1

Einstein on EPR*

1.1 Introduction

One of the most detailed studies of Einstein's dissatisfaction with quantum mechanics is certainly Don Howard's paper, '"Nicht sein kann was nicht sein darf"' (Howard 1990).[1] In it, Howard argues that Einstein was principally motivated by an *a priori* commitment to the separability of physical systems, and that this commitment arose from an even deeper commitment to describe interactions universally via field theories.

We shall not focus particularly on the aspect of Howard's argument regarding field theories as the path to a unified theory, but shall instead focus mainly on the degree to which Einstein's belief in the separability of systems drove his criticism of quantum mechanics in the years leading up to EPR. We agree with Howard that the failure of separability for Einstein was crucial in judging quantum mechanics was incomplete. Because nonseparability just is what we now call entanglement, one may justifiably state that Einstein's concerns related at least indirectly to the phenomenon of entanglement.

We also agree with Howard (as well as with Harvey Brown (1981) and Arthur Fine (1986, p. 28)) that Einstein's concerns were *not* primarily to do with the simultaneous measurability of conjugate variables. The issue of quantum mech-

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[1] But see also his 1985 paper, which is one of the first published discussions of the 1935 correspondence between Einstein and Schrödinger (Howard 1985).

anical completeness was, for him, a separate matter from Heisenberg's uncertainty relations, which he accepted as part of the correct formalism. Debates as to whether and when Einstein accepted the uncertainty relations in principle are typically grounded in studies of the various photon-box thought experiments. We add to these analyses by incorporating some new and some neglected primary sources, including the Einstein–Laue correspondence from 1936. We believe Einstein's recollections to Laue regarding the Rupp experiments show that already in 1926 Einstein was prepared to accept at least one direction of the energy–time uncertainty relation. We point out that the thought experiment Einstein cites in this correspondence as motivation for the Rupp collaboration is not only a precursor to the photon box, but also fits nicely with the views on the statistical nature of the quantum state that Einstein was to express in the context of EPR.

Given this earlier evidence of Einstein's thoughts on incompleteness, what was the real intended thrust of the 1930 photon-box thought experiment? We suggest it might have been whether a precise determination of energy implies a statistical spread in the aperture time – affirmation of which would impute uncertainty to a *macroscopic object* (the clockwork in the box).

The modified history of the evolution of the photon box and of Einstein's thinking proffered here will not only incorporate new materials (like the Einstein–Rupp proposal), but will also examine several moments in this story that have received less attention (e.g. the Einstein–Tolman–Podolsky paper from 1931), yet which we believe contribute importantly to the broader picture.

Following our re-examination of the years leading up to 1935, we present an updated narrative not only about the structure of the EPR paper, but also consider the logic of EPR in immediate contrast to Einstein's own views as expressed in his correspondence with Schrödinger in the summer and autumn of 1935.

A brief caveat on terminology. In his analysis of Einstein's critique of quantum mechanics, Howard provides the following definitions of separability and locality (Howard 1990, p. 64). Separability is 'the condition that spatio-temporally separated systems possess well-defined real states, such that the joint state of the composite system is wholly determined by these two separate states', while locality is 'the condition that such a real state is unaffected by events in regions of spacetime separated from it by a spacelike interval'. However, the concept Einstein christens the 'Separation Principle' in his letter of 19 June 1935 to Schrödinger (see p. 297) does not clearly distinguish between the concepts of locality and separability so defined. Indeed, Howard demonstrates that it is only in Einstein's correspondence with Epstein after World War II that the former makes this conceptual distinction explicit (Howard 1990, p. 64, note 7). Therefore, for the purpose of the present chapter we shall be satisfied to let the term 'separability' stand in for both separability and locality.

1.2 Einstein's Unpublished Hidden Variables Theory of 1927

On 5 May 1927, Einstein gave a report at the meeting of the Prussian Academy of Sciences in Berlin in which he described a hidden variables theory of his own devising.[2] This theory was also the subject of Einstein's brief note to Ehrenfest on that same day, wherein we read the following (EA 10-162 and Einstein 2018a, p. 815):

> I have now also done a little study of Schrödingery, in which I show that in a completely unique way the solutions [of Schrödinger's equation] can be assigned determinate motions that make any statistical interpretation unnecessary.

The paper was never published, for Einstein asked it to be retracted before printing. Various authors have variously interpreted this move; Howard (1990, pp. 89–91) makes a particularly convincing case. Based on examination of Einstein's addition in proof to the 1927 manuscript, Howard suggests that Einstein abandoned this hidden variables attempt upon realisation that its nonseparable features (implied by the non-vanishing cross-terms in the composite wavefunction of two subsystems) would violate 'a general requirement that must be imposed on a general law of motion for systems' (Einstein's 'Nachtrag', as quoted in Howard 1990, p. 90), namely separability of the subsystems.

In his treatment of Einstein's unpublished manuscript, Belousek suggests that Einstein's dissatisfaction with the work stemmed from its inability to provide sufficient causal explanations (Belousek 1996, p. 445; cf. Bacciagaluppi and Valentini 2009, p. 239). Belousek cites as evidence for his claim two 'sins' committed in the 1927 manuscript, the second of which is a sin of omission. Einstein's use of configuration space demonstrates his awareness of the possibility of waves living in a greater than three-dimensional space. And yet Einstein conspicuously avoids commenting on how quantum waves in such non-physical spaces could themselves be considered physical entities. A more recent paper by Holland (2005) describes several specific ways in which Einstein's theory would have failed to reproduce the statistical results already known to hold in quantum theory. Bacciagaluppi and Valentini (2009, p. 235) suggest that Einstein may have been planning to present his hidden variables theory at the Solvay conference that October until realisations such as those discussed by Howard led him to reconsider.

[2] EA 2-100 (unpublished manuscript, 6pp., in German). For more on the 1927 manuscript of Einstein, see Belousek (1996) and references therein.

1.3 The 1927 Solvay Conference

As is well known from the literature (e.g. Jammer 1974, Brown 1981, Howard 1990, Bacciagaluppi and Valentini 2009), during the October 1927 Solvay conference in Brussels, Einstein proposed one of the first in a long line of thought experiments intended to demonstrate shortcomings with the quantum theory. Einstein considers electrons passing through a single slit and impinging on a semi-spherical photographic film. Assigning wavefunctions to the electrons leaves open two interpretations of the event recorded by the photographic film, each associated with a cost. Brown summarises Einstein's reasoning here as follows (Brown 1981, p. 61):

> [Einstein] observed, using an extremely simple example, that a wave perturbation associated with a particle in wave mechanics seems to lead to an action-at-a-distance sort of effect during the process of locating the particle if we consider the wave to represent (as maintained by Bohr and Heisenberg) a complete description of the individual particle. On the other hand, the problem disappears if the [wave] description merely describes probabilities (following the Born rule) in relation to an 'ensemble' of independent particles with well-defined positions at every instant. In this second interpretation, wave mechanics represents an incomplete or 'statistical' description of a system only, leaving open the question of the possibility of a causal mechanism which governs the behaviour of the individual system. It was this position to which Einstein constantly adhered after 1927. [. . .] The crucial point here is that Einstein's comments constitute a criticism of the Copenhagen school, and they do not question the consistency of the formalism of quantum mechanics (and the criticism does not directly concern the indeterminacy relations). Moreover, the points raised by Einstein clearly already carry the germ of his more compelling and detailed 1935 arguments.[3]

Perhaps taking to heart the very problems encountered in his report to the Prussian Academy, Einstein concludes his Solvay comments by declaring dissatisfaction with any theory that relies on multi-dimensional configuration space, because permutations give rise to distinct configurations in this space 'which is not in accord with the new results in statistics', and because it leads to a 'less natural expression' of forces acting at short spatial distances than would a three- or four-dimensional description (Bacciagaluppi and Valentini 2009, p. 442).

There are two points in Brown's analysis we wish to emphasise. The first is the suggestion that Einstein's issues with quantum theory were *not* rooted in the uncertainty relations. Brown was one of the first authors to articulate this crucial point. Howard (1990, p. 94) would later corroborate this claim by proposing that if Einstein's concerns at Solvay in 1927 were tied to the uncertainty relations, it would be hard to explain his utter silence about them in correspondence and in print afterwards.

[3] Special thanks to Harvey Brown for sending us a copy of this paper, and even highlighting relevant passages! We also thank him for sharing relevant bits of his doctoral thesis, and for helpful discussions.

The second aspect we wish to emphasise is Brown's observation – correct in our view – that Einstein's comments at Solvay already contain the germ of EPR's arguments. This can be understood in several ways. To start, it is clear that Einstein already in 1927 is worrying about the tension between quantum mechanics and relativistic locality: how do spacelike separated detectors know when the other detector has fired already? One obvious difference with EPR is that the 1927 example does not rely on entanglement.[4]

But one can also argue for a closer parallel between 1927 and 1935, as Norsen (2015) has done, pointing out that Einstein's informal presentation of the EPR argument to Schrödinger at the start of his letter on 19 June 1935 (discussed here in Section 1.5.2) does not turn on entanglement, but rather employs a setup with just one 'particle' (a ball) and two 'locations' (boxes) in which it may be found. This version of the incompleteness argument *does* more closely parallel Einstein's 1927 considerations, wherein components of the wavefunction travel in different directions.

Even though not made explicit in his thought experiment during the 1927 Solvay conference, Einstein was at this time clearly worried about the nature of composite systems in quantum theory. This is evinced by his remark that an ensemble reading of the wavefunction may not comport with conservation laws at the individual level (see Howard 1990, pp. 94–97). So although there are noteworthy parallels between Einstein's commentary in 1927 and his incompleteness arguments in 1935, a more robust historical lineage for the EPR thought experiment is arguably to be traced through several transformations of another of Einstein's thought experiments: the photon box.

