Chapter 6
The Copenhagen Interpretation

The Copenhagen interpretation of quantum mechanics is the set of ideas, about how the theory should be understood, that was chiefly developed by Niels Bohr in collaboration with various colleagues, most notably Werner Heisenberg, in the 1920s and 1930s. Bohr's philosophy rapidly achieved the status of a kind of orthodoxy within the physics community, with early dissenters (such as Einstein and Schrödinger) being typically dismissed with charges of senility, and occasional critics from later decades (such as Bohm and Bell and Everett) being regarded practically as heretics, sinners against the true and proper nature of science. It became commonplace for proponents of the Copenhagen interpretation to insist that there was, in fact, no logically viable alternative to it at all, and authors of quantum mechanics textbooks continue, to the present day, to pay universal (if typically brief) lip service to Bohr's philosophy.

All of that said, however, the question of what, precisely, the Copenhagen interpretation *says* is surprisingly controversial. It has been joked that there are as many different versions of the Copenhagen interpretation as there are physicists who claim to follow it, and even scholars who study Bohr's writings in detail tend to come up with radically different interpretations of what he says and means. And yet, despite this unclarity, there is somehow nevertheless a fairly clear dichotomy between Bohr's actual views (whatever they were exactly) and the shallow, pragmatic version of them that students typically absorb from their textbooks and teachers.

The present chapter therefore begins with a rather long "guided tour" of several of the most relevant and important papers by Bohr and Heisenberg, so that readers can start to develop some direct acquaintance with their actual views. In the middle part of the chapter, we look at Bohr's analysis of several thought experiments that Einstein had proposed by way of criticizing the Copenhagen approach. Only then, toward the end of the chapter, will we step back and discuss (more briefly) the typical contemporary understanding of the Copenhagen interpretation, and how it is viewed by both its proponents and its critics.

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6.1 Bohr's Como Lecture

Bohr's "Como Lecture" was first delivered at a celebration for Alexander Volta in Como, Italy, in the fall of 1927 and was subsequently published in *Nature* the following year [1]. Its actual title was "The Quantum Postulate and the Recent Development of Atomic Theory" and it provides an illuminating summary of Bohr's philosophical interpretation of the first several years of the development of quantum mechanics.

Bohr cuts right to the chase in the first paragraph:

> The quantum theory is characterised by the acknowledgment of a fundamental limitation in the classical physical ideas when applied to atomic phenomena. The situation thus created is of a peculiar nature, since our interpretation of the experimental material rests essentially upon the classical concepts. Notwithstanding the difficulties which hence are involved in the formulation of the quantum theory, it seems, as we shall see, that its essence may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's quantum of action [1].

Already here we see a central theme of Bohr's Copenhagen philosophy, concerning the tension between (i) the supposed necessity of our continuing to use "the classical concepts" and (ii) the limitations of these concepts in capturing the uniquely quantum processes. Bohr elaborates in the following paragraph:

> This [quantum] postulate implies a renunciation as regards the causal space-time co-ordination of atomic processes. Indeed, our usual description of physical phenomena is based entirely on the idea that the phenomena concerned may be observed without disturbing them appreciably. This appears, for example, clearly in the theory of relativity, which has been so fruitful for the elucidation of the classical theories. As emphasised by Einstein, every observation or measurement ultimately rests on the coincidence of two independent events at the same space-time point. Just these coincidences will not be affected by any differences which the space-time co-ordination of different observers otherwise may exhibit. Now the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation. After all, the concept of observation is in so far arbitrary as it depends upon which objects are included in the system to be observed. Ultimately every observation can of course be reduced to our sense perceptions. The circumstance, however, that in interpreting observations use has always to be made of theoretical notions, entails that for every particular case it is a question of convenience at what point the concept of observation involving the quantum postulate with its inherent 'irrationality' is brought in [1].

I would summarize this by saying that, according to Bohr, our everyday, classical notions of external physical reality (for example, assigning definite "states" to various objects, talking about the causal interactions of objects in space and time, etc.) tacitly rely on the assumption that the act of observation can be taken as having no (or at least negligible) influence on the objects being observed. Whereas, in the quantum realm, "observation ... will involve an interaction with the agency of observation not to be neglected." The act of observation, in short, *disturbs* the state of the observed object in an ineliminable and unpredictable way, thus rendering it impossible to acquire knowledge of the (pre-existing, undisturbed) state of the object and, indeed, thereby rendering talk of such "pre-existing states" empirically meaningless. We must therefore take a more holistic perspective on the broader system comprising both the "observer" and the "observed system" and recognize that any sharp division between them (such as would be implied in analyzing the interaction in terms of separate systems, each with its own well-defined state, interacting) is an arbitrary construct – one which we perhaps cannot avoid imposing in discussing and reporting our observations, but one which nevertheless in some fundamental sense distorts the real situation.

The overall line of reasoning here resonates with a general philosophical framework (that was quite popular at the time) called "positivism" (and/or sometimes the closely-related idea of "operationalism"), one of whose essential points was the idea that meaningful assertions must be *verifiable* by direct observation. To use a slightly silly and unfair example, just to try to clarify the idea, a positivist might claim that it is literally meaningless to speculate about what happens to the light inside your refrigerator when the door is closed. Since there is (or rather: assuming it was somehow the case that there is) no way to observe how light or dark it is inside the refrigerator when the door is shut (because observing this requires opening the door!), it is literally meaningless to even speculate about it, and any such speculative talk should be dismissed from rational scientific discourse as worthless and "metaphysical".

Bohr's perspective here also recalls that of the famous 18th century German philosopher, Immanuel Kant, who influentially argued that we are fundamentally cut off from true (so-called "noumenal") reality because, in effect, our minds are hard-wired to categorize the incoming sensory information in certain ways. We are thus aware, by ordinary means, only of the so-called "phenomenal" world – i.e., the world of appearances, of things-as-processed-by-us whose true natures must remain forever inaccessible. One commentator, Henry Krips, has suggested that Bohr can be understood as continuing a trend initiated by 19th century thinkers who "physiologiz[ed] the Kantian conception of observation" by locating the supposedly distorting process not in the mind, but in the physiology of perception. According to Krips,

> Bohr extended this position by proposing that the 'external procedures' that affect the forms of sensible intuition include the processes of observation themselves. Thus Bohr stood at the end of a long historical trajectory: Kant conceived the apparatus of observation as an inner mental faculty, analogous to a pair of spectacles that mediated and in particular gave form to and interpreted raw sense impressions. Neo-Kantians projected the interpretative aspect of vision outwards, reconceiving it as a bodily, and specifically physiological process. Bohr took this further by including observation as [affecting] not merely what we see but also the terms in which we describe it [2].

In any case, though, and whatever the historical precedents, for Bohr the fundamental lesson of the quantum theory was that there is a kind of inherent "graininess" and unpredictability to interactions, including the interactions between "external object and "observer" (or "measuring apparatus"). Such interactions supposedly imply a finite, non-negligible, and uncontrollable *disturbance*, of the "external object", whenever we try to observe it. And so we are cut off from the possibility of scientifically meaningful descriptions of the microscopic world, for just the same reasons that (in the silly example of the last paragraph) we are cut off from scientifically meaningful descriptions of the state of illumination inside a closed refrigerator: the very act of trying to *verify* any hypothesis about the state of the object in question, *disturbs* its state and thereby undercuts the attempted verification. This is why Bohr speaks of a "*renunciation*" of the applicability of our classical concepts.

Bohr continues to explain that

> This situation has far-reaching consequences. On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterises the classical theories, as complementary but exclusive features of the description, symbolising the idealisation of observation and definition respectively. Just as the relativity theory has taught us that the convenience of distinguishing sharply between space and time rests solely on the smallness of the velocities ordinarily met with compared to the velocity of light, we learn from the quantum theory that the appropriateness of our usual causal space-time description depends entirely upon the small value of the quantum of action as compared to the actions involved in ordinary sense perceptions. Indeed, in the description of atomic phenomena, the quantum postulate presents us with the task of developing a 'complementarity' theory the consistency of which can be judged only by weighing the possibilities of definition and observation [1].

Here we first encounter Bohr's fundamental concept of "complementarity". In this paragraph, he describes, as "complementary", the causal and space-time perspectives on events. His point is that, whereas in the context of classical physics it is taken for granted that both perspectives are simultaneously applicable and indeed classical descriptions by definition provide causal accounts of spatio-temporal events, the two perspectives are mutually exclusive in the quantum realm. Meaningful attribution of precise spatial and temporal coordinates to events requires, for example, careful position measurements. But such measurements, as we have seen, imply physical interactions which disrupt the causal processes which might otherwise, in the absence of said interactions, have been taking place.

For Bohr, the "causal" description means one taking account of energy and momentum conservation. So the "complementarity" between space-time and causal descriptions arises specifically from the fact that position measurements imply an interaction involving unpredictable momentum exchange between the system in question and the position-measuring apparatus. One thus sees an intimate connection (which we will continue to explore as this chapter develops) between Bohr's view that "space-time" and "causal" descriptions are mutually exclusive, and Heisenberg's important discovery that position and momentum (as well as time and energy) jointly obey an uncertainty (or indeterminacy) principle.

Bohr saw a similar sort of complementarity between the wave and particle pictures of light (and, subsequently, electrons), controversy about which had given rise to quantum mechanics in the first place:

> This view is already clearly brought out by the much-discussed question of the nature of light and the ultimate constituents of matter. As regards light, its propagation in space and time is adequately expressed by the electromagnetic theory. Especially the interference phenomena *in vacuo* and the optical properties of material media are completely governed by the wave theory superposition principle. Nevertheless, the conservation of energy and momentum during the interaction between radiation and matter, as evident in the photoelectric and Compton effect, finds its adequate expression just in the light quantum idea put forward by Einstein. As is well known, the doubts regarding the validity of the superposition principle on the one hand and of the conservation laws on the other, which were suggested by this apparent contradiction, have been definitely disproved through direct experiments. This situation would seem clearly to indicate the impossibility of a causal space-time description of the light phenomena. On one hand, in attempting to trace the laws of the time-spatial propagation of light according to the quantum postulate, we are confined to statistical considerations. On the other hand, the fulfilment of the claim of causality for the individual light processes, characterised by the quantum of action, entails a renunciation as regards the space-time description. Of course, there can be no question of a quite independent application of the ideas of space and time and of causality. The two views of the nature of light are rather to be considered as different attempts at an interpretation of experimental evidence in which the limitation of the classical concepts is expressed in complementary ways.
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> The problem of the nature of the constituents of matter presents us with an analogous situation. The individuality of the elementary electrical corpuscles is forced upon us by general evidence. Nevertheless, recent experience, above all the discovery of the selective reflection of electrons from metal crystals, requires the use of the wave theory superposition principle in accordance with the ideas of L. de Broglie. Just as in the case of light, we have consequently in the question of the nature of matter, so far as we adhere to classical concepts, to face an inevitable dilemma, which has to be regarded as the very expression of experimental evidence. In fact, here again we are not dealing with contradictory but with complementary pictures of the phenomena, which only together offer a natural generalisation of the classical mode of description. In the discussion of these questions, it must be kept in mind that, according to the view taken above, radiation in free space as well as isolated material particles are abstractions, their properties on the quantum theory being definable and observable only through their interaction with other systems. Nevertheless, these abstractions are, as we shall see, indispensable for a description of experimental evidence in connexion with our ordinary space-time view [1].

