

# The Past of an Electron: Young Popper’s *Gedankenexperiment* Against the Indeterminacy Relations

Marco Giovanelli 

Università di Torino  
Department of Philosophy and Educational Sciences  
Via S. Ottavio, 20 10124 - Torino, Italy  
`marco.giovanelli@unito.it`

---

Between 1934 and 1936, Popper wrote at least eight manuscripts on the interpretation of quantum mechanics, proposing a *Gedankenexperiment* based on electron-photon collision in which reconstructing the past trajectory of an electron would allow one, via momentum conservation, to predict both position and momentum of a photon with arbitrary precision, thus apparently ‘beating’ the indeterminacy relations. Popper corresponded about it with leading physicists such as Weisskopf, Heisenberg, von Weizsäcker, and Einstein, who pointed out the flaws in his reasoning. In retrospect, Popper described these efforts as a “gross mistake”. Yet Jammer (1974) suggested that this mistake may have influenced the 1935 Einstein-Podolsky-Rosen argument, while Margenau (1974) credited Popper with anticipating the later distinction between preparation and measurement. Drawing on unpublished material, this paper argues that both claims are doubtful. Popper’s thought experiment is more closely related to the 1931 Einstein-Tolman-Podolsky setup, though he reached the opposite conclusion precisely because he misunderstood the link between preparation and measurement. The same ‘mistake’, the paper concludes, also undermines the EPR-like thought experiment Popper proposed at the turn of the 1980s.

---

*Keywords:* Karl R. Popper • quantum mechanics • uncertainty relations • thought experiments • preparation vs. measurement

## Introduction

Between 1934 and 1936, Karl R. Popper composed at least eight manuscripts revolving around a *Gedankenexperiment* intended to challenge the widespread interpretation of the so-called indeterminacy or uncertainty relations, which, in quantum mechanics, were thought to set a fundamental limit to the joint *measurement* of conjugate quantities such as position and momentum, or time and energy:

“Zur Kritik der Ungenauigkeitsrelationen” – August 1934<sup>1</sup>

“Ergänzung zu der vorstehenden kurzen Mitteilung” – before November 1934<sup>2</sup>

“Bemerkungen zur Quantenmechanik” (chapter 2.VII of *Logik der Forschung*<sup>3</sup>) and Appendix V, VI, VII — Fall 1934

“Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” – December 1934<sup>4</sup>

“Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [B]” – December 1934<sup>5</sup>

“Erwiderung auf die Kritik Heisenberg-Weizsäcker” – late 1934 to early 1935

“Meßanordnung” — February 1935

“Zur Kritik der Ungenauigkeitsrelationen [1935]” – August 1935<sup>6</sup>

“Bemerkung zum Komplementaritätsproblem der Quantenmechanik” – 1936<sup>7</sup>

Only 1 and 3 were published during Popper’s lifetime. Numbers 6 and 7 have been lost, although 6 may in fact be identical with 5. The remaining material was published only recently in a collection of Popper’s early writings.<sup>8</sup> While drafting these texts, the young Popper—at that time a schoolteacher with only a single short publication to his name<sup>9</sup>—engaged in extensive correspondence with leading physicists of his time: (1) Victor Weisskopf, then assistant to Pauli in Zürich; (2) Heisenberg and his assistants Carl Friedrich von Weizsäcker and Hans Euler; and (3) Einstein. Some of this correspondence is preserved at the Karl Popper Archives and remains unpublished. Moreover, Popper discussed his ideas in person with Viennese physicists he could reach: Franz Urbach, his cousin, the crystallographer Käthe Schiff, Hans Thirring<sup>a</sup> and his assistant Eugene Guth, as well as Felix Ehrenhaft. Naturally, *verba volant*: we have little insight into the concrete content of these exchanges.

The late 1920s and early 1930s saw a proliferation of *Gedankenexperimente* as tools for probing conceptual issues *outside* the quantum-mechanical formalism—either to demonstrate its consistency or to attempt to refute it. Heisenberg<sup>11</sup> formulated his famous  $\gamma$ -ray microscope thought experiment to show that the uncertainty relations reflect the mutual disturbance arising from the experimental determination of position and momentum. By contrast, Einstein was regarded as having devised *Gedankenexperimente*, such as the light-quantum box thought experiment, to challenge the uncertainty relations.<sup>12</sup> Popper, for his part, accepted the mathematical *derivation* of the uncertainty relations from the quantum formalism; however, he devised a *Gedankenexperiment* intended to expose the tension between two different *interpretations* of the uncertainty relations. It was Popper’s important insight that what are called ‘measurements’ in quantum mechanics in fact typically refer to two distinct operations: (a) the *physical selection* or filtering of an ensemble of systems with, for example, the same position or momentum; and (b) the *measurement* or registration of position or momentum for a single system via an irreversible trace in a macroscopic apparatus. Popper aimed to show that the indeterminacy relations impose a limitation on the *physical selection* of an ensemble of systems in which both position *and* momentum are sharply defined; however, *pace* Heisenberg, they do not impose any limitation on the accuracy of the *measurement* of position *and* momentum for a single system.

Popper aimed to exploit the fact that quantum physicists generally conceded that in quantum mechanics there is an asymmetry between what he called *non-predictive measurements*, that is, the retrospective reconstruction of a single particle’s past position-cum-momentum, or path, and *predictive measurements*, that is, predictions of its future one. Given a physical selection, path reconstructions of arbitrary precision performed once a registration has occurred were widely regarded as *permitted*. By contrast, path predictions of arbitrary sharpness before a registration occurs were typically considered strictly *forbidden*. According to Popper, however, path reconstructions are not only ‘permitted’ but *required* by quantum mechanics. The frequency predictions of quantum mechanics ultimately concern probability distributions over *past* paths. Indeed, the physical reality of such trajectories cannot be *verified*, since any attempt to do so would

---

<sup>a</sup>According to Popper’s recollection, in 1934 (possibly 1935) Thirring invited him to his seminar ((ref. 10)).

disturb the path. However, Popper argued that without assuming the physical reality of past paths, the predictions of quantum mechanics could not be *falsified*. In this light, Popper could thus portray physicists' inference from the non-observability to the non-existence of well-defined paths as a manifestation of the same positivist disregard for the asymmetry between 'verification' and 'falsification' that the *Logik der Forschung* was intended to denounce.

According to Popper,<sup>13</sup> the prohibition of predictive measurements of arbitrary sharpness was ultimately rooted in physicists' tacit assumption of what he called the *coupling-hypothesis* (*Kopplungshypothese*)<sup>b</sup> between 'physical selections' and 'predictive measurements'. This hypothesis claims that predictive 'measurements', that is, measurements concerning future registrations, always presuppose a 'physical selection', that is the previous filtering of an ensemble of particles. As a consequence, they are subject to the constraints of the statistical uncertainty relations. Popper believed he could demonstrate that, granting the validity of standard conservation laws, the coupling-hypothesis was inconsistent with the formalism of quantum mechanics. To this end, he devised a thought experiment based on an electron-light quantum collision. By using a *non-predictive measurement* (a momentum filtering followed by a position registration) to reconstruct the past trajectory of an electron, the set-up was intended to exploit momentum conservation so as to bypass the need to filter the light quanta according to momentum. In this way, without violating the statistical uncertainty relations relations, the experiment was meant to allow arbitrarily precise *predictive measurements* of the latter's future registration position after the collision.

In retrospect, Popper himself would describe his thought experiment as "a gross mistake", for which he had been "deeply sorry and ashamed."<sup>15</sup> Yet commentators, writing at the height of Popper's fame in the 1970s, attributed some significance to his early engagement with quantum mechanics. Max Jammer<sup>16</sup> conjectured that Popper's mistaken thought experiment was at least instrumental in inspiring Einstein-Podolsky-Rosen (EPR) thought experiment.<sup>17</sup> Indeed, according to Jammer, Popper's setup bore a "striking resemblance" to the EPR setup, raising the question of a possible influence from the work of Einstein and his collaborators.<sup>18</sup> Henry Margenau<sup>19</sup> credited Popper with anticipating the distinction between 'preparation' (which Popper called 'physical selections') and 'measurement', a distinction he himself proposed around the same time<sup>20</sup> and which later became standard. In particular, according to Margenau, Popper was among the first philosophers to appreciate that the indeterminacy relations expressed a 'preparation uncertainty' rather than a 'measurement uncertainty'.<sup>21</sup>

This paper, based on little-known or unpublished material from the Karl Popper Archives, aims to demonstrate that both claims fail to withstand critical scrutiny. Popper was wary of implying that the "gross mistake made by a nobody (like myself)" might have influenced Einstein, noting only that "from a purely temporal standpoint" the possibility could not be ruled out.<sup>22</sup> However, it is precisely the chronology that seems to exclude any influence of Popper's thought experiment on the emergence of the 1935 EPR argument. What can be said is that Popper independently formulated a variant of the lesser-known 1931<sup>23</sup> Einstein-Tolman-Podolsky (ETP) thought experiment, though he arrived at the opposite conclusion. Contrary to the received view,<sup>24</sup> the paper argues that the reason was ultimately that Popper deeply misunderstood the relation between

---

<sup>b</sup>Popper (ref. 14) later adopted the translation 'hypothesis of linkage', possibly because 'coupling' in English typically implies a physical interaction. However, I prefer to retain a rendering closer to the original German, which better reflects its resonance with contemporary physical discourse.

‘preparation’ and ‘measurement’ that he allegedly anticipated. Popper does not appear to have taken an interest in Margenau’s key insight that measurements may *destroy* the state of a system, nor, more importantly, did he recognize that preparations invariably *create* a new state. Indeed, Popper’s thought experiment is ultimately based on the implicit assumption that what he calls a ‘selection’ is *not* a ‘preparation’ in Margenau’s sense, since it merely *reveals* a pre-existing state. From Margenau’s point of view, preparation and measurement must indeed be distinguished; however, their *coupling* is not an ‘additional hypothesis’, ultimately at odds with the rest of the formalism, as Popper argued, but rather a central feature of quantum mechanics that distinguishes it from classical physics.

Although ‘Young Popper’ has long been a well-established ‘scholarly field’,<sup>25</sup> this episode has received little attention. The present paper seeks to redress this lacuna in the literature. Against the background of the 1930s debate on the past trajectories of particles in quantum mechanics (section 1), it reconstructs the development of Popper’s thought experiment (sections 2 to 4) and examines the criticisms raised by Heisenberg and his Leipzig group (section 5), as well as by Einstein (section 6). Ultimately, though only after considerable resistance, Popper conceded that his thought experiment was a ‘mistake’. The failure of the thought experiment should have convinced him of the validity of the coupling-hypothesis between preparations and measurements—the very principle he had aimed to challenge. Instead, Popper concluded that the flaw lay in the specific ETP-like setup. Accordingly, Popper,<sup>26</sup> sought to replace the ETP-like experiment with an EPR-like one. For this reason, recent historical literature<sup>27</sup> tends to dismiss Popper’s early attempt as a youthful blunder, focusing instead on the later setup.<sup>28</sup> Yet this paper argues that the latter was ultimately grounded in the same ‘mistake’: appealing to a perfect correlation—this time, of relative position—to circumvent the preparation-measurement coupling.

## 1 Setting the Stage: The Past Trajectory Problem

### 1.1 The Heisenberg-Schlick Dispute

In the spring of 1929, Heisenberg delivered a series of lectures on quantum mechanics at the University of Chicago. A written account of the lectures was completed in March 1930 and appeared later that summer in print, in both English and German editions.<sup>29</sup> In presenting the indeterminacy relations, Heisenberg takes the opportunity to address, although rather in passing, the vexed question of the retrodictability of sharp joint values of position and momentum variables of, say, an electron. As already pointed out in Bohr’s Como lecture,<sup>30</sup> if one performs two position measurements at successive times, then from the time of flight one can, in principle, reconstruct the electron’s momentum *between* these two position measurements with arbitrary precision. Heisenberg discussed a similar thought experiment in his book, but replaced the first position measurement with a momentum measurement:

[I]t should be noted that the uncertainty relations apparently do not apply to the past. For if the electron’s *velocity* is initially known and then its *position* is measured precisely, one can also calculate the electron’s positions for the *time before the position measurement* with precision; for this past,  $\Delta p \Delta q$  is then smaller than the usual limit. Yet this *knowledge of the past* is purely speculative, since (because of the *change of momentum during the position measurement*) it in no way enters as an initial condition into any calculation concerning the electron’s future and does not appear in any physical experiment at all.

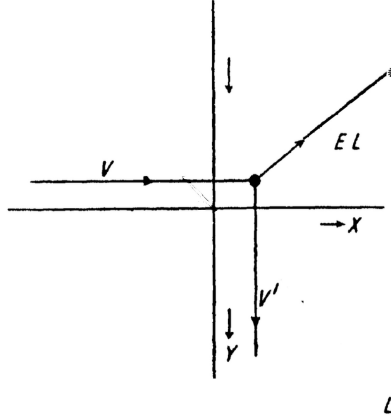


Fig. 1. Adapted from Werner Heisenberg, *Die physikalischen Prinzipien der Quantentheorie*, Leipzig, 1930, 19

Whether one should ascribe any physical reality to the aforementioned calculation about the electron's past is therefore purely a *matter of taste* [*Geschmacksfrage*].<sup>31</sup>

Heisenberg likely had in mind the measuring procedure he describes a few pages later.<sup>32</sup> Electrons move along the  $+x$  direction with a known longitudinal momentum  $p_x$ , while light quanta enter along the  $-x$  axis (figure 1). The Doppler shift  $\delta\nu$  of a scattered light quantum at time  $t_0$ , assuming energy and momentum conservation, allows one to infer the electron's transverse momentum  $\delta p_y$ ; the point where the electron later arrives on the detection screen at time  $t_1$  determines its position  $\delta y$ .<sup>c</sup> This seems to permit reconstruction of the electron's 'past path' between  $t_0$  and  $t_1$  with arbitrary precision,  $\delta y \delta p_y \approx 0$ . Heisenberg's reference to the 'time before the position measurement' is somewhat ambiguous, as it seems to imply the possibility of tracing the electron's path even *before* the first momentum measurement at  $t_0$ . Popper indeed relied on this interpretation of the passage, as discussed below (section 5.1).