1.4 The Photon-Box Experiment

Einstein's famous photon-box thought experiment is usually dated to his debate with Bohr in the autumn of 1930 during the sixth Solvay conference in Brussels. However, in a letter to Laue in 1936 discussing their recently discredited collaborator Rupp, Einstein reveals that already in 1926 he had devised a thought experiment that is clearly a precursor to the 1930 photon box. The context in which this earlier thought experiment was devised is of course importantly distinct from its particular use as a criticism of quantum mechanics in 1930 and afterwards, but we nevertheless believe this report by Einstein underscores a continuous trend in his thinking about quantum phenomena. We begin this section therefore with a discussion of the 1926 thought experiment, and continue with a brief overview of the better-known history of the photon box.

[4] And, indeed, the explanation of this thought experiment provided by de Broglie's theory does *not* require action at a distance, whereas EPR's example does.

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1.4.1 The Photon Box in 1926

In the spring of 1926, Einstein devised several experiments later (allegedly) carried out by Rupp as a means to probe the particle- or wavelike aspects of light (Van Dongen 2007a, b). Although in the interim period experiments by Compton, Bothe and Geiger, and solo work by Bothe, had overwhelmingly confirmed the quantised nature of light, Einstein was nevertheless motivated to seek further empirical evidence. Van Dongen writes: 'He did not have a straightforward answer to tough questions on how the wave train that was clearly exhibited in interference phenomena could be brought in line with an instantaneous emission process' (Van Dongen 2007b, p. 82; cf. Howard 1990, pp. 76ff.). As Einstein would colourfully write to Ehrenfest on 28 August 1926 – some time after Rupp had reported to Einstein that his experiments had yielded the expected wavelike results (as translated in Einstein 2018b, p. 359):

> I already wrote to you about this, that the canal ray experiments came out entirely in line with the wave theory. Here waves, here quanta! The reality of both is rock solid. Only the devil can make any *real* rhyme or reason out of it.[5]

Thanks to the careful work of Van Dongen we now know the details of the story of the Einstein–Rupp experiments, despite their having been largely forgotten after it transpired that Rupp's results for these tests (and many others besides) were fabricated. Van Dongen describes how although Rupp's status in the world of experimental physics became increasingly suspect into the 1930s, Laue and Einstein – both having published papers with Rupp – were not eager to accept the latter's discreditation. It seems Einstein and Laue only really dropped Rupp in 1935, after a damning paper by Gerlach and Rüchardt on the impossibility of Rupp's results appeared in the prominent journal *Annalen der Physik*, and after the German Physical Society had officially expelled Rupp from its membership for fabricating experimental results (Van Dongen 2007a, pp. 114–119).[6]

Thus it is a full decade after Einstein first suggesting wave-particle tests to Rupp and in the wake of Rupp's downfall that Laue initiates a brief exchange of letters with Einstein attempting to get clear on the physics at stake. The 1936 correspondence between Laue and Einstein comprises four letters: Laue to Einstein on 8 June and 29 July; a presumed but (alas) missing letter from Einstein to Laue between those dates, and a response to Laue on 29 August.[7] It is this last letter we find particularly interesting. Einstein writes:

[5] The last sentence displays an idiosyncratic mix of idioms rather characteristic of Einstein: 'Aber der Teufel macht einen Vers darauf (der sich *wirklich* reimt)'.
[6] See Van Dongen (2007a, b) for more on this fascinating episode.
[7] These letters are found, respectively, in EA 16-110 (typed, 2pp.), EA 16-111 (typed, 2pp.) and EA 16-113 (handwritten, 3pp., typed transcription EA 16-112, 3pp.), all letters in German.

No consideration at the time could have been a self-contained logical deduction [Keine damalige Überlegung konnte keine [*sic*] einheitliche logische Deduktion sein], because at the time we did not possess a theory that fit the entire complex of the phenomena, and still do not today. Thus it can only be a matter of using premises one believes are certain to obtain, yet without being able to provide a unified theoretical basis for them.

If we are farther along today than we were then, then in my opinion it is due to the following realisation: one largely does justice to the facts if one proceeds like this. One calculates consistently with waves and considers these as probability waves for the spatial occurrence of particles, or quanta. One must however add that in doing so one has to treat a plurality of particles as *one* particle in a multidimensional space. Neither of us, of course, believes that this method amounts to the final truth. But we are confident that, in applying this sort of consideration within our domain of interest, one does not arrive at gross inconsistencies. But at the time I wrote the short note, this insight was not yet achieved. At that time the question was: the quantum notion and the undulatory notion clash, but both contain something correct; how much of either is correct?

The simple example from which I was then proceeding (not in the publication) is as follows: in a mirror-lined box there is monochromatic radiation, and in fact a single quantum. A little door opens for a short time τ ($\tau = n\frac{\lambda}{c}$; λ wavelength, n finite). If the quantum escapes, is its frequency exactly that of the quantum initially located in the box, or does it deviate statistically from it? The naive quantum view leads to an expectation corresponding to the first case, the undulatory view instead to one corresponding to the second case. Today we are completely persuaded that the consequence drawn from the undulatory view corresponds to reality. Back then, however, I saw no direct possibility of deciding, and thus sought a sufficiently analogous case in which our knowledge enabled a decision even without conducting an experiment. So I was led to the case [kam ich auf den Fall] of a radiating particle which flies past *directly behind a slit* and radiates.

The decisive ‘sufficiently analogous case’ Einstein then details for Laue is close to the version Rupp adapted for the canal ray experiments he supposedly undertook in April and May of 1926. That is: the Einstein–Rupp experiment is a *transposition of the original photon box*.[8] After explaining the setup and detailing his argument for statistical spread in the measured frequency of the photon, Einstein asserts: ‘Therefore I do not think that my erstwhile considerations are superfluous or false, in fact I even believe that they may claim some modest interest. For in my opinion, we still do not possess a serious theory even today.’

This letter is briefly referenced by Fine in support of his claim that ‘right up to the end, Einstein believed that the conception of the ψ-function as corresponding to an ensemble of systems “was not logically refuted”’ (Fine 1986, p. 42, note 3). The Einstein–Laue letters in 1936 are also invoked in order to corroborate Fine’s thesis that at the time of EPR, Einstein was more worried about how to interpret

[8] Note, however, that Einstein had proposed Rupp-like experiments even as early as December 1921 (Einstein 1921), as discussed by Klein (1970, section 3) and by Tauschinsky and van Dongen (2008). Thanks to Jeroen van Dongen for alerting us to this fact.

the wavefunction than, say, the metaphysical status of the uncertainty relations. We certainly agree with this analysis, but wish to draw special attention to the thought experiment Einstein gives in his letter to Laue, reportedly from 1926.

In particular, examination of this thought experiment leads us to suppose that *already in 1926* Einstein had convinced himself that if the time window for the emission of the photon is short, then the frequency of the emitted photon will exhibit a statistical spread. Thus it seems we have direct evidence that Einstein was prepared to accept the time–energy uncertainty relation soon to be proposed by Heisenberg, at least in one direction: a precise time measurement implies a spread in energy. Indeed, we learn from Van Dongen (2015) that the Einstein–Rupp experiments were keenly seized upon by Bohr and Heisenberg, and contributed specifically to the development of the uncertainty relations. In the Chicago lectures (Heisenberg 1930b, pp. 79–80) Heisenberg cites the Einstein–Rupp tests as one of a handful of experiments seminal for the establishment of the new quantum theory.[9]

Einstein's phrasing in this letter (e.g. when he asks 'is its frequency exactly that of the quantum initially located in the box, or does it deviate statistically from it?') further suggests that he understood the uncertainty in frequency to be 'statistical' in the strict sense: the photon *does* possess a well-defined frequency (unlike a segment of a classical wave train!), but in an ensemble one will find a spread of frequencies rather than always the frequency of the original photon in the box. Of course, Einstein admits that this is far from being a straightforward picture ('[b]ut the devil knows how one should *really* understand this')!

Finally, Einstein's concluding remarks to Laue in this exchange confirm that in 19*36* he still believed his 19*26* argument for the statistical spread in the photon frequency to be correct (and perhaps better than arguments based on the uncertainty principle). So we have here not just the first instance of the photon box, but a likely smoking gun for the claim that the better-known 1930 version of this experiment was not intended by Einstein to demonstrate the *incorrectness* of quantum mechanics.

1.4.2 The Photon Box in 1930 and Beyond

The version of the thought experiment proposed by Einstein at the October 1930 Solvay conference involves a photon being released from a weighable box containing a clockwork mechanism for the release of the photon. The difference in weight of the box before and after the emission of light enables calculation of the photon's

[9] In addition to Bohr's and Heisenberg's initial interpretation of the Einstein–Rupp experiments, when re-analysed in the 1930s for the purpose of exposing Rupp as a fraud, these experiments were again interpreted explicitly as testing the validity of the uncertainty relations (Van Dongen 2015).

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energy, and the clockwork fixes the time of release of the photon. Thus we appear to obtain precise knowledge both of the photon's energy and time of emission. In this version of the thought experiment no manipulation of the system of interest (the photon) is made, only inferences to its properties based on measurements of an ancillary system (the box).

One may be tempted to assume that Einstein's aim in this instance was to refute the energy–time uncertainty relation. This temptation is seemingly vindicated by Bohr's own recollection of the 1930 thought experiment – both in his contribution to the Schilpp volume (1949, pp. 225–226) and later when interviewed by Kuhn.[10] But if as argued here and elsewhere (with especial perspicuity in Howard (2007)) Einstein was not trying to cast doubt on the uncertainty relations, what *was* the intended thrust of this thought experiment? One might conjecture as follows: although Einstein had convinced himself that a short, precisely determinable time for the shutter's opening implied a spread in the photon's energy, he required further convincing that a precise measurement of energy of the *microscopic* system would likewise imply an uncertainty in the duration of the opening of the *macroscopic* shutter. The well-known analysis of the photon box in which weighing it changes its position in the gravitational field – thereby restoring uncertainty to the predictions – would indeed speak to this point (Bohr 1949, pp. 224–228[11]).

