the EPR paper, did not like the argument it contained, and proposed, instead, his own rather different argument for the incompleteness of quantum mechanics, one that is, indeed, foreshadowed by the argument that Ehrenfest reports to Bohr. That later argument, like the one Ehrenfest describes to Bohr, turns upon the consequences of the space-like separation between two events, this being in both cases the premise for ascribing to the systems involved in each (box and photon in the case of the 1930 Solvay thought experiment) separate, independent, real states of affairs.

What is happening here is that the Einstein who early intimated the deep role that entanglement would play in quantum mechanics and found by 1927 that he could not get rid of the entanglement by a hidden variables interpretation has now come to see in entanglement (still not known by that name) the chief point of difference between quantum mechanics and field theories like general relativity as alternative frameworks for a future fundamental physics and the chief reason for preferring the latter to the former. Einstein will need still a few more years to say exactly why entanglement is unacceptable (again, stay tuned), but that it is unacceptable is something that he seems already clearly to have decided by about 1930.

1935 and Beyond: EPR and Its Aftermath

In the turbulent years between 1930 and 1935 (the triumph of Hitler in Germany, exile, finding a new home in Princeton), Einstein revises and refines his arguments for the incompleteness of quantum mechanics. A note here, a letter there. But finally, in June of 1935, there appears in *Physical Review* what has since become its most frequently cited paper but also one of most misunderstood papers in history of twentieth-century foundations of physics, the EPR paper.

The outlines of the argument are familiar. EPR assume a completeness condition: "*every element of the physical reality must have a counterpart in the physical theory*"; and a reality condition: "*If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity*" (Einstein, Podolsky, and Rosen 1935, 777). They consider then a thought experiment involving two previously interacting systems, I and II, whose post-interaction joint state can be written as a superposition over products of either momentum or position eigenstates of I and II. Under these circumstances, measuring the momentum of system I allows one to predict with certainty the momentum of II, and measuring the position of I allows one to predict with certainty the position of II. Thus, according to the reality condition, there exist elements of physical reality corresponding to both the momentum and the position of system II. But since the operators for position and momentum do not commute (Heisenberg indeterminacy), quantum mechanics cannot contain counterparts for these two elements of the physical reality of system II. Thus, quantum mechanics is incomplete.

One of the most interesting features of the EPR argument is that in order to prove quantum mechanics incomplete, it employs a thought experiment that some might think was designed to exhibit possible violations of Heisenberg indeterminacy. Of course, Einstein, Podolsky, and Rosen did not assert the direct simultaneous measurability of the position and momentum of system I, and so do not assert the indirect simultaneous measurability of the position and momentum of system II, but the argument is supposed to licence an inference to the simultaneous existence of elements of physical reality corresponding to both the position and the momentum of system II, which one might take, wrongly, as proof not only of the incompleteness of the quantum mechanical description of system II, but also its incorrectness. That one might so misinterpret the intended import of the EPR thought experiment reminds one of the fact that Bohr, in his memoir of his debates with Einstein, did so misinterpret or misremember the import of the photon box thought experiment. Bohr thought it was intended to prove quantum mechanics incorrect by exhibiting violations of Heisenberg indeterminacy, but Einstein explained to Ehrenfest that the real point was to prove incompleteness.

Shortly after publication of the EPR paper, Schrödinger wrote to Einstein on 7 June 1935 to congratulate him: "I was very pleased that in the work which just appeared in *Phys. Rev.* you openly seized dogmatic quantum mechanics by the scruff of the neck, something we had already discussed so much in Berlin" (EA 22-044). He was no doubt surprised to receive a reply from Einstein, written on 19 June, that began with these words:

> I was very pleased with your detailed letter, which speaks about the little essay. For reasons of language, this was written by Podolsky after many discussions. But still it has not come

> out as well as I really wanted; on the contrary, the main point was, so to speak, buried by the erudition [die Hauptsache ist sozusagen durch Gelehrsamkeit verschüttet]. (EA 22-047)

Einstein went on to sketch an argument strikingly different from the EPR argument. It begins with what Einstein terms the "separation principle" or "separation hypothesis," according to which the real state of affairs in one part of space cannot be affected instantaneously or super-luminally by events in a distant part of space. Consider, then, two quantum mechanical systems, *A* and *B*, that collide and separate in a manner that engenders correlations between observables of the two systems, a typical scattering problem in which, say, momentum and energy are conserved. I can choose to measure any of various observables of system *A*. Depending upon my choice, quantum mechanics tells me to assign different states to *B*. Of such a situation, Einstein comments:

