Schrödinger, Szilard, and the emergence of the EPR argument

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1 Introduction

Erwin Schrödinger, one of the founding fathers of quantum theory, remained throughout his life a critic of the "statistical"[1] interpretation of quantum mechanics championed by Born, Heisenberg, and Bohr and accepted by almost all of his contemporaries. In particular, his coinage of the term "entanglement" and his famous cat paradox in [Schrödinger, 1935b] were intended to bring out what he saw as fundamental problems of the mainstream position. Together with the paper by Albert Einstein, Boris Podolsky, and Nathan Rosen (EPR) of 1935 [Einstein et al., 1935], which used an entangled state to argue that quantum mechanics cannot be a complete description of physical reality, it has become the foundation of a flourishing field of research in the nature and applications of quantum mechanical entanglement, such as quantum information theory and quantum computation.[2]

However, Schrödinger's worries about entanglement and its implications for the interpretation of quantum theory did not start in 1935. Using his extensive research notes, we will discuss how the emergence of his worries can be dated all the way back to 1926, when quantum theory was first developed. Also based on his research notes and correspondence, we can show that in contrast to the received view among historians and philosophers of quantum theory, that Schrödinger's 1935 paper was merely responding to the EPR publication, he actually struggled with the essential content of this argument four years earlier.[3]

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[1]We follow here Schrödinger's use of the term "statistical interpretation" in a very wide sense of all interpretations accepting the Born probability rule and the reduction postulate, not in the now more common and narrower sense of an ensemble interpretation, which takes wave functions only as referring to statistical ensembles. Hence, Schrödinger disagreed both with the mainstream, which was later called Copenhagen interpretation, and also with Einstein, who defended an ensemble interpretation.

[2]https://www.nobelprize.org/prizes/physics/2022/press-release/

[3]One of us has previously discussed Schrödinger's reaction to EPR in [Uffink, 2020]

We also analyze how his contributions fit into the gradual emergence of the EPR argument. Finally, we show that Schrödinger's notebooks and correspondence contain hitherto unappreciated material that clarifies the origin of the thought experiment and Einstein's further path to the EPR paper. His notes show that there was a further important actor in the development of the argument, Leo Szilard, whose role was until now completely unknown.[4]

2 The EPR argument and Schrödinger's subsequent papers

To begin with, a brief and barely necessary record of the relevant publications: In March 1935, Albert Einstein, Boris Podolsky, and Nathan Rosen (EPR) published their paper on the question whether the quantum mechanical description of reality can be considered complete [Einstein et al., 1935]. They discussed this question by introducing a necessary criterion for a theoretical description to be be complete, and a sufficient criterion for what they take to be 'elements of physical reality'. They argued that the usual view, i.e. that the assumption that the quantum mechanical description of physical systems by means of a wave wave function is complete, leads to a contradiction with their stated criterion for reality. They illustrated this by means of a concrete example, using a pair of particles described by a wave function which is a common eigenfunction of both $P + P'$ as well of $Q - Q'$, where P, Q denote position and momentum of the first particle and P', Q' of the second particle.

In the summer of this same year, Schrödinger started work on what became a series of three papers in which he introduced his famous cat experiment and coined the term "entanglement" to bring out the peculiarity of the quantum description of compound systems in terms of their parts, as well as the idea of "steering".

The first paper he wrote was "*Die gegenwärtige Situation in der Quantenmechanik* [Schrödinger, 1935b], probably by invitation from Arnold Berliner, the editor of *Die Naturwissenschaften* in June 1935.[5] This paper was meant for a general audience, and Schrödinger reports the manuscript as being basically finished in a letter to Berliner of July 25 that year, and sending it off on August 11 1935. It was published in three installments in *die Naturwissenschaften* on November 29, December 6 and December 13. Meanwhile, he also produced a second manuscript "*Discussion of probability relations between separated systems*" that contained more detailed mathematical elaborations and was published by the *Proceedings of the Cambridge Philosophical Society*, where it was received

[4] Leo Szilard (1898 – 1964) had emigrated from his native Budapest to Berlin in 1918 and stayed there until he fled from the Nazis in 1933 to the United States. This is not the place to discuss his wide-ranging research, from initiating information theory with his work on Maxwell's demon, over his essential contributions to nuclear physics and microbiology, to countless patents and his political activism and writing. An extensive biography is [Lanouette and Silard, 1992]

[5] See [Meyenn, 2011, 547].

on August 14 and published in October of 1935 [Schrödinger, 1935a]. The third paper "*Probability relations between separated systems*" was received April 21 1936 and published in the PCPS in October 1936 [Schrödinger, 1936].

It is commonly assumed by philosophers and historians of physics that this series of works by Schrödinger was triggered by the EPR paper. This assumption is, of course, corroborated by Schrödinger's own acknowledgement in a footnote of the first paper: "The publication of this paper gave the impulse for the present–should I say review or general confession?"[6] However, while there is no doubt that Schrödinger was inspired by the EPR paper to *publish* his views, this does not mean that he only *formed* these views in response to EPR. Indeed, his correspondence and research notes show that Schrödinger struggled with, and developed his views on the notion of entanglement ever since 1926, long before he baptized the term. What is more, we show that Schrödinger was quite familiar with the essence of the EPR argument and its consequences already in 1931, four years before the EPR paper appeared. Indeed, it seems likely to us that Schrödinger contributed in the genesis of this argument, or at least that the seminal ideas in the EPR paper stem from the discussions recorded in Schrödinger's research notes.

3 Schrödinger's encounter with entanglement

While Schrödinger coined the term 'entanglement' only in his 1935 paper, his research notebooks show that he encountered the phenomenon early in his development of wave mechanics. Shortly after he had shown the translatability between Heisenberg and Born's matrix mechanics and his own wave mechanics [Schrödinger, 1926b], a debate with Heisenberg began about the correct interpretation of the theory [Lehner and Jähnert, 2022]. Schrödinger tried to preserve his fundamental intuition that wave mechanics showed that atomic processes could be explained by a continuous theory of matter waves, while Heisenberg maintained that the new quantum mechanics was a theory of particles that included discontinuous quantum jumps. To argue for his position, Heisenberg published a short paper [Heisenberg, 1927] in which he showed that in the new theory two coupled oscillators did not exchange energy continuously, as in classical mechanics. Each of them could only have two possible energy values, so the energy exchange had to be in discrete jumps.

Schrödinger tried to counter this argument in two notebooks entitled "Undulatory Statistics."[7] Early on, he clearly formulated the aim of these notebooks: "Averaging over the phases has to completely replace the statistical obscenity from Göttingen."[8] This means, Schrödinger attempted to replace Born's statis-

[6]"Das Erscheinen dieses Arbeit gab den Anstoß zu dem vorliegenden – soll ich sagen Referat oder Generalberichte?" [Schrödinger, 1935b, 845]

[7]"Undulatorische Statistik," Österr. Zentralbibliothek für Physik, Nachlass Erwin Schrödinger, W33-737, https://phaidra.univie.ac.at/o:165508 and W33-738, https://phaidra.univie.ac.at/o:165527.

[8]"Das Mittelbilden über die Phasen muß vollkommen die statistische Schweinerei aus Göttingen ersetzen." W33-737, loose leaf after p. 2

tical interpretation of the wave function by phase averages of continuous wave processes. In this context, he also treated the problem of coupled oscillators, attempting to replace Heisenberg's statistical obscenity by an explicit calculation of the continuous energy exchange, using the time-dependent perturbation theory of wave mechanics he had developed in [Schrödinger, 1926a]. Initially, Schrödinger was quite confident that this would be a straightforward calculation, but he soon realized that there is a problem:

> But if it indeed is the case that there are slow transformations, how does it happen, that in the case of "reaction degeneracy" [i.e. two systems with equal transition energies], Heisenberg could get his strange result with the "as-if"-discontinuities? Obviously, because the joint oscillatory states, which arise through the interaction [. . .], are of such a kind, that they are not resolvable into states of the single systems. Worse, it seems, even if one applies the interaction slowly and removes it again, the remaining oscillatory state will still be of the above-mentioned "irresolvable kind."[9]

These "irresolvable" states are of course nothing but what Schrödinger in 1935 would call 'entangled,' and Schrödinger realized that it was exactly these states that were responsible for Heisenberg's result: The continuously changing state of the two coupled oscillators did not imply that the two subsystems had also states that would change continuously. Hence, it was not possible to say that their energy changed continuously.