According to the more recent understanding of the 1930 photon box (which is not incompatible with the above), Einstein was arguing that quantum mechanics yielded a merely statistical description precisely *because* the photon must already know the answers to the predictions one can make by manipulating the box alone. In other words, while the uncertainty relations are correct, and while presumably Einstein had been inclined all along to read them epistemically, with the photon box Einstein is working towards an *argument* to the effect that the uncertainty relations are to be read as such – as limitations on what we can know or predict – rather than ontically as limitations on what is really there.

Bohr himself gives a near-contemporary description of the thought experiment – as well as a resolution of the 'paradox' posed by it – in a talk delivered in Bristol on 5 October 1931.[12] Specifically, Bohr says:

> These new principles, introduced by quantum mechanics, are of similar character as those of Einstein's theory of relativity and the question arises whether they are independent from each other, or one superseding the other, or contradictory to each other, or reconcilable. To my mind there is not the least difficulty, as the fundamental relations are relativistic[ally]

[10] AHQP 1419-001, section Bohr-006. Also available online at www.aip.org/history-programs/niels-bohr-library/oral-histories/4517-5.

[11] Bohr states (p. 226) that Einstein actively contributed to this analysis.

[12] AHQP-BMSS 12, section 5 (typescript, 14pp., in English). Transcription in Bohr (1985, pp. 367–369).

invariant and can be adapted to each Lorentz-frame of coordinates, but I shall enlarge on this subject in the published account of this lecture.[13]

I shall now proceed to discuss a problem designed by Einstein which seemed to show that the foregoing considerations are only valid as long as the view point of theory of general relativity is left out of the discussion, i.e. as long as gravitation is left out. Einstein proposed the following arrangement with which he thought it possible to determine the energy change of an object accurately and also the time at which this change took place accurately:

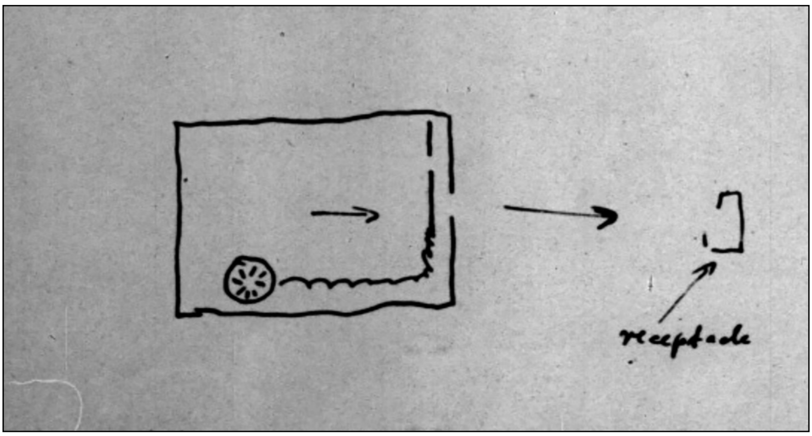

Fig. 1.1 Bohr's first figure: Einstein's photon box, with internal clockwork mechanism and external 'receptacle'.

The box contains a clock which, by some arrangement, can move a shutter, so as to open the box for a very short time ΔT sufficient to let pass out of the box just one light-quantum. We know, therefore, the time at which the light-quantum escaped with accuracy ΔT which can be made indefinitely small. In addition to this we will weigh the box in a gravitational field before and after the process to which there is also no limit of accuracy, so that time and energy of the emission of the light-quantum would both be known accurately. The solution of this paradox can be found by taking into account the principles of general relativity to a fuller extent, and by going into greater detail about the process of weighing. We will do the weighing by hanging up the box on a spring and reading the extension of the spring by means of a pointer attached to the box and running over a vertical scale.

Now the reading of the pointer cannot be done with infinite accuracy because this would involve infinite inaccuracy in our knowledge of the momentum of the whole box which would mean that the whole process of weighing was illusory. We must therefore be content with a finite accuracy in the determination of the mass. Let us say, for instance, that we do the experiment of reading the pointer by some arrangement which takes the time T. Then the uncertainty in mass ΔM will be connected with the gravitational force $\partial\varphi/\partial x$ and the uncertainty in the momentum by the equation:

[13] [Alas, Bohr did not publish the account of this lecture (*eds.*).]

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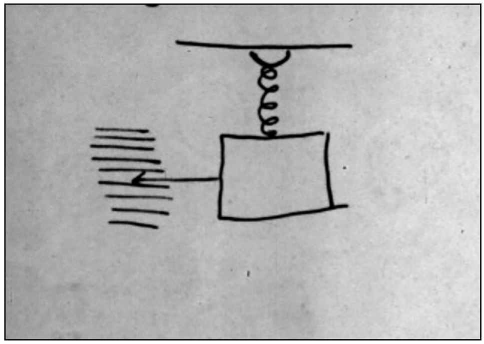

Fig. 1.2 Bohr's second figure: the weighable photon box with vertical scale.

$$\Delta M \cdot \frac{\partial \varphi}{\partial x} T = \Delta P_x = \frac{h}{\Delta x}$$

or

$$\Delta M \Delta \varphi T = h \tag{1}$$

Such an arrangement would therefore involve also an uncertainty about the gravitational potential at which the whole box and the clock inside of it was placed. But this fact, according to a fundamental principle of general relativity would implicate uncertainty in the time scale indicated by the clock:

$$\Delta T = \Delta \varphi \cdot T / c^2$$

or, according to (1)

$$\Delta T = \frac{h}{c^2 \Delta M} \tag{2}$$

Finally, we must remember that, also according to the relativity theory, mass and energy are in every case connected by the relation

$$Mc^2 = E$$

and this brings (2) back to the same form of uncertainty relation which we found valid in all those cases in which gravitation was not involved.[14]

The foregoing analysis of the paradox brought forward by Einstein shows that the phenomenon of gravitation which, up till now, in its mathematical treatment stands so far apart from the rest of modern physics does so by no means with respect to the primary physical concepts evolved by Einstein in his theory of general relativity in order to remove the obstacles which until then seemed to prevent the reconciliation of these two groups of physical phenomena.

This lecture together with his better-known recollections from 1949 present a consistent picture of what Bohr took away from discussions with Einstein at the

[14] [In the manuscript Bohr writes these last two equations $\Delta T = \frac{c^2 h}{\Delta M}$ and $M = c^2 E$ (*eds.*).]

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1930 Solvay conference. What Einstein himself took away can be reconstructed from a number of sources from the early 1930s: a letter by Ehrenfest to Bohr in July of 1931, reports of talks given by Einstein in Berlin and Leiden in November of 1931, and some notes by Schrödinger from around the same time. Subsequent correspondence between Ehrenfest and Einstein in 1932 (and a related anecdote from Rosenfeld, to be mentioned in the next section) indicate some further developments.[15] Finally, while visiting Caltech in the winter of 1930, Einstein co-authored a paper with Tolman and Podolsky (he of EPR fame) on a version of the photon box. Despite its chronological precedence, we leave Einstein's Caltech collaboration (hereafter 'ETP') for the next subsection. This is because the ETP thought experiment (as confirmed in the report of Einstein's Berlin seminar) appears to be an extension of the photon-box experiment presented in the other sources. Indeed, we shall use the ETP experiment as a means of introducing some further aspects of both the EPR paper and the debates it instigated.

Ehrenfest's letter to Bohr on 9 July 1931[16] is prime evidence for Einstein's true intent with the photon box. Ehrenfest writes to Bohr in characteristic style (via Mrs Bohr and by way of Casimir as courier):

> I think now I understand again very well why he [Einstein], despite perfect familiarity with the uncertainty relation and the corresponding limited applicability of the classical concepts, still refuses to be put off the search for *micro*-field theories that can eventually prove fruitful for the observable phenomena. If one could just arrange it so that you two marvellous people do not speak PAST EACH OTHER.
>
> Of course I have also asked Einstein why he continually thinks up new "perpetua mobilia" = machines against the uncertainty relation. He told me that already since very long ago he absolutely no longer doubts the uncertainty relation and thus e.g. that he has not thought up the "weighable light-flash box" [. . .] AT ALL "contra the uncertainty relation", but for an entirely different purpose.
>
> [. . .]
>
> We want to think up a "machine" that ejects a "projectile". And AFTER the projectile has left the machine once and for all a "questioner" can, at his discretion, ask the machine operator to *predict* by means of a *subsequent examination of the machine*
>
> *either* which numerical value a for a quantity A
>
> *OR* which numerical value b for a quantity B
>
> the questioner will later find on the projectile (reflected and coming back after a frightfully long time) by performing either an A-measurement or a B-measurement.
>
> Here A and B shall be non-commuting quantities, so e.g. conjugate ones.

15 Most of these sources (barring the Schrödinger notes, to which we shall return more extensively in Chapter 3 and which we translate as Chapter 8) are discussed in Jammer (1974), Fine (1986), Howard (1990) and Held (1998).

16 AHQP-BSC 18, section 4 and BSC-EHR-310709t (typewritten, 2pp., in German).

> Thus for Einstein it is out of the question and beyond doubt that because of the uncertainty relation one must of course choose between either and or. But the questioner can choose between them AFTER the projectile is once and for all on its way.
>
> Can one think up such a machine? – And why is Einstein interested in it?
>
> *I can sympathise very well with Einstein about the latter*! But nevertheless I am unable to formulate it in a way as to be sure that he would be happy with my formulation. I would say: it is interesting to be clear about the fact that the projectile, which is already flying about in isolation "on its own", must be ready to satisfy very different ""non-commutative"" [*sic*] prophecies, "without even knowing" which one of these prophecies one will make (and check).
>
> [...]
>
> Now, his weighable light-flash box is meant to be such a machine.