Here Bohr stresses that each side of the wave-particle duality has a secure foundation in experimental evidence: for light, for example, the continuous space-time propagation as described by Maxwell's equations is required to account for interference phenomena, whereas Einstein's "light quantum" (i.e., "light particle" or "photon") picture is necessary to account for phenomena such as the photoelectric effect and Compton scattering. Bohr's view seems to be that if we take either picture too seriously – i.e., if we take either picture as capturing, fully and finally, the true physical nature of light – we would have a clear contradiction with some aspect of the experimental evidence which can only be described by the alternative picture. So, for Bohr, we must not take either picture fully seriously: the contradiction is merely "apparent". Yet, simultaneously, we must take both pictures quite seriously, in the sense that only together do they allow us to understand the totality of experimental evidence. The two pictures, that is, are mutually exclusive (in the sense that, taken as capturing the full truth, they contradict one another) and yet jointly exhaustive (in the sense that we need both, together, to capture all aspects of the phenomena revealed by observation).

It is interesting, here, to compare Bohr's view with another possible interpretation of the wave-particle duality. Einstein, for example, considered (during this same period) a "pilot-wave" model of photons, in which the wave-particle "duality" is taken quite literally: each individual "photon" in this model consists of a literal point particle (carrying the energy) which is guided (or piloted) by a surrounding wave obeying Maxwell's equations. We will explore this type of model further (but for massive particles rather than photons) in Chap. 7. For now, the point is just that there seem to exist various ways that one might consider really reconciling – *unifying* – the aspects that Bohr considers "complementary". Doing this of course requires modifying the classical concepts. For example, in this pilot-wave model of photons, there is still a wave obeying Maxwell's equations, but its role is completely different – instead of actually being the seat of light's energy and momentum, it is a kind of behind-the-scenes "ghost", pushing and pulling the associated photon particle.[1] And similarly, although there is a particle with a definite trajectory, it does not obey the familiar dynamical laws of Newtonian mechanics, but instead something completely novel which gives rise to all sorts of unexpected and surprising motions (e.g., when a photon reflects from a mirror, the particle stops and sits still for some time some distance in front of the mirror!). In any case, this example illustrates, I think, the attitude that people like Einstein and Schrödinger had toward the "apparent contradiction" Bohr mentions here. They, like Bohr, saw the conflicts as pointing to inadequacies in the existing models. But they took for granted that it should be possible to build new theories – new pictures of microscopic reality with new associated dynamical laws – that would unify and explain *all* available experimental evidence.

But Bohr would have none of this. For Bohr, the classical models may be "abstractions" (whose domain of applicability we stretch when we use them to describe the microscopic world), but they are *necessary* – almost "hard-wired" in a kind of Kantian sense – abstractions that can not and/or should not be abandoned, modified, or replaced. For Bohr, the quantum theory was not so much an attempt to accurately describe microscopic reality (this being supposedly impossible, for the philosophical reasons we have been sketching) but was rather a formal and precise mathematical scheme to referee disputes between complementary (i.e., individually inadequate but still jointly necessary) perspectives.

Here again the Heisenberg uncertainty relations are crucial and central. As Bohr explains,

> ...in the classical theories any succeeding observation permits a prediction of future events with ever-increasing accuracy, because it improves our knowledge of the initial state of the system. According to the quantum theory, just the impossibility of neglecting the interaction with the agency of measurement means that every observation introduces a new uncontrollable element. Indeed, it follows from the above considerations that the measurement of the positional coordinates of a particle is accompanied not only by a finite change in the dynamical variables, but also the fixation of its position means a complete rupture in the causal description of its dynamical behaviour, while the determination of its momentum always implies a gap in the knowledge of its spatial propagation. Just this situation brings

[1] Einstein literally called it a "Gespensterfeld", ghost-field.

> out most strikingly the complementary character of the description of atomic phenomena which appears as an inevitable consequence of the contrast between the quantum postulate and the distinction between object and agency of measurement, inherent in our very idea of observation [1].

We will hear more about the connection between the uncertainty principle and the Copenhagen interpretation from Heisenberg himself, in the following section.

Before turning to that, however, here is one last excerpt from Bohr's Como lecture, in which he discusses Schrödinger's wave mechanics and echoes some of the issues we reviewed in the previous chapter:

> ...Schrödinger has expressed the hope that the development of the wave theory will eventually remove the irrational element expressed by the quantum postulate and open the way for a complete description of atomic phenomena along the line of the classical theories. In support of this view, Schrödinger, in a recent paper (...) emphasises the fact that the discontinuous exchange of energy between atoms required by the quantum postulate, from the point of view of the wave theory, is replaced by a simple resonance phenomenon. In particular, the idea of individual stationary states would be an illusion and its applicability only an illustration of the resonance mentioned. It must be kept in mind, however, that just in the resonance problem mentioned we are concerned with a closed system which, according to the view presented here, is not accessible to observation. In fact, wave mechanics ... represents a symbolic transcription of the problem of motion of classical mechanics adapted to the requirements of quantum theory and only to be interpreted by an explicit use of the quantum postulate. ....
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> The symbolical character of Schrödinger's method appears not only from the circumstance that its simplicity ... depends essentially upon the use of imaginary arithmetic quantities. But above all there can be no question of an immediate connexion with our ordinary conceptions because the 'geometrical' problem represented by the wave equation is associated with the so-called co-ordinate [i.e., configuration] space, the number of dimensions of which is equal to the number of degrees of freedom of the system, and hence in general greater than the number of dimensions of ordinary space. Further, Schrödinger's formulation of the interaction problem ... involves a neglect of the finite velocity of propagation of the forces claimed by relativity theory.
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> On the whole, it would scarcely seem justifiable, in the case of the interaction problem, to demand a visualisation by means of ordinary space-time pictures. In fact, all our knowledge concerning the internal properties of atoms is derived from experiments on their radiation or collision reactions, such that the interpretation of experimental facts ultimately depends on the abstractions of radiation in free space, and free material particles. Hence, our whole space-time view of physical phenomena, as well as the definition of energy and momentum, depends ultimately upon these abstractions. In judging the applications of these auxiliary ideas we should only demand inner consistency, in which connexion special regard has to be paid to the possibilities of definition and observation [1].

Bohr's description of Schrödinger's waves as "symbolical" – on (largely) the grounds that, as waves in *configuration* space, the wave functions clearly cannot be taken seriously as physically real – is particularly interesting. It should be becoming clear that, whatever exactly Bohr and his colleagues may have meant when they made claims implying the *completeness* of the quantum mechanical description, it was not exactly the same kind of thing that Einstein and Schrödinger meant by this same word, and against which their objections were made.[2] For Bohr and the other Copenhagenists, the completeness of quantum mechanics did not mean that the theory provides a literal and direct and exhaustive description of the physical states of external objects. Indeed, as we have seen, for Bohr, the essential lesson of quantum theory is precisely that such an exhaustive description is, for supposedly deep philosophical reasons, impossible to achieve and thus inappropriate to seek. For Bohr, "completeness" is used instead in an epistemological or semantic sense (rather than the realist or descriptive sense we have largely assumed in earlier chapters) – something less along the lines of "no aspect of objective reality has been missed" and instead more along the lines of "you can't rationally ask for anything more (than this formal refereeing between the complementary classical concepts) without lapsing into meaningless, unscientific, metaphysical talk".

This point of view will become somewhat clearer when we review Bohr's analysis of some concrete examples. But first let's consider the Copenhagen interpretation as explained by its second-most-important proponent, Werner Heisenberg.

6.2 Heisenberg

In Heisenberg's writings, one finds an overall agreement with the perspectives of Bohr. But Heisenberg is a little simpler and a little more practical – a little less careful and a lot less philosophically grandiose – in his way of expressing himself. Let us begin here by giving the overall flavor of Heisenberg's style by quoting, "rapid-fire," some excerpts from his essay on "The History of Quantum Theory" [4]:

- "... from this time on ... the physicists learned to ask the right questions.... What were these questions? Practically all of them had to do with the strange apparent contradictions between the results of different experiments. How could it be that the same radiation that produces interference patterns, and therefore must consist of waves, also produces the photoelectric effect, and therefore must consist of particles? How could it be that the frequency of the orbital motion of the electron in the atom does not show up in the frequency of the emitted radiation? .... Again and again one found that the attempts to describe atomic events in the traditional terms of physics led to contradictions."
- "Gradually, during the early twenties, the physicists became accustomed to these difficulties, they acquired a certain vague knowledge about where trouble would occur, and they learned to avoid contradictions. .... This was not sufficient to form a consistent general picture of what happens in a quantum process, but it changed the minds of the physicists in such a way that they somehow got into the spirit of quantum theory."

[2] The "completeness" claim was made at least as early as 1927 when Max Born and Heisenberg declared: "We maintain that quantum mechanics is a complete theory; its basic physical and mathematical hypotheses are not further susceptible of modifications" [3].

- "The strangest experience of those years was that the paradoxes of quantum theory did not disappear during this process of clarification; on the contrary, they became even more marked and more exciting."
- "The two experiments – one on the interference of scattered light and the other on the change of frequency of the scattered light – seemed to contradict each other without any possibility of compromise."
- "But in what sense did the new formalism describe the atom? The paradoxes of the dualism between wave picture and particle picture were not solved; they were hidden somehow in the mathematical scheme."
- "The electromagnetic waves were interpreted not as 'real' waves but as probability waves, the intensity of which determines in every point the probability for the absorption ... of a light quantum by an atom at this point."
- "The probability wave ... meant a tendency for something. It was a quantitative version of the old concept of 'potentia' in Aristotelian philosophy. It introduced something standing in the middle between the idea of an event and the actual event, a strange kind of physical reality just in the middle between possibility and reality."
- "Bohr considered the two pictures – particle picture and wave picture – as two complementary descriptions of the same reality. Any of these descriptions can be only partially true, there must be limitations to the use of the particle concept as well as of the wave concept, else one could not avoid contradictions. If one takes into account those limitations which can be expressed by the uncertainty relations, the contradictions disappear."