Heisenberg immediately pointed out that the reconstruction of the electron's past has no predictive significance for its future behavior, and therefore does not constitute a violation of the indeterminacy relations, since it cannot be used as an initial condition for any subsequent dynamical calculation. However, the question of whether such reconstructions possess any 'physical meaning'—which Heisenberg dismissed as a 'matter of taste'—constitutes, as one might expect, an interesting philosophical puzzle. In fact, the issue was immediately taken up by Moritz Schlick, who was at that time working on a manuscript on the problem of causality. At the end of November, Arnold Berliner (the editor of *Die Naturwissenschaften*) confirmed receipt of 'Die Kausalität in der gegenwärtigen Physik', which soon circulated among physicists, prompting correspondence with Einstein, Heisenberg, and Pauli. The paper identifies causality with predictability; thus, Schlick accepts that Heisenberg's relation concerns an indeterminacy of prediction, thereby limiting the applicability of causality on the microscopic scale.<sup>33</sup> Schlick was aware that such indeterminacy does not apply to retrodictions, as Heisenberg himself points out,<sup>34</sup> Nevertheless, Schlick remained unconvinced by Heisenberg's agnostic stance toward the physical reality of past trajectories, since it stood in contrast to his own 'positivism'.<sup>35</sup>

Indeed, Heisenberg's wording seems implicitly to concede that electrons do have a path, albeit an *unknown* one, since any attempt to measure such a path would inevitably

<sup>c</sup>In the following, I use  $\Delta q$  and  $\Delta p$  to denote the dispersions of the statistical distributions, and  $\delta q$

disturb it. For Schlick, however, Heisenberg, if faithful to his own philosophical stance, should have held that the very notion of a path is *meaningless*:

Heisenberg thinks that “whether any physical reality should be assigned to the calculation concerning the past of the electron is purely a question of taste”<sup>[36]</sup>. But I should prefer to put it more strongly, in complete agreement with what I take to be the incontestable basic viewpoint of Bohr and Heisenberg themselves. If a statement about an electron’s position is not *verifiable* within atomic dimensions, *we can attach no meaning to it*; it becomes impossible to speak of the ‘path’ of a particle between two points at which it has been observed. (This is not, of course, true of bodies of molar dimensions, for it is in principle possible to verify afterwards that the projectile was located at the intervening points).<sup>37</sup>

Schlick argues that although quantum theory’s equations allow one to calculate an electron’s past positions after exactly measuring its velocity and then its position, such results are unverifiable in principle: one cannot verify whether the electron was actually moving with that velocity at the calculated point, since any observation would disturb its path in an uncontrollable way. For Schlick, therefore, the very idea of a sharply defined past path of the electron lacked physical meaning, just like the notion of its future path.

### 1.2 The ETP Thought Experiment

It is not surprising that, in his comments on Schlick’s article in late 1930, Einstein described Schlick’s stance as too positivistic.<sup>38</sup> Concluding his letter, Einstein mentioned that he would spend the coming winter in Pasadena. Just a fortnight after Schlick’s paper<sup>39</sup> appeared on February 13, 1931, Einstein, in collaboration with Richard Tolman and Boris Podolsky (ETP), submitted to the *Physical Review* a short letter reporting the results of his American visit. The paper was published on March 15.<sup>40</sup> Neither Heisenberg nor Schlick are mentioned; however, Einstein and his collaborators address the same question of the past path of particles in quantum mechanics. They argue that *if* sharp retrodictions about one particle were possible, *then* one could, with the help of conservation laws, infer predictions about the future path of a second particle that would otherwise be excluded by the uncertainty relations. Thus, ETP conclude that retrodictions should *not* be allowed. The indeterminacy relations apply equally to the past and the future.

The proposed arrangement (figure 2) resembles the one Einstein reportedly used in the 1930 debate with Bohr<sup>41</sup> in Brussels.<sup>42</sup> A box  $B$  contains identical particles. At time  $t_0$ , a shutter  $S$  releases two particles,  $a$  and  $b$ . By weighing the box before and after the emission, the observer can determine the total energy loss,  $E_{\text{tot}} = E_a + E_b$ . At  $O$ , particle  $a$  may undergo different measurements. Its momentum  $p_a$  (and energy  $E_a$ ) can be determined, for example, by a Doppler-effect measurement at  $O_1$ , while its arrival position and time  $t_1$  can be recorded at  $O_2$  by some mechanical time-recording counter. From the measured position of  $a$  at  $t_1$ , together with its past momentum  $p_a$ , the shutter opening time  $t_0$  can, in principle, be reconstructed. If  $E_a$  has been measured, then the energy of particle  $b$  can be inferred from  $E_b = E_{\text{tot}} - E_a$ . Once  $t_0$  has been determined, the arrival time  $t_b$  at  $O_2$  can be inferred from the geometrical arrangement of the setup, that is, from  $SRO$ .

At this stage, a backdoor appears to open, seemingly offering a way around the energy-time uncertainty relation  $\delta E, \delta t \leq \frac{h}{4\pi}$ . Yet ETP quickly close it again.<sup>43</sup> One

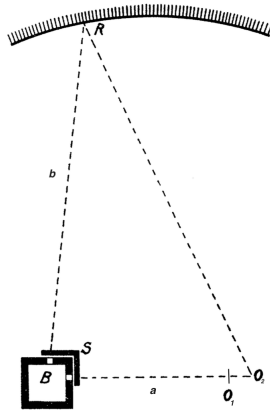


Fig. 2. Adapted from Albert Einstein, Richard C. Tolman, and Boris Podolsky, Knowledge of Past and Future in Quantum Mechanics. *Physical Review* 37 6 (1931), p. 780

might attempt to measure momentum via the Doppler shift of low-energy light quanta, that is, long-wavelength radiation (e.g., in the infrared range). Assuming conservation of momentum and energy, this method, in principle, allows one to infer the momentum of a particle before and after scattering from the frequency shift of the scattered photon. However, to determine the frequency with high precision, many oscillation periods must be observed, which blurs the exact time of the scattering event. As a result, the precise moment at which the shutter  $S$  was opened cannot be reconstructed. Conversely, if one tries to determine the scattering time very precisely, one must use high-frequency (short-wavelength) radiation, thereby losing information about the momentum before the collision. The authors summarized their conclusion as follows:

Thus in our example, although the velocity of the first particle could be determined both before and after interaction with the infrared light, it would not be possible to determine the exact position along the path  $SO$  at which the change in velocity occurred as would be necessary to obtain the exact time at which the shutter was open.

It is hence to be concluded 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* events. It is also to be noted that although it is possible to measure *the momentum of a particle and follow this with a measurement of position*, this will not give sufficient information *for a complete reconstruction of its past path*, since it has been shown that there can be *no method for measuring the momentum of a particle without changing its value*.<sup>44</sup>

Thus, ETP point out the flaw in this kind of experiment: one can reconstruct the path of a particle *between* the momentum and the position measurements, but not *before* the momentum measurement at  $O_1$ . The uncertainty principle therefore not only restricts predictions about the future but also imposes limitations on the reconstruction of the past: the path reconstruction before the momentum measurement must be physically forbidden *if* the indeterminacy relations are to hold.

As Ehrenfest wrote to Bohr in the summer of 1931, Einstein, contrary to common perception,<sup>45</sup> was clearly not attempting to devise “new *perpetua mobilia* = machines against the uncertainty relation”.<sup>46</sup> Rather, Einstein’s point was that, *because of* the uncertainty relations, the experimental *choice* between determining the time or the momentum of system  $a$  appears to retroactively determine whether the emitted light quantum  $b$  possesses a definite energy or a definite emission time, even *after* it has

already left the box<sup>47</sup> After a talk in Leiden in November 1931, Einstein might have been stimulated by Ehrenfest to replace the energy-time with the position-momentum uncertainty.<sup>48</sup> Indeed, as later recalled by Léon Rosenfeld,<sup>49</sup> Einstein, after a seminar in Brussels in the spring of 1933, apparently reformulated the argument without the aid of the box, considering two particles emitted simultaneously: a measurement of the momentum (or position) of the first particle allowed an inference of the corresponding momentum (or position) of the second. If Rosenfeld’s testimony is accurate, Einstein possessed, in embryonic form, the gist of the EPR paper before permanently moving to the United States on October 17, 1933.

## 2 Popper’s Turn to Quantum Theory: The Correspondence with Weisskopf

At the turn of 1934, Popper was reworking the manuscript of *Die Beiden Grundprobleme der Erkenntnistheorie*<sup>50</sup> into what would become *Logik der Forschung*.<sup>51</sup> In the spring, Popper submitted a first version of the manuscript to Schlick, the editor of the series. At the request of the publisher Springer, the manuscript had to be shortened considerably.<sup>52</sup> While Popper was adding two technical chapters on probability and one on quantum mechanics, much of the editing was carried out with the help of his uncle, Walter Schiff. Between May and June 1934, Popper likely began to devote special attention to the quantum physics chapter, discussing it with Viennese physicists like Urbach, Urbach’s friend Weisskopf, and his cousin Käthe.<sup>53</sup> At that time, Weisskopf was in Zurich as Pauli’s assistant, so their exchange had to take place by mail, leaving us written evidence of Popper’s thinking during that period. Although Popper’s original letter has not survived, much of his position can be inferred from Weisskopf’s reply.

Weisskopf seems to concede to Popper that the uncertainty relations do not exclude the possibility that the position and momentum of a single electron could, in principle, be ‘known’ (*i.e.*, measured) with arbitrary accuracy. If the position is fixed by a very narrow slit, the emerging beam no longer consists of electrons with a well-defined momentum or energy. The diffraction pattern on the screen reveals this momentum (or angular) distribution and the associated energy spread. When the electrons strike a target, some atoms are excited even though their excitation requires more energy than the mean kinetic energy of the electrons. From these excitations one may infer both position and energy, and thus momentum of single electrons. As Weisskopf put it, “[t]his consideration is of course not changed if, by some measurements, I determine which energies are present in this accumulation of electrons *without altering their spatial arrangement*. In this, I agree with you and admit my mistake.”<sup>54</sup>

However, Weisskopf immediately raises a counter-objection: “But now comes the point: if I can measure these energies without disturbing the spatial arrangement, then I have devices with which I can also *produce [herstellen]* beams (aggregations of electrons) that are 1.) spatially confined and 2.) possess a sharp energy (= momentum of free particles).”<sup>55</sup> The possibility of producing such aggregates of electrons would indeed violate the indeterminacy relations, since a wave localized in space must contain infinitely many frequencies, and thus momenta and energies. Popper probably replied that instruments allowing *measurements* of sharp electron energy and position might exist without thereby enabling the *production* of an *aggregation* of such electron beams. However, Weisskopf doubted this, arguing that if we can *measure* electron energy, we should also be able to *separate* electrons according to predetermined values; that is, to

---

and  $\delta p$  to denote the measurement errors.

produce an aggregate of electrons all with the same energy and the same position:

(1) Apparatuses that can *measure* both position and energy at the same time are not necessarily able to *produce* electron aggregates with sharp position and energy. I consider this conclusion not entirely correct, since - in the case that one has the possibility of determining the energy of electrons at sharp position - it must surely also be technically possible somehow to remove them, for example, if they have too much energy.

(2) You can probably anticipate my line of reasoning, which leads you to say that quantum mechanics is only capable of dealing with that world in which such *apparatuses* are not applied. But I would already regard that as a breakdown. After all, apparatuses are also part of nature and should therefore be valid within it.

There would, however, have to exist accumulations of electrons whose position and energy are sharply determined, even though this is impossible according to wave theory. Thus the statement that electrons can in fact always be represented by waves would not always be correct, and in this lies perhaps the end of quantum mechanics.<sup>56</sup>

Here the fundamental issues at stake are laid out quite clearly: (1) the distinction between (1A) *producing* an aggregate of electrons with arbitrarily sharp position and momentum and (1B) *measuring* the position and momentum of a single electron with arbitrary precision; (2) the *coupling* between these two operations, that is, the claim that the possibility of (1B) necessarily implies that of (1A), which would in turn constitute a violation of the indeterminacy relations.

Weisskopf asked Popper to share his opinion on these ideas. However, regarding Popper's request to cite their discussions in his forthcoming book, he asked not to be mentioned by name. In the following weeks, Popper may have come up with a clever trick to counter Weisskopf's objection: a *Gedankenexperiment* in which an apparatus was laid out that could *measure* with arbitrary precision the position and momentum of a single particle without allowing the *production* of an ensemble more homogeneous than the indeterminacy relations would permit. Already, on August 27, 1934, Popper submitted a short note, 'Zur Kritik der Ungenauigkeitsrelationen', to *Die Naturwissenschaften*, one of the most prestigious scientific journals of that time.<sup>57</sup> As we shall see, Popper independently envisaged the same backdoor to the indeterminacy relations as the one ETP had already suggested, only using momentum and position instead of energy and time.

Popper seems to have soon realized that this backdoor could indeed be closed in the same way suggested by ETP. However, contrary to Einstein, Popper wanted to leave the backdoor open. Thus, he wrote a short supplement, *Ergänzung zu der vorstehenden kurzen Mitteilung*, to clarify the matter. By the autumn of 1934, Popper had completed *Logik der Forschung*.<sup>58</sup> Thus, by that time, chapter 2.VII, titled *Bemerkungen zur Quantenmechanik*, together with the three appendices V, VI, and VII devoted to the same topic, can be regarded as complete. The manuscript of the *Ergänzung* was probably written after the paper had already been submitted to *Die Naturwissenschaften*, but before it was published.<sup>d</sup> It must also have been written prior to Appendices VI and VII, which appear to be Popper's attempts to address problems raised in the *Ergänzung*.

---

<sup>d</sup>No reference is made to von Weizsäcker's response that was published alongside Popper's article 1934 article in the *Die Naturwissenschaften*; see below section 5.1.

### 3 Popper's Interpretation of the Indeterminacy Relations

Throughout his various treatments of quantum mechanics, including the more extensive discussion in the *Logik der Forschung*, Popper takes for granted the central role that the so-called Heisenberg's indeterminacy relations (*Unbestimmtheitsrelation*) plays in the formalism of quantum mechanics; specifically, the inequality:

$$\Delta p_x \Delta x > h/4\pi \quad (1)$$

where  $x$  denotes the position of a point mass and  $p_x$  represents the corresponding momentum component. Popper's aim was not to call into question the mathematical *derivation* of Heisenberg's formula, but rather to reassess its physical *interpretation*. The indeterminacy relations can be interpreted (a) *à la* Heisenberg as 'imprecision relations,' (*Ungenauigkeitsrelationen*) which set a limit on the accuracy of simultaneous measurements of  $x$  and  $p_x$  for a *single* particle; or (b) as 'dispersion relations,' (*Streuungsrelationen*), expressing the limit on the standard deviations of repeated  $x$  and  $p_x$  measurements on an *ensemble* of particles. It is undeniable that Popper was among the first to make this distinction, at least in the philosophical literature,<sup>e</sup> although similar views were defended around the same time by other Viennese scholars, such as Richard von Mises,<sup>61</sup> whose work Popper might well have known. However, Popper possibly arrived at a similar conclusion independently, starting from von Mises'<sup>62</sup> frequency interpretation of probability, which he defends in Ch. VI.

#### 3.1 Physical Selections and Measurements

According to Popper, Heisenberg's *philosophical justification* for eq. (1) was based on the idea that measurement itself disturbs physical systems: precision in one variable (e.g., position) can be achieved only at the expense of precision in its conjugate (e.g., momentum), with eq. (1) establishing the bound. Against this view, Popper argued that the *mathematical derivation* of eq. (1) follows from Born's interpretation of the  $\psi$ -function as a probability amplitude, with  $|\psi(x)|^2 dx$  giving the probability of finding a particle along the  $x$ -axis within the infinitesimal interval between  $x$  and  $x + dx$ . Using Fourier analysis, one can show that the position and momentum probability distributions are mathematically related in such a way that a narrower distribution in position entails a broader distribution in momentum, and vice versa. In this reading, eq. (1) regulates the dispersion relation between these distributions rather than limits the precision of measurement.

For Popper, the confusion between these two *interpretations* ultimately results from physicists' failure to distinguish clearly between (1) a *physical<sup>f</sup> selection* (*Aussonderung*), which occurs when, for example, a beam of particles is actively filtered so that only those passing, say, through a narrow slit (a region  $\Delta x$ ) remain, thereby selecting an 'aggregate' of particles all with the same position, a so-called 'pure case',<sup>g</sup> and (2) a

---

<sup>e</sup>In the physics literature this distinction was at least implied by Earle H. Kennard (ref. 59) and Hermann Weyl (ref. 60). Popper was familiar with the latter.

<sup>f</sup>As opposed to merely "mental" or "imagined" selection (ref. 63)). These correspond to conditional selections, which define a sub-ensemble after the fact by conditioning on the value of a random variable obtained in a measurement (say, an ensemble of particles  $B$  that have the same momentum as  $A$ ).

<sup>g</sup>An aggregate (*Menge*) corresponds to what von Mises called a *Kollektiv*, in modern terms an 'ensemble'. As Popper later recognized, at that time he did not clearly distinguish between an aggregate of co-present particles, such as a 'beam' (a spatial ensemble), and an aggregate consisting of repetitions of an experiment performed with one particle or one system of particles (a temporal ensemble) (ref. 64).

*measurement* (*Messung*), which involves the actual registration of a value, for example, the position of a single particle, through an irreversible change in a piece of macroscopic apparatus (such as a spot on a screen or a pointer deflection).<sup>65</sup> Young Popper thus appears to have articulated a distinction similar to that between ‘preparation’ and ‘measurement’ formulated by Margenau.<sup>66</sup> around the same time.<sup>h</sup> He argues that *not every physical selection amounts to a measurement*: for instance, a velocity filter produces an ensemble of electrons with definite momentum (a monochromatic plane wave), yet no momentum value is recorded; and conversely, *not every measurement is a physical selection*: electrons from a monochromatic beam may be registered by a Geiger counter at a given location, although they are not physically separated by position.<sup>67</sup>

The two possible *interpretations* of eq. (1) can then be rephrased as follows:

URa - the indeterminacy relations limit the degree of *statistical homogeneity* that can be achieved in the *physical selection* of an *ensemble* of identical systems;

URb - the indeterminacy relations limit the *precision* of the *measurement* performed on a *single* system.<sup>68</sup>

According to the dispersion uncertainty relations URa, given an ensemble of particles with sharp momenta (pure case), we are not allowed to select a sub-ensemble that also has a sharp position (what Popper called a super-pure case); the imprecision uncertainty relations URb refer to the simultaneous measurement of position and momentum on a single particle, in which the two measurements hinder each other. What Popper sought to establish can then be articulated as follows:

I - URa follows *logically* from Born’s probabilistic interpretation;

II - URb does not *logically* follow from URa alone;

III - URb follows *logically* from URa only if one adds an *additional hypothesis*;

IV - URa + URb + additional hypothesis turns out to be *contradictory*.<sup>69</sup>

With regard to point I, Popper notes that it has rarely been observed that the mathematical derivability of the Heisenberg formulas from the fundamental equations of quantum mechanics must also be matched by a corresponding derivability of the interpretation of those formulas from the interpretation of the fundamental equations. Some, like Arthur March,<sup>70</sup> regard the statistical dispersions as a mere consequence of measurement inaccuracies; others, like Weyl,<sup>71</sup> seem at least to accept the reverse, namely that the limitations on single measurements follow from the statistical view. Popper rejects both options. Quantum formulas make *frequency predictions* (*Häufigkeitsprognosen*). The latter cannot imply prohibitions on individual measurements—apart from the trivial cases of probability 1 or 0.<sup>72</sup> The uncertainty relations imply that if, say, 1000 position measurements show the electron to be at the same place, then it may be expected that 1000 momentum measurements would yield widely divergent results. A ‘sharp’ measurement of both  $x$  and  $p_x$ —that is, an exceptionally precise outcome

---

<sup>h</sup>However, below I argue that Popper’s notion of ‘physical selection’ is not identical to the modern notion of ‘preparation’.

for both variables—is not impossible but highly improbable. It represents an event analogous to a statistical fluctuation within the ensemble distribution.

In this way Popper addressed point II of his program: from a logical point of view all alleged ‘proofs’ that exact position and momentum *measurements* contradict the formalism of quantum mechanics are flawed. The indeterminacy relations do not *forbid* exact position-momentum measurements; they merely maintain that exact position-momentum *predictions* cannot be *derived* from them.<sup>73</sup> Heisenberg himself could not conceal the tension between these two alternatives: [1] he argues that, since the indeterminacy relations *forbid* selecting an ensemble of systems with exact values of both position and momentum, *predictive measurements* of the future position-momentum trajectory of a particle with arbitrary precision are excluded; [2] he nevertheless concedes, almost in passing, that the indeterminacy relations *allow* arbitrary *non-predictive measurements* of position and momentum, that is reconstructions of past trajectories.<sup>1</sup> These reconstructions can, in principle, be obtained by combining ‘measurements’ of (a) position followed by position, (b) momentum followed by position, or (c) position followed by momentum,<sup>74</sup> with the two operations separated by a finite time interval.

Because predictive position-momentum measurements cannot be derived from quantum mechanics, physicists generally conclude that the very notion of a *future* trajectory has lost physical meaning in quantum mechanics.<sup>75</sup> Yet the question of the physical meaning of the *past* trajectory between two measurements remains ambiguous. Popper could then enter the Heisenberg-Schlick debate presented in section 1.1. As we have seen, (a) Heisenberg dismissed the issue as a ‘question of taste’, and (b) Schlick regarded it as a ‘meaningless question’. However, both ultimately shared the positivist prejudice that, since such reconstructions cannot be *verified*, that is no testable predictions can be derived from them, they belong to the realm of ‘metaphysics’. As one might expect, however, for Popper the question in this form is ill-posed. The point is that if sharp past trajectory reconstructions were not possible, the frequency predictions of quantum mechanics could not be *falsified*, and the theory would therefore belong to the realm of ‘metaphysics’. Without the possibility of reconstructing the past paths of particles, one could not subject the theory to empirical control (*Nachprüfung*). The latter is always, indeed, a comparison between the observed distribution of reconstructed paths and the predicted probability distribution.<sup>76</sup>

### 3.2 The Coupling-Hypothesis

As one can see, Popper articulated an early version of what might be called an ‘objective statistical interpretation of quantum mechanics’,<sup>77</sup> in which the collapse of the wave function is reinterpreted as the mere selection of a sub-ensemble. When a momentum measurement is performed, one in fact selects an ensemble of systems that have the same sharply defined momentum.<sup>78</sup> In this view, the indeterminacy relations imply that one cannot further select a sub-ensemble whose members also possess the same precisely defined position. By June 1934, Popper had managed to convince Weisskopf that the indeterminacy relations *forbid* physical selections of an ensemble more homogeneous than a pure case, but still *allow* arbitrarily precise measurements on single systems (see above section 2). However, Weisskopf may have prompted Popper to take a further step. As we have seen, Weisskopf argued that if an apparatus capable of *measuring* the

---

<sup>1</sup>See the example discussed in section 1.1.

position and momentum of a single particle with arbitrary sharpness were possible, then the same device could also be used to *produce* ensembles of particles whose position and momentum spread would be smaller than that allowed by the indeterminacy relations. In this sense, the indeterminacy relations, as a prohibition of a super-pure case, also imply the prohibition of arbitrarily sharp predictive measurements.

In *Logik der Forschung*, Popper attributes this objection to an unnamed physicist—without any doubt Weisskopf. Indeed, Popper<sup>79</sup> reproduces several passages from Weisskopf's letter almost verbatim, though without explicitly naming him as the source, as the latter had requested. Popper now offers a more articulated reply to Weisskopf's objection. According to Popper, Weisskopf's argument does not *prove* that sharp predictions contradict quantum mechanics. Rather, it tacitly introduces an *additional hypothesis* mentioned in point III, which Popper calls the *coupling-hypothesis* (*Kopplungshypothese*). The latter maintains that predictive measurements and physical selections are inevitably linked, in the sense that any predictive measurement also constitutes a physical selection—or, operationally speaking, that any instrument capable of measuring position and momentum with arbitrary precision could, at least in principle, also be used to select a corresponding super-pure case:

But a strict proof of the contention [...] has not been given [...]. None of these arguments prove that the precise predictions would contradict the quantum theory. They all introduce an *additional hypothesis*. The statement (which corresponds to Heisenberg's view) that exact single predictions are impossible turns out to be equivalent to the hypothesis that predictive measurements and physical selections are inseparably coupled. With this new theoretical system—the conjunction of quantum theory with this auxiliary *coupling-hypothesis*—my conception must indeed clash. What remains to be shown is that the system, consisting of statistically interpreted quantum mechanics (together with the laws of momentum and energy conservation), combined with the coupling hypothesis, is contradictory.<sup>80</sup>

Popper had now realized that the most deep-seated prejudice of quantum physicists lay in the unexamined belief that physical selections and measurements are necessarily coupled. With this, he considered point III of his program accomplished. What remained to be addressed was point IV: to demonstrate that the system—statistically interpreted quantum mechanics, together with the conservation laws of momentum and energy, when combined with the coupling hypothesis—leads to a contradiction. Popper deserves credit for having recognized with remarkable clarity the crucial point on which his whole critique of quantum mechanics rests—and upon which it also founders.

#### 4 Popper's ETP-like Thought Experiment

The coupling-hypothesis ultimately takes the form of a *non-existence statement*.<sup>81</sup> no device exists that allows for a predictive measurement of the values of both  $x$  and  $p_x$  without presupposing a physical selection. Consequently, to disprove the coupling-hypothesis, Popper needed to demonstrate that such a device is possible, that is, that it does not contradict quantum mechanics when combined with the known conservation laws. In particular, he aimed to show that a device could be constructed which, on the basis of a *non-predictive measurement* (the reconstruction of a particle's past path), would, by exploiting a conservation law, yield an arbitrarily sharp *predictive measurement* (the prediction of its future path) without presupposing a prior physical selection. This device would then violate the single-measurement

uncertainty relations while still complying with the statistical-ensemble uncertainty relations thereby showing that the latter does not imply the former.<sup>82</sup>

#### 4.1 The Experimental Setup

As mentioned in section 3.1, there are three possible combinations of ‘measurements’ allowed for the combination of particle’s trajectory: (a) position followed by position, (b) momentum followed by position, (c) position followed by momentum. The case singled out by Popper is (b). Although Popper speaks rather ambiguously here of momentum and position ‘measurements’, as we shall see, starting from Appendix VI he emphasizes that one should in fact speak of a momentum ‘selection’ followed by a position ‘measurement’. The choice of (b) is not accidental. Indeed, in Popper’s reading, this is “just that case [b] which, *according to Heisenberg* [...], permits ‘a calculation about the past of the electron’.”<sup>83</sup> Indeed, as we have discussed above in section 1.1, Heisenberg<sup>84</sup> uses precisely a sequence (b) of a momentum measurement via the Doppler effect followed by a position measurement to argue for the possibility of a ‘calculation of the past trajectory’ of the electron upstream of the second position measurement. Popper appears to read this claim as implying that (b) allows one to calculate the past trajectory also *before* the first momentum measurement, whereas (a) and (c) allow such a calculation only in the stretch of the trajectory *between* the two measurements.<sup>i</sup> As will become clear, this is the key but shaky premise on which Popper’s argument hinges. On this basis, Popper proposed a thought experiment involving an electron-light quantum collision. After the collision, by performing a *non-predictive* measurement on the electron (momentum followed by position), and using momentum conservation, one could in principle determine an arbitrarily sharp *predictive* measurement of the position and momentum of the quantum of radiation. As one can see, Popper independently arrived at a thought experiment analogous to the ETP scenario (see section 1.2 above), except that energy and time are replaced by position and momentum. Of course, Popper’s goal was the opposite of ETP’s: to show that the asymmetry between past and future does indeed allow one to overcome the indeterminacy relations when interpreted as ‘inaccuracy relations’.

The idealized experimental setup (figure 3) consists of two beams intersecting at  $S$  (*Schnittpunkt*). Each beam is treated as if it were a ‘pure case’. The electron beam  $A$  is monochromatic, carrying a sharp known momentum  $\mathbf{a}_1$ , while the light quantum beam  $B$  is slit-selected, with a sharp position at  $Bl$ , and thus momentum of unsharp direction but known magnitude  $\mathbf{b}_1$ . One can then perform a *post hoc* mental selection focusing on the partial rays  $[A]$  and  $[B]$  that collide at  $S$ , in a manner similar to the Compton-Simon and Bothe-Geiger experiments. An electron from beam  $A$  emerges at detector  $X$  with momentum  $\mathbf{a}_2$ , while a light quantum from beam  $B$  emerges at detector  $Y$  with momentum  $\mathbf{b}_2$ . The process is governed by momentum conservation, such that  $\mathbf{a}_1 + \mathbf{b}_1 = \mathbf{a}_2 + \mathbf{b}_2$ . Since the light quantum has a broad momentum-direction distribution, in some runs of the experiment the electron ‘keeps’ its original momentum, while in others it ‘inherits’ the light quantum’s momentum. If one considers only the electrons, disregarding the light quanta with which they interacted, the electron beam is a non-pure case after the collision.

Popper suggests placing an apparatus at  $X$  to measure the momentum  $\mathbf{a}_2$  of the electron after the collision and its position at the time  $t_A$ . In the Note for *Die*

---

<sup>i</sup>In my view, there is little evidence to support this interpretation. Heisenberg’s remark is simply somewhat careless.

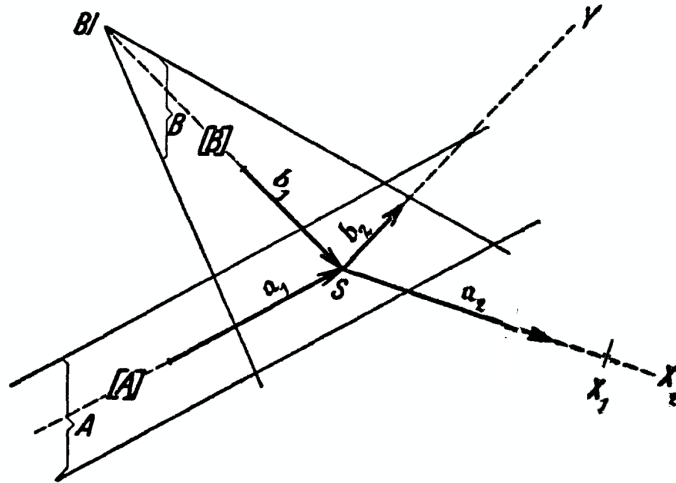


Fig. 3. Popper's experimental setup. Adapted from Karl R. Popper, *Logik der Forschung*. Berlin: Springer, 1935, 179

*Naturwissenschaften*, as well as in the main body of *Logik der Forschung*, he proposes using a moving photographic film (*Filmanstreifen*). When a particle strikes, it leaves a mark at a specific spatial coordinate (position) on the strip, and because the strip is moving, this also indicates a time coordinate (moment of impact). In this setup, the photographic film is also supposed to measure the energy of the electron indirectly through the energy transferred to the atoms of its sensitive material.<sup>j</sup> From the energy one can calculate the electron's momentum  $\mathbf{a}_2$  in the  $SX$  direction. Popper contends that “[t]he accuracy of this calculation is, for the  $SX$  direction (with a suitable arrangement), subject to no fundamental limitation of the kind implied by the uncertainty relations. It is assumed here that the magnitude of momentum and the time of an incoming particle—‘non-predictive’ measurements—can be measured with arbitrary precision.”<sup>85</sup>

Popper does not discuss this premise at this stage; he only argues that, *if* this is the case, from  $t_A$  and the momentum  $\mathbf{a}_2$  the time of collision  $t_S$  can be reconstructed. Via momentum conservation, one can infer the light quantum's momentum after the collision  $\mathbf{b}_2$  from the electron's momentum,  $\mathbf{b}_2 = (\mathbf{a}_1 + \mathbf{b}_1) - \mathbf{a}_2$ . Consequently, from the time of the collision and the light quantum's momentum  $\mathbf{b}_2$ , one can make a predictive measurement sharper than allowed by the indeterminacy relations: the light quantum from beam  $B$  will reach  $Y$  at time  $t_B$  with momentum  $\mathbf{b}_2$ . This prediction can, in principle, be tested empirically. Reconstructing the *past of an electron* via an arbitrarily sharp non-predictive measurement together with a conservation law appears to allow an arbitrarily predictive measurement of the *future of a light quantum*.

Popper clarifies that the apparatus “does *not* permit the assignment of a definite position and a definite momentum arbitrarily to *particular* particles.”<sup>86</sup> One can infer something about  $B$  once one measures  $A$ , but one cannot control or reproduce that same condition on  $B$  in the next trial without physically selecting  $A$  again. Repeating the experiment many times would yield a statistical distribution of  $AB$  particle pairs, all with *different* but correlated positions and momenta, not an ensemble of  $B$  particles all having the *same* position and momentum: “The experiment therefore provides no indication for the production of a collection of particles that is more homogeneous

<sup>j</sup>See next section for a criticism of this setup.

than a *pure case*.”<sup>87</sup> Popper can then conclude that the thought experiment shows that one can determine a predictive measurement of the position and momentum of a single  $B$ -particle without allowing for the selection of an ensemble of  $B$ -particles having both sharp momentum and sharp position.<sup>k</sup> While fully complying with Heisenberg’s inequalities eq. (1) understood as statistical dispersion relations (*Streuungsrelationen*), Popper believed that one could nevertheless produce a case that violates Heisenberg’s inequalities understood as limits of measurement accuracy (*Ungenauigkeitsrelationen*).

#### 4.2 The Issue of the First Momentum Measurement

According to Popper, the possibility of making an arbitrarily sharp predictive measurement should suggest that each particle in the ensemble possesses a *pre-assigned, yet unknown* momentum and position, thus opening the possibility that deterministic ‘precision laws’ might underlie the quantum ‘frequency laws’.<sup>89</sup> As already noted, Popper was aware that the device underlying the thought experiment works only if the momentum of  $A$  remains unaffected by the selection at  $X$ , allowing the entire path of  $A$  to be reconstructed up to the collision at  $S$ . Popper was probably initially confident that this was the case, relying on Heisenberg’s somewhat elliptical remark<sup>90</sup> (see section 1 above). An unpublished *Ergänzung* to the Note, written sometime between summer and fall 1934, shows that Popper soon realized a legitimate objection could be raised against this assumption: “One could, namely, take the following view: Non-predictive measurements allow us to make exact calculations only for certain intervals, namely for the interval *between* two measurements. For the time *before* the first or *after* the second measurement, however, the uncertainty relation holds, whose validity is therefore (contrary to Heisenberg’s remark) symmetrical for past and future.”<sup>91</sup> It may be recalled (section 1.2) that this was precisely the upshot of the ETP paper, which Popper, without any doubt, did not know of. Yet Popper sought to reach the opposite conclusion. He therefore had to offer at least a clarification concerning the ‘measurement’ of momentum. In particular, he had to justify the assumption that the path of the  $A$ -particles could be reconstructed even *before* the first momentum measurement, up to  $S$ . This discussion was later incorporated into Appendix VI. This proves to be the true Achilles’ heel of Popper’s setup. As mentioned, Popper first proposed placing the film strip at  $X$  to measure not only position and time but also energy, and hence momentum, of the incoming particle. He soon realized<sup>1</sup> that such an apparatus would violate the indeterminacy relations.<sup>92</sup> In the *Ergänzung*, Popper therefore outlined a revised setup, later included in Appendix VI of *Logik der Forschung*.

It can be described as follows (figure 3). (i) At  $X_1$ , an electrostatic analyzer with parallel plates producing a uniform electric field  $E$  perpendicular to the beam direction (see figure 4 below) filters a sub-ensemble of electrons with the same *predetermined* momentum  $\mathfrak{a}_2$  *without changing it*.<sup>93</sup> Since, for Popper, filtering momentum leaves both momentum and position unchanged, though the latter remains unknown. (ii) The unknown position may later be revealed by a second measurement at  $X_2$ , where a Geiger counter (or clock-driven moving film strip) records position  $x$  and arrival

<sup>k</sup>According to Readhead, Popper is “confusing” conditional and predictive selection (ref. 88). I would rather say that Popper, more or less consciously, is *exploiting* this very difference to infer  $\mathfrak{b}_2$ , given  $\mathfrak{a}_2$ , without physically selecting a new ensemble of  $B$ -particles. Popper should perhaps have used the label  $[\mathfrak{b}_2]$  to emphasize that this is only an epistemic selection and not a physical one.

<sup>1</sup>See also Einstein’s objection, section 6.1.

time  $t$ . As the first measurement leaves the electron’s state unchanged, one might think it possible to reconstruct the past of the electron not only *between* the two measurements—momentum followed by position—but also *before* the first measurement at  $X_1$ .<sup>m</sup>

Thus, Popper regards it as essential that the sequence of operations required for a proper path reconstruction should not be a momentum measurement followed by a position measurement, but rather a momentum *selection* followed by a position measurement. Popper realized that two possible objections can be raised against the possibility of a momentum selection as described:

- *The filter leaves momentum unchanged but alters position.* Popper argues that, unlike a position selection (a narrow slit), which spreads the trajectory, a momentum selection (an electron spectrometer) allows particles of a certain momentum to pass along the same path while blocking others. Assuming that momentum selection unpredictably disturbs position would imply a discontinuous (superluminal) jump to another point, contradicting quantum mechanics, which permits discontinuities only for bound, not free, particles. Popper concludes that, according to the statistical interpretation of the indeterminacy relations, momentum selection leaves positions unknown but *unchanged*.<sup>95</sup>
- *If the filter left momentum unchanged, one could produce a ‘super-pure case’.* By reversing the order of selection—first localizing the particle through a narrow slit or short ‘momentary shutter’ (*Momentverschluss*), then selecting momentum with a filter<sup>n</sup>—one might expect, after many runs, to obtain an ensemble with sharp  $x$  and  $p_x$ . Popper replies that, unlike momentum selection, position selection spreads momentum: the sharper the position, the greater the diffraction and the fewer particles pass the filter. Only rare detections occur, revealing merely the *statistical distribution* of position and momentum in an ‘anonymous’ ensemble, not sharp individual trajectories.<sup>96</sup>

Popper’s reasoning rests on an alleged asymmetry between position and momentum selection that, as will be shown below ([Conclusion](#)), is only apparent. What is relevant here is that Popper clearly sees that the question of the momentum ‘measurement’ decides the destiny of his thought experiment: “[a] Assuming that the  $x$ -coordinates of the particles are not disturbed by the momentum measurement [*Impulsmessung*], then the exact determination of position and momentum also extends to the time *before* the momentum selection [at  $X_1$ ]. [b] Assuming that the momentum selection [*Impulsaussonderung*] disturbs the  $x$ -coordinates, then we can calculate the trajectory exactly only for the time *between* the two measurements [at  $X_1$  and  $X_2$ ].”<sup>97</sup> As we shall see, Popper was compelled to respond to several interlocutors—some of the greatest physicists of all time—who challenged the legitimacy of [a] and argued that [b] was the case. He eventually gave it up, but not without a fight.

## 5 The Leipzig Response

*Logik der Forschung* was completed and sent to the publisher in the fall, and printed at the end of December 1934 (despite the colophon bearing the date 1935). In the

---

<sup>m</sup>Popper does not explicitly distinguish between  $X_1$  and  $X_2$ , but I follow Redhead’s useful suggestion (ref. 94).

<sup>n</sup>I mention this detail since Einstein referred to this apparatus; see below section 6.3.

intervening months, Berliner, the editor of the *Naturwissenschaften*, had sent Heisenberg the galley proofs of the short Note that Popper had submitted in August. Heisenberg wrote to Popper on November 23, explaining that, at Berliner's request, he discussed the Note with his assistant von Weizsäcker. They subsequently sent Berliner von Weizsäcker's brief rejoinder, asking him to forward it to Popper: "You will then see what kind of criticism it is. Essentially, it concerns the concept of the 'non-predictive measurement,' which in our view appears to be misunderstood in your work."<sup>98</sup> Let us not forget that Popper was virtually unknown at the time, and it is quite remarkable that Heisenberg (newly appointed to a chair at Leipzig) and his group chose to engage in a detailed refutation of various variants of his thought experiment rather than dismiss him as a crank.

### 5.1 Von Weizsäcker's Objection and Popper's Reply

In his brief response, von Weizsäcker, like Heisenberg, suggested using a scattering method (the Doppler effect) to measure the electron's momentum at  $X_1$ , followed by a Geiger counter and clockwork to record its position and time at  $X_2$ . With this setup, von Weizsäcker reached precisely the conclusion Popper sought to avoid: the electron 'trajectory'  $A$  can be reconstructed only for the interval *between*  $X_1$  (momentum determination) and  $X_2$  (position and time recording), but not *before*  $X_1$ . He conceded that one may know the momentum before and after the measurement exactly; however, measuring momentum via the Doppler effect requires determining the frequency ( $\Delta\nu$ ) of the scattered radiation over a finite observation time ( $\Delta t$ ); improving one worsens the other. Thus, one cannot know when the impact between the apparatus and the particle occurred, or for how long it had the 'momentum before the impact' or 'after the impact'. This uncertainty blurs the knowledge of position  $X$  before the impact; hence, the collision time at  $S$  remains indeterminate, and the trajectory [B] cannot be predicted. Von Weizsäcker concluded:

This trajectory, however, is *in principle* uncontrollable. It is valid only for the time interval between the end of the momentum measurement and the beginning of the position measurement, during which the particle has no interaction whatsoever with its surroundings, and it cannot be extended into the period before the momentum measurement, since the latter itself destroys the knowledge of the position in accordance with the uncertainty relation. [...] Thus, even if the position [at  $X_2$ ] after the momentum measurement is exactly known, one still cannot infer the position *before* the momentum measurement [at  $X_1$ ] with an accuracy greater than the error  $h/4\pi\Delta p$ . That is, our knowledge of the trajectory of particle  $A$  before the momentum measurement at  $X$ , and thus also of the trajectory of particle  $B$  after the collision at  $S$ , is in accordance with the uncertainty relation.<sup>99</sup>

Von Weizsäcker concedes that the proof applies only to the *specific* experimental setup he described. However, he had no reason to doubt that *any* other combination of measuring devices at  $X$  would yield the same result. The problem is that Popper does not distinguish between 'non-predictive measurements' and verifiable measurements concerning the past, such as records left by irreversible physical processes. The former escape the indeterminacy relations because they are not physical measurements at all, insofar as they cannot be used as initial conditions for existing dynamical laws to make testable predictions. The latter, by contrast, do satisfy the indeterminacy relations, which von Weizsäcker regarded as symmetric for the past and the future.<sup>100</sup>

On November 26, Popper replied by sending the galley proofs of Appendices VI and VII of the *Logik* to Heisenberg.<sup>101</sup> Popper, somewhat prone to paranoia, felt

that von Weizsäcker had not taken him seriously and reacted rather aggressively, emphasizing two points: 1. in the Note he did not *prove* the legitimacy of calculating the past of the electron, but merely *assumed* it, citing Heisenberg’s own remark on p. 15 of his book as justification; 2. von Weizsäcker had refuted only one method of *momentum measurement*—that by scattering. Popper maintained instead that *momentum selection* of an aggregate of electrons (*Teilchenmenge*) by means of filters could determine momentum  $p_x$  *without* disturbing the  $x$ -coordinate, thus allowing for a non-predictive retrodiction before  $X_1$ . Popper wrote to Heisenberg:

You will now ask why I did not elaborate further in the note on what I am writing here. The reason is that I assumed that the so-called ‘non-predictive measurement’ (momentum selection followed by a position measurement) and the possibility of calculating the ‘past’ prior to the momentum measurement were already known. This assumption was based on a remark of yours (*Phys. Prinzipien*, p. 15) concerning case (b) and the ‘calculation of the past of the electron’. In my note, however, I had to be brief and restrict myself to what was essential, that is, above all, to what was new. Only from Mr. Weizsäcker’s note did I realize that at least he was not familiar with this case.<sup>102</sup>

Since he mentions p. 15 of the German edition of Heisenberg’s 1930 book, Popper probably refers to Heisenberg’s claim that the ‘calculation of the past of the electron’ can be reconstructed *before the position measurement*—without specifying whether it can also be reconstructed before the momentum selection. At any rate, the time was too short for Popper to prepare a rejoinder. On November 30, 1934, the Note appeared in volume 22 of *Die Naturwissenschaften*, together with von Weizsäcker’s critical comment.