Notice that Ehrenfest repeats *three times* (even in capitals and underlined) that the choice of which prediction to make is delayed until after the projectile has been emitted. Ehrenfest continues from here to describe a highly detailed version of the thought experiment: at 0 hours one sets up the clock to briefly open the shutter at 1,000 hours, and also weighs the box for 500 hours, after which one bolts it to the external frame. When the shutter opens, the light travels to a fixed mirror half a light-year away. One waits until 1,500 hours (in the external frame) and *then* makes the choice to predict either the time of return of the light front, or the colour of the returning light. In the first case, one leaves the photon box bolted to the external frame, opens it, and inspects the clock inside. One thereby determines what the gravitational lag of the clock has been and can correct for it, so as to know at what time in the external frame of reference the shutter had opened. In the second case instead, one unbolts the photon box from the external frame and re-weighs it.

Presumably, in the first case, opening the box and reading the time results in an uncontrollable exchange of energy with the photon box. And clearly in the second case, re-weighing the box allows one to determine the energy of the escaped photon, but disturbs once again the rate of the clock in the box. While at the end of the re-weighing (at 2,000 hours) it will also be possible to open the box and check the reading of the clock (and thus determine the *total* effect of gravitation on the clock) it will be impossible to reconstruct what external time corresponded to the intermediate 1,000-hours reading of the internal clock. Thus Ehrenfest not only clearly states that for Einstein these predictions are mutually exclusive, but also describes in considerable detail *how* they exclude one another.

Finally, notice in this version of the thought experiment that if one determines precisely the energy of the photon, it is the exact time *when the shutter first opens* that is uncertain – and thus the exact time of the arrival of the *wavefront* (one year later, since the distant mirror is half a light-year away). But now Ehrenfest highlights another paradoxical element: if the time window during which the shutter is open can be made quite small, only an extremely short wave train can escape, which is then 'very unmonochromatic' – that is, does not have a precise energy:

I am worried about the following circumstance: even *if* one allowed the shutter to be open only FOR A VERY SHORT TIME, nevertheless one could determine through the double weighing the loss of weight very precisely, and thus also the energy of the emitted quantum; on the other hand it is clear that a very short light cork[17] is very unmonochromatic. I am deeply ashamed to be so stupid, but I cannot see what to think about this "contradiction". Of course it is only something stupid. Maybe Casimir, by whose hands I am letting this letter get to you, will sort out this question already.

This puzzling aspect was present already in the 1926 thought experiment: the opening of the shutter was presumed short, but Einstein concluded that the energy of the photon would yield a typically wavelike spread (which he interpreted statistically, however), and other versions of the photon-box experiment appear to lay emphasis on the duration of the opening.[18]

In any case, given the way Bohr described the thought experiment less than three months later, one might surmise that Ehrenfest's letter did not have the desired effect of clarifying Einstein's views to Bohr.[19]

Another source purporting to describe Einstein's version of the thought experiment is Schrödinger, in two pages of notes translated here as Chapter 8, and discussed more fully in Chapter 3. The description by Schrödinger closes with a verbatim attribution to Einstein (before Schrödinger goes on to express thoughts of his own):

I have a box with automatic shutter, which tomorrow at midday shall briefly open a lid. I use the meantime to weigh the box very exactly.

In the box there is very diluted monochromatic γ-radiation, the time during which the box is open is determined in *such* a way that it is probable that one quantum comes out. After the light quantum is gone, I can still choose whether I wish to transmit to observer B exactly the time of departure, so that he knows exactly the time of arrival, or whether I wish to weigh my box exactly and let him know the colour.

Both together does not work, because through a *short* light signal to B, I change the weight uncontrollably. But already the possibility of an arbitrarily accurate communication of the colour is paradoxical, because the wave train *is* in fact short. I am forced to assume that for observer B the *temporal duration* of the lid opening is indeterminate.

So Einstein said to me.

[17] [Ehrenfest refers to the emitted light quite consistently as a 'Lichtpropf' – a 'light stopper' – so to speak a cork popping out of Einstein's bottle (*eds*.).]

[18] See below for Casimir's own recollections of the photon box. He does not comment on this particular question, however.

[19] Indeed, one wonders if he even read it! For Ehrenfest recommended to Mrs Bohr: 'Do not pass on this letter at all to Niels if he is too tired', before sending his regards to 'all of you Bohrs and Bohrikins together'.

These notes are, admittedly, difficult to date: Einstein and Schrödinger had been colleagues in Berlin since October 1927, and Schrödinger's phrasing seems to refer elliptically to a conversation with Einstein. However, the version of the experiment described here by Schrödinger is slightly different than the one described by Ehrenfest: the emphasis is on the trade-off between the emitted energy and the *duration* of the opening of the shutter, rather than the time of the photon's emission. Exclusivity of measurements and delayed choice are again the salient features.

This is closer to a version of the experiment given by Einstein in a Berlin seminar in November 1931, a report of which was published in *Angewandte Chemie* early in 1932 (Einstein 1932; translated here as Chapter 7). The only significant difference from previous versions is that the report does not mention emission of a photon, but rather of a 'monochromatic light ray [. . .] of about 100 wave trains' (see p. 152). Details of the manipulations are not described in the brief report, but both the exclusivity of the two possible measurements and the option of delayed choice are again stressed: 'Only *one* measurement – of the time *or* the colour – can be carried out precisely, and in fact according to EINSTEIN one can still decide after the departure of the light ray which of the two predictions one wants to choose.' The report concludes by referring the reader to Tolman's extension of this argument in the ETP paper.

There are other fascinating materials from Schrödinger connected directly to the Berlin seminar: a set of notes on the discussion with further elaborations by Schrödinger (which will be discussed in forthcoming work by Jos Uffink and Christoph Lehner), and a letter from Schrödinger to Sommerfeld dated 11 December 1931,[20] containing an analogue of Einstein's photon box formulated in terms of the *position and momentum* of a photon and mirror, and where Schrödinger explicitly mentions – but then expresses reservations about – the idea that the unmanipulated photon possesses both a definite position and a definite momentum:

> [O]ur mirror is a 'universal instrument' for position *and* momentum of the light quantum in the following sense: if one 'applies it' to the light quantum in the above-described manner, then position *and* velocity of the light quantum leave a trace on the instrument, and in fact they are registered with a precision whose product can be pushed well below the limit h. Both traces really exist, because it depends on my free volition to make use of the one or of the other (whereby, however, the second gets smeared out). Since the light quantum no longer has anything to do with my manipulations at the mirror, one cannot well say that it 'receives' a sharp position or sharp momentum only through one of these operations, at any rate it is no 'receiving' in the real [sense], but at most in the mental sense. One might like to conclude therefore that the light quantum *possesses* at all times a fully definite position

[20] AHQP 84, section S-010 (handwritten, 5pp., in German), transcription in Meyenn (2011, vol. 1, pp. 488–492). Our thanks to Chris Fuchs for bringing this letter to our attention.

and a fully definite momentum – a notion that we actually abandoned long ago as too harsh and paradoxical.

In Chapter 3 we investigate further Schrödinger's notes and his letter to Sommerfeld, and analyse what reservations (if any) Schrödinger might have had in fully embracing the conclusion of the incompleteness argument alluded to here.

There is yet one more piece of evidence on the photon box in 1931, coming from a talk Einstein gave in Leiden at about the same time as the Berlin seminar. In his 1982 biography of Einstein, Pais relates the following anecdote (Pais 1982, p. 449):

My friend Casimir has written to me about a colloquium Einstein gave in Leiden, with Ehrenfest in the chair (this must have been in November 1930). In his talk, Einstein discussed several aspects of the clock-in-the-box experiments. In the subsequent discussion, it was mentioned that no conflict with quantum mechanics existed. Einstein reacted to this statement as follows: [. . .] I know, this business is free of contradictions, yet in my view it contains a certain unreasonableness.

There is a small historical discrepancy here: Einstein could not have been in Leiden either immediately preceding or following the 1930 Solvay conference that October. Prior to the Solvay meeting he spent time in southern Germany, and his official documents permitting entrance to Belgium for the Solvay conference are directly from Germany and do not mention the Netherlands. Einstein left the 1930 Solvay conference for the United Kingdom promptly at its conclusion, as he had an appointment on the evening of 30 October in London. Shortly thereafter Einstein set sail for the United States.

The date of the Leiden lecture is corrected in Casimir's recently reissued autobiography (Casimir 2010). There, an entire appendix is dedicated to 'The Discussions Between Bohr and Einstein on the Interpretation of Quantum Mechanics'. In it, Casimir distinguishes three phases of Einstein's complaints against quantum mechanics: the 'First Phase' corresponds to the 1927 Solvay conference and the 'Second Phase' to the conference of 1930. It is the 'Third Phase' that, by Casimir's lights, captures Einstein's stance from Solvay 1930 onwards to the publication of EPR and beyond (Casimir 2010, p. 316):

Einstein conceded that the theory was free of contradictions, that it was consistent. He also admitted that it was extremely powerful and fertile. But he could not regard it as a complete description of nature and therefore he could not regard it as final. And here I can draw on my own personal experience. The scene is a colloquium at Leiden, almost certainly during the winter of 1931–32 when I was assistant to Ehrenfest. Ehrenfest, of course, was in the chair, but more silent than usual, and Einstein was giving a lecture. What exactly did he discuss? I think – but I have to admit that I am not completely certain – that he started with a box, containing a clockwork and a shutter and placed on a sensitive balance, as in his 1930 paradox. But this time he no longer tried to beat Heisenberg's principle. He stressed that after the light quantum had escaped we can still choose whether we want to read the clock or alternatively determine the weight of the box. So, without touching the light quantum in any way, we can either determine its energy or the time at which it will return from a distant mirror. As I have said, I am not completely certain this was the experiment he discussed. I am certain that it was a case where we can still choose whether we want to know one quantity or the other (energy or time, place or momentum), although the particle itself is already out of reach; and that was also the gist of the later paper by Einstein, Rosen, and Podolsky. Ehrenfest had entrusted me with the task of opening the discussion and I explained to the best of my ability the Copenhagen views on such questions. Einstein listened, perhaps slightly impatiently, and then he said – and I believe now I can vouch for the exact words: 'Ich weiss es, widerspruchsfrei ist die Sache schon, aber sie enthält meines Erachtens doch eine gewisse Härte'. Pais, who also mentions this episode, translates this as: 'I know, this business is free of contradictions, yet in my view it contains a certain unreasonableness'. I think 'a certain unpalatability' comes closer to what Einstein had in mind.