Leaving aside their very different manners of expression, the biggest difference between the views of Heisenberg and Bohr is probably on this point of the wave function (i.e., "probability wave") representing some kind of at least half- or proto-real thing. Heisenberg's view here is much closer to the spirit of the view that, for example, Einstein had criticized – as seemingly in conflict with the principle of locality – in his "boxes" type arguments. We will return to this point later in the chapter.

For now, let us turn to one of Heisenberg's more careful attempts to articulate the Copenhagen philosophy. The first paragraph of his essay (written later, in the 1950s) on "The Copenhagen Interpretation of Quantum Theory" [5] contains a nice summary of the ideas we reviewed in the previous section:

> The Copenhagen interpretation of quantum theory starts from a paradox. Any experiment in physics, whether it refers to the phenomena of daily life or to atomic events, is to be described in the terms of classical physics. The concepts of classical physics form the language by which we describe the arrangement of our experiments and state the results. We cannot and should not replace these concepts by any others. Still the application of these concepts is limited by the relations of uncertainty. We must keep in mind this limited range of applicability of the classical concepts while using them, but we cannot and should not try to improve them [5].

The last sentence there, to me, captures the essence of the Copenhagen interpretation: because we must continue to use the classical concepts, even while acknowledging their limitations, we must in some deep sense renounce the goal of attempting to understand and explain quantum phenomena in the clear, consistent, unified, literal way that had always been aimed at in "classical" physics.

Heisenberg elaborates his view about the nature of the quantum mechanical wave function later in the same article:

> ...it is useful to compare the procedure for the theoretical interpretation of an experiment in classical physics and in quantum theory. In Newton's mechanics, for instance, we may start by measuring the position and the velocity of the planet whose motion we are going to study. The result of the observation is translated into mathematics by deriving numbers for the co-ordinates and the momenta of the planet from the observation. Then the equations of motion are used to derive from these values of the co-ordinates and momenta at a given time the values of these co-ordinates ... at a later time, and in this way the astronomer can predict the properties of the system at a later time. He can, for instance, predict the exact time for an eclipse of the moon.
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> In quantum theory the procedure is slightly different. We could for instance be interested in the motion of an electron through a cloud chamber and could determine by some kind of observation the initial position and velocity of the electron. But this determination will not be accurate; it will at least contain the inaccuracies following from the uncertainty relations and will probably contain still larger errors due to the difficulty of the experiment. It is the first of these inaccuracies which allows us to translate the result of the observation into the mathematical scheme of quantum theory. A probability function is written down which represents the experimental situation at the time of the measurement, including even the possible errors of the measurement.
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> This probability function represents a mixture of two things, partly a fact and partly our knowledge of a fact. It represents a fact in so far as it assigns at the initial time the probability unity (i.e., complete certainty) to the initial situation: the electron moving with the observed velocity at the observed position; 'observed' means observed within the accuracy of the experiment. It represents our knowledge in so far as another observer could perhaps know the position of the electron more accurately. The error in the experiment does – at least to some extent – not represent a property of the electron but a deficiency in our knowledge of the electron. Also this deficiency of knowledge is expressed in the probability function.
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> In classical physics one should in a careful investigation also consider the error of the observation. As a result one would get a probability distribution for the initial values of the co-ordinates and velocities and therefore something very similar to the probability function in quantum mechanics. Only the necessary uncertainty due to the uncertainty relations is lacking in classical physics.
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> When the probability function in quantum theory has been determined at the initial time from the observation, one can from the laws of quantum theory calculate the probability function at any later time and can thereby determine the probability for a measurement giving a specified value of the measured quantity. We can, for instance, predict the probability for finding the electron at a later time at a given point in the cloud chamber. It should be emphasized, however, that the probability function does not in itself represent a course of events in the course of time. It represents a tendency for events and our knowledge of events. The probability function can be connected with reality only if one essential condition is fulfilled: if a new measurement is made to determine a certain property of the system. Only then does the probability function allow us to calculate the probable result of the new measurement. The result of the measurement again will be stated in terms of classical physics.
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> Therefore, the theoretical interpretation of an experiment requires three distinct steps: (1) the translation of the initial experimental situation into a probability function; (2) the following up of this function in the course of time; (3) the statement of a new measurement to be made of the system, the result of which can then be calculated from the probability function. For the first step the fulfillment of the uncertainty relations is a necessary condition. The second

> step cannot be described in terms of the classical concepts; there is no description of what happens to the system between the initial observation and the next measurement. It is only in the third step that we change over again from the 'possible' to the 'actual' [5].

This passage raises a number of questions about how Heisenberg's views relate to Bohr's views as well as to the worries discussed in previous chapters. I'll invite you to think about some of these issues in the Projects.

Heisenberg's positivist philosophy is also on display in this essay. For example, in discussing the idea of electrons orbiting nuclei in atoms, he remarks: "one can never observe more than one point in the orbit of the electron; therefore, there is no orbit in the ordinary sense" [5]. What the electron does between observations is thus dismissed not merely as unknowable (and thus not meaningful to speak of) but as altogether non-existent. Indeed, this kind of inference – from unknowability to unreality – pushes beyond mere positivism and recalls the idealist philosophy of, for example, Bishop George Berkeley, who famously decreed "esse est percipi" – "to be, is to be perceived". The extent to which this sort of anti-realism, about (at least) the microscopic quantum realm, should be considered an official part of the Copenhagen doctrine, is one of those controversial issues about which there is no real consensus.

Heisenberg's continuing elaboration provides an illuminating perspective on Bohr's concept of "complementarity":

> Actually we need not speak of particles at all. For many experiments it is more convenient to speak of matter waves; for instance, of stationary matter waves around the atomic nucleus. Such a description would directly contradict the other description if one does not pay attention to the limitations given by the uncertainty relations. Through the limitations the contradiction is avoided. The use of 'matter waves' is convenient, for example, when dealing with the radiation emitted by the atom. By means of its frequencies and intensities the radiation gives information about the oscillating charge distribution in the atom, and there the wave picture comes much nearer to the truth than the particle picture. Therefore, Bohr advocated the use of both pictures, which he called 'complementary' to each other. The two pictures are of course mutually exclusive, because a certain thing cannot at the same time be a particle (i.e., substance confined to a very small volume) and a wave (i.e., a field spread out over a large space), but the two complement each other. By playing with both pictures, by going from the one picture to the other and back again, we finally get the right impression of the strange kind of reality behind our atomic experiments [5].

Once again, not only in the style of the writing, but also in some of the content of his remarks, one senses that Heisenberg's understanding of "complementarity" is a little more easy-going and pragmatic than Bohr's. For example, one doubts that Bohr would agree with Heisenberg's statement that the 'matter wave' picture "comes much nearer to the truth" in its description of the electrons orbiting a nucleus in an atom. This kind of (perhaps inadvertent) concession to the existence of some "real truth" about such things leaves Heisenberg, I think, much more open to the kinds of criticisms we reviewed in the last few chapters. Whereas Bohr's dense prose functions more effectively as an impenetrable barrier against such attacks.

Heisenberg returns to the theme of anti-realism (about unobserved microscopic phenomena) in his comments on the double-slit experiment:

> We assume that a small source of monochromatic light radiates toward a black screen with two small holes in it. The diameter of the holes may be not much bigger than the wave length of the light, but their [separation] will be very much bigger. At some distance behind the screen a photographic plate registers the incident light. If one describes this experiment in terms of the wave picture, one says that the primary wave penetrates through the two holes; there will be secondary spherical waves starting from the holes that interfere with one another, and the interference will produce a pattern of varying intensity on the photographic plate.
>
> The blackening of the photographic plate is a quantum process, a chemical reaction produced by a single light quanta. Therefore, it must also be possible to describe the experiment in terms of light quanta. If it would be permissible to say what happens to the single light quantum between its emission from the light source and its absorption in the photographic plate, one could argue as follows: The single light quantum can come through the first hole or through the second one. If it goes through the first hole and is scattered there, its probability for being absorbed at a certain point of the photographic plate cannot depend upon whether the second hole is closed or open. The probability distribution on the plate will be the same as if only the first hole was open. If the experiment is repeated many times and one takes together all cases in which the light quantum has gone through the first hole, the blackening of the plate due to these cases will correspond to this probability distribution. If one considers only those light quanta that go through the second hole, the blackening should correspond to a probability distribution derived from the assumption that only the second hole is open. The total blackening, therefore, should just be the sum of the blackenings in the two cases; in other words, there should be no interference pattern. But we know this is not correct, and the experiment will show the interference pattern. Therefore, the statement that any light quantum must have gone *either* through the first *or* through the second hole is problematic and leads to contradictions. This example shows clearly that the concept of the probability function does not allow a description of what happens between two observations. Any attempt to find such a description would lead to contradictions; this must mean that the term 'happens' is restricted to the observation [5].

One may have questions about why, in the case of an electron in an atom, "the wave picture comes much nearer to the truth", whereas in the case of a particle traversing a double-slit apparatus the wave does not in any sense provide a realistic "description of what happens". And of course one may also have philosophical concerns about the idea that nothing happens beyond that which is observed. Here I will just point out that, in addition to such philosophical concerns, one might also have (to use Bell's terminology from Chap. 3) "professional" concerns about this idea: if, according to quantum mechanics, physical reality (what "happens") is restricted, somehow, to observation, shouldn't we insist on a sharp definition of "observation", i.e., a clear discrimination between those interactions which do, and those which do not, count as "observations" and thereby give rise to real physical "happenings"? Otherwise the theory's account of what is real would necessarily remain "unprofessionally vague and ambiguous". But of course, such a concern presupposes something that Heisenberg and Bohr apparently did not accept – namely, that it is the proper goal of a physical theory to provide a clear and unambiguous account of what is real.

In terms of the quantum mechanical formalism, the question about the precise meaning of "observation" becomes the question of how to understand, and when precisely to apply, the postulate of wave function collapse. About this Heisenberg writes:

> The observation itself changes the probability function discontinuously; it selects of all possible events the actual one that has taken place. Since through the observation our knowledge of the system has changed discontinuously, its mathematical representation also has undergone the discontinuous change and we speak of a 'quantum jump'.
>
> Therefore, the transition from the 'possible' to the 'actual' takes place during the act of observation. If we want to describe what happens in an atomic event, we have to realize that the word 'happens' can apply only to the observation, not to the state of affairs between two observations. It applies to the physical, not the psychical act of observation, and we may say that the transition from the 'possible' to the 'actual' takes place as soon as the interaction of the object with the measuring device, and thereby with the rest of the world has come into play; it is not connected with the act of registration of the result by the mind of the observer. The discontinuous change in the probability function, however, takes place with the act of registration, because it is the discontinuous change of our knowledge in the instant of registration that has its image in the discontinuous change of the probability function [5].

One can see here, again, how Heisenberg's formulations invite some of the objections we have discussed previously. For example, if the change in the quantum state (induced by observation) merely represents a change in our knowledge of the system, doesn't that imply that the observation is simply revealing a fact about the observed system which was perfectly definite (though unknown) prior to the observation, such that the (earlier) quantum mechanical description was simply incomplete?

But on the other hand, we also begin to appreciate the very different underlying philosophical perspective that immunized Bohr and Heisenberg against such objections: if, for example, reference to unknown or unobserved elements of physical reality is literally meaningless, then the clean division between the epistemic and ontological interpretations of wave function collapse dissolves and the incompleteness objection loses its force.

Heisenberg continues, addressing (what would later become) Bell's objection that the vagueness and arbitrariness of the division of the world implied by the distinction between "observation processes" and "regular processes":

> It has been said that we always start with a division of the world into an object, which we are going to study, and the rest of the world, and that this division is to some extent arbitrary. It should indeed not make any difference in the final result if we, e.g., add some part of the measuring device or the whole device to the object and apply the laws of quantum theory to this more complicated object. It can be shown that such an alteration of the theoretical treatment would not alter the predictions concerning a given experiment. This follows mathematically from the fact that the laws of quantum theory are for the phenomena in which Planck's constant can be considered as a very small quantity, approximately identical with the classical laws. But it would be a mistake to believe that this application of the quantum theoretical laws to the measuring device could help to avoid the fundamental paradox of quantum theory.
>
> The measuring device deserves this name only if it is in close contact with the rest of the world, if there is an interaction between the device and the observer. Therefore, the uncertainty with respect to the microscopic behavior of the world will enter into the quantum-theoretical system here just as well as in the first interpretation. If the measuring device would be isolated from the rest of the world, it would be neither a measuring device nor could it be described in the terms of classical physics at all. ....
>
> Certainly quantum theory does not contain genuine subjective features, it does not introduce the mind of the physicist as a part of the atomic event. But it starts from the division of the

> world into the 'object' and the rest of the world, and from the fact that at least for the rest of the world we use the classical concepts in our description. This division is arbitrary and historically a direct consequence of our scientific method; the use of the classical concepts is finally a consequence of the general human way of thinking. But this is already a reference to ourselves and ... so ... our description is not completely objective [5].

Heisenberg thus rather explicitly acknowledges the criticisms of the theory that started our discussion of the measurement problem in Chap. 3: the theory indeed "starts from the division of the world into the 'object' and the rest of the world", this "division is arbitrary", and the systems on the two sides of the division are to be described in radically different theoretical terms.

But, again, on the other hand, it also becomes increasingly clear that Heisenberg does not see any of this as some sort of fatal flaw in the way that the critics (Schrödinger, Einstein, Bell, etc.) did. For Heisenberg, the theory is simply not an attempt to provide a literal, realistic description of the world. Its structure – and in particular the fact that it necessarily divides the world into two realms which are described very differently – should instead be understood as having merely an epistemological significance, growing out of the nature of "our scientific method" and "the general human way of thinking". The theory, in short, should be understood less as an attempt to provide an objective description of nature, and more as a kind of practical algorithm (with few if any ontological commitments) for making empirical predictions.

From Heisenberg's point of view, then, the criticisms of the critics are largely misplaced – even though, he would have to admit, the theory does fail to provide the kind of literal, direct description of physical processes that the critics ultimately wanted. According to the Copenhagen philosophy, however, this is no kind of deficiency in the quantum theory. Instead, from the point of view of Bohr and Heisenberg, the flaw lies in the misplaced demands of the critics: what they want, according to the Copenhagen point of view, is unattainable and indeed at odds with the nature of human scientific knowledge, so they are simply wrong to want it.

Let's give Heisenberg the final word:

> ...it has sometimes been suggested that one should depart from the classical concepts altogether and that a radical change in the concepts used for describing the experiments might possibly lead back to a nonstatical, completely objective description of nature.
>
> This suggestion, however, rests upon a misunderstanding. The concepts of classical physics are just a refinement of the concepts of daily life and are an essential part of the language which forms the basis of all natural science. Our actual situation in science is such that we *do* use the classical concepts for the description of the experiments, and it was the problem of quantum theory to find theoretical interpretation of the experiments on that basis. There is no use in discussing what could be done if we were other beings than we are [5].

6.3 Bohr on Einstein's Diffraction Example

In the last two sections, we've attempted to give a broad philosophical overview of the Copenhagen interpretation, closely grounded in the writings of Bohr and Heisenberg. We discussed, again in very abstract terms, some of the ways in which their views

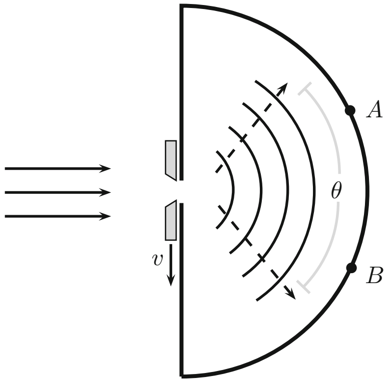

Fig. 6.1 Updated version of Bohr's illustration of Einstein's diffraction example (compare to the earlier Fig. 4.1). The diffracted wave has an angular spread θ implying appreciable probability for the particle to localize at many different points including A and B. Einstein's argument was that the (anti-) correlation between seeing the particle at A and seeing it at B implied either a kind of spooky action at a distance, or that the particle had a definite location all along (such that the description in terms of a diffracting wave was incomplete). In his version of the setup, Bohr also includes a moveable aperture (the gray trapezoids) which can be slid in place as shown – so there is a single slit of width a – or moved down at speed v to block the slit

seem to relate to the sorts of criticisms we reviewed in the previous three chapters. But Bohr, especially, engaged very directly with the critics, especially Einstein, on several example scenarios where their different points of view can be seen to clash in much more concrete terms. In this section (and the two following ones) we will thus turn to further elucidating the Copenhagen interpretation in the context of a few of these concrete examples.

We begin with Bohr's discussion of the example Einstein raised at the 1927 Solvay conference (which we discussed in Chap. 4 in the "Einstein's Boxes" section). Bohr discusses this in his beautifully written and rightly famous 1949 reminiscence, "Discussion with Einstein on Epistemological Problems in Atomic Physics" [6].

Bohr begins his discussion by summarizing Einstein's example as follows:

> To illustrate his attitude, Einstein referred ... to the simple example illustrated by [Fig. 6.1], of a particle (electron or photon) penetrating through a hole or a narrow slit in a diaphragm placed at some distance before a photographic plate. On account of the diffraction of the wave connected with the motion of the particle and indicated in the figure by the thin lines, it is under such conditions not possible to predict with certainty at what point the electron will arrive at the photographic plate, but only to calculate the probability that, in an experiment, the electron will be found within any given region of the plate. The apparent difficulty, in this description, which Einstein felt so acutely, is the fact that, if in the experiment the electron is recorded at one point A ... then it is out of the question of ever observing an effect of this electron at another point (B), although the laws of ordinary wave propagation offer no room for a correlation between two such events.
>
> Einstein's attitude gave rise to ardent discussions.... [which] centered on the question of whether the quantum-mechanical description exhausted the possibilities of accounting for observable phenomena or, as Einstein maintained, the analysis could be carried further and, especially, of whether a fuller description of the phenomena could be obtained by bringing into consideration the detailed balance of energy and momentum in individual processes [6].

The first paragraph seems like a perfectly good summary of Einstein's arguments (although the role of "locality" is perhaps not adequately stressed). But Bohr doesn't seem to have understood Einstein's argument (that, if one assumes locality, a full description of the physical state of the system must include more facts than are contained in the quantum description) as the primary issue here. Instead, Bohr focuses on analyzing the suggestion that, by monitoring the recoil of the diaphragm, one might improve one's ability to predict where the particle might eventually be detected: for example, assuming the incident particle and the diaphragm have no initial vertical momentum, then if (prior to the particle's arrival at the screen) one observes that the diaphragm has acquired (say) a *downward* momentum, it must be (assuming momentum conservation) that the particle has deflected *upward*, toward (say) point A rather than point B.

Recall from Chap. 4 that, according to Einstein, the application of the locality concept to this example requires that one "not describe the process solely by the Schrödinger wave, but that at the same time one localises the particle during the propagation." What Einstein meant to be arguing for, that is, is the claim that the particle *has* a definite location (say, near A or near B) even before it is observed. For Einstein, this reality claim would stand independently of whether or not the pre-measurement location of the particle could be (in some indirect sense, as for example by monitoring the recoil of the diaphragm) determined, and, indeed, whether or not the inclusion of the particle trajectory in one's theoretical description would change the operational predictions. For Bohr, though, the idea of a physical reality which is unobservable and/or irrelevant to theoretical predictions is a kind of contradiction in terms. So it makes sense to some degree that Bohr interpreted Einstein as arguing that it should be possible to improve (beyond what is allowed by ordinary quantum theory) one's practical ability to predict where the particle will hit the screen. This explains why Bohr's analysis of Einstein's diffraction example focuses on defending the self-consistency of the limitations placed on the theory's predictive accuracy by the Heisenberg uncertainty formulas.

That analysis proceeds as follows. Suppose the incoming particle has momentum $p = h/\lambda$ and the slit (when open) has width a. Then the particle will acquire, assuming it passes the slit, an uncertainty in its transverse position $\Delta q \approx a$. Then the standard relation for the angular width of a diffraction pattern ($\theta \approx \lambda/a$) implies that there is an uncertainty in the transverse component of the particle's momentum of order

$$\Delta p \approx \theta \cdot p \approx \frac{h}{\Delta q} \tag{6.1}$$

which is just the usual Heisenberg uncertainty relation. As Bohr notes: "This result could, of course, also be obtained directly by noticing that, due to the limited extension of the wave-field at the place of the slit, the component of the wave-number parallel to the plane of the diaphragm will involve a latitude $\Delta k \approx (1/a) \approx (1/\Delta q)$."

Now, if we suppose that the shutter opens the width-a slit only for a time Δt, the wave packet will have a spread of frequencies of width

$$\Delta\nu \approx \frac{1}{\Delta t} \tag{6.2}$$

which then implies, using the usual quantum energy-frequency relation $E = h\nu$, an uncertainty in the particle's energy of order

$$\Delta E \approx h\,\Delta\nu \approx h/\Delta t. \tag{6.3}$$

This is again the usual (energy-time) Heisenberg uncertainty relation.

Bohr then raises the question of where these latitudes in the particle's momentum and energy come from. That is, if ΔE and Δp represent the expected sizes of *changes* in the energy and momentum of the particle as it traverses the slit – and if the total energy and total momentum of an isolated system are strictly conserved – where do the new contributions to the energy and momentum of the particle come from? Bohr explains:

> From the point of view of the laws of conservation, the origin of such latitudes entering into the description of the state of the particle after passing through the hole may be traced to the possibilities of momentum and energy exchange with the diaphragm or the shutter. In the reference system [of the Figure], the velocity of the diaphragm may be disregarded and only a change of momentum Δp between the particle and the diaphragm needs to be taken into consideration. The shutter, however, which leaves the hole opened during the time Δt, moves with a considerable velocity $v \approx a/\Delta t$, and a momentum transfer Δp involves therefore an energy exchange with the particle, amounting to [$\Delta E = \int vF(t)dt \approx$]$v\,\Delta p \approx (1/\Delta t)\Delta q\Delta p \approx h/\Delta t$, being just of the same order of magnitude as the latitude ΔE given by [Eq. (6.3)] and, thus, allowing for momentum and energy balance [6].

With all of that laid out, Bohr then turns to confront Einstein's concerns (as he understood them) more directly. If, for example, the position and momentum of the diaphragm itself are carefully controlled and monitored, and conservation of energy and momentum are assumed in the interaction between the diaphragm and the passing particle, could one predict the subsequent behavior of the particle more accurately than would be allowed according to the quantum description?

> The problem raised by Einstein was now to what extent a control of the momentum and energy transfer, involved in a location of the particle in space and time, can be used for a further specification of the state of the particle after passing through the hole. Here, it must be taken into consideration that the position and the motion of the diaphragm and the shutter have so far been assumed to be accurately co-ordinated with the space-time reference frame. This assumption implies, in the description of the state of these bodies, an essential latitude as to their momentum and energy which need not, of course, noticeably affect the velocities, if the diaphragm and the shutter are sufficiently heavy. However, as soon as we want to know the momentum and energy of these parts of the measuring arrangement with an accuracy sufficient to control the momentum and energy exchange with the particle under investigation, we shall, in accordance with the general indeterminacy relations, lose the possibility of their accurate location in space and time. We have, therefore, to examine how far this circumstance will affect the intended use of the whole arrangement and, as we shall see, this crucial point clearly brings out the complementary character of the phenomenon [6].

Bohr means here that, up to now, we have assumed that the diaphragm is rigidly fixed in place so that, for example, its velocity is exactly zero and its position (the location

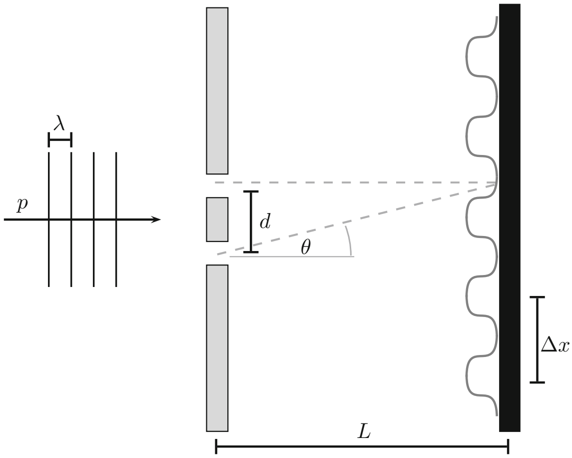

Fig. 6.2 The setup of the two-slit experiment discussed by Bohr in Ref. [6]. The *dashed grey lines* indicate two paths, differing in angle by θ, which a particle might take to some point on the screen

of the slit that the particle goes through) is precisely known. To make this concrete, one might imagine that the diaphragm structure is physically bolted down to the solid earth. But such bolts would allow and indeed necessitate a physical interaction, by means of which arbitrarily large quantities of energy and momentum might be exchanged between the diaphragm and the earth. So by bolting the diaphragm down (and thus precisely fixing its spatial location) we lose all control of its energy and momentum – and hence lose any ability to infer, from some hypothetical later measurement of its energy or momentum, any further information about the location of the now-distant particle.

Of course, by unbolting the diaphragm and, say, letting it glide freely along a frictionless track (running vertically in the Figure), we could remove the ability of the diaphragm to exchange energy and momentum with the earth, and thereby recover the ability to infer, from a later measurement of the energy or momentum of the diaphragm, something about the energy or momentum of the now-distant particle. But then, by the uncertainty principle, the spatial location of the diaphragm (and hence that of the particle) would become completely undefined. Thus, in a broad qualitative sense, Einstein's idea, as understood by Bohr, seems doomed.

Bohr proceeds to develop a closely-related example in which these ideas can more easily be analyzed quantitatively:

> The importance of considerations of this kind was, in the course of the discussions, most interestingly illuminated by the examination of an arrangement [involving a] diaphragm with two parallel slits, as is shown in [Fig. 6.2]. If a parallel beam of electrons (or photons) falls from the left [we shall] observe on the plate an interference pattern indicated by the [dark grey curve]. With intense beams, this pattern is built up by the accumulation of a large number of individual processes, each giving rise to a small spot on the photographic plate, and the distribution of these spots follows a simple law derivable from the wave analysis. The same distribution should also be found in the statistical account of many experiments performed with beams so faint that in a single exposure only one electron (or photon) will arrive at the photographic plate at some spot.... Since, now, as indicated by the [dashed lines], the momentum transferred to the ... diaphragm ought to be different if the electron

> was assumed to pass through the upper or the lower slit ..., Einstein suggested that a control of the momentum transfer would permit a closer analysis of the phenomenon and, in particular, to decide through which of the two slits the electron had passed before arriving at the plate [6].

Einstein, that is, had the idea that by carefully monitoring the position and momentum of the diaphragm (with, now, two slits in it), one could infer (from the final location at which the particle hits the detection screen) which slit the particle had gone through, because the momentum transfer between the particle and the diaphragm would need to have been slightly different in the two cases.

Bohr then presents the following rebuttal of Einstein's idea. The incident particle has momentum $p = h/\lambda$. The key idea here is that the momentum transfer between the particle and the diaphragm will be different, depending on which slit the particle goes through. For simplicity, in the Figure we have shown the case where there is *no* vertical momentum transfer if the particle goes through the top slit, whereas if the particle goes through the bottom slit it must bend upward by angle θ which implies it acquires a vertical momentum component of magnitude $p\sin(\theta) \approx (h/\lambda)(d/L)$ where we have written $\sin(\theta)$ in terms of the slit-spacing d and screen-distance L shown in the figure.

Now the idea is supposed to be that, by monitoring the vertical momentum of the diaphragm, we can determine, after the particle has passed, which slit it must have gone through. This will require that we can discriminate between the case in which the particle goes through the upper slit and the case in which the particle goes through the lower slit. But this requires that the uncertainty ΔP on the vertical momentum of the *diaphragm* be less than the difference between the momenta imparted to it when the particle goes through the different slits:

$$\Delta P \leq \frac{h}{\lambda}\frac{d}{L}. \tag{6.4}$$

But then, if we apply Heisenberg's uncertainty principle *to the diaphragm* we see that it must also have an uncertainty in its vertical position satisfying

$$\Delta Q \geq \frac{h}{\Delta P} \geq \frac{\lambda \cdot L}{d}. \tag{6.5}$$

This, as it turns out, is a very interesting result, because it is exactly the distance Δx between interference fringes on the screen. Therefore, in order to be able to determine which slit the particle went through by subsequently monitoring the vertical momentum of the diaphragm, the vertical position of the diaphragm must be uncertain by an amount greater than the fringe spacing on the screen:

$$\Delta Q \geq \Delta x. \tag{6.6}$$

But this obviously means that the interference pattern will be washed out: from one particle to the next, the probability distribution for the particle to hit the screen will shift up and down randomly by a distance as big as the spacing between the interference fringes. And so the statistical pattern that builds up will no longer display the characteristic two-slit interference pattern.

Bohr summarizes the implications as follows:

> This point is of great logical consequence, since it is only the circumstance that we are presented with a choice of *either* tracing the path of the particle *or* observing interference effects, which allows us to escape from the paradoxical necessity of concluding that the behaviour of an electron or a photon should depend on the presence of a slit in the diaphragm through which it could be proved not to pass. We have here to do with a typical example of how the complementary phenomena appear under mutually exclusive experimental arrangements ... and are just faced with the impossibility, in the analysis of quantum effects, of drawing any sharp separation between an independent behaviour of atomic objects and their interaction with the measuring instruments which serve to define the conditions under which the phenomena occur [6].

The example thus shows not only that one cannot determine more details about the particle than is permitted according to the Heisenberg uncertainty principle, but also demonstrates the complementarity of the wave and particle descriptions: by adjusting the experimental arrangement in a way that allows an unambiguous determination of the particle's path, its wave character (namely, the appearance of interference) is thereby suppressed.

6.4 The Photon Box Thought Experiment

In his "Discussions with Einstein" essay, Bohr goes on to present another famous thought experiment that Einstein had proposed at the 1930 Solvay Conference:

> As an objection to the view that a control of the interchange of momentum and energy between the objects and the measuring instruments was excluded if these instruments should serve their purpose of defining the space-time frame of the phenomena, Einstein brought forward the argument that such control should be possible when the exigencies of relativity theory were taken into consideration. In particular, the general relationship between energy and mass, expressed in Einstein's famous formula
>
> $$E = mc^2$$
>
> should allow, by means of a simple weighing, to measure the total energy of any system and, thus, in principle to control the energy transferred to it when it interacts with an atomic object.
>
> As an arrangement suited for such purpose, Einstein proposed the device indicated in [Fig. 6.3], consisting of a box with a hole in its side, which could be opened or closed by a shutter moved by means of a clock-work within the box. If, in the beginning, the box contained a certain amount of radiation and the clock was set to open the shutter for a very short interval at a chosen time, it could be achieved that a single photon was released through the hole at a moment known with as great accuracy as desired. Moreover, it would apparently also be possible, by weighing the whole box before and after this event, to measure the energy of the photon with any accuracy wanted, in definite contradiction to the reciprocal indeterminacy of time and energy quantities in quantum mechanics [6].