Schrödinger talked about this problem in three letters, to Georg Joos on Nov. 17, 1926, to Hans A. Kramers on Nov. 19, and to Niels Bohr on Nov. 25.[10] In the letter to Bohr he is already very explicit about the generality of the problem:

> If one combines in thought any two (let us say unequal) systems to a single one, one obtains the eigenfunctions by forming products, as is well known. But obviously, a linear combination of these products with arbitrary coefficients can in general *not* be conceived as a product of two linear combinations referring to the individual systems. Hence, by a mere *combination in thought*, the state manifold would be enlarged enormously, to wit by states that cannot be described by giving the states of the two individual systems. That is probably nonsense. In the statistical interpretation of the "c" proposed by Born, the nonsense disappears.[11]

[9]"Wenn es sich nun denn aber wirklich um langsame Transformationen handelt, wie kommt es, daß dann in dem Falle, wo es sich um "Reaktionsentartung" handelt, Heisenberg sein merkwürdiges Resultat mit den "Als-ob"-Diskontinuitäten erhalten konnte? Nun offenbar daher, weil die gemeinsamen Schwingungszustände, die beim Wechselwirken sofort sich einstellen [. . .], von solcher Art sind, daß sie nicht mehr in Zustände der Einzelsysteme auflösbar sind. Schlimmer, es scheint, selbst wenn man die Wechselw. langsam anbringt und wieder aufhebt, der übrigbleibende Schwingungszustand von der oben besprochenen "unauflösbaren Art" sein wird." W33-738, p. 17

[10][Meyenn, 2011], letters 110, 111, 116.

[11]"Fügt man zwei beliebige (sagen wir ungleiche) Systeme gedanklich zu einem einzigen zusammen, so erhält man die Eigenfunktionen bekanntlich durch Produktbildung. Ein lineares Aggregat dieser Produkte mit willkürlichen Koeffizienten läßt sich aber natürlich im Allgemeinen *nicht* als Produkt zweier auf die Einzelsysteme bezüglicher Aggregate auffassen. Die

At first, Schrödinger tried to get rid of these "irresolvable states" by *fiat*. He attempted to postulate that in an entangled state $\chi = c_1\phi_1\psi_1 + c_2\phi_2\psi_2$ of two systems Φ and Ψ one could describe one subsystem by simply ignoring the other subsystem, so that the state of Φ would be $\phi = c_1\phi_1 + c_2\phi_2$, even though, as he admitted, this is not "strictly correct." He calmed his conscience with a 'heuristic hypothesis' used in the fourth communication on wave mechanics [Schrödinger, 1926a, 118], that the spatial charge distribution of one particle in a many-particle-system could be calculated by integrating out the coordinates of all other particles in $\chi\bar{\chi}$, hence in this case the charge distribution for Φ would be $c_1^2\phi_1\bar{\phi_1} + c_2^2\phi_2\bar{\phi_2}$, which is the same as $\phi\bar{\phi}$, the charge distribution obtained from the reduced state.

However, it did not take long until Schrödinger realized that his heuristic hypothesis did not work. In the second notebook, he writes:

> Meaning of the Amplitudes
>
> The following difficulty presents itself.
>
> Earlier, I claimed that one could treat the total eigenfunction that one is left with after the separation of the systems simply as the eigenfunction of the separated system; the orthogonal functions of the other system still attached to it are not a problem.
>
> This is wrong for several reasons.[12]

Schrödinger realized that (in the case of a charged oscillator) the reduced state ϕ, as a superposition of different energy eigenstates, would imply an oscillating charge distribution and hence an emission of radiation, while this was not the case for the entangled state χ. Hence his argument that the two states were physically equivalent could not be correct and entangled states could not be reduced to states in physical space

At the Solvay Conference in the fall of 1927, Schrödinger still defended his interpretation of quantum mechanics as a continuous and deterministic theory, but by 1928 he had fallen silent on the issue and it seems that he had given up the hope that such an interpretation was possible. However, even though Schrödinger had already admitted that Born's statistical interpretation could make better sense of entangled states (since they could be understood simply as describing statistical correlations), he remained utterly unconvinced about its viability in general.[13] Rather, he concluded that the quantum mechanical formalism could not correctly describe the energy exchange between two systems

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Zustandsmannigfaltigkeit würde also durch das bloße *Zusammendenken* ungeheuer vergrößert, und zwar um solche Zustände, die sich nicht beschreiben lassen durch Angabe der Zustände der beiden Teilsysteme. Das ist wohl ein Unsinn. Bei der von Born vorgeschlagenen statistischen Deutung der "c" verschwindet der Unsinn."

[12]"Bedeutung der Amplituden: folgende Schwierigkeit bietet sich dar. Ich habe früher behauptet, nach der Trennung der Systeme könne man die Gesamteigenfunktion, die man übrig behält, ruhig als die Eigenfunktion des abgetrennten Systems behandeln, die ihr noch anhaftenden Orthogonalfunktionen des anderen Systems seien nicht störend. Das ist aus verschiedenen Gründen falsch." W33-738, p. 14. This passage can be dated by Schrödinger's mention of a talk with Erwin Fues, Walter Heitler, and Fritz London on May 27 (1927, since this is the year all three were in Zürich with Schrödinger.)

[13]A detailed discussion of Schrödinger's arguments against the statistical interpretation in

through electromagnetic coupling since it did not include a description of radiation and only used forces acting at a distance. This was also to explain why the entanglement of the systems did not cease after the interaction was turned off. In his published response to Heisenberg titled “Energy exchange according to wave mechanics” [Schrödinger, 1927] he used this argument when he mentioned the problem as an “inconvenience” in a footnote. But the notebooks show that he was aware of the fundamental nature of the problem.

4 Discussions in Berlin

After the publication of EPR, Schrödinger began a correspondence with Einstein that has long been considered highly illuminative for their thoughts about the problem.[14] Schrödinger’s first letter starts as follows:

> Dear Einstein! I was very pleased that in the recently published article in the Physical Review you caught the dogmatic quantum mechanics by that coattail that we have discussed so much in Berlin.[15]

And similarly, in his subsequent letter to Einstein:

> We have, after all, discussed these things a lot and with hot heads in the seminars, after you pointed them out.[16]

In these letters, Schrödinger (in Oxford) refers to extensive discussions with Einstein (writing from his holiday address in Old Lyme, Connecticut) on the topic of the EPR paper they had during seminar(s) while they were colleagues in Berlin, from October 1927 until early 1933.

One might wonder about the nature of the discussions Schrödinger is referring to. Is he thinking of general discussions about the interpretation of QM? Surely, they must have had many occasions to talk about that issue, since both were thinking hard and deeply about that problem, and both opposed the Copenhagen interpretation (“die dogmatische Quantenmechanik”, as Schrödinger calls it in the above letter), although in different ways.

Or, one might wonder, is Schrödinger referring to more particular discussions about the thesis defended in the EPR paper, namely that the QM description of reality is *not* complete? This seems to be the view of Von Meyenn, who edited these letters, referring to a retracted Einstein manuscript of 1927 about the completeness of quantum theory [Meyenn, 2011, 527].

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its various forms would go beyond the limits of this paper. See [Lehner and Jähnert, 2022] and Bitbol [1996]. One of us has discussed his specific arguments against Einstein’s statistical interpretation of entangled states in 1935 in [Lehner, 2014].

[14] See, e.g. Fine [1986], Howard [1990], Lehner [2014].