We know that Einstein visited the University of Leiden some time during the last week of November in 1931 (leaving Berlin on 23 November and arriving in Antwerp in order to sail on 1 December). Thus, it was mostly likely in this time period that Einstein gave the talk remembered by Casimir.[21] The latter's report can thus be considered a good barometer of Einstein's thinking at the time, and it corroborates other sources mentioned here, especially regarding emphasis on free choice of measuring one or another quantity after interaction has (presumably) ceased.

In the spring following Einstein's Berlin colloquium, we hear from the man himself: in a missive dated 5 April 1932 from Rotterdam Harbour, Einstein confesses to Ehrenfest that the latter has provoked him to think more about the thought experiment. Relevant bits from this letter read as follows (EA 10-231):

Dear Ehrenfest!

You piqued me yesterday about altering the 'box experiment' so that it uses concepts that are less foreign to the wave theorist. I do this in what follows, in which I only use idealisations which I know you find harmless.

It employs a schematised Compton effect.

Let the mass m follow a straight line. A radiation pulse is sent from Q through the lens with 'precisely' known time of emission. It is collimated by the lens and reaches the straight line at right angles to it. Let the light pulse be diffracted by m, and let it likewise travel along the straight line (this is unclean but unimportant). Let the segment AB be long enough that we may assume that constraining the initial position of m on AB is compatible enough with the assumption that the mass is initially at rest.

One inquires about location, resp. momentum, of the mass m after the Compton effect has occurred, whereby experiments may be carried out with the diffracted light quantum.

[21] Thanks to Barbara Wolff (formerly at the Albert Einstein Archives) for investigating this point with one of us (EC).

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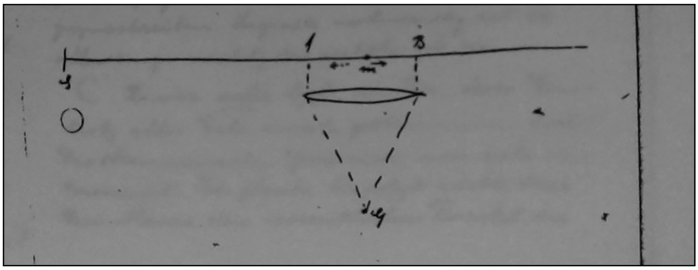

Fig. 1.3 Einstein's sketch of a wave-theoretic thought experiment.

Let the mirror S be somewhere far away, which sends the light quantum back toward the right. Let the experimenter sit somewhere near A. While the quantum is en route, he can still freely choose whether he wants to prophesy what the momentum [may be] or whether he wants to prophesy [what] the location of m may be (after the completed Compton process), and in fact without doing anything with the mass m.

Indeed, according to the law of momentum conservation, the momentum I of m is given by

$$I = \frac{h\nu}{c}$$

where ν is the frequency of the diffracted and reflected quantum. Thus if I measure ν I learn I.

But I can also determine the time of arrival of the quantum. Then I know exactly the time elapsed since the Compton action, thus also the location of m immediately after the Compton action. The greater the mass m is, the more exactly this location coincides with that which can be determined at A as time of arrival after the return of the quantum.

Thus it is possible to prophesy at will *either* the momentum *or* the location of m with arbitrary precision without an experiment on m.

This is the reason why I feel compelled to ascribe objective reality to *both*. It is not logically necessary, however, that I admit.

As has been remarked in the literature, this letter contains the first evidence of Einstein himself couching the thought experiment in terms of position and momentum, thus a setup (in line with Ehrenfest's urging) amenable to a description in terms of wavefunctions – even though Einstein does not provide then such an explication.[22]

[22] Jammer (1974, pp. 173–174) describes what appears to be an intermediate stage of development of the example taken from Einstein's recollections in his 1945 correspondence with Epstein: measuring the position of the box at the time of release of the photon allows one to predict the time of arrival (and hence the position) of the photon, while measuring the momentum recoil on the box allows one to predict the energy or momentum of the photon.

In his 1990 paper, Howard provides a reading of the photon-box experiment *qua* incompleteness argument, undergirding this reading with evidence from Ehrenfest's above-quoted letter to Bohr. Howard also remarks upon the striking similarity between Einstein's critique of quantum theory in 1931 and in 1935 (Howard 1990, p. 99):

> In fact, the logic of the 1931 version of the photon-box *Gedankenexperiment* is almost exactly that of Einstein's own 1935 incompleteness argument. The whole point of placing the reflector $\frac{1}{2}$ light-year away is to assure a spacelike separation between the inspection of the photon-box and the projectile. The argument works as a criticism of the quantum theory only if one assumes that the projectile, when thus separated from the box, is, in virtue of that separation and its therefore possessing its own independent reality, wholly unaffected by what we do to the box when we inspect it. But quantum mechanics makes a different assumption. [...] [Q]uantum mechanics says that the state we ascribe to the photon depends crucially on what we do to the box. What Einstein is thus arguing is that classical assumptions about the separability of previously interacting systems lead to different results than the quantum mechanical account of interactions.

We agree, with one small qualification. While the separation between the box and the photon is spacelike at the time we *perform* our freely chosen manipulation of the box, in Einstein's presentations of the experiment, by the time we later *verify* our prophecy, the photon has been invariably reflected by a (fixed) mirror. Thus the separation between the prediction and its verification is always timelike. This may seem a minor point, but we shall argue in Chapter 5 that it is of some significance in the context of Bohr's reply to EPR.

1.4.3 The ETP Paper

Einstein visited Caltech in the winter of 1930–31. The fruits of his labour there include a paper co-authored with Tolman and Podolsky, published in February 1931 (Einstein, Tolman and Podolsky 1931, reprinted herein as Chapter 6). Not quite two pages in length, ETP nevertheless manage to describe and assess a thought experiment closely resembling the photon box – yet with notable differences.

What most readily springs to the eye from a technical vantage point is that the ETP paper considers *two* particles being emitted in different directions from a two-holed box (and also that the particles are not necessarily photons, but merely 'particles in thermal agitation'). One might acknowledge here a superficial similarity with the EPR arrangement, but we wish to point out that there is even closer similarity with *Bohr's* version of the EPR example (see Chapter 5).

Indeed, in his reply Bohr considers two particles, each passing through a different slit in a screen. The initial total momentum of the particles and the screen is known, as is the distance between the slits. By measuring the momentum of the

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screen immediately after the passage of the particles, we know the sum of the particles' momenta and we know the difference in their positions. The ETP thought experiment, then, is just the energy–time analogue of Bohr's version of the EPR example. In the ETP experiment, the initial total energy of the particles plus the box is known (via a first weighing), and later the particles are released simultaneously. After the particles are emitted, we weigh the box a second time and so learn the sum of the particle energies; we also know the time difference of their escape (zero). The rest of the story is analogous to the single-photon box: measurements on one particle can be used to make predictions about the other particle (which travels very far in order to allow sufficient time to accurately re-weigh the box); these predictions can be verified when the second particle at last returns from being 'elastically reflected'.

Yet the thought experiment described by ETP is more than just another Einsteinian photon box (apart from details in setup). In the case of the latter, the original version is often taken as suggesting that simultaneous measurements on one system (the box) would enable simultaneous predictions of the other (the photon). But the uncertainty relations forbid this consequence, and careful analysis reveals that measurements on the macroscopic box must also be subject to the constraints of uncertainty. Hence the photon box illustrated the correctness of quantum mechanics at the macroscopic as well as microscopic scale, while at the same time suggesting it is incomplete by virtue of the possibility of a delayed, arbitrarily chosen measurement.

The ETP paper instead initially suggests a *different* loophole for obtaining simultaneous predictions on the distant particle. Then – as with Einstein's 1931 photon boxes – from grounds of uncertainty, ETP argue that no attempt to exploit this loophole can succeed. Thus in ETP quantum mechanics is *explicitly assumed* to be correct, whatever else may follow.

What is this new apparent loophole? Before delving into ETP's argument, it will be useful to distinguish between two aspects of a measurement: (1) determining the value of a quantity at a time immediately *before* the measurement; and (2) preparing a state at a time immediately *after* the measurement. The standard reading of the uncertainty relations – and the one ETP presuppose – is as a limitation on simultaneous state preparation: there are no measurements such that the prepared state allows one to exactly predict the results of further measurements of either of two conjugate quantities, *despite* the fact that scenarios like the photon box suggest that a system simultaneously possesses values for such quantities. However, what was generally supposed – an assumption ETP aimed to correct – was that retrodictions of values of conjugate quantities were possible, even though physically meaningless because devoid of predictive power. This is the line taken by Heisenberg in the Chicago lectures, for instance (Heisenberg 1930b, p. 20). The classic example

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is of two successive position measurements taken at definite times: the (average) momentum of a particle can be reconstructed for the past, while the second position measurement makes the momentum of the particle uncertain for the purpose of future measurements.

While Heisenberg took it as a 'matter of taste' whether one wanted to attribute physical reality to the past position and momentum of a particle, certainly Einstein would have had no qualms attributing reality to retrodicted values. To Bohr might be attributed yet a third stance: retrodiction is in fact what happens in *all* measurements, but it is noticeably limited by complementarity only in quantum mechanical cases. Indeed, in any measurement a system of interest interacts with a measuring apparatus. We then manipulate the apparatus in such a way as to be able to reconstruct *certain features* of the previous interaction, while other features are uncontrollably lost. This allows us to make certain further predictions on the system, while also explaining why we have cut ourselves off from the possibility of making certain other predictions.[23]

Though the further details of ETP's thought experiment are slightly involved, the reasoning is clear enough. Because the box gets re-weighed with great precision, the time of the particles's escape cannot be read off the clockwork mechanism internal to the box (this realisation came out of Bohr and Einstein's tussle over the photon box in 1930[24]). It must instead be reconstructed after the fact by conducting measurements on the first particle. ETP imagine that while the first particle is in flight, one can measure its momentum without disturbing it (this action also yields a measurement of the particle's energy at the time of escape from the box). Measuring the time of arrival of the first particle then also allows one to reconstruct the time of release of both particles, and therefore (given that the box gets re-weighed), enables the prediction of both the energy and time of arrival of the second particle.

Being able to predict both the energy and time of arrival of the second particle, however, is precluded by the (standard reading of the) uncertainty relations, which ETP assume to be correct. 'It is hence to be concluded', they write, 'that the principles of the quantum mechanics must involve an uncertainty in the description of past events which is analogous to the uncertainty in the prediction of future

[23] This take on Bohr is very close to Howard's (1994) analysis of Bohr's doctrine of classical concepts. A similar take was articulated in considerable detail by Grete Hermann (1935a, b), who emphasises the idea that the context of observation determines which features of the interaction are causally relevant for explaining a given measurement result, so that quantum mechanics is in fact causally complete *within* a measurement context (though not *across* all possible contexts). We shall return to the views of Hermann at some length in Chapter 4, and to the views of Bohr at considerable length in Chapter 5, where we also discuss Pauli's arguably related discussion of measurements in his 1933 handbook article (Pauli 1933, 1990, section A.9).

[24] ETP note that 'it is of special interest to emphasize the remarkable conclusion that the principles of quantum mechanics would actually impose limitations on the localization in time of a macroscopic phenomenon such as the opening and closing of a shutter' (ETP, p. 781; see p. 151). See also the end of Schrödinger's notes (Chapter 8).

events' (ETP, p. 781; see p. 151). It follows that a momentum measurement which does not disturb the momentum of the particle is impossible to carry out, and time-of-flight measurements enabling the reconstruction of the particle's trajectory are impossible. This last point is presumably a veiled criticism of Heisenberg's discussion of retrodiction. A similar (but not in the least veiled) negative judgement on Heisenberg's treatment of time-of-flight measurements is made by Hermann (1935a) in regards to her thesis about the relative context of observation. (Hermann is doing precisely the kind of limited retrodiction that ETP are envisaging and sanctioning here.)

In a way, then, the ETP argument (so to speak) sides with Bohr and Hermann against Heisenberg: it shows that if we were able to reconstruct the past of one particle in a way that violates the constraints of uncertainty, we would be able to make simultaneous predictions about the other particle that also violate uncertainty. This is the new loophole ETP find – and then close off – using their thought experiment. The paper does not *really* side with Bohr (nor with Hermann), in that ETP tacitly assume throughout (e.g. by talking about disturbing the momentum of the particle) that the limitations imposed by uncertainty – and also on the possibility of retrodicting the energy and time of release of the particles – are purely *epistemic* limitations. The paper is even titled 'Knowledge of Past and Future in Quantum Mechanics'. Had they instead argued for an ontic reading of uncertainty based on their thought experiment, they would have anticipated Bohr's reply to EPR!

1.4.4 Thinking Outside the (Photon) Box

In 1933 Rosenfeld gave a lecture in Brussels based on a joint paper with Bohr, and Einstein was in attendance. After the lecture, Einstein described to Rosenfeld a thought experiment involving no boxes, only two interacting particles – that is, an experiment nearly identical to that of EPR. Here is Rosenfeld's recollection of Einstein's words on that occasion (Rosenfeld 1967, pp. 127–128):

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

Jammer (1974) has suggested that Einstein's inspiration for removing the box from his quantum mechanical thought experiments was a 1934 paper by Karl Popper, 'Zur Kritik der Ungenauigkeitsrelationen' ['A Critique of the Uncertainty Relations'] (Popper 1934) which contained, according to Popper's own later assessment, a grievous mistake. Jammer writes, 'It is not impossible that it was precisely this "mistake" which prompted Einstein (who immediately recognized the error) to publish, together with Podolsky and Rosen, the argument against the completeness of quantum mechanics' (Jammer 1974, p. 174). We have just seen that Einstein started thinking in terms of examples that did not involve the photon box prior to 1934, but Popper's paper (which was published together with a critical reply by Weizsäcker) might well have contributed to motivate Einstein to publish his ideas on the subject.

Indeed, Popper's claim *sounds* like Einstein's: that quantum mechanics is correct but incomplete, in the sense that the uncertainty relations are empirically correct but need to be interpreted epistemically. But Popper is doubly mistaken: his argument to establish the incompleteness of quantum mechanics is incorrect (as spelled out by Weizsäcker), and even had the argument been correct, it would have established that the limits imposed by the uncertainty relations *can* be overcome. Einstein may well have been unhappy to let the published record of the question end on such a note.

Popper's thought experiment involves a ray of electrons and a ray of photons (with known momenta) crossing each other, giving rise to Compton scattering for individual electron–photon pairs. Popper then claims that one can measure the momentum a particle has on the journey to a detector together with its time of arrival. From this one can then reconstruct both the time and place of the collision, and the momentum of the other particle; thereby one can predict both the momentum and the time of arrival of that particle. This scenario is uncannily similar to that of the ETP paper, and so Einstein was very well aware that a measurement of the kind Popper proposed was impossible. Indeed, both ETP and Weizsäcker use the same example of a momentum measurement (employing the Doppler effect) to illustrate that such a procedure does not provide the desired information. And ETP clearly recognise that if such a procedure were to yield the desired information, this would constitute an empirical violation of the uncertainty relations; hence they conclude to its general impossibility.

Popper's second mistake was to think that because he was considering *rays* of photons and electrons, the information gathered through the measurement would tell us that there was *a* particle in the other ray with such and such properties, but it would not allow us to tell *which* particle that was, ergo it would not allow us to select a sub-ensemble of particles from the ray that was more informative than a quantum mechanical pure case. But nothing, at least in principle, prevents one from using rays of such low intensity that individual particles can be tracked.

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Even though Popper was trying to establish that quantum mechanics was correct but incomplete, he failed to realise that for his argument he should have applied ETP's *modus tollens* instead.[25]

1.5 The EPR Paradox

We now turn to the EPR paper itself (Einstein, Podolsky and Rosen 1935; see Chapter 9), beginning with a summary of its contents and continuing with an analysis of its logic and an examination of Einstein's own stance – different from the published version.

When Einstein arrived at the Princeton Institute for Advanced Study in late 1933 he was quickly joined by Podolsky (with whom he had already collaborated on the ETP paper) and also by Rosen, who had been at MIT as a graduate student and, among other things, had published on two-body problems in quantum mechanics.[26] Specifically, in December 1931 Rosen had published a paper in the *Physical Review* (Rosen 1931), which gives a calculation using the variational method (as opposed to more common perturbation approach) of dissociation energy, moment of inertia and fundamental vibrational frequency of a two-body system, in particular the hydrogen molecule. In such a bound two-particle system, the distance between the two particles is approximately fixed, so an eigenstate of momentum of the hydrogen molecule could be thought of as an EPR state. Still, such a state does not appear explicitly in Rosen's paper.[27] It further appears that the idea of a collaborative paper came from Rosen (Rosen 1985; see also Pais 1982, p. 494).

1.5.1 Structure and Logic of the EPR Paper

EPR begin by providing distinct definitions for the correctness and completeness of a physical theory: correctness is meant in the empirical sense, while completeness is defined as the condition that '*every element of the physical reality must have a counterpart in the physical theory*'; they then formulate their (in)famous sufficient

[25] Popper gives a more detailed discussion of the thought experiment, embedded within a more extensive discussion of quantum mechanics, in *Logik der Forschung* (1935, chapter 7 and appendices V–VII), which he sent to Einstein along with a reprint of his paper. In appendix VI he gives an explicit argument for why such selective measurements cannot be used to prepare 'overpure' states, but the argument equivocates between the inefficiency of such a procedure and its impossibility. N.B. Grete Hermann (1935a) also criticises Popper's arguments, at the same time as she criticises Heisenberg's agnostic reading of 'time-of-flight' measurements. In the English version of *The Logic of Scientific Discovery* (1959), Popper retracted his original experiment, but reiterated his critique of quantum mechanics, invoking the EPR thought experiment instead. See Frappier (2017) for a detailed discussion of Popper and Hermann on these issues.

[26] Details of how the three authors came together at the Princeton Institute for Advanced Study are given in Jammer (1974, pp. 180–181).

[27] See Uffink (2018) and his expected new work with Lehner for some crucial new discoveries about a different possible origin of the EPR state.

criterion for establishing physical reality (EPR, p. 777; see p. 158, all emphases in the quotations are original):

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.*

This criterion also applies to quantum mechanics: if a particle is in an eigenstate of momentum (thus allowing for the prediction of its momentum with probability 1), the corresponding value of momentum is an element of reality. In quantum mechanics, however, one also interprets the fact that for this same state there is a uniform probability for the conjugate variable of position as implying that the position '*has no physical reality*' (p. 778/159). More generally, if one assumes that the wavefunction provides a complete description of physical reality, then quantities corresponding to non-commuting operators '*cannot have simultaneous reality*' (p. 778/159).