> After the collision, the real state of (*AB*) consists precisely of the real state *A* and the real state of *B*, which two states have nothing to do with one another. *The real state of* B *thus cannot depend upon the kind of measurement I carry out on* A. ("Separation hypothesis" from above.) But then for the same state of *B* there are two (in general arbitrarily many) equally justified Ψ_B, which contradicts the hypothesis of a one-to-one or complete description of the real states. (EA 22-047)

No reality condition. No Heisenberg indeterminacy. The quantum theory is incomplete simply because it assigns more than one theoretical state to what is supposed to be one and the same real physical state of affairs. And the latter assumption–that *B* possess one and the same real state of affairs regardless of what one chooses to measure on *A*–is guaranteed by nothing but the separation principle. Deny separation principle, affirm the entanglement of systems *A* and *B*, and this argument falls to the ground.[12]

Could it be that Einstein preferred this argument for incompleteness because, by avoiding reliance on Heisenberg indeterminacy, it avoided the confusion that might afflict the EPR argument and did afflict the photon box thought experiment? Whatever the reason, this is the version of the incompleteness argument that Einstein presented in virtually all subsequent published and unpublished discussions of the problem.[13] Einstein later noted that the separation principle is a conjunction of two logically independent assumptions, today termed separability and locality, and he presented deep philosophical premises for each. But the basic logic of Einstein's intended incompleteness argument remained the same.

In brief, separability asserts the existence of independent real states of affairs in spatially separated regions, and locality asserts that the real state of affairs in one region of space cannot be affected super-luminally by events in another region. Locality is entailed not only by relativistic locality constraints but also by the requirement that theories be testable, for were locality not to obtain then there would be no principled way to distinguish the falsity of theory's prediction about the outcome of a measurement from the effects of stray extraneous influences from afar. Separability is defended as a well-nigh necessary, a priori condition for the possibility of the objective individuation of physical systems (more on this below).

There was much debate in the community around Bohr about who should reply to EPR and how. Heisenberg and Wolfgang Pauli discussed the need for a "pedagogical" reply to counter the harm that might otherwise be done among physics students in the US who could be led astray by Einstein's prestige. Heisenberg actually drafted a reply, but deferred when he heard that Bohr had written one as well.[14]

Bohr's reply appeared in the *Physical Review* in October. The reply is curious in that, while Bohr focuses on the published EPR paper, Einstein's own intended argument evidently having not yet made its way to him, he does so in a surprising way that targets directly, if not explicitly, the separability assumption that underlay Einstein's intended argument. The problem, Bohr says, is that there is an "essential ambiguity" in the EPR reality condition. In the EPR thought experiment, the measurement on system (1) produces no classical physical disturbance of system (2), and so the antecedent in the reality condition–"if, without in any way disturbing a system"–seems to be satisfied. But there is, nonetheless, says Bohr, "*an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system*" (Bohr 1935, 700). What does this mean? It means that, in choosing to measure one observable rather than another on system (1), we are choosing between different contexts or "experimental procedures" specification of which is necessary for defining "unambiguously" the "complementary classical concepts" that we might apply in describing measurements on system (2). What does this mean? And what does it have to do with separability?

Work is required to elucidate the connections in such typically Bohrian remarks.[15] Here is the short version. As Bohr said in the Como lecture, according to the quantum theory object and instrument form an entangled pair, neither can be ascribed an independent reality. It follows that we cannot speak unambiguously of the measured value of a property of the object all by itself. But in order to speak intelligibly (Bohr says "objectively") about measurements we must be able to speak of measured values of a property of the object all by itself. That means that we must pretend, contrary to quantum mechanical fact, that object and instrument are not entangled. Doing that is what Bohr means by a description in terms of "classical concepts." The descriptions are "classical" simply in the sense that entanglement is denied and separability is affirmed (again, contrary to quantum mechanical fact). But we can give such "classical" descriptions only relative to the specification of a particular experimental context and then only as regards the physical properties of systems measurable in that context.[16] Such "classical" descriptions are complementary because the contexts necessary for measuring incompatible observables like position and momentum cannot be realized simultaneously.