[15] “Lieber Einstein! Ich hab’ mich sehr gefreut daß Du in der eben erscheinenen Arbeit im Physical Review die dogmatische Quantenmechanik auch öffentlich bei dem Schlafittchen erwischt hast, über das wir in Berlin soviel diskutiert haben."Schrödinger to Einstein, 7 June 1935, [Meyenn, 2011, 527]

[16] “Wir haben ja die Dinge, nachdem Du schon vor Jahren in Berlin darauf hingewiesen hattest in den Seminaren viel und mit heißen Köpfen diskutiert.” Schrödinger to Einstein,13 July 1935, [Meyenn, 2011, 551]

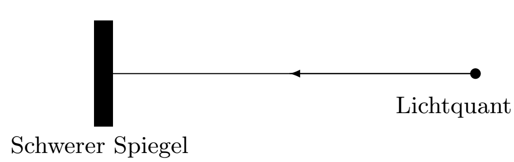
Or could it be, even more specifically, that he refers to the very heart of the EPR argument? To wit, a pair of separated particles described by a particular (entangled) wave function, such that, by a choice of measurement on one particle, we can choose to predict, without ever interfering with the other, either the position or momentum of the other, with arbitrary precision, and the ensuing contradiction with the criterion of reality? Von Meyenn [2011, 540] notes that Rosenfeld [1967] reports an early form of the EPR argument brought up by Einstein in Brussels before leaving for the US in 1933. Another indication for this possibility is that the same letter of June 7, 1935, by Schrödinger also contains the sentence "I have taken the contradiction as proven all along, and don't think of what I am presenting here as a real objection, as I already said in the beginning."[17] The statement that Schrödinger already regarded the contradiction that EPR point out as *settled*, i.e. before he learned of the EPR paper, strongly supports the hypothesis that he had considered the situation that EPR discuss in detail before.

Here, we will argue for this last point of view, and that the discussions mentioned by Schrödinger can be dated to a seminar by Einstein on November 4, 1931. In support of this claim, we discuss research notes in the *Schrödinger Nachlass*, and letters in [Meyenn, 2011] that have as yet not received attention in the philosophical and historical literature. Using these sources we can reconstruct Schrödinger's gradually emerging views in that period.

5 The mirror experiment

The first evidence for our claim that a thought experiment with a striking likeness to that used by EPR, and with almost the same purpose, was discussed in late 1931, is a letter from Schrödinger to Sommerfeld, dated 11 December 1931. He writes:

> The question of the foundations torments me more and more — and unfortunately quite fruitlessly. Can I babble a bit to you? We have discussed in the Seminar for hours about the following, actually quite simple problem (taken one-dimensionally):



> We measure the *momentum* (=0) of a heavy mirror *very* accurately, and simultaneously the *position* of a light quantum *very* accurately. (the color

[17]"Ich hielt bisher den Widerspruch für erwiesen und halte auch, wie ich schon anfangs sagte, das hier Vorgebrachte nicht für einen wirklichen Einwand."[Meyenn, 2011, 528]

> of the light quantum and the position of the mirror are therefore very uncertain.) Then I let the light quantum reflect on the mirror. It thereby absorbs twice the momentum of the light quantum. After this has happened and the light quantum has moved a distance away, I have the *choice*, by measuring *on the mirror*, of determining either the *position* or the *color* of the light quantum.
>
> To wit, if I measure the *position* of the mirror, this will coincide (for a sufficiently heavy mirror) with sufficient precision with *that* position at which the collision occurred to make an inference possible about the position of the light quantum, the initial position of which was unknown, like l said. If I decide, however, to perform a momentum measurement on the mirror (forgoing the *position* of the mirror), then I can infer the color of the light quantum, since its initial momentum was known.
>
> Thus, even though I can *not* learn *both* position and color of the light quantum simultaneously, I can learn, *without intervention on the light quantum*, either one or the other, whichever pleases me. [...] Since the light quantum has nothing to do with my manipulations of the mirror, one cannot properly say that it "obtains" the precise position or precise momentum only by one of these operations, at least this is no "obtaining" in the real, at most in the mental sense. Therefore one likes to conclude that the light quantum *possesses* at any time a completely specified position *and* a completely specified momentum—a notion, one wants to object, that we have long ago abandoned as too strict and paradoxical.[18]

It is almost unnecessary to point out the many similarities between Schrödinger's *babbling* and the example of the EPR paper: it concerns a compound of two systems, that have once interacted (by collision), but are since spatially separated. Secondly, there is a *choice* of measuring either position or momentum on one of those (the mirror), and thereby inferring a prediction on the other,

[18]"Die Grundlagenfrage quält mich mehr und mehr – und leider ganz unfruchtbar. Darf ich Ihnen ein Bißchen vorschwätzen? Wir haben im Seminar stundenlang diskutiert über den folgenden, eigentlich so einfachen Fall (eindimensional gedacht):

Wir messen von einem schweren Spiegel den *Impuls* (=0) *sehr* genau und gleichzeitig den *Ort* eines Lichtquants *sehr* genau. (Farbe des Lichtquants und Ort des Spiegels sind also sehr unsicher.) Dann lasse ich das Lichtquant an dem Spiegel reflektieren. Er nimmt dabei den doppelten Impuls des Lichtquants auf. Nachdem dies geschehen sei und das Lichtquant sich ein Stück entfernt hat, habe ich die *Wahl* durch Messung *am Spiegel* entweder den *Ort* oder die *Farbe* des Lichtquants festzustellen. Messe ich nämlich den *Ort* des Spiegels, so stimmt derselbe (bei hinreichend schwerem Spiegel) noch hinreichend genau mit *dem* Ort, wo der Zusammenstoß stattfand, überein, um eine sehr genaue Ortsangabe über das Lichtquant, dessen Anfangsort ja unbekannt war, zu machen. Entschließe ich mich hingegen (unter Verzicht auf den Spiegel*ort*) eine Impulsmessung am Spiegel vorzunehmen, so kann ich, da sein Anfangsimpuls bekannt war, auf die *Farbe* des Lichtquants schließen.

Ich kann also *nicht* Ort *und* Farbe des Lichtquants gleichzeitig erfahren. Aber ich kann *ohne Zugriff am Lichtquant* entweder das eine oder das andere erfahren, je nachdem es mir beliebt. [...] Da das Lichtquant mit meinen Manipulationen am Spiegel gar nichts zu schaffen hat, kann man nicht gut sagen, es "bekommt" den scharfen Ort oder den scharfen Impuls erst durch eine dieser Operationen, jedenfalls ist das kein "Bekommen" im realen , sondern höchstens in mentalem Sinne. Man möchte darum schließen, daß das Lichtquant jederzeit einen ganz bestimmten Ort *und* einen ganz bestimmten Impuls *besitzt* – eine Auffassung, die wir doch eigentlich längst als zu hart und paradox verlassen haben. " [Meyenn, 2011, 489–490] without physically interfering with ("*without intervention on*") the other system, even if we cannot actually make both choices simultaneously. Schrödinger argues that *both* these inferences must correspond to "physical reality" precisely because one can choose arbitrarily which procedure to perform, without disturbing the second system.[19]

Indeed, the only aspects of the EPR paper that are *not* present in the above argument are (i) the clear logical formulation of the premisses in the argument, and (ii) any concerns about completeness (we will argue that this is a persistent difference between Einstein and Schrödinger) and, of course, (iii) that this letter, unlike the EPR paper, does not offer a concrete wave function to substantiate the claims made.

There are two points we want to note from this letter of Schrödinger. First, it shows that the main content of the EPR argument was quite familiar to him in December 1931. Secondly, this letter again refers to long discussions after a seminar in Berlin, which we can now date to late 1931.