On October 6, Popper sent Heisenberg a second communication<sup>103</sup> in response to von Weizsäcker, which he hoped to publish. Popper also attached a copy of the *Logik der Forschung* that had been just printed but still not distributed. Popper’s counterargument is revealing of his way of thinking at that time.<sup>o</sup> He observes that von Weizsäcker had used momentum *measurement* at  $X_1$  via the Doppler effect, that is, by using light quantum collision. The latter, however, has the drawback that it “disturbs the particle’s momentum (recoil),” so that “the exact instant of the disturbance is not known”<sup>104</sup> Popper concedes that no reconstruction of the momentum before the scattering event is possible in this way.<sup>p</sup> However, following the same argument he had developed in Appendix VI, Popper suggests that one can resort to “a momentum *selection* (electron spectrometer, light filter),” which “unlike the momentum measurement of individual particles, for example by means of the Doppler effect—has the property of not affecting the momenta or momentum components of the selected particles.”<sup>105</sup> Thus, if  $[A]$  is an electron beam, “only electrons with a certain *predetermined* amount of momentum enter the Geiger counter”, and if  $[A]$  is a light beam, the filter<sup>q</sup> lets through only those light quanta that *already* possessed the required momentum before entering it.<sup>106</sup>

---

<sup>o</sup>As discussed below (see [Conclusion](#)), Popper’s reconstruction of the event twenty years later seems to me quite different.

<sup>p</sup>See fn. y.

<sup>q</sup>This is one instance in which the difference between Popper’s notion of ‘selection’ and Margenau’s notion of ‘preparation’ comes to the fore with particular clarity. Both momentum measurement via the Doppler effect and momentum selection via a velocity filter count as preparations in Margenau’s sense: both involve a ‘disturbance’ of the system insofar as they prepare it in a state with sharply defined momentum and hence with a correspondingly spread position. This already undermines the foundations of Popper’s imaginary experiment entirely.

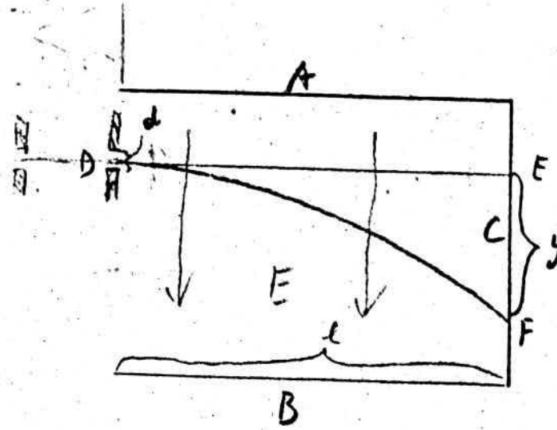


Fig. 4. From the letter by von Weizsäcker to Popper, Dec. 6, 1934; *Karl Popper Archives* (henceforth cited as KPA), 360-21

One might object, Popper points out, that we do not really know in detail what a light filter does, and that its operation might itself disturb the light quantum's state. However, Popper argues, a filter produces not only light but also sharp images (*Bilder*)—thus, it does not, in any case, disturb the directions of the quanta. If the filter did disturb the light quanta, one would have to assume some mechanism that restores their direction after passage: either (1) the quanta pass through the filter but are unpredictably displaced along their paths, with their previous direction suddenly reestablished (*Zurückversetzung auf der Bahn*), or (2) the filtering process involves absorption and directed re-emission (*gerichtete Emission*) in the same direction.<sup>107</sup> Yet both options appear problematic: the first contradicts the continuity of paths still required by quantum theory; the second assumes a kind of 'directed fluorescence'<sup>r</sup> not known to occur.<sup>108</sup> Popper therefore concluded that the most plausible hypothesis is that, also in the case of the filter, the quanta with the 'right' momentum are allowed to pass through unchanged.

### 5.2 Von Weizsäcker's and Heisenberg's Counter-Replies

On December 6, the same day as Popper's letter, von Weizsäcker drafted a detailed response,<sup>109</sup> which Heisenberg forwarded to Popper on December 10. Von Weizsäcker considered the setup suggested by Popper in Appendix VI, an electrostatic analyzer (see figure 4), and showed that the very geometry of the deflection experiment already contains the limits expressed by the uncertainty principle. To determine the momentum of the particle from its impact point on the screen  $EF$ , the beam must pass through diaphragms that fix its path length precisely. But narrowing the slit  $D$  to improve accuracy inevitably produces diffraction, so that the transverse spread  $\Delta y = EF$  of the beam at the detector cannot be made smaller than a definite bound. In other words, the position on the screen, which is supposed to measure momentum, is blurred by the same diffraction effects introduced by the diaphragms.<sup>110</sup>

Since the vertical displacement  $y$  depends on the flight time  $t = L/v$ , this transverse uncertainty also propagates into the longitudinal motion. An uncertainty in  $y$  translates into an uncertainty in the velocity  $v$ , and thus into both the longitudinal momentum  $p_x$

<sup>r</sup>'Fluorescence' is the emission of electromagnetic radiation by atoms or molecules after absorbing radiation, occurring when they return to a lower energy state. The emission is generally spontaneous and isotropic, not directed.

and the coordinate  $x = vt$ . When these effects are consistently taken into account, one finds that the product  $\Delta p_x \Delta x$  is likewise bounded below by Planck's constant. Figure 4 illustrates the intuition: the more tightly the beam is collimated at  $D$ , the more it spreads out when it reaches the screen at  $E-F$ , so that no arrangement of diaphragms or fields can evade the uncertainty relation. The situation was not dissimilar to the one described by Heisenberg<sup>111</sup> in his book, where he used a homogeneous magnetic field instead of an electric field to measure the momentum.<sup>112</sup>

In his reply of December 10, Heisenberg enclosed von Weizsäcker's criticism of the electrostatic analyzer and also addressed Popper's suggestion of using a light filter instead of an electron spectrometer.<sup>113</sup> Heisenberg suggested that one might use reflective resonance filters, such as sodium or mercury vapor, following Wood's classical residual-ray experiments (*Reststrahlenverfahren*). When light strikes such a medium, radiation at the resonance frequencies is strongly absorbed and re-emitted (or reflected), while other frequencies pass through unaffected. Thus, a broad-spectrum beam incident on sodium vapor is filtered and yields only the reflected sodium D-lines—a doublet of closely spaced spectral lines in the yellow region of the visible spectrum. Heisenberg invoked Wood's experiment to show that the filtering process always involves finite line widths, not infinitely sharp spectra of a single frequency  $\nu$ . When an atom absorbs a light quantum at its resonance frequency, it is excited to a higher state and, after a short lifetime  $t$ , decays by re-emitting a light quantum at the same frequency. Because the excited state exists only for a finite time, its energy cannot be perfectly sharp, in compliance with the time-energy uncertainty relation.

Even when light is 'filtered' into sharp momentum (frequency) states, the finite lifetime of the excited atoms ensures a residual spread  $\Delta\nu$  (and hence  $\Delta p$ ). This constitutes a momentum selection, since light quantum momentum is  $p = h/\lambda$ , but its precision is limited by the finite resonance linewidth, determined by the lifetime of the excited state,  $\Delta t = 1/\Delta\nu$ . Thus, light quanta cannot be prepared with arbitrarily sharp momentum. Even if the light quantum's later position is known precisely, its position before reflection cannot be reconstructed, because the time spent in the reflecting atom is indeterminate. Hence there is an uncertainty  $\Delta x$  such that  $\Delta x \cdot \Delta p \approx \frac{h}{4\pi}$ . The filter selects not an exact frequency, but a narrow band, whose spread is intrinsic to quantum mechanics through the lifetime-linewidth relation.

In his reply of December 16, Popper sent Heisenberg a revised version of the second communication summarizing the debate's main points.<sup>114</sup> He conceded that von Weizsäcker's argument against the electrostatic selector was correct but defended his idea of using a light quantum filter against Heisenberg's objections. He noted that Heisenberg's remarks on Wood's experiment concerned only absorbed and re-emitted light quanta at the resonance line, whereas "the light of other frequencies *passes through*, and it is only to this transmitted light that my considerations apply."<sup>115</sup> Only by assuming that transmitted light also underwent absorption and re-emission would the thought experiment lose validity. He further dismissed the claim that the transmitted light was too inhomogeneous: "It is in fact possible, by means of a set of filters, or even with a green filter alone, to isolate a finite range of transmitted frequencies  $\Delta\nu$ , i.e. a finite  $\Delta p$  interval."<sup>116</sup> Once the light quantum had passed through the filter, its position could later be measured with arbitrary accuracy  $\Delta x \rightarrow 0$ , so that "the *product* of the two uncertainties could approach zero, even for imperfect filters."<sup>117</sup>

Popper did not relent. He proposed a new idea for an apparatus, which Heisenberg left to his assistant Hans Euler to examine.<sup>118</sup> On February 4, 1935, Euler sent Popper

a detailed refutation. On February 12, Popper drafted a somewhat impatient response that was never sent, and another on April 14. I will not go into this exchange, since Popper’s original proposal has been lost, and he seems to have later abandoned the idea. At this point, however, the Leipzig group was starting to become mildly annoyed by Popper’s insistence. In a mid-March letter to Grete Hermann, von Weizsäcker, at her request for clarification,<sup>s</sup> reconstructs the entire series of experimental conjectures and refutations, concluding somewhat humorously that it has become “a parlor game for us in Leipzig, to refute Popper’s setups.”<sup>121</sup> It was Heisenberg who ultimately decided that it was time to bring the game to an end a few days later.

In his final letter to Popper, on March 19, Heisenberg explained that all the considerations of his previous letter apply to transmitted light as well.<sup>122</sup> He considered a substance opaque over nearly the entire spectrum due to strong absorption bands, yet transparent within a narrow region between two band heads (*Bandköpfe*). Light quanta with frequencies inside this window pass quickly, while those near the band heads are absorbed and re-emitted after a delay comparable to the lifetime of the excited states. As the interval between the band heads narrows, more quanta experience delayed re-emission, lengthening and blurring the passage time. In the limit of an infinitesimally narrow band, the mean delay grows without bound, consistent with the ‘principle of harmonic resolving power’ or Fourier relation  $\Delta\nu\Delta t$  or  $\Delta x\Delta k$ : a sharp frequency (or momentum) definition entails an indeterminacy in time (or position).<sup>123</sup> At this stage, Popper might have sought support from Weisskopf. Yet the latter confessed that he had met with Heisenberg and had come to side with him and von Weizsäcker.<sup>124</sup> The game had run its course—but Popper still had one card up his sleeve.

## 6 The Popper-Einstein Correspondence

On March 25, 1935, Einstein, Podolsky, and Rosen submitted the eponymous EPR paper to the *Physical Review*; it was published on May. In the intervening weeks, Popper managed to get a copy of *Logik der Forschung* to Einstein. Popper’s friend, the pianist Rudolf Serkin, performed with the Busch Quartet and had recently married the daughter of Frieda and Adolf Busch, who knew Einstein. Frieda sent him a copy of Popper’s book on April 28, 1935.<sup>125</sup> As one can see, Einstein received Popper’s work after the EPR paper had already been submitted. Jammer’s<sup>126</sup> hypothesis that Popper’s paper might have served as an intermediary between the ETP and the EPR paper is intriguing. As mentioned above, Popper’s experiment is essentially a version of the ETP experiment in which energy and time are replaced by momentum and position, as in the EPR argument. It therefore seems natural to suggest that he may have served as a bridge between ETP and EPR. However, this hypothesis is ultimately unlikely for chronological reasons.<sup>t</sup> However, it is plausible that Einstein immediately recognized that Popper’s experimental setup was essentially a variant of his own ETP scheme and thus immediately dismissed it.

### 6.1 Einstein’s Comment on the *Logik der Forschung*

Einstein replied to Popper on June 15, endorsing his general philosophy of science but criticizing the proposed thought experiment as flawed: “It is not correct that position

---

<sup>s</sup>The correspondence between von Weizsäcker and Hermann, and Hermann’s remarks ((ref. 119) and (ref. 120)) on Popper’s interpretation of quantum mechanics would require a separate investigation.

<sup>t</sup>I could not find any indication that Popper had already sent an offprint of the 1934 Note to

and momentum of the particle at  $Y$  (p. 179) can be predicted. To this end you would have to measure with the help of your ‘film’ time and momentum of the particle [at  $X$ ] (that is, time and energy) which is impossible. You will see this easily if you think a little more about it.”<sup>128</sup> The objection is probably the consequence of Popper’s unfortunate choice of measuring apparatus on p. 179 of *Logik der Forschung*, a ‘film’ at  $X$  measuring momentum and energy, and position and time of arrival.

However, as discussed above (section 4.2), in Appendix VI of *Logik der Forschung* Popper had already proposed a different two-stage arrangement, a momentum filter at  $X_1$  (electron spectrometer or light filter) and a position-time measuring apparatus (a film) at  $X_2$ . Still, in his lengthy reply on July 18, he had by that time already accepted that even this setup could not work:

First, the question of the thought experiment (in the book p. 179). Unfortunately, regarding the measurement of the particle arriving at  $X$ —as you rightly noted—there are quite a few inaccuracies in the text of the book (for example, the use of the electric field and practically the whole Appendix VI). I admit my mistakes, but after having discussed them thoroughly with all the physicists available to me in Vienna, among others with Professor Thirring and his assistant Guth (with Professor Heisenberg and his assistant Weizsäcker I discussed the matter in writing), it still seems to me that the issue is not yet settled. From the discussions so far (and in this view I am not alone, but, among others, also Guth), the impossibility of my thought experiment has not yet been demonstrated. That is, it seems possible to avoid the aforementioned errors and to maintain the thought experiment in agreement with quantum mechanics. I do not wish to be ‘proven right’; I would only be very glad if this matter could be clarified—even if the final decision goes against me. I hope I am not abusing your time and patience too much by presenting the situation as it currently appears to me.<sup>129</sup>

Popper tried to investigate a different arrangement, in which a light filter is placed at  $X_1$ . He acknowledged that this setup—the optical version of the one described in the book—would not yield the desired result by using known filters (as Heisenberg showed him). However, Popper suspected that a combination of filters might exist that selected photons of a sharply defined momentum (frequency) without having them interact for a long time.

Popper went into some detail (see next Section) but, not wanting to make an already long eight-page letter even longer, he promised to send Einstein a supplement he had written two months earlier at Felix Ehrenhaft’s suggestion.<sup>130</sup> Throughout the spring of 1935, Popper had worked on a published response to the Leipzig objections, expanding it into a longer manuscript again titled ‘Zur Kritik der Ungenauigkeitsrelationen’. Apparently he sent it to Carnap, Ehrenhaft, Heisenberg, and Schrödinger, hoping Ehrenhaft would forward it to Einstein.<sup>131</sup> On July 18, he decided to send it himself. Popper admitted he was beset by doubts but, after further discussion with Weisskopf, realized the latter could not fully refute his argument. Having already announced the manuscript, he felt compelled to send it despite lingering uncertainties, having given up hope of a prompt clarification. Moreover, Weisskopf had informed him of Einstein’s recent critique of quantum mechanics (the EPR paper), which might have nurtured his hopes of finding in Einstein a more receptive audience.<sup>132</sup>

## 6.2 Popper’s 1935 Manuscript

The first part of Popper’s 1935 manuscript restates his stance toward the uncertainty relations with additional reference to some literature that he had not cited in the

book, such as von Neumann’s 1932<sup>133</sup> textbook. Thus, for example, Popper began to use the expression ‘dispersion-free ensemble’ to indicate what he had previously called a ‘super-pure case’.<sup>134</sup> However, the main message remains the same: the prohibition of dispersion-free ensembles does not imply the prohibition of arbitrarily sharp measurements. Popper presupposes that the reader already knows his November 1934 Note and once again clearly identified the issue with his original setup:

One would have to apply a ‘non-predictive measurement’ according to case (b)—a position measurement [at  $X_2$ ] preceded by a momentum measurement at [at  $X_1$ ]—arranged in such a way that one can calculate the trajectory not only in the interval *between* the two measurements, but also for the time *before* the momentum measurement. The position coordinate is measured precisely by the peak counter (the moment of incidence [at  $X_2$ ]). *Question: Is there a method of momentum measurement [at  $X_1$ ] that could precede this position measurement without destroying (blurring) the position coordinate of the particle?* Weizsäcker disputes this<sup>u</sup>; the case he discusses (momentum measurement of the particle using the Doppler effect) is indeed unsuitable. In my book I propose the following arrangement: 1. Momentum selection by means of a grating filter, 2. Position measurement by means of the counter. There I defend the thesis that such a momentum measurement, carried out with a filter, should not disturb the position coordinates; this thesis should here be examined more closely.<sup>135</sup>

In other terms, Popper believes he could devise a filter that would let light quanta with unchanged momentum—and therefore position—pass through. His reasoning seems to have been roughly as follows.

A *real* filter must satisfy the Fourier constraint: the narrower the transmitted range of frequencies  $\Delta\nu$  or wavelengths  $\Delta\lambda$ —that is, the sharper the momentum selection—the longer the temporal response  $\Delta t$  and the greater the spatial extension  $\Delta x$  of the transmitted wave packet. However, Popper argues that one could in principle conceive of two different kinds of *ideal* filters, corresponding to the limiting cases of the Fourier relation:<sup>136</sup>

- *Type I apparatus:* Filters capable of spectrally resolving arbitrarily short light flashes ( $\Delta t \rightarrow 0, \Delta\nu \rightarrow \infty$ ). Such filters must necessarily operate *with spatial blurring*, since decomposing a very short pulse without spreading its position  $\Delta x$  would contradict the Fourier relation.
- *Type II apparatus:* Filters that cannot resolve very short wave packets but act only on infinitely long wave trains ( $\Delta\nu \rightarrow 0, \Delta t \rightarrow \infty$ ). In principle, such filters could operate *without spatial blurring*, transmitting only radiation of a single wavelength (or momentum).

To construct a filter of type II, Popper suggests using colored glass filters followed by gas layers.<sup>137</sup> The glass filters, say a green filter, are passive filters that work by transmission: they allow a relatively broad band of wavelengths centered around some mean wavelength to pass while blocking both the high-frequency and low-frequency edges. Gas layers act actively, through absorption and delayed re-emission, and provide a further selection (since each gas has sharp absorption lines), ideally centered well away from any resonance edges that cause re-emission or time delay.

Popper probably believed that the double selection would exclude the spectral region around the resonance band edges, which was the basis of Heisenberg’s objection (see above section 5.2). This would ensure that the quanta that go through the multi-stage

---

Einstein in December, as claimed by Jammer (ref. 127).

<sup>u</sup>See above section 5.1.

filter are transmitted directly without delay and not absorbed and re-emitted. For this reason, one might assume that their momentum (frequency) is left unchanged. This implies that no mechanism exists that could alter their positions. The filter, Popper concludes, selects a homogeneous ensemble of light quanta with the same momentum, whose maximally spread individual positions are completely *unknown* yet remain unchanged. They can then be reconstructed by a subsequent position measurement: “A momentum-selection apparatus without spatial smearing would therefore not contradict quantum mechanics if it is of type (II).”<sup>138</sup>

### 6.3 Einstein’s Comment on Popper’s Manuscript

Einstein replied only a few weeks later. In a letter of September 11, acknowledging the broad direction of Popper’s argument, he rejected the details of the experimental setup Popper proposed:

Dear Mr. Popper,

I have looked at your paper, and I largely [*weitgehend*] agree. Only I do not believe in the possibility of producing a ‘super-pure case’ which would allow us to predict position and momentum (colour) of a photon with ‘inadmissible’ precision. The means proposed by you (a screen with a fast shutter in conjunction with a selective set of glass filters) I hold to be ineffective in principle, for the reason that I firmly believe that a filter of this kind would act in such a way as to ‘smear’ the position, just like a spectroscopic grid.<sup>139</sup>

This passage is somewhat puzzling. Einstein seems to think that Popper was proposing a method to prepare a super-pure case using a slit with shutter followed by filters. However, just as he did in *Logik der Forschung*, in the manuscript Popper<sup>140</sup> uses the example of the screen with fast shutter followed by a filter to *exclude* that his setup would allow the construction of a super-pure case by measuring position and momentum (see above section 4.2).

Popper’s proposal was a light filter followed by a Geiger counter. Einstein may have gone through the manuscript rather quickly; yet the last part of his objection did hit the mark. The glass filter decomposes a short light pulse into quasi-monochromatic wave trains  $W_n$ . Absorbing filters cut out all colors  $W_n$  except one,  $W_1$ . The filter thereby places light quanta in a state with sharp momentum  $W_1$ , but at the cost of making position unsharp.<sup>141</sup> As Popper realized early on, if the filter smears the position, his thought experiment breaks down. The  $A$  path reconstruction stops at the momentum measurement; one cannot determine the time of collision at  $S$ , and thus one cannot determine arbitrarily sharp predictions of the  $B$  particles via a conservation law. This is exactly the result that ETP had obtained a few years earlier.

Referring to his joint paper with Podolsky and Rosen,<sup>142</sup> Einstein explained that he did not have any copies at hand but could briefly summarize its content. The situation is indeed not dissimilar to that of Popper’s thought experiment. After, say, a collision, two particles are described by the joint, nonfactorizable wave function<sup>v</sup>  $\psi(x_1, x_2)$ , with sharp relative position ( $x_B - x_A = 0$ ) and sharp total momentum ( $p_A + p_B = 0$ )—but spread individual particle momenta. If one measures, say, the momentum  $p_A$  of particle  $A$ , its state collapses into the corresponding momentum eigenstate  $\psi_A$ ; quantum mechanics then assigns to subsystem  $B$  a conditional momentum eigenstate  $\psi_B$ . For this state, however, the position  $x_B$  is completely undetermined, which blocks any attempt to

<sup>v</sup>As is well known, the term ‘entanglement’ was first introduced by Schrödinger ((ref. 143) and (ref. 144)) in the ensuing months.

beat the uncertainty relations, as Popper had hoped. Einstein’s point, rather, was that the specific form of  $\psi_B$  would have been different had one performed a position measurement on system  $A$ . This means that two different  $\psi$ -functions correspond to one and the same physical state of  $B$ , which itself has not changed physically: “It is therefore not possible to regard the  $\psi$ -function as the complete description of the state of the system.”<sup>145</sup>

In Einstein’s contemporary correspondence, the ‘private’ formulation of Einstein’s argument ends here,<sup>146</sup> without reference to the indeterminacy relations. Rather, it involves the measurement of a single variable together with a conservation law or geometrical correlation.<sup>147</sup> However, since Popper had asked him to summarize the published paper, Einstein adds that, since both position and momentum of  $B$  can be predicted with certainty, according to the infamous EPR ‘criterion of reality’, *both* quantities must correspond to something that exists in physical reality. This situation, because of the indeterminacy relations, finds no counterpart in the formalism of quantum mechanics.<sup>148</sup> Although the argument was again meant to convey the incompleteness of the theory, it also seems to conflict with the spirit of the indeterminacy relations. Indeed, Popper<sup>149</sup> would later describe the EPR argument, as presented in this letter, as a weaker (no prediction for both position and momentum simultaneously) but correct (both position and momentum are simultaneously part of reality) version of his own failed ETP-like thought experiment.

## Conclusion

After the objection from Einstein, Popper’s scientific hero, he gave up. Thanks to Weisskopf’s mediation,<sup>150</sup> he had the opportunity to meet Bohr in Copenhagen in June 1936,<sup>151</sup> where he was invited, at the last minute, to the Second International Congress for the Unity of Science. However, at that time he felt defeated. Only at the turn of the 1950s did Popper return to the philosophy of quantum theory<sup>152</sup>—however, this time in the name of realism rather than determinism.<sup>153</sup> The reflections on the topic written between 1950 and 1956 were meant to appear in a *Postscript* to the English edition of *Logik der Forschung*, but their publication was ultimately delayed.<sup>154</sup> Still, in the footnotes and appendices added to the new edition (marked by starred numbers), Popper made no secret of the fact that his old thought experiment was “based upon a mistake,” as had been pointed out to him by von Weizsäcker, Heisenberg, and Einstein: the past of an electron cannot be reconstructed without blurring its position..<sup>155</sup> The young Popper ultimately failed to follow his own advice concerning the proper use and misuse of thought experiments in quantum theory, as articulated in the new Appendix XI\*. His aim was to provide a genuinely *critical* thought experiment, one that would reveal hidden assumptions embedded in a theory (like the coupling-hypothesis) rather than an *apologetic* one designed to conceal them. However, as Popper himself later warned, a proper critical imaginary experiment should introduce only those special assumptions that favor one’s opponents.

Popper appears, in this respect, to have been curiously oblivious to the fact that his own youthful attempts to modify his original set-up by devising ever more convoluted filter set-ups failed precisely to meet this requirement.<sup>156</sup> Popper ultimately concedes that von Weizsäcker was correct: “for the electric field perpendicular to the direction of a beam of electrons” used in Appendix VI does not act as he expected: “for the width of the beam must be considerable if the electrons are to move parallel to the  $x$ -axis, and

as a consequence, *their position before their entry into the field cannot be calculated* with precision after they have been deflected by the field.”<sup>157</sup> Moreover, Popper argues, “as Einstein shows” in his 1935 letter,<sup>w</sup> the same can be said “for a filter acting upon a light quantum.”<sup>159</sup> Popper acknowledges that the conclusion is inescapable: “non-predictive measurements determine the path of a particle only *between* two measurements,” such as a measurement of momentum followed by one of position (or vice versa): “it is not possible, according to quantum theory, to project the path further back, *i.e.*, to the region of time *before* the first of these measurements.”<sup>160</sup> As we have seen, Popper realized early on that, if this is the case, his thought experiment—and, more generally, ETP-like thought experiments—“collapses.”<sup>161</sup>

However, in a long footnote to Appendix VI, as well as in the new Appendix \*XI, Popper appears to blame Heisenberg for his ‘mistake’. According to Popper, Heisenberg “*fails to establish that measurements of position and of momentum are symmetrical.*”<sup>162</sup> In Heisenberg’s famous  $\gamma$ -ray microscope thought experiment, position is determined using high-frequency light, which strongly *disturbs* the electron’s momentum. Conversely, momentum can be determined, in principle, using low-frequency (long-wavelength) radiation via the Doppler effect so as to *avoid* disturbance; but in this case, the position remains indeterminate.<sup>163</sup> Popper claims that he had extended this asymmetry to physical selections. Position selection via a slit disturbs the momentum (producing the diffraction pattern); momentum selection via a velocity filter only makes the position *unknown*: “But I now believe that I was wrong in assuming that what holds for Heisenberg’s imaginary ‘observations’ or ‘measurements’ would also hold for my ‘selections’.”<sup>164</sup> Popper even argues that his thought experiment “can be used to point out an inconsistency in Heisenberg’s discussion of the observation of an electron.”<sup>165</sup>

Popper’s recollection of the events strikes me as rather self-serving and appears to be contradicted by the unpublished textual evidence from that period. As we have seen, in his reply to Heisenberg, as well as in his unpublished second communication, Popper<sup>166</sup> explicitly argues that the momentum determination should occur not through momentum *measurement* via the Doppler effect, as von Weizsäcker had proposed, because the latter does disturb the position; on the contrary, a momentum *selection* via an electron spectrometer or a light filter does not. The latter leaves the given momentum unchanged, and therefore also the position—albeit the latter remains unknown. Popper was not misled by Heisenberg’s notion of ‘measurement’,<sup>x</sup> but by the notion of ‘physical selection’ to which he committed himself.<sup>y</sup> Every accusation Popper levels against Heisenberg’s notion of ‘measurement’ is ultimately a confession about his own notion of ‘physical selection’. It was, indeed, Popper rather than Heisenberg who tried to exploit a non-existent asymmetry between position and momentum selections. Popper<sup>169</sup> had some reason to claim that his notion of ‘physical selection’ was similar in spirit to the modern concept of ‘preparation’, as outlined around the same time by Margenau.<sup>170</sup> Yet it is clearly not equivalent to it. Paraphrasing Popper, one can say that the failure of his thought experiment “can be used to point out an inconsistency” in his notion of

---

<sup>w</sup>Reproduced and translated in the first English translation of *Logik der Forschung* (ref. 158).

<sup>x</sup>At most, Popper was misled by Heisenberg’s ambiguous phrasing on p. 15 of his 1930 lecture notes (ref. 167); see above section 1.1.

<sup>y</sup>One might argue that in Heisenberg’s analysis (ref. 168), the position measurement constitutes a genuine ‘measurement’ (recording); by contrast, the momentum measurement is a ‘preparation’ of an ensemble characterized by a definite momentum distribution, while the position remains indeterminate. However, the idea that ‘undetermined’ here means ‘unchanged, but unknown’ is Popper’s responsibility. This is, indeed, an instance in which Popper is confusing ‘preparation’ with mere ‘selection’.

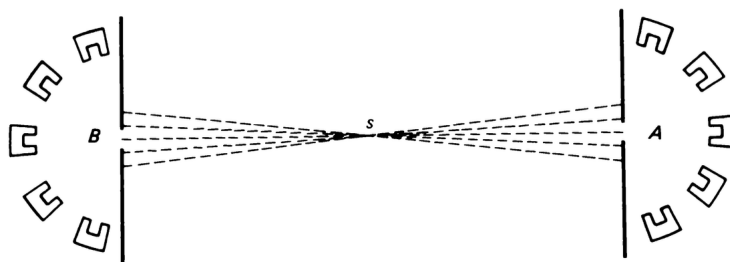


Fig. 5. From Karl R. Popper, *Quantum Theory and the Schism in Physics*, London: Routledge, 1982, 28

‘physical selection’.

Popper seems to have conceived of the momentum ‘filter’ intuitively as analogous to a classical ‘sieve’ that merely *selects* an ensemble of particles that already possess the *old* preassigned sharp momentum state, and simply leaves the position unknown. However, unlike such a classical device, the quantum-mechanical filter actually *prepares* an ensemble of systems such that each member is in a *new* momentum eigenstate, and thus completely spread position. The asymmetry between position and momentum preparations on which Popper relies is apparent. It arises from the fact that position is not an eigenstate of the Hamiltonian operator of a free particle, whereas momentum is. For this reason, momentum selection appears not to produce any analogous ‘disturbance’ to that occurring in a position measurement; it merely seems to allow particles to pass through the filter with unchanged momentum. Yet the point Popper missed is that a momentum preparation still projects the system into a momentum eigenstate, rendering its position completely undetermined, since position is not an eigenstate of the momentum operator. Consequently, there is no way to combine information from a preparation and a measurement to reconstruct an individual trajectory before the momentum selection, as Popper had hoped.

Popper did realize that his ETP-like arrangement was based on a ‘mistake’; however, he ultimately does not seem to have appreciated the deeper conceptual reason behind it. In the second edition of the *Logic*, Popper complained that those “critics who rightly rejected the idea of this imaginary experiment appear to have believed that they had thereby also refuted the preceding analysis.”<sup>171</sup> However, Popper was adamant that this was not so. He not only rightly continued to maintain that preparation and measurement should be *distinguished*, but also held on to the idea that, for this reason, the two can be *uncoupled*. To his credit, Popper made it unmistakably clear that this was the central point of his argument. However, the repeated failure of his experimental setups should have suggested that this point does not stand. Far from being an additional or auxiliary assumption, the coupling-hypothesis lies at the core of quantum mechanics, which is ultimately a theory of *transition* probabilities between prepared and measured states. Popper’s repeated attempts to refute the ‘coupling-hypothesis’ inadvertently underscore its centrality to quantum mechanics: a measurement outcome, whether predictive or non-predictive, is physically meaningful only when defined relative to a prepared quantum state.

Even though he acknowledged the problem that affected his setup, he remained convinced that one could “replace [his] invalid experiment”<sup>172</sup> of the ETP-type with a valid EPR-type experiment. After Popper<sup>173</sup> returned to work on quantum physics, he outlined, around 1977 or 1978, a thought experiment of this kind, which he made public in the early 1980s.<sup>174</sup> Popper was at the height of his fame, and his variant of the EPR

argument sparked considerable public controversy.<sup>175</sup> However, on closer inspection, even this much more famous thought experiment rests on a similar maneuver. Once again, Popper tries to exploit a perfect correlation (*i.e.*, relative position) inherent in a jointly prepared state to bypass the preparation-measurement coupling. He starts from an entangled state with sharp relative position ( $y_B - y_A = 0$ ) and sharp total momentum ( $p_A + p_B = 0$ ) for two particles emitted from a common source  $S$  (figure 5). He argues that changing the preparation of system  $A$  (narrowing  $\Delta y_A$  via a slit) would change the measurement statistics of the distant system  $B$  (increasing the spread  $\Delta p_{y_B}$ ) *without changing its own preparation*. Once again, the thought experiment fails for the same ‘mistake’: perfect correlations belong to the joint preparation of the composite system and cannot be used to circumvent the preparation-measurement coupling for one of the subsystems.

## Acknowledgments

Quotations from unpublished materials by Karl R. Popper are reproduced with permission of the Karl Popper Archives, University of Klagenfurt. I thank Thomas Hainscho for valuable support.

This paper was funded by PRIN Bando 2022, Prot. 20224HXFLY. An earlier version of this paper was presented at a workshop in Rome, 22–24 September 2025. I thank the participants, especially Vincenzo Fano, for helpful discussions.

## Notes

<sup>1</sup> Karl R. Popper, “Zur Kritik der Ungenauigkeitsrelationen,” *Die Naturwissenschaften* 22, no. 48 (1934), 807–808.

<sup>2</sup> Karl R. Popper, “Ergänzung zu der vorstehenden kurzen Mitteilung,” first pub. in Karl R. Popper, *Frühe Schriften* (Tübingen: Mohr Siebeck, 2006), Doc. 9.

<sup>3</sup> Karl R. Popper, *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft* (Berlin: Springer, 1935). Henceforth I cite the page number of the original German edition followed by a slash and the page number of the English translation.

<sup>4</sup> Karl R. Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A],” first pub. in Popper, *Frühe Schriften* (ref. 2), Doc. 10.

<sup>5</sup> Karl R. Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [B],” first pub. in Popper, *Frühe Schriften* (ref. 2), Doc. 11.

<sup>6</sup> Karl R. Popper, “Zur Kritik der Ungenauigkeitsrelationen,” first pub. in Popper, *Frühe Schriften* (ref. 2), Doc. 12.

<sup>7</sup> Karl R. Popper, “Bemerkung zum Komplementaritätsproblem der Quantenmechanik,” first pub. in Popper, *Frühe Schriften* (ref. 2), Doc. 13.

<sup>8</sup> Popper, *Frühe Schriften* (ref. 2).

<sup>9</sup> Karl R. Popper, “Ein Kriterium des empirischen Charakters theoretischer Systeme,” *Erkenntnis* 3 (1932–1933), 426–428.

<sup>10</sup> Paul Arthur Schilpp, *The Philosophy of Karl Popper* (La Salle, Illinois: Open Court, 1974), 1125.

<sup>11</sup> Werner Heisenberg, “Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik,” *Zeitschrift für Physik* 43, no. 4 (1927), 172–198.

<sup>12</sup> Niels Bohr, “Space-Time Continuity and Atomic Physics,” H.H. Wills Memorial Lecture, given at the University of Bristol on October 5, 1931, first published in Jørgen Kalckar, ed., *Niels Bohr’s Collected Works* (Amsterdam: North-Holland, 1985), 6: Doc. VII, 127–28.

<sup>13</sup> Popper, *Logik der Forschung* (ref. 3), 174/239.

<sup>14</sup> Karl R. Popper, *The Logic of Scientific Discovery* (New York: Hutchinson & Co., 1959), 239.

<sup>15</sup> Karl R. Popper, *Quantum Theory and the Schism in Physics. From the Postscript to the Logic of Scientific Discovery* (London: Routledge, 1982), 15.

<sup>16</sup> Max Jammer, *The Philosophy of Quantum Mechanics. The Interpretations of QM in Historical*

*Perspective* (New York: Wiley, 1974), 178–79.

<sup>17</sup> Albert Einstein, Boris Yakovlevich Podolsky, and Nathan Rosen, “Can Quantum-mechanical Description of Physical Reality Be Considered Complete?” *Physical Review* 47, no. 10 (1935), 777–780.

<sup>18</sup> Jammer (ref. 16), 178.

<sup>19</sup> Henry Margenau, “Popper’s Philosophy of Science,” in *The Philosophy of Karl Popper*, ed. by Paul Arthur Schilpp (La Salle, Illinois: Open Court, 1974), 221–242, 757–58.

<sup>20</sup> Henry Margenau, “Quantum-Mechanical Description,” *Physical Review* 49, no. 3 (1936), 240–242; Henry Margenau, “Critical Points in Modern Physical Theory,” *Philosophy of Science* 4, no. 3 (1937), 337–370.

<sup>21</sup> See Karl R. Popper, “Intellectual Autobiography,” in *The Philosophy of Karl Popper*, ed. by Paul Arthur Schilpp (La Salle, Illinois: Open Court, 1974), 1–181, 72–3.

<sup>22</sup> Popper to Jammer, Apr. 13, 1967; Jammer (ref. 16), 178; fn. 30.

<sup>23</sup> Albert Einstein, Richard C. Tolman, and Boris Podolsky, “Knowledge of Past and Future in Quantum Mechanics,” *Physical Review* 37, no. 6 (1931), 780–781.

<sup>24</sup> See Nicholas Maxwell, “Popper’s Paradoxical Pursuit of Natural Philosophy,” in *The Cambridge Companion to Popper*, ed. by Jeremy Shearmur and Geoffrey Stokes (Cambridge: Cambridge University Press, 2016), 170–207, sec. 6.

<sup>25</sup> Malachi H. Hacoen, “Young Popper as a Scholarly Field,” in *Karl Popper: A Centenary Assessment*, ed. by Ian Jarvie, Karl Milford, and David Miller (Hants, England: Ashgate Pub. Ltd., 2004), 99–110.

<sup>26</sup> Popper, *Quantum Theory and the Schism in Physics* (ref. 15).

<sup>27</sup> Don Howard, “Popper and Bohr on Realism in Quantum Mechanics,” *Quanta* 1 (2012), 33–57; William M. Shields, “A Historical Survey of Sir Karl Popper’s Contribution to Quantum Mechanics,” *Quanta* 1, no. 1 (2012), 1–12; Maxwell (ref. 24), sec. 6; Flavio Del Santo, “Genesis of Karl Popper’s EPR-Like Experiment and Its Resonance amongst the Physics Community in the 1980s,” *Studies in History and Philosophy of Science. Part B: Studies in History and Philosophy of Modern Physics* 62 (2018), 56–70; Flavio Del Santo, “Karl Popper’s Forgotten Role in the Quantum Debate,” *Studies in History and Philosophy of Science. Part B: Studies in History and Philosophy of Modern Physics* 67 (2019), 78–88.

<sup>28</sup> Karl R. Popper, “Quantum Mechanics without the Observer,” in *Quantum Theory and Reality*, ed. by Mario Bunge (Berlin, 1967), 7–44; Popper, *Quantum Theory and the Schism in Physics* (ref. 15).

<sup>29</sup> Werner Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (Leipzig: Hirzel, 1930); Werner Heisenberg, *The Physical Principles of the Quantum Theory* (Chicago: The University of Chicago Press, 1930).

<sup>30</sup> Niels Bohr, “The Quantum Postulate and the Recent Development of Atomic Theory,” *Nature* 121 (1928), 580–590, 583.

<sup>31</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.

<sup>32</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 19–20.

<sup>33</sup> Michael Stöltzner, “Can Meaning Criteria Account for Indeterminism?” in *Moritz Schlick Leben, Werk und Wirkung*, ed. by Fynn Ole Engler and Matthias Iven (Berlin: Parerga, 2008), 215–245.

<sup>34</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.

<sup>35</sup> Kristian Camilleri, *Heisenberg and the Interpretation of Quantum Mechanics. The Physicist As Philosopher* (Cambridge: Cambridge University Press, 2009), 105–6.

<sup>36</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.

<sup>37</sup> Moritz Schlick, “Die Kausalität in der gegenwärtigen Physik,” *Die Naturwissenschaften* 19, no. 7 (1931), 145–162, 159.

<sup>38</sup> Einstein to Schlick, Nov. 28, 1930; Albert Einstein Archives, 21-603.

<sup>39</sup> Schlick (ref. 37).

<sup>40</sup> Einstein, Tolman, and Podolsky (ref. 23).

<sup>41</sup> Bohr, “Space-Time Continuity and Atomic Physics” (ref. 12), 127–28.

<sup>42</sup> Guido Bacciagaluppi and Elise Crull, *The Einstein Paradox. The Debate on Nonlocality and Incompleteness in 1935* (Cambridge, UK: Cambridge University Press, 2024), 1.4.3.

<sup>43</sup> Einstein, Tolman, and Podolsky (ref. 23), 781.

<sup>44</sup> Einstein, Tolman, and Podolsky (ref. 23), 781; my emphasis.

<sup>45</sup> This brief overview is based on Bacciagaluppi and Crull (ref. 42), Ch. 1.

<sup>46</sup> Ehrenfest to Bohr, Jul. 9, 1931, AHQP/BSC 18.

<sup>47</sup> See also Albert Einstein, “Über die Unbestimmtheitsrelationen,” *Zeitschrift für angewandte Chemie* 45, no. 1 (1932), 23.

- <sup>48</sup> Einstein to Ehrenfest, Apr. 5, 1932; Albert Einstein Archives, 10-231.
- <sup>49</sup> Léon Rosenfeld, “Niels Bohr in the Thirties,” in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, ed. by Stefan Rozenthal (Amsterdam: North-Holland, 1967), 114–137, 127–28.
- <sup>50</sup> Karl R. Popper, “Die beiden Grundprobleme der Erkenntnistheorie,” first published as *Die beiden Grundprobleme der Erkenntnistheorie*, ed. by Troels Eggers Hansen (Tübingen: Mohr, 1979; 2nd ed. 1994).
- <sup>51</sup> Popper, *Logik der Forschung* (ref. 3).
- <sup>52</sup> Malachi Haim Hacoen, *Karl Popper. Politics and Philosophy in Interwar Vienna* (Cambridge: Cambridge University Press, 2010), 235–44.
- <sup>53</sup> Hacoen (ref. 52), 237.
- <sup>54</sup> Weisskopf to Popper, Jun. 26, 1934; Karl Popper Archives 360-21.
- <sup>55</sup> Weisskopf to Popper, Jun. 26, 1934; Karl Popper Archives 361-21.
- <sup>56</sup> Weisskopf to Popper, Jun. 26, 1934; Karl Popper Archives 360.21.
- <sup>57</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 1).
- <sup>58</sup> Popper, “Ergänzung zu der vorstehenden kurzen Mitteilung” (ref. 2).
- <sup>59</sup> Earle Hesse Kennard, “Zur Quantenmechanik einfacher Bewegungstypen,” *Zeitschrift für Physik* 44, no. 4-5 (1927), 326–352.
- <sup>60</sup> Hermann Weyl, *Gruppentheorie und Quantenmechanik* (Leipzig: Hirzel, 1928), 67–8.
- <sup>61</sup> Richard von Mises, “Über kausale und statistische Gesetzmäßigkeit in der Physik,” *Erkenntnis* 1, no. 1 (1930), 189–210; Richard von Mises, “Über Heisenbergs Ungenauigkeitsbeziehungen und ihre erkenntnistheoretische Bedeutung,” *Naturwissenschaften* 22, no. 49 (1934), 822.
- <sup>62</sup> Richard von Mises, *Wahrscheinlichkeit Statistik und Wahrheit* (Wien: Springer, 1928).
- <sup>63</sup> Popper, *Logik der Forschung* (ref. 3), 163/226.
- <sup>64</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 225; fn. \*1; starred numbers mark the footnotes and appendices added to the English translation.
- <sup>65</sup> Popper, *Logik der Forschung* (ref. 3), 163/225–26.
- <sup>66</sup> Margenau, “Quantum-Mechanical Description” (ref. 20); Margenau, “Critical Points in Modern Physical Theory” (ref. 20).
- <sup>67</sup> Popper, *Logik der Forschung* (ref. 3), 163–64/225–26.
- <sup>68</sup> Popper, *Logik der Forschung* (ref. 3), 164/226–27.
- <sup>69</sup> Popper, *Logik der Forschung* (ref. 3), 154–55/215–16.
- <sup>70</sup> Arthur March, *Die Grundlagen der Quantenmechanik* (Leipzig: Barth, 1931), 1–2, 57.
- <sup>71</sup> Weyl (ref. 60), 68.
- <sup>72</sup> Popper, *Logik der Forschung* (ref. 3), 165–66/235–36.
- <sup>73</sup> Popper, *Logik der Forschung* (ref. 3), 166/235–36.
- <sup>74</sup> Popper, *Logik der Forschung* (ref. 3), 156–57, 167, 177/219, 230, 240.
- <sup>75</sup> March (ref. 70), 55.
- <sup>76</sup> Popper, *Logik der Forschung* (ref. 3), 169/230–31.
- <sup>77</sup> Alexander Pechenkin, “The Statistical (Ensemble) Interpretation of Quantum Mechanics,” in *The Oxford Handbook of the History of Quantum Interpretations*, ed. by Olival Freire et al. (Oxford: Oxford University Press, 2022), 1223–1246, 50.2.
- <sup>78</sup> Popper, *Logik der Forschung* (ref. 3), 171/235–36.
- <sup>79</sup> Popper, *Logik der Forschung* (ref. 3), 173–74.
- <sup>80</sup> Popper, *Logik der Forschung* (ref. 3), 174/238–39.
- <sup>81</sup> Popper, *Logik der Forschung* (ref. 3), 33/59.
- <sup>82</sup> Popper, *Logik der Forschung* (ref. 3), §77.
- <sup>83</sup> Popper, *Logik der Forschung* (ref. 3), 177/241–42; my emphasis.
- <sup>84</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.
- <sup>85</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 1), 807.
- <sup>86</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 1), 807; my emphasis.
- <sup>87</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 1), 807.
- <sup>88</sup> Michael Redhead, “Popper and the Quantum Theory,” *Royal Institute of Philosophy Supplement* 39 (1995), 163–176, 166.
- <sup>89</sup> Popper, *Logik der Forschung* (ref. 3), §78.
- <sup>90</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.
- <sup>91</sup> Popper, “Ergänzung zu der vorstehenden kurzen Mitteilung” (ref. 2), 397–98.
- <sup>92</sup> Popper, “Ergänzung zu der vorstehenden kurzen Mitteilung” (ref. 2), 400.
- <sup>93</sup> Popper, *Logik der Forschung* (ref. 3), 220–21/299–300.

- <sup>94</sup> Redhead (ref. 88), 164.
- <sup>95</sup> Popper, “Ergänzung zu der vorstehenden kurzen Mitteilung” (ref. 2), 401.
- <sup>96</sup> Popper, *Logik der Forschung* (ref. 3), 221–22/300–01.
- <sup>97</sup> Popper, *Logik der Forschung* (ref. 3), 220/300; translation modified.
- <sup>98</sup> Heisenberg to Popper, Nov. 23, 1934; Karl Popper Archives 305-32.
- <sup>99</sup> Von Weizsäcker in Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 1), 808.
- <sup>100</sup> Cf. Reinhard F. Werner and Terry Farrelly, “Uncertainty from Heisenberg to Today,” *Foundations of Physics* 49, no. 6 (2019), 460–491, fn. 21.
- <sup>101</sup> Popper to Heisenberg, Nov. 26, 1934; Karl Popper Archives 305-52.
- <sup>102</sup> Popper to Heisenberg, Nov. 26, 1934; Karl Popper Archives 305-52.
- <sup>103</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4).
- <sup>104</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4), 406.
- <sup>105</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4), 406.
- <sup>106</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4), 406.
- <sup>107</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4), 407.
- <sup>108</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4), 407.
- <sup>109</sup> Von Weizsäcker to Popper, Dec. 6, 1934; Karl Popper Archives 360-21.
- <sup>110</sup> Von Weizsäcker to Popper, Dec. 6, 1934; Karl Popper Archives 360-21.
- <sup>111</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 21–2.
- <sup>112</sup> Von Weizsäcker to Popper, Dec. 6, 1934; Karl Popper Archives 360-21.
- <sup>113</sup> Heisenberg to Popper, Dec. 10, 1934; Karl Popper Archives 305-32.
- <sup>114</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [B]” (ref. 5).
- <sup>115</sup> Popper to Heisenberg, Dec. 16, 1934; Karl Popper Archives 305-32.
- <sup>116</sup> Popper to Heisenberg, Dec. 16, 1934; Karl Popper Archives 305-32.
- <sup>117</sup> Popper to Heisenberg, Dec. 16, 1934; Karl Popper Archives 305-32.
- <sup>118</sup> Popper-Euler Correspondence, Karl Popper Archives 293-29.
- <sup>119</sup> Grete Hermann, “Die naturphilosophischen Grundlagen der Quantenmechanik,” *Abhandlungen der Fries’schen Schule* 6, no. 2 (1935), 59–152, 123; fn. 10.
- <sup>120</sup> Grete Hermann, “Review of Popper, *Logik der Forschung*,” *Physikalische Zeitschrift* 36, no. 13 (1935), 481–482.
- <sup>121</sup> Von Weizsäcker to Hermann, Mar. 13, 1935, in Kay Herrmann, *Grete Henry-Hermann: Philosophie – Mathematik – Quantenmechanik. Texte zur Naturphilosophie und Erkenntnistheorie, mathematisch-physikalische Beiträge sowie ausgewählte Korrespondenz aus den Jahren 1925 bis 1982* (Wiesbaden: Springer, 2019), Brief 15.
- <sup>122</sup> Heisenberg to Popper, Mar. 19, 1935; Karl Popper Archives 305-32.
- <sup>123</sup> Heisenberg to Popper, Mar. 19, 1935; Karl Popper Archives 305-32.
- <sup>124</sup> Weisskopf to Popper, Jan. 21, 1934; Karl Popper Archives 360-21.
- <sup>125</sup> Busch to Einstein, Apr. 28, 1935; Albert Einstein Archives, 34-338.
- <sup>126</sup> Jammer (ref. 16).
- <sup>127</sup> Jammer (ref. 16), 178.
- <sup>128</sup> Einstein to Popper, Jun. 15, 1935; Albert Einstein Archives, 19-124.
- <sup>129</sup> Popper to Einstein, Jul. 18, 1935; Albert Einstein Archives, 19-126.
- <sup>130</sup> Popper to Einstein, Jul. 18, 1935; Albert Einstein Archives, 19-126.
- <sup>131</sup> Popper to Carnap, Jun. 10, 1935; Carnap Collection.
- <sup>132</sup> Popper to Einstein, Aug. 29, 1935; Albert Einstein Archives, 19-127.
- <sup>133</sup> Johann von Neumann, *Mathematische Grundlagen der Quantenmechanik* (Berlin: Springer, 1932).
- <sup>134</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 48.
- <sup>135</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 427.
- <sup>136</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 428.
- <sup>137</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 429.
- <sup>138</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 429.
- <sup>139</sup> Einstein to Popper, Sep. 11, 1935; Albert Einstein Archives, 19-130.
- <sup>140</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen” (ref. 6), 401–3.
- <sup>141</sup> Einstein to Popper, Sep. 11, 1935; Albert Einstein Archives, 19-130.
- <sup>142</sup> Einstein, Podolsky, and Rosen (ref. 17).
- <sup>143</sup> Erwin Schrödinger, “Die gegenwärtige Situation in der Quantenmechanik,” *Die Naturwissenschaften* 23, no. 48 (1935), 807–812, 823–828, 844–849.
- <sup>144</sup> Erwin Schrödinger, “Discussion of Probability Relations between Separated Systems,” *Mathemati-*

- cal *Proceedings of the Cambridge Philosophical Society* 31, no. 4 (1935), 555–563.
- <sup>145</sup> Einstein to Popper, Sep. 11, 1935; Albert Einstein Archives, 19-130.
- <sup>146</sup> Einstein to Schrödinger, Jun. 19, 1935; Albert Einstein Archives, 22-047.
- <sup>147</sup> Don Howard, “Einstein on Locality and Separability,” *Studies in the History of the Philosophy of Science* 16, no. 3 (1985), 171–201; Arthur Fine, *The Shaky Game. Einstein, Realism, and the Quantum Theory* (Chicago: University of Chicago Press, 1986), 38.
- <sup>148</sup> Einstein to Popper, Sep. 11, 1935; Albert Einstein Archives, 19-130.
- <sup>149</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 244; fn. \*4.
- <sup>150</sup> Weisskopf to Popper, Oct. 17, 1936; Karl Popper Archives 360-21.
- <sup>151</sup> See Popper, “Bemerkung zum Komplementaritätsproblem der Quantenmechanik” (ref. 7).
- <sup>152</sup> Del Santo, “Karl Popper’s Forgotten Role in the Quantum Debate” (ref. 27).
- <sup>153</sup> Howard, “Popper and Bohr on Realism in Quantum Mechanics” (ref. 27).
- <sup>154</sup> Popper, *Quantum Theory and the Schism in Physics* (ref. 15).
- <sup>155</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 217; fn. \*1.
- <sup>156</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 444–6.
- <sup>157</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 299; fn. \*1; my emphasis.
- <sup>158</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), Appendix \*xii.
- <sup>159</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 299; fn. \*1.
- <sup>160</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 242; fn. \*3; my emphasis.
- <sup>161</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 242; fn. \*3.
- <sup>162</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 451.
- <sup>163</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 451.
- <sup>164</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 299; fn. \*1.
- <sup>165</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 299; fn. \*1.
- <sup>166</sup> Popper, “Zur Kritik der Ungenauigkeitsrelationen, 2. Mitteilung [A]” (ref. 4).
- <sup>167</sup> Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (ref. 29), 15.
- <sup>168</sup> Heisenberg, “Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik” (ref. 11).
- <sup>169</sup> Popper, “Intellectual Autobiography” (ref. 21), 72–73; Popper, *Quantum Theory and the Schism in Physics* (ref. 15), 23; fn. 32.
- <sup>170</sup> Margenau, “Quantum-Mechanical Description” (ref. 20); Margenau, “Critical Points in Modern Physical Theory” (ref. 20).
- <sup>171</sup> Popper, *The Logic of Scientific Discovery* (ref. 14), 239; fn. \*2.
- <sup>172</sup> Karl R. Popper, *The Logic of Scientific Discovery* (New York: Hutchinson & Co., 1972), 236; fn. \*\*.
- <sup>173</sup> Popper, “Quantum Mechanics without the Observer” (ref. 28).
- <sup>174</sup> Popper, *Quantum Theory and the Schism in Physics* (ref. 15).
- <sup>175</sup> Del Santo, “Genesis of Karl Popper’s EPR-Like Experiment and Its Resonance amongst the Physics Community in the 1980s” (ref. 27).