Demonstrating this last point–that the contexts for measuring incompatible observables cannot be realized simultaneously–was the reason for Bohr's detailed discussion in the reply to EPR of the "realistic" version of the two-slit diffraction experiment. We can measure the vertical position of the particle traversing the apparatus if the first diaphragm, the one containing the single collimating slit, is firmly bolted to the lab bench. We can measure the particle's momentum along the vertical axis if the first diaphragm floats suspended from a spring, allowing an inference to the particle's momentum from the recoil momentum of the diaphragm as the particle passes through it. But that diaphragm cannot be both bolted to the lab bench and floating freely on a spring.[17]

Here is how Bohr, himself, put many of these ideas together just three years later in what is, for Bohr, a comparatively lucid way:

> The elucidation of the paradoxes of atomic physics has disclosed the fact that the unavoidable interaction between the objects and the measuring instruments sets an absolute limit to the possibility of speaking of a behavior of atomic objects which is independent of the means of observation.
>
> We are here faced with an epistemological problem quite new in natural philosophy, where all description of experience has so far been based on the assumption, already inherent in ordinary conventions of language, that it is possible to distinguish sharply between the behavior of objects and the means of observation. This assumption is not only fully justified by all everyday experience but even constitutes the whole basis of classical physics. . . . As soon as we are dealing, however, with phenomena like individual atomic processes which, due to their very nature, are essentially determined by the interaction between the objects in question and the measuring instruments necessary for the definition of the experimental arrangement, we are, therefore, forced to examine more closely the question of what kind of knowledge can be obtained concerning the objects. In this respect, we must, on the one hand, realize that the aim of every physical experiment–to gain knowledge under reproducible and communicable conditions–leaves us no choice but to use everyday concepts, perhaps refined by the terminology of classical physics, not only in all accounts of the construction and manipulation of the measuring instruments but

> also in the description of the actual experimental results. On the other hand, it is equally important to understand that just this circumstance implies that no result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics can be interpreted as giving information about independent properties of the objects. (Bohr 1938, 25-26)

To return to Bohr's reply to EPR, the issues of separability and entanglement are central. When Bohr remarks that there is an influence on the conditions defining possible types of predictions regarding system (2), he has in mind the foregoing analysis of the role of the experimental context. Such attention to context is required only as the means whereby to secure, in an "as if" manner, the kind of "classical" (disentangled) description of measurements required for unambiguous talk of measurement outcomes. And we are forced to this expedient of context-dependent "classical" descriptions only by the circumstance that object and instrument form, strictly speaking, an entangled pair. Were there no entanglement, were object and instrument or the coupled systems in the EPR thought experiment separable, as Einstein assumes, then there would be no question of an influence on the conditions defining possible types of predictions regarding system (2).

Others might struggle to understand Bohr's point. Einstein did not. Einstein saw exactly what Bohr was saying. Writing about the debate over the incompleteness of quantum mechanics fourteen years after EPR, he remarked:

> Of the "orthodox" quantum theoreticians whose position I know, Niels Bohr's seems to me to come nearest to doing justice to the problem. Translated into my own way of putting it, he argues as follows:
>
> If the partial systems A and B form a total system which is described by its Ψ-function $\Psi(AB)$, there is no reason why any mutually independent existence (state of reality) should be ascribed to the partial systems A and B viewed separately, *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, the real situation of B could not be (directly) influenced by any measurement taken on A is, therefore, within the framework of quantum theory, unfounded and (as the paradox shows) unacceptable. (Einstein 1949, 681-682)

Others understood as readily as did Einstein. That is why, prodded by his correspondence with Einstein in the wake of EPR, Schrödinger set about producing, later in 1935, the series of three papers in which he coined the term "entanglement" and developed the details of the quantum mechanical interaction formalism (Schrödinger 1935a, 1935b, 1936). Schrödinger shared Einstein's convictions about the alleged incompleteness of quantum mechanics, but that it assumed entanglement and that the latter assumption enjoyed at least indirect confirmation from the predictive successes of quantum were facts well known to the Schrödinger who first given us the formal tools for describing entanglement when he introduced wave mechanics ten years earlier.