6 Einstein's colloquium of November 4, 1931

The series of Physical Colloquia on Wednesday afternoons were more or less the intellectual heartbeat for the community of theoretical physicists in Berlin in the late 20s and early 30s.[20] However, there is, as far as we know, no surviving full record of the speakers and their talks in this period. There are, however, further manuscripts in Schrödinger's *Nachlass* that provide a clue to what particular seminar he is referring to in his letters to Sommerfeld and to Einstein discussed in the last two sections. We will present the documents in this section but give a detailed analysis of their content only in the next section. For later reference, we will number the documents in square brackets.

There are two folders named "*Szilard Spiegelmessung 1931*" in Schrödinger's *Nachlass*[21]. The second of these folders contains an untitled and undated manuscript [1] consisting of just 2 pages.[22] It sketches exactly the same thought experiment mentioned in his letter to Sommerfeld, but doesn't draw any conclusions from the setup. By its inclusion in a folder named "Szilard's mirror experiment", and by being the *only* document in this file that discusses an experiment with a mirror, we can associate Leo Szilard's name with the thought experiment that Schrödinger describes in his letter to Sommerfeld.

[19]It might be of interest to report Sommerfeld's December 15 1931 response to this argument, not contained in Von Meyenn's collection: "About the other problem, Bethe says: You have to talk consistently about position and *momentum* (not velocity) of the light quantum. With your universal instrument you can measure the position of the light quantum, up to a precision that depends on the momentum, or the momentum with a corresponding ignorance of the position. Whether that calms your worries?" The quote shows that Bethe, like other physicists that later responded to EPR, mistook the argument as a simple corollary to the uncertainty relations.

[20]Moore [1989, 242]; Kallmann [1966].

[21]Österr. Zentralbibliothek für Physik, W33-386/1, https://phaidra.univie.ac.at/o:259770 and W33-386/2, https://phaidra.univie.ac.at/o:259837.

[22]Page 40–41 of https://phaidra.univie.ac.at/o:259770

Another manuscript [2] from the first folder called *Szilard Spiegelmessung 1931*[23] is also untitled but dated November 4, 1931, and starts with the sentence: “This concerns a question raised by Szilard at the colloquium yesterday." Taken literally, this would refer to a colloquium on Tuesday, November 3 1931, but since the weekly colloquium series in Berlin always took place on Wednesdays, it seems unlikely there was any such colloquium on that day. In fact, this manuscript, written in pencil with corrections in ink, does have an ink correction on its date, the original pencil manuscript read “5 November 1931”. Hence it is plausible to assume that Schrödinger corrected the date to refer to the colloquium itself, rather than the date the manuscript was written.

This manuscript has 11 numbered pages. It explicitly attributes his whole train of thought to a question raised by Szilard at a colloquium the day before. This manuscript is much more interesting than the previous document, since it analyses the thought experiment on a more general and abstract level of an entangled wave function for two arbitrary quantum systems, again attributed to Szilard. It analyses the mathematical conditions needed for a conclusion of the kind mentioned in his letter to Sommerfeld. Indeed, the entangled wave function for this bipartite system is equivalent to the example used by EPR four years later. Although this manuscript shows Schrödinger grasping his way more or less reluctantly about the conclusion, by the end he has convinced himself that this bipartite entangled wave function does allow the conclusion he would announce to Sommerfeld.

Already Max Jammer [1974, 172] has noted that Einstein presented a colloquium on November 4, 1931, entitled “ *Über die Unbestimmtheitsrelation*” in Berlin. Although no manuscript of this talk is available, a summary was published in the *Zeitschrift für Angewandte Chemie* in 1932 [Anonymous, 1932]. From this summary it is clear that Einstein presented a version of his well-known photon box thought experiment, which he had earlier discussed at the Solvay meeting in 1930 [Bohr, 1949], and that he also used in his joint publication with Richard Tolman and Boris Podolsky in March 1931 [Einstein et al., 1931]. As Jammer has shown, the central point of the photon-box experiment was that an observer could choose, by performing one of two possible measurements on a box, from which a photon has escaped, which of two noncommuting observables to measure on a photon, without in any way interacting with the photon. In this regard, it was a direct precursor of the EPR argument.

We therefore conclude that this is the particular colloquium that Schrödinger referred to in the letters mentioned above. There is further evidence that Schrödinger knew of the contents of this talk: an undated manuscript [3] entitled *Bohr-Einstein-Beispiel* in an unnamed folder with various notes about quantum mechanics[24]. Even though they contain the remark “as Einstein said to me” and therefore are probably not notes from the talk, they fit the description of the published summary of this talk very well. It is of interest to note that the thought experiment discussed in this colloquium does include a reflection of

[23] Page 3–13 of https://phaidra.univie.ac.at/o:259837

[24] Österr. Zentralbibliothek für Physik, W33-781/4, https://phaidra.univie.ac.at/o:167034

a light quantum by a mirror, even when this was quite unessential to Einstein's main argument.

Hence, the presumable course of events is that in the discussion after Einstein's talk, Leo Szilard brought up the much simpler example of a light quantum being reflected from a heavy mirror with the claim that it should also achieve Einstein's claim of measuring either of two noncommuting observables without interaction. Schrödinger recorded Szilard's remarks in the undated two-page manuscript in the folder *Szilard Spiegelmessung 2*, which clearly reflects his skepticism about the claim. On the next day, he began work on the long November 4 (or 5) document in the folder *Szilard Spiegelmessung 1*, initially trying to disprove Szilard's proposal. While manuscripts [1] and [3] deal with the problem only in terms of uncertainty relations, in manuscript [2] the question is analyzed in terms of biorthogonal decompositions for entangled wave functions of two arbitrary quantum systems. In the course of this work, which will be described in detail in the next section, he finally convinces himself of the correctness of Szilard's argument. This work finally gets him to the argument summarized to Sommerfeld in December 1931.

The last manuscript [4] we consider is contained in the folder *Supraleitung 1931*. It is often the case that these folders do not merely contain manuscripts relating to the subject on the cover, nor do they contain only manuscripts written in the year mentioned. (For example, this folder also contains lecture notes for his presentation in Pasadena in 1927.) But we rarely encountered cases where dated folders contain manuscripts written after the date on the cover. This circumstance, and the fact that Schrödinger's 1931 letter to Sommerfeld, besides the "babbling" about the mirror experiment, focuses on a particular conjecture Schrödinger entertained on the mechanism of superconductivity, leads us to believe that this manuscript should also be dated to late 1931. This manuscript is entitled *"Unique Decomposability into a Biorthogonal Series."*[25] It shows that in 1931 Schrödinger considered the question of whether the biorthogonal decomposition of an entangled wave bipartite wave function is unique, which is crucial for the example used in an EPR-like argument. We will find him rehearsing the same question in 1935, immediately before his notes in preparation of his June 1935 letter to Einstein,[26] and the same argument is included almost literally in [Schrödinger, 1935a].

7 Analysis of the documents mentioned

Einstein's presentation *Über die Unbestimmtheitsrelation* on November 4 1931 concerned, according to the summary in *Zeitschrift für Angewandte Chemie*, a box filled with radiation, and containing a clock mechanism that is so constructed that it will open a shutter for a very brief time, allowing a monochro-

[25] *"Eindeutige Zerfällbarkeit in eine biorthogonale Reihe"*. Österr. Zentralbibliothek für Physik W33-388, pages 35–37 of https://phaidra.univie.ac.at/o:259940

[26] *"Entwicklung e. Funktion zweier Variablen in einebiorthogonale Reihe"*. Österr. Zentralbibliothek für Physik W33-598, pages 95–96 of https://phaidra.univie.ac.at/o:159711

matic light pulse of about a hundred wave lenghts to escape from the box. This light pulse is allowed to travel for a long time until it hits a mirror at a known location (many lightyears away) and sent back to an observation site. The crux of Einstein's argument is that after the light pulse left the box we have a choice between two different options: (i) we could reweigh the box very accurately, from which we learn the amount of energy that is missing from the box, and thereby make an accurate prediction of the energy (or colour) of the light pulse, or (ii) inspect the clock mechanism, which would allow an accurate determination of the time at which the pulse was released and thus allow a prediction of the time of its return to the observation site. It is emphasized that we cannot do both simultaneously, but that we have a choice of doing one or the other measurement, even while the pulse is arbitrarily far away, and obtain a definite prediction about either the energy or about the time of arrival of the photon. Einstein also mentioned that Richard Tolman had extended the thought experiment to show that even for the past one can make only one of those two statements accurately.

This thought experiment is today commonly known as the *photon box* thought experiment. Einstein had presented a similar argument at the Solvay conference in 1930, well-known from the discussion that Bohr [1949] provided in his "Discussions with Einstein on epistemological issues." The same set-up also appeared in Einstein et al. [1931] (although with a different purpose). Schrödinger's document titled "Einstein-Bohr Beispiel" [3] also discsses this example. The argument presented is very similar to the summary in [Anonymous, 1932], except that Schrödinger explicitly locates the mirror at Sirius, and consistently uses the term "light quantum" where the summary talks of a light beam. Notably, Schrödinger's notes are critical of Einstein's conclusions. He remarks (with reference to a remark by Eugene Wigner) that any quantum system in an eigenstate of energy is also stationary in the course of time, and to expect that such a system behaves one way at 12^h00^{min} and do something completely different at $12^h00.000.0001^{min}$ would be foolish.

The short document [1] in the folder *Szilard Spiegelmessung 1931 Teil 2* begins with a sketch of a mirror and a light quantum very similar to the sketch in the letter to Sommerfeld. It also characterizes the initial uncertainties of momentum and position in the same way: At the initial time $t = 0$, before the reflection of the light quantum, it is assumed that both systems are in minimal uncertainty states; the uncertainty in velocity ΔV of the mirror of large mass M is small, while the uncertainty $\Delta\chi$ in its position is large. For the light quantum, it is assumed that the uncertainty in its momentum Δp is large, while its uncertainty in position Δx is small. Schrödinger also gives the two minimum uncertainty relations

$$M\Delta\chi\Delta V = h \tag{1}$$

$$\Delta x\Delta p = h \tag{2}$$

At a later time t, Schrödinger contemplates either of two measurements on the mirror.

(i): we could measure its momentum, again with the same small inaccuracy $M\Delta V$. from this we can infer the momentum p of the light quantum, with an uncertainty

$$\Delta' p = 2M\Delta V \tag{3}$$

It follows from the uncertainty relation that in that case the spatial width of the photon must at least be of the order $h/(2M\Delta V) = \Delta\chi/2$.

or (ii): We make a very precise measurement of the mirror's position with an uncertainty $\Delta'\chi \ll \Delta\chi$ From this we can infer the position of the photon, with an uncertainty

$$\Delta' x = \Delta x + \Delta'\chi + t\Delta V \tag{4}$$

Rather than stating the conclusion that the product of these two uncertainties can be arbitrarily small, Schrödinger calculates limits on the time t of the second measurement, possibly with the motivation to disprove this conclusion. But he only gets that $t\Delta V$ must be at least of the order of magnitude of h/Mc, which is no limit on the uncertainty since M can be arbitrarily large. He ends with the question: "So beim Licht. Wie bei Korpuskeln?"

However one might interpret these sketchy notes, this discussion of the mirror thought experiment is clearly the basis of the discussion in Schrödinger's letter to Sommerfeld, even though it doesn't state the conclusion, of which Schrödinger still was skeptical at this point (as we will see from the next notes). It provides the same logic and intention as Einstein's photon box argument, but avoids the complications of dealing with general relativity and the difficulty of the time-energy uncertainty relation.

The notes [2] dated November 4 1931 are much more elaborate and interesting. Schrödinger begins by formulating the problem as follows:

> This concerns the following question, raised yesterday by Szilard during the Colloquium: Two systems S and S' are separated; is it possible by a measurement to be performed on S, depending on how one sets it up, to make either one of two conjugated variables (A', B') of system S' "sharp" to such an extent that the product of both their unsharpnesses is less than h?[27]

Already from two preliminary ideas he formulates, it becomes clear that he is highly skeptical about Szilard's idea: he describes the possibility of such a measurement as "quantum mechanical nonsense" and "paradoxon." On the following pages, he tries to find a proof from the formalism of quantum mechanics that such a measurement cannot exist.

Schrödinger starts out by writing down, as an "extreme case," a bilinear decomposition of an entangled state

$$\Psi = \sum c_n \psi^{S}_{C,n} \psi^{S'}_{A',n} \tag{5}$$

[27]"Es handelt sich um folgende, gestern von Szilard im Kolloquium aufgeworfene Frage. 2 Systeme S und S' werden getrennt; ist es möglich, durch eine an S anzustellende Messung, je nachdem man sie einrichtet, entweder die eine oder die andere von zwei konjugierten Variablen [inserted: (A', B')] des Systems S' in solchem Maße 'scharf' zu machen, dass das Produkt dieser beiden Unschärfen kleiner ist als h?" Österr. Zentralbibliothek für Physik, W33-386/1, https://phaidra.univie.ac.at/o:259770, p. 3

in eigenstates $\psi_{A',n}$ of A' on S' and eigenstates $\psi_{C,n}$ of a suitable variable C on S, assuming that the state Ψ allows a bilinear decomposition in eigenstates of A'. (He confirms, with a short calculation, that in general the decompositon with a given A' will not lead to pairwise orthogonal functions on S, which of course implies that they cannot be eigenstates of any variable C.) He now wants to show that, if Ψ is decomposed into eigenfunctions of B',

$$\Psi = \sum c_n \psi_n^S \psi_{B',n}^{S'} \tag{6}$$

the corresponding component functions ψ_n^S *cannot* be eigenfunctions of any variable D on S.[28]

However, Schrödinger doesn't pursue this track, but shifts to continuous variables, writing Ψ as an integral of wave functions in position space

$$\Psi = \int \psi(Q, C)\phi(q, C)dC, \tag{7}$$

where Q and q are the position variables in S and S', respectively. Only from the context it becomes clear that he is thinking of the $\psi(Q, C)$ as eigenfunctions of C in S and $\phi(q, C)$ the unnormalized but also orthogonal relative functions on S' determined by the overall state Ψ, so that a measurement of C on S will determine the relative state of S' to be $\phi(q, C)$. On page 4 of the manuscript, he formulates the following question:

> "I will first suppose: every C-value will push the variance of q below a certain limit. Then, the variance of p will be larger than a certain limit for all $f(p)$. I next want to show that by linear combination one cannot build a function with a smaller variance. Is this so? this would already be a major part of my conjectured theorem."[29]

This means, he has now shifted to using p and q as notations for the two conjugated variables of system S' that he previously called A' and B'. He assumes that the $\phi(q, C)$ are narrow functions of q, so that a measurement of C will determine q with a precision narrower than a certain limit $\Delta q = \gamma$. From that it follows by the uncertainty relation that C determines relative wave functions $f(p, C)$ of the conjugate momentum p on S' that can determine the value of p

[28] The second "preliminary idea" already makes clear two important aspects of Schrödinger's thinking: He takes the correctness of Szilard's assumption without further discussion as a violation of the uncertainty relations (hence "quantum mechanical nonsense"), i. e., already here he implicitly assumes the point he argues for in the letter to Sommerfeld, that the mere possibility of the two measurements without interference establishes a fact about both values; secondly, he also admits a more general situation than later EPR, allowing the possibility that the determination of D will only sometimes lead to a determination of B' that conflicts with the uncertainty relations. This will also be a point he brings up in his letter to Einstein from June 7, 1935, criticizing the EPR example as an exceptional case.

[29] "Ich werde zuerst annehmen: *alle* C-Werte drücken die Streuung von q unterhalb eine gewisse Grenze herab. Dann liegt die Streuung von p für *alle* $f(p)$ oberhalb einer gewissen Grenze. Dann will ich zeigen, daß durch "Linearkombination" aus den f keine Funktion erzeugt werden kann mit kleinerer Streuung. Ist das so? Das wäre schon ein großer Teil des vermuteten Satzes."

only with a precision lower than $h/4\pi\gamma$. He now wants to show that by measuring any other variable D on S one will get relative wave functions $g(p, C)$ on S' that cannot determine the value of p with greater precision. He hopes to achieve this since also the $f(p, C)$ form an orthogonal basis on S' and hence the $g(p, C)$ must be linear combinations of them, so he only has to show that the variance of a linear combination of complex functions is larger than any of its components.

The more general "conjectured theorem" is the statistical version of this statement, which he formulated on p. 2: If only a fraction w_A of C-values give a precision better than γ, the fraction w_B of D-values that give a precision of $h/4\pi\gamma$ must be satisfy the inequality $w_A + w_B \leqslant 1$. If that is the case, then we can assume, even though we cannot perform both measurements at the same time, that there are no cases where the uncertainty relations are violated. Note that it is exactly this kind of statistical argument that was falsified by the Bell inequalities.

Of course, with the virtue of hindsight, we know that Schrödinger's conjecture is wrong in the case of complex functions: any wave-packet with finite variance in (say) position can be decomposed into plane waves, where each component has infinite variance; it can also be decomposed into delta functions with zero variance. There is no theorem that could tell us whether the variance of a given operator for a given wave function will either exceed or be less than the variances of the same operator for the components of that superposition. What mislead Schrödinger is that this is the case for positive-valued probability distributions, which he proves on p. 5. However, his attempt to generalize the proof to complex functions fails. His calculations are not carried to the full end, but stop in the midst of an argument, when he finally realizes that he is on the wrong track:

> *Finished*, the end. In this simple form the statement is *wrong*. [the last word underlined three times].[30]

Schrödinger makes another attempt, proving the much weaker statement that if all C-measurements determine q with a precision narrower than γ, then the *overall* variance of p (for any C) will be larger than $h/4\pi\gamma$. However, his attempts break off in mid-sentence.

Instead, on p. 8 of the manuscript a completely new train of thoughts begins. Schrödinger writes:

> Szilard gives the following counterexample against the theorem I conjectured:
> $$\Psi(x, y) = e^{ik_0(x+y)} \int_{-\infty}^{+\infty} \alpha(k) e^{ik(x-y)} dk \tag{8}$$

We can note immediately the striking similarity of this state with the one given by EPR, but more about that below. Szilard's function is an exact eigenfunction of the total momentum $p_x + p_y = \frac{\hbar}{i}(\frac{\partial}{\partial x} + \frac{\partial}{\partial y})$ with eigenvalue $2\hbar k_0$. It is also,

[30]"*Schluß*, Ende. So einfach ist der Satz *falsch*."

when $\alpha(k)$ is mostly constant over a wide range, so that the integral in the above expression is a very narrow function of $x - y$, nearly an eigenfunction of $x - y$. Schrödinger immediately notes that (8) fulfills Szilards conjecture: If we measure the position x of the first subsystem, we can very nearly predict the value of y of the other. But if we measure the momentum p_x of the first subsystem, we can exactly predict the momentum of the other.[31]

In the following pages, Schrödinger thinks about how to generalize the function given by Szilard. He finds that if a joint wave function is to give an exact correlation between two coordinates, it simply has to depend only on the difference between them, $\Psi(x, y) = \psi(x - y)$, and that any such function will also imply for the momenta that $p_x + p_y = 0$. He concludes that the factor $e^{ik_0(x+y)}$ in Szilard's example only adds a total momentum and can be removed by a change of the reference frame. He also discusses the possibility of putting $\alpha(k) = const.$; interestingly he dismisses it as meaningless, presumably following von Neumann in rejecting non-normalizable and improper eigenfunctions like Dirac's delta-function. But he does note that if $\alpha(k)$ is, say, a function that is constant over a broad range of k-values, the integral in (8) will be a function that has a sharp peak in the x-values.

As we already remarked above, Szilard's function is remarkably similar to the wave function used by EPR in 1935, which, in the present notation, reads

$$\Psi(x, y) = \int_{-\infty}^{+\infty} e^{ik(x-y+x_0)} dk \tag{9}$$

The only differences are: (i) EPR do not use the factor $e^{ik_0(x+y)}$ introducing a total momentum. (ii) They do introduce an additional summand x_0, which means that $x - y$ doesn't peak at 0 but at x_0. This is physically more plausible, since the argument is that the two systems do not interact, hence they should not be at the same position. In Schrödinger's notes, there are only fleeting references to the condition that the two subsystems in question are spatially separated and have ceased to interact. But there are such references: on p. 1 he mentions that the two systems are separated ("getrennt") and on p. 6 he includes the words "nach der Entkopplung", suggesting that the two subsystems have interacted in the past but have ceased to do so. (iii) More importantly: EPR do not use a factor $\alpha(k)$ in the integral which makes it *both* an improper (non-normalized) eigenfunction of $x - y$ as well as $p_x + p_y$ and results in the relative functions of the second particle after a position measurement of the first to be delta-functions. However, this means that both position and momentum are exactly correlated. This is obviously related to the fact that EPR wanted to display "elements of physical reality" that can be predicted with 100% probability, while Schrödinger is satisfied by obtaining predictions that are more definite than allowed by the uncertainty relations.

[31] Schrödinger does make a mistake in the calculation of the relation of the momenta at this point, instead of $p_y = 2k_0 - p_x$ he writes $p_y = p_x - 2k_0$. This is not of importance for his further thought, but would have obscured the analogy with the mirror experiment, in which of course the sum of the momenta is a constant. Schrödinger, in any case, does not attempt to tie Szilard's example to the mirror experiment at all.

By the end of this manuscript, Schrödinger thus seems to have given up his initial skepticism and accepted Szilard's claims not only about the specific wave function, but also about the mirror experiment, even though he never connects one to the other in his notes. But clearly he presents the mirror experiment in his letter to Sommerfeld one month later without any of his initial doubts. Even though he presents the problem there in terms of the mirror experiment, it is the entangled wave function that convinced him, and in his 1935 paper he would not use any physical examples but only the formal apparatus of quantum mechanics. Also, the manuscripts are only concerned with the mathematical conditions that need to be satisfied for a wave function of a compound system to allow the kind of conclusions intended in the thought experiment. There is no trace of the worries about the physical and philosophical implications, besides the early mention of the paradoxical nature of the mirror experiment. However, the letter to Sommerfeld expresses clearly if concisely that Schrödinger agreed with Einstein, that entangled states presented a fundamental problem for the mainstream interpretation of quantum mechanics. When he encountered entanglement in 1926, he saw it as a problem for his interpretation of wave mechanics; now he realized, that it was just as well a problem for his opponents.

8 Einstein's development towards the EPR paper

Einstein's route from his objections to quantum theory presented at the Solvay conferences to the development of the EPR argument has been studied by many historians before. But it has always been a major obstacle that no manuscripts or notes could be found in Einstein's papers, and so the research had to rely on circumstantial evidence like Bohr's remembrances in [Bohr, 1949], which were certainly not impartial or infallible. This is particularly the case for Einstein's photon box experiment. In his presentation, Bohr takes the purpose of Einstein's argument to disprove the uncertainty relation between time and energy. Bohr then proceeds by showing this thought experiment cannot attain this purpose by appealing to the gravitational redshift formula of general relativity. However, as has been convincingly argued by Jammer [1974] and Howard [1990], this was not the intention Einstein had in mind when proposed his thought experiment. In a letter from Ehrenfest to Bohr from July 9, 1931, written immediately after Ehrenfest visited Einstein in Berlin, Ehrenfest wrote:

> [Einstein] said to me, that he hasn't for a long time doubted the uncertainty relation and that he hence thought up the "weighable light-flash box" [...] *in no way* "against the uncertainty relation," but for a quite different purpose.[32]

Ehrenfest continues by describing the photon box as a "machine" that ejects a "projectile" in such a way that, by a two different measurements on the ma-

[32]"[Einstein] sagte mir, dass er schon sehr lange absolut nicht mehr an die [sic] Unsicherheits-relation zweifelt und dass er also z. B. den 'waegbaren Lichtblitz-Kasten' [...] DURCHAUS nicht 'contra Unsicherheits-Relation' ausgedacht hat, sondern fuer einen ganz anderen Zweck." Niels Bohr Archive, Copenhagen

chine, one can predict precisely two noncommuting observables on the projectile, stressing that Einstein agrees that the uncertainty relations prevent us from performing both measurements. However,

> [...] it is interesting to make clear to oneself, that the projectile, which is flying around isolated and 'by itself,' must be prepared to satisfy very different 'non-commuting' prophecies, 'without still knowing,' which of these prophecies one will make (and test). [33]

This is undeniably the logic of EPR, and so we can take it as established that in this regard, the photon box was a direct precursor of the argument in 1935, and that this basic idea was established by the summer of 1931 the latest. On the other hand, it is quite clear that the EPR paper differs from the photon box argument in several important points:

First of all, the photon box argument proposes the procedure of weighing the box to determine its energy content. This idea relies on general relativity and its equivalence principle in particular. Of course, there is, even today, no accepted theory that successfully combines general relativity and quantum mechanics. It is therefore rather unsatisfying that Einstein had to call upon general relativity to express his misgivings about quantum mechanics, and that Bohr's reply used the gravitational red-shift formula to refute Einstein. The appeal to general relativity is at least a distracting point obscuring the essential part of the argument.

Second, the photon box argument focuses on the time-energy uncertainty relation. However, unlike the uncertainty relation for position and momentum, the time-energy uncertainty relation has no simple foundation in the standard formalism of quantum mechanics, where time is not expressed through an operator. As a consequence, there are many proposed formulations of a time-energy uncertainty relation. Again, this is a source of distraction from the issue at stake. EPR avoids both these distractions by operating with position and momentum, the measurability of both is undoubted in standard quantum mechanics.

Third, and most obviously, EPR do not argue with phenomenal descriptions of an experimental setup, but with a specific state given in the formalism of quantum mechanics. This allowed to transform the argument from a metatheoretical critique of quantum mechanics to a question about the coherence and meaning of the theory itself, which through the work of David Bohm and John Stuart Bell has set off the impressive amount of experimental, theoretical, and philosophical work that we face today.

It is clear that Szilard's mirror experiment and the wave function he gave to Schrödinger are the missing piece that would allow Einstein to move beyond the photon box argument towards EPR. The only substantial modification that Einstein added was, as we discussed above, that he abandoned Szilard's form factor $\alpha(k)$, so that the EPR wave function gives an exact correlation of both

[33] "[...] es ist interessant sich deutlich zu machen, dass das Projektil, dass da schon isoliert 'fuer sich selber' herumfliegt darauf vorbereitet sein muss sehr verschiedenen 'nichtkommutatieven' [sic] Prophezeihungen [sic] zugenuegen [sic], 'ohne noch zu wissen' welche dieser Prophezeihungen man machen (und pruefen) wird."

position and momentum and not just a correlation that is stronger than the uncertainty relations would permit. And while we have no direct evidence that Einstein acknowledged Szilard's contribution, he certainly was aware of the mirror experiment, which Szilard, after all, presented in the discussion after his colloquium. It is also highly plausible that Einstein learned, if only through Schrödinger, about Szilard's wave function. When Schrödinger, in his letters from 1935 presented above, expresses his familiarity with Einstein's argument and says that it has been discussed with "hot heads" back in Berlin, this refers, after all, not to any physically motivated thought experiments, which do not appear in EPR, but to the argument based on the explicitly given wave function.

[Howard, 1990] presents several thought experiments that Einstein considered in the early thirties, which we argue, show the impact of Szilard's mirror experiment on his thought. When Einstein met with Ehrenfest in Rotterdam in April 1932, he wrote to him on April 5:

> Yesterday you nudged me into modifying the 'box-experiment' in such a way that would employ concepts less foreign to the wave theorists. I do this in the following, where I employ only such idealizations that I know will appear unobjectionable to you.[34]

He then presents the argument in terms of a photon that scatters from a massive body (both assumed to be moving along the same axis). A subsequent measurement on the photon is then able to reveal either position or momentum of the body. This is simply a reversal of the mirror experiment, with the measurements performed on the photon instead of the body (which is of course Szilard's mirror). The only substantial difference is that now the photon is coming from a direction orthogonal to the axis of movement, so that its original momentum along that axis is zero. Another version, reported by Léon Rosenfeld [1967] as having occurred in 1933 in Belgium, before Einstein's final departure to the United States, is described by him quite briefly and incompletely, but again involves the scattering of two particles and subsequent measurement of position or momentum.

The third, most interesting version is given by Einstein himself in a letter to Paul S. Epstein from 10 Nov. 1945[35], after Epstein had told him that he didn't fully understand the EPR argument. Instead of trying to clarify the formal argument from the paper (for which he referred him to Schrödinger's papers), Einstein offered again a thought experiment, claiming that it was by this thought experiment how he arrived at the argument given in EPR. Here Einstein again presents a photon box able to emit a photon of unknown frequency at an exactly defined moment of time. Initially, the photon box has the precisely measured momentum zero along the axis. After the emission of the photon in direction of a distant receiver, the observer can either measure the momentum of the box

[34]"Du hast mich gestern darauf gestupft, das 'Kasten-Experiment' so abzuändern, dass es dem Wellen-Theoretiker weniger fernliegende Begriffe verwendet. Das mache ich im Folgenden, wobei ich nur solche Idealisierungen verwende, von denen ich weiss, dass sie Dir unbedenklich erscheinen."Hebrew University, Einstein Archives 10-231.

[35]Hebrew University, Einstein Archives, 10-583 and 10-584.

and determine from this the energy of the photon, or measure its position (which hasn't changed by a relevant amount since the previous emission because the box can be made as heavy as one wants) and so determine the time of arrival at the receiver. While this account may seem substantially different at first sight from Szilard's mirror experiment, upon closer inspection, it is a direct translation of it into Einstein's time-energy-photon-box talk: The heavy photon box takes the place of Szilard's mirror with initial momentum zero, instead of a reflection of the photon with initially well defined position we have an emission event that defines the initial position, and the determined arrival time and energy are exactly equivalent to position and momentum of the photon. We therefore conclude, that Einstein's account is a case of the harmonization of memory well-known to the historian: Einstein had formed his convictions with the original photon box experiment early on, but now used the mirror experiment to strip it from its cumbersome and superfluous details in hindsight.

It is striking that in all of these cases, Einstein makes no mention of the specific entangled state used in EPR and its origin. But we think Einstein himself gives a rather straightforward explanation of this in his letter to Epstein:

> I myself am not sufficiently at home in the formalism of quantum mechanics to [...] present the case convincingly.[36]

It was Einstein's general working style in his later years to delegate formal calculations to his assistants. From 1929 to 1933, his assistant was Walther Mayer, a mathematician that was well-versed in differential geometry and so could work with Einstein on unified field theories, but had no experience in the formalism of quantum mechanics. Only when Einstein had settled in Princeton and was able to work with the physicists Podolsky and Rosen in 1934, he was able to develop his argument based on Szilard's entangled state and give the formal argument offered in EPR. Essential to his thinking were always the physical considerations, however.

9 Szilard and Schrödinger after 1931

This leaves us with two questions that may puzzle the reader: If Szilard's contribution was as essential as Schrödinger describes it, why did he completely disappear from the record, and what were his own views on the matter? And secondly, if Schrödinger had all the material for his 1935/36 papers already in 1931, why didn't he publish any of his insights until after the EPR paper?

To the question why Szilard, if he proposed the mirror experiment and the EPR wave function already in 1931, never claimed any priority for this, we have no definitive answer. Szilard's Nachlass does not seem to contain any non-trivial material relating to his period in Berlin (apart from his patents). But there is reason to doubt that Szilard attributed as much relevance to them as Einstein and Schrödinger did: There are two Schrödinger letters about the EPR paper

[36]"Ich selber bin nicht genug zuhause in dem Formalismus der Quantenmechanik, um ohne grossen Zeitaufwand [...] die Sache überzeugend darzustellen."

in which he mentions Szilard's response to the EPR paradox as being in line with other orthodox quantum theorists. He writes to Born on 29 June 1935:

> You act like everyone else (e. g. London, Teller, Szilard) in the beginning. You analyze the matter clearly. An after you said for two and a half pages only things that I could have said exactly the same way, you say abruptly: I cannot understand where there should be anything mysterious about this. [37]

And to Pauli:

> And whether you really think, the Einstein case—let us call it thus—doesn't give us anything to think about, but is completely clear and simple and self-evident. This is how everyone thought when I first talked to them about it, because they have well learned their Copenhagen Credo in *Unum Sanctum*. Three days later there usually came the message: what I said the other day was of course quite wrong, much too complicated. Or (like Szilard): I must first think about what it is that I should forbid you. But I have not yet received a clear explanation why everything is so clear and simple.[38]

Hence it seems that Szilard, like most of his contemporaries, was not impressed at all by EPR. Like Einstein, they interpreted the EPR state as an expression of statistical correlations. But unlike Einstein, they took any question about a reality beyond the statistics of observations as violating the Copenhagen *Credo* and as useless metaphysics. It might well be that Szilard was even embarrassed to admit that he was involved in such shady business. And thus, Schrödinger's critique, just like Einstein's, was ignored by the physics community for decades.

Also the answer to the second question, why Schrödinger did not publish his results earlier, must remain somewhat speculative. One reason might be that unlike Einstein, who by the 1930's had accepted his position as an outsider in the physics community and could afford to because of his longstanding fame, Schrödinger was less willing to openly disclose his disagreements with the mainstream. One can see an example of this when Schrödinger prepared a talk at the German Physical Society in 1929 to be about a critique of "Dogmatic

[37]"Sie machen es nämlich ziemlich genau so wie alle anderen (z.B. London, Teller, Szilard) auf den ersten Anhieb. Sie setzen mit Klarheit den Sachverhalt auseinander. Und nachdem sie auf zweieinhalb Seiten nur Dinge gesagt haben, die ich genau ebenso hätte sagen können, sagen Sie ganz unvermittelt: ich kann nicht begreifen, wo da etwas dunkel sein soll."[Meyenn, 2011, 544]

[38]"Und ob Du wirklich meinst, der Einsteinfall – nennen wir ihn so – restlos nichts zu denken gibt, sondern ganz klar und einfach und selbstverständlich ist. So meinten bisher alle, mit denen ich zum ersten Mal darüber sprach, weil sie ihr Kopenhagener Credo in *unum sanctum* gut gelernt hatten. Drei Tage nachher kam meistens die Mitteilung: was ich neulich sagte war natürlich ganz unrichtig, viel zu kompliziert. Oder es hieß (Szilard): ich muss mir erst überlegen, was ich Ihnen verbieten muß. Aber klare Auskunft, warum alles so klar und einfach ist, bekam ich noch nicht."[Meyenn, 2011, 550] The Credo refers to the passage of the Nicene Creed "I believe in one, holy, catholic and apostolic Church." Schrödinger was certainly well enough versed in catholic liturgy to know that the the correct form is *unAM, sanctAM, catholicam et apostolicam ecclesiam.* The change of gender is thus most probably an ironic reference to a specific high priest in Copenhagen.

Quantum Mechanics," but at a later stage decided to talk about something uncontroversial, the concept of force in wave mechanics.

We think, a more crucial reason is to recall the difference in outlook between Einstein and Schrödinger. Einstein had long made up his mind that quantum mechanics was merely a statistical theory that needed to be completed by a unified field theory, which not only would give a deeper unification of gravitation and electromagnetism, but would also explain the behavior of light quanta and elementary particles [Lehner, 2014]. So Einstein and his collaborators forged the thought experiment into a question of whether the quantum mechanical description of reality is complete. The EPR paper famously ends by saying:

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible. [Einstein et al., 1935, 780]

In other words, EPR conclude by pointing to a positive way out of the conundrum. Schrödinger, by contrast, did not believe that a complete description compatible with quantum-mechanics could exist. He gives a detailed argument against hidden variables in his letter to Einstein from 19 August 1935 [Meyenn, 2011, 565] and makes a similar but more physical argument in [Schrödinger, 1935b, section 4].[39]

Hence, as his writings make abundantly clear, Schrödinger did not see any way out of the paradox in and instead of EPR's confident tone published his critique as a "general confession" about the present state of quantum mechanics. He could only offer the vague hope that the paradox might disappear in a future fully relativistic quantum theory. At the end of [Schrödinger, 1935b] he points out the quandaries of contemporary quantum field theory to argue that the book is not yet closed on a complete relativistic quantum physics. We believe this lack of confidence makes it understandable why Schrödinger, after ten years of worrying about entanglement, its mathematical structure, and envisaging the EPR argument long before the EPR paper, was nevertheless reluctant to publish, even though he could rush out several amazingly sophisticated papers immediately after the EPR paper appeared.

On the other hand, this means that Schrödinger's analysis of entanglement has stood the test of time. Both Einstein and the mainstream saw in entanglement only a statistical correlation and hence did not recognize its full import. Schrödinger however, as the only one that was still critical of the "statistical interpretation," did not only coin the term 'entanglement' but also saw its relevance clearly:

> I would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought. [Schrödinger, 1935a, 555]

[39]Schrödinger's argument, like von Neumann's more famous one, is based on the assumption that quantum mechanical observables are non-contextual. Of course now we have a much stronger argument for the correctness of his claim in the experimental confirmation of the Bell inequalities.

10 Conclusion

We have seen that Schrödinger was well aware of the fact that wave mechanics required the use of entangled states to describe composite systems from 1926 onwards. In his letters he confessed more clearly that this presented an obstacle to his own interpretation of this theory, and it was one of the reasons why he stopped defending it against the statistical interpretation However, until 1931 he did not recognize that entangled wave functions would present an obstacle to the orthodox interpretation too, and was initially even quite skeptical of both Einstein's photon box and of Szilard's mirror experiment. Only when Szilard showed him an explicit wave function to support his thought experiment, Schrödinger realized that entanglement posed a far more fundamental problem for the understanding of quantum mechanics. Schrödinger's research notes show that by 1931 he was well aware of all the crucial aspects of the EPR experiment. This includes not only the essential idea of a thought experiment that allows us either to predict one or another of two conjugated variables with arbitrary precision on a system that is entangled but not interacting with another system. It also extends to the use of a wave function equivalent to the EPR wave function to substantiate these claims.

Since Schrödinger and Einstein were close colleagues and friends in Berlin at this time, it seems eminently likely that Schrödinger must have talked to Einstein about his analyses, and this suggests a likely path how Einstein finally arrived at the simple example exhibited in the EPR experiment. We believe this conclusively shows that Schrödinger's involvement with the EPR argument was not merely a reponse that was triggered by the publication of the EPR paper. Instead, Schrödinger was actively involved in crafting this argument, even if the logic of the argument is clearly due to Einstein. In this light, the many comments from Schrödinger's letters in 1935 that the contents of the EPR paper were already quite familiar to him and discussed at great length back in Berlin ought to be taken more seriously than the historical literature has so far acknowledged. But we also conclude that credit for the original formulation of the famous EPR state has to go to a hitherto overlooked actor, Leo Szilard, adding another example to his impressive list of visionary ideas. Nevertheless, Schrödinger remains the one physicist of his time that understood the fundamental importance of entanglement, while both Einstein and the mainstream physicists thought of it as a mere instance of statistical correlation.

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