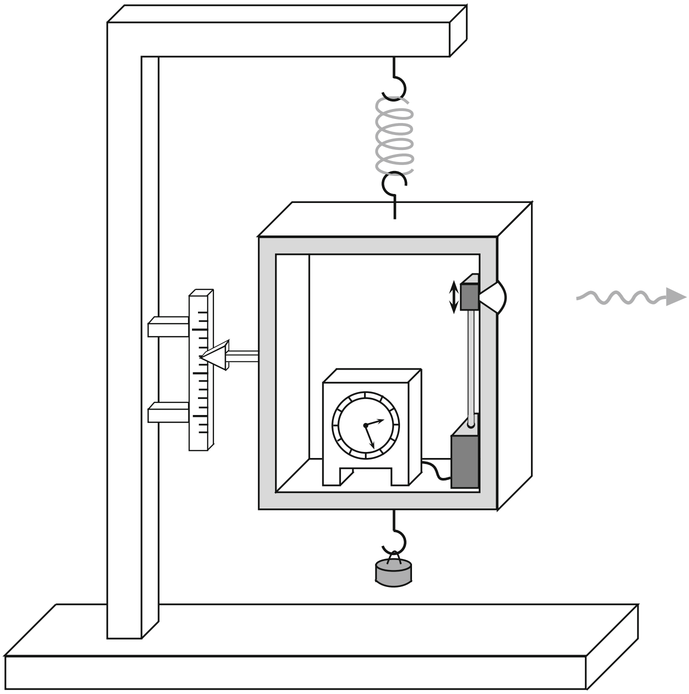

Fig. 6.3 Einstein's photon box setup as discussed by Bohr in Ref. [6]. An alarm clock inside the box triggers, at a pre-determined time as registered by the clock, a mechanical apparatus which briefly opens a shutter, allowing a single photon to escape to the *right*. The box hangs from a spring so that the weight of the box can be read from the scale *on the left*. After the photon is released, additional weight can be hung from the bottom of the box until the pointer returns to its original position, thus allowing a determination of the energy E of the escaped photon

So the idea is as follows: the clock mechanism inside the box will open the shutter at, say, time t, for a short duration Δt. During this period, a single photon will emerge from the aperture toward the right. The photon will be represented quantum mechanically as a wave packet of temporal duration Δt. This implies, by the standard energy-time uncertainty formula, that the energy of the photon will be "fuzzy" by an amount $\Delta E \geq h/\Delta t$.

Einstein's idea, however, was that the energy of the photon could be determined with arbitrary accuracy, after its emission, by carefully weighing the box from which the photon had emerged. And so the energy of the (now distant!) photon can be determined – and must hence be physically well-defined – to an accuracy greater than its quantum mechanical uncertainty ΔE. And so the quantum mechanical description must be incomplete.

According to Bohr, however, "it became clear ... that this argument could not be upheld." For, as Bohr goes on to explain, the process of weighing the box is itself subject to uncertainty principle constraints:

> The weighing of the box may ... be performed with any given accuracy Δm by adjusting the balance to its zero position by means of suitable loads. The essential point is now that any determination of this position with a given accuracy Δq will involve a minimum latitude Δp in the control of the momentum of the box connected with Δq by the [Heisenberg uncertainty principle]. This latitude must obviously again be smaller than the total impulse which, during the whole interval T of the balancing procedure, can be given by the gravitational field to a body with a mass Δm, or
>
> $$\Delta p \approx \frac{h}{\Delta q} < T \cdot g \cdot \Delta m$$
>
> where g is the gravity constant. The greater the accuracy of the reading q of the pointer, the longer must, consequently, be the balancing interval T, if a given accuracy Δm of the weighing of the box with its content shall be obtained.

Then, in a masterful judo-like move of using one of Einstein's greatest accomplishments against him, Bohr notes that

> according to general relativity theory, a clock, when displaced in the direction of the gravitational force by an amount of Δq, will change its rate in such a way that its reading in the course of a time interval T will differ by an amount ΔT given by the relation
>
> $$\frac{\Delta T}{T} = \frac{1}{c^2} g \Delta q.$$
>
> By comparing [the last two equations] we see, therefore, that after the weighing procedure there will in our knowledge of the adjustment of the clock be a latitude
>
> $$\Delta T > \frac{h}{c^2 \Delta m}.$$
>
> Together with [the formula $\Delta E = \Delta m\, c^2$ coming from Einstein's famous equation], this relation again leads to
>
> $$\Delta T \cdot \Delta E > h$$
>
> in accordance with the indeterminacy principle. Consequently, the use of the apparatus as a means of accurately measuring the energy of the photon will prevent us from controlling the moment of its escape [6].

Although the physics involved is rather more complicated, the conclusion here is essentially identical to what Bohr said in the analysis of the diffraction and interference experiments from the previous section: using the photon box in the intended way to determine, after its emission, both the energy and release-time of the photon, would require *mutually exclusive* experimental procedures. In particular, an accurate determination of the release-time of the photon requires that the box (with its internal clock) be held rigidly fixed in the background gravitational field; such fixation, though, allows energy transfer (with the earth or whatever the box is fixed to) and thus precludes subsequently inferring, from the weight of the box, the energy of the now-distant photon. And conversely, leaving the box free to oscillate vertically, so that the energy of the emitted photon can be reliably inferred, means that (due to the general relativistic time-dilation effect) the reading of the clock can no longer reliably indicate the release-time of the photon.

Thus – if it had indeed been Einstein's claim that the energy E and emission time T of the photon could both be determined, by subsequent examination of the box, to an accuracy greater than should be allowed by the Heisenberg uncertainty relations – then Bohr has shown that in fact, no, this is not after all possible. As long as we consistently apply the Heisenberg uncertainty principle to all elements of the physical system under examination (including the measuring equipment!) it seems to turn out that the uncertainty principle cannot be beaten. Bohr regards this as the essential proof for the completeness of the quantum mechanical description. And since in supposedly refuting Einstein's objection, Bohr had used Einstein's own general relativity against him, this episode is widely regarded as a rhetorical triumph for Bohr and the Copenhagen philosophy.

But was this, after all, Einstein's claim?

Interestingly, Bohr reports that, around 1933

> Einstein was far from satisfied and with his usual acuteness had discerned new aspects of the situation which strengthened his critical attitude. In fact, by further examining the possibilities for the application of the balance arrangement, Einstein had perceived alternative procedures which, even if they did not allow the use he originally intended, might seem to enhance the paradoxes beyond the possibilities of logical solution [6].

Such comments by Bohr have given rise to a widespread suggestion that, between 1930 and 1935 (when the EPR paper finally appeared) Einstein responded to his supposed defeats (in 1927 and 1930) by fundamentally changing his approach to criticizing the developing Copenhagen orthodoxy. In particular, according to this viewpoint, Einstein finally came to grips with the internal consistency of the theory and began to explore instead the "new aspects" that would appear explicitly in the 1935 EPR paper.

Recall from Sect. 4.1, however, the centrality of the concept of locality to Einstein's concerns as he expressed them already in 1927. As we have seen, Bohr's 1949 recapitulation of the 1927 discussions seem to completely omit the aspects of Einstein's concerns that render them quite in line with the later EPR argument. So one begins to suspect that the "new aspects" (of Einstein's thinking) which Bohr recognized only after 1930, were not new at all; instead, Bohr had simply failed to understand them prior to this point.

In regard to the photon box thought experiment, this suspicion would imply that the concept of locality played, somehow, a more central and important role than is apparent in Bohr's analysis. In particular, one suspects that, for Einstein, it was crucial that (after some time) the emitted photon is *spatially separated* from the box. Locality would then seem to imply that our choice of which measuring procedure to implement on the box, could have no effect on the physical state of the distant photon. One suspects, that is, that for Einstein locality was the crucial assumption warranting inference from "we could measure *either* of two complementary properties on the nearby system" to "*both* of the corresponding properties must exist for the distant system".

The suspicion is strongly confirmed by the contents of a letter written to Bohr by Paul Ehrenfest (with whom Einstein had discussed the dialogues from the 1930 meeting shortly afterwards). We quote here philosopher Don Howard's description of this entire episode:

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

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

He [Einstein] said to me that, for a very long time already, he absolutely no longer doubted the uncertainty relations, and that he thus, e.g., had BY NO MEANS invented the 'weighable light-flash box' (let us call it simply L-F-box) 'contra uncertainty relation,' but for a totally different purpose. [Ehrenfest to Bohr, 9 July 1931]

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

As Ehrenfest explains to Bohr, Einstein's idea was this. Let the photon leave the box and be reflected back from a great time and distance, say one-half light year. At about the time when the photon is reflected, we can either weigh the box or check the clock, making possible our predicting either the exact time of the photon's return or its energy (literally, its color), which is to say that, depending upon which measurement we choose, we ascribe a different theoretical state to the photon, one with definite energy, one entailing a definite time of arrival. Crucial is the fact that the event of performing the measurement on the box – weighing it the second time or checking the clock – is [spatially] separated from the event of the photon's distant reflection, because then our choice of a measurement to perform can have no effect on the real state of affairs of the photon, meaning that the photon's real state of affairs when it returns will be one and the same, regardless of the measurement we performed on the box. This is all just quantum mechanics, in Einstein's view. But then quantum mechanics has associated two different theoretical states with one real state of affairs, which is possible only if the quantum theory's state descriptions are incomplete [7].

So it definitely appears that Einstein's early (pre-1935) arguments were simply not understood properly by Bohr. (And it is curious that Bohr's later account of these early discussions did not attempt to correct the misunderstanding, but instead reinforced it for posterity.) In any case, though, it represents progress that, by around 1933, Bohr (in his re-telling) began to recognize and more directly confront the "new aspects" of Einstein's arguments.

Here is Bohr's description of these "new aspects" in the context of the photon box thought experiment:

> Einstein had pointed out that, after a preliminary weighing of the box with the clock and the subsequent escape of the photon, one was still left with the choice of either repeating the weighing or opening the box and comparing the reading of the clock with the standard time scale. Consequently, we are at this stage still free to choose whether we want to draw conclusions either about the energy of the photon or about the moment when it left the box. Without in any way interfering with the photon between its escape and its later interaction with other suitable measuring instruments, we are, thus, able to make accurate predictions pertaining *either* to the moment of its arrival *or* to the amount of energy liberated by its absorption. Since, however, according to the quantum-mechanical formalism, the specification of the state of an isolated particle cannot involve both a well-defined connection with the time scale and an accurate fixation of the energy, it might thus appear as if this formalism did not offer the means of an adequate description [6].

Now that, for sure, captures the concern that Einstein seems to have had in mind all along, and would eventually appear most famously and explicitly in the 1935 EPR paper. Note in particular the exact parallel to the EPR reasoning: by measuring one property of the box, we can determine an exact value for the corresponding property of the distant photon; on the other hand, by instead measuring a different property of the box, we can determine an exact value for a different property of the distant photon; but since our measurements on the box must – by the locality assumption – have no effect on the physical state of the distant photon, the possibility of our determining either of these properties implies that both properties have, already, sharp (if unknown) values. And so the quantum mechanical formalism (which precludes such simultaneous value assignments) must be incomplete.

So how, then, does Bohr respond to this early version of the EPR argument?