Epilogue

After 1935, Einstein and Bohr returned many times to the question of the quantum theory's completeness and the role of entanglement in securing it.[18] There is a certain repetitiveness in Bohr's later rehearsals of the debate. By contrast, Einstein seemed more open to new reflections. Under the press of Bohr's repeated critiques, Einstein dove steadily deeper in his understanding of the roots of his commitment to separability. In a 1948 article he pointed out that field theories like general relativity assume separability in the most extreme possible form, since, in effect, they regard each point of the space-time manifold as a separable physical system, endowed with its own, independent physical state in the form of, say, the value of the metric tensor at that point (Einstein 1948, 321). He finally got to the bottom of the matter in remarks to Max Born also in 1948. The ostensible subject is Einstein's belief in a robust conception of physical reality. Einstein wrote:

> I just want to explain what I mean when I say that we should try to hold on to physical reality. We are, to be sure, all of us aware of the situation regarding what will turn out to be the basic foundational concepts in physics: the point-mass or the particle is surely

> not among them; the field, in the Faraday-Maxwell sense, might be, but not with certainty. But that which we conceive as existing ("real") should somehow be localized in time and space. That is, the real in one part of space, *A*, should (in theory) somehow "exist" independently of that which is thought of as real in another part of space, *B*. If a physical system stretches over the parts of space *A* and *B*, then what is present in *B* should somehow have an existence independent of what is present in *A*. What is actually present in *B* should thus not depend upon the type of measurement carried out in the part of space, *A*; it should also be independent of whether or not, after all, a measurement is made in *A*.
>
> If one adheres to this program, then one can hardly view the quantum-theoretical description as a complete representation of the physically real. If one attempts, nevertheless, so to view it, then one must assume that the physically real in *B* undergoes a sudden change because of a measurement in *A*. My physical instincts bristle at that suggestion.
>
> However, if one renounces the assumption that what is present in different parts of space has an independent, real existence, then I do not at all see what physics is supposed to describe. For what is thought to be a "system" is, after all, just conventional, and I do not see how one is supposed to divide up the world objectively so that one can make statements about the parts. (Einstein to Born, March 1948, Born 1969, 223-224)

Einstein's point seems to be that only if one adopts a "joints everywhere" view does one have the guarantee of an objective scheme for the individuation of physical systems. If any spatial separation, even an infinitesimal one, suffices to mark two systems as separable, the we can, by convention, carve up the universe any way we wish, confident in the knowledge that we have thereby objectively specified the basis of our physical ontology.

But if quantum mechanics and its descendants, like quantum field theory, are well confirmed, as they so far seem to be, then the entanglement that they take as fundamental seems likewise to be well established, which means that Einstein was wrong. But then the burden falls upon the defenders of quantum mechanics to explain how, while denying separability, quantum mechanics nevertheless rests on an objective ontological foundation.[19]

Einstein was a stubborn defender of the separability principle that he regarded as fundamental to field theories like general relativity and as necessary for our having a coherent ontological foundation for physics. Bohr was a stubborn defender of the quantum theory's integrity, seeing in entanglement not a source of incoherence but the clue to the deep philosophical lesson of complementarity. Theirs was not a clash between a dogmatic bully and a senile old man. Theirs was a clash between two determined seekers after truth. They both knew that a deep truth was to be discovered where separability and entanglement came into conflict.

A Concluding, Personal Remark

The issues discussed in this paper were regular topics of conversation between Mara and me over a period of more than fifteen years. A complete record of my intellectual debt to her would require a footnote at the end of every paragraph thanking Mara for this or that insightful point or for pressing me to seek greater clarity about this or that aspect of an argument. Let this note, instead, do the work of acknowledging that the influence of Mara's always friendly, always constructive, while always tough-minded philosophical spirit is felt as every word makes its way to the page. I miss those conversations with Mara. I am the poorer for want of them. I am the better for having been graced by her friendship.

ENDNOTES

1. Useful histories of quantum mechanics include Jammer 1966, 1974 and Mehra and Rechenberg 1982-2001. Nothing of comparable scope or thoroughness has been attempted for relativity, but one helpful source is Kragh 1999, and much of the history of relativity can be learned from Pais 1982.

2. Some of the better literature on Bohr's philosophy of physics includes Faye 1991, Faye and Folse 1994, and Murdoch 1987. An especially helpful source specifically on Bohr's views on interpretation is Chevalley 1991. See also Howard 1979, 1994, and 2004, and Pais 1991,

3. Important primary and secondary sources on Einstein and philosophy include Schilpp 1949, Holton 1968, Fine 1986a, and Paty 1993. See also Pais 1982 and Howard 1990b, 1993, 2005, forthcoming.

4. Bohr 1949 and Jammer 1974, 1985 are the principal sources for this standard narrative. That the standard narrative still thrives is shown by the example of Bolles 2004.

5. For doubts about the standard narrative, see Fine 1981, Stachel 1986, Howard 1990a, and Cushing 1994.

6. Beller's is but the culmination of a long series of works critically engaging the history of interpretations of quantum mechanics. See also Beller 1992a, 1992b, 1993, 1996a, 1996b, 1997, and Beller and Fine 1994.

7. Heisenberg's role in creating the Copenhagen interpretation and how that view differed from Bohr's views on interpretation are discussed in Howard 2004.

8. For a discussion of the early and enduring place of entanglement in the development of the quantum theory and in debates about its interpretation, see Howard 2006.

9. For a more detailed discussion of Einstein's 1905 photon hypothesis paper and the subsequent history of Einstein's worries about entanglement, see Howard 1990a.

10. I discuss Bohr's complementarity interpretation of quantum mechanics in much greater detail in Howard 1979, 1994, and 2004.

11. That Einstein had never intended the photon box thought experiment as an objection to indeterminacy has been overlooked in part because Jammer mistranslated the crucial clause in this letter. Where Ehrenfest says to Bohr that Einstein told him that he "had BY NO MEANS invented the 'weighable light-flash box' . . . 'contra uncertainty relation'" ["Er sagte mir, daß er schon sehr lange absolut nicht mehr an die Unsicherheitsrelation zweifelt und dass er also z.B. den 'wägbaren Lichtblitz-Kasten' (lass ihn kurz L-W-Kasten heissen) DURCHAUS nicht 'contra Unsicherheits-Relation' ausgedacht hat, sondern für einen ganz anderen Zweck."], Jammer paraphrases Ehrenfest thus: "Einstein, continued Ehrenfest in his letter to Bohr, no longer intends to use the box experiment as an argument 'against the indeterminacy relations' but for a completely different purpose." There is a major difference between saying that the photon box thought experiment "had by no means been invented" to refute indeterminacy and saying that one "no longer intends" that it be used thusly. See Jammer 1974, 171-172, and 1985, 134-135.

12. Fine was the first person to draw attention the fact that Einstein's intended incompleteness argument differed in important ways from the published EPR argument, drawing attention, in particular, to Einstein's June 19, 1935 letter to Schrödinger. See Fine 1981.

13. A detailed accounting of the later history is found in Howard 1985 and 1990a.

14. The exchange between Heisenberg and Pauli and Heisenberg's manuscript are to be found in Pauli 1985, 402-405, 407-418..

15. For an attempt at such an elucidation, see again Howard 1994 and 2004.

16. A technical result underpins this conceptual point. Assume that we are concerned with observables with discrete spectra. Give me a pure entangled joint state for two interacting systems and specify a "context" in the form of a set of co-measurable joint observables. I will then write down a mixed state that reproduces for those observables (but not, in general, observables incompatible with those constituting the context) exactly the same statistical predictions as were provided by the original pure entangled joint state. Such mixed states are factorizable, allowing one to describe the situation *as if* object and instrument were not entangled. For details, see Howard 1994. For a generalization to observables with continuous spectra, see Halvorson 2004.

17. See Howard 1994 for details.

18. A reasonably complete survey of these later discussions will be found in Howard 1985.

19. Of course Bohmian critics of orthodox quantum mechanics argue that the solution is easy, since quantum mechanics in its Bohmian formulation is a separable theory. There are some intersting technical problems to be addressed in determining whether and how the notion of separability applies in the Bohmian framework. It might seem obvious that Bohmian mechanics is separable, whereas orthodox quantum mechanics is not, simply because Bohmian mechanics assumes that individual physical systems always follow well-defined, space-time trajectories. But it is not often enough noted that, for multi-particle systems, the quantum potential is non-separable. There is also the worry that Bohmian mechanics manages to be a separable theory only at the cost of relativistic non-localities at the level of the hidden variables. Opinion varies on whether this is a failing or a virtue, though there is something more like consensus about the fact that, absent control over distributions of hidden variables in an ensemble of systems, non-locality at the level of the hidden variables cannot be made manifest at the macroscopic level. For a helpful discussion of some of these questions, see Belousek 1999. For thoughts on another approach to fixing an objective quantum ontology, see Howard 1997.

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