EPR next consider a state of the form

$$\Psi(x_1, x_2) = \sum_{n=1}^{\infty} \psi_n(x_1) u_n(x_2) = \sum_{s=1}^{\infty} \varphi_s(x_1) v_s(x_2) \tag{1}$$

(their equations (7)–(8)), where the $u_n(x_2)$ are eigenfunctions of a quantity A, and the $v_s(x_2)$ of a quantity B. According to the standard '*reduction of the wave packet*', a measurement of A on the second system leaves the first system in a state $\psi_k(x_1)$, a measurement of B in a state $\varphi_r(x_1)$. They write (p. 779/161):

On the other hand, since at the time of measurement the two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system. This is, of course, merely a statement of what is meant by the absence of an interaction between the two systems. Thus, *it is possible to assign two different wave functions* (in our example ψ_k and φ_r) *to the same reality* (the second system after the interaction with the first).

They then introduce the EPR state as a special case, and conclude that 'by measuring either A or B we are in a position to predict with certainty, and without in any way disturbing the second system, either the value of the quantity P (that is p_k) or the value of Q (that is q_r)' (p. 780/162). EPR apply their criterion of reality to this situation, concluding that p_k and q_r are elements of reality. Thus, they claim, from the assumption of completeness it also follows that 'two physical quantities, with non-commuting operators, can have simultaneous reality' (p. 780/162). Since the assumption that the wavefunction is a complete description of the quantum state leads to contradictory statements in this way, it must be false.

EPR concede that one might object along the following line: the criterion of reality ought to be applied to physical quantities '*only when they can be simultaneously* *measured or predicted*' (p. 780/162). But were this so, the reality of P and Q for one system would depend on the measurement process on the other system, which does not disturb the first. And of course this would be unacceptable. Nevertheless, EPR end on an optimistic note, expressing their belief that a theory providing a complete description of physical reality is possible.

Much has been said already on how to read the argument, and how Einstein himself was unsatisfied with the published version. Starting with the latter, as famously pointed out by Fine (1981, 1986) and Howard (1985), in his letter of 19 June 1935 to Schrödinger – which we here translate in full for the first time (see p. 297) – Einstein complained that the article had been redacted by Podolsky and that the argument had been 'buried in erudition'. This is evident regarding the point made in the preceding paragraph: while in the paper we saw that EPR use (7)–(8) to show that '*it is possible to assign two different wave functions* [. . .] *to the same reality*', according to Einstein this general case *already establishes incompleteness* without appeal to the specific EPR state, nor indeed to the controversial criterion of reality. Einstein writes (see p. 299; notation adapted):[28]

What is essential now is only that [ψ_k] and [φ_r] are at all different from each other. I claim that this difference is irreconcilable with the hypothesis that the ψ-description corresponds bijectively to physical reality (to the real state).

Indeed, after the collision the real state of [(I,II)] consists of the real state of [I] and the real state of [II], two states that have nothing to do with each other. *The real state of* [I] *now cannot depend on what measurement I perform on* [II]. ('Separation principle' from above.) Then, however, corresponding to the same state of [I] there are two (in fact arbitrarily many) equally legitimate [wavefunctions for I], which contradicts the hypothesis of a bijective or complete description of the real states.

An argument very similar to this is nicely expressed by Schrödinger in his cat paper (see p. 196; again, notation adapted):

Suppose now that in this way [by carrying out a measurement plan for II] I derived a [I]-catalog in a particular case. Then I can reflect and consider whether I might perhaps have found a *different one* if I had put into action a *different* measuring plan for [II]. But since after all I neither have actually touched the system [I], nor in the imagined other case would have touched it, the statements of the other catalog, whatever it might be, must all *also* be correct. They must therefore be entirely contained within the first, since the first is maximal. But so is the second. So it must be identical with the first.

Strangely enough, [. . .] examples can be set up where the requirement is necessarily violated.

[28] Note that in the letter, Einstein is *not* careful to point out that the different wavefunctions in fact depend on the result k or r one obtains on the other system, a point emphasised instead by Schrödinger (see Section 3.4.1). For detailed discussion of this issue, see Uffink (2020).

Neither Einstein nor Schrödinger mention elements of reality in these passages, yet both directly infer violation of completeness (or 'maximality'). Nor in fact does either passage make use of a particular observable pair. Indeed, the states of system I (while each of them will be the eigenstate of *some* observable) need not form an orthogonal family, and will in general form one only for a single measurement on system II (uniqueness of biorthogonal decompositions), as Einstein presumably appreciates (see p. 299):

Note: I don't care a *fiddle* [ist mir *wurst*] whether the [ψ_n] and [φ_r] can be considered eigenfunctions of observables [A], [B].

In Born's letter to Schrödinger of 28 June 1935 commenting on how disappointed he is about the EPR paper, he also focuses on the generic non-maximally entangled example rather than using the EPR state (see p. 300).

Let us now turn to the interpretation of the logic in the published paper.[29]

First of all, it is unclear where completeness actually enters into the demonstration that p_n and q_r are elements of reality. We shall presently discuss this in some detail.

Second, the repeated use of 'can' in the formulations has raised questions regarding the modal aspects of the argument – for example, whether in their premises EPR mean the *possibility* of making predictions and in their conclusions mean the *possibility* of simultaneous existence, or whether they mean to conclude from possible measurements to actual simultaneous reality, and so on.[30] We shall not discuss this at length, but it seems to us that the argument runs from the separate possibility of certain measurements to the simultaneous actuality of certain elements of reality.[31]

Third, it seems obvious that EPR are criticising an ontic reading of uncertainty (e.g. that position does not always have a physical reality).

Fourth, the EPR state is not strictly speaking a special case of their equations (7)–(8) because it is not a normalisable wavefunction, but it is well known that one can run the argument using Bohm's example of the spin singlet state (Bohm 1951, pp. 614–615). Neither does Einstein's preferred version of the argument rely on the EPR state, instead using the more general case of (7)–(8).

Returning to the role of completeness, it seems EPR do not carry out their argument as initially declared. They propose to argue from the assumption of completeness (from which it follows that non-commuting quantities do not have simultaneous reality) to a contradictory conclusion: that non-commuting quantities do have simultaneous reality after all. However, their derivation of the simultaneous

[29] On this subject, see in particular also Whitaker (2004) and Fine (2017).
[30] Cf. van Fraassen (1974) and Dickson (2002a).
[31] This seems to be the case in Schrödinger's reading of the argument in the cat paper, and in Section 5.3 we shall argue that this is also how Bohr understands EPR's argument.

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reality of position and momentum is often read as calling upon the reality criterion and not the completeness condition. Through the former one establishes directly that position and momentum of the distant particle are elements of reality, and this already contradicts completeness. There is a clear consensus in the secondary literature on this point. (Even Einstein's preferred version of the argument just needs the separation principle.)

One author who begs to differ, however, is *Schrödinger*; he does so in his letter to Born of 29 June 1935 (see p. 304):

> Note the very cautious form of the conclusion of that paper: if one accepts the present interpretation with the addendum that the description (in the pure case) is complete, then one arrives at the following contradiction: 1) conjugates are not determinable simultaneously; 2) conjugates must be determinable simultaneously. Thus, what one has accepted is inconsistent. Statements 1) and 2) are deduced only to bring out this contradiction.

Incidentally, Schrödinger is making an extraordinary claim here – one not mentioned by EPR: that the assumption of completeness implies (absurdly, of course) that the position and momentum of the distant particle must be simultaneously determinable.[32] Be that as it may, he is clearly reading the overarching logic of the EPR argument as proceeding *from the assumption of completeness* to two contradictory conclusions.

But what role can completeness be playing in deriving the simultaneous existence (or possibly even determinability) of position and momentum? Einstein may be giving us a clue in his 19 June letter (which, allowing 10 days for the letter to reach Schrödinger in England, was received in time to conceivably influence Schrödinger's comments to Born).In it, Einstein appears to draw a conclusion from completeness that is not explicit in the EPR paper (see p. 298):

> One would now like to say the following: ψ corresponds bijectively to the real state of the real system. The statistical character of the measurement outcomes is to be imputed exclusively to the measurement apparatus, or the process of measurement. If this works, I talk of a complete description of reality through the theory. If, however, such an interpretation is not feasible, I call the theoretical description 'incomplete'.

The remark about the statistical character is presumably to be understood in the light of a parallel passage in the immediately preceding description of the two-box experiment: 'The statistics only come about because the observation introduces insufficiently known factors foreign to the described system' (see p. 297).

In the case of classical mechanics, a complete state description is already enough to provide full explanations for the results of all possible measurements (with no

[32] On the question of why Schrödinger might be making this further extraordinary claim, see our discussion in Section 3.4.1.

detailed analysis of the measurement process needed). But assuming that the wavefunction is a complete description of the state of a quantum system is clearly *insufficient* for explaining individual measurement results. Thus unless quantum theory can provide a successful explanation of individual measurement results via an explicit analysis of the *process of measurement*, the only way to explain measurement results will be to admit that the wavefunction is in fact an incomplete description of the state of the system.

This reading of completeness fits nicely with the ostensive argumentative strategy in the EPR paper: assume quantum mechanics is complete. Then, not only is the state description assumed complete, but also some appropriate analysis of the 'foreign factors' brought in through the measurement process is assumed, which will provide a full explanation for individual measurement results. Apply this to a measurement (say, of position) on one of the systems in the EPR example. In principle, knowledge of the 'foreign factors' brought in by the act of measuring one particle's position should allow us to predict with certainty the result of such a measurement. But due to the perfect correlations exhibited by the EPR state, we should also be able to 'predict with certainty (i.e. with probability equal to unity) the value of a physical quantity' of the second particle. And assuming a separation principle, we can make such predictions 'without in any way disturbing' the unmeasured particle. In other words: assuming quantum mechanics is complete, the argument suggested by Einstein's remarks allows one to establish the *antecedent* of the criterion of reality, and in this way completeness *does* feature as a premise in the published version of the argument.

Our conjecture is that during their many conversations Einstein, Podolsky and Rosen discussed this version of the argument and that Podolsky had this in mind when writing the initial paragraphs announcing the logical strategy to be used, but failed to bring it out clearly later in the body of the paper. Of course, as we mentioned above, once the separability principle is assumed, incompleteness of the state description can be shown without any appeal to the criterion of reality. On this view, the core of the incompleteness argument stands independently of the faintly baroque logic within which it lies buried in the published version.

Nonetheless, some good may be borne of this roundabout argument for incompleteness of the state description. For EPR establish along the way that granting the assumption of state-description completeness, a purely quantum mechanical analysis of the measurement process *cannot explain the results of individual measurements* – otherwise known as the measurement problem.[33]

[33] More precisely, in the literature such a result is known as an 'insolubility theorem': it is not possible to *solve* the measurement problem simply by postulating some appropriate unitary interaction of the system to be measured with an imperfectly known state of a macroscopic apparatus; see Brown (1986) and Bacciagaluppi (2013) for discussion and further references, and Bassi and Ghirardi (2000) for the simplest and most general

Let us now turn to a deeper examination of Einstein's arguments as given in his correspondence with Schrödinger in the latter half of 1935.

1.5.2 Einstein's 1935 Correspondence with Schrödinger

The correspondence between Einstein and Schrödinger germane to EPR comprises the following eight letters, translated into English in full for the first time in this volume:

7 June: Schrödinger to Einstein (p. 281)
17 June: Einstein to Schrödinger (p. 295)
19 June: Einstein to Schrödinger (p. 297)
13 July: Schrödinger to Einstein (p. 317)
8 Aug.: Einstein to Schrödinger (p. 327)
19 Aug.: Schrödinger to Einstein (p. 332)
4 Sept.: Einstein to Schrödinger (p. 335)
4 Oct.: Schrödinger to Einstein (p. 343).

We shall be discussing some further aspects of this correspondence in our chapter dedicated to Schrödinger (Chapter 3). For now we focus on Einstein.

One of the most weighty contributions to this correspondence is the aforementioned letter of 19 June. In it, Einstein articulates *his* central complaint against quantum mechanics as opposed to the argument 'buried in erudition' in EPR, and presents a characterisation of interpretations of the wavefunction in a manner strongly reminiscent of his comments at the 1927 Solvay conference. This portion of the letter, perhaps more frequently quoted than any other part of this correspondence, runs as follows (see p. 297):

Before me stand two boxes with hinged lids into which I can look if they are opened; the latter is called 'making an observation'. In addition, there is a ball, which is always found in one box or the other when one makes an observation.

Now I describe a state thus: *the probability that the ball is in the first box is* $\frac{1}{2}$. – Is this a complete description?

No: A complete statement is: the ball *is* in the first box (or it is not). This then is how the characterisation of the state should look like in a complete description.

Yes: Before I lift the lid of the box, the ball is not at all in *one* of the two boxes. This being in a definite box only comes about through my lifting of the lid. Only through this does

proof. The first version of the theorem was proved already by von Neumann in his book, because such a process would of necessity violate unitarity (von Neumann 1932, p. 233; 1955, pp. 437–439). Our preferred construal of EPR's logic can then be seen as an alternative proof of the insolubility theorem – which we might call the 'EPR Theorem'. A preliminary reading of Einstein along these lines is proposed in Bacciagaluppi (2013), and a related proof of the insolubility theorem is spelled out in Bacciagaluppi (2014).

the statistical character of the world of experience, or rather of its empirical regularities, come about. The state before lifting the lid is *completely* characterised by the number $\frac{1}{2}$, whose meaning, however, when carrying out observations manifests itself only as a statistical result. The statistics only come about because the observation introduces insufficiently known factors foreign to the described system.

According to Einstein, the 'Schrödingerian' view that the wavefunction is a complete description commits one, at least in principle, to providing an explanation of definite experimental results in terms of external factors brought in by the measurement interaction. (It is perhaps because no such detailed explanation is provided that Einstein calls this view 'spiritualist' – or maybe because the only way this view can work is by invoking 'spooky' action at a distance!) Under a 'Bornian' interpretation a more complete description is available in principle, and a measurement simply reveals the elements of this more complete description, rendering unnecessary any appeal to external factors introduced through measurement.

A wee note is needed on Einstein's use of the term 'Bornian' here. Is Born not one of the champions of the idea that the quantum mechanical description is complete? Is Einstein equivocating on Born's use of the term 'statistical' in characterising his own approach to quantum mechanics?[34] As a matter of fact, it appears that in the papers in which Born first introduced and discussed the 'statistical interpretation' (Born 1926a, b, c), Born was applying it *exclusively* to the statistics of stationary states – consistent with the assumption held at that time that a system is *always* in a stationary state and performs quantum jumps between them. Thus Born's interpretation of quantum mechanics is 'statistical' in the Einsteinian sense that the wavefunction does not describe the state of the system completely, but only the statistical distribution of stationary states. And this despite Born's own pronouncements about the completeness of the theory.[35]

Returning now to the letter of 19 June: Einstein remarks that the 'Talmudist' (Bohr) refuses to take seriously talk about reality and conflates the Bornian and Schrödingerian interpretations. But insisting on the separability principle exposes this conflation. Why? Because if one insists on the separability principle, one realises that a mechanism explaining why one gets a certain result when one

[34] As early as November 1928, in a speech delivered to the Gesellschaft der Wissenschaften zu Göttingen [Scientific Society of Göttingen] Born states that it can be shown in a mathematically exact way that the established formalism of quantum mechanics allows for no completion. Thus if one wants to retain the hope that determinism will someday be recovered, one must consider the present theory '*contentually false*' (Born 1929, p. 118). This is an echo of the views that von Neumann expressed in the paper wherein he first proved what became known as his 'no-go' theorem against hidden variables – a paper that was presented by Born to the very same society almost exactly one year earlier (von Neumann 1927). On von Neumann's paper, see also Bub (2010, 2011), Crull and Bacciagaluppi (2017b) and Bacciagaluppi (2022).

[35] For a more detailed discussion of Born's statistical interpretation, see Bacciagaluppi (2008), Bacciagaluppi and Valentini (2009, chapter 3), Bacciagaluppi, Crull and Maroney (2017), and Bacciagaluppi (2022).

performs a measurement cannot explain the result (whether positive or negative[36]) obtained *at a distance*. Thus the Schrödingerian view and the Bornian view are not only very different, but the former is *ruled out* because it fails in general to explain nonlocal results. The only view that remains is the Bornian one. Einstein then proceeds to give his preferred version of the quantum mechanical thought experiment, as already discussed above.

Also revealing is Einstein's letter of 8 August, in which he presents an entirely different argument against the 'Schrödingerian' view (see p. 327):

> Let the system be a substance in a chemically unstable equilibrium – say, a pile of gunpowder that can ignite itself by internal forces, where the average lifetime is of the order of one year. In principle this is very easy to represent quantum mechanically. Initially the ψ-function characterises a sufficiently definite macroscopic state. Your equation, however, ensures that this is no longer the case after a year has passed. The ψ-function describes then rather a kind of mixture of a not-yet and an already-exploded system. Through no interpretational artistry can this ψ-function be made into an adequate description of a real state of affairs; in truth there just is no middle thing between exploded and unexploded. Thus your equation certainly cannot provide the description of the actual process as you in fact imagine. (On the other hand, in the statistical sense it can certainly reproduce correctly the changes in a collection of systems.) With this example I want to suggest that your attempt at an interpretation fails with respect to what we know with certainty from coarse macroscopic experience.

Schrödinger quickly replies with his famous cat example, and in his 9 September letter Einstein concedes that in this matter he and Schrödinger are very much in agreement. What is worth emphasising is the difference between this argument and the EPR argument, and even Einstein's preferred version of EPR: the gunpowder thought experiment dispatches the 'Schrödingerian' view directly, without any need for an additional separation principle. This makes sense if one recalls that Einstein invoked the separation principle in the 19 June letter specifically in order to beat the 'Talmudist'– who might not otherwise care – and in order to argue for a distinct 'Bornian' view.

We know from various sources that Einstein remained convinced of the incompleteness of quantum mechanics until his death. Among these sources are a paper written less than a year following EPR, entitled 'Physik und Realität' ['Physics and Reality'] (Einstein 1936), which contains a clear albeit informal statement of Einstein's preferred version of the incompleteness argument.[37] Another source is a

[36] For a striking (and possibly the earliest) discussion of 'negative result' or 'null result' experiments, see Schrödinger (1934) and our related remarks in Chapter 3.

[37] For a translation and brief discussion of the relevant portion of this article, see Howard (1985, pp. 184–185). Note also the separate argument in Einstein's footnote 3: even when described by a superposition of energy eigenstates, a quantum state must have a definite energy, because of $E = mc^2$ and because the *inertia* of a system must have a well-defined value. Applied to the photon box, this argument of course again shows that Einstein thought of the spread in energy as purely statistical.

paper written as a contribution to the *Festschrift* occasioned by Born's retirement from the University of Edinburgh (Einstein 1953). The latter makes an explicit plea for the 'Bornian' view of quantum mechanics using a gunpowder-like argument, this time with a little macroscopic ball bouncing between two walls in a box: quantum mechanics describes this by a standing wave and predicts correctly the statistics of position and momentum, but a macroscopic ball surely has a definite position and momentum at all times.[38]

Before going on to discuss Schrödinger's point of view in Chapter 3, we shall give a brief survey of reactions to the EPR paper in the coming chapter, including the 'minor' responses to EPR. This will also provide a wider context in which to situate our detailed discussion in subsequent chapters.

[38] For discussions of Einstein's 1953 paper (where Einstein uses his example also to criticise Bohm's (1952) new theory), see Myrvold (2003) and Bacciagaluppi (2016b).