> Once more Einstein's searching spirit had elicited a peculiar aspect of the situation in quantum theory, which in a most striking manner illustrated how far we have here transcended customary explanation of natural phenomena. Still, I could not agree with the trend of his remarks.... In my opinion, there could be no other way to deem a logically consistent mathematical formalism as inadequate than by demonstrating the departure of its consequences from experience or by proving that its predictions did not exhaust the possibilities of observation, and Einstein's argumentation could be directed to neither of these ends. In fact, we must realize that in the problem in question we are not dealing with a *single* specified experimental arrangement, but are referring to *two* different, mutually exclusive arrangements. In the one, the balance together with another piece of apparatus like a spectrometer is used for the study of the energy transferred to the photon; in the other, a shutter regulated by a standardized clock together with another apparatus of similar kind, accurately timed relatively to the clock, is used for the study of the time of propagation of a photon over a given distance. In both these cases, as also assumed by Einstein, the observable effects are expected to be in complete conformity with the predictions of the theory.
>
> The problem again emphasizes the necessity of considering the *whole* experimental arrangement, the specification of which is imperative for any well-defined application of the quantum-mechanical formalism [6].

I think it is probably safe to say that one will either regard this response as satisfying, or not, depending on the extent to which one's philosophical attitudes align with those of Bohr, or Einstein, respectively.

In the next section, we will continue to explore Bohr's responses to Einstein's concerns by considering Bohr's official response to the 1935 paper of Einstein, Podolsky, and Rosen.

6.5 Bohr's Reply to EPR

Let us then finally turn to Bohr's response to the actual EPR paper of 1935. It is of historical interest that, according to the later recollection of Bohr's close colleague Rosenfeld, the EPR paper was an "onslaught" which "came down upon us as a bolt from the blue." Rosenfeld reports that "as soon as Bohr had heard my report of Einstein's argument, everything else was abandoned" as they dedicated themselves to rebutting the argument [8].

So, after the days and weeks of careful thinking, how did Bohr respond? Early on in the essay, Bohr reviews the idea that the impossibility of attributing definite properties to measured systems arises from the uncontrollable physical disturbance of their states during the physical interaction with the measuring apparatus:

> The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails – because of the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose – the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality [9].

But surely the intention of the EPR argument was precisely to neutralize this "disturbance" defense, by separating the measurement event from the particle to which properties are being attributed. If, in the EPR case, the definite inferred properties of the distant particle in any sense arise, newly, as a result of the measurement on the nearby partner, this would be the very sort of nonlocal causation that EPR regarded as unacceptable or, more precisely, in conflict with relativity's prohibition on faster-than-light causation.

Bohr seems to only partially appreciate this, and his response is thus notoriously difficult to understand. He insists that the EPR criterion of reality (inside of which, remember, the crucial concept of locality was buried) "contains – however cautious its formulation may appear – an essential ambiguity when it is applied to the actual problems with which we are here concerned" [9]. Here is his detailed statement of the alleged ambiguity:

> From our point of view we now see that the wording of the above-mentioned criterion of physical reality proposed by Einstein, Podolsky, and Rosen contains an ambiguity as regards the meaning of the expression 'without in any way disturbing a system.' Of course there

> is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term 'physical reality' can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete. On the contrary this description, as appears from the preceding discussion, may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory. In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic principles of science. It is just this entirely new situation as regards the description of physical phenomena, that the notion of *complementarity* aims at characterizing [9].

It seems that Bohr is agreeing with EPR that a "mechanical disturbance" – that is, a nonlocal causal influence on the distant particle – is unacceptable. But, he seems to say, there is another kind of influence – what one commentator [10] has described as a "semantic disturbance"... not, apparently, a physical influence per se, but instead an influence on what we can *say* about the distant system.

Here is what Bell would write, later, about Bohr's response:

> While imagining that I understand the position of Einstein, as regards the EPR correlations, I have very little understanding of the position of his principal opponent, Bohr. Yet most contemporary theorists have the impression that Bohr got the better of Einstein in the argument and are under the impression that they themselves share Bohr's views. As an indication of those views I quote a passage from his reply to Einstein, Podolsky and Rosen. It is a passage which Bohr himself seems to have regarded as definitive, quoting it himself when summing up much later. Einstein, Podolsky and Rosen had assumed that '...if, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity'. Bohr replied: '...the wording of the above mentioned criterion... contains an ambiguity as regards the meaning of the expression "without in any way disturbing a system". Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system* [so] their argumentation does not justify their conclusion that quantum mechanical description is essentially incomplete ... This description may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory'.
>
> Indeed I have very little idea what this means. I do not understand in what sense the word 'mechanical' is used, in characterizing the disturbances which Bohr does not contemplate, as distinct from those which he does. I do not know what the italicized passage means – 'an influence on the very conditions...'. Could it mean just that different experiments on the first system give different kinds of information about the second? But this was just one of the main points of EPR, who observed that one could learn *either* the position *or* the momentum of the second system. And then I do not understand the final reference to 'uncontrollable interactions between measuring instruments and objects', [as] it seems just to ignore the essential point of EPR that in the absence of action at a distance, only the first system could

> be supposed disturbed by the first measurement and yet definite predictions become possible for the second system. Is Bohr just rejecting the premise – 'no action at a distance' – rather than refuting the argument? [11].

Bell, that is, suggests reading Bohr as conceding (despite his explicit denial of a specifically "mechanical" disturbance) that there is a non-local action-at-a-distance at work in this situation, according to quantum mechanics.

Einstein could also do no better than this same uncomfortable understanding of Bohr's response. In his 1949 commentary, he wrote:

> Of the 'orthodox' quantum theoreticians whose position I know, Niels Bohr's seems to me to come nearest to doing justice to the problem. Translated into my own way of putting it, he argues as follows:
>
> If the partial systems A and B form a total system which is described by its Ψ-function $\Psi(AB)$, there is no reason why any mutually independent existence (state of reality) should be ascribed to the partial systems A and B viewed separately, *not even if the partial systems are spatially separated from each other at the particular time under consideration*. The assertion that, in this latter case, the real situation of B could not be (directly) influenced by any measurement taken on A is, therefore, within the framework of quantum theory, unfounded and (as the paradox shows) unacceptable [12].

From the point of view of a "realist" such as Einstein – meaning simply someone who believes in the existence of an external physical world that is what it is independent of any observation and which observation is ultimately observation *of* – Bohr's reply to EPR will always remain deeply unsatisfying. Yet we must remember that from the point of view of the Copenhagen philosophy, it is precisely, at the end of the day, this assumption of "realism" which is being challenged. Heisenberg, for example, wrote that

> the idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them ... is impossible.... [13]

Bohr, similarly, insisted that in quantum mechanics we meet

> in a new light the old truth that in our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience [14].

Bohr stressed repeatedly this point that physical theories must not aim at describing some independent, objective physical reality:

> The entire formalism is to be considered as a tool for deriving predictions, of definite or statistical character, as regards information obtainable under experimental conditions described in classical terms and specified by means of parameters entering into the algebraic or differential equations.... These symbols themselves are not susceptible to pictorial interpretation [15].

And according to Bohr's colleague Aage Petersen, when Bohr was once asked whether the theory could in any sense be understood as describing an objective reality, Bohr replied

> There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature [16].

Bohr's dismissal of the EPR argument for incompleteness may indeed be the only coherent and rational response given this deeper philosophical point of view.

6.6 Contemporary Perspectives

I mentioned in the introduction of this chapter that the Copenhagen philosophy achieved a kind of orthodox status in the 1930s and has essentially held this position to the present day. But I think most physics students who have been exposed to this orthodoxy – and indeed most physics professors who have accepted it and even taken part in teaching it – will probably find some aspects of the philosophy, as explained by Bohr and Heisenberg, a little surprising. Does the Copenhagen interpretation really insist, for example, that there simply is no real physical world at the microscopic level? This seems bizarre if not downright incomprehensible.

One can certainly find contemporary proponents of the Copenhagen philosophy who embrace such radical philosophical positions. The eminent Austrian experimentalist Anton Zeilinger, for example, summarized "The message of the quantum" in Copenhagen terms. He stresses the failure of classical notions of causality as follows:

> The discovery that individual events are irreducibly random is probably one of the most significant findings of the twentieth century. Before this, one could find comfort in the assumption that random events only seem random because of our ignorance. For example, although the brownian motion of a particle appears random, it can still be causally described if we know enough about the motions of the particles surrounding it.... But for the individual event in quantum physics, not only do we not know the cause, there is no cause. The instant when a radioactive atom decays, or the path taken by a photon behind a half-silvered beam-splitter are objectively random. There is nothing in the Universe that determines the way an individual event will happen. Since individual events may very well have macroscopic consequences ... the Universe is fundamentally unpredictable and open, not causally closed [17].

Zeilinger then insists that "the concept of reality itself is at stake" in certain experiments that we will discuss further in Chap. 8. As he elaborates:

> A criticism of realism also emerges from the notion of complementarity. It is not just that we are unable to measure two complementary quantities of a particle, such as its position and momentum, at the same time. Rather, the assumption that a particle possesses both position and momentum, before the measurement is made, is wrong. Our choice of measurement apparatus decides which of these quantities can become reality in the experiment.
>
> So, what is the message of the quantum? I suggest we look at the situation from a new angle. We have learned in the history of physics that it is important not to make distinctions that have no basis – such as the pre-newtonian distinction between the laws on Earth and those that govern the motion of heavenly bodies. I suggest that in a similar way, the distinction between reality and our knowledge of reality, between reality and information, cannot be made. There is no way to refer to reality without using the information we have about it [17].

Undoubtedly this almost idealistic (in the sense of Berkeley) anti-realism is part of "the Copenhagen interpretation" for many contemporary physicists.

But I think most physicists would find themselves slightly embarrassed by this kind of openly philosophical, anti-realist speculation. The more mainstream understanding of "the Copenhagen interpretation" is thus, I think, a little more restrained and pragmatic. This attitude is nicely captured in the widely used quantum mechanics text by David Griffiths, who explains that Born's statistical interpretation of the wave function

> ...introduces a kind of **indeterminacy** into quantum mechanics, for even if you know everything the theory has to tell you about the particle (to wit: its wave function), you cannot predict with certainty the outcome of a simple experiment to measure its position – all quantum mechanics has to offer is *statistical* information about the *possible* results. This indeterminacy has been profoundly disturbing to physicists and philosophers alike. Is it a peculiarity of nature, a deficiency in the theory, a fault in the measuring apparatus, or *what*?
>
> Suppose I *do* measure the position of the particle, and I find it to be at [some particular] point C. Question: Where was the particle just *before* I made the measurement? There are three plausible answers to this question, and they serve to characterize the main schools of thought regarding quantum indeterminacy:
>
> **1.** The **realist** position: *The particle was at* C. This certainly seems like a sensible response, and it is the one Einstein advocated. Note, however, that if this is true then quantum mechanics is an **incomplete** theory, since the particle *really was* at C, and yet quantum mechanics was unable to tell us so. To the realist, indeterminacy is not a fact of nature, but a reflection of our ignorance.... Evidently Ψ is not the whole story – some additional information (known as a **hidden variable**) is needed to provide a complete description of the particle.
>
> **2.** The **orthodox** position: *The particle wasn't really anywhere.* It was the act of measurement that forced the particle to 'take a stand' (though how and why it decided on the point C we dare not ask). Jordan said it most starkly: 'Observations not only *disturb* what is to be measured, they *produce* it. ... We *compel* [the particle] to assume a definite position.' This view (the so-called **Copenhagen interpretation**) is associated with Bohr and his followers. Among physicists it has always been the most widely accepted position. Note, however, that if it is correct there is something very peculiar about the act of measurement – something that over half a century of debate has done precious little to illuminate.
>
> **3.** The **agnostic** position: *Refuse to answer.* This is not quite as silly as it sounds – after all, what sense can there be in making assertions about the status of a particle *before* a measurement, when the only way of knowing whether you were right is precisely to conduct a measurement, in which case what you get is no longer 'before the measurement'? It is metaphysics (in the pejorative sense of the word) to worry about something that cannot, by its nature, be tested. Pauli said, 'One should no more rack one's brain about the problem of whether something one cannot know anything about exists all the same, than about the ancient question of how many angels are able to sit on the point of a needle.' For decades this was the 'fall-back' position of most physicists: They'd try to sell you answer 2, but if you were persistent they'd switch to 3 and terminate the conversation [18].

Incidentally, Griffiths goes on to suggest (just like Zeilinger) that certain experiments (pertaining to something called Bell's Theorem that is the subject of our Chap. 8) have recently "eliminated agnosticism as a viable option" and have "confirmed decisively the orthodox interpretation." About this, Griffiths is (like Zeilinger) simply wrong (in part because of an unnecessarily restrictive conception of the "realist" alternative); this will become clearer in the following two chapters.

In his overall characterization of the three options, however, Griffiths is admirably open and reasonable; many textbooks don't even acknowledge something like option

1 but instead just insist dogmatically that some superposition of **2** and **3** is the final truth, handed down from on high by Bohr, and not to be questioned. The final quoted sentence above is also, in my experience, perfectly accurate about the attitude of most physicists: they know they are supposed to believe **2** and so will do some minimal amount of due diligence trying to propagandize on behalf of the Copenhagen interpretation; but at the end of the day they don't take it too seriously and frankly don't really care and are perfectly content to just stop talking about it.

This pragmatic attitude was brilliantly captured by N. David Mermin, who wrote in a 1989 essay in *Physics Today*:

> If I were forced to sum up in one sentence what the Copenhagen interpretation says to me, it would be 'Shut up and calculate! [19].

This is probably the best – certainly the briefest – summary of how most physicists today understand "the Copenhagen interpretation". It captures perfectly the typical physicist's impatience for idle philosophical speculation and desire to get on with obviously practical things like using the theory to calculate predictions for how measurements should come out, and then testing those predictions with actual experiments.

Of course, this attitude is rather contrary to the point of view adopted in the present book. One should not, however, regard this as an endorsement of "idle philosophical speculation". Just the opposite, in fact. There is, I think, a deep irony in the fact that "Shut up and calculate!" is almost always deployed against people who want to *criticize* the orthodox, Copenhagen interpretation and construct an alternative theory that, for example, resolves the measurement problem. Such alternative theories typically postulate new sorts of microscopic objects, obeying new dynamical equations, in terms of which a uniform and coherent description of microscopic processes might be shown (through calculations!) to become possible. It is very surprising that physicists who value precisely-formulated theories and the calculations these make possible would prefer Bohr's philosophical speeches rather than the more hard-headed alternative theories we will cover in the remainder of the book. In a rational world, that is, "Shut up and calculate!" is what the *critics* should say to the Copenhagenists, whose dogmatic (yet simultaneously unserious) attachment to Bohr's philosophy prevents them from even asking the kinds of questions that might lead to real practical advances.

Clearly there are some deep issues – philosophical issues about the proper goals of science and sociological issues about how the physics community deals with disagreements over what questions are legitimate – that we will not be able to answer here. But one thing is for sure: to whatever extent the Copenhagen philosophy insists that it is not merely wrong, but *impossible*, to provide a uniform, coherent, realistic description of the world, which is nevertheless consistent with all known experimental facts, the Copenhagen philosophy is in that regard simply *wrong*. Several such candidate theories exist. Exploring them will occupy us for most of the rest of the book, starting, in the next chapter, with the pilot-wave theory of de Broglie and Bohm, which provides a stark, eye-opening contrast to the Copenhagen interpretation.

Projects:

6.1 Read the published version of Bohr's Como lecture [1] and report back on any aspects that you find surprising, interesting, novel, or illuminating.

6.2 In one of the passages quoted in Sect. 6.2, Heisenberg writes, about the quantum mechanical wave function:

> This probability function represents a mixture of two things, partly a fact and partly our knowledge of a fact. It represents a fact in so far as it assigns at the initial time the probability unity (i.e., complete certainty) to the initial situation: the electron moving with the observed velocity at the observed position; 'observed' means observed within the accuracy of the experiment. It represents our knowledge in so far as another observer could perhaps know the position of the electron more accurately. The error in the experiment does – at least to some extent – not represent a property of the electron but a deficiency in our knowledge of the electron. Also this deficiency of knowledge is expressed in the probability function [5].

Here Heisenberg wants to draw a distinction between the fundamental, irreducible type of uncertainty (described by his famous uncertainty relations) and the ordinary type of uncertainty that arises from, for example, imperfect measurements. Does Heisenberg's position here leave him open to the criticism that, if the same physical situation can be described by two different quantum mechanical wave functions (based on different amounts of uncertainty in at least the second sense), the quantum mechanical descriptions of physical states cannot be complete? Explain.

6.3 Do you think Bohr would agree with Heisenberg's suggestion that "another observer could perhaps know the position of the electron more accurately"? Explain.

6.4 What, according to Heisenberg, is the difference between the use of probabilities in classical physics, and their use in quantum mechanics?

6.5 Read Heisenberg's essay on "The Copenhagen Interpretation" [5] and report back on anything you find surprising, interesting, novel, or illuminating.

6.6 Read Bohr's "Discussion with Einstein..." [6] paper and report back on anything you find surprising, interesting, novel, or illuminating.

6.7 Show that, as claimed in Bohr's analysis of the two-slit experiment discussed in Sect. 6.3, the spacing Δx between adjacent interference maxima (see Fig. 6.2) is $\lambda L/d$.

6.8 In a 1979 paper [20], Wootters and Zurek provide a more detailed and quantitative analysis of the 2-slit experiment discussed in Sect. 6.3. Read their paper and summarize their arguments and conclusions.

6.9 Richard Feynman discusses the 2-slit experiment and its interpretation in Ref. [21]. How does Feynman's philosophical attitude toward quantum mechanics relate to the Copenhagen interpretation?

6.10 In the text it was suggested that (in his 1949 reminiscence) Bohr had misunderstood or misrepresented Einstein's 1930 photon box argument. Do you think Bohr might also have misunderstood/misrepresented Einstein's arguments regarding the diffraction and interference experiments we discussed in

Section 6.3? If so, explain how those arguments could be reformulated in a way that makes it clearer how they anticipate the 1935 EPR argument. If not, explain why the diffraction/interference examples are importantly different.

6.11 Work through the mathematical details of Bohr's analysis of the photon box experiment. (You might need or want to do a little independent research to understand the general relativistic formula for gravitational time-dilation.)

6.12 In Ref. [22], Dieks and Lam present a detailed analysis of Einstein's "photon box" thought experiment. Read their paper and summarize their arguments and conclusions. (Note that this requires a familiarity with operators and their commutators that is slightly beyond the level required elsewhere in this book.)

6.13 Commentators on the Einstein–Bohr debates often characterize Einstein as a kind of stubborn old conservative who simply couldn't get with the new quantum program. For example, Heisenberg wrote:

> Most scientists are willing to accept new empirical data and to recognize new results, provided they fit into their philosophical framework. But in the course of scientific progress it can happen that a new range of empirical data can be completely understood only when the enormous effort is made to enlarge this framework and to change the very structure of the thought process. In the case of quantum mechanics, Einstein was apparently no longer willing to take this step, or perhaps no longer able to do so [23].

And Max Born wrote:

> At first there were quite a number of serious scientists who did not want to know anything about the theory of relativity; conservative individuals, who were unable to free their minds from the prevailing philosophical principles.... Einstein himself belonged to this group in later years; he could no longer take in certain new ideas in physics which contradicted his own firmly held philosophical convictions [23].

What do you think of this? Of the two interlocutors, Einstein and Bohr, which one was really open to new theoretical concepts, and which one insisted on preserving old ideas, come what may?

6.14 Read Bohr's reply [9] to the EPR paper and report on anything you find surprising, interesting, novel, or illuminating.

6.15 In his reply [9] to EPR, Bohr provides a concrete kind of setup which would give rise to something like the entangled EPR state, in which "a subsequent single measurement of either of the position or of the momentum of one of the particles will automatically determine the position or momentum, respectively, of the other particle with any desired accuracy." What does he say about this and how would Einstein reply?

6.16 Interview some physicists about the Copenhagen interpretation. Ask them whether they basically agree with it. Then ask them to summarize what it says. You might also ask them specifically about whether Bohr successfully refuted the EPR argument and, if so, how the refutation works exactly. Summarize and share your findings.

6.17 The *Wikipedia* page on the Copenhagen interpretation provides (as of this writing) the following supposedly Copenhagen response to Schrödinger's cat: "The wave function reflects our knowledge of the system. The wave function

$[\frac{1}{\sqrt{2}}(\psi_{\text{alive}} + \psi_{\text{dead}})]$ means that, once the cat is observed, there is a 50% chance it will be dead, and 50% chance it will be alive." Do you think this accurately captures what Bohr would have said about Schrödinger's cat?

6.17 The *Wikipedia* page on the Copenhagen interpretation provides (as of this writing) the following supposedly Copenhagen response to the EPR argument: "Assuming wave functions are not real, wave-function collapse is interpreted subjectively. The moment one observer measures the spin of one particle, he knows the spin of the other. However, another observer cannot benefit until the results of that measurement have been relayed to him, at less than or equal to the speed of light." Do you think this provides a fair summary of Bohr's response to EPR?

6.18 What do you think Bohr would have thought about the slogan "Shut up and calculate!"? It might be helpful to do some research here regarding Bohr's thoughts about the applicability of "complementarity" outside of physics; see, for example, Mara Beller's "The Sokal Hoax: At Whom are we Laughing?" [24].

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