Arkady Plotnitsky

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Epistemology and Probability

Bohr, Heisenberg, Schrödinger, and the Nature of Quantum-Theoretical Thinking

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Preface

This book offers an exploration of the relationships between epistemology and probability in the work of Niels Bohr, Werner Heisenberg, and Erwin Schrödinger, and in quantum mechanics and in modern physics as a whole. It also considers the implications of these relationships and of quantum theory itself for our understanding of the nature of human thinking and knowledge in general, or the "epistemological lesson of quantum mechanics," as Bohr liked to say.[1] These implications are radical and controversial. While they have been seen as scientifically productive and intellectually liberating to some, Bohr and Heisenberg among them, they have been troublesome to many others, such as Schrödinger and, most prominently, Albert Einstein. Einstein famously refused to believe that God would resort to playing dice or rather to playing with nature *in the way* quantum mechanics appeared to suggest, which is indeed quite different from playing dice. According to his later (sometime around 1953) remark, a lesser known or commented upon but arguably more important one: "That the Lord should play [dice], all right; but that He should gamble according to definite rules [i.e., according to the rules of quantum mechanics, rather than by merely throwing dice], that is beyond me."[2] Although Einstein's invocation of God is taken literally sometimes, he was not talking about God but about the way nature works. Bohr's reply on an earlier occasion to Einstein's question

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[1] Cf., "Quantum Physics and Philosophy: Causality and Complementarity," *Philosophical Writings of Niels Bohr*, 3 vols. (Bohr 1987, vol. 2, p. 91; vol. 3, p. 12), hereafter to be referred to as *PWNB* followed by a volume number. A supplementary volume of Bohr's essays was published as Niels Bohr, *The Philosophical Writings of Niels Bohr*, *Volume IV: Causality and Complementarity, Supplementary Papers* (Bohr 1998) and will be referred as *PWNB 4*. These four volumes contain most of Bohr's works on quantum mechanics and complementarity to be cited here. The essays from these volumes cited here are listed separately in this book's bibliography with the original publication date, to be given in the text as well (e.g., the article just cited will be referred to as "Bohr 1958, *PWNB* 3," followed by page numbers).

[2] Cited in John A. Wheeler and Wojciech H. Zurek, eds., *Quantum Theory and Measurement* (Wheeler and Zurek, p. 8). This collection, which contains a number of articles to be cited here, will hereafter be referred to as *QTM*. All the articles from this collection are, however, also separately indicated in the text and are separately listed in the bibliography with the original publication date.

whether "we could really believe that the providential authorities took recourse to dice-playing" in the same "humorous spirit that animated the discussions" is nevertheless worth recalling: "I replied by pointing at the great caution, already called for by ancient thinkers, in ascribing attributes to Providence in everyday language" (Bohr 1949; *PWNB* 2, p. 47). Einstein was reluctant to believe that nature, at the ultimate level of its constitution, would behave in this way. In other words, he was unwilling to accept that quantum mechanics, or any theory of the same (epistemological) type, ultimately reflects nature. Nor did he think that an alternative theory that would be more philosophically palatable and, by his criteria (essentially derived from classical physics), more complete could not eventually be found. He often said that quantum mechanics revealed a beautiful element of truth in nature, but not its ultimate truth, and he thought that "it offers no useful point of departure for future development" (Einstein 1949a, p. 83).

Quantum mechanics has, however, been around for near a century now, and within its proper scope has remained the standard theory, well confirmed experimentally, as are most currently standard quantum theories, such as and in particular quantum electrodynamics, arguably the best confirmed physical theory ever, and other quantum field theories.[3] In part by virtue of its epistemologically complex and controversial character, quantum mechanics or quantum theory, more generally, has occupied a special place in physics and philosophy, and in intellectual history as a whole in the twentieth and by now the twenty-first centuries. In either respect, relativity has been and remains its main rival—at

[3] Throughout this study, by "quantum mechanics" or "the standard quantum mechanics," I mean the *standard* version of quantum mechanics, covered by Heisenberg's or Schrödinger's formalism, or other, more or less mathematically equivalent, versions of the quantum-mechanical formalism, such as those of Paul Dirac or John von Neumann. As will be seen, there are certain differences between these versions and in how they are used, but these are not essential for my main epistemological argument here, which applies to any of these versions. In any event, the term "quantum mechanics" will not refer to alternative accounts of the experimental data in question, such as those offered by Bohmian mechanics, for example (and there are several versions of the latter as well). On the other hand, "quantum theory" will refer more generally to theoretical thinking concerning quantum phenomena. This denomination, thus, also includes the quantum theory (sometimes referred to as the old quantum theory) that preceded quantum mechanics; the alternatives to the standard quantum mechanics just mentioned; and higher level quantum theories, such as quantum electrodynamics and other quantum field theories (e.g., quantum chromodynamics, which deals with quantum processes inside atomic nuclei), comprising the so-called standard model. "Quantum phenomena" will refer to those observable phenomena in considering which, for example and in particular by means of quantum mechanics, the role of Max Planck's constant h (which has a very small magnitude) cannot be disregarded, as it can be in the case of the phenomena considered by classical physics. Unless specified otherwise, "quantum phenomena" will refer to those phenomena that fall within the scope of quantum mechanics, which are my main concern in this study. Finally, "quantum physics" will refer to the overall assembly of the available quantum phenomena (including the quantitative experimental data involved, beginning with h) and theoretical accounts, of whatever kind, of these phenomena. The terms "classical mechanics" (or "Newtonian mechanics"), "classical (physical) theory," and "classical physics" will be used along the respective parallel lines.

least among physical theories, since certain developments of modern mathematics, computer sciences, and biology may be argued to have a similar cultural prominence and impact. Indeed, more recently biology and information sciences appear to have surpassed physics as the dominant sciences in our culture. The recent completion of the Large Hadron Collider (LHC) at the *Organisation Européenne pour la Recherche Nucléaire* [The European Organization for Nuclear Research], known as CERN, near Geneva, and its promise to reveal "the deeper secrets of nature," as newspapers like to have it, appear to have brought fundamental physics back to the cultural center stage. (After a brief period of preliminary runs, the Collider is on hold at the moment due to a major problem with its superconducting bending magnets, but it is expected to be operational again later in 2009.)

Whatever its role in our culture, the state of contemporary fundamental physics is in part defined (and some would say marred) by the physical incompatibility between quantum theory and general relativity as the currently accepted theory of gravity, as against the unity of classical mechanics and Newton's classical theory of gravity. The fact that each theory itself has, thus far, been confirmed as, within its proper limits, a correct theory of the corresponding phenomena with an extraordinary degree of precision makes the incompatibility all the more glaring. In one respect, the situation is somewhat mitigated by the fact that gravity is much weaker than other forces of nature (on the scale of 10^{35} weaker in its order of magnitude than electromagnetism). This discrepancy makes it possible to make progress in quantum theory without taking gravity into account, or conversely in relativistic physics without considering quantum effects, and many developments in both areas in recent decades have been spectacular. On the other hand, the discrepancy (also known as "the hierarchy problem") is enigmatic and, many feel, needs to be theoretically explained and, as it were, bridged by a theory where it would appear logically. This situation provides the impetus for string theory and its extensions to brane theories and beyond, or for certain other proposed alternatives. There are also other, more physically immediate, motivations for developing such a theory or theories. Arguably the most pressing among these motivations are the following two. The first arises from a generally (albeit not quite universally) shared assumption that, just as that of other forces of nature, the ultimate nature of gravity is quantum. The second arises from another, equally generally (but, again, not universally) shared, assumption that the earliest stages of the Universe, which would explain its current architecture and dynamics, are shaped by quantum processes, possibly of a unified character. The corresponding theories, when and if developed, might or might not be related, or be the same theory. In any event, a theory or a set of theories that would bring general relativity and quantum theory together or at least that are operative at deeper levels but consistent with both within their proper limits, appears to be far away, although string and brane theories, and a few other approaches do offer some, but thus far tenuous, hope. In this respect, in addition to, hopefully, straightening out as yet unresolved difficulties of the standard model, the currently, by and large, accepted quantum theory of the known forces of nature (electroweak and strong) apart from gravity, the experiments to be performed at the LHC may offer us some new clues.[4] These experiments might reveal the existence of particles predicted by hypothetical theories beyond the standard model, such as the so-called supersymmetry (there are several versions of the latter as well, including of the string-theoretical variety), which offer some hope for a theory of quantum gravity. They can also send physicists back to the drawing boards as concerns our current theories, not inconceivably quantum mechanics among them, although this appears somewhat less likely given that these are higher level quantum theories and, again, most especially the standard model, that are about to be tested in these experiments.[5]

It is difficult, if not impossible, to estimate what such theories will reveal philosophically, even though they do pose significant philosophical questions, extending those already posed by relativity and quantum mechanics and some new ones.[6] Even in the near future the situation may change considerably in any

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[4] The standard model has thus far only been able to unify (quantized) electromagnetism and weak (nuclear) force, but not all three forces just mentioned. Such a more complete unification (which is expected to be accomplished by string theory or other fundamental theories just mentioned) appears desirable to many and is often stated as a goal, and the inability of the standard model to achieve this goal thus far is sometimes held against it. There is, however, some debate as to whether, even assuming that it is possible in the first place, this more complete unification is necessary, or that the failure, thus far, to offer it is a real drawback of the standard model. The latter does account for the three fundamental forces in question, even if it does so without unifying them. The problem of quantum gravity is of course different, since we do not have a quantum theory of gravity as such, although it is assumed to be ultimately quantum, and some of the properties of its quanta, gravitons, are predicted already.

[5] Proposals to test quantum mechanics, usually in order to prove it wrong, even within its own limits, continue to be advanced. Given the epistemological discontent with the theory on the part of the majority of physicists and philosophers (Einstein seems to have won this battle for now), it would be difficult to expect that they will stop worrying and learn to love quantum mechanics. There is of course nothing wrong with offering such proposals. It is possible that quantum mechanics will prove to be wrong even within its own limits in view of some experimental discovery, similarly to the way Newton's gravity was proved wrong in its account of the behavior of Mercury, which requires general relativity to be properly accounted for (although Newton's theory is still an excellent approximation, sufficient in most cases).

[6] Cf., a relatively recent (although already requiring considerable updating, especially as concerns brane theory) collection (Callender and Huggett 2001), for some among the philosophical aspects and implications of such future theories. One of the more philosophically, as well as physically, significant areas of investigation is that of the (thus far mostly hypothetical) cosmological theories that attempt to relate quantum theory and relativity. Some of the attempts along these lines, such as the so-called "cosmic landscape" theories, also significantly involve the question of chance, including potentially the type of chance found in quantum physics (as against classical statistical physics), one of my main subjects here. I shall comment on some of these developments later in this study. For an excellent recent survey, see Carroll (2006), and for good nontechnical discussions of the brane-theory-oriented cosmology, see Randall (2005) and Randall (2007). For the "landscape" cosmology, see Susskind (2006). See also a survey "Year of Physics: A Celebration" published during the centenary of Einstein's *anus mirabilis* in 2005 (*Nature* **433**, 213 [2005]), which, although it requires some updating, still offers a good picture of the current state of fundamental physics.

given direction or reveal new, yet unknown and even unimagined, gradients of new thinking. It is possible that these theories will return us to more classical and, in particular, more realist and causal ways of thinking and knowledge in physics, and, following Einstein, many hope that they will. However much quantum theory appears, at least to some of us, to suggest otherwise, it is not inconceivable that nature, or our thinking concerning it, might offer new support for such hopes. It is also possible, however, that these theories will reveal something that is even more radical philosophically than quantum theory, difficult as it may be to think of something epistemologically more radical than quantum mechanics. As I shall suggest later in this study, quantum field theory already appears to lead us beyond quantum mechanics.

Whatever the future holds in store for physics or philosophy, quantum theory has radically transformed our understanding of the nature of human knowledge, and of the relationships between physics and philosophy, the discipline primarily concerned with human thinking and knowledge. It may indeed be noted that, if quantum mechanics had proven to be incorrect even within its proper scope and were to be replaced by an epistemologically more classical theory, similar, say, to classical mechanics, it would not devalue the alternative "nonclassical," as I shall term it, epistemology of quantum mechanics adopted in this study. This epistemology, which I shall outline in detail in the Introduction, may be developed into a corresponding theory elsewhere, for example, in philosophy (as to some degree it will be here) or biology. This transformation and the relationships themselves between physics and philosophy are among the primary concerns of this study, as they have been in most philosophical treatments of quantum theory, or, in the case of these relationships, in philosophical treatments of all physics, beginning with Galileo or Aristotle, both of whom were concerned with these relationships as well. However, the mathematical or, more accurately, mathematical–experimental character of modern physics, from classical physics to relativity to quantum physics, as, in Galileo's language, "a mathematical science of nature," unavoidably converts these relationships into those *among physics, mathematics, and philosophy*.[7] These triple relationships are significant not only for our philosophical *understanding* of quantum mechanics and its interpretation, but also for this *interpretation* itself. However implicit these relationships may be, they are, in the language of Immanuel Kant, among the *conditions of possibility* of the interpretation of quantum mechanics, including as concerns the question of probability there. By "interpretation" I mean, most immediately, giving a proper explication of physical content and meaning to a given physical theory, in particular as concerns the relationships (descriptive or predictive) between the mathematical formalism and the measurable quantities established by experiments. As will be

[7] The concept of a "mathematical science of nature," rather than that of "physics," defines (perhaps against Aristotle's conception of physics) what we now call physics in Galileo's *Dialogues Concerning Two New Sciences* (Galileo 1991). For a related discussion of Galileo, see Plotnitsky and Reed (2001).

seen below, a more complex concept of interpretation is required even in classical physics or relativity, and especially in quantum theory. These triple relationships among physics, mathematics, and philosophy are central to my argument in this study, and this preface and the Introduction are designed to position this argument accordingly.

Quantum theory was initiated by Max Planck's discovery, in 1900, of his black body radiation law, which revealed that radiation, such as light, previously considered a continuous phenomenon in all circumstances, could also exhibit certain features of discreteness or of particle-like behavior in some circumstances. The limit at which this discontinuity appears is defined by the frequency of the radiation and a universal constant of a very small magnitude, h, Planck's constant, which Planck himself termed "the quantum of action" and which turned out to be one of the most fundamental constants of all physics. The indivisible (energy) quantum of radiation in each case is the product of h and the frequency ν, $E = h\nu$. The role of Planck's constant h may be seen as analogous to the role of c in special relativity (the constancy of the speed of light in a vacuum in its independence of the speed of the source) in terms of both necessitating a departure from classical theory and of introducing the first principles of a new theory. The rest, one might argue, follows "naturally" in both cases, even though it might have taken a bit longer to develop the consequences of Planck's assumption in the case of quantum theory. The parallel formulas for energy, $E=mc^2$ (admittedly, a consequence rather than a postulate in special relativity theory) and $E = h\nu$, amplify the parallel between the two situations.

Other earlier developments, sometimes referred to as the "old quantum theory," made apparent yet further complexities of quantum phenomena and posed new questions concerning these phenomena. Arguably, the most significant among these developments was Einstein's introduction of the idea of photon, eventually understood as "the particle of light," in 1905 (Planck himself thought of discrete portions, *quanta*, of energy, rather than of particles); Bohr's 1913 theory of the hydrogen atom, extended, primarily by Bohr, Arnold Sommerfeld, and others to larger atoms during the decade to follow; Einstein's further work on Planck's law in 1909 and 1916; Louis de Broglie's conjecture, introduced in the early 1920s and quickly confirmed experimentally, that not only radiation but also the elementary constituents of matter, such as electrons, exhibit the same dual, particle-like and wave-like, aspects in their behavior; and Wolfgang Pauli's exclusion principle, which imposed rigorous constraints, fundamentally quantum in nature, upon the behavior of electrons in atoms. Although established by means of the old quantum theory, Pauli's exclusion principle and, to some degree, de Broglie's theory, may be seen as on the borderline between the old quantum theory and quantum mechanics, with which the *new* quantum theory, quantum theory as it is currently constituted, commenced. Even closer to quantum mechanics was the work of Satyendra N. Bose, and following him, that of Einstein (who used de Broglie's ideas) on Planck's law and quantum statistics published just before the discovery of quantum mechanics by Heisenberg in 1925.

It is worth noting (this is often forgotten retrospectively) that the old quantum theory was spectacularly successful in many respects, and parts of it are still used now, sometimes with surprising successes.[8] It did have, however, major physical problems, which ultimately proved to be insurmountable for the theory and which quantum mechanics, discovered by Heisenberg and Schrödinger in, respectively, 1925 and 1926, was able to solve, while retaining all those predictions of the old quantum theory that were correct. The consistency between both theories on that score was an important part of the development of quantum mechanics. Quantum mechanics was further developed in the work of Born, Jordan, Dirac, Pauli, and (primarily in terms of interpretation) Bohr. One should also acknowledge the contribution of lesser known but highly accomplished theoretical physicists, such as Hendrik Kramers, Ralph Kronig, Cornelius Lanszos, and others, and of course of many experimentalists.[9] The theory was nonrelativistic and dealt with the motion of electrons at speeds significantly slower than those of light, although the initial work on relativistic quantum theory, quantum electrodynamics, was virtually contemporary, and Dirac introduced his famous relativistic equation for the electron in 1928. Schrödinger's equation treats the behavior of the electron in quantum mechanics.

While, however, quantum mechanics resolved most *physical* difficulties that beset the old quantum theory and brought with it a certain closure of nonrelativistic quantum theory by becoming and remaining ever since the standard theory, it brought with it new and more radical *epistemological* complexities.[10] Indeed, to some, beginning, again, with Einstein, these complexities were even more troubling than those of the old quantum theory. Bohr registered this fact by noting, in 1929, that in this regard quantum mechanics proved to be a "disappointment" to some, and it has remained a disappointment to many ever since (Bohr 1929, *PWNB* 1, p. 92). The majority of even the most resilient critics, Einstein and Schrödinger among them, acknowledged that quantum mechanics brought with it considerable improvements as concerns the predictive capacity of quantum theory. What bothered these critics and even some proponents was the manifest deficiency of the explanatory-descriptive capacity of quantum mechanics with respect to quantum objects themselves. The situation was as follows.

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[8] See, for example, Svidzinsky et al. (2005).

[9] Most key founding papers on quantum mechanics are assembled in *Sources of Quantum Mechanics*, ed. B. L. van der Waerden (1968) and will be cited from this volume, hereafter referred to as *SQM*.

[10] Heisenberg spoke in terms of a "closed theory" in the following sense. While the technical work in such a "closed" theory would continue (in principle indefinitely), this work will, by and large, remain within the scope of an already established, even if not altogether fixed, conceptual architecture. The concept has some affinities with Thomas Kuhn's distinction between "revolutionary science" (corresponding to Heisenberg's open theory, such as quantum electrodynamics was in the late 1920s, as opposed to quantum mechanics) and "normal science" (closed theory) in *The Structure of Scientific Revolutions* (Kuhn 1962).

The old quantum theory dealt reasonably well with statistical matters and was or, rather, seemed to be analogous to classical statistical physics. What was lacking was the mechanics describing individual objects that would underlie the manifest statistical behavior of multiplicities in a manner similar to Newtonian mechanics, which explains causally the motion of individual molecules in the classical statistical theory. Bohr's 1913 theory of the hydrogen atom (and much of his work that followed it prior to quantum mechanics), Einstein's 1916 work on the so-called induced and spontaneous emissions, and several other earlier theories did deal with individual quantum processes, which were encountered already in the case of radioactivity and were predicted by the corresponding formulas.[11] While, however, describing well the atomic spectra in terms of the discontinuous transition ("quantum jumps") of electrons in the atom from one energy level to another (even at the expense of both classical mechanics and classical electrodynamics, both incompatible with such a view), Bohr's theory did not account for the mechanism of this transition. Indeed, the theory was *incompatible* with classical mechanics and classical electrodynamics alike. Einstein's arguments just mentioned suggested that probability might be irreducible even in considering the individual quantum processes (rather than those involving large multiplicities of quantum objects), yet one more of Einstein's revolutionary insights and a defining feature of quantum theory ever since, which, however, was also to contribute to Einstein's discontent with quantum mechanics later on. This point was amplified by the 1924 proposal of Bohr, Kramers, and John Slater (BKS), which aimed to resolve some of the difficulties of the old quantum theory by suspending the strict application of the energy conservation law in quantum processes, where the law would only apply statistically (Bohr et al. 1924). The proposal, controversial to begin with, was abandoned in view of the new experimental findings, most especially those by Walther Bothe and Hans Geiger, which confirmed the exact energy conservation in quantum interactions. These works, thus, further showed that the true mechanics was lacking, before quantum mechanics revealed that such a "true" mechanics (i.e., if conceived on the model of classical mechanics) was perhaps no longer possible. It became clear almost immediately in the wake of Planck's discovery of his law, however, that, contrary to Planck's original argument (accordingly incorrect in this respect, as Einstein was among the first to show), this law was incompatible with a classical-like underlying picture. For that reason the way of statistical counting is different in classical and quantum statistics. Planck's counting was correct, even though part of his physics was wrong, an error that, as Einstein observed, was most fortunate for physics.[12]

[11] For useful discussions and references on Einstein's work on this subject, see Pais (1982, pp. 402–414) and Pais (1991, pp. 191–192).

[12] See Einstein's discussion, leading from his commentary on the imperfections of Planck's argument to his assessment of Bohr's 1913 theory of the hydrogen atom as "the highest form of musicality in the sphere of thought" (Einstein 1949a, pp. 37–43).

The new quantum mechanics, which was expected to resolve these problems, was, however, nothing like classical mechanics, and hence was not a theory that was expected or hoped for by most at the time either. Certain aspects of the old quantum theory, in particular, again, Bohr's 1913 theory of the atom and most of Einstein's work on quantum theory, were harbingers of the new, and to many disconcerting, features which quantum mechanics was to retain and enhance rather than to eliminate, as some hoped a proper mechanics would or should. Skipping for the moment greater epistemological complexities, the new quantum theory could only predict, in general only probabilistically or statistically, the outcome of certain events, such as collisions between particles and a silver bromide photographic screen, but it appeared unable to describe the motion of quantum objects in a manner analogous to classical physics. In short, quantum mechanics would *predict* the outcome of the experiments in question (classical-like theories would fail to do so), but it would *not describe* the behavior of physical objects in the way classical physics would. Indeed, classical mechanics predicts *because* it describes.

Nor, again, would quantum mechanics predict in the same way either. Far from having eliminated chance at the ultimate level of the theory (as it would be in classical statistical physics, where the ultimate underlying objects and processes are treated as causal), quantum mechanics gave chance a more radical character. It made chance irreducible in principle, even and in particular in dealing with individual, rather than only collective, behavior. At least, it suggested that such might be the case, since in principle one could still contemplate or hope for the ultimate causality of quantum nature and the (idealized) realism of a future theory describing it. Peculiarly, the collective behavior could exhibit a certain correlational order in certain circumstances, reflected in what was previously perceived to be a wave behavior of quantum objects. Thus, quantum mechanics does not proceed in the way classical statistical physics does, from causal and, often, deterministic individual behavior to statistical collective behavior. Instead it combines or rather responds to a combination, found in nature, of the irreducibly lawless individual behavior with a relatively ordered (statistically correlated) collective behavior, which combination is one of the great mysteries and miracles of quantum phenomena. This combination leaves space for probabilistic predictions, which space quantum mechanics appears to use maximally. But it leaves little, if any, space to the description of the actual physical processes responsible for such predictions.

This, as Bohr was eventually to call it, "entirely new situation as regards the description of physical phenomena" (Bohr 1935b, p. 700) was not helped by Bohr's understanding of quantum mechanics in terms of complementarity, arguably the first reasonably consistent attempt to interpret both quantum phenomena and quantum mechanics, introduced in 1927. There are several reasons why Bohr's argument for complementarity, while welcome to some, offered little to alleviate epistemological discontent concerning quantum mechanics. Most significant among these reasons were, and still are, the following two. The first is the apparent impossibility to have a proper (by the standards of classical physics) hold on the actual behavior of individual quantum systems and in particular to offer a realist and, especially, causal description of this behavior, a situation to be understood in this study as epistemologically "nonclassical," the concept, again, to be properly explained in the Introduction. The second, in part correlative, is that the theory retains the irreducible role of randomness and probability, as against the causal character of classical mechanics, at least in principle and in the case of idealized models. But then, from Galileo and Newton on, modern physics deals only with such idealized models (which then can be used in considering actual physical objects); and no such models appear to be possible, and on the view to be ultimately adopted in this study are not possible in quantum theory. Similarly to Heisenberg in his work on his new quantum mechanics (as discussed in Chapters 3 and 4), Bohr saw these complexities as a way to a solution rather than a problem. He built the edifice of his interpretation of quantum mechanics upon both features, the lack of realism and the lack of causality in the quantum mechanical description of the phenomena in question. This, however, was not the kind of solution that would be palatable to Einstein and others who shared his concerns. In addition, Einstein and others argued (correctly, at least at the time) that certain questions concerning the quantum mechanical account of these phenomena remained unresolved. It took Bohr another decade and, in some respects, even longer to resolve some of these difficulties, although not everyone, beginning, again, with Einstein, accepted this resolution as satisfactory. The situation led to an intense debate, in particular to the great confrontation between Einstein and Bohr concerning, to use the title phrase of Bohr's 1949 seminal essay "Discussion with Einstein on Epistemological Problems in Atomic Physics" (Bohr 1949), "epistemological problems in atomic physics." This confrontation began in 1927 and has overshadowed the history of quantum mechanics and the debates concerning it ever since, debates that still continue with undiminished intensity.

Bohr's thinking, on the philosophical side, and that of Heisenberg, on the physical side, shape the argument of this book most significantly, and most of the book is devoted to them. One of the book's objectives is to reconsider, from a perspective that joins epistemology and probability, Bohr's work on complementarity and its significance in the history of both modern physics, from Galileo and Newton on, and modern philosophy, from Descartes on. First of all, this reconsideration concerns the significance of probability for Bohr's thinking. The epistemological problematic, especially the questions of reality and causality, has been the primary concern of most studies devoted to Bohr and many commentaries on his work, although this book offers a more radical view on the subject than most of the previous treatments. On the other hand, the book's focus on *probability*, rather than *only on causality* or *determinism*, and especially the book's affinities with the Bayesian view of probability are unusual in the literature on Bohr, or on Heisenberg and Schrödinger, and this focus is among the distinctive new contributions of the book's project. (I shall properly define the terms "reality," "causality," "determinism," "probability," and “Bayesian probability” in the Introduction.) This shift of the primary focus to the question of probability also distinguishes the book from the previous treatments of Bohr and quantum theory by the present author (e.g., Plotnitsky 2006b). I shall argue, however, that probability had a much greater significance in Bohr’s thinking and work, or those of Heisenberg and Schrödinger (in the latter case along more classical lines of resistance to the lack of causality in quantum mechanics), than has been commonly acknowledged.[13]

This focus also helps to clear some misconceptions concerning Bohr’s main concepts and arguments. (I am not saying that these arguments are always free of difficulties or problems.) In particular, I shall argue here that the wave–particle complementarity (the capacity of quantum objects to exhibit both the wave and the particle behavior, but each in different, mutually exclusive circumstances), with which the concept of complementarity is arguably associated most, did not play a significant, if any, role in Bohr’s thinking or that of Heisenberg. I shall further argue that it was not seen by either as a rigorously defined or even rigorously *definable* concept, a view that this study will support and amplify, in part by using the relationships between epistemology and probability in quantum theory. One might even say, perhaps a bit too strongly, that the wave–particle complementarity is one of the greatest and least productive fictions that have accompanied the history of reading Bohr and of quantum theory and its interpretation in general. To be candid, the present author, too, on occasion contributed to propagating this fiction in his earlier work, although not for quite some time now.

Nor, I shall also argue, can one rigorously speak of the mutually exclusive or complementary nature of the space–time description (provided by an act of observation, which uncontrollably “disturbs” the behavior of quantum objects) and the causality of independent, “undisturbed,” behavior of quantum objects. This is another common and persistent misconception, and Bohr briefly entertained the idea himself. It was proposed by him in 1927 in the so-called Como lecture and even was used to define the concept of complementarity (Bohr 1927, *PWNB* 1, pp. 53–54). Bohr, however, quickly abandoned this idea, in part under the impact of his initial exchanges with Einstein in 1927. He also significantly refined and gave a proper rigor to the concept of complementarity itself.

While the concept was not quite ever stated in the form about to be formulated here by Bohr himself, a more rigorous meaning of complementarity may be surmised from his later formulations, although one would generally need several of Bohr’s separate statements to properly establish this definition. He comes closest to it in his 1949 “Discussion with Einstein on Epistemological Problems in Atomic Physics.” Bohr states there: “Evidence obtained under different experimental conditions cannot be comprehended within a single

[13] A notable exception is F. S. C. Northrop’s “Introduction” to Heisenberg’s *Physics and Philosophy* (Northtrop 1962; Heisenberg 1962), which is focused on the role of probability in Heisenberg’s book.

picture [i.e., is mutually exclusive], but must be regarded as *complementary* in the sense that only the totality of the phenomena exhausts the possible information about the objects" (Bohr 1949, *PWNB* 2, p. 40). Accordingly, complementarity is defined by (a) a mutual exclusivity of certain phenomena, entities, or conceptions; and yet (b) the possibility of applying each one of them separately at any given point; and (c) the necessity of using all of them at different moments for a comprehensive account of the totality of phenomena that we must consider. Part (b) is not stated in the above formulation from "Discussion with Einstein" either. It can, however, easily be established from Bohr's other elaborations there, such as the one, via the Compton effect, immediately following this formulation (Bohr 1949, *PWNB* 2, p. 40) or elsewhere, especially in his reply to EPR, where, as will be seen, Bohr's whole argument essentially depends on this possibility. Parts (b) and (c) of this definition are just as important as part (a); and to miss or disregard them, as is often done, is to miss much of the import of Bohr's conception. As noted above and as will be discussed in detail in Chapter 5, Bohr himself to some degree contributed to this misunderstanding by his Como argument. The complementarity, just mentioned, of the space–time description, obtained by observation, and the causality of the independent, "undisturbed," quantum behavior, central to the Como argument, does not properly conform to this more rigorous definition. Arguably the most significant examples of complementarity rigorously following this definition are those of the complementary nature of the position and the momentum measurements, and of the space–time coordination and the application of momentum or energy conservation laws (there are, thus, two complementarities here), correlative to Heisenberg's uncertainty relations (e.g., Bohr 1958, *PWNB* 3, p. 5).

This conception of complementarity will be termed here "*complementarity in the narrow sense*." Because complementary phenomena are characteristic of quantum vis-à-vis classical physics, the concept also guided Bohr's overall thinking concerning quantum phenomena and quantum mechanics. Ultimately the concept came to ground his interpretation of jointly both, and the term complementarity came to designate this interpretation as well, to be termed here "*complementarity in the broad sense*."

It would, however, be difficult and, I would argue, impossible to properly consider complementarity without substantively addressing the work of Heisenberg and Schrödinger, the co-discoverers of quantum mechanics in, respectively, its matrix and wave versions. Although based on very different physical approaches and philosophical views, the two versions quickly proved to be equivalent in terms of their mathematical formalism and their predictive capacity. Both discoveries, and the ideas that led to them, had a decisive influence on Bohr's thinking. The contributions of Heisenberg and Schrödinger are also extraordinary in their own right, physically, mathematically, and philosophically, including in shaping our understanding of the relationships between epistemology and probability in quantum theory. Accordingly, this study is equally concerned with their work.

The influence of Bohr's thinking upon that of Heisenberg and that of Heisenberg's on Bohr's is well known, although the latter has been given less attention in the literature, as against the more symmetrical treatment of the case offered in this study. Arguably, the greatest examples of this mutual influence are the impact of Bohr's ideas on Heisenberg's thinking leading to his discovery of quantum mechanics and, conversely, that of Heisenberg's ideas and findings, in particular, the uncertainty relations, on Bohr's work on complementarity. Heisenberg's later (roughly post-1930s) thinking concerning quantum theory retains its affinity with Bohr's thought, but it also departs, sometimes significantly, from Bohr's views. While Heisenberg's later ideas will be given less attention in this study than his earlier work on quantum mechanics, considering them is important for my overall argument. There were also differences in their views at earlier stages of Heisenberg's work on quantum theory, which I shall address as well. It may be argued, however, that the affinities between their respective ways of thinking were more prominent and significant than the differences at all stages of their thinking.

Schrödinger's case is different. His wave mechanics was crucial for Bohr's early thinking concerning complementarity, in this case (as against that of Heisenberg's work) in contrast to his subsequent thinking, which led to significant refinements and, in some cases, the abandonment of his earlier ideas. Bohr, of course, continued to recognize the significance of Schrödinger's equation itself. Schrödinger's physical ideas and philosophical views are a different matter altogether. These views have a very different genealogy, and his philosophical ideas are in direct conflict with those of Bohr and Heisenberg, at least from the time of Schrödinger's invention of his wave mechanics on. Intriguingly, some of Schrödinger's earlier views concerning, indeed *against*, causality were similar to those of Bohr and Heisenberg. This conflict led to a number of heated exchanges between them, without, it is worth noting, ever having affected their mutual respect for each other. These exchanges and Schrödinger's opposition to Bohr's epistemological position helped Bohr to sharpen his ideas, arguably as much as did the affinities (or differences) between his philosophical thinking and that of Heisenberg. The primary positive role of both Heisenberg's and Schrödinger's work for Bohr's thinking was, however, defined by the extraordinary physics this work contained.

Schrödinger's philosophical views were, at least, again, from the time of the creation of quantum mechanics on, close to those of Einstein, with whom he had close intellectual affinities and important exchanges on quantum theory throughout his life. The two shared a discontent concerning quantum theory, which both of them helped to create (Einstein at earlier stages of quantum theory), and for which Schrödinger's wave mechanics, as against Heisenberg's theory, initially offered better prospects in their view. These hopes for Schrödinger's theory, however, failed to materialize, since it proved to be all but impossible to bring its mathematics in line with these philosophical desiderata. Schrödinger's view of quantum theory, even if not his general philosophical orientation, ultimately diverged from that of Einstein as well.

Einstein's critique of quantum mechanics and his great confrontation with Bohr concerning, to return to Bohr's expression, "epistemological problems in atomic physics" have overshadowed the history of quantum mechanics and of the debates concerning it. Einstein's name may, thus, be conspicuous by its absence in this book's title. On the other hand, Einstein's thought is hardly absent in the book itself. Indeed, it would be impossible to avoid his towering presence in the history of quantum theory, which he helped to shape and advance both at the earlier stages of its development and, against the grain of his own views, in his later criticism of quantum mechanics. His ideas and arguments are repeatedly invoked and are often considered in detail throughout the book. Although several other founding figures of quantum theory, such as Born, Dirac, Pauli, and von Neumann, will also be extensively addressed, Einstein remains the most frequent guest in this study as much as he is in the works of Heisenberg, Schrödinger, and, most especially, Bohr. Einstein and Schrödinger also became uncompromising critics of quantum mechanics and, especially, of "the spirit of Copenhagen," as Heisenberg called it (which, as I shall explain in the Introduction, is not the same as "the Copenhagen interpretation" of quantum mechanics). Their criticism helped both Heisenberg and Bohr refine and develop their key physical and philosophical ideas, and this criticism is, accordingly, important for this study.

In addition, as I said, Einstein made several extraordinary contributions at earlier stages of quantum theory, beginning with the idea of photon, but far from ending there. These contributions were indispensable to the emergence of quantum mechanics. Later on, in 1935, as part of his critique of quantum mechanics, he also introduced, in an article cowritten with Boris Podolsky and Nathan Rosen, the famous thought experiment, known as the experiment of Einstein, Podolsky, and Rosen or the EPR *experiment*, to be distinguished from EPR's *argument* concerning this experiment. The experiment posed deep questions concerning quantum phenomena and quantum mechanics, in particular the question of correlations, "the EPR correlations," between distant (spatially separated) quantum events. The experiment has had a decisive impact on foundational thinking in quantum mechanics, especially from the 1960s onward, following Bell's theorem and related developments, which extended and amplified the problematic established by the experiment. The correlations themselves in question are sometimes also known as the EPR–Bell correlations.

Indeed, in large measure, this problematic defines the current stage of this thinking and the debate (as intense as ever) concerning quantum phenomena and quantum mechanics. The subject is, accordingly, important for the argument of this book, and will be addressed in detail in it, especially in Chapters 8 and 9, where the concepts just mentioned will be properly explained as well. As will also be seen in Chapter 8, however, the essential questions at stake in the EPR experiment, specifically those concerning reality and locality and their relationships, were raised and reflected upon by Bohr already in 1925, even before (albeit only by a few months) Heisenberg's introduction of quantum mechanics. This is one of the reasons for Bohr's contention that EPR's argument contained nothing essentially new. While this contention may not be altogether justified, Bohr has a point. It is also worth noting that the concept of entanglement [*Verschränkung*], which reflects one of the most crucial aspects of the EPR experiment, arguably the most central to the developments just mentioned, was introduced by Schrödinger, rather than by Einstein.

Important as it may be, however, the EPR–Bell problematic is only a consequence, one of many consequences, of the situation defined by quantum phenomena and quantum mechanics, established well before this problematic was introduced as such. I would even argue that, while productive, centering the discussion and debate concerning quantum theory on the EPR–Bell problematic and entanglement has led to overfocusing and sometimes narrowing our quantum-theoretical thinking as concerns the foundations of both quantum mechanics itself and higher level quantum theories, such as quantum field theory. I would also contend that the discovery of quantum mechanics was a much more momentous discovery than the EPR experiment or Bell's theorem, which, combined, acquired a dominant and sometimes nearly fetish-like status in recent discussions. This assessment is not aimed to deny the achievement or significance of either contribution, but instead to put quantum theory and its history into a broader and, I would argue, more balanced perspective. Quantum mechanics, especially keeping in mind its extension to quantum electrodynamics and quantum field theories, is arguably the single most important discovery in the history of quantum physics. It might be added that it has also had a major impact on the twentieth- and by now twenty-first-century mathematics, including, as will be seen, in some among the most advanced areas of mathematical research.

Accordingly, my subtitle designates, with Heisenberg and Schrödinger, the creators of quantum mechanics and, with Bohr, the figure whose contribution was uniquely significant for the history of the interpretation of quantum phenomena and quantum mechanics, especially from the perspective of the relationships between epistemology and probability, my main subject in this study. This significance can, I would argue, be ascertained regardless of how one assesses Bohr's own interpretation of both as complementarity, although the present study of course views this interpretation as a major achievement as well.

More generally, I aim to focus this study on the *positive* significance (physical, mathematical, and philosophical) of our understanding of quantum phenomena and of quantum mechanics, as an effective theory of these phenomena, rather than on the criticism that it elicits, even though this criticism, again, remains important, including for our understanding of this positive significance. Part of this significance, especially in the case of Heisenberg's work, is defined by the connections between quantum mechanics and Bohr's work in quantum theory preceding quantum mechanics, especially, as mentioned above, his 1913 theory of the hydrogen atom, which brought him his Nobel Prize. This theory already reflected some among the physical and epistemological complexities that came to define quantum mechanics and complementarity. The work of other figures mentioned above was important as well, in particular, again, the work of Einstein that preceded quantum mechanics and the work of Dirac on both quantum mechanics and, especially, on quantum electrodynamics, the invention of which by Dirac and Jordan closely followed the invention of quantum mechanics. However, since the rise of modern physics in the work of Galileo and Newton, nothing, with the possible exception of relativity, can in my view be compared to the significance of quantum mechanics for our thinking about nature and physics, and for the nature of our thought and knowledge. The greatest philosophical significance of Bohr's thinking lies in capturing the essence and power of this transformation and in grounding complementarity in the fundamentals responsible for it. This transformation, however, would not be possible apart from the discovery of quantum mechanics by Heisenberg and Schrödinger.

The physical significance of quantum mechanics is hardly in question and has been readily acknowledged even, as I said, by those who have been dissatisfied with it philosophically, beginning with Einstein. On the other hand, as indicated above, the case is complex as concerns the philosophical questions raised by quantum mechanics or, again, already by earlier developments of quantum theory, beginning with Planck's discovery of quantum phenomena in 1900. Historically, while Bohr's philosophical thought had a major impact on several figures mentioned above, most especially Heisenberg and Pauli, it is difficult to think of a *positive* shaping impact of the philosophical thought of any of these figures on Bohr. Although Heisenberg was an exception in this respect, the philosophical impact of Heisenberg on Bohr was primarily a result of interactions between them, whose philosophical aspects were, at least at the time of the creation of quantum mechanics, largely shaped by Bohr's thought, as against the physical and mathematical aspects of these interactions, shaped primarily by Heisenberg's ideas.

Bohr's thought was, again, greatly helped by criticism, specifically that of Schrödinger at earlier stages of Bohr's thinking concerning quantum mechanics and, most crucially, that of Einstein, to the point of transforming Bohr's initial argument concerning complementarity almost immediately after its introduction in 1927. Einstein's skeptical attitude toward and his criticism of quantum mechanics continued to shape Bohr's thinking on quantum physics for the rest of his life. Indeed it appears that Bohr's active work on complementarity, which, as he said, was his *life*, pretty much ended with Einstein's death in 1954. One is tempted to argue the case, even though there are subsequent writings on quantum mechanics and complementarity, and some important interviews, literally until the day he died, just (literally the next day) after his interview with Thomas Kuhn in 1962 (Bohr 1962). Bohr appears to have needed Einstein to arrive where he did, even though and perhaps because Einstein did not see the road taken by Bohr as possible for himself (he admitted that it was possible in principle) and never stopped his search for, to him, epistemologically more palatable alternatives. Bohr did not appear to have needed Einstein (only Heisenberg and Schrödinger) to start on this road by introducing complementarity in the Como lecture. He appears, however, to have needed Einstein to continue to move forward, at least in the way he did, which, as I shall argue, was a powerful way to do so. On the other hand, helped by Einstein, Bohr had reached quite far, perhaps as far as it appears possible to reach on this particular road, at least for now. Would he have arrived there without Einstein? It would be difficult to argue that this would not have been possible, and it is intriguing (albeit very hard) to contemplate how Bohr's thought on quantum mechanics would have developed if Einstein had taken a positive, rather than critical, view of quantum mechanics. As it happened, Einstein's critical impact seems to have been an overdetermining factor.

Einstein was preoccupied with the subject all his life as well and continued to comment on the subject until his death, although, unlike his previous work on quantum theory, his contribution remained limited to his criticism of quantum mechanics. This criticism was, again, far from unproductive, but it was only a criticism, except when it was made against his own grain, as in the case of his conception of the experiments of the EPR type, which laid fertile ground for much productive positive thinking concerning quantum phenomena and quantum theory, including of the kind to be explored here. As his response to Bohr's argumentation shows, this kind of thinking was not something Einstein was willing to accept as a "useful point of departure for future development" (Einstein 1949a, p. 83). He did acknowledge this thinking to be "logically possible without contradiction," but found it "contrary to his scientific instinct" (Einstein 1936, p. 375; Bohr 1949, *PWNB* 2, p. 61).

But then, we need not be always guided by Einstein's scientific or (which is really the case here) philosophical instincts, and we should not be afraid to follow other paths or be in turn critical of Einstein. Einstein is not God, any more than Bohr or anyone else is. When Friedrich Nietzsche famously said that God is dead, he meant that all human gods are now dead, too, for us. More generally, significant and even unique as the contributions of these figures might have been, they, their names, function in this study primarily as the indicators of or signatures (sometimes collective) under conceptual formations or problems, each of which requires a critical exploration and analysis. In other words, each of these names represents a conceptual field under investigation in interaction with each other. This is, for example, how I would see Bohr's essay "Discussion with Einstein on Epistemological Problems in Atomic Physics," beginning with the title (Bohr 1949, *PWNB* 2, pp. 32–66). It is a confrontation of fields of thought and more than two such fields. For this confrontation involves fields of thought to which history assigns names or signatures other than Einstein or Bohr, in particular Heisenberg and Schrödinger, but also Planck, Rutherford, de Broglie, and others.

Whatever positive, shaping philosophical influences Bohr's thinking concerning complementarity might have had, these influences came from elsewhere, for example, from Bernhard Riemann's mathematical ideas (these ideas were also philosophical, however) or from the philosophical critique of causality, extending from David Hume and Immanuel Kant to Nietzsche. For

Nietzsche, the death of God (it has many meanings) is also the death of causality and determinism—when the latter are seen as gods governing thought. They do have their place, indeed their necessary place, in our thinking otherwise. A friend of Bohr's father, Georg Brandes, whom Bohr admired, was one of the early champions of Nietzsche and taught the first ever university course on Nietzsche at the University of Copenhagen. Primarily, however, Bohr arrived at complementarity, at least as an interpretation of quantum phenomena and quantum mechanics, through thinking *philosophically*, or both physically *and* philosophically, *through physics*, the physics created, along with Bohr himself, by Heisenberg and Schrödinger, and other figures just mentioned. It was above all physics that made complementarity both possible and necessary for him, and that made it philosophy through a radical transformation of our understanding of the nature of scientific knowledge and, ultimately, of all human knowledge.

The same type of argument can also be made, and will be made here, for Heisenberg, who was compelled to make some of his radical epistemological moves, just as some of his radical mathematical moves, in order to solve physical problems he had to confront. By contrast, although physics was still crucial for him as well, Schrödinger's program for his wave mechanics was guided to a much greater degree by certain philosophical principles (classical-like in nature), in part in a deliberate juxtaposition to Heisenberg's matrix mechanics. The mathematical equivalence of both types of formalism became apparent later, albeit quickly, and Schrödinger was one of the first to establish it, which fact of course took from under his feet much of the ground for his objections to matrix mechanics. It is much more difficult to argue against mathematics than against philosophy, especially if it is the same mathematics that is used to support the opposing philosophy in which this argument was based. Schrödinger did not change his philosophy. Instead, he came to doubt and even to repudiate quantum mechanics, at least as a desirable way of doing physics, although he acknowledged that the theory and even understanding it in "the spirit of Copenhagen" (which remained philosophically deplorable to him) may have been imposed on us by nature itself. This view came to define, as he called it in his famous cat-paradox paper of 1935, "the present situation in quantum mechanics" (Schrödinger 1935a, *QTM*, p. 152), with, however, the hope at the time that it might change, in part in view of the EPR experiment, just introduced then, and perceptively analyzed by Schrödinger in the paper itself.

On that score the situation is not that different now: It is still "the present situation of quantum mechanics," although subsequent developments, including those around the EPR experiment, enriched it. The controversy surrounding quantum theory and the intensity of the debates concerning it have remained undiminished, which is a testimony to the impact of quantum theory on our thinking in physics and beyond. No end of either appears to be in sight, and, as they deepen our understanding of quantum phenomena and quantum theory, certain recent developments, such as quantum information theory, add more fuel to this controversy and the debates concerning quantum phenomena and quantum theory. One can witness the intensity of this continuing process, as I have throughout the writing of this book, virtually on a daily basis. Every other issue of *Nature* or *Science*, to mention only the two most prominent scientific journals, contains an article or a review that reflects and continues these debates.

At the same time, in spite of the enormous and ever-proliferating number of accounts of and commentaries on the subject, some of the deeper aspects of quantum physics and of the thought of the figures considered in this study, most notably, but far from exclusively, Bohr, often remain missed or misunderstood and as a result unexplored. I am not saying that these figures were always right. They were, as I said, not gods (although they might appear to be sometimes) and had to struggle hard in confronting the difficulties of quantum physics, and they have made mistakes. Such mistakes often help us understand the deeper complexities of quantum theory nearly as much as correct arguments offered by these figures, and sometimes more than do the latter. In particular, as noted above, Bohr's initial argument concerning complementarity in the so-called Como lecture of 1927 required considerable revisions, and they ultimately led Bohr to what was in effect a different interpretation of quantum mechanics. Although this study ultimately sides with Bohr and Heisenberg as against Einstein and Schrödinger, it by no means unconditionally accepts the arguments of the first two thinkers or unconditionally rejects the arguments of the last two. There are also differences, sometimes significant, between Bohr's and Heisenberg's views, or between those of Einstein and Schrödinger, which differences play their roles in shaping this study's view concerning quantum theory.

Of course, even leaving aside those parts of this study's argument that expressly depart from those of Bohr, Heisenberg, and Schrödinger, this study can only offer an *interpretation*, one among several possible interpretations, of quantum mechanics itself or of the views held by each of these figures, or by any other figure considered in this study. This interpretive "inflection" is unavoidable, no matter how close one's reading is or how attentive to the proper norms of rigor and scholarship. Respecting such norms is imperative, for otherwise one would be free to say just about anything and unable to argue for the rigor or validity of one's interpretation or against the problems of other interpretations. Both types of argumentation are essential for maintaining the intellectual and ethical integrity of our knowledge and discussions. It is a greater conceptual and historical rigor of reading that allows us to understand better certain key aspects of the thought of Bohr or others, such as the greater than previously perceived significance of mathematics in Bohr's work. It also allows us to see the problems of other readings or inflections of Bohr, Heisenberg, and Schrödinger.

There is, however, no uninflected reading that would guarantee us the true meaning of a given work; not even a reading by Bohr himself could do so, although, were he around, he *might* have done better than most other readers. Might! For, as will be seen, he is not always helpful or does not always avoid misreading or at least "inflections" in readings of his earlier works in his later works. Bohr was interminably interpreting and reinterpreting quantum mechanics and his interpretation of it in the process that was, as he said, "his life," and that indeed was terminated only by his death, speaking nearly literally, since he was explicating and, given the way his thought worked, quite possibly reinterpreting his interpretation in an interview with Kuhn, cited above, just before he died (Bohr 1962). Hence, my quotation marks around "inflected." For, given these (irreducible) conditions, the question would be: Against what uninflected meaning does such an inflection occur? Even if it existed, such an uninflected meaning could never be available, although, as just indicated, one can speak rigorously of relative inflections, or one might say inflections between earlier and later inflections. In any event, Bohr is no longer around to help us, and neither are Heisenberg and Schrödinger. We must try our best without them, and pay our debt and tribute to them by trying to do our best in reading them, both for the reasons of scholarly rigor and, more crucially, in order to gain a better understanding of quantum theory.

The ultimate aim of this book is to contribute to this understanding, and to the understanding of the reasons for its extraordinary impact and for the controversies surrounding it, in particular, again, those related to the difficulties of developing a realist and causal theory of quantum phenomena of the type classical mechanics offered for classical physical phenomena. One might feel, with Einstein, Schrödinger, and others, such as John S. Bell (of Bell's theorem fame), that the beauty and power of classical physical and epistemological thinking or what Schrödinger called the "classical [physical] ideal" are lost with quantum mechanics (Schrödinger 1935a, *QTM*, p. 152). On the same occasion, Schrödinger referred to quantum mechanics in the type of interpretation to be adopted here as "the doctrine born of distress" (Schrödinger 1935a, *QTM*, p. 154). One could understand and even sympathize with Einstein's or Schrödinger's view of the "doctrine." One need not, however, agree with this view or share his sense, that of desperation, concerning "the present situation in quantum mechanics," which is still much the same as it was 70 years ago as concerns the essential features of nature and the theory considered by Schrödinger. While, however, this situation itself may not be that different *epistemologically*, we, I would argue, have a different and deeper understanding of this situation.

In part for that reason, the kind of feeling that the present author has or that this book aims to convey concerning quantum mechanics is quite different from that of Einstein and Schrödinger, even though this book does aim to convey the beauty and power of classical thinking, physical or philosophical, along with the beauty and power of quantum-theoretical thinking. We need both ways of thinking, even in quantum theory, let alone elsewhere in physics. Even if and to the degree that this loss of the classical ideal is unavoidable, it is, at least for some of us, compensated by gaining a different kind and a different understanding of thought and knowledge. This understanding is perhaps equally beautiful and powerful, if also more modest, since the unknowable and the unthinkable become a permanent part of our knowledge and thought. Nature, or our thought, given to us courtesy of the brain, appears to exceed the capacity of our thought to fully comprehend it. We are able, however, to comprehend something about it, actually quite a bit—not the least, with quantum mechanics, the limits at which the possibility of this excess must be posed as a rigorous physical and philosophical question. This is an extraordinary achievement of quantum-theoretical thinking, which, beginning with quantum mechanics itself, a great product of this thinking, is a testimony that the brain is one of nature's more remarkable products.

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Contents

Abbreviations

MR: J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, 6 vols. (Springer; Berlin, 2001).

PWNB: N. Bohr, *The Philosophical Writings of Niels Bohr*, 3 vols. (Ox Bow Press; Woodbridge, CT, 1987); N. Bohr, *The Philosophical Writings of Niels Bohr, Volume 4: Causality and Complementarity, Supplementary Papers*, eds., J. Faye and H. J. Folse (Ox Bow Press; Woodbridge, CT, 1998).

QTM: J. A. Wheeler and W. H. Zurek, eds., *Quantum Theory and Measurement* (Princeton University Press; Princeton, NJ, 1983).

SQM: B. L. van der Warden, *Sources of Quantum Mechanics* (Dover; New York, 1968).

Chapter 1
Introduction—Epistemology and Probability in Quantum Theory: Physics, Mathematics, and Philosophy

Abstract The introduction offers an outline of, as I shall term it here, the "nonclassical" epistemology of quantum mechanics and the corresponding view of quantum probability, which, jointly, ground the argument of this study. It also discusses the relationships among physics, philosophy, and mathematics from this perspective. Sections 1.1 and 1.2 give a general philosophical outline of nonclassical epistemology and the corresponding view of probability. Section 1.3 considers the relationships among physics, mathematics, and philosophy in quantum mechanics from this epistemological perspective. Section 1.4 offers a discussion of concepts, including the very idea, or *concept*, of concept, in physics, mathematics, and philosophy. I conclude in Section 1.5 with a discussion of the role of interpretation in quantum mechanics, and of the possible differences between Bohr's and the present views concerning the status of the two respective interpretations of quantum mechanics.

1.1 Classical and Nonclassical Epistemology

I argue in this study that quantum phenomena and quantum mechanics bring about and possibly require a new form of knowledge in physics and beyond, and a new understanding of the nature of knowledge itself, a new epistemology. Bohr, accordingly, speaks of "the entirely new situation as regards the description of physical phenomena" and of "the epistemological lesson of atomic physics" (Bohr 1935b, p. 700; *PWNB* 2, p. 91). There is some debate as to how far one needs to go in "renouncing customary demands as regards the explanation of natural phenomena," and, as Bohr said on the same occasion, he "had... only little success in convincing [his] listeners" that it may be necessary to go that far (Bohr 1949, *PWNB* 2, p. 63). Indeed, one finds several stages of this renunciation in Bohr's own thought, reaching its limit sometime in the late 1930s, following his exchanges with Einstein on the EPR thought-experiment, to be discussed in Chapters 8 and 9. This introduction gives a preliminary outline of the epistemology in question in its arguably most radical form, which I shall here term "nonclassical," and of the corresponding view of

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quantum probability. This epistemology more or less corresponds to Bohr's ultimate view of the quantum situation (in the present interpretation). A detailed discussion of the subject is given in Chapter 10. As will be seen in Section 1.5 of this introduction, there might be differences between the present view and that of Bohr. They concern the question of whether this epistemology is inevitable in our interactions with the quantum aspects of nature or is only part of a particular interpretation of quantum phenomena and quantum mechanics. In general, nonclassical epistemology allows for a certain spectrum of such interpretations, including as concerns the role of probability in quantum mechanics, for example, whether one sees quantum probability along the "Bayesian" lines, which is a view adopted here, or along the "frequentist" lines. (I shall explain these terms properly below.)[1]

The term "nonclassical" is in part due to the juxtaposition of classical and quantum physics. I leave relativity aside for the moment, although, as will be seen below and throughout this study, its borderline physical and epistemological status vis-à-vis classical and quantum physics is important. The difference between classical and quantum physics is essentially defined physically by the

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[1] The literature, whether technical, philosophical, historical, or popular, dealing with quantum mechanics is immense, literally having reached apocalyptic proportions in the age of the Internet; even the number of articles on the subject posted on the *arXiv*, while by no means exhaustive, is well beyond any possible survey. At least two dozen books and essay collections published during the last year alone attracted the attention of the present author. Either in selecting the available offerings or in offering one's own work, one literally deals with the "eBay" of quantum foundations, and the present study is no exception on either count. It is nearly an exception in this landscape as concerns the philosophical position it takes, which is shared by or is in harmony with only a few views concerning quantum foundations. Even the list of classic works is long and cannot easily be claimed to be definitive in view of the diversity of the views concerning virtually every aspect of quantum mechanics. This immensity is nearly matched by the number of interpretations of quantum mechanics, and only a small portion of them can be mentioned and even a smaller one addressed in this study, following its particular concerns. See, for example, <http://en.wikipedia.org/wiki/Interpretation_of_quantum_mechanics > for a still limited survey. There are other useful on line sources on various interpretations of quantum mechanics, such as *Stanford Encyclopedia of Philosophy*. Even within the cluster of the Copenhagen interpretations, to which Bohr's interpretation or, again, his several interpretations belong, the range of views is formidable, as manifest in the views of such founding figures as, in addition to Bohr, Heisenberg, Born, Pauli, Dirac, von Neumann, and Wigner. I shall comment on some of these views as I proceed. The profusion of new interpretations during recent decades was in part motivated by the EPR argument. It may, however, be seen as triggered by Bohm's reformulation of the EPR argument in terms of spin and then his hidden-variables interpretation, introduced in 1952. It received a further impetus from Bell's theorem (1966) and related findings, and then from Alain Aspect's experiments (around 1980) confirming these findings, which I shall address in Chapters 8 and 9. A similar profusion of literature is found when it comes to the key philosophical questions to be discussed here, such as reality and causality, in which case we indeed deal with a history extending from the pre-Socratics to the present. Accordingly, I shall adopt the same strategy as in dealing with literature on quantum mechanics, and shall, especially as concerns recent commentaries, refer only to a limited number of works that I found to be especially pertinent to my argument in my perusal of the philosophical literature.

role of Planck's constant *h* in quantum physics, and the currently standard versions of the mathematical formalism of the two types of theories are correspondingly different as well. Classical physics, however, can also be and usually is interpreted in epistemologically classical terms as well. By contrast quantum physics and specifically quantum mechanics appear to resist such an interpretation, even though one could not altogether exclude the possibility that they could be interpreted, or the fact that some do attempt to interpret them, in this way.[2] This is the main reason for using the term "nonclassical epistemology" as a particular form of epistemology to be specifically defined here, rather than the more graceful "quantum epistemology," which could suggest that no other epistemology of quantum theory than the one offered here has been entertained. Both the specific character of the experimental data in question in classical and quantum physics and the difference between the two respective mathematical formalisms may be brought into accord with the difference between classical and

[2] I am not referring, for the moment, to Bohmian and other hidden variables theories, even though they are manifestly realist and causal. First, however, they are mathematically different from quantum mechanics, and, secondly, they are problematic in view of their nonlocality, which makes them incompatible with relativity. Relativity disallows the physical connections or causal influences between events that propagate faster than the speed of light in a vacuum, *c*. Nonlocal theories, such as Bohmian mechanics, allow for and even entail such connections; indeed they allow for instantaneous connections of this type. See Bohm and Hiley (1993) for an exposition of the last version of the theory developed by Bohm himself (in collaboration with B. Hiley). Nonlocality in this sense is generally seen as a highly undesirable property for a theory to have, and, accordingly, other realist nonlocal theories, such as most forms of the so-called "spontaneous collapse" theories, are put aside here as well. As will be discussed in Chapters 8 and 9, the standard quantum mechanics appears to avoid it, although, as will also be discussed there, the question of the *locality* (in the sense of compatibility with relativity) of quantum mechanics or quantum phenomena is a matter of considerable subtlety and controversy. There are also alternative conceptions of nonlocality, some of which apply in the standard quantum mechanics, and I shall comment on them below. Unless specified otherwise, however, I shall use "locality" and "nonlocality" in the sense of, respectively, compatibility and incompatibility with relativity. I also leave aside the many-worlds interpretations of quantum mechanics (e.g., DeWitt and Graham 1973). These interpretations are manifestly realist and causal interpretations of the standard formalism, but remain too speculative and highly unlikely to be verifiable, although they have enjoyed a somewhat wider following in recent years. In any event, they are not of much interest in the present context, given their trivially realist and causal nature, although this is arguably the main reason for their appeal. I have in mind, in addition to some earlier versions, such as Richard Feynman's (Feynman 1948), certain versions of the modal interpretation, specifically that of Bas van Fraassen (van Fraassen 1991), Robert Griffiths's histories interpretation (Griffiths 2003), Roland Omnés's logical interpretation, a version of the histories interpretation (Omnés 1994), or Peter Mittelstaedt's minimal interpretation (Mittelstaedt 1998). All of these approaches claim some degree of realism, although usually not causality. It is a different question as to how successful these attempts are in reaching more classical or (given how different they are from classical physics) rather *less* nonclassical and specifically more realist interpretations of quantum mechanics, which appears to be their main aim. I shall refrain from a definitive judgment concerning these attempts and permit myself to refer to an earlier discussion of them in (Plotnitsky 2006b, pp. 84–86).

nonclassical epistemology. I shall also argue that the nature of quantum probability can be linked to nonclassical epistemology as well.

An epistemologically classical theory, such as classical physics, would determine all the *objects* or processes it considers as, in principle, knowable or at least conceivable, thinkable, usually by means of the theory itself. Classical physics is not the only known theory that is also epistemologically classical. Most known theories in physics or elsewhere are. These are nonclassical theories that are exceptions. I here understand the objects or processes of a given classical theory as entities that are defined in a given way *by this theory* itself, rather than as necessarily existing as thus defined in nature. Rigorously speaking, the processes in question are also idealized *objects* of the theory involved, but the distinction between "objects" and "processes" is useful in physics. Thus, in classical mechanics, we deal with idealized models of material entities in nature and their behavior. The objects considered by such models are abstracted from nature by disregarding many other features of these entities and their behavior, primarily in order to make these models mathematical. In other words, the idealizations and models of classical mechanics are those of what we actually observe in nature, such as planets in their motion around the sun. We start with objects and processes that are phenomenal representations of actual objects and processes, and then refine these representations in order to be able to use mathematical models in describing them. By contrast, quantum objects and their behavior cannot be either observed in the way classical physical objects are or, it appears, modeled on classical mechanics.

Phenomena are representations; that is, they are something that *appears to our mind*, to our conscious intuition, while objects, at least those considered in classical physics, that may be thus phenomenally represented are assumed to exist in nature (leaving aside hypothetical cases and thought-experiments). This distinction was the starting point of Kant's philosophy. Phenomena, as "object[s] of sensible intuition, i.e. as appearance[s]" are *what we know experientially through senses* (Kant 1997, p. 115). I shall retain this concept of phenomenon in considering both classical and quantum physics and classical and nonclassical epistemology. Phenomena are opposed by Kant to *noumena*, each of which is seen as "ein *Ding an sich*" or a thing-in-itself. Corresponding to the ancient Greek etymology of the word (something "thought-of"), noumena are those objects, material or mental, that we can only *think about*, rather than cognize as something represented in our mind and present to our intuition. Our knowledge concerning noumena can be at most indirect, inferential, and is often provisional, since our inference may be proven wrong at a later point. The concept of noumena applies in classical physics. The actual objects and processes in nature, to which classical physical objects and processes as *phenomenal* idealizations correspond, are noumenal. This, as I shall explain presently, is not necessarily the case in quantum physics, at least in a nonclassical view, which retains Kant's concept of phenomenon but radicalizes the difference between quantum phenomena and quantum objects beyond Kant.

While, however, the objects and processes in nature considered by classical physics are noumenal, most of classical physics, in particular classical mechanics, can disregard their noumenal nature by being able to idealize these objects and processes for the purposes of the mathematical descriptions it provides via the data obtained in the experiments and the equations of classical mechanics. Such descriptions also enable predictions of the behavior of actual objects in nature considered in classical physics, such as, again, planets moving around the sun. Classical physics can, thus, bypass the fact that it actually deals not with natural objects but with phenomena arising by virtue of our *interaction* with nature, which can, in principle, have a totally different constitution (as a Kantian thing-in-itself) than our phenomenal experience of it may suggest. Philosophically, this possibility was of course recognized and considered even before Kant, indeed beginning with the pre-Socratics, specifically the atomists, and Plato, and in the history of modern physics, for example, by both Descartes and Newton (specifically in their optical studies). This difference between natural objects as noumena and their representation as phenomena proved, however, to be inessential for the purposes of classical physics. This is primarily because we can observe the behavior of the corresponding objects in nature "without disturbing them appreciably," as against the situation that obtains in quantum physics, a point that became crucial for both Heisenberg and Bohr, whom I cite here (Bohr 1927, *PWNB* 1, p. 52). It is of course true that the Copernican picture of the solar system did not correspond to our direct phenomenal experience, which created some problems for Galileo in advocating it. One could, however, still intuitively visualize the Copernican system by forming the corresponding mental picture and indeed could actually draw such pictures, just as in the case of the motion of other physical bodies. Such (idealized) pictorial representations are found in Galileo's works.[3] The observations involved, again, do not affect the actual behavior of the planets observed, and we can make our predictions on the basis of this model, just as we can on the basis of the previous geocentric one. (With Kepler's discovery that planets move along elliptical orbits these predictions were improved considerably.) Later on, in the case of the kinetic theory of gases, it was understood, just as it was by the ancient atomists, that the atomic constitution of nature was not directly available to us. Nevertheless, individual atoms and their behavior would still be conceived on the classical model, specifically that of classical mechanics (hence, the language of "kinetic"), which enabled an effective statistical physical theory, even if it was not completely free of conceptual and epistemological complexities. One could, again, intuitively visualize this individual atomic behavior by forming a mental image

[3] See Plotnitsky and Reed (1991). Bohr's detailed drawings of classical apparatuses used in quantum experiments are aimed to reflect this situation as well by only showing bulky, heavy pieces of classical equipment with *traces* left by the interactions between quantum objects and these pieces (Bohr 1949, *PWNB*2, pp. 48, 49, 54). Bohr, thus, literally shows that we can draw what happens there, but we cannot draw anything that happens at the quantum level. Cf., Fig. 2.1 in Chapter 2 of this study.

of it. Accordingly, such objects could be effectively considered not only as thinkable (which defines Kant's noumena) but also as, in principle, knowable, even if indirectly, and in any event available to physical modeling, which could, through its descriptive idealization, enable excellent predictions concerning the observed phenomena and, hence, of the behavior of the objects considered. This view represents what Schrödinger called the "classical ideal" in physics (Schrödinger 1935a, *QTM*, p. 152).

This type of treatment of physical objects no longer appears possible in quantum theory, beginning with the fact that quantum objects (electrons, photons, and so forth, including composite quantum entities) are unobservable *as quantum objects*, although in certain circumstances some among quantum objects, individual or collective, can be treated as if they were classical objects. Indeed, the ultimate constitution of all observable classical objects is now generally believed (there are alternative views) to be quantum. The situation, needless to say, involves great physical and epistemological complexities, which are far from resolved and will concern this study throughout, beginning with the question of what one would accept or even consider a resolution of such a problem. Bohr and Einstein, for example, would irreconcilably disagree on this point. The situation, however, compelled Bohr to theorize or idealize *quantum objects*, first, as irreducibly different from what is observed in measuring instruments impacted by quantum objects, which observations define *quantum phenomena*; and second, more radically, as entities placed beyond quantum-theoretical description and even beyond any possible description, knowledge, and ultimately conception.

It is this epistemology—that of the classically knowable effects of the interactions between measuring instruments or other classical macro-objects and certain unknowable and ultimately inconceivable, unthinkable objects—that I define as "nonclassical." I, again, apply the term to quantum objects and phenomena as they are to be configured or (vis-à-vis nature) idealized by a given nonclassical interpretation. Nature itself is only assumed to possess a certain, quantum, level of constitution that is responsible for the appearance of certain knowable phenomena, manifest in measuring instruments or other macro-objects, which are considered as quantum phenomena and which can be physically treated by classical physics, although the latter fails to properly predict the data that are thus observed. These phenomena differ from those considered in classical physics only by the fact that in considering and, specifically, predicting them, the role of Planck's constant, h, cannot be neglected in the way it can be in classical physics. As against the level of nature described by classical physics, the quantum constitution of nature is interpreted as irreducibly inaccessible not only to any description but also, and more radically, to any conception that we can form, now and possibly ever. In other words, there is no *specific* form of ontology or reality that we can, even partially, assign to this constitution. This circumstance also precludes, in principle, the possibility of knowing the processes that lead to the effects of quantum objects upon our (classical) world, which are responsible for the appearance of quantum phenomena. It follows that, by virtue of involving an irreducible quantum stratum, the ultimate nature of the formation of classical objects from their elementary quantum constituents will remain beyond our grasp as well, if this interpretation holds. One might say that nonclassical epistemology idealizes quantum objects and their behavior by defining them as being beyond any possible idealization.

Accordingly, in this view the ontology of quantum objects and processes (to the degree one can even speak of ontology here, apart from the fact that such objects are assumed to exist) allows for no description, even in principle or by way of idealized models and, ultimately, for no conception that we can form. There is no account (no "story") to be given that would describe how these effects are possible. Einstein once famously asked Abraham Pais "whether [he] [Pais] really believed that the moon exists only when [he looked] at it" (Pais 1979, p. 907). Well, at least in the present view, the answer is yes! The moon exists *as the moon* only when there is somebody who can look at it. It does not exist as the moon if there is no one to look at it. This does not of course mean that nothing exists where we see the moon. But whatever it is—and we do not know and perhaps cannot conceive what it ultimately is—it would not be what we see as the moon, and we do not see the moon in its ultimate constitution, say, as a quantum object, or again, as something that compels us to speak of quantum objects. At the same time, for various reasons, for example, those arising in view of the EPR experiment, it is essential to assume the *independent existence* (*apart* from our interaction with them by means of measuring instruments) of certain objects in nature idealized via the concept of quantum objects as indescribable, unknowable, and ultimately inconceivable. That is, it is essential to assume the existence of something that compels us to use this language. It is this existence (also as the existence of something that had existed before we appeared in the Universe and that will exist when we ourselves will no longer exist) that is responsible for the effects in question, for quantum phenomena. In this sense, quantum objects may also be said to be *real*, even though it may not be possible to have a realist theory of them, even by way of idealization, and it would, again, be difficult to have any other theory, even in classical physics. Quantum objects are real insofar as they exist, but there is nothing we can say or, in the first place, think about their reality except as manifest in the effects of their interactions with measuring instruments upon those instruments.

This view of nature at the quantum level of its constitution may be seen as *ontological*, following the etymology of the term, derived from the ancient Greek, *on*, to exist, without, however, being *realist* as concerns any possible conception of this existence. While, however, I do want to stress the *independent material existence* of something in nature idealized via quantum objects, specifically as nonclassically understood, I am reluctant to use the term "ontological" in characterizing the present view, and prefer nonclassical. Made prominent in philosophical discussions during the last century by Martin Heidegger in his earlier works, in particular, *Being and Time* (Heidegger 1996), originally published in 1927, this term, too, is charged with certain, from the present viewpoint, undesirable connotations of a classical type.[4] I need to explain more properly what I mean by "realist" first.

By realist I understand theories or, more broadly, conceptions of the following two types, which understanding appears to be sufficiently comprehensive to absorb most uses of the term, including those to be discussed in this study. This view also makes the terms realist and classical, as here defined, just about equivalent, which is justified in the context of the history of physics, from Galileo or even Aristotle on. According to the first type of realism, a realist conception or theory would offer a mapping of the properties of the physical system considered and their behavior, which mapping may be exact or approximate, or displaced, as in Bohmian mechanics, for example. According to the second type of realism, one would presuppose an independent architecture of reality governing this behavior, even if this architecture cannot be mapped by a theory, however, partially or approximately, at a given point of history and perhaps even ever, but if so, only due to practical limitations. In this case, a theory that is merely predictive may be accepted for the lack of a realist alternative, but usually under the assumption that, enabled by our conceptual and mathematical imagination, a future theory will do better. This view defined, for example, Einstein's attitude toward quantum mechanics. What unites both conceptions of realism and ultimately defines realism most generally is the assumption, along Kantian lines, that this type of architecture, which may of course be temporal, objectively exists. In other words, realism is defined by an assumption that the ultimate constitution of nature possesses attributes that may be unknown or even unknowable, but that are *thinkable*, conceivable. In particular, it is deemed conceivable on the *model* of classical physics and its idea of physical reality, assumed to be at least partially approachable in an idealized way through the mediation of the mathematized concepts of classical physics. At the very least, realism assumes that the concept of organization can in principle apply to this constitution, no matter how much off the mark anything we can specifically come up with in approaching this constitution may be. The hope, however, is that we can capture, even if, again, approximately and by way of idealized models, something of this architecture.

Nonclassical epistemology not only does not make any of these assumptions, in particular, again, that any description of or even conception of quantum objects and their independent behavior, of the *nature* or *mode* of their existence, is possible, but it also disallows them. It does maintain the presupposition that *quantum objects* or, again, some entities in nature that they thus idealize exist independently of our interaction with them, and that it is their existence that is indeed responsible, through this interaction, for this situation. In other words,

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[4] The term ontological is sometimes used in Bohmian mechanics (Bohm and Hiley 1993), which relates to quantum objects and their behavior in space and time, and hence is, on the present definition, classical. Bohmian mechanics is of course different from classical mechanics.

the character of the existence of such entities is such that it disallows us to describe, or again, even to form a conception of, this nature. This impossibility would make such terms as "quantum," "object," or "ontology" in any sense that we can give to them, or any other terms or concepts (such concepts may not be given actual terms, may not be named) provisional and ultimately inapplicable to such objects. The lack of causality is an immediate consequence, given that quantum objects and processes are now beyond any possible description and even conception. Causality would be a feature of such a description and hence is disallowed automatically, as Schrödinger realized in his cat-paradox paper, again, by way of a very different assessment of this type of argumentation, which he saw as "a doctrine born of distress" (Schrödinger 1935a, *QTM,* pp. 152, 154). I shall return to causality in the next section.

For the moment, it follows that nonclassical epistemology extends and radicalizes Kant's epistemology of noumena or things-in-themselves vs. phenomena. For, while Kant's noumena are *unknowable*, they are still in principle *conceivable*, thinkable, even though it may be difficult to verify our conceptions of them (Kant 1997, p. 115). By contrast, quantum objects do not appear to allow us to form any conception of them and their behavior. They are literally unthinkable. On the other hand, the physical *phenomena* considered by quantum mechanics, quantum phenomena (which, just as phenomena considered in classical physics, we can perceive, know, and describe), appear only as a result of the interactions between quantum objects and measuring instruments, a crucial point for Bohr and the present study. As I said, in Bohr's and the present interpretation, quantum phenomena themselves are physically described by classical physics, which cannot, however, either properly predict these phenomena or describe their emergence. But then, quantum mechanics cannot describe their emergence either, and in a nonclassical view, nothing can. It follows that in this interpretation measuring instruments are quantum at one end, where they interact with quantum objects, and classical at the other, where the effects defining quantum phenomena are manifest. Accordingly, the first stratum is not describable by quantum theory or otherwise and is ultimately inconceivable, while the second stratum is described by classical physics. This double—quantum at one end and classical at the other—character of the constitution of measuring instruments is an often missed point on the part of critics of Bohr.

In this view, while quantum objects or, again, certain entities in nature that make us speak in these terms do exist independently of our interaction with them by means of measuring instruments, they cannot be meaningfully considered apart from these interactions. Nothing could be rigorously ascertained concerning what actually happened between experiments or how what is manifest in measuring instruments as a result of their interactions with quantum objects came about. By the same token, quantum mechanics itself does not describe quantum objects and their behavior, but only predicts, in general probabilistically, the numerical outcomes of experiments, which, due to the interactions between quantum objects and measuring instruments, lead to the appearance of quantum phenomena. This view also suggests a different and, in a way, *truer* sense of the term "experiment" itself, which becomes possible in quantum physics. For, it is not, as in classical physics, merely the independent behavior of the systems considered that we track, but what kinds of experiments we perform, how we *experiment* with nature, that defines what happens.[5] Of course, the practice of physics, both theoretical and experimental, has always been, from Galileo (and before) on, defined by the freedom and ingenuity, *art*, of experimentation, a creation of new configurations of thought and knowledge, as have been other sciences, mathematics, philosophy, or art. The difference in question is defined by the representational and nonrepresentational relationships between these configurations and *the ultimate workings of nature*, placed beyond the limits of thought by nonclassical thinking. The underlined qualification is crucial, since otherwise both types of relationships are found, albeit in turn differently balanced, in classical and nonclassical thinking alike. From this perspective, quantum mechanics in a nonclassical interpretation deals with those physically and epistemologically classical phenomena (defined by the role of Planck's constant, h) and those configurations of such phenomena that cannot be accounted for by means of classical physics. In particular, classical physics cannot predict either each such phenomenon or these configurations and, specifically, certain statistically correlated patterns they exhibit in some circumstances. One needs quantum mechanics to be able to do so. In a nonclassical interpretation, however, quantum mechanics does not describe quantum objects and processes themselves, which are responsible for the existence of these phenomena and these configurations.

The (nonclassical) view of the quantum situation is admitted to be *logically possible* by most of its opponents, beginning with Einstein (e.g., Einstein 1936), although the full measure of the epistemology inherent in this view and its radical implications are rarely considered or even perceived. It is, for example, not clear to what degree Einstein ultimately perceived them either, which may not matter, since, as will be seen, he rejected an even less radical position entertained by Bohr at earlier stages of his thinking. According to this earlier view, one could attribute certain properties to quantum objects themselves, but only at the time of measurement (and hence not independently) and under the constraints of the uncertainty relations. Most physicists and philosophers followed Einstein, and even this view, let alone a fully nonclassical view, is, while, again, admitted to be logical is rarely viewed as satisfactory, and the search for (from a classical-like perspective, such as that of Einstein) more complete, specifically more *descriptive*, alternatives to either this interpretation or to quantum mechanics itself continues.

This search does not appear likely to be abandoned anytime soon, given the appeal of classical thinking and indeed its fundamental role in our thinking as such. For, as was often stressed by both Bohr and Heisenberg, our thinking appears to be, *as thinking*, classical in nature even in the case of nonclassical theoretical "thinking," that is, argumentation (and in this study I use the

[5] I am indebted to Giacomo Mauro d'Ariano on this point.

expression "nonclassical thinking" only in this sense), since it is still reached by means of classical thinking, or perception to begin with. It may be argued that classical thinking and indeed in particular "the brain's sense of movement" reflect the essential workings of our neurological machinery born with our evolutionary emergence as human animals and, in part, enabling our survival.[6] Given the neurological–evolutionary origins of classical theoretical thinking, it is hardly surprising that it was so effective and so pervasive in mathematics, science, and philosophy, or in our Western culture overall. In other words, our *thinking in general*, as the product of this machinery, is classical, even in nonclassical situations, since the entities, such as quantum objects, that make a theory considering them nonclassical are, by definition, beyond the capacity of our thought. They are, again, literally unthinkable, and, if a given nonclassical situation becomes eventually reconfigured so that the (previously) unthinkable becomes available to thought or knowledge, it would no longer qualify as nonclassical, but becomes classical instead. As explained above, we are compelled to infer the existence of such entities from certain configurations of their effects upon what we can think and know, in part inevitably through classical theoretical thinking. This inference is, thus, always *theoretical*.

Accordingly, by advocating nonclassical ways of theoretical thinking, I am not suggesting that classical thinking should be abandoned, which, for the reasons just explained, would be impossible in any event. Classical thinking retains its positive significance in nonclassical theorizing, which takes thought to what it cannot think, and in the case of quantum theory, discovers or, one might say, *constructs* nature as *un*-constructible—unavoidably.[7] It is not a matter of an ideological, aesthetic or other preference (although such preferences may not be avoidable), or of a simple choice between classical and nonclassical theories in physics, philosophy, or elsewhere, but of the necessity of using either one or the other, or different combinations of both, in different circumstances. The classically based attitudes, such as that of Einstein, often aim to avoid or exclude nonclassical approaches and attitudes. By contrast, nonclassically based attitudes, at least the one adopted here, not only deploy, as they, again, must, but also embrace classical physical and philosophical thinking, both within their own limits and in nonclassical domains.

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[6] See Alain Berthoz's *The Brain's Sense of Movement* (Berthoz 2000), (Berthoz 2003), and (Llinás 2002).

[7] In this respect, quantum mechanics in a nonclassical interpretation strongly supports the argument that it is difficult and perhaps impossible to assume that nature is ever fully captured by any given "construction," phenomenal, conceptual (the concept of nature included), or cultural. This point is often missed by social-constructivist philosophers, historians, and sociologists of science, although a number of works along these lines published during the last decade are more attentive to this aspect of our construction of nature and offer a more critical analysis of social constructivism itself. The "construction" or (this is a form of construction, too) "unconstructibility" of nature by science or culture is a major subject in its own right, which has been debated throughout the history of science, philosophy, and other disciplines. While this subject cannot be properly considered in this study, it cannot be altogether bypassed by it either, and I shall return to it on several occasions.

1.2 Nonclassical Epistemology and Quantum Probability

While the epistemological argument of this study, as just outlined, is more radical than most treatments of Bohr or most interpretations of quantum phenomena and quantum mechanics, it may nevertheless be seen as extending the primary concerns of the discussions and debates concerning quantum theory. By contrast, the study departs from most approaches to quantum theory and the work of Bohr, Heisenberg, and Schrödinger in its focus on *probability*, and specifically on the Bayesian view of probability, recently adopted in the Bayesian quantum informational approach to quantum foundations (e.g., Fuchs 2003; Caves et al. 2007; and further references there).[8] It should be stressed that at stake here is *probability* rather than only quantum *randomness*, although the latter is crucial. First, I would like to establish more firmly the key concepts involved in my analysis of causality and probability, as these concepts are to be used here (they can be defined otherwise).

I understand "causality" as an *ontological* category relating to the behavior of the physical systems whose evolution is defined by the fact that the state of a given system is, at least, again, at the level of idealized models, exactly determined at all points by their state at a particular point, indeed at any

[8] In Caves et al. (2007), the authors dissociate themselves from Bohr, although they acknowledge that this dissociation concerns only a particular interpretation of quantum mechanics associated with Bohr, which they see as prevalent, and that other views of Bohr are possible. However widespread this view of Bohr might be (I am not contesting the authors on this point), it is, in my view, difficult to attribute to Bohr, and it is manifestly different from the interpretation of Bohr's interpretation offered in this study. Caves, Fuchs, and Schack associate the quantum-informational understanding of quantum mechanics with certain arguments considered by Einstein in the works to be discussed later in this study (e.g., Einstein 1949a). As will be seen in Chapter 10, Einstein himself sees such a view as a problem, entailing, for him, the incompleteness of quantum mechanics (insofar as it does not offer an account of individual quantum systems), and he associates this view with the spirit of Copenhagen and specifically with Bohr. By contrast, as I argue here, Bohr's understanding of quantum mechanics as a probabilistic theory of even individual quantum phenomena and events is close to that of Caves, Fuchs, and Schack, although Bohr's interpretation may be different from theirs in other respects. In particular, Bohr's interpretation (or the present interpretation) centers primarily on certain particular, *physical configurations* of quantum phenomena as *physical* phenomena, such as those observed in the double-slit and other quantum experiments, and on the irreducible difference in the *architecture* of these configurations from those considered in classical physics. Complementarity in the narrow sense reflects this architecture and this difference, which may of course also be seen in terms of the architecture (organization) of information. The Bayesian program of quantum information theory is, in my view, consistent with and sheds new light on this understanding, since the fundamental constituents of this architecture are *individual* quantum phenomena and events, and our predictions concerning them are irreducibly probabilistic. E. T. Jaynes, one of the leading advocates of the Bayesian view of probability, appears to have been the first to suggest a link between the Bayesian approach and Bohr's argument concerning the EPR experiment (Jaynes 1990, p. 387).

given point.[9] I understand "determinism" as an *epistemological* category having to do with our ability to predict the state of a system, at least, again, as defined by an idealized model, *exactly* (rather than probabilistically) at any and all points once we *know* its state at a given point. Again, usually the knowledge of the state at any point suffices. It should be noted that determinism is sometimes used in the same sense as causality is used here.

While it follows automatically that noncausal behavior, again, *considered at the level of a given model*, cannot be handled deterministically, the reverse is self-evidently not true. I qualify this statement because we can obviously have causal models of processes in nature that may not be ultimately causal, which may be the case in physics at the ultimate level, in particular, if this level is quantum in character, although thus far we cannot, again, be certain on that last score either. In other words, while the causal models of classical physics apply and are effective within the proper limits of classical physics, this does not mean that the ultimate character of the actual processes that are responsible for classical phenomena is causal. For example, as will be explained later, an electron sufficiently far away from the nucleus of an atom can sometimes be treated as a classical object moving along an orbit, which, however, does not mean that this is how it actually behaves, since the situation is more consistently treated as quantum and, hence, likely noncausal. Classical physics decouples the possibly noncausal origin of the causal behavior it idealizes. It is, again, true that noncausal models of the quantum constitution of nature are idealizations in turn, which may be necessary (not everyone agrees) given the nature of quantum phenomena. Rigorously, it is only determinism that these phenomena prevent, by virtue of the fact that the identically prepared experiments in general lead to different outcomes. In sum, the causal or, conversely, noncausal character of our models does not guarantee that the actual behavior thus modeled is respectively causal or noncausal, and in this sense causality may be seen as an epistemological concept as well. It is, however, useful to retain the concepts of causality and determinism, and the difference between them, at the level of models, first of all, because, while our deterministic predictions can apply in classical physics with very good approximations to actual objects, such as, again, planets moving around the sun, these predictions are made by means of descriptive causal models. Secondly, and most crucially for this study, while such causal models are not only possible but also effective in classical mechanics, the situation is different in quantum theory. At the very least, an

[9] A qualification may be in order here. It is usually assumed that a cause always precedes its effect, or at the limit is simultaneous with it. Relativity further restricts causes to those occurring in the backward (past) light cone of the event that is seen as an effect of this cause, while no event can be a cause of any event outside the forward (future) light cone of that event. These restrictions follow from the assumption that causal influences cannot travel faster than the speed of light in a vacuum, c, that is, with an assumption of locality. In any event, when speaking of the lack of causality in quantum mechanics, I only mean the inapplicability of the concept of causality found in classical physics and not any incompatibility with relativity.

introduction of such causal or, again and to begin with, realist models does not appear possible in quantum physics without major difficulties or highly undesirable consequences.

Thus, classical mechanics (excluding the chaos-theoretical part of it) deals deterministically with causal systems, at least, again, at the level of idealized models, since there are practical limitations upon the measurements involved. As Bohr noted: "Newtonian mechanics . . . evidently represents an ideal form of causal relationships, expressed by the notion of *determinism*," which, however, as he added, has a wider scope, for example, in classical electromagnetism (Bohr 1958, *PWNB* 3, p. 1). Classical statistical physics deals with causal systems, but only statistically, in terms of probabilities, rather than deterministically. Chaos theory deals with systems that are, in principle, causal, but whose behavior cannot be predicted even in statistical terms in view of their nonlinearity and, hence, their sensitivity to initial conditions. These and other classical physical theories (those of electromagnetic radiation and other continuous phenomena included) are *causal* insofar as they deal, deterministically or not, with (idealized) systems that are assumed to behave causally. As explained above, these theories, at least in most versions or interpretations, are also, and in part correlatively, realist on the definition given above, and, indeed, as Ludwig Wittgenstein noted, conversely, any actual realist description may have to be causal (Wittgenstein 1924, p. 175). Both classical mechanics (again, exclusive of its chaos-theoretical part) and chaos theory are realist insofar as such a mapping is assumed to take place, at least as an idealization or as a good approximation that such models provide, although chaos theory is not deterministic. By contrast, classical statistical physics is not realist in the same sense insofar as its equations do not describe the behavior of its ultimate objects, such as molecules of a gas. It is, however, generally based on the realist assumption of an underlying nonstatistical multiplicity, whose individual members in principle conform to the causal laws of classical, Newtonian mechanics of individual physical systems. Although this assumption does involve certain conceptual difficulties, it is generally maintained in classical statistical physics. Initially (in the old quantum theory) it appeared possible to adopt this assumption in quantum theory as well, even if with certain qualifications, to be discussed later in this study. Attempts to reintroduce causality in quantum theory have never died and they still continue, although more often by means of developing alternative theories of quantum phenomena, either altogether replacing or supplementing the standard quantum mechanics.

By contrast, in the nonclassical view adopted here following Bohr, quantum mechanics is neither, more manifestly, a deterministic nor, more crucially, a causal or, in the first place, realist theory. It is not causal, first, insofar as a given quantum event or phenomenon, manifest, ontologically and phenomenally, in our measuring instruments, cannot be determinately connected to other such events. At least, such a connection can never be guaranteed at the level of individual events. For it is a well-confirmed experimental fact that the identically prepared quantum experiments, as concerns the manifest classical state of our measuring instruments, in general lead to, in Bohr's fitting language, "different recordings" of their outcomes (Bohr 1954, *PWNB* 2, p. 73). This identical preparation is always possible, as possible as it is in classical physics; indeed it is possible because the manifest physical state of measuring instruments can be treated classically. In this sense, as well as phenomenally, all such events are discrete (discontinuous relative to each other) and, hence, are also irreducibly individual. Thus, if we consider the traces of photons emitted from a given source on the silver bromide photographic plate (as in the double-slit experiment, to be discussed in Chapter 2), we can only ascertain a correlation of such events (emission and traces) statistically. Indeed, we cannot be certain whether a given trace corresponds to an emission. Accordingly, a theory properly accounting for these phenomena, cannot, by definition, be deterministic, at least thus far, although it may in principle be causal, at least as concerns the independent behavior of quantum objects, as in the case of Bohmian theories or certain interpretations of quantum mechanics to be discussed in Chapter 6. It should be qualified that exact predictions concerning certain quantities involved are possible under certain idealized circumstances, such as separately that of the position or the momentum associated with a quantum objects, but never both together. This possibility grounds, for example and in particular, the EPR thought-experiment (in its original form it cannot be performed in the laboratory), which led EPR and Einstein to think that quantum mechanics is incomplete. Even in an idealized case like this, such exact predictions would not make it possible to follow the course of quantum processes in the way it is possible in classical mechanics for classical processes. The impossibility of the exact measurement or knowledge of both the position and the momentum of a given object simultaneously, reflected in the uncertainty relations, is correlative to this situation, since knowing only one only allows us to estimate, by means of quantum mechanics, the probabilities with which certain events might occur. This probability may in certain idealized cases be equal to unity, but in general is not, and were we to repeat the experiment in which such exact predictions can be obtained in the same setting, the outcome would be different.[10] It is the knowledge of both

[10] As will be explained in Chapter 8, a given EPR prediction which is exact, involves a pair of quantum objects and is accomplished by means of a measurement performed on one object of the pair and concerns the other object of this pair. If we want to repeat the experiment on another pair of objects, we can set up the initial stage of the experiment identically. However, the outcome of the corresponding EPR measurement and, hence, of the EPR prediction in question will not be the same as in the first experiment, even though in both cases these predictions will be exact. The situation may be a bit more delicate in the case of discrete variables, such as spin, where predictions of the EPR type are possible as well. For an illuminating and comprehensive discussion, see Mermin (1990, pp. 110–176). However, the present argument still applies, both in the case of the EPR predictions and in general. Indeed, spin, the famously inconceivable "angular momentum" (a metaphor borrowed from classical physics but ultimately inadequate), is a good case of the difficulties of applying classical thinking in quantum theory. Spin has no classical counterpart, and we can only access it through its effects upon measuring devices and speak of it only by way of metaphorical analogies, which inevitably break down at a certain point. It manifests itself only through a consistent set of numbers obtained in measurements.

at any point in classical mechanics that guarantees both the causality and the determinism of the latter theory, again, at least, at the level of idealized models.

Now, as I said, one could argue (and many, beginning, again, with Einstein, have done so) that these circumstances in themselves do not necessarily exclude the possibility that the underlying (quantum) physical processes that connect such quantum phenomena (this connection is of course essential) are ultimately causal. As just explained, given how such concepts as causality (or reality, to begin with) and determinism are defined, in particular, via idealized physical models, conceptually nothing prevents this possibility in principle. If such were the case, insofar as it correctly predicts the outcome at the level of phenomena and measurement, quantum mechanics would be a merely correct but not complete theory of this ultimately causal underlying dynamics, which is what Einstein believed, under the assumption of locality. As I argue here, however, following Heisenberg and Bohr (who responded to Einstein accordingly), such does not appear to be the case, and quantum mechanics can still be seen as complete without giving up locality. It does not appear possible to causally (or continuously) connect the "dots" observed (although in the literal sense of the term) in quantum experiments. Accordingly, quantum mechanics may be seen as "nonlocal" in the following, very different sense, which corresponds to the discreteness or/as individuality of quantum phenomena. Unlike classical physics or relativity, quantum mechanics does not make its predictions by means of algorithms based, at least in principle, on following the infinitesimal continuous changes in the state of the system in question, which changes are described by the equations used, again, at least in principle and in an idealized way. This kind of local tracking, or again, any physical description of quantum behavior, appears to be impossible in quantum theory, at least in the standard version considered here. Instead, quantum mechanics makes predictions concerning certain future events spatially separated from the events on which these predictions are based. While it does not appear possible to explain how these predictions come about, in other words, to offer a physical description of quantum objects and processes, the situation need not involve nonlocality in the sense of the instantaneous physical connections, forbidden by relativity, whether these connections are expressly manifest or not. In other words, as I shall discuss in Chapter 8, one might speak of spooky predictions at a distance, but without any "spooky actions at a distance," which troubled Einstein. But then, Einstein would not and in effect did not accept the idea of such spooky predictions at a distance either, since his vision of physics was defined by both forms of locality in question. Indeed, it might appear that the two require each other. However, as quantum mechanics tells us, such is not necessarily the case.[11]

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[11] The situation is, thus, the opposite of that found in Bohmian theories, where a kind of tracking is in principle possible, even though we displace the actual state of the system in the process. Bohmian theories are thus *local* in this sense, but are nonlocal in the sense of allowing an instantaneous physical action at a distance, even though the latter may not be actually manifested in observations.

Furthermore, for the reasons to be explained later in this study, it does not appear possible and, in a nonclassical view, is impossible even to apply the concept of state as conceived in classical physics to quantum objects and processes, a circumstance that is correlative to Heisenberg's uncertainty relations *in the corresponding interpretation*. Technically, as expressed by the famous formula, $\Delta q \Delta p \cong h$ (where Δ is the root-mean-square deviation of the value of a given variable from its mean value and h is Planck's constant), the uncertainty relations only prohibit, in principle, the simultaneous *exact* measurement of both the position and the momentum associated with a given quantum object. More accurately, they pertain to the coordinate and the momentum in the corresponding direction; and in the uncertainty relations for the position and the momentum associated with a quantum object in the three-dimensional space, each quantity will have three components, defined by the chosen coordinate system. Importantly, the uncertainty relations apply regardless of the capacity of our measuring instruments; in other words, unlike in classical physics, the uncertainty relations would be valid even if we had ideal instruments. In other words, *in practice* in classical and quantum physics alike, one can only measure or predict each variable with arbitrary precision *within* the capacity of our measuring instruments, and in classical physics one can, in principle, so measure both variables simultaneously. The uncertainty relations, however, would prohibit us from doing so for *both* variables regardless of this capacity, and this fact is well confirmed experimentally, thus making the uncertainty relations a law of nature, reflecting the uncircumventable features of quantum phenomena. Thus, the uncertainty relations make each type of measurement mutually exclusive to the other. More rigorously, they are complementary in Bohr's sense. This means that such measurements can never be performed simultaneously, and yet, it is always possible to perform either type of measurement separately at any given point, and it is necessary to use both types of measurements at different moments for a comprehensive account of the totality of phenomena, quantum phenomena, that quantum mechanics must consider. Indeed, in Bohr's interpretation of the uncertainty relations one not only cannot measure both variables simultaneously but also cannot even define both simultaneously for the same quantum object. The joint simultaneous measurement and definition of both variables is, however, always possible, at least in principle in the case of idealized models, in classical physics, and it is this possibility that allows one to maintain causality there. Thus, it does not appear possible to ascertain a well-defined (in spatio-temporal terms) quantum-level event or sequence of events that result as outcomes of quantum experiments, such as traces on the screen in the double-slit experiment. That is to say, such events or phenomena cannot be rigorously connected, even statistically (which is possible only in correlating classical-level observable phenomena), to any quantum-level sequence of events that preceded it, or that will follow it. It does not appear possible to know or even to conceive of what actually happens between quantum experiments. In Heisenberg's words: "There is no description of what happens to the system between the initial observation and the next measurement" (Heisenberg 1962, p. 47). Accordingly, it is difficult to make a theory accounting for this situation causal or, to begin with, realist.[12]

By "randomness" or "chance" I refer to a manifestation of the unpredictable. A random or chance event is an unpredictable event. It may or may not be possible to estimate whether such an event would occur or often to anticipate it as an event. Physically, such an event may or may not manifest some ultimate underlying causal dynamics unavailable to us. The first case (that of the underlying causal dynamics) defines what I shall call, in accordance with the epistemological argument of this study, *classical* randomness and chance. In classical statistical physics, randomness and probability might be, and commonly are, seen as resulting from insufficient information concerning systems that are at bottom causal but whose mechanical complexity (usually defined by the large numbers of their individual constituents) prevents us from accessing their causal behavior and making deterministic predictions concerning this behavior. As just explained, the situation is more complex in quantum mechanics, given the difficulties of sustaining arguments for the causality of the independent behavior of quantum systems. The interpretation adopted here suspends the possibility of causality in considering either observed quantum phenomena or the independent behavior of quantum objects, which is unobservable as such and, hence, is indescribable and possibly inconceivable by any means available to us. This suspension corresponds to what I shall call *nonclassical* randomness and chance. Expressions such as "fundamentally or irreducibly random" and "probabilistic" refer to nonclassical chance and probability as well, that is, chance and probability that are irreducible not only in practice (which may, again, be the case in classical physics and is always the case in classical statistical physics), but also in principle.

Probability and statistics deal, theoretically or practically, with providing numerical estimates of occurrences of certain individual or collective events, in accordance with mathematical probability theories, which theorize such estimations. The *irreducible* randomness and probability, unavoidable in principle rather than only in practice, in question in quantum theory refer not to the *mathematics* of probability involved, but to the difference in the *physics* and, correlatively, *epistemology* found in quantum mechanics in the present interpretation, as against classical physics or certain alternative views of quantum mechanics. Although related and often used interchangeably, the terms probabilistic and statistical are generally somewhat different. Probabilistic refers to

[12] These considerations, again, do not mean that causal interpretations of quantum mechanics or alternative causal accounts of quantum mechanics are, in principle, impossible. Bohmian mechanics is causal or realist or ontological, to begin with, although it is not deterministic, since its predictions coincide with those of the standard quantum mechanics. It achieves its classical epistemology only at the cost of nonlocality, or to put it strongly, at the cost of being incorrect as a physical theory, assuming that relativity is true. Even in Bohmian theories, however, no undistorted *description* of quantum behavior is possible and the uncertainty relations still hold. One also obtains the same statistical predictions as in the standard quantum mechanics. As noted above (Note 2), there are also arguments for causal and realist (local) interpretations of the standard quantum mechanics.

our estimates of the probabilities of either certain individual or certain collective events, such as of a coin toss or of finding a quantum object, such as an electron or a photon, in a given region of space. Statistical refers to estimating the average behavior of, in a given context, identical systems, say, in the present context, certain quantum objects, such as electrons or photons, and identically prepared measuring instruments in certain experiments. The standard use of the term "quantum statistics" refers to the behavior of large multiplicities of identical quantum objects (e.g., quantum gas), such as electrons and photons, which famously behave differently in such multiplicities. Both conceptions are important in quantum mechanics, and both terms will be used in this study, sometimes jointly.

My primary concern will be with probability as relating, on Bayesian lines *in a broad sense*, to our estimates concerning the outcomes of *individual* or even unique events on the basis of the information we have, vis-à-vis the so-called frequentist or ensemble approach, which relates to *ensembles* of repeated events, such as a series of coin tosses. I speak of Bayesian lines of approach to quantum probability "in a broad sense" for the following reasons. I am not significantly concerned here with the different definitions or, more generally, the corresponding philosophy of probability. Bayes's theorem itself, which relates the conditional and marginal probabilities of two random events, according to a definite very simple formula, and is the origin of Bayesian interpretations (there are several), is general and valid in both types of approach.[13] Bayesians, however, understand probabilities themselves in terms of expectations or beliefs and degrees of uncertainty, while frequentists (in the broad sense) understand probabilities by assigning them to random events according to their frequencies of occurrence or to subsets of samples as proportions of the whole. Predictably, this difference led to a great debate concerning the nature and interpretation of probability, which cannot and need not be considered here (e.g. Kendall 1949; Jaynes 2003). The primary motivation behind my appeal to the Bayesian view of probability is the following crucial circumstance, which I have stressed earlier and to which I shall return throughout this study. On experimental grounds, our predictions are unavoidably probabilistic because identically prepared experiments in general lead to different outcomes even in the case of individual quantum events, as against the situation that obtains, at least in principle, in classical mechanics, where the possible probabilistic considerations involved are practical (e.g., measurement errors and deviations). In quantum theory, even an ideal experiment, were it possible, could

[13] Bayes's formula or theorem relates the conditional and marginal probabilities of events A and B, where B has a non-zero probability of occurrence:

$$P(\mathrm{A}|\mathrm{B}) = \frac{P(\mathrm{B}|\mathrm{A})P(\mathrm{A})}{P(\mathrm{B})}.$$

Here $P(\mathrm{A})$ is the prior probability or marginal probability of A (also known as the "prior"), which does not take into account any information concerning B; $P(\mathrm{A}|\mathrm{B})$ is the conditional probability of A, given B, called the posterior probability, which depends on the value of B; $P(\mathrm{B}|\mathrm{A})$ is the conditional probability of B given A; and $P(\mathrm{B})$ is the prior probability of B. The theorem may be seen as describing how one's beliefs about observing A are updated by having observed B.

not be repeated identically, that is, unlike in classical physics, this type of idealization is not possible. Quantum mechanics properly responds to this situation.

Nonclassical epistemology in turn bears on the use of the Bayesian probability in physics, since epistemologically classical situations, in physics or elsewhere, that are essentially probabilistic could, in principle, be theorized in Bayesian terms as well, and most of Bayesian accounts outside quantum theory deal with such situations. In quantum theory, too, one can combine, philosophically and practically, classical epistemology and the Bayesian view of probability.[14] On the other hand, epistemologically nonclassical interpretations of quantum mechanics appear to invite, if not require, a Bayesian view, since, in this type of interpretation, the theory only deals with probabilistic expectations concerning individual quantum events, suspending any causal underlying dynamics that would ultimately be responsible for these outcomes. It is possible to combine nonclassical epistemology and a frequentist interpretation of quantum mechanics. As will be seen in Chapter 6, however, frequentist approaches, such as that of von Neumann, tend to allow for a causal view of independent quantum processes, while the statistics are brought in due to the disturbance introduced by observation. By contrast, combining nonclassical epistemology and Bayesian probability suspends any causality in quantum physics. Either way, however, in the case of quantum phenomena cum quantum mechanics, one deals with a peculiar and even unique combination of randomness and probability.

On the one hand, given that, as just explained, identically prepared quantum experiments lead to different recordings of their outcomes, there is randomness inherent in each primitive individual quantum event. By "primitive" I mean that such an event cannot be decomposed into simpler sub-events, in contrast to physically classical random events, such as a coin toss. The latter is, physically, a sequence of many events and is, in principle, a subject of a causal classical description. It is only the mechanical complexity of the underlying situation defining a given event that prevents us from predicting the outcome of the event exactly. According to Bohr: "The unrestricted applicability of the causal mode of description to physical phenomena has hardly been seriously questioned until Planck's discovery of the quantum of action, which disclosed a novel feature of atomicity in the laws of nature supplementing in such unsuspected manner the old doctrine of the limited divisibility of matter" (Bohr 1938, *PWNB* 4, p. 94). For instance, in classical statistical physics the underlying individual processes involved are still seen as causal, which indeed hardly amounts to a serious questioning of the causal mode of physical description, as

[14] E. T. Jaynes, who, as I said, appears to have been the first to suggest a link between the Bayesian approach and Bohr's views, takes a classical, *ontological* and realist, view of complementarity and of quantum theory itself (Jaynes 1990, p. 387). On the other hand, he offers a more critical assessment of Bohr and the spirit of Copenhagen, which he sees as pervading quantum physics, in his magnum opus *Probability Theory: The Logic of Science* (Jaynes 2003, pp. 328–329). Either way, Jaynes's philosophical position is closer to Einstein's realist desiderata, as against recent Bayesian arguments in quantum information theory cited above.

against what one encounters in quantum physics. The old doctrine here referred to by Bohr, which extends from Democritus to the pre-quantum-theoretical concept of atoms developed from the late eighteenth century on, applies to the ultimate constituents ("atoms") of matter itself. In the case of quantum phenomena, the *randomness* in question, again, defines even those events that cannot be decomposed into any sequence of sub-events, which are "atomic" in the original, ancient Greek, sense of being indivisible any further. This etymology was used by Bohr in his definition of individual quantum or "atomic" phenomena, which he links to the irreducible quantum randomness. On this definition, Bohr's "novel feature of atomicity" refers to the particular configurations of effects manifest in measuring instruments, as impacted by their interactions with quantum objects, rather than to these objects themselves, conceived as inconceivable along the lines of nonclassical epistemology.

On the other hand, in a Bayesian way of putting it, our expectations concerning such outcomes, specifically in individual cases, can be helped and even defined by certain rigorously determined and unambiguously communicable, and in this sense, "objective," *probability* rules, even though, as actual expectations, such expectations are subjective, mental, and as such are singular. In quantum theory and elsewhere, probability is as much about order, about patterns, as it is about randomness; in other words, it is about the interplay of randomness and patterns. In quantum physics, moreover, under certain circumstances, such as those found in such key quantum experiments as the double-slit experiment or the experiments of the EPR type, we encounter correlational and hence partially ordered patterns. These correlational patterns are fundamentally quantum: they are not found in classical physics. The probability rules predicting these patterns are encoded in the mathematical formalism of quantum mechanics, even though this encoding is in turn peculiar and appears to invite an epistemologically nonclassical understanding. This is one of the reasons for Einstein's puzzlement, cited above, concerning the possibility that the Lord, if he plays dice, "should gamble according to definite rules [of quantum mechanics], that is beyond me."

Democritus saw nature as the interplay of chance and necessity. Quantum phenomena retain chance, but replace "necessity" with correlational order, found, again, only in some circumstances, which is peculiar and even mysterious, or nonclassically inaccessible. The situation compels us to speak of the atomic constitution of nature, or at least of our interactions with nature at the ultimate level of its constitution, as an interplay of *chance and probability*.

1.3 Physics, Mathematics, and Philosophy in Quantum Theory

More recent developments, such as those arising from the EPR experiment and Bell's and related theorems, which largely define the current state of the debate concerning the foundations of quantum theory have brought significant new insights into our understanding of the subject. I would argue, however, that the thought of the founding figures of quantum theory has by no means lost its substantive, let alone historical, significance. It still reveals some of the deepest aspects of quantum phenomena and quantum theory and often does so at least as well as the best new work, in part because of the deep epistemological and philosophical problems that appear unavoidable in quantum theory and that are far from being resolved. Indeed, these problems appear unlikely to have a final resolution acceptable to all parties concerned, especially given the apparently irreconcilable philosophical principles behind many contrasting positions, as exemplified by the Bohr–Einstein confrontation. We appear to be confronting conceptual and epistemological abysses that may have no bottom. Given this situation, we may never be able to leave behind the thought of the founding figures of quantum theory or even of the founding figures of classical physics, such as Galileo and Newton, any more than in philosophy itself we can leave behind the thought of Descartes and Kant or Plato and Aristotle. Thus, as will be seen, Plato and Aristotle may help us to gain a deeper understanding of Heisenberg's thinking that led him to his discovery of quantum mechanics. The longer history just invoked affects not only quantum mechanics but also the most recent developments of physics, from quantum field theory and the standard model to string and brane theories and beyond. As I argue here, however, this history, at least its modern part, from Galileo on, compels us to examine the relationships not only between physics and philosophy, but also among physics, philosophy, and mathematics in quantum theory as, in Galileo's words, a mathematical science of nature.

The relationships among physics, mathematics, and philosophy were crucial to the history of quantum theory from its inception in Planck's work to the old quantum theory in the work, developed by, in addition to Planck, Einstein, Bohr, Sommerfeld, and others, to the rise of quantum mechanics, which, I argue, cannot be rigorously understood apart from these relationships.[15] I further argue, contrary to the prevailing view, that Bohr's thinking concerning quantum mechanics and complementarity cannot be adequately understood apart from these triple relationships either. This is why this study gives major attention to the significance of the mathematics of quantum theory and of mathematics *for* quantum theory, and in particular to the new relationships between mathematics and physics that emerged there, beginning, again, with Heisenberg's first paper on quantum mechanics.

Heisenberg's approach to quantum mechanics radically redefined these relationships from the outset, especially as concerns the relationships between physics and mathematics, as against their functioning in classical physics or relativity,

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[15] While most treatments of quantum mechanics in the traditional philosophy of physics tend to contain a great (and sometimes excessive) amount of technical mathematics, the relationships among physics, mathematics, and philosophy are rarely *philosophically* considered there. The reasons for this omission are complex and require a discussion beyond the scope of this study. More generally, although they are sometimes historically and conceptually traced (e.g., Omnés 1999), the triple relationships among physics, mathematics, and philosophy in quantum mechanics or in physics are rarely expressly addressed in more recent discussions of quantum foundations. Notable exceptions are John Archibald Wheeler (e.g., Wheeler 1994) and Abner Shimony (e.g., Shimony 1983).

where these relationships were of course decisive as well, or the old quantum theory, which was in this respect close to classical physics. The old quantum theory was conceived as a realist theory, at least, again, at the level of idealized models, the ideal that Schrödinger also wanted, but ultimately failed, to retain in his wave mechanics, via a form of wave ontology. While, thus, quantum theory follows the program of modern physics as a mathematical science of nature, it establishes a radically different way of enacting this program. Heisenberg's theory dispensed with the specifiable quantum-level ontology, even if it not rigorously prohibited it, as Bohr's interpretation eventually did when it reached the level of nonclassical epistemology (this took a while).

Intensely preoccupied with finding a new mathematical scheme suitable for quantum phenomena, which pursuit culminated in his great discovery, Heisenberg might not have been especially focused on the role of the triple relationships among physics, mathematics, and philosophy in his theory. As will be seen in Chapter 3, however, he was far from being unaware of these relationships or their significance and implications. His previous interactions with Bohr and Pauli point in this direction as well.[16] Heisenberg realized that he found a radically new way in which mathematics and physics were brought together via his "new kinematics," both a different physical conception of the variables involved and a different mathematical representation of these variables. These variables no longer related to physical entities, such as coordinates and velocities, in the way kinematical elements do in classical physics, but only to the probabilities of occurrences of certain quantum events. In other words, as explained above, unlike classical mechanics, quantum mechanics no longer offers an (idealized) description of the behavior of quantum objects and predictions, generally determinate, based on this description. It only gives us knowledge or information, generally probabilistic in character and manifest in measuring instruments, concerning what can happen in certain future specified experiments on the basis of other previously performed experiments. Unlike classical mechanics, the theory does not tell us what "happens" between experiments, assuming, again, that even the term "happens" can apply. Bohr quickly grasped the epistemological significance and implications of Heisenberg's scheme, for which he was not unprepared, since, as will be seen, he realized that "the ordinary space-time description" of quantum processes may have to be abandoned (Letter to Heisenberg, 18 April 1925, Bohr 1972–1996, vol. 5, pp. 79–80). While, roughly after his discovery of the

[16] Pauli encouraged Heisenberg to develop stronger philosophical groundings for his physical thinking and hoped that his spending some time in Bohr's institute in Copenhagen would accomplish this. On Pauli's view of Heisenberg's work prior to quantum mechanics and on his own philosophical interest at the time, see Jagdish Mehra and Helmut Rechenberg's discussion in *The Historical Development of Quantum Theory* (Mehra and Rechenberg 1991–2001, vol. 2, pp. 132–135). Mehra and Rechenberg's treatise, hereafter cited as *MR*, contains an enormous wealth of factual material and remains an essential reference on the history of quantum mechanics. Although the present study offers a significantly different and often opposing view of quantum mechanics and its interpretation, and of the work of the key figures involved, most especially Bohr and Heisenberg, it is indebted to Mehra and Rechenberg's work.

uncertainty relations in 1927, Heisenberg's physical interests moved elsewhere, in particular to quantum field theory, Bohr's preoccupation with the epistemology of quantum mechanics continued throughout his life and, as he said, was his life (Bohr 1962). Bohr, however, was far from having neglected the mathematical aspects of Heisenberg's discovery, as his immediate response to Heisenberg's paper and Born and Jordan's work developing it shows by expressing the hope that "a new era of mutual stimulation of mechanics and mathematics has commenced" (Bohr 1925b, *PWNB*1, p. 51).

It may be unusual to think of Bohr's work in terms of the conjunction of mathematics, physics, and philosophy, rather than only in terms of the conjunction of physics and philosophy. Commentators on Bohr rarely give due attention to the importance of the role of mathematics in quantum theory for Bohr's thinking. If anything, this importance is often denied altogether, and Bohr's work is juxtaposed and sometimes unfavorably compared to that of Heisenberg along these lines. I believe these commentaries to be in error, and such views are, I would contend, often based on superficial or prejudiced readings of his works. Against this common misconception, I shall argue that the role of mathematics in quantum mechanics and the relationships between mathematics and physics in general are essential for Bohr's thinking. Accordingly while it is true that Bohr's work on quantum mechanics was primarily defined by his concerns with the relationships between physics and philosophy in quantum theory, his appreciation of the role of mathematics in quantum physics makes him convert these relationships into those among physics, mathematics, and philosophy. It would be difficult for him not to do so, even leaving aside that his younger brother Harald A. Bohr, to whom Bohr talked daily, was a major mathematician in the field of functional analysis, which is closely related to the mathematics of quantum mechanics.[17] Bohr not only (self-

[17] Early in his career Harald Bohr spent some time in Göttingen, the center of mathematical physics in the early twentieth century (roughly through the 1930s). Richard Courant and David Hilbert's *Methods of Mathematical Physics* (Courant and Hilbert 1991), instrumental to, among many other developments, quantum theory, was a product of the mathematical spirit of Göttingen. With Heisenberg's discovery, the matrix form of quantum mechanics was born and developed there by Heisenberg himself, Born, and Jordan, all in Göttingen at the time, and Pauli, a frequent visitor. Earlier the place was also central to the development of both the mathematics and physics of relativity in the work of Hilbert, who was a co-discoverer, with Einstein, of the equations of general relativity (although the case is subject to some controversy), Hermann Minkowski, Hermann Weyl, and Amy Noether. These figures made major mathematical and physical contributions to quantum theory as well, including through functional analysis, Harald Bohr's main field. His work was highly respected in Göttingen, and he was well known there before his older brother, who subsequently gave his famous lectures on quantum theory (the so-called "Bohr's fest") there in 1922. These lectures were attended by Heisenberg, who made his acquaintance and, famously, engaged in his first exchanges with Bohr, which started him on the road that eventually led him to his discovery of quantum mechanics. In 1934 Harald Bohr became the director of the Mathematics Institute in Copenhagen, next to the Institute for Theoretical Physics, directed by Bohr himself, and their close interaction continued until Harald Bohr's death in 1951.

evidently) understood that modern physics, from Galileo to quantum theory, cannot be considered apart from mathematics, any more than apart from experimentation, but also realized that the philosophical considerations pertinent to and indeed unavoidable in physics cannot be rigorously understood apart from the role of mathematics there, alongside that of experimentation. Bohr addressed the subject throughout his work and devoted to it a separate essay, "Mathematics and Natural Philosophy" (Bohr 1956), which I shall discuss in Chapter 4.

There are, of course, significant differences between Bohr and Heisenberg as regards the actual *role* of mathematics in their work itself. Heisenberg's work, or that of Schrödinger, is manifestly defined by a much more sustained deployment of mathematics than that of Bohr. Also Bohr sometimes questioned an overly excessive emphasis on mathematics in addressing the epistemological or even physical problems of quantum mechanics. Bohr's exchange with von Neumann following the Warsaw lecture "The Causality Problem in Atomic Physics," of 1938, is indicative of this attitude (Bohr 1938, *PWNB* 4, pp. 108–116). This is, however, quite different from questioning the significance of mathematics in quantum mechanics. While Bohr might have underappreciated the importance of von Neumann's work for the development of quantum theory (its significance for quantum epistemology is a separate issue), he certainly deeply appreciated the mathematical aspects of Heisenberg's work or that of others, such as Hendrik Kramers, his assistant prior to Heisenberg, or Born, Jordan, and Dirac, who mathematically developed quantum mechanics. It is also true that Bohr saw quantum mechanics as a theory that tells us that nature cannot be contained by the mathematics of quantum or, conceivably, any theory. The view, however, that quantum theory exceeds its mathematics and, as against classical physics, exceeds it irreducibly (as concerns the possibility of the mathematical representation of quantum objects and processes) need not imply, quite the contrary, a lesser significance of mathematics for physics in quantum theory, certainly not any lesser than in classical physics. The *nature* of the relationships between mathematics and physics in classical and quantum physics is fundamentally different because one can interpret classical physics as implying the possibility of the mathematization of the *behavior* of its objects, at least on the level of idealized models, while this kind of mathematization appears to be difficult or even impossible in quantum physics. The *significance* of the relationships between physics and mathematics is equally great in both.

Accordingly, the juxtaposition in question between Bohr and Heisenberg as concerns the role of mathematics in their work must be qualified. Indeed, it must be qualified in both directions. Heisenberg was concerned with the non-mathematical and specifically philosophical aspects of quantum mechanics far more than is commonly acknowledged, including in his early, more technical works, rather than in the more overtly philosophical commentaries of his later years. One also finds subtle relationships among physics, mathematics, and philosophy in Schrödinger's work, rarely discussed from this perspective, in my view to the detriment of our understanding of his thought. While the balance may be different, the thought of Bohr, Heisenberg, and Schrödinger, and of most figures considered here, in particular that of Einstein, is defined by the relationships among all three—physics, mathematics, and philosophy. This is hardly surprising. These relationships are essential in all physics, especially at the times of its greatest inventions, beginning with the invention of physics itself as a mathematical science of nature, or at the times of its greatest crises, such as that which gave rise to quantum mechanics, and these relationships define the thought of most major physicists, from Galileo and Newton on.

1.4 The Architecture of Quantum-Theoretical Concepts

According to Frank Wilczek, a leading contemporary quantum field theorist and a Nobel Prize laureate (in 2004), speaking in the context of the current state of quantum field theory, "the primary goal of fundamental physics is to discover profound *concepts* that illuminate our understanding of nature" (Wilczek 2005, p. 239; emphasis added). It might be preferable to see this "discovery" in terms of the *creation* of new concepts, following and extending to science Gilles Deleuze and Félix Guattari's argument concerning *philosophical concepts* in *What is Philosophy?* (Deleuze and Guattari 1993). Moreover, as I argue here, along with their mathematical and experimental components, physical concepts contain irreducibly philosophical components. More generally, following upon Deleuze and Guattari's *concept* of concept (again, applied by them to philosophical concepts), physical concepts are multi-component entities in each of their aspects, physical, mathematical, and philosophical, and in the interactions between and among them. Thus understood, a concept (be it philosophical, physical, or mathematical in its disciplinary functioning) is not an entity that would, in accordance with the conventional view of concepts, be established by a generalization from particulars or any general or abstract idea (Deleuze and Guattari 1993, pp. 11–24). This type of generalization may be involved in building a concept and be part of its functioning, but it is rarely sufficient. In general, a concept is a conglomerative phenomenon that has a complex, multi-component architecture—physical, mathematical, and philosophical.[18]

Bohr's complementarity (in the narrow sense) is such a concept. First of all, it was not waiting to be discovered as something already there; it had to be created by thought, even if one can speak (not a simple matter, however) of certain complementary quantum phenomena as something experimentally established and thus preceding complementarity. This is also true about Heisenberg's "new kinematics" of quantum mechanics, as a new form of the relationships between

[18] Deleuze and Guattari themselves tend to see the invention of concepts, or the concept of concept just described, as primarily found in philosophy. I shall leave aside here their position on the relationships between philosophical and scientific concepts, and on philosophy, mathematics, and science as different modes of thought. It is a separate subject, and it does not affect the present argument concerning the nature of physical concepts.

mathematical elements of the theory and observable and measurable quantities, since these elements had a more indirect relation to the experimental facts. At the same time, Heisenberg also created a new concept of the relationships between his theory and the experiments, and between the mathematical formalism of a physical theory and experiments, and thus between mathematics and physics. In classical mechanics these connections are based on the description of the physical processes in question. By contrast, in Heisenberg's theory these connections were, as I said, strictly predictive as concerns the outcomes of certain possible quantum experiments on the basis of certain previously performed experiments, without offering any description of the underlying (quantum) physical processes themselves. Heisenberg did not trust the concepts of classical physics *as applicable to quantum objects and processes*. One could hardly see Schrödinger's thinking in his work on his wave mechanics apart from the creation of new concepts and, by connecting these concepts, a new theory. Both were also created by him rather than "found." The same is true as concerns Einstein's creation of both special and general relativity theories, or of his many contributions to quantum theory, in particular his invention of the concept of photon, a radically new concept.

This is not to say that such newly created concepts have no genealogy in the history of physics or philosophy. "Every concept has a history" (Deleuze and Guattari 1994, p. 17). The originality of a concept, its novelty, emerges in and from this history, as Hegel has shown already. One can also speak of a kind of Darwinian view of this history: new concepts emerge and crystallize in their history, rather than existing somewhere ready-made and waiting to be discovered. One can of course still speak, as I do here, say, of Heisenberg's *discovery* of quantum mechanics. One gives, however, a new meaning to, or, one might say, introduces a new concept of discovery as creation.

The concepts that Wilczek has in mind in the statement cited above are physical concepts (such as gauge invariance, symmetry breaking, the Higgs field, or superconductivity), which fundamentally involve and, in the sense to be explained presently, even *are*, along with most key concepts of quantum theory, mathematical concepts. As stressed above, however, the architecture of these and all physical concepts also involves philosophical concepts in the sense of new phenomenal entities that help physical concepts to guide us to new ways of thought and to solve and, to begin with, pose new problems. That is, the discovery of new laws of nature or, in the case of mathematics, new laws of (mathematical) thought may not be possible except by discovering concepts whose character is not only physical or mathematical, or both, but also, at least partly and implicitly, philosophical. The philosophical part of the architecture of these concepts is significant, even though they must be linked to the rigorously established frames of reference defined by and defining experimental data, as we subject these concepts and our theories to, in Heisenberg's words, "the inexorable test of experiment" (Heisenberg 1967, p. 95).

This procedure has been at work throughout the history of modern physics, from Galileo on, and is found already in Aristotle's physics. Aristotle was well aware of and was the first to rigorously establish the differences between various sciences or fields of knowledge (*episteme*), such as, to use his nomenclature, physics, metaphysics (the study of the divine), logic, ethics, meteorology (mathematics was already established as *episteme*). He introduced the sciences, including the ones just listed, as different from each other. In the process, he defined the architecture of knowledge on which we and our institutions of knowledge (for example, the departmental structures of our universities) still rely. Modern physics may essentially depart from Aristotle in other respects, most especially, again, by being a mathematical science of nature and nature only, rather than of both nature and mind, as in Aristotle.[19] It does, however, continue the project of Aristotle's physics as concerns its deployment of philosophical concepts, beginning with such basic concepts as object and motion. The latter, however, are no longer applicable at the quantum level, and in this respect quantum mechanics enacts a truly radical break from Aristotle, well beyond the departure from Aristotle found in classical physics as a physics of objects and motion.

I speak of these concepts as philosophical rather than as those of everyday life (as some do, including Bohr and Heisenberg), because these concepts are refinements of the concepts of everyday life already on the philosophical level, rather than only on the physical or mathematical one. In classical physics, while still remaining linked to the concepts and language of everyday life, such as those of motion, these philosophical concepts undergo further idealization and, arguably most crucially for modern physics, mathematization. It also appears, however, that the epistemological complexities of quantum phenomena make philosophical considerations and concepts take a special significance in quantum mechanics and all quantum theory, for example, quantum field theory, and

[19] The question of mathematics in Aristotle's physics is more complex than it might appear. This should not be surprising, given that Aristotle had a deep understanding of the nature of mathematics and addressed it with a greater philosophical rigor than did Plato. Aristotle's view of mathematics itself also departs from that of Plato and is, along with his view of logic (another discipline that he helped to establish) closer to certain twentieth-century approaches to the philosophy of mathematics, especially those along constructivist lines (e.g., in Books 2 and 3 of *Metaphysics* [Aristotle 1984, vol. 2, pp. 1569–1584]). It is true that in Aristotle's physics, physical concepts were defined primarily by observable entities, rather than measurable quantities, in this case both material and mental, and by the less prominent and mostly implicit role of *mathematical thinking*. The latter, however, was far from absent either. It would of course be difficult to speak of mathematical formalism in the sense of modern, post-Galilean, physics in Aristotle's physics. That, however, need not mean that mathematical *thinking* itself in the sense of a certain *mathesis* of nature, or in this case, also thought, is absent. Indeed a certain form of "mathematical formalism" is found in Aristotle's *Physics* (*Physics*, Book 4, 215–216, Aristotle 1984, vol. 1, p. 366), which thus contains formulas or at least something formula-like for laws of motion. The "laws" expressed by these formulas may be incorrect, but giving these laws a mathematical form was a momentous move, which gave the conceptualization of motion both geometry (which governed physics until quantum mechanics) and a certain algebra or proto-algebra, which still governs physics, quantum theory included. Aristotle, thus, might have been the first to bring physics, mathematics, and philosophy together within physics and might have influenced Galileo's thinking in this regard.

to some degree in relativity. This point was often made by Bohr and Heisenberg, both of whom also stressed the significance of the history of philosophy and its concepts for physics, old and new. In introducing complementarity in the Como lecture, Bohr noted that “the idea of complementarity,” which “is suited to characterize the [new] situation” that emerged in physics with Planck’s discovery, “bears a deep-going analogy to the general difficulty in the formation of human ideas, inherent in the distinction between subject and object” (Bohr 1927, *PWNB* 1, p. 91). This statement places quantum theory and Bohr’s own thinking concerning complementarity within the history of post-Kantian philosophy. Bohr’s concepts of (nonconceptualizable) quantum objects and of phenomena are, as philosophical concepts, part of this history, even though they have emerged, just as has complementarity, from physical problems posed by quantum physics, and in the case of phenomena, specifically that posed by EPR’s argument.

According to Heisenberg, following Bohr and writing in the context of complementarity in his Chicago lectures of 1929:

> To mold our thought and language to agree with the observed facts of atomic physics is a very difficult task, as it was in the case of the relativity theory. In the case of the latter, it proved advantageous to return to the older philosophical discussions of the problems of space and time. In the same way it is now [in the case of quantum phenomena and quantum mechanics] profitable to review the fundamental discussions, so important for epistemology, of the difficulty of separating the subjective and objective aspects of the world. Many of the abstractions that are characteristic of modern theoretical physics are to be found discussed in the philosophy of past centuries. At that time these abstractions could be disregarded as mere mental exercises by those scientists whose only concern was with reality, but today we are compelled by the refinement of experimental art to consider them seriously. (Heisenberg 1930, p. 65)

One might further argue that the nature of quantum phenomena appears to take us beyond anything that philosophy has considered before the emergences of quantum physics. In the process, it also leads us to new philosophical concepts, such as Bohr’s concepts of complementarity and phenomenon, or atomicity, or quantum objects, in this case conceptualized as being beyond any conception. From early on, Bohr aimed “to emphasize as strongly as possible how profoundly the new knowledge [found in quantum theory] has shaken the foundations underlying the building up of *concepts*, on which not only the classical description of physics rests but also all our ordinary mode of thinking” (Bohr 1929a, *PWNB* 1, p. 101; emphasis added). The very phrase “the building up of concepts” is worth noting here. Bohr had to build new physical–philosophical concepts, such as complementarity, phenomena, and atomicity, although, as will be seen, these concepts also involve old physical concepts, which he argued to be both necessary and yet irreducibly insufficient in quantum mechanics. As he said later on: “In the last resort an artificial word like ‘complementarity’ which does not belong to our daily concepts serves only briefly to remind us of the epistemological situation here encountered, which at least in physics is of an entirely novel character” (Bohr 1937, *PWNB* 4, p. 87;

emphasis added). The statement defines complementarity as a new physical–philosophical concept, "which does not belong to our daily concepts" and must, accordingly, be understood in the specific sense Bohr gives it. This concept is nonclassical insofar as it defines and is defined by nonclassical epistemology, even though its architecture inevitably involves epistemologically classical conceptual components as well.

When it comes to interpretation, which often involves epistemological considerations, philosophy becomes unavoidable, however implicitly it may be used. Even leaving aside the question of interpretation, however, to invent a physical concept, one has to construct a general phenomenal entity or set of entities and relations between them, properly coupled to a given mathematical architecture and connectable, at least in principle, to observable phenomena and measurable quantities associated with these phenomena. Such a construction is a philosophical activity, although this resulting phenomenal entity may be mathematical (which also implies a phenomenal and hence philosophical component in the construction of mathematical concepts). This is often the case in quantum theory, beginning with Heisenberg's invention of his matrices, enabled by his extraordinary idea of arranging algebraic elements corresponding to numerical quantities (transition probabilities) into infinite square tables. This arrangement was a phenomenal construction, which also amounted to the construction of a mathematical object, a matrix. Matrices were, thus, reinvented by Heisenberg, who did not appear to be aware or did not appear to think at the time of the fact that such entities were already used in mathematics, in linear algebra. Dirac appears to have proceeded more by working with mathematical entities ("playing with equations," as he playfully called this rather serious activity) in developing phenomenal components of his concepts, which would only then be linked to measurable quantities, by means of, in the case of quantum mechanics, a specific and rather nontrivial procedure, in turn constructed by Dirac. By contrast, the starting point for Schrödinger was a certain phenomenal concept of wave motion, which he tried to mathematize by an equation, thus following the *ontological* model of classical physics, albeit only in part, since his waves had a different character from those considered in classical wave physics. Ultimately, it proved to be impossible to construct a properly descriptive (ontological) physical model of wave motion (of the type one finds in classical physics), corresponding to Schrödinger's equation, which, however, correctly predicted the outcome of the relevant experiments. Schrödinger's waves were instead reconceptualized by Born in terms of probability, closer to, and in part following, the epistemology of Heisenberg's approach.

Bohr's complementarity in the narrow sense provides an example of a new physical concept in which a more general phenomenal component is more pronounced and more open to being decoupled from physics and used in philosophy or elsewhere. The genealogy of the concept is unquestionably philosophical in part, but *only in part*, although many specific links of this genealogy are uncertain and are a matter of much debate among the scholars of Bohr. One well-established and important link, confirmed by Bohr, is Riemann's theory of functions of complex variables, which, earlier in his life (before he moved to physics), Bohr tried to apply to psychology rather than physics (Plotnitsky 2002a, pp. 236–238). Even apart from this philosophical genealogy, complementarity appears to have been invented as a general philosophical concept, albeit, crucially (hence my emphasis on "*only in part*"), coupled to physical phenomena and the formalism of quantum mechanics. In this latter respect and, accordingly, as a physical concept, it was defined by three key components—the phenomenological (as concerns its general composition), the mathematical (as concerns its connections to the mathematical formalism of quantum mechanics), and the experimental (as concerns its relations to the experimental data in question).

Philosophically, complementarity in the narrow sense is, we recall, defined by (a) a mutual exclusivity of certain phenomena, entities, or conceptions; (b) the possibility of establishing each one of them separately at any given point; and (c) the necessity of using all of them at different moments for a comprehensive account of the totality of phenomena that the corresponding theory, such as quantum mechanics, must consider. In this general definition (which, again, goes beyond merely the mutual exclusivity of certain entities), complementarity is a new philosophical concept, which need not refer to any physical phenomena. The last two parts, (b) and (c), do imply some domain of application, which need not be physical. It may be philosophical, psychological, biological, or still other, and Bohr made suggestions, albeit only tentative and speculative, of such applications.

Bohr, however, appears to have been led to complementarity through his attempts to give consistency to the manifold of quantum phenomena, such as that manifest in the uncertainty relations, and to rigorously relate this manifold to the mathematical formalism of quantum mechanics. This is why I stressed above that only *part* of the genealogy of this concept is elsewhere, while another and the most crucial part of it was in quantum physics. Although, as will be seen, Bohr's initial orientation in the Como lecture was more philosophical, he quickly came to center his interpretation of complementary quantum phenomena defined, more empirically, by the (observable) effects of the interaction between quantum objects and the measuring instruments. The latter are described by means of classical physics, whose concepts, accordingly, become part of the physical conceptual architecture of Bohr's complementarity, both in the narrow and in the broad sense. These effects can be linked to the measurable quantities, predicted, on the basis of already established numerical quantities, by means of the mathematical formalism of quantum mechanics. That these predictions are in general probabilistic is also part, a crucial part, of the conceptual architecture of complementarity. It is only this combination of all three—(1) the philosophical concept; (2) physical phenomena and measurable quantities associated with them; and (3) the mathematics of quantum mechanics—that makes complementarity in the narrow sense a properly physical concept.

Eventually, complementarity was further coupled to the "concept" of quantum objects and their behavior, "conceived" nonclassically as inconceivable (and hence not really conceivable and forming an actual concept), and their in turn inconceivable quantum interaction with measuring instruments. This concept of quantum object defines the nonclassical nature of Bohr's epistemology. The concept itself involves a general philosophical concept as well, that of the irreducibly inconceivable, and as such it can be given a general philosophical content and, thus, can be considered independently or transferred into other domains. It also has an important physical dimension, since the existence of certain material entities compelling us to introduce this concept in order to account for quantum phenomena is essential. On the other hand, it has no mathematical dimension and is only coupled to the mathematical architecture of other concepts involved in Bohr's interpretation, such as complementarity in the narrow sense.

The preceding discussion might appear to be in conflict with Bohr's well-known argument that the quantum mechanical situation does not require *new physical concepts*, as against those of classical physics, and that complementarity allows one to adequately handle this situation by means of the (old) concepts of classical physics. Bohr offers this argument in part in response to the contention of Einstein and Schrödinger that the quantum mechanical situation calls for new physical concepts (e.g., Schrödinger to Bohr 5 May, 1928; Bohr to Schrödinger 23 May, 1928; Einstein to Schrödinger, 31 May, 1928, Bohr 1972–1996, vol. 6, pp. 47–48). These concepts, they hoped, would give a *realist, classical grounding* to quantum physics by enabling one to offer an (idealized) description of the behavior of quantum objects in the way classical physics enables us to do by means of its concepts, or relativity, especially general relativity, was able to do by means of its new concepts. As will be discussed later, this approach reflects more generally Einstein's philosophy of physics and his brand of realism, defined by the mediation of concepts, to which the spectacular success of general relativity gave a major support, including vis-à-vis quantum theory, which, in his view, failed in this respect (e.g., Einstein 1949a, pp. 83–85). By contrast, in Bohr's approach, the use of the concepts of classical physics is part of a very different, ultimately *nonclassical interpretation* of the quantum mechanical situation, and while the exchange in question occurred before Bohr's interpretation reached its nonclassical stage, his thinking was already moving in this direction. In his view such classical concepts applied primarily and, in his eventual nonclassical view, strictly to the classical behavior of certain observable parts of measuring instruments and never to quantum objects themselves. Ultimately, the latter are placed beyond any possible description or even conception, in short, *beyond all concepts*, old or new, thus in principle precluding realism at this level.

Bohr's argument concerning the role of classical physical concepts in quantum theory has further subtleties, which I shall discuss later along with persistent misunderstandings of his views on the subject. It is, however, clear from these remarks and the preceding discussion as a whole that there is no contradiction between this argument and the possibility and significance of new physical concepts, Bohr's own included, in quantum theory. There is only a difference in the nature and epistemological functioning of physical concepts in two respective views of the situation. When, in responding to Einstein or

Schrödinger, Bohr speaks of "old physical concepts," he clearly means the *descriptive* concepts of classical physics, specifically those associated with the motion (which is itself such a concept) of classical objects and observable measurable quantities that could be directly assigned to these bodies themselves. At least such an assignment is possible in principle, even if, in practice, this assignment may require measuring instruments. As just explained, however, such (old) classical physical concepts are *only part* of the overall conceptual architecture of complementarity and are specifically used only to describe the physics of measuring instruments, which is classical and, hence, realist, and, as such, is part—but, again, only part—of this overall architecture. In this regard, old classical physical concepts are indeed indispensable, which is part of Bohr's argument. On the other hand, since no concepts of the ontologically descriptive type, old or new, can apply at the quantum level in any event, the *need* for new concepts at this level would be meaningless in Bohr's interpretation. The latter, however, along with quantum mechanics itself, appears to be sufficient for what nature allows us to do.

By contrast, in hoping for new concepts, Einstein or Schrödinger clearly have in mind new concepts of this descriptive type, which would enable a classical-*like* idealization or model of quantum behavior. I stress "like" because the resulting "quantum mechanics" would have to be different from classical mechanics. This approach is indeed in accord with Schrödinger's initial program for his wave mechanics, which was welcomed by Einstein, in part for this reason, as against Heisenberg's approach, as the letter exchange in question makes apparent as well. As will be seen in Chapter 5, Schrödinger's program aimed at a new conception of waves that would be operative at the quantum level. The program ultimately failed, but the hope was never abandoned, at least by Einstein, since Schrödinger eventually grew more pessimistic in this regard. The question, then, is whether our inability to offer a classical-like description of quantum objects and processes is a failure of quantum mechanics in its current form or of our scientific imagination, or whether nature at the quantum level of its constitution allows for such a classical theory. I shall now explain why the new or significantly new (as against those of classical physics) physical concepts of the idealized descriptive type envisioned by Einstein or Schrödinger may not be possible.

As both Bohr and Heisenberg argue, classical physical concepts and all intuitable concepts that would enable a meaningful physical ontology are the products of a suitably mathematized refinement of our everyday representational intuition (*Anschaulichkeit*), conceptuality, language, and so forth (e.g., Bohr 1954, *PWNB* 2, pp. 68–69; Heisenberg 1930, pp. 11, 64–65; Heisenberg 1962, pp. 56, 91–92). They have emerged in our interaction with the world on the scales that are very different from those of quantum theory (or at the opposite end of the available physical scales from those of the Universe) and may be determined evolutionarily by the biological constitution of our bodies. According to Bohr's letter to Schrödinger, mentioned above: "I am not quite in agreement with your emphasis on the necessity to develop 'new' concepts. We have not only, as far as I can see, no basis for such a new-fashioning so far, but the ‘old’ empirical concepts appear to me to be inseparably linked to the foundation of the human means of visualization” (Bohr to Schrödinger 23 May, 1928, Bohr 1972–1996, vol. 6, p. [48]). It is worth citing here Heisenberg’s comment, made in his later *Physics and Philosophy*, but in the context of the Copenhagen (essentially Bohr’s) interpretation of quantum mechanics, as it emerged in the 1930s. Heisenberg addressed the key paradox (or what so appears) at the heart of the Copenhagen interpretation. This paradox was, as will be seen, at the core of his own response to EPR’s argument. This paradox is as follows: “[The Copenhagen interpretation] starts from the fact that we describe our experiments in the terms of classical physics and at the same time from the knowledge that these concepts do not fit nature accurately. The tension between these two starting points is the root of the statistical character of quantum theory” (Heisenberg 1962, p. 56). Heisenberg then says

> Therefore, 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 nonstat[ist]ical [sic!], 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 this basis. There is no use in discussing what could be done if we were other beings than we are. At this point we have to realize, as von Weizsäcker has put it, that “Nature is earlier than man, but man is earlier than natural science.” The first part of the sentence justifies classical physics, with its ideal of complete objectivity. The second part tells us why we cannot escape the paradox of quantum theory, namely, the necessity of using classical concepts. (Heisenberg 1962, p. 56)

In other words, as indicated earlier, classical concepts reflect the essential workings of our neurological machinery born with our evolutionary emergence as human animals. Our *thinking in general*, as the product of this machinery, is classical, even in nonclassical situations, since the entities, such as quantum objects, that make a theory considering them nonclassical are, by definition, beyond the capacity of our thought. That is, all concepts that we can actually form, that are available to our conceptual intuition, are epistemologically classical in the present definition. As such they have a phenomenal, mental ontology in the sense of either forming conceivable entities, such as spatial positions, or referring to such entities or, in Einstein’s language, elements of the physical reality via measurable quantities, which enables these concepts to map, in an idealized way, the behavior of physical objects in question.[20] Accordingly, it is far from

[20] Such physical quantities as momentum or energy are not phenomenally visualizable entities, as against, say, positional marks on rods and clocks, located in space. They can, however, be related to measurable quantities (ultimately, always defined by certain positions of one pointer or another) by means of rods and clocks. In this respect, these concepts in classical and quantum physics (and they and the corresponding conservation laws apply in both cases) are closer to each other—closer but not quite the same, at least in a nonclassical

clear how far classical concepts, that is, again, all concepts we can possibly form, can be used in considering quantum objects and processes at the quantum level. Indeed, it appears that they are not likely to apply at this level at all.

One might speak more rigorously of forming *new mathematical concepts*. Mathematical concepts, Bohr argues, are, too, ultimately "a refinement of general language, supplement[ed] ... with appropriate tools to represent relations for which ordinary verbal expression is imprecise or cumbersome" (Bohr 1954, *PWNB* 2, p. 68). To complicate Eugene Wigner's famous contention, this view makes the effectiveness of mathematics unreasonable (enigmatic) only in quantum but not in classical physics (Wigner 1960). It is also worth citing Bohr's added remark here: "it may be stressed that, just by avoiding the reference to the conscious subject which infiltrates daily language, the use of mathematical symbols secures the unambiguity of definition required for objective description," or unambiguous communication, which is also true in physics, classical and quantum alike (Bohr 1954, *PWNB* 2, p. 68). Certain mathematical concepts, however, are further removed from our everyday intuitions, specifically those (i.e., of objects and/in motion) that found their way into classical mechanics. Indeed, as Hermann Weyl argued this refinement can take us quite far from our everyday experience, concepts, and language (Weyl 1918, p. 108). The mathematical concepts used in quantum mechanics, such as those of the Hilbert-space formalism, are precisely such concepts. Highly unvisualizable in turn, they are not suited for the kind of classical-like (based in our everyday intuition) description of physical behavior that defined the models of classical physics or even relativity, although the latter already has significant complexities in this regard. According to Heisenberg

> It is not surprising that our language [or conceptuality] should be incapable of describing processes occurring within atoms, for ... it was invented to describe the experiences of daily life, and these consist only of processes involving exceedingly large numbers of atoms. Furthermore, it is very difficult to modify our language so that it will be able to describe these atomic processes, for words can only describe things of which we can

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view of the situation. Unlike a position or a temporal coordinate, which is never observable and never determinately assignable to quantum objects, the latter can, *in a certain sense*, be assigned momentum and energy (or stably, charge) even in a nonclassical view. In the case of momentum, it can be done, for example, by measuring the change of the momentum of a certain part of a measuring device under the impact of a quantum object. Still, in a nonclassical view of the situation, one could only speak of an assignment of this momentum to the quantum object itself and in an ultimately provisional sense, since there is no way to determine this momentum apart from involving a measuring instrument. As I said, while, in practice, the use of measuring instruments is necessary in classical physics as well, their role can, in principle, be neglected, and the quantities in question could be assumed to be assigned to classical physical objects themselves. One may also express this point by saying that a classical object may be made into a measuring instrument, a rod or a clock. This is not possible in the case of quantum objects, even if they are macroscopic, since they cannot be observed *as* quantum objects and their quantum nature can only be ascertained via quantum phenomena observed in measuring instruments.

> form mental pictures, and this ability, too, is a result of daily experience. Fortunately, mathematics is not subject to this limitation, and it has been possible to invent a mathematical scheme—the quantum theory—which seems entirely adequate for the treatment of atomic processes. (Heisenberg 1930, p. 11)

One might contest the view that "words can only describe things of which we can form mental pictures." Heisenberg's larger point is more crucial, however. Mathematics and its concepts rigorously apply in quantum theory, as well as in classical physics, and this application of mathematics was made possible through the invention or deployment of new mathematics, at least new as far as their use in physics was concerned, since much of this mathematics was previously developed in mathematics itself. In his later reflection on this situation in *Physics and Philosophy*, Heisenberg describes it from a deeper and more grounded philosophical perspective, in addressing the relationships between quantum theory and Kant's philosophy. He says

> Any concepts or words which have been formed in the past through the interplay between the world and ourselves are not really sharply defined with respect to their meaning; that is to say, we do not know exactly how far they will help us in finding our way in the world. We often know that they can be applied to a wide range of inner or outer experiences, but we practically never know precisely the limits of their applicability. This is true even of the simplest and most general concepts like "existence" and "space and time." Therefore, it will never be possible by pure reason to arrive at some absolute truth [as Kant thought it might be].
>
> The concepts may, however, be sharply defined with regard to their connections. This is actually the fact when the concepts become a part of a system of axioms and definitions which can be expressed consistently by a mathematical scheme. Such a group of connected concepts may be applicable to a wide field of experience and will help us to find our way in this field. But the limits of the applicability will in general not be known, at least not completely. (Heisenberg 1962, p. 92)

Mathematics helps us establish a set of (more) sharply defined concepts, and in classical physics these concepts could be linked, by way of idealized models, to our space–time intuitions concerning and rigorous representations of the behavior of classical objects, such as planets moving around the sun. However, relativity and then, more radically, quantum mechanics revealed the limits of and severe limitations upon the applicability of such concepts. In relativity these difficulties become apparent, for example, if one tries to assign the classical spatial or temporal properties to a moving photon in special relativity. Were it possible to put a clock on a photon, it would stand still, while at the same time the photon would be found in all locations of its trajectory at once within its own spatial frame of reference. Rigorously, this means that the very concepts of clock and (measuring) rod lose their meaning in the frame of reference of a moving photon (there are no other photons), as does the concept of frame of reference. The latter concept is crucial to special relativity, when applied to moving systems other than photons, or indeed to all physics and other sciences. The problems of both causality and realism become even more formidable in general relativity, even leaving aside the role of quantum theory there, as in considering the Hawking radiation of black holes. For the moment, in quantum mechanics mathematics enters the conceptual architecture of the theory very differently from the way it does in classical physics. There is a set of connections through which the concepts, physical, mathematical, and philosophical, are sharply defined in quantum mechanics. But, at least in a nonclassical view, these connections are quite different from those found in classical physics. In quantum theory, they are not representational, but are strictly predictive and probabilistic. The mathematics of quantum theory does not serve as part of the architecture of descriptive physical concepts applied to quantum objects in the way the mathematized physical concepts are applied to physical objects in classical physics.

It follows that the definition or the *concept* of physical concept adopted here is broader than that customarily used in classical physics, insofar as by the latter one means the (mathematically idealized) description of the behavior of physical objects. To some degree, this type of extension of the concept of physical concept could be used in classical physics as well or of course in relativity. The conceptuality of both theories, especially relativity, is richer than that involved in describing, physically and mathematically, classical physical objects and their behavior. But, in Bohr's and the present (nonclassical) view, this extension is necessary in quantum physics. In this view, the overall conceptual architecture of, say, Heisenberg's new kinematics or, more elaborately, complementarity is a new physical conceptual conglomerate in the sense defined here, and it involves both old concepts of classical physics and new philosophical concepts, and both old mathematics (that of classical physics) and new mathematics (that of quantum physics). What it does not involve—and this difference is itself part of its new architecture—are concepts available to our space–time intuition and physically and mathematically describing, even by means of idealized models, quantum objects and their behavior. Both are defined as indescribable by means of quantum mechanics and as ultimately inconceivable by any means available to us. This is how a nonclassical conceptual architecture *idealizes* the ultimate constitution of nature.

These are new mathematical schemes, coupled to configurations, including new configurations, of experimental technology (these are again classical), that enable and assure the construction of new concepts in quantum theory, rather than, as has been the case in classical physics, new (idealized and often mathematized) descriptive concepts or models. Indeed, in illustrating his claim, cited above, that "the primary goal of fundamental physics is to discover profound concepts that illuminate our understanding of nature" (Wilczek 2005, p. 239), Wilczek associates and even identifies the currently most significant concepts with mathematical objects, in particular, particles with symmetry groups. This is hardly anything physical in the classical sense. This point, it is worth observing was made by Heisenberg long ago in his article "What is an Elementary Particle?" via Plato (Heisenberg 1989, pp. 82–83). Commenting on his nonclassical epistemology in one of his later essays, Bohr said: "Such argumentation does of course not imply that, in atomic [quantum] physics, we have no more to learn as regards experimental evidence and the mathematical tools appropriate to its comprehension. In fact, it seems likely that the introduction of still further abstractions into the formalism will be required to account for the novel features revealed by the exploration of atomic processes of very high energy [i.e., in quantum field theory]" (Bohr 1958; *PWNB* 3, p. 6). This is still true. The same point is found in Bohr's earlier essays, especially in the context of Dirac's work in quantum electrodynamics. The new physical conceptuality of quantum theory, such as quantum field theory, is created not by means of new descriptive concepts but by joining new mathematical "abstractions" and new experimental evidence and new configurations of such evidence, possibly, as Bohr noted elsewhere, requiring an epistemology that is even more radical than that of complementarity (Bohr 1937; *PWNB* 4, p. 88).

The rigor that defines mathematics and science is possible in view of the formal nature of mathematics and the experimental, or, again, mathematical–experimental, character of modern science, at least physics, from Galileo on. As Heidegger argues, in Galileo's and subsequent physics (quantum physics included), as a mathematical science of nature, experiment and mathematics not only relate to each other, but also mutually define each other, once one bases one's account of nature on measurable quantities and the reference frames through which these quantities are defined (Heidegger 1967, p. 93). This argument has far-reaching implications for quantum mechanics, beginning, again, with its discovery by Heisenberg, who was able to establish the reciprocity of the mathematical and the experimental in his new mechanics, and thus define it as a mathematical–experimental science of nature. Quantum mechanics, however, also led to a profound questioning of how we can and cannot use such measurable quantities in our physical theories. It questioned the ways in which physics relates to its frames of reference, most especially in view of the irreducibly probabilistic nature of the quantum mechanical predictions, as against those of classical physics, even though the physical nature of the respective data and reference frames is the same. In spite of this questioning, in both cases, that of mathematics and that of science (at least in mathematical sciences), numerical quantities are inevitably involved.

Bohr's work on quantum theory is defined by his labor of *ordering*, unambiguous ordering, our scientific experience in the wake of new complexities or even new forms of chaos introduced by quantum physics, from Planck on. As he said: "[With] the complementary description of quantum physics ... [the] history of physical science thus demonstrates how the exploration of ever wider fields of experience, in revealing unsuspected limitations of accustomed ideas [such as the causal mode of description], indicates new ways of restoring logical *order*" (Bohr 1954, *PWNB* 2, p. 74; emphasis added). From the moment of Heisenberg's discovery of quantum mechanics, he saw quantum mechanics as his greatest scientific help in this war, scientific and philosophical, against chaos, or, again, against chaos, on the one hand, and against dogmatically held physical and philosophical preconceptions, on the other. He thought that we equally need all three, mathematics, physics, and philosophy in this struggle, which is still ongoing for us and is unlikely to ever end. Bohr's *epistemology* may be seen as more Kantian than Hegelian (although it may be argued to be more radical than that of Kant himself). Bohr's *work* may, however, be seen as Hegelian as concerns his invention of new concepts, through which it is possible to handle this epistemology and put it to work, as physics and as philosophy. Our capacity to create such concepts in the face of radical epistemological situations, threatening our thought, may be seen as a crucial part of Hegel's response to Kant, although the practice itself of creating new concepts is of course found in Kant's work as well.

According to Heisenberg: "Bohr was primarily a philosopher, not a physicist, but he understood that natural philosophy in our day and age carries weight only if its every detail can be subjected to *the inexorable test of experiment*" (Heisenberg 1967, p. 95, emphasis added). One might question whether natural philosophy in our day and age carries weight only in this case, or whether Bohr was *primarily* a philosopher, rather than a physicist. I would argue he was both. I would also argue that he was at his best philosophically when dealing with physics, rather than in extending his ideas, such as complementarity, beyond physics, which, as he always noted, he was doing in tentative and preliminary ways in any event. Heisenberg's remark does, however, convey an important aspect of the relationships between physics and philosophy, and among mathematics, physics, and philosophy, in Bohr and beyond Bohr. This is one way of bringing philosophy, physics, and mathematics together: a creation of new physical–philosophical concepts that make it possible to link a given (mathematical) theory, such as quantum mechanics, to new, in this case specifically complementary, configurations of frames of reference, established by experiment. Heisenberg's discovery of quantum mechanics was defined by the process described by him in the sentence immediately preceding the one just cited. He says: "Thus I understood [from these conversations with Bohr, preceding his discovery]: knowledge of Nature was primarily obtained in this way [by intense occupation with the actual phenomena], and only as the next step can one succeed in fixing one's knowledge in mathematical form and subjecting it to complete rational analysis" (Heisenberg 1967, p. 95). That may not be the only way to obtain such knowledge (one can sometimes start with mathematics, as Dirac often did), but it was for Heisenberg, at least at this stage of his work.

1.5 Epistemology and Interpretation

As explained at the outset of this study, by an interpretation of a physical theory I mean a rigorous explication of the physical content of the theory, as defined by its mathematical formalism cum the experimental data to which this formalism relates either by way of (suitably idealized) description or in terms of predictions, or both. This understanding of interpretation requires further qualifications, especially as concerns the interpretive and philosophical elements involved already in the formation of a theory or, to begin with, of experimental data or “facts,” even if one leaves aside the cultural factors involved. For “facts” can never be defined apart from some “theory” (which need not, however, be mathematical, but may be philosophical) and “theory” apart from some interpretation. Interpretive and epistemological adjustments are often necessary even when moving from one mathematical version of the quantum mechanical formalism to another, although such versions may be seen as equivalent mathematically or in terms of their predictive capacities. I shall address these questions later in this study. For the moment, a nonclassical interpretation of quantum mechanics, such as that of Bohr or the one offered in this study, goes farther than most arguments concerning the difficulties of using causal and realist approaches in quantum theory. As noted from the outset, most, even among those thinking close to “the spirit of Copenhagen,” are reluctant to go that far in “renouncing customary demands as regards the explanation of natural phenomena” (Bohr 1949, *PWNB* 2, p. 63). For, in this view, a physical description or even conception concerning quantum objects and processes (ultimately including such concepts as quantum, object, and process) is, in Bohr’s words, not merely subject to an “arbitrary renunciation” but is “*in principle* excluded” (Bohr 1949, *PWNB* 2, p. 62). Bohr is known to have replied to Harald Hoffding’s question “Where can the photon be said to be?” with “To be, to be, what does it mean to be?” (cited in Wheeler 1998, p. 131). Both of these questions are still unanswered and, in Bohr’s view, they *are* indeed unanswerable. Bohr’s ontology only allows quantum objects to exist but does not and cannot say anything about them apart from the manifest effects of their interaction with measuring instruments or the classical world, where such questions as “Where can something be said to be?” can be answered or meaningfully asked, to begin with. At most we can say what it means to measure the position of something else *associated* with a given quantum object and its, ultimately unknown, position. This associated position (no quotation marks necessary) is that of a certain mark in measuring instruments (or a pointer position), for which “to be” can also have a rigorous physical meaning. By contrast, for a quantum object “to be” might only mean to be able to produce and to be derived as an inference from such effects, each of which is physically describable in terms of classical physics, which at the same time cannot adequately predict such effects or describe the physical processes responsible for them. This is, however, a sufficiently efficacious way of existing and of manifesting one’s existence. One might, again, say that, while quantum objects, *or something in nature that compels us to think or speak about them*, do exist, their existence has no specifiable ontology that we can assign to them or even conceive of.[21]

[21] It is perhaps in this sense that we may best understand Bohr’s famous (reported) statement, apparently in response to the question “whether the algorithm of quantum mechanics could be considered as somehow mirroring an underlying quantum world”: “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”

Moreover, as I have noted from the outset and as the underlined qualification suggests, the actual features of nature responsible for these effects may still need to be seen as different even from quantum objects and processes, as defined by a given nonclassical idealization. Following upon this qualification, the epistemological position adopted by this study *might* deviate from that of Bohr. While otherwise not essentially different from Bohr's interpretation, the nonclassical interpretation adopted here is only seen as *an* interpretation, one among several possible interpretations, rather than *the* interpretation, of quantum phenomena and quantum mechanics claimed to correspond to *the* true state of affairs in nature. I say "might" because Bohr's position on the issue appears to have been ambivalent, and, while certain of Bohr's statements appear to suggest stronger claims, others appear to indicate that he was at least inclined to a view similar to the one adopted here (hence I speak of "a *possible* contrast" with Bohr's view). Bohr's customary caution would suggest that he might have been so inclined. Consider, for example, this formulation in his reply to EPR: "a *viewpoint* ... termed 'complementarity' is explained from which quantum-mechanical description of physical phenomena *would seem to fulfill*, within its scope, all rational demands of completeness" (Bohr 1935b, p. 696; emphasis added). In part by virtue of this ambivalence, this position is itself subject to interpretation and thus is part of one's *interpretation* of Bohr's interpretation, and, as I said, there is no uninterpreted interpretation of Bohr, even for Bohr himself. Bohr's ambivalence only adds a certain additional specificity to this particular case. I shall not take a strong position concerning the matter, although one could, I think, mount an argument for Bohr's *ambivalence* in this respect, possibly never quite resolved. On the other hand, one can maintain with a reasonable degree of certainty that Bohr saw as the state of affairs, at least as things stand now, such features as complementarity in the narrow sense, the irreducible role of measuring instruments in forming quantum phenomena, and the probabilistic nature of our predictions concerning the outcome of quantum experiments. The present study takes the same view as concerns these experimental circumstances. It takes, again, a more qualified view only as concerns a given nonclassical interpretation, at least the one adopted here, of quantum phenomena and quantum mechanics, by considering it only as one possible interpretation.

(reported in Petersen 1985, p. 305). One must exercise extreme caution in considering such extemporaneous comments, especially when they are reported, and this one, as reported by Petersen, appears to lack Bohr's customary carefulness, found in his writings, although this is not uncommon in extemporaneous remarks like this one. This said, the statement, especially given the context defined by the question to which it replies, may be best read, along nonclassical lines, as denying not the existence of quantum objects but the applicability of any conceivable description or conception to them and their behavior, including "quantum" and "world," or again, "object" and "behavior." "An abstract physical description," provided by quantum mechanics, does not describe the behavior of quantum objects, but only provides a set of algorithms for predicting, probabilistically, the outcomes of quantum experiments.

Accordingly, the present view leaves space for other possible interpretations of quantum phenomena and quantum mechanics, which might at some point supersede any nonclassical interpretation of them. It also leaves space for the alternative accounts of these phenomena, that is, theories whose scope is the same as that of quantum mechanics. Given the currently available experimental evidence, quantum mechanics may be a correct and a complete, as complete as now possible, theory of quantum phenomena. That, however, does not mean that we cannot have an alternative theory of these phenomena, possibly even a theory that is mathematically not equivalent to quantum mechanics (which, again, has several equivalent mathematical versions). It is of course also possible that the ultimate constitution of nature may allow for a more classical epistemological treatment. If such proves to be the case, as it might one day (and some, again, would contend it is the case already), the epistemological status of quantum physics will change as against nonclassical epistemology. The *philosophical concept* of nonclassical epistemology will, again, not be affected, and may continue to work elsewhere in mathematics and science, philosophy, or art.

At the same time, the *more cautious* view adopted here has epistemological implications of its own, and may even be argued to be *more radical* epistemologically than the view that such an interpretation is definitive as concerns the quantum constitution of nature. For, as indicated above, this view implies that this "constitution" itself (ultimately this term, too, or any corresponding concept does not apply any more than any other term under these conditions) is placed at a yet further remove from our thought. That is, quantum objects and processes may be inaccessible even as inaccessible, unrepresentable even as unrepresentable, unknowable even as unknowable, unthinkable even as unthinkable, inconceivable even as inconceivable, unintuitable even as unintuitable, and so forth. Nothing can be said or even thought about them, now or possibly ever, except that they or, again, something in nature that makes us appeal to such a conception, "exists." But then, under these conditions we cannot *uncritically* use the term "exist" either, any more than the terms "object," "nature," "the ultimate constitution of nature," and "inaccessible," or any other term. We must continue to critically examine this use, perhaps and even likely interminably, since whatever we can come up with is bound to fail at some point or in any event to become part of a development that we cannot control.

In any event, in the present view the character of quantum phenomena is defined by an epistemological double rupture. The first rupture, which radicalizes the Kantian rupture between phenomena and noumena, is that between quantum phenomena and quantum objects, considered as the *objects of quantum mechanics cum a nonclassical interpretation*, as objects placed beyond the reach of the theory itself or any possible conception, made unthinkable, as against the thinkable nature of Kant's noumena. The second rupture is between this theoretical-interpretative scheme and the possible constitution of nature. As will be discussed in detail later, the first rupture provides the proper meaning of quantum discontinuity according to Bohr, in contrast to a classical particle-like discontinuity, with which quantum phenomena were associated in the wake of Planck's discovery and still are sometimes. The second rupture defines the first rupture as a conceptual and theoretical (quantum-mechanical) idealization. Thus, while, along with quantum mechanics itself, this interpretation appears to tell us something, indeed something radical and remarkable, about our interaction with nature, it may leave nature itself to an even greater mystery, or conversely, to something less mysterious. A recent comment by Anton Zeilinger and coauthors, made in conjunction with the centenary of Einstein's introduction of the idea of the photon, offers a compelling view of the situation. They write

> Evidently, Einstein's 1905 proposal of the photon concept has had tremendous impact. But Einstein should also be highly credited for his various criticisms of quantum physics that were part of the early debate with his contemporaries (including Bohr). They triggered a body of both theory and experiments concerned with individual quantum systems. In this context, experiments with photons have had a pioneering role. Although such experiments now rule out Einstein's point of view, they gave rise to the new field of quantum information processing. But the conceptual problems are not fully settled. This is signified by the wide spectrum of different interpretations of quantum physics that compete with each other. In our view, a common trait of many interpretations is that entities are taken to be 'real' beyond necessity. This is most obvious for the case of the 'many-worlds interpretation,' where the coexistence of parallel worlds is claimed without compelling evidence, but it also holds, for example, for the Bohm interpretation where, again without compelling evidence, each particle is given a well-defined position and momentum at any time. We suggest that these are simply attempts to keep, in one way or other, a [realist?] view of the world. It may well be that in the future, quantum physics will be superseded by a new theory, but it is likely that this will be much more radical than anything we have today. (Zeilinger et al. 2005, pp. 236–237)

The emphasis on "a theory and experiments concerned with individual quantum systems" is worth noting, except that of course this is, according to Heisenberg and Bohr, what quantum mechanics has been beginning with its introduction by Heisenberg, but, it is also worth noting, only a probabilistic theory of such systems, in part indeed by virtue of not being realist. Not everyone would agree that the "experiments" in question (mostly of the EPR type) "now rule out Einstein's point of view," although this study, too, suggests that it appears so, at least for now. The final point of this statement is, however, compelling, and is not altogether surprising given what higher level quantum theories appear to tell us. The epistemological argument offered here only applies to quantum mechanics as a theory operative within its particular scope and limits, just as classical physics is operative within its scope and limits, or various quantum field theories are within their respective scopes and limits. As I shall discuss in the conclusion, the epistemology of quantum field theory, beginning with quantum electrodynamics, may require still more radical renunciations of our epistemological ideas and ideals, as Bohr, again, pointed out (Bohr 1949, *PWNB* 2, pp. 6, 60; Bohr 1958, *PWNB* 3, p. 6; Bohr 1937, *PWNB* 4, p. 88). But then, it may not. The prospects are even less certain for more comprehensive theories that are necessary (as our theories at present are manifestly incomplete) but yet to be developed. As we have not heard their last word or perhaps even the last word on quantum mechanics (which is to say the next word, since there may not ever be such a last word), one cannot be sure. Nature might show itself less mysterious at the next stage of our, it appears, interminably inconclusive encounter with it—an encounter in which, as quantum mechanics and Bohr reminded us, "we are both onlookers and actors in the great drama of existence" (*PWMB* 1, 119).

Chapter 2
Quantum Phenomena and the Double-Slit Experiment

Abstract The aim of this chapter is to introduce, in concrete physical terms, quantum phenomena, by which I, again, mean those physical phenomena in considering which Planck's constant h cannot be neglected. I shall do so by way of the double-slit experiment, a paradigmatic or, it is sometimes argued, even *the* paradigmatic quantum experiment, in which the famously strange features of quantum phenomena manifest themselves. This experiment and the way it reflects such key features of quantum phenomena as the uncertainty relations and the probabilistic nature of our quantum predictions are considered in Sections 2.1 and 2.2. Sections 2.3 and 2.4 discuss two other experiments that are closely related to the double-slit experiment: the delayed-choice experiment, due to John A. Wheeler, and the quantum eraser experiment, due to Marlan Scully and his coworkers. Section 2.5 uses the quantum eraser experiment to establish the fundamental difference between classical and quantum physics by considering the repetition of the identically prepared experiments in each domain.

2.1 The Double-Slit Experiment: From an (Almost) Classical to a Nonclassical View

This section offers a general discussion of the double-slit experiment, initially by using the concepts and language of classical physics, as far as it is possible to do so, as the "almost" of my title indicates. Certain qualifications are necessary from the outset in view of the fact that these concepts and language appear to be ultimately inapplicable to quantum objects and processes, which are responsible for the phenomena appearing in the double-slit experiment. Part of the paradigmatic importance of the double-slit experiment consists in the fact that it reveals these difficulties, to which the nonclassical epistemology of quantum phenomena and quantum mechanics responds. Indeed, "an *almost* nonclassical to a *nonclassical* view" might be a more accurate description of my discussion of the double-slit experiment here.

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One does not need quantum mechanics to explain its key features, and in this regard this chapter is properly introductory, since it does not depend either on quantum mechanics itself or on the discussion of quantum mechanics offered in the subsequent chapters of this study. This experiment can be performed with all quantum objects (even, as was shown more recently, composite ones, such as carbon 60 fullerene molecules, which are rather large relative to elementary particles), rather than only with radiation. Not until the 1960s was it actually performed as a *quantum experiment* with anything other than light. Before then, it functioned as a thought experiment, without, however, much doubt that it could in principle be performed on any type of quantum objects. This confidence was further supported by a number of other key quantum experiments, which have been performed and which exhibit the key features of quantum phenomena exhibited in the double-slit experiment. As a classical experiment, Thomas Young's double-slit experiment with light has been around since 1801, when it was performed in order to resolve the question whether the light was composed of particles (according to Newton's corpuscular theory) or was formed by waves traveling through some form of ether. The interference patterns found in the experiment appeared to have answered the question in favor of the wave theory, which became a prevalent view before Planck's discovery of his black body radiation law and related developments of quantum theory. Other elementary constituents of matter, such as electrons, eventually revealed the same dual character, conjectured by Louis de Broglie and quickly demonstrated experimentally in the 1920s. Bohr's complementarity responded to and, to begin with, posed the question of the wave vs. particle nature of light in a very different form, whereby neither concept was any longer applicable to quantum objects and their behavior.

The double-slit experiment was first performed with electrons in the 1960s by Claus Jönsson and with "one electron at a time" by Pier Giorgio Merli in 1974. In the 2002 poll conducted in *Physics World* (September 1, 2002), Jönsson's experiment was voted "the most beautiful experiment" ever performed, just edging Galileo's experiment with falling bodies. Young's original experiment made the top 10 as well: It ranked fifth, following Newton's decomposition of sunlight with a prism. It is difficult to say whether one can, or needs to, claim as much for the double-slit experiment, whether as concerns its beauty (a more subjective matter to begin with, as the *Physics World* poll acknowledges) or even as concerns its archetypal significance in quantum physics. It has a few formidable rivals that are nearly as famous and can be, and have been, used equally well for illustrating the famously strange features of quantum phenomena. One can mention, for example, the Stern–Gerlach experiment, various experiments in quantum interferometry, the beam-splitter experiment and other experiments with half-silvered mirrors, or experiments of the EPR type, which I shall discuss in Chapters 8 and 9. However, the double-slit experiment appears to remain the most famous quantum experiment and the one most frequently deployed for these purposes, although it has been nearly supplanted by the experiments of the EPR type in more recent discussions, those following Bell's theorem and related developments.

Another advantage of the double-slit experiment is that, as I said, it can be especially easily (more so than the other quantum experiments mentioned above) explained *qualitatively* without any technical knowledge of quantum theory. Properly predicting the *quantitative* data associated with the outcomes of the corresponding actually performed (or simulated) experiments would, of course, require the mathematical formalism of some quantum theory, such as quantum mechanics, which predicts such outcomes with great accuracy. Finally, the double-slit experiment also manifests especially dramatically the key probabilistic and statistical aspects of our predictions concerning quantum phenomena—in particular, the relationships between randomness and probability and hence between randomness and certain (correlational) order, which the probabilistic predictions of quantum mechanics capture.

The experiment was crucial to Bohr's thinking about quantum phenomena and quantum mechanics, and to his exchanges with Einstein, including those concerning the EPR experiment. The double-slit experiment did not figure in Bohr's Como lecture of 1927, which introduced complementarity, or in the preceding work on quantum mechanics by Heisenberg, Schrödinger, and others, on which the Como argument was based. However, the experiment became central to Bohr's exchanges with Einstein immediately thereafter (Bohr 1949, *PWNB* 2, pp. 41–42). The main reason for the persistent appeal to the experiment on Bohr's part is that it can be effectively used to test our claims concerning quantum phenomena and quantum mechanics, which properly predicts the numerical data found in the double-slit experiment and thus responds to the peculiar character of the phenomena observed. In other words, once a given argument concerning either quantum phenomena or quantum mechanics leads to a conflict with these features, this argument may be set aside as something that is in conflict with the experimental evidence. As noted above, while the double-slit experiment was not actually performed as a quantum experiment until later, other quantum experiments that had been performed could be considered as equivalent to it with respect to the key features of quantum phenomena at stake, which enabled one to use the double-slit experiment as a thought experiment in theoretical arguments.[1] In particular, any attempt to circumvent Heisenberg's uncertainty relations, $\Delta q \Delta p \cong h$, leads to this type of inconsistency with the double-slit experiment. The physical meaning or interpretation of the uncertainty relations is subtle matter, which I shall

[1] Cf. Bohr's comments on the subject in (Bohr 1935b, 698, n.). This is not unusual in dealing with thought experiments. The EPR experiment, as originally proposed by EPR, cannot be performed in a laboratory, though this has never put in question its legitimacy for the theoretical arguments concerning or based on it. Related experiments, most famously those by Alain Aspect, based on Bohm's version of the EPR experiment for spin (Aspect et al. 1982), have subsequently been performed, as were experiments statistically approximating the EPR experiment.

address in the next section. Bohr saw the uncertainty relations as experimentally given, a law of nature, and the fact that they can be derived from quantum mechanics as further testimony that the latter adequately reflects the experimental data in question. Indeed, the uncertainty relations and the data observed in the double-slit experiment are equivalent to each other, a fact used by Bohr throughout his arguments for complementarity, especially, again, in his exchanges with Einstein (e.g., Bohr 1935b, pp. 697–700; Bohr 1949, *PWNB* 2, pp. 43–47, 52–61).

The experimental arrangement defining the double-slit experiment consists of a source, such as that of a monochromatic light (which makes it possible to emit photons one by one), and, at some distance from it, a diaphragm with a single slit (A); at a sufficient distance from it a diaphragm with two slits (B and C), widely separated; and finally, at a sufficient distance from the diaphragm, a screen, a silver bromide photographic plate (Fig. 2.1).[2] Two setups are considered, in each of which a sufficient number (say, a million) of quantum objects, such as electrons or photons, emitted from a source, are allowed to pass through the slits and collide with the screen, where the traces of these collisions become recorded. It is crucial that we can only observe such traces as *effects* of the processes involving a certain type of physical objects (ultimately seen as quantum objects), the existence of which we infer on the basis of such traces. In other words, in each event we can only observe a mark, which we infer to be a trace left by a "collision" between a quantum object and the screen. Each such collision is similar in appearance to a very small object, idealized as a particle in classical physics, and in their outward appearance, both cases are similar. In this sense, such individual quantum phenomena may be associated with the particle-*like* behavior of quantum objects, which need not and in the present view does not mean that quantum objects are particles (any more than they are waves) in the sense of classical physics.

In the first setup, both slits open and we do not—or more significantly, in principle cannot—know which slit each particle passes through. In the second, we can—either in practice or, again, in principle—have such knowledge by installing devices, such as counters, which allow us to do so without appreciably disturbing the course of each individual run of the experiment (defined by an individual emission from the source). Such devices are sometimes called the "which-path" or "which-way" devices. We can also close one of the slits for each such run, which allows each object to go through one slit only. A given quantum object could of course also be blocked by the diaphragm, but these runs of the experiment are discounted. There are more or less equivalent experiments that

[2] Technically, one does not need the first diaphragm and can merely use the source itself to define the initial stage of the experiment. The arrangement described here is sometimes convenient, however, especially if one wants to relate the experiment to the uncertainty relations. Bohr uses this arrangement in most of his arguments (Bohr 1935b, p. 697; Bohr 1949, *PWNB* 2, pp. 45–46, Fig. 3, pp. 47–48, Fig. 4).

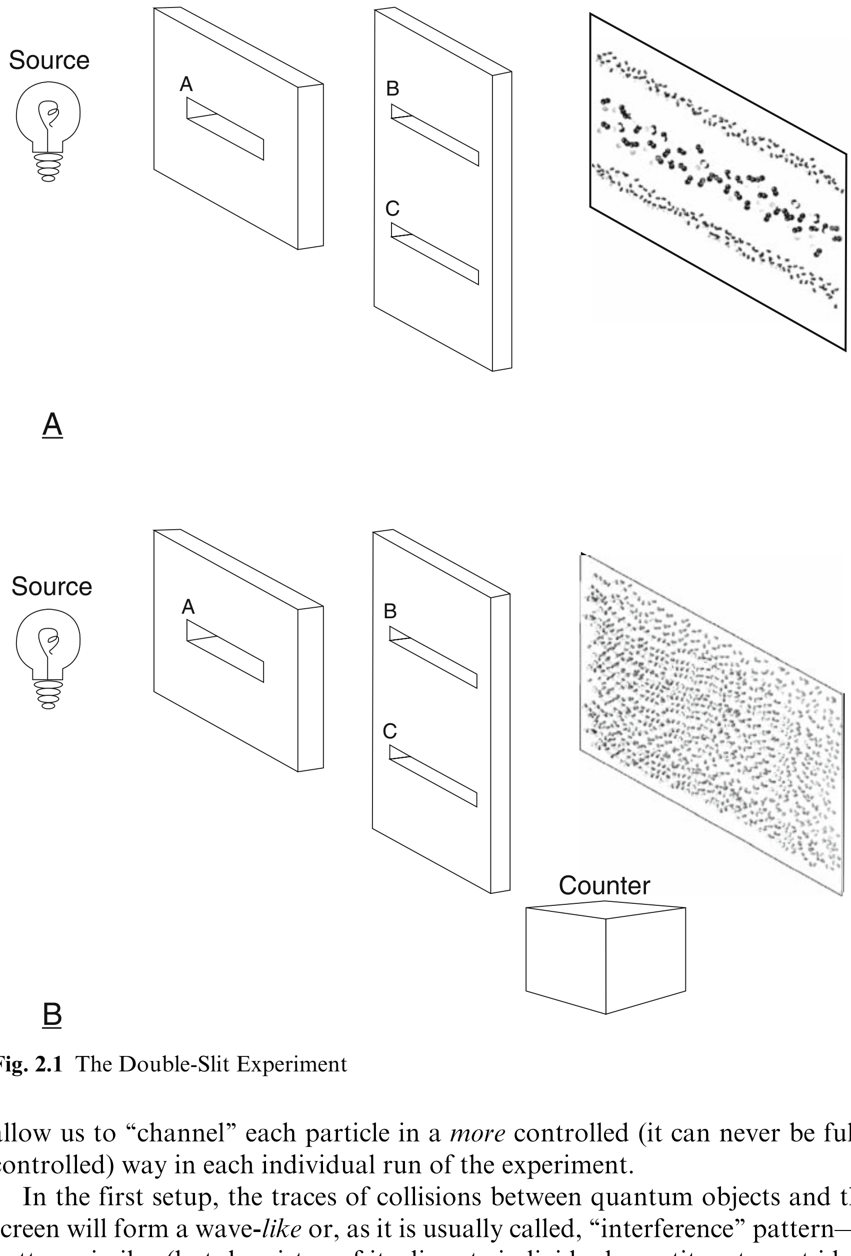

Fig. 2.1 The Double-Slit Experiment

allow us to "channel" each particle in a *more* controlled (it can never be fully controlled) way in each individual run of the experiment.

In the first setup, the traces of collisions between quantum objects and the screen will form a wave-*like* or, as it is usually called, "interference" pattern—a pattern similar (but, by virtue of its discrete individual constituents, not identical) to that produced by the traces of the wave processes in an appropriate medium (Fig. 2.1a). In principle (there could be practical limitations), the interference pattern will appear regardless of the distance between slits or the time interval between the emissions. This interval can be made sufficiently long for each emission to take place after the previously emitted object has reached the screen and been destroyed by its collision with the latter, which makes the appearance of the interference pattern especially remarkable and enigmatic. This interference pattern is the actual physical manifestation and, according to Bohr's and most interpretations, the *only* physical manifestation of quantum waves. Such wave-like effects are more pronounced and more suggestive of a physical wave propagation when we deal with very strong beams consisting of very large numbers of photons following one another in quick succession, which effects were at some point responsible for wave theories of light, culminating in Maxwell's electrodynamics. Indeed, the language of interference may not be rigorously suitable in this situation. Interference between what and what?[3] The "correlational pattern"—that is, something that refers to a correlated, ordered, rather than random, distribution of traces—appears to be a better term, and it is further justified by other correlational data characterizing quantum phenomena, such as that in the experiments of the EPR type.

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[3] Dirac contended that "each photon ... interferes only with itself. Interference between different photons never occurs" (Dirac 1958, p. 9). Dirac is correct on the second point, and he shows that making the assumption of interference between different photons "would contradict the conservation of energy" (p. 9). That interference between different photons does not occur is made clear by the fact that the interference pattern would emerge even if the interval between each emission were sufficiently large for the next photon to be emitted only after the previous photon has hit the screen and been destroyed by the collision. To assume interference between different photons under these circumstances would amount to rather radical assumptions concerning the collective behavior of photons. On the other hand, Dirac's statement that "each photon ... interferes only with itself" may not be sufficiently precise or sufficiently explained. It does, however, capture something essential about the situation. Part of the problem here is, again, the language of "interference," borrowed from classical wave physics. Dirac grounds his contention in the following point: "Some time before the discovery of quantum mechanics people realized that the connexion between light waves and photons must be of a statistical character. What they did not clearly realize, however, was that the wave function gives information about the probability of *one* photon being in a particular place and not the probable number of photons in that place" (p. 9). This is a profound statement which, apart from capturing the probabilistic nature of the wave function, suggests a Bayesian view of quantum probability adopted here, by linking the wave functions to the probabilities of *individual* events. The statement concerning each photon interfering only with itself could then be read as follows. (I am not sure whether Dirac says only this, rather than making further claims concerning the physical behavior of photons, but he appears to say at least this.) The probabilities encoded in the wave function enabling our predictions concerning where each photon will hit the screen correspond to the interference-pattern distribution of the traces left on the screen that will, inevitably, emerge in the corresponding setup. The probabilities are different in the alternative (no-interference-pattern) setup of the double-slit experiment and are differently predicted by quantum mechanics. Dirac also says that "[quantum mechanics] gets over the difficulty by making each photon go partly into each of the two components [of equal intensity]" into which a beam of light is split by a beam-splitter device (p. 9). As I shall argue below, that statement is difficult to sustain in this form, and Dirac does not appear to properly support it. While somewhat ambiguous, his overall analysis of the situation does not appear to necessarily imply that this statement is meant in a physical sense; rather, it concerns the linear superposition of quantum states defined by the wave function, which, again, deals with probabilities concerning the outcome of experiments (pp. 11–14). I shall explain these concepts below, and I shall revisit Dirac's argument in Chapter 6.

Now, in the second setup, when we install devices that allow us—either in practice or in principle—to know which slit each particle passes through (which can in principle be done without appreciably disturbing the course of each run of the experiment), the wave-like interference pattern never appears (Fig. 2.1b). Accordingly, in this setup quantum objects behave in a particle-like manner both individually *and* collectively, that is, the observed random pattern of collisions is similar to that which would appear if we conducted an analogous experiment with classical objects, idealized as particles. As I said, merely setting up the apparatus in a way that such knowledge could in principle be possible, even if not actually obtained, would suffice.

This strange behavior is sometimes referred to as the quantum measurement paradox, which is expressed by statements to the effect that, depending on how one staged the experiment, quantum objects change their "nature," or at least their behavior, from the particle-like to the wave-like one. It is worth reiterating that in each individual run of the experiment, the observed behavior or, better, phenomenon (a mark on the screen) is always particle-like; in the first setup just considered, the interference pattern only emerges out of multiple individual events (one needs about 70,000). This well-recognized fact does not prevent arguments to the effect that the unobserved, or even unobservable, behavior of individual quantum objects can be wave-like, specifically in the situation when the interference pattern or certain analogous phenomena appear. I shall address some among these arguments below, and shall only note at this point that there is no experiment that allows us to ever observe individual quantum objects as passing through both slits, and in this sense as behaving individually in the wave-like manner. The main point is that the totality of the phenomena observed in the double-slit experiment is incompatible with an explanation that classical physics (that of particle-like or wave-like classical objects) can provide. It might be added that classical physics also cannot properly predict the numerical data associated with these phenomena. Indeed, the situation appears to defy any possible explanation of how quantum objects behave in space and time, and possibly even the application of these latter concepts to this behavior. The behavior leading to the effects observed in the double-slit experiment cannot be exhibited by the same classical entities even in different circumstances; nor can we phenomenally conceive of entities that would be simultaneously particles and waves, or continuous and discontinuous, to begin with.[4] The classical objects exhibiting the different observed behaviors leading to the two incompatible kinds of observable effects are described by two rigorously different types of theories—by classical mechanics in the case of particle-like objects and by classical electrodynamics in the case of radiation. This is why

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[4] Even in Bohmian theories, where both concepts are used in describing the behavior of quantum objects, these concepts are not fused in a single entity: A wave accompanies or/and guides the particle in question, following de Broglie's idea, in turn inspired by Einstein's earlier suggestion, which was discarded by Einstein himself.

Planck's discovery that radiation can, under certain conditions, behave in the particle-like manner was such a shock.

The nonclassical epistemology of quantum phenomena and quantum mechanics responds to these difficulties, and, I argue, it does so in a logically and physically consistent way. Indeed, this epistemology owes most to Bohr's lifelong struggle, with a few defeats along the way, to develop, through complementarity, this kind of logically and physically consistent response to this "entirely new situation as regards the description of physical phenomena" (Bohr 1935b, p. 700). I would like now to use the key features of the double-slit experiment to illustrate how nonclassical thinking works in quantum physics, and reciprocally, to use this thinking to gain a better understanding of these features, and those of the delayed-choice experiment and the quantum eraser experiments, to be discussed below.

I shall begin by introducing two postulates that are motivated by these features and that are among my grounding assumptions in this study. While both of these postulates arise from and are motivated by experimental facts, they cannot in themselves be claimed to represent experimental facts. Nor, accordingly, can they be seen as physical laws, analogous, for example, to the uncertainty relations, although the experimental status of the latter is not straightforward either and requires careful consideration. However, these postulates are consistent both with the experimental data pertaining to quantum phenomena and with quantum mechanics as a physical theory accounting for these data in (probabilistically) predictive terms—and nature may not allow us to achieve more. These postulates can and here will be interpreted in nonclassical terms of suspending any possible description of quantum objects themselves and their behavior, and thus in accordance with Bohr's ultimate, nonclassical view of quantum phenomena, even though they were not expressly formulated by Bohr himself. The second postulate may be seen as related to the "quantum postulate," introduced by Bohr in the Como lecture as an interpretation of Planck's postulate concerning the discrete and, in Bohr's terms, individual character of radiation in certain circumstances. Both postulates could also be interpreted otherwise, including in ontological or realist terms, and in this sense they are more general. However, neither postulate, in whatever interpretation, would necessarily be acceptable to all physicists and philosophers working in quantum theory.

The first postulate may be called *the existence postulate* and is stated as follows:

> *Postulate 1.* There exist material physical systems, designated as "quantum objects," whose nature and behavior, as manifested in their impact upon our measuring instruments, cannot be described by means of classical physics. It is also assumed that the ultimate constituents of nature, "elementary particles," are quantum objects, although quantum objects can also be composite.

The postulate, thus, only asserts the existence of physical objects that cannot be treated by means of classical physics as concerns either the description or predictions of their nature and behavior. Accordingly, the postulate allows for a range of possible views concerning how far our theories can reach in approaching quantum objects and processes. If one adopts a nonclassical view, the postulate may be seen as *weakly* ontological, insofar as it postulates only *the existence* of quantum objects manifested in their capacity to have effects upon the world we observe, in particular in our measuring instruments. A nonclassical view does not imply or ultimately allow for any further claims concerning this existence; it deals only with what we can or cannot know concerning quantum *phenomena*, defined by the effects upon measuring instruments of the interactions between quantum objects and those instruments. In other words, there is no quantum-level *specifiable* ontology. On the other hand, the postulate allows one to ground a nonclassical interpretation of quantum phenomena and quantum mechanics in the specifiable classical and, hence, realist ontology of measuring instruments and the classical *epistemology* defined by this realist ontology—by what we *know* concerning the impact of quantum objects upon these instruments. It need not follow that classical objects are rigorously classical at the ultimate level of their constitution, since they may be—and generally (there are exceptions) are assumed to be—ultimately quantum.[5] A nonclassical view of the situation, too, would assume at the very least that measuring instruments have quantum strata, which enables their interaction with quantum objects. Only certain strata of measuring instruments may be described classically, and only in certain contexts; indeed, it appears they must be so described, since it does not appear possible for us to interact with quantum objects otherwise.

The second postulate, which may be called the *individuality* or *discreteness* postulate and which is close to Feynman's view (as well as to Bohr's), may also appear to be quantum-level ontological and is provisionally stated here in these terms for the sake of economy. However, it too need not be seen in this way, and it will be interpreted here in nonclassical terms.[6] The postulate is as follows:

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[5] Among the more intriguing alternatives is Anthony J. Leggett's argument for "macrorealism," an argument he advanced for over two decades (e.g., Leggett 1988). It can be summarized as follows: While quantum mechanics adequately describes the workings of nature at its ultimate micro-level, it may be incorrect at the macro-level. In other words, the question is whether quantum mechanics has a limited (micro)scale of application, rather than properly reflecting the constitution of all physical objects. Leggett designed clearly defined experiments for testing his proposal, although these experiments are difficult and as yet remain unperformed. This argument relates to a thorny and still unresolved problem of the transition from the quantum to the classical domain—a problem addressed with, in the present view, at most limited success by decoherence theories (e.g., Zurek 2003; Schlosshauer 2007), on which I shall comment in Chapter 10. More accurately, one should speak of the transition from the domain treated by quantum mechanics to that treated by classical physics, assuming, again, that the ultimate constitution of nature is quantum.

[6] It may be noted that, while Feynman states more unequivocally that "light behaves like particles," and not like waves, his actual interpretation is not that far from the one offered here, and his "*like* particles" already qualify his claim (Feynman 1985, p. 15).

> *Postulate 2. In certain specific respects*, quantum objects individually behave physically *like* particles, while they never *individually* behave physically like waves or other continuously propagating (spreading) objects.

In particular, keeping in mind the qualifications indicated by my emphasis and speaking provisionally of quantum objects themselves and their behavior, in the double-slit experiment a quantum object—say, a photon—never goes through both slits, regardless of the setup. Each photon passes through one and only one slit, whether we do not or cannot know which slit it has passed through (which leads to the emergence of the interference pattern, once the experiment is repeated a sufficient number of times) or whether we have, or can in principle have, such knowledge (which precludes the emergence of the interference pattern). It is sometimes argued to the contrary that this—that is, passing through both slits and, hence, behaving in a wave-like spreading manner—is what quantum objects do in the first setup, and I shall discuss some arguments of this type and the reasons why I believe them to be problematic below.

Now, even though I state more unequivocally that photons *never* individually behave physically like waves and make only a qualified appeal to photons' particle-*like* individual behavior, both claims require further qualifications. For one thing, they amount to at least a partial assessment concerning how quantum objects behave or (as will be seen, this difference is important here) at least *do not behave* apart from measurements, which is hazardous in quantum theory. These qualifications will be presupposed whenever I use—again, for the sake of economy—ontological language, for example, stating that a quantum object passes through only one slit and never both slits. Qualifications concerning the appeal to quantum waves, as *physical* waves, are common, although the idea is not dead, even beyond the Bohmian theories, in which quantum waves are given a physical meaning, along with and alongside particles. As I have stressed, however, qualifications that must be made concerning the particle-*like* behavior of quantum objects, while less common, are no less significant. First of all, as I said, we do not appear to be able—nobody has ever accomplished this thus far—to observe, through any instrument, the independent behavior, say, motion (particle-like or wave-like), of quantum objects. We can only observe certain trace-like effects of this behavior manifested in these instruments, and we infer the existence of quantum objects from these effects. The drawings in Fig. 2.1 aim to symbolically capture this in the style of Bohr's famous drawings, mentioned in the Introduction (Bohr 1949, *PWNB* 2, pp. 48–49, 54). The instruments are schematically represented as large and heavy, while the pictures of the screen are semi-realistic in style, symbolizing what one would actually see.[7] Such traces, such as those registered in cloud chambers, can of course be seen as registering temporal classical processes and can also filmed accordingly. In any event, however, no motions of quantum objects are ever observed, only

[7] Cf. famous pictures found in Tonomura et al. (1989) and displaying, in the title of their important paper itself, a "demonstration of single-electron buildup of an interference pattern."

the irreducibly amplified traces of their interaction with a medium such as a silver bromide screen. Quantum objects themselves are usually destroyed in the process of this "irreversible amplification" of their quantum interaction with measuring instruments to the classical level (Bohr 1949, *PWNB* 2, p. 51; Bohr 1958, *PWNB* 3, p. 3). Some of these effects, such as a trace on a silver bromide screen or a click in a detector, define, in Bohr's terms, *individual* quantum events or phenomena. These phenomena are always particle-*like* in this sense of the character of the individual traces, that is, they form contained, point-*like*, individual entities, which are discrete relative to each other.[8] This is why I speak of this postulate as the *individuality* or *discreteness* postulate.

Accordingly, in the present interpretation, the statement "a photon passed through a slit" only means that a measuring device registered an event that is *analogous* to a certain classical physical event, say, that of the hitting of a screen by a small classical object that passed through an opening in some diaphragm on its way. The statement "a photon never passes through both slits" means that no event corresponding to such a statement can be observed or registered. We can never register an individual event simultaneously linked to both slits, say, by placing a detector near each slit. Only one of these detectors registers each individual event: The two detectors never click simultaneously. This fact already poses considerable difficulties for the assumption that a photon can pass through both slits, and, as will be seen, these difficulties are amplified by other factors. On the other hand, one could speak of a single photon as "passing through a slit" in the sense that the corresponding event could have been registered by a measuring device (a "which-path" measuring device), if this device were installed, but only in this sense. The very difficulty of conceiving of the independent behavior of individual quantum objects is in part due to the apparent change in their behavior depending on the measuring arrangements, such as their "propensity" to fit into collective patterns in some, but only some, arrangements, such as the interference pattern in the double-slit experiment or analogous patterns in other experiments.

In sum, either type of characterization—particle-*like* (which can be both individual and collective) and wave-*like* (which is only collective)—only relates to the behavior of quantum objects as concerns the effects of this behavior upon measuring instruments, or phenomena in Bohr's sense, since we observe nothing else. Neither concept—that of "wave" or that of "particle"—applies as a physical concept to quantum objects and their behavior themselves. The individual phenomenal *effects* in question, for example, in the double-slit experiment, may be seen as *particle-like* insofar as they are *similar* to the kind of traces classical particle-like objects colliding with the screen would leave as well. One cannot, however, automatically infer from this similarity that quantum objects are particle-like objects of the type we deal with in classical physics. Quantum

[8] These traces are not really "points" either; they appear as discrete entities ("dots" on the screen in the double-slit experiment) only at a low resolution, and they actually comprise millions of atoms (Ulfbeck and Bohr 2001; Bohr et al. 2004).

objects certainly do not behave in the way particles do in classical physics (our primary model for the idea of particle), any more than in the way classical waves do. In particular, because of the uncertainty relations, a quantum object cannot be simultaneously assigned—as, at least ideally, in classical physics—both an exact position and an exact momentum, and hence a trajectory—the difficulty that became apparent early in the history of quantum theory.[9] In a nonclassical interpretation of the situation, we cannot ever assign even a single such property, any more than any other, to a quantum object, which view goes beyond the uncertainty relations but is obviously consistent with them. Nor can we apply to quantum objects classical physical concepts associated with these properties, such as those of motion, or even use words such as "happens" or "occurs." As both Bohr and Heisenberg argue, such words can apply only at the level of observation manifested in measuring instruments and not to what happens before an observation or between observations and hence not to quantum objects themselves (Heisenberg 1962, pp. 51–58).

Bohr, accordingly, sees the quantum mechanical situation as indicating "the ambiguity in ascribing customary physical attributes to atomic [quantum] objects" themselves or to their independent behavior, as against phenomena in his sense, something that is actually observed or registered (Bohr 1949, *PWNB* 2, p. 51). This perspective allows him to give a consistent view of the double-slit experiment and related experiments in terms of the complementary nature of the phenomena in question there ("complementarity in the narrow

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[9] When elementary particles are considered in quantum theory, even when assigned a mass, they are idealized as zero-dimensional, point-like objects (or possibly one-dimensional strings). Such objects can be given a rigorous mathematical meaning but not a rigorous physical meaning. It is true that this type of point-like (mathematical) idealization of physical objects is also used in classical physics. There, however, this idealization allows one to approximate the actual behavior of the objects considered and, on the basis of this descriptive approximation, to make excellent predictions concerning this actual behavior. The physical objects themselves thus considered may be, and usually are, assumed to have extension, the property that has defined physical objects or, more generally, material bodies (*res extensa*) at least since Descartes. In the case of quantum objects, such an assumption is difficult to sustain. For example, in the case of electrons it leads to well-known contradictions with classical electrodynamics, since, if assumed to have extension, an electron would be torn apart by its negative charge. It is this circumstance that led to the idealization of the electron even before quantum mechanics. Accordingly, the nature of the point-like idealization of elementary quantum objects is different in classical and quantum physics. Quantum physics gives reasons for and logic to a still more radical idealization found in nonclassical interpretations of quantum objects, which keeps us from idealizing them or their behavior on any conceivable model—physical, mathematical, or other. Importantly, this view applies to composite quantum objects as well, including those that reach the level of macro-objects, such as the "squids," or even more interestingly, carbon 60 fullerenes, which can be observed either as classical or as quantum objects (Arndt et al. 1999). That is, we can also observe such objects, as concerns their macro-aspects and behavior, as classical macro-objects, which we cannot do with the elementary quantum objects, such as electrons and photons. On the other hand, just as in the case of these elementary constituents themselves, the constitution of such quantum macro-objects as quantum is beyond our capacity to observe and only manifest in their effects upon our measuring instruments.

sense"). He writes, "To my mind, there is no other alternative than to admit that, in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena we want to study" (ibid.). The interference pattern itself only reflects the correlationally ordered distribution of the traces left by photons in the first setup, as opposed to a different distribution of such traces found in the second setup. The situation is correlative to the uncertainty relations, and both reflect the irreducible randomness of the outcome of quantum experiments. In other words, we are, again, dealing not with properties of quantum objects but with two different and mutually exclusive types of (individual) observable or registered effects upon measuring instruments of the interaction between quantum objects and those instruments under particular, rigorously specified physical conditions. By the same token, it is *not our knowledge of the behavior of quantum objects* but *our knowledge, actual or in principle possible, concerning classical physical events registered in measuring instruments* that defines the absence or the appearance of the interference pattern in the double-slit experiment. This view, I shall argue, is supported and amplified by other paradigmatic quantum experiments, such as the delayed-choice and the quantum eraser experiments, to be discussed in this chapter, or the EPR experiment, to be discussed in Chapters 8 and 9. First, however, I would like to consider the relationships among the double-slit experiment, the uncertainty relations, and the unavoidably probabilistic character of our predictions concerning the outcomes of quantum experiments.

2.2 The Double-Slit Experiment, the Uncertainty Relations, and Probability

As I said, the uncertainty relations, $\Delta q \Delta p \cong h$, reflect the insuperable limits on the simultaneous determination, either in measurement or in prediction, of both the position and the momentum (or certain other pairs of variables, such as time and energy) associated with a quantum object.[10] It is an experimental fact that both quantities can never be measured simultaneously beyond the limits of accuracy defined by h, regardless of the precision of our instruments. As I shall explain presently, the uncertainty relations would apply even in the case of ideal instruments, whose capacity far exceeds that of any instruments we could in principle have. Given that we do not have such instruments, this means the limitations expressed by the uncertainty relations are not a matter of the accuracy of our measuring instruments, which type of limitation we also encounter in classical

[10] As noted in the Introduction, technically, we measure the momentum in a given direction, and the uncertainty relations apply to this momentum and the corresponding coordinate. In the uncertainty relations for the position and the momentum associated with a quantum object in the three-dimensional space, each quantity will have three components defined by the chosen coordinate system.

physics. According to Bohr's interpretation of the uncertainty relations themselves, the two quantities not only can never be measured simultaneously they also cannot be assigned or even properly defined simultaneously. One can also put it as follows, given that, as I stressed from the outset, in physics we always deal with idealized mathematical models. Classical physics is grounded in mathematical models in which both the position and the momentum of a given object can simultaneously be assigned definite values, thus also assuring the causality of the behavior of classical objects (thus idealized). The uncertainty relations tell us that such models might no longer be possible in dealing with quantum objects, and they are strictly precluded in a nonclassical interpretation of quantum phenomena. Indeed, in the latter case, even a single such quantity (or any quantity) cannot be assigned to a quantum object, although it (but, again, never simultaneously both quantities together) can be assigned to a certain part of the measuring apparatus involved. By the same token, we can only obtain probabilistic predictions concerning *primitive* (indecomposable) individual quantum processes and events, as opposed to classical physics where, when probabilities are involved, they concern the outcomes of collective or otherwise non-simple processes and events.

The physical or experimental meaning of the formula itself is a subtle matter. Here and throughout the study, I adopt the following view, courtesy of Asher Peres, which is consistent with nonclassical epistemology:

> The only correct interpretation of [an uncertainty relation $\Delta x \Delta p \cong h$, where x is a coordinate and p the momentum in the same direction] is the following: If the *same* preparation procedure is repeated many times, and is followed either by a measurement of x, or by a measurement of p, the various results obtained for x and for p have standard deviations, Δx and Δp, whose product cannot be less than [h]. There never is any question here that a measurement of x "disturbs" the value of p and vice-versa, as sometimes claimed. These measurements are indeed incompatible, but they are performed on *different* [*quantum objects*] (all of which were identically prepared) and therefore these measurements cannot disturb each other in any way. An uncertainty relation [$\Delta x \Delta p \cong h$] [or the corresponding representation in the formalism of quantum mechanics] only reflects the intrinsic randomness of the outcomes of quantum tests. (Peres 1993, p. 93)

Not everyone would subscribe to Peres's claim that this is "the only correct interpretation" or to this interpretation itself. For my purposes in this study, it suffices that this interpretation is consistent with the experimental evidence in question. As Peres also observes, consistently with the view expressed above, "an uncertainty relation [...] is not a statement about the accuracy of our measuring instruments. On the contrary, its derivation assumes the existence of *perfect* instruments (the experimental errors due to common laboratory hardware are usually much larger than quantum uncertainty)" (Peres 1993, p. 93). Bohr corroborates this view in a striking sentence that also brings in the role of measuring instruments, which define *quantum phenomena*, as different from quantum *objects*. He says, "[I]n this context, we are of course not concerned with a restriction as to the accuracy of measurements, but with a limitation of the well-defined application of space–time concepts and dynamical conservation laws, entailed by the necessary distinction between measuring instruments and atomic objects" (Bohr 1958, *PWNB* 3, p. 5, also Bohr 1937, *PWNB* 4, p. 86; Bohr 1954, *PWNB* 2, pp. 72–73).

This situation (and accordingly the uncertainty relations themselves) is properly reflected in the mathematical formalism of quantum mechanics, and for Bohr is reciprocally a physical interpretation of both. As Heisenberg explains in his uncertainty relations paper

> One can . . . say that associated with every quantum-theoretical quantity or matrix is a number which gives its "value" within a certain definite statistical error. The statistical error depends on the coordinate system. For every quantum-theoretical quantity there exists a coordinate system in which the statistical error for this quantity is zero. Therefore a definite experiment can never give exact information on all quantum-theoretical quantities. Rather, it divides physical quantities into "known" and "unknown" (or more or less accurately known quantities) in a way characteristic of the experiment in question. The results of two experiments can be derived exactly one from the other only then when the two experiments divide the physical quantities in the same way into "known" and "unknown". . .. When two experiments use different divisions into "known" and "unknown," then their results can be related only statistically. (Heisenberg 1927, p. 70)

It may be noted, however, that—their physical, philosophical, and historical significance notwithstanding—the uncertainty relations are, to some degree, a remnant of classical physics. Feynman argues as follows: "I would like to put the uncertainty principle in its historical place: When the revolutionary ideas of quantum physics were first coming out, people still tried to understand them in terms of old-fashioned ideas (such as, light goes in straight lines). But at a certain point the old-fashioned ideas would begin to fail, so a warning was developed that said, in effect, 'Your old-fashioned ideas are no damn good when . . .' If you get rid of all the old-fashioned ideas and instead use the ideas that I'm expounding in these lectures—adding *arrows* for all the ways an event can happen—there is no need for an uncertainty principle!" (Feynman 1985, pp. 55, n. 3). This procedure amounts to estimating probabilities of the outcomes of certain experiments via the so-called "amplitudes," which I shall explain below. The situation may be more complex than Feynman makes apparent, but the statement conveys an important point, and perhaps nothing reflects it more strongly than the impossibility of ever assigning, as sharply defined, both quantities involved to the same quantum object.

As Feynman also notes on the same occasion and explains in detail elsewhere (Feynman et al. 1977, vol. 3, pp. 1–11), the situation is equivalent to the one that is obtained in the double-slit experiment. For the emergence of the interference pattern may be properly correlated with the possibility of the (ideally) precise *momentum* measurement for each quantum object involved in the corresponding setup, while the lack of the interference pattern is properly correlated with the possibility of the (ideally) precise *position* measurement for each quantum object in the alternative setup. However, the two outcomes are mutually exclusive or complementary, and both quantities cannot be measured at once—although one must be careful in applying Bohr's concepts to collective, rather than individual, phenomena. As I noted above, the same type of argumentation is used by Bohr throughout his exchanges with Einstein in order to counterargue Einstein's criticism of quantum mechanics (e.g., Bohr 1935b, pp. 697–700; Bohr 1949, *PWNB* 2, pp. 43–47, 52–61). By the same token, the situation is equivalent to the probabilistic and (statistically) correlational nature of our quantum predictions, with correlations manifested in the interference-pattern setup of the double-slit experiment—although that pattern and, hence, the corresponding correlational order, are, again, formed by an accumulation of *random* individual events. The "history" of any single event as such can never be certain, and no single run of the experiment is ever guaranteed to be repeatable. A single event registered by a counter cannot be used to establish unconditionally that an object passed through a slit, any more than can any given trace on the screen. As I said, these circumstances reflect one of the greatest mysteries of quantum phenomena, perhaps their greatest ultimate mystery: How events that are irreducibly random can, under certain circumstances, give rise to order, even if only a correlational order. Thus, the double-slit experiment and the uncertainty relations both, and correlatively, reflect the probabilistic and correlational order found in quantum phenomena, and hence the enigmatic relationships between chance and probability in quantum physics.

To further illustrate this aspect of the quantum situation, I would like to use, following Anthony J. Leggett, an experiment taken from the field of quantum interferometry, which, while different physically, is epistemologically equivalent to the double-slit experiment (Leggett 1988). Using this experiment also illustrates the pervasiveness of the features found in the double-slit experiment elsewhere in quantum experiments, in a certain sense in all quantum experiments. In this experiment, we consider the initial state A, two possible intermediate states B and C, and then a final state E, parallel, respectively, to a particle passing through the slit in the first diaphragm, one of the slits in the second, and, finally, hitting the screen in the double-slit experiment (Fig. 2.1). I, again, provisionally speak, as Leggett does, in terms of particles, while keeping in mind the qualifications indicated earlier, which apply here as well. In contrast to the double-slit experiment, however, where some quantum objects hit the diaphragm and do not pass through the slits, each quantum object involved in this experiment always passes through one of the two possible channels. This fact allows one to take into account each individual run of the experiment, since we need not discount those objects that collide with the diaphragm in the double-slit experiment and do not pass through the slit. First, we arrange to block the path via state C, but leave the path via state B open. (Unlike in the double-slit experiment, in this case we do not install any additional devices to check directly whether the object has in fact passed through state B.) In a large number of trials (say, again, a million), we record the number of objects or, in Leggett's language, micro-systems, reaching state E. Then we repeat the same number of runs of the experiment, this time blocking the path via B and leaving the path via C open. Finally, we repeat the experiment again with the same number of runs, now with both paths open. In Leggett's words, "the striking feature of the experimentally observed results is, of course, summarized in the statement [that] ... the number reaching *E* via 'either *B* or *C*' appears to be unequal to the sum of the numbers reaching *E* 'via *B*' or 'via *C*' " (Leggett 1988, p. 940).

The situation is, thus, equivalent to the emergence of the interference pattern when both slits are open in the double-slit experiment. In the absence of any means of establishing through which slit each particle passes or, again, could even in principle have passed, or in any situation in which the interference pattern is found, one cannot assign probabilities to the two alternative "histories" of an object passing through either B or C on its way to the screen. If we do, the above probability sum rule would not be obeyed and the conflict with the interference pattern will inevitably emerge, as Bohr, again, stressed on many occasions (Bohr 1949, *PWNB* 2, pp. 46–47; Bohr 1935b, pp. 697–700). The fact, discovered already by Planck, that the counting of probabilities is different in classical and quantum physics is, again, due to the same type of behavior of quantum systems. One can also put it as follows. In calculating the probabilities of the outcomes of such experiments, we must, as it were, take into account the possibility of an object passing through both states B and C (or through both slits in the double-slit experiment) when both are open to it. As discussed above, however, it is difficult to assume that such an event physically occurs for any single quantum object, since there have been no experiments performed so far that would allow us to make such an assumption, while any attempt to establish which slit a given object passes through will inevitably disallow an emergence of an interference pattern. Leggett concludes his analysis as follows:

> In the light of this result, it is difficult to avoid the conclusion that each microsystem in some sense *samples both* intermediate states *B* and *C*. (The only obvious alternative would be to postulate that the ensemble as a whole possesses properties in this respect that are not possessed by its individual members—a postulate which would seem to require a radical revision of assumptions we are accustomed to regard as basic.) ...
>
> On the other hand, it is perfectly possible to set up a "measurement apparatus" to detect which of the intermediate states (*B* or *C*) any particular microsystem passed through. If we do so, then as we know we will always find a definite result, i.e., each particular microsystem is found to have passed *either B or C*; we never find both possibilities simultaneously represented. (Needless to say, under these [different] physical conditions we no longer see any interference between the two processes.) ... (Clearly, we can read off the result of the measurement only when it has been amplified to a macroscopic level, e.g., in the form of a pointer position.) (Leggett 1988, pp. 940–941; emphasis on "samples" added)

It is worth reiterating that we find a certain statistical distribution in either case, since we always register different outcomes, manifested in the distribution of traces on the screen in either setup of the double-slit experiment. The main point for the moment, however, is that correlational patterns are found only in one type of setup, those defined by the impossibility of knowing the "path" of the object, and not in the other, in which this knowledge is possible.

This "behavior" is indeed as remarkable as Leggett finds it to be, and it is difficult to conclude otherwise. It is all the more remarkable given the fact, noted earlier, that the interval between emissions could be made large enough for the preceding quantum object to be destroyed before the next one is emitted, without affecting the probability counting or the appearance or disappearance of the interference pattern in a given setup. Other standard locutions include strange, puzzling, mysterious (and sometimes mystical), and incomprehensible, for reasons that Leggett's statement makes apparent. The first possibility, here indicated, corresponds to a more familiar question asked in the case of the double-slit experiment, given that if one speaks in terms of quantum objects themselves, in the interference picture the behavior of each appears to be "influenced" by the location of the slits. How do particles "know," individually or (which may indeed be even more disconcerting) collectively, that both slits are open and no counters are installed or, conversely, that counters are installed to check which slits particles pass through and modify their behavior accordingly? Indeed, Einstein in his first paper on photo-effect and light quanta already spoke of optical wave observations of light as referring to [statistical] time averages and not "instantaneous values" (Einstein 1905, p. 132). He apparently also approved Johannes Stark's observation, made in 1909, that light quanta statistically add together, "conspire," as it were, to lead to the interference phenomena of light, and while overly anthropomorphic, the idea of the *conspiracy* of photons is not surprising.[11] The alternative proposed by Leggett would be as remarkable as any "explanation" of the mysterious behavior of quantum objects. Attempts to conceive of this behavior in terms of physical attributes of quantum objects themselves appear to lead to unacceptable or at least highly problematic consequences. Among such consequences are logical contradictions; incompatibility with one aspect of experimental evidence or the other; a bizarre behavior of quantum objects based on difficult assumptions, such as attributing volition or personification to nature in allowing these objects individual or collective "choices," like the one proposed by Leggett; or the nonlocality of the situation, in the sense of its incompatible with relativity.[12]

[11] The comment is reported in Mehra and Rechenberg's *The Historical Development of Quantum Theory* (*MR* 6, p. 43).

[12] Yet another possibility to explain the situation would be a retroaction in time, which is hardly less problematic, although it is not inconceivable and is entertained by some (cf. Stapp (1997) and, for counterarguments, Mermin (1998b) and Shimony and Stein (2001)). As will be seen below, a retroaction in time also follows from the assumption that a quantum object can pass through both slits in the delayed-choice experiment. It is true that the possibility of retroaction in time is a mathematical consequence of general relativity, that is, a possible solution of its equations, as was demonstrated by Gödel (Gödel 1949). There are also arguments concerning the "wormholes" in general-relativistic physics that draw this implication. As things stand now, however, very few would accept retroaction at time as physically possible, given both the logical consequences involved, such as that of potentially changing the past, and the limits of the theories involved, such as the fact that general relativity is incompatible with quantum theory (there is, again, no quantum gravity as yet).

On the other hand, one can consistently account for the situation by means of a nonclassical argumentation, such as that of Bohr, or even by means of Bohr's proto-nonclassical argument, to be discussed in Chapter 7, according to which one could still attribute one of the two complementarity quantities to a given quantum object itself at the time of measurement. Bohr's logic is grounded in the fact that these two setups and the two types of phenomena occurring in the double-slit or related experiments are always mutually exclusive. Bohr was, arguably, the first to take advantage of this fact. It allowed him to contend that the features of quantum phenomena exhibited in the double-slit experiments or other key quantum experiments need not be seen as paradoxical. As he says, "[I]t is only the circumstance that we are presented with a choice of *either* tracing the path of a particle *or* observing interference effects, which allows us to escape from the paradoxical necessity of concluding that the behavior 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" (Bohr 1949, *PWNB* 2, p. 46). Our tracing of the path of any quantum object could, again, only amount to (by classical standards) incomplete and indirect information, and indeed "tracing the path" (not the best expression here) only means that we can know which slits the particle has passed through, but this information is sufficient to avoid the paradoxes in question.[13]

Quantum mechanics predicts the probabilities in question in exact correspondence with experiment. It manages to do so in the following ingenious and hitherto unprecedented way, and we might indeed be lucky that nature allows us to do so. Added ad hoc, the procedure is not derived from the rest of the quantum mechanical formalism; it is justified only by the fact that it works, spectacularly well, in getting the right experimental predictions. In calculating the probabilities for the outcome of a certain event, such as that of a photon hitting the screen (in a given area), quantum mechanics first assigns to such an event—via the wave function, or the ψ-function, as it was called by Schrödinger—what is called "probability amplitude" or just "amplitude." (I shall explain the historical reasons for this terminology in Chapter 3.) However, this ψ-function is a complex-valued function, that is to say, the application of such a function generally yields a complex, rather than real, number quantity. A complex number is a number of the form $x + yi$, where x and y are real numbers and i is the imaginary unit equal to the square root of -1. Probabilities, on the other hand, are real numbers greater than 0 and less than or equal to 1. In quantum mechanics, the probability of an event is derived via the absolute square of the amplitude (technically, via the so-called square modulus $x^2 + y^2$ of a complex quantity), written as $|\psi|^2$, which is always a positive real number. The quantum mechanical formalism allows us to adjust the wave function so as to make the final outcome of the procedure a positive real number that is less

[13] See Busch and Shilladay (2006) for an illuminating discussion of the relationships between uncertainty relations and complementarity.

than 1, just as probabilities are. The procedure involves further technical details and complexities (the wave function is a multi-dimensional entity and in most cases considered in this study indeed an infinite-dimensional one, and we need to integrate $|\psi|^2$ to get the probability), glossed over by my summary here and to be addressed later in this study. But these complexities do not affect the main point here. It is not the wave function itself (again, generally a complex-valued function) but its square modulus that yields a probability for the outcome of the experiment we want to predict. The procedure allows us to properly assess the probabilities involved in the experiments considered here, such as those in the interference-pattern setup in the double-slit experiments, when two paths are open to a photon (again, keeping in mind the qualifications given above), or in the corresponding setup of the experiment described by Leggett for neutrons. In these cases, we do not add (as we conventionally do in classical physics or elsewhere) the probabilities for the two alternatives—probabilities that can be established on the basis of the alternative setup of the experiment. If we did, our predictions would be incorrect, which is Leggett's main point. Instead, we add the corresponding amplitudes, ψ_1 and ψ_2, then derive the probability by squaring the modulus of the sum, $|\psi_1 + \psi_2|^2$, according to the rule just described. The two "states," usually designated $|\psi_1>$ and $|\psi_2>$, are considered to be in (linear) "superposition"—the concept of "state" requiring further qualifications, to be given later, since it has complex relations to the physical state of a quantum object.[14]

That quantum mechanics works so well in predicting the outcomes of quantum experiments does not, again, mean that alternative accounts of quantum phenomena (either mathematically equivalent but perhaps less artificial, or not) are not possible, although descriptive accounts appear difficult to achieve—difficult, but again not in principle impossible. Be that as it may, the nonclassical epistemology of quantum phenomena and of quantum mechanics responds both to the conceptual difficulties of the situation and to the fact that quantum mechanics properly predicts all of its numerical aspects. It does so by suspending or even forbidding, in principle, the possibility of knowing how such or any other effects of quantum objects upon our (classical) world are possible; or, correlatively, the possibility of knowing what actually happens to quantum objects between the experiments. More radically, it precludes any knowledge or even conceptualization of quantum objects and processes. In this respect, quantum mechanics may be even more incomplete than Einstein argues—although, as will be seen, eventually he comes close to making this point with a very different evaluation of it and, again, a hope that a better theory might eventually be possible. It is not only a matter of obtaining at most partial information concerning quantum objects and their behavior—for example, knowing only the position and not the momentum of a given quantum object

[14] See again Feynman's accounts of the situation in Feynman (1951) and Feynman et al. (1977, vol. 3, pp. 1–11).

at a given point, in accordance with the uncertainty relations (if one applies them to quantum objects, rather than to measuring instruments impacted by quantum objects). There is no knowledge of any kind available—and ultimately no conceptualization (beginning with conceiving of them as quantum objects or objects in any specific form) possible—concerning quantum objects and their behavior at all. There is only knowledge, essentially predictive and probabilistic in nature, concerning the effects of quantum objects upon measuring instruments or other macro-objects in the (classical) world we observe, conceptualize, and know. Thus, nonclassical epistemology does not resolve the great enigma and mysteries of quantum physics: What are quantum objects and how do they behave? How are the observable features of quantum phenomena possible? For example, how can the irreducibly random character of individual events coexist with the correlational patterns at the collective level found in certain experimental setups? The significance of nonclassical epistemology is that it tells us that this enigma may be irresolvable: Perhaps no explanation or spatio-temporal conceptualization of the behavior of quantum objects will ever be possible.

2.3 The Delayed-Choice Experiment

Following Wheeler's and most other discussions of the delayed-choice experiment, I shall, for the sake of convenience, use photons in considering it, and I shall do the same in my discussion of the quantum eraser experiment in the next section. As in the double-slit experiment, however, other quantum objects could be used just as well. Also, whenever I use (again, for the sake of convenience and economy) ontological language in referring to the behavior of quantum objects, such as photons, rather than to the events observed in measuring instruments, my earlier qualifications concerning such use should be kept in mind.

We may, in the double-slit experiment, set up our equipment beforehand in either way—to enable or to disable the appearance of the interference pattern—by switching off or on the counters, installed between the diaphragm with the slit and the screen, in the two corresponding runs of the experiment. We can, by means of suitable devices, also establish the possibility of knowing which slit each photon passes through even before each photon reaches the diaphragm and thus guarantee the absence of the interference pattern, as we do in quantum eraser experiments. If, however, we place the detectors between the diaphragm and the screen (as in Fig. 2.1b), we can decide to switch the detectors on in each run of the experiment after the photon has passed through the slit and is on its way to the screen. Making our decision at this later point does not change the outcomes of the two respective sets of runs of the experiment, corresponding to each setup, provided that the detectors are sufficiently far from the diaphragm for us to have time to do so before the photon hits the screen. This modified setup converts the double-slit experiment into Wheeler's delayed choice experiment, defined by this *delayed* decision on our part as concerns how we set up the detectors and, thereby, determine the outcome of the experiment. The experiment becomes especially dramatic when considered on the cosmological scale, at which we can, at least in principle, perform it by using photons emitted by a quasar and an intervening galaxy, both billions of light-years away from us (Wheeler 1983, pp. 190–192).[15] Split and focused by the galaxy, the light from the quasar will, in principle, display an interference pattern on a screen that we can set up. If, however, we install—billions of years after the photons in question have passed the galaxy!—a detector that allows us to determine the "path" taken by each photon, the interference pattern will no longer appear.

It is immediately evident that the assumption—apparently made by, among others, Wheeler in his analysis of the delayed-choice experiment and countered by Postulate 2 (again, keeping in mind the above qualifications concerning it)—that a single photon ever passes through both slits poses major difficulties. The *first* is, again, that no event confirming or even justifying this assumption has been registered in any experiment thus far; the assumption is only justified (by those who make it) as referring to an event that cannot be observed. The *second* difficulty, also noted above, is that the assumption appears to lead to spatial nonlocality in the sense of the conflict with relativity, if the diaphragms are sufficiently close to each other and the slits in the second diaphragm are sufficiently far apart. Since the interference pattern would still emerge under these conditions as well, under this assumption each photon would have to spread faster than c to pass through both slits, which relativity forbids.[16] In the case of the delayed-choice experiment on the cosmological scale, the assumption entails a highly implausible event of the (wave-like) spreading of a photon across a galaxy. Finally, the *third* difficulty is the following. By switching the counters on or off after a photon passes through the slits but before it reaches the screen, we can define the *past* event in two mutually exclusive ways—as that of the photon passing through one of the slits (as a particle would) or that of the photon passing through both slits (as a wave would). The assumption would, thus, entail a radical temporal nonlocality. This is a highly undesirable and even unacceptable feature, at least in the present view and in most views of quantum mechanics or physics in general, although it is, as I said, admitted by and even argued for by some. However, this difficulty (like the two others just mentioned) is avoided by the assumption, adopted here and expressed in Postulate 2, that a photon always passes through one slit and one slit only. The only physical manifestation (such as that by means of the interference pattern in the corresponding setup of the double-slit experiment) is created by many photons *sequentially* each passing through either one or the other slit and not by physical

[15] Wheeler actually uses the beam-splitter experiment, but it does not affect the argument given here (Wheeler 1983, p. 183).

[16] Again, I leave aside Bohmian theories, to which my argument does not apply but which are manifestly nonlocal in any event.

waves conjectured on the basis of the assumption that a single photon can pass through both slits. As I have stressed, the appearance or lack of the interference pattern is, in this view, only due to our knowledge of what did or did not happen in the past, acquired (no matter at what point!) before the photons involved in the experiments hit the screen, without, accordingly, implying any retroactive determination of actual physical events.

At the same time, no claim concerning the actual behavior of photons is made. At least, *no positive claim* is made as to what actually happens apart from measurement, since we do not know which slit each photon passed through or, to begin with, in what way it "moves" (to the degree, again, that the latter concept applies to photons). This is an important qualification. Consider, for instance, Heisenberg's elaboration in his analysis of the double-slit experiment, which might appear to allow for the possibility that an individual photon can, in principle, go through both slits in the absence of measuring devices that would allow us to determine which slit it passes through (the interference-pattern setup). This view is sometimes attributed to Heisenberg (or to Bohr), in my view incorrectly. According to Heisenberg, "Therefore the statement that any light quantum must have gone *either* through the first *or* the second [slit] [in the interference-pattern set-up] 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 'happen' is restricted to the observation" (Heisenberg 1962, p. 52). Indeed, as noted earlier, it is the very nature of quantum phenomena that "does not allow a description of what happens between two observations; this must mean that the term 'happen' is restricted to the observation." As Bohr argued throughout his writing, in part in responding to Einstein's criticism of quantum mechanics, the difficulty or impossibility of carrying our analysis beyond a certain point, encoded in the uncertainty relations, appears to be defined by nature, at least as things stand now, and does not merely reflect the limitation of quantum mechanics, as Einstein appears to have thought. At least, in Bohr's view, Einstein did not demonstrate otherwise in his arguments, such as those to be discussed in Chapters 8, 9, and 10. Accordingly, quantum mechanics responds to these experimentally defined circumstances as well as possible, and nonclassical interpretations, such as that of Bohr, give a consistent physical and epistemological account of both quantum phenomena and quantum mechanics.

Heisenberg's second sentence is clearly in accord with this view. Would not, however, his first sentence—"Therefore the statement that any light quantum must have gone *either* through the first *or* the second [slit] [in the interference-pattern set-up] is problematic and leads to contradictions"—be in conflict with Postulate 2 and the present argument? Would not this statement contradict the claim that a photon always goes through only either one slit or the other, and never through both even in the interference-pattern set-up? Not necessarily, in my view. I would not deny that one might in principle read *this sentence* in this way. However, as Heisenberg's overall argument on this occasion and his discussions of quantum mechanics elsewhere suggest, the statement could also be read, consistently with the present analysis, as reflecting that it is in principle impossible, in the interference-pattern set, to ever ascertain which of the two slits (either the first or the second) a photon went through. If such a conclusion—pertaining to each photon involved—is possible, the interference pattern will not appear. In fact, as will be discussed below, in the delayed-choice version of the quantum eraser experiment, we could, in principle, separate the multiple photons involved and the corresponding traces on the screen into two groups, so as to be able to make such a conclusion (again, even in principle) or not to be able to do so. Then, the traces left on the screen by the photons from the second group will form an interference pattern, and those from the first will not. Thus, while it appears that we can never say what *actually happens* between experiments, we can say with reasonable certainty (as much as physics allows us) that certain things *cannot happen,* if the assumption that they do lead to consequences incompatible with the established experimental data. The assumption that a photon can pass through both slits would be in conflict with the locality requirements (spatial or temporal) imposed by relativity, thus far well confirmed by experiment.

However, the assumption that a single photon can go through both slits (no matter how wide apart they are) is not uncommon, and those who hold this view are in rather distinguished company. This company includes Einstein (at least at some point of his exchanges with Bohr), possibly Dirac, Wheeler (Wheeler 1993, p. 183), and, to give an example of a popular exposition, Brian Greene in his book *The Fabric of the Cosmos* (Greene 2004, pp. 176–204). In the case of Einstein, this view served his criticism of quantum theory, and this criticism would indeed be justified were this assumption necessary. By contrast, both Wheeler and Greene embrace quantum theory and the strangeness of quantum phenomena, which they appear to see as amplified by this assumption. The assumption leads Wheeler and, following him, Greene to speak of the participatory universe, in which even the past or, at least, the actualization of the past is defined by our subsequent participation in the observation and measurement process. In Wheeler's cosmological-scale version of the delayed-choice experiment, this actualization can take place literally millions of years and, in principle, arbitrarily long after the event. That Wheeler subscribes to the idea that a photon can pass through both slits (or both paths open to it in other quantum experiments) is especially intriguing because he is among the stronger advocates of Bohr's views, which appear to be in conflict with this idea, at least in the present reading of Bohr. In any event, I do not believe that statements to the contrary ever occur in Bohr's works, while statements at least suggesting the opposite are found throughout Bohr's writings (e.g., Bohr 1949, *PWNB* 2, pp. 46–47; Bohr 1935b, pp. 697–700).

In fairness, both Wheeler and Greene do assume that the past is physically fixed in the case of such quantum events as well and that the paradox arises only because our conventional ideas concerning temporality are not applicable at the quantum level. While they might be right on this last point (and the question of time in quantum mechanics is complicated on several grounds), it does not appear to me that their conception of the past as an array of future possibilities is workable or in any event is sufficiently developed by them. Neither Wheeler nor Greene—nor, to my knowledge, others who subscribe to the assumption that a single photon can pass through two slits or analogous assumptions related in other experiments—manage to find a satisfactory way of making such assumptions work. Could one assume that each photon is a wave-like object that always goes through both slits, if differently in different circumstances? Even apart from the difficulties of explaining the particle-like aspects of the behavior of quantum objects when they are registered by detectors or in other circumstances, this, again, does not solve the problem of affecting the past by the subsequent action. For the act of switching the detectors on or off would still change the way a single photon had propagated, as a wave, through the slits. Of course, if one believes that the past could be affected by the present, then the assumption that a photon can pass through both slits may be acceptable, even though such an event can never be observed.

We can make better sense of the situation by assuming that each photon passes through one and only one slit, while establishing that at any point before or after this passage, the *possibility* of knowing which slit it passes through destroys the possibility of the appearance of the interference pattern. It should be reiterated, however, that, in the present view, one could speak of the "fact" of a photon passing through a slit only in the sense that the corresponding phenomenal event, a "click," could, in principle, be registered by a measuring device, and that this fact would, again, destroy the possibility of observing the interference pattern. In this view, the two incompatible outcomes result from the fact that each of these two cases establishes a different measurement setup, which is mutually exclusive with or complementary to the alternative setup, and hence leads to the alternative predictions concerning the outcomes of the experiment and the correspondingly different statistical distributions of the traces on the screen. No concept of the independent behavior, individual or collective, of photons themselves needs to be assumed (Bohr 1949, *PWNB* 2, pp. 63, 50–51). The same considerations would also apply to analogous events recorded in other quantum experiments. Furthermore, as the quantum eraser experiment (discussed in the next section) tells us, the situation is defined not only by what we *actually* know or do not know but by what is *in principle* possible or impossible to know concerning our interactions with quantum objects. It is the possibility or impossibility of this knowledge that defines the kind of predictions we can or cannot make in each case—for example, whether the interference pattern will or will not appear on the screen in the double-slit experiment (or its delayed-choice and quantum eraser versions).

2.4 The Quantum Eraser

At least in the present view, then, it is our *interaction* with quantum objects by means of our experimental technology (whose role is irreducible in contrast to the situation that is obtained in classical physics) that defines our knowledge concerning them or, again, more accurately, concerning the effects of this interaction upon the world that we can describe. The quantum eraser experiments, designed by Marlan Scully and coworkers, further support this argument and amplify its significance (Scully and Drühl 1982).

In the (double-slit version of the) quantum eraser experiment, before photons pass the diaphragm with the slits, they are "marked" in such a way that by examining the traces of the collisions between each photon and the screen, we can use this marking to establish which slit each photon has passed through. How this marking and erasure is accomplished is not essential here, although the originality of the idea and the ingenuity of the experimental technique used command high respect. What is crucial is that we can mark photons in this way and that if we do so, the interference pattern will not emerge, *regardless* of whether we actually examine each trace and establish which slit each photon has passed through. This is further testimony to the fact that the mere possibility of such knowledge is sufficient to prevent the appearance of the interference pattern, and it reveals a deeper meaning of this fact.

In the alternative setup of the experiment, after photons pass the diaphragm but before they reach the screen, the Scully marking is erased (this marking of each photon is what the quantum eraser *erases*) so as to prevent us from knowing, even in principle, which slit each photon passes through. Once we do so, the interference pattern appears. It is crucial that the erasure forever disables the knowledge in question (concerning the passing of each photon through one slit or the other), as against the standard version of the double-slit experiment, where counters enable this knowledge, at the inevitable cost of making it impossible to observe an interference pattern. Thus, it is, again, the possibility or impossibility of this knowledge that defines the kind of predictions we can or cannot make in each case—for example, whether an interference pattern will or will not appear on the screen.

Accordingly, while the features of the quantum eraser experiment may appear to be, and in some respects are, remarkable, they should not be unexpected. Indeed, one might say that given what we know about quantum phenomena—for example, those observed in the double-slit experiment—it would be more remarkable if these features did not appear.[17] The (erased)

[17] The delayed-choice version of the quantum eraser experiment uses half-silvered mirrors and EPR-type entangled photon pairs (Scully and Drühl 1982; Greene 2004, pp. 182–213). This setup allows us to gain or erase the knowledge in question (which way a given photon goes) without examining and hence in any way interfering with that given photon. We do this by interfering with and examining its EPR companion photons. This examination can, in principle, take place in a delayed-choice manner, indeed with arbitrary delay—for example,

"knowledge" in question is never available to us—assuming that one can speak of "knowledge" under these conditions, beyond knowing that photons *were* marked. Knowledge here only means that unless the second ("erasing") device is installed, in principle such knowledge would have been available because a corresponding experiment could be performed so as to yield an expected result—the lack of an interference pattern. As will be seen presently, the "erasure" of the *preceding* information as relevant to our future predictions, once a new measurement is made, is a defining feature of quantum phenomena, as against those of classical physics.

By the same token, it is our interaction with quantum objects that establishes the pattern of our knowledge concerning them—or, again, concerning the effects of this interaction upon the world that we can properly describe. Whatever version (i.e., standard or delayed choice) of the quantum eraser experiment one considers, it, I argue, demonstrates precisely this fact and its significance. The Scully marking of a photon is an interaction with a photon by means of our measuring devices, an act of measurement that allows for the possibility of knowledge that is incompatible with the appearance of the interference pattern, once a sufficient number of events are registered. Erasing a Scully marking is an alternative interaction with a photon, an interaction which restructures the measuring procedure involved and, by so doing, disables the possibility of such knowledge. Thus, once the experiment is repeated a sufficient number of times, this type of interaction reestablishes the possibility of the appearance of the interference pattern.

One should indeed say *establishes,* rather than reestablishes, since in each case we deal with two sets of disconnected and mutually exclusive—complementary—setups, and hence two sets of experiments that are completely disconnected from each other.[18] We cannot perform both experiments on any single photon at the same time, or combine them in the way we can combine the measurement of position and momentum in classical physics (which is what distinguishes classical from quantum physics, in view of the role of the uncertainty relations in quantum physics). The two corresponding experiments require two different photons. This fact becomes crucial in the EPR experiment, and while it was underappreciated in EPR's and Einstein's subsequent arguments concerning the subject, it largely shapes Bohr's reply to EPR and this

years after the actual events of the experiment have physically taken place. The immediate examination of the medium (screen) with traces of photons will not reveal any interference pattern. However, the subsequent (delayed) examination of the traces left by their companions—whose Scully markings have been erased, thus disabling our knowledge concerning which way these particular photons went (again, possibly years ago)—will show the interference pattern of the corresponding subset of the traces left on the screen. I shall not discuss this version of the experiment further. While its features may appear even more striking than those of the standard version, they are consistent with the nature of quantum phenomena as manifest in the double-slit and other experiments, and as such, they are, again, more expected than unexpected.

[18] On further connections to Bohr's complementarity, see Herzog et al. (1995).

study's analysis of the situation in Chapters 8 and 9. In the case of the quantum eraser experiment, either a photon (each of the photons involved) is marked or it is no longer marked, and the knowledge concerning which slit it has passed through is no longer available, which must lead to the interference pattern, once the experiment is repeated a sufficient number of times. The quantum eraser erases *the (previously made) markings of the photon*s *involved* and *not the outcome of the same experiment*. The erasure of markings establishes a new set of measuring arrangements and individual experiments defined by them.[19]

Nor, given that the identically prepared quantum experiments do not in general produce the same outcomes, can we repeat the experiment so as to ever guarantee that a single photon would pass through the same slit, any more than we could guarantee the repetition of any other effect found in the run of the original experiment. There is no way to ever recover the behavior—or, again, more accurately, the effect—of any given single run of the experiment with a marked photon once this marking is erased, or for that matter if we repeat the experiment with another single photon, either marked or unmarked. In this regard, the situation is the same as in the standard double-slit experiment, where each individual run of the experiment in general gives a different outcome, even though each emission is identically prepared as concerns the state of the source. The delayed-choice nature of the "unmarking" of a photon after it passes through one slit or another is analogous to switching the detectors off before a photon reaches them in the standard double-slit experiment (thereby assuring the appearance of the interference pattern) and, hence, does not change anything in this respect. The quantum eraser experiment is as irreducibly *statistical* in this respect as all quantum experiments are, beginning, again, with the double-slit experiment. Both the double-slit experiment and the quantum eraser experiment are of course also statistical insofar as they deal with the collective effects (such as the interference/correlation pattern or the lack thereof) of multiple experiments. Similar considerations apply to the delayed-choice version of the quantum eraser experiment (which uses the beam-splitter experiment), in which one encounters the alternative sets of patterns, discernible only when we know for which photons such knowledge is not available.

The quantum eraser experiment further demonstrates the key aspects of quantum phenomena apparent in all paradigmatic quantum experiments. A striking feature of the quantum eraser experiment is the defining role of the *determinate possibilities of knowledge* defined by the experiments involved rather than the *actual knowledge* already obtained, which gives a new and more radical meaning to our interactions with quantum objects.

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[19] This point also applies to the delayed-choice version of quantum eraser. As explained in note 17, in this case those photons that are marked and those that are unmarked are sorted out later, thus enabling us to "carve out" the interference in the overall pictures related to those individual runs of the experiment in which the erasure of the Scully marking took place. However, each set is defined strictly in accordance with the marking or unmarking pertaining to particular photons from the two respective different sets, which thus disconnects them.

2.5 Repetition and Erasure, Classical and Quantum

The quantum eraser experiment is particularly revealing in exposing the significance of those features of quantum phenomena and quantum mechanics that have to do with repeated experiments. These features fundamentally distinguish the way the repetition of identically prepared experiments works in quantum and classical physics, as stressed by Bohr, Heisenberg, Schrödinger, Pauli, von Neumann, and others. In recent years, the question of the difference between quantum and classical physics has been primarily considered, via quantum correlations, in the context of the EPR experiment and Bell's and related theorems, and the key experiments confirming Bell's theorem, especially those by Aspect (Aspect et al. 1982). As will be seen in Chapters 7 and 8, the question of repeated measurements, including as correlative to the statistical nature of our predictions concerning quantum phenomena, is crucial in this context as well. For the moment, however, I would like to focus on the experiments discussed in this chapter, especially the quantum eraser experiment, and on the concept of "erasure," which is defined more generally than in relation to the quantum eraser experiment, in particular as essentially linked to the concept of "repetition." The particular conjunction of the two concepts as applicable to quantum phenomena manifests and even defines the essential difference between them and the phenomena of classical physics.

According to Bohr, "[In quantum experiments, as against the classical ones] a subsequent measurement to a certain degree deprives the information given by a previous measurement *of its significance for predicting the future course of the phenomena*. Obviously, these facts not only set a limit to the *extent* of the information obtainable by measurements, but they also set a limit to the *meaning* which we may attribute to such information" (*PWNB* 1, p. 18; emphasis added). Thus, whether one deals with the same or identically prepared different objects, one might speak of how the data obtained or even potentially obtainable in one measurement is "erased" by a second measurement and thus no longer useful for the purposes of our predictions concerning the subsequent outcome of the experiment. Bohr amplifies this point in the Como lecture: "It must not be forgotten ... that 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" (Bohr 1927, *PWNB* 1, p. 68). Accordingly, we can no longer use observation and measurement in the way we do in classical physics to help our quantum predictions. As Bohr notes, Heisenberg makes the same point in his uncertainty relations paper (Heisenberg 1927, pp. 66, 72–76; also Heisenberg 1930, p. 36). This point is equally crucial to Schrödinger in his cat-paradox paper (Schrödinger 1935a, *QTM*, pp. 152, 154, 157–158).

Pauli comments on this situation in his letter to Born (of March 31, 1954). The letter refers to Bohr and essentially follows Bohr's argumentation just cited, and, not coincidentally, it deals with Einstein's argumentation of the EPR type and Born's misunderstanding of this argumentation, acknowledged by Born. Pauli first considers "the determination of the path of a planet" as an example. He says, "if one is in possession of the simple *laws* for the motion of the body (for example, Newton's law of gravitation), one is able to *calculate* the *path* (also position *and* velocity at any given time) of the body with as *high* an accuracy *as one likes* (and also to test the assumed law again at different times). Repeated measurements of the position with limited accuracy can therefore successfully replace *one* measurement of the position with high accuracy. The assumption of the relatively simple law of force like that of Newton (and not some irregular zig-zag motion or other on a small scale) then appears as an idealization which is permissible in the sense of classical mechanics." By contrast, in quantum mechanics "the repetition of positional measurement in sequence with the same accuracy . . . is of *no use at all* in predicting subsequent positional measurements. For [given the uncertainty relations] every positional measurement to [the same] accuracy at [a given] time implies the inaccuracy [defined by the uncertainty relations] at a later time, and *destroys the possibility of using all previous positional measurements within these limits of error*! (If I am not mistaken, Bohr discussed this example with me many years ago.)" (Born 2005, p. 219). Indeed, Bohr's comments cited above clearly follow this logic. By the same token, our predictions are irreducibly probabilistic, even in the case of individual events.

This situation is also reflected in the impossibility (stressed throughout this study) of repeating identically prepared experiments with the same outcome, since one experiment does not tell us exactly what will happen in the next (again, identically prepared). Suppose we track an electron in a hydrogen atom. With Schrödinger's equation and Born's rule in hand, a given measurement allows us to form certain expectations concerning the future behavior of the electron. However, if we perform a subsequent measurement, the previous measurement becomes meaningless for any future prediction: Only the last measurement performed is meaningful for these purposes. Correlatively, if we prepare an atom in a certain state in order to make such predictions (and in fact "tracking" an electron usually amounts to such a preparation), any previous preparation—either in a different state or in the same prepared state (a repeated experiment)—offers no further help in predicting the measurement outcome.

I shall return to these considerations at several key junctures of this study, especially, again, in my discussion of the EPR-type experiment in Chapter 7. It may be briefly noted here that, in this case, we can prepare a pair of quantum objects in such a way that a subsequent measurement of, say, the momentum of the one allows us to predict the momentum of the other exactly. In the case of spin measurements, we can, as quantum information theorists would have it, even "teleport" the state of the first object in the sense that the second will be in the same state in terms of the outcome of the corresponding spin measurement. However, if we prepare two "identical" pairs of objects (identical, again, as concerns the state of the measuring instruments where the two preparations take place), the outcome of the measurement on the first object of the second pair will not be identical to that on the first object of the first pair. Nor, accordingly, will be our prediction concerning the second object of the second pair, although this prediction will be exact, just as was its counterpart in the first experiment. Any new measurement redefine our predictions. This irreducible coupling of "repetition" and "difference" gives a new meaning to the idea of "repeating" an experiment.

From this perspective, the difference between classical and quantum phenomena may be seen as follows. Suppose that one performs an experiment on a classical physical object—say, as Galileo did—by dropping a stone from a certain height. One can, at least ideally, repeat the same experiment on the same object or an identical object and obtain the same outcome. Indeed, such a repetition is always, in principle, possible insofar as one retains a proper record of the experiment. In classical physics—unlike in quantum theory—the distinction between the behavior of the objects under consideration and the observed phenomena, while present, can be disregarded. This possibility of repeating identically prepared experiments is essential to the disciplinary nature of modern physics, classical or quantum. There is, however, a crucial difference: In quantum experiments, we can only repeat the statistical data obtained in a given set of experiments, since the identically prepared experiments in general lead to different outcomes. In classical physics, this possibility of repeating a given experiment can be destroyed only by literally erasing the data in question, obliterating it without a trace. As long as the data are still available, the experiment could, in principle, be reconstituted exactly on a given—and for all theoretical and practical purposes identical—object, and in practice this of course happens all the time. In the case of quantum phenomena, one encounters the *effects* accompanying this type of erasure of the preceding history in any given experiment. While the data necessary to repeat the experiment on an object, such as a photon or electron, are identical to that used in the previous experiment (since all photons or all electrons are indistinguishable from each other), it is, again, in general impossible to repeat any given experiment with the same outcome. Once a measurement is made for the purposes of a prediction, the experiment is closed and the corresponding quantum object is no longer available for these purposes: It is as good as destroyed for the purposes of any future predictions compatible with this measurement. Conversely, any subsequent measurement establishes a new field of possible predictions. Accordingly, any given measurement "erases" the information previously obtained in the sense of making it meaningless for the purposes of predictions, which are defined only by the last measurement performed. While in classical physics an analogous erasure would require a complete obliteration of the previous relevant data, in quantum physics any measurement is an erasure of the previous data as relevant to our future predictions concerning the behavior of the object in question.

Chapter 3
Heisenberg's Revolutions: New Kinematics, New Mathematics, and New Philosophy

... one might readily regard the ensemble of quantities $A(n, n - \alpha)$ $e^{i\omega(n,\, n-\alpha)t}$ as a representation of the quantity $x(t)$.
—Werner Heisenberg, "On Quantum-Theoretical Re-Interpretation of Kinematic and Mechanical Relations" (Heisenberg 1925, *SQM*, p. 264).

Abstract This and the next chapter explore the physical, mathematical, and philosophical significance of Heisenberg's discovery of quantum mechanics. This discovery was among the most momentous discoveries in the history of physics, comparable to that of classical mechanics by Newton, Maxwell's discovery of his equations for electromagnetism, and Einstein's discoveries of special and then general relativity. The comparison with Newton's discovery of classical mechanics is especially apt, since in both cases at stake was the introduction of a new calculus—differential calculus by Newton and matrix (de facto Hilbert-space operator) calculus by Heisenberg. The relationships between the mathematical calculus deployed and the physical phenomena considered are, however, entirely different in the case of Heisenberg's mechanics. Unlike Newton's mechanics, Maxwell's electromagnetic theory, or Einstein's relativity, Heisenberg's calculus does not describe the behavior of *quantum objects*, but only relates, in terms of probabilistic predictions, to *quantum phenomena*, manifested in our measuring instruments impacted by quantum objects. Section 3.1 offers a general introduction to Heisenberg's quantum mechanics, developed into the matrix quantum mechanics shortly thereafter by Born, Jordan, and Heisenberg himself. Sections 3.2 and 3.3 discuss the key physical and philosophical principles that shaped Heisenberg's approach—the principle of dealing with "quantities, which in principle are observable" and the correspondence principle. Section 3.4 considers the key features of Heisenberg's quantum mechanics. It contains some technical mathematical details and may be skipped by readers unfamiliar with mathematics at this level. Its key conceptual points are explained in nontechnical terms in Section 3.1.

A. Plotnitsky, *Epistemology and Probability*, Fundamental Theories of Physics 161,
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3.1 "A Step of Probably Fundamental Importance": From Bohr to Heisenberg

The history of quantum mechanics and, particularly, of its interpretation is indissociable from Bohr's work on complementarity and his confrontation with Einstein. This historical assessment is rarely doubted, although opinions concerning Bohr's work and its impact vary. As this study traces the development of Bohr's argument from the Como lecture in 1927 to its ultimate nonclassical version, crystallized sometime in the late 1930s or 1940s, it takes a stratified view of this impact or of complementarity itself, especially as concerns the Como argument, which, as will be seen in Chapter 6, contains significant problems. It is to Bohr's credit, however, that he quickly (within less than a year) revised his Como argument and began his journey toward the nonclassical epistemology of quantum theory, in part under the impact of his exchanges with Einstein, which continued to shape his thinking from that point on. As I said, the Como argument is unique insofar as it was not influenced by Bohr's dialogue with Einstein. Einstein's earlier work, specifically as concerns the wave–particle duality in quantum physics, was important for Bohr's thinking during the decade preceding the discovery of quantum mechanics. By the time of his work on complementarity, however, quantum mechanics had become the most decisive force shaping Bohr's thought.

The *invention* of complementarity is unimaginable apart from the discovery of quantum mechanics by Heisenberg and Schrödinger, and, I would argue, apart from Heisenberg's discovery of the uncertainty relations. Bohr's concept responds to these discoveries by way of providing an interpretation of the new theory, or at least, making crucial initial steps toward such an interpretation. Heisenberg was also responsible for several key physical ideas and some philosophical ones that shaped the conceptual architecture of complementarity, especially at earlier stages of Bohr's thinking concerning it. Some of the key refinements of Bohr's thinking on complementarity, while impacted most by Einstein's criticism, were influenced by Heisenberg's ideas as well, and some of these ideas, too, can be traced to Heisenberg's first paper on quantum mechanics. The key ingredients of Heisenberg's approach, especially the way probability is used there, have continued to remain crucial throughout Bohr's work. Most significantly in the context of this chapter, however, the changes in Bohr's argument (eventually leading him to the nonclassical epistemology of quantum theory) were accompanied by his return to the approach used by Heisenberg in developing his matrix version of quantum mechanics in 1925. Indeed, Bohr's nonclassical version of complementarity may be seen as a radicalized version of the epistemology adopted by Heisenberg in his initial work on quantum mechanics.

These considerations are not intended to diminish the originality and significance of Bohr's contribution, even apart from the fact that Bohr's influence on Heisenberg's thinking was decisive in turn, including on both of his great discoveries—quantum mechanics and the uncertainty relations.[1] Instead, they help to trace the genealogy of Bohr's thought and gain a better understanding of the nature of his contribution. On the other hand, these considerations *are intended* to suggest a greater importance of Heisenberg's philosophical thinking to that contribution than is generally (there are exceptions) associated with Heisenberg's work. The extraordinary significance of Heisenberg's physical and mathematical contribution to quantum mechanics and quantum field theory is of course universally acknowledged.

Bohr was among the first to notice—and probably the first to clearly grasp—the radical nature of Heisenberg's ideas and their far-reaching implications. Apart perhaps from his famous postulates concerning discontinuous and mechanically unexplainable transitions ("quantum jumps") of electrons in atoms from one orbit to another, Bohr's most radical statement on the epistemology of quantum theory prior to his post-EPR writings occurs in his 1925 survey "Atomic Theory and Mechanics" (Bohr 1925b, *PWNB* 1, pp. 25–51). Thus, it occurs before he introduced complementarity in the Como lecture, and even before Schrödinger's wave mechanics and Heisenberg's discovery of the uncertainty relations, both of which shaped the Como argument. However, the statement is made in the immediate wake of and in response to Heisenberg's paper introducing his "rational quantum mechanics" and the development of Heisenberg's ideas into a more cohesive version of matrix mechanics by Born and Jordan.[2] Heisenberg's "step," Bohr rightly guessed (a good Bayesian bet!), was "probably of fundamental importance." Ultimately, it proved to be one of the most decisive and radical steps in the history of twentieth-century physics and, in some respects, of all physics. Bohr wrote, "In contrast to ordinary mechanics, the new quantum mechanics does not deal with a space–time description of the motion of atomic particles" (Bohr 1925b, *PWNB* 1, p. 48). Bohr was not unprepared for this eventuality, as is clear from his letter to Heisenberg written in the wake of the collapse of the Bohr–Kramers–Slater (BKS) proposal and shortly before Heisenberg's discovery of quantum mechanics (Letter to Heisenberg, April 18, 1925, Bohr 1972–1996, vol. 5, pp. 79–80).[3]

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[1] The latter case is complex. Heisenberg's progress in his work, while in Bohr's Institute in Copenhagen, on the physical meaning of quantum mechanics, which led him to the uncertainty relations, appears to have been helped by Bohr's absence (on a skiing vocation in Norway) from the institute at the time. This is not to say that Bohr's *influence* was absent. By the time Bohr returned, Heisenberg had finished his paper and submitted it for publication without Bohr having read it. Heisenberg famously added a note in proofs reflecting some of Bohr's criticism of the paper. The paper, again, had a momentous impact on Bohr's thinking at the time, eventually leading him to complementarity.

[2] The article was in preparation as a survey of the state of the atomic theory before Heisenberg's discovery of quantum mechanics, but was modified in view of this discovery and Born and Jordan's work on casting Heisenberg's mechanics into its properly matrix form.

[3] The failure of the BKS proposal led Bohr to his arguably first realization that it might be necessary to accept the impossibility of a descriptive theory of the physical nature and behavior of quantum objects, and, as will be seen in Chapter 8, correlatively certain other

Thus, the epistemology of Heisenberg's matrix mechanics and his own thinking at the time could have led Bohr to his nonclassical version of complementarity more directly than his way to it from the argument, via Schrödinger, given in the Como lecture. The possibility of a nonclassical argument was intimated in the Como lecture, where Bohr notes that both particles and waves are "abstractions, their properties on the quantum theory being definable and observable only through their interactions with other systems" (Bohr 1927, *PWNB* 1, p. 57). It was, however, only an intimation, albeit an important one. First of all, this formulation and the Como argument as a whole leave space for a possible assignment of *some* among such properties, at the time of measurement, even though, in view of the uncertainty relations, never more than half of the properties necessary to define the (causal) behavior of physical objects in classical mechanics. Thus, it may be possible to assign either a position or a momentum to a quantum object, but never both at the same time. Second, as against Heisenberg's original approach and close to Schrödinger's and especially Dirac's view at the time, the Como lecture assigns causality to the independent behavior of quantum objects, even though this behavior is unobservable. The term "abstraction" in the above formulation, however, indicates ambivalence in Bohr's attitude and ambiguities of his conception of quantum phenomena at the time. These ambiguities were not fully resolved by Bohr before his introduction in the wake of EPR's paper (although not in his reply to EPR) of his concept of phenomenon, which only refers, nonclassically, to what is classically observed in measuring instruments. By contrast, an assignment of any properties, even single ones and even at the time of measurement, to quantum objects is disallowed.

This is, again, a more radical view than that adopted by Heisenberg in his 1925 approach, which merely does not consider, as in principle unobservable, the independent behavior of quantum objects. Nevertheless, Bohr, thus, might have been better off following and developing Heisenberg's initial approach all

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radical features of quantum phenomena, specifically those at stake in the EPR experiment. Indeed, the proposal was Bohr's last-ditch effort to save at least some form of describing what actually happens at the quantum level. As I noted in the Preface, the proposal was controversial because it suspended the strict application of the energy conservation law in quantum processes (it would apply only statistically there) and was abandoned in view of Walther Bothe and Hans Geiger's experiment, which confirmed the exact energy conservation in quantum interactions. (Schrödinger was initially one of its supporters, although he changed his attitude toward a more causal one in his work on his wave mechanics.) The proposal was, however, important conceptually in its emphasis on the potentially unavoidable probabilistic nature of quantum theory, which quantum mechanics was to confirm, while importantly retaining the energy conservation. It was also significant in introducing the idea of "a probability wave," which, however, implied a violation of the conservation of energy within their framework. Heisenberg's early work on quantum mechanics was clearly influenced by the BKS proposal. Establishing that energy is conserved in quantum processes was, however, a crucial step in Heisenberg's discovery of his matrix mechanics, and then Born was able to develop his physically viable concept of "a probability wave" by using and interpreting Schrödinger's wave function. The concept still requires considerable further qualifications, which will be offered in Chapter 5.

along, rather than taking a detour, via Schrödinger's wave theory, based on the classical-like ideas of wave motion, or even Heisenberg's argument concerning the uncertainty relations. As will be seen in Chapter 6, Heisenberg's argument, too, in part returns to the notions of physical properties and space–time motion of quantum objects, as constrained by the uncertainty relations, in his uncertainty-relations paper. By contrast, Bohr's initial commentary, just cited, suggests that Heisenberg's first paper on quantum mechanics, especially as supplemented by Born and Jordan's more properly matrix formulation of the theory, contained *most of the ingredients* that Bohr needed for his ultimate, nonclassical view—most, but *not all*. First of all, the idea of complementarity in the narrow sense, crucial for all of Bohr's work, was helped by Schrödinger's wave theory, on the one hand, and the uncertainty relations, on the other hand. Bohr's thinking leading to complementarity was also influenced by Dirac's transformation theory, which rigorously connected Heisenberg's and Schrödinger's formalisms in mathematical and predictive terms and which was developed by Dirac in 1927, while in Copenhagen, and independently by Jordan. The theory also played an important role in Heisenberg's paper on the uncertainty relations.[4] Finally, Bohr's exchanges with Einstein, especially those concerning EPR's argument and related arguments by Einstein dealing with the question of locality of quantum mechanics, brought Bohr's thinking concerning complementarity to a new level. In sum, Bohr, first, needed the ideas shaping the Como arguments in order to introduce complementarity in the narrow sense, on which all versions of complementarity in the broader sense of the interpretation of quantum mechanics depend. Then, however, some of these ideas had to be abandoned in order to develop a nonclassical version of complementarity. Nevertheless,

[4] Bohr's later thinking, eventually leading him to nonclassical epistemology, was more helped by Dirac's work on quantum electrodynamics. Bohr himself made significant contributions to our understanding of measurement in quantum field theory in the 1930s, extending to this domain his ideas concerning measurement in quantum mechanics. Dirac was in Bohr's institute in Copenhagen when he did his pioneering work on both the transformation theory (which influenced Heisenberg's work, also while in Copenhagen, on the uncertainty relations, as well as Bohr's Como argument) and quantum electrodynamics in 1927. Dirac was also involved in preparing the English version of the Como lecture. Bohr's work on measurement in quantum field theory is contained in his two collaborations on the subject with Léon Rosenfeld (Bohr and Rosenfeld 1933, 1950). The first one was written in 1933 in response to Lev Landau and Rudolf Peierls's argument concerning the inapplicability of the uncertainty relations to quantum field measurements. The exchange between Bohr and EPR took place shortly thereafter, in 1935, and thus closely followed Bohr's work on quantum field theory, and in writing his reply Bohr was assisted by Rosenfeld. It would be difficult to claim any strict causality here. However, one can hardly doubt that Bohr's thinking concerning quantum field theory was part of his quantum theoretical thinking and shaped his arguments concerning the epistemology of quantum mechanics, including his counterargument to EPR. He clearly saw this epistemology as extendable to and possibly taking a still more radical form in quantum field theory, and, hence, he saw the theory itself as supporting his argument concerning quantum mechanics (Bohr 1937; *PWNB* 4, p. 88).

Heisenberg's thinking in his discovery of quantum mechanics had an indelible impact on all of Bohr's work on complementarity.

At some point in his work on his new quantum mechanics, Heisenberg realized that he did not need a space–time description of quantum objects to be able to predict the outcomes of the experiments he considered, which were concerned with atomic spectra. This was an extraordinary move at the time, given that throughout the history of mechanics from Galileo or even earlier, such a description appeared necessary for making such predictions. Accordingly, his decision to forgo *a physical description of the ultimate objects in question in the theory* was a daring step. This step was also nontrivial theoretically, given that, in yet another unexpected and crucial move, Heisenberg retained the classical *equations* of motion, whose variables no longer related to motion but only to the probabilities of predictions of the outcomes of quantum experiments. Quantum theorists had tried to adjust or change classical equations while keeping the same physical variables, in part by thinking in descriptive terms of classical mechanics and in order to be able to continue to do so. In other words, they saw *equations and not variables* as the problem. In a move characteristic of his way of seeing a solution where others saw a problem, Heisenberg reversed this thinking. He regarded equations and the lack of descriptive capacity in the theory as solutions; variables and, correlatively, attempts to approach the situation in terms of description were problems.

My emphasis above on *a physical description of the ultimate objects in question in the theory*, as abandoned by Heisenberg, reflects a subtle and often overlooked point of Heisenberg's original paper. Heisenberg's new theory *did not deal only* with "quantities which in principle are observable," as he famously stated in opening his paper, although it was concerned with *predicting only* such quantities. The theory, nevertheless, *did deal* and was *concerned*, even essentially concerned, with (unobservable) quantum objects. As such, it departed from most forms of positivism, such as that of Mach, who argued that physics should be essentially concerned only with what can be observed, which, as Einstein noted and as I shall discuss below, is a problematic assertion. Heisenberg's approach was also different from that of Einstein in relativity, when Einstein dispensed with absolute space and time or with ether (as an independent background of physical events), as both unobservable and nonexistent or at least having no impact on what we can observe, and hence, dispensable. Although in part inspired by Einstein's relativity, Heisenberg's *theory* itself was, epistemologically, more analogous to Bohr's 1913 theory of the atom. Bohr's theory, too, did not offer a mechanical description of the transitions ("quantum jumps") of electrons from one energy level to another, as they absorbed or emitted Planck's energy quanta. Bohr noted that such an analysis appeared to be impossible at the time. In any event, while—in contrast to classical mechanics or the old quantum theory—Heisenberg's theory *did not describe* quantum objects and their behavior (which did not appear possible at the time, according to Heisenberg, echoing Bohr's 1913 argument), the *existence* of quantum objects was essential. Heisenberg's argument clearly meant that it was this existence that is responsible for the observable quantities in question, even though the way in which quantum objects exist was not considered by the theory and was perhaps, in principle, unavailable to the theory or to observation. As will be seen, the significance of the independent *existence* of quantum objects was reinforced by locality considerations in the context of the EPR experiment, and Bohr carried it into his nonclassical epistemology. As I argue here, ultimately one might need to speak of the existence of certain entities in nature that necessitate this concept of quantum objects. However, this qualification only amplifies my point, especially against positivism. Quantum objects and their existence are meaningful even though—and because—there is nothing meaningful that we can say about their independent behavior.

As explained in the Introduction, quantum mechanics was born, in Heisenberg's work, out of the difficulties and ultimately the impossibility of bringing the semiclassical mechanical pictures of the old quantum theory into an adequate correspondence with experiments. It also appeared impossible to achieve a harmony within the theory itself, beginning with the concept of stationary states as represented by Keplerian electrons orbiting atomic nuclei—or, historically speaking, ending there. The theory began with Bohr, who abandoned as hopeless an attempt to offer a mechanical explanation for such transitions, as opposed to the stationary states themselves. The latter, he said, "can be discussed by help of the ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis" (Bohr 1913, p. 7). Bohr postulated discontinuous "quantum jumps" between stationary states, resulting in the emission of Planck's quanta of radiation, without electrons radiating continuously while remaining in orbit. In addition, in contradiction to the laws of classical electrodynamics, Bohr postulated that there would exist a lowest energy level at which electrons would not radiate. Bohr's postulates proved to be correct and have remained part of quantum theory ever since. They were given a proper mathematical theory with Heisenberg's and Schrödinger's quantum mechanics, especially when coupled with Born's probability interpretation of Schrödinger's wave function. The interpretation also gave Schrödinger's mechanics the Heisenbergian epistemology, and for that reason was not especially welcomed by Schrödinger, which is hardly surprising since it sounded the death knell for Schrödinger's program of wave mechanics.

While some aspects of both classical mechanics and classical electrodynamics were retained in the old quantum theory (which is in part why it was called semiclassical), Heisenberg's scheme abandoned the application of the concepts of classical physics to quantum objects altogether, in particular the assumption of a classical description (e.g., "orbits") even in the case of stationary states. This move was, again, especially remarkable given that Heisenberg also decided to use the classical equations of motion used to describe the behavior (for example, an orbital behavior) of objects in classical mechanics. According to Mehra and Rechenberg, "Although this development of Heisenberg's thought appears to be natural and logical, in fact this reasoning was quite audacious in spring 1925. After the theoreticians had discovered the breakdown of classical mechanics in atomic theory, they had abandoned the use of equations of motion altogether. They confined themselves to taking up mathematical relations from the classical theory of multiply periodic systems—relations, which had been obtained after going through the details of integration of the equations of motion—and translating them by more or less systematic guessing into the corresponding quantum-theoretical formulae" (*MR* 2, pp. 231–232). This is not altogether true, since equations of classical mechanics were used by quantum theorists, for example, by Sommerfeld, although they were indeed used primarily as part of this systematic guessing. But then, the whole point here—and what was most remarkable—is that in contrast to Schrödinger's approach, which, too, proceeded from the equations of motion, Heisenberg did not use and did not aim to use these *equations* as equations of *motion*. The subtraction of motion and ultimately of all physical ontology from the equations of motion may also be seen (with due qualifications) as a Platonist move, as against classical physics, which, as the physics of motion, remained Aristotelian.

While, again, daring (most, including Pauli, were reluctant to make it), the move was not merely a product of a lucky guess, since by then the assumption that the concept of motion was applicable to any aspect of the behavior of electrons had become increasingly difficult to sustain. Thus, rather than *semi-classical*, Heisenberg's new mechanics was a *strictly quantum* theory, the first such theory, even though it retained classical physics in two capacities. First, classical physics functioned as a certain limit of the new (quantum) mechanics, since the predictions of both quantum and classical mechanics were assumed to coincide, once one reached the domain previously governed by classical physics. This assumption essentially constituted the content of Bohr's correspondence principle, although, as will be seen below, the latter was also given a different meaning by quantum mechanics, and even can be nearly dispensed with, once quantum mechanics is in place. Second, classical physics could be—and perhaps (as in Bohr's interpretation) would have to be—used in the description of the measuring instruments involved and hence of the actual outcomes of the experiments considered. This "split" of the classical description at the level of measuring instruments and indescribability at the level of quantum objects is essentially different from the "mix" of some classical description (e.g., orbits) and some quantum description (e.g., quantum jumps) in considering quantum objects and their behavior in the old quantum theory. A fully consistent, uniform quantum mathematical treatment of the stationary states (no longer "orbits" or conforming to any classical-like mechanical model) and of transitions between them was provided by Born and Jordan (Born and Jordan 1925) and then by the three-man paper (*Dreimannarbeit*) of Born, Heisenberg, and Jordan later in 1925 (Born, Heisenberg and Jordan 1926).

The scope of this study does not extend to a discussion of the history of this struggle from Bohr's 1913 theory of the hydrogen atom to quantum mechanics, the history usefully traced by Bohr himself in "Atomic Theory and Mechanics" (Bohr 1925b). However, taking advantage of Bohr's account there, I would like to cite his summary of the state of quantum theory as it stood before Heisenberg's discovery, with Kramers and Heisenberg's paper on dispersion theory representing its most significant preceding achievement and a transitional step to Heisenberg's work. This summary closes the penultimate section, "Insufficiency of Mechanical Pictures," before the final section, "The Development of a Rational Quantum Mechanics," devoted to Heisenberg's discovery and to matrix mechanics, developed by, in addition to Heisenberg, Born and Jordan. These titles themselves capture the move from a manifest "insufficiency of mechanical pictures" in the old (*semi*classical) quantum theory to their abandonment in Heisenberg's new "rational quantum mechanics." Bohr writes

> While this description [by Kramers and Heisenberg] of optical phenomena was entirely in harmony with the fundamental ideas of the quantum theory, it soon appeared that it stood in strange contradiction to the use of the mechanical pictures previously employed for an analysis of the stationary states. In the first place, it is impossible on the basis of the scattering activity of illuminated atoms demanded by the dispersion theory to construct an asymptotic connection between the reaction of an atom in alternating fields of smaller and smaller frequency and the reaction in constant fields as calculated from quantization rules of the theory of periodicity systems. This difficulty strengthened the doubts about this theory to which . . . the problem of the hydrogen atom in crossed electric and magnetic fields had led. Secondly, it had to be regarded as especially unsatisfactory that the theory of periodicity systems was apparently helpless in the problem of the quantitative determination of the transition probabilities on the basis of the mechanical pictures of stationary states. This was felt all the more, as it was possible in several cases to obtain a quantitative formulation of the general statements of the correspondence principle as regards these transition probabilities with the help of viewpoints suggested by an analysis of the optical behaviour of electrodynamic models. These results stood in excellent agreement with measurements on the relative intensities of spectral lines, as they have been developed especially in Utrecht during the last few years, but they could only in a very artificial way be included in the schemes governed by the rules of quantization. (Bohr 1925b, *PWNB* 1, pp. 46–47)[5]

It is worth noting that the findings of Kramers' dispersion theory, while in accord with the corresponding experiments, did not appear to depend on a classical mechanical description of subatomic processes, a fact that played a significant role in the trajectory or "quantum jumps" leading Heisenberg to his discovery. Most crucial, then, was that these difficulties compelled Heisenberg to abandon classical-like physical considerations at the quantum level.

Heisenberg was led to a realization that, as he called it, quantum mechanics required a "new kinematics," the development of which proved to be one of his major innovations. The phrase and the concept were partially owed to Einstein's appeal to a new form of kinematics in his papers on special relativity, but Heisenberg's concept is more radical by virtue of being, as it were, far less, actually not at all, kinematical physically. Traditionally, as its etymology indicates, "kinematics" refers to a representation, usually by means of

[5] Although essential to quantum mechanics, both the exclusion principle by Pauli and spin by Samuel Goudsmit and George Uhlenbeck were discovered (in 1924 and 1926, respectively) by semiclassical means of the old quantum theory.

continuous (indeed differential) functions, of the attributes of motion, such as spatial (or temporal) coordinates or velocities of a body. The representations of dynamic properties, such as momentum and energy, are dependent on and are functions of kinematical properties. By contrast, Heisenberg's "new kinematics" related its elements to what is observable in measuring instruments under the impact of quantum objects, rather than representing the attributes of the motion of these objects. The kinematical elements of Heisenberg's theory were no longer functions, or any form of representation, of properties of quantum objects or their behavior, the aspect of kinematics that was retained from classical physics in Einstein's kinematics of special relativity. These elements were conceived as infinite-dimensional square tables, matrices, of complex variables, eventually rethought in terms of operators in a Hilbert space, or still more abstract entities, such as elements of C^*-algebras, or even objects of category and topos theories (e.g., Isham and Butterfield 1998; Butterfield and Isham 1999). They had, again, no classical-like specifiable relation to the attributes of motion of quantum objects, as kinematical elements would in classical physics or special relativity, or even (finite-dimensional) tensors of general relativity.[6] Instead, they were designed only to relate, probabilistically, to the impact of quantum objects upon measuring instruments (i.e., the impact of light quanta, emitted by electrons, upon photographic plates registering these quanta as parts of atomic spectra). As Born and Jordan discovered in their paper, given the data in question, these matrices had to be infinite to treat these data consistently, and indeed, lacking finite traces, could not even be "bounded" infinite matrices, as they are called (Born and Jordan 1925, p. 291).

It also became clear shortly thereafter that these matrices needed to be infinite for the matrix scheme to be consistent with the uncertainty relations for the continuous variables, such as position and momentum. Although one can derive the uncertainty relations more immediately and in a more elementary way by using de Broglie's formulas for matter waves, Heisenberg also derived them from quantum mechanics. His derivation, proceeding via the transformation theory of Dirac and Jordan, used the formula, the first equation of the matrix mechanics,

$$\mathbf{PQ} - \mathbf{QP} = \left(\frac{h}{2\pi i}\right) \times \mathbf{1}\,(\mathbf{1} \text{ is a unit matrix}).$$

The equation, introduced by Born and Jordan, gave a more rigorous mathematical meaning to Heisenberg's quantum conditions in his original paper and was then developed by Heisenberg himself—in the three-man paper (Born and

[6] Tensors of the second rank are matrices, and, accordingly, in general they do not commute. This noncommutativity, however, does not play the same role in relativity, and it did not attract attention. It is only with quantum mechanics that noncommutative mathematics enters physics in an essential way.

Jordan 1925, p. 292).[7] Heisenberg's derivation of the uncertainty relations from the formalism of quantum theory had a special significance. On the one hand, as was shown by Heisenberg's γ-ray-microscope thought experiment in his paper and later by Bohr's derivation in the Como lecture, the uncertainty relations could be seen as experimentally given, and they were so seen by both Heisenberg and Bohr. That is, they were seen as a law of nature. On the other hand, Heisenberg's derivation showed that quantum mechanics "contains" the uncertainty relations (which can be derived from Schrödinger's equation as well, as Schrödinger was among the first to show). In other words, it showed that quantum mechanics adequately responds to the experimental data in question.

In his initial commentary on Heisenberg's paper, cited above, Bohr observed that the "fundamental importance" [of Heisenberg's step] was in "formulating the problems of the quantum theory in a novel way by which the difficulties [that had besieged quantum theory since Planck] attached to the use of mechanical pictures may, it is hoped, be avoided" (Bohr 1925b, *PWNB* 1, p. 48). Ultimately, the difficulties were avoided by abandoning such a use altogether, while, remarkably enough, the formalism enables excellent probabilistic predictions concerning the outcome of experiments, and the key physical laws, such as conservation laws, still apply (Bohr 1925b, *PWNB* 1, pp. 48, 51). The proof of the conservation theorems—first for the particular case of an aharmonic oscillator, by Heisenberg, and then more generally by Born and Jordan—was essential for establishing the new mechanics as a proper physical theory, and, in addition, as an irreducibly probabilistic theory.[8] Bohr elaborates as follows:

> In this theory the attempt is made to transcribe every use of mechanical concepts in a way suited to the nature of the quantum theory, and such that in every stage of the computation only directly observable quantities enter. In contrast to ordinary mechanics, the new quantum mechanics does not deal with a space–time description of the motion of atomic particles. It operates with manifolds of quantities which replace the harmonic oscillating components of the motion and symbolize the possibilities of transitions between stationary states in conformity with the correspondence principle. These quantities satisfy certain relations which take the place of the mechanical equations of motion and the quantization rules [of the old quantum theory]. (Bohr 1925b, *PWNB* 1, p. 48)[9]

[7] Technically, the equation is an immediate consequence of Heisenberg's "quantum condition," through which the Planck constant, h, enters his new mechanics, as Born was perhaps the first to realize. However, it took yet another major step on Heisenberg's part to discover, two years later, that this formula is correlative to the uncertainty relations.

[8] These findings were also significant in view of the immediately preceding history of quantum theory, in particular, again, the BKS proposal, mentioned earlier (Bohr et al. 1924), which implied a violation of the exact energy conservation and was abandoned in view of the experimental evidence to the contrary. As noted earlier, for Bohr, the abandonment of the proposal carried nonclassical implications as concerns, interactively, a renunciation of the space–time description of quantum processes and the assumption of a coupling of quantum processes in separated atoms. This is the kind of situation found in the experiments of the EPR type, proposed by Einstein 10 years later. I shall discuss these connections in Chapter 8.

[9] Bohr reprises this assessment in the Como lecture (Bohr 1927, *PWNB* 1, pp. 70–71).

The classical (Newtonian, as in Heisenberg's paper, or Hamiltonian, as in Born and Jordan's paper) equations of motion are, again, formally retained in these relations but are applied only to matrix variables (used for the purposes of predictions) and no longer to any variables describing the motion of particles. They are, again, no longer equations of motion. The variables in question are the "amplitudes" of probabilities of transitions between different states of an atom. One thus retains the term "amplitude" from the classical theory but also reinterprets it so as to deprive it of a reference to the physical (e.g., oscillating) motion of physical objects, which reference defines the use of the term in classical physics, and instead to relate it to the probabilities in question. In other words, they de facto are what are now known as "probability amplitudes," as explained in Chapter 2. The rule itself for moving from these "amplitudes" to probabilities and hence *real numbers* (between 0 and 1), as postulated by Heisenberg, is equivalent to Born's square-moduli rule for complex numbers (applicable more generally, rather than only in the case of transitions between stationary states), while amplitudes themselves are treated as *complex vectors* (Heisenberg 1925, *SQM*, p. 263). One must keep in mind Bohr's warnings concerning the use of such terms, given that the term "amplitude" cannot be seen here as corresponding to a physical process of the type from which the term originates (Bohr 1929b, *PWNB* 1, p. 17). The theory becomes only probabilistically predictive rather than descriptive. As Heisenberg stated even before his scheme was fully worked out, "What I like in this scheme is that one can really reduce *all interactions* between atoms and the external world (apart from the problem of degeneracy) to transition probabilities" (Letter to Kronig, 5 June 1925; cited in *MR* 2, p. 242; emphasis added).

In sum, as Bohr said, Heisenberg interpreted $|q(nm)|^2$, the square of the transition amplitudes associated with the position coordinate of a periodic system as the probability for a quantum jump in the system. Thus, Heisenberg's scheme all but implied both Schrödinger's wave function and Born's probabilistic interpretation of it, and hence the mathematical equivalence of the two mechanics. To make these claims rigorous, one needed a more fully developed scheme of both matrix and wave mechanics. Heisenberg arrives at this proposition via Kramers's dispersion theory, to which he himself contributed previously, but which in turn helped his argument, in particular in establishing the noncommutativity of the multiplication in his new formalism, which proved to be one of the most crucial features of quantum theory. As indicated above, the fact, noted by Born in particular, that key formulas of dispersion theory did not depend on semiclassical mechanical models was significant and came to play an important role in Heisenberg's work.

The two key moves, both made by Heisenberg, enabled him to arrive at this noncommutativity. The first was the use of the Fourier representation to construct his variables. According to Mehra and Rechenberg, "Heisenberg's discovery of the noncommutation of the product of quantum-theoretical amplitudes would turn out to be the crucial step in the mathematical transition from classical to quantum mechanics, and one might wonder why it had not been noticed earlier, especially in connection with dispersion theory. Indeed, the easiest way of recognizing noncommutation is to take the limit of the very high frequency v (much larger than all absorption and emission frequencies in the atom) of Kramers' dispersion formula" (*MR* 2, p. 229). They then show that, given this formula, "the difference of products $XY–YX$ in the stationary state of the atom denoted by the quantum number n [in the formula] . . . cannot be zero" (*MR* 2, p. 230). Mehra and Rechenberg then note, "Although a demonstration of noncommutation of the product of quantum-theoretical variables was possible as early as 1924, and Kramers, Born, or Kramers and Heisenberg could have come across it on the basis of the simplest dispersion formula, all these authors missed it. The main reason was that it did not occur to anybody at that time to write it all down in terms of what corresponded to the classical Fourier series. That was done only by Heisenberg in spring 1925" (*MR* 2, p. 230).

Mehra and Rechenberg are also right to note Heisenberg's awareness that X and Y themselves in the product XY have to be taken as representing different stationary "states," as against classical theory, where such variables always refer to the same physical state of the system in question. Focusing, as most commentators do, on noncommutativity, Mehra and Rechenberg do not pursue the radical consequences of this fact. However, apart from, or rather *along with*, the fact of the noncommutativity of the multiplication of the variables as the immediate consequence or correlative, this is a crucial physical point. It de facto entails complementarity (in the narrow sense) as well as the uncertainty relations, even though it took some time for Bohr and Heisenberg, respectively, to realize this. For, as against classical mechanics, both such (conjugate) variables can never be defined for the same physical state, their definition is complementary in Bohr's sense because we can also always assign either one such variable or the other and define the respective physical state accordingly. This situation also implies, as a correlation to noncommutativity, that changing the order of measurement defines two different states and different values of the corresponding variables, of either one such variable or the other. If we measure first the ("coordinate") variable q (associated with the amplitude X) and then the ("momentum") variable p (associated with the amplitude Y), only p can be assigned a known value after this measurement, never q—and vice versa. (I leave aside for the moment the complexities related to the role of measuring instruments in this situation, a role not fully realized at the time.) It follows that, as explained in Chapter 2, a new measurement "erases" the outcome of any preceding measurement for the purposes of any future predictions concerning a given quantum object. What is remarkable is that this noncommutativity of the formalism is correlative to this, in Bohr's words, "entirely new situation as regards the description of physical phenomena that the notion of complementarity aims at characterizing" (Bohr 1935b, p. 700).

An arguably even more decisive mathematical move of Heisenberg was the discovery of the different nature of these new variables themselves and of the way to multiply them as variables used in equations, which proved to be those of matrix algebra. Indeed, physically determined by the Rydberg–Ritz combination rules for frequencies of atomic radiation, in Heisenberg's logic this step *preceded* the general noncommutativity of his new quantum theoretical variables. He noted that this general noncommutativity was a consequence of his multiplication rule (which he applied in the paper to commuting multiplications, actually degrees of the same variable), but he did not use it in the paper itself, since the particular physical case he considered, that of an aharmonic oscillator, did not require it.[10] Intriguingly, Pauli saw this noncommutativity as the most problematic feature of Heisenberg's new mechanics and thought that it should be removed from the theory. On the contrary, it proved to be a decisive feature of quantum mechanics, as Pauli, too, came to appreciate, following both Born and Dirac, in part given that Schrödinger's equation (which Pauli appears to have been the first to prove to be mathematically equivalent to Heisenberg's scheme) implied this feature as well. Still, one might argue that, as concerns Heisenberg's *invention* of quantum mechanics, discovering the matrix-like character of his new kinematical elements and finding how to *multiply* them formed (jointly) the most crucial step. Noncommutativity followed automatically. Born and Jordan noted this in their paper: "The mathematical basis of Heisenberg's treatment is the *law of multiplication* of [new, matrix-like] quantum-theoretical quantities, which he derived from an ingenious consideration of correspondence arguments. ... [T]his rule of multiplication is none other than the well-known mathematical rule of *matrix multiplication*" (Born and Jordan 1925, *SQM*, p. 278). Of course, this multiplication is, in general, noncommutative.

The scheme, Heisenberg noted, contains "a complete determination not only of frequencies and energy values [in accordance with Bohr's formulas], but also of quantum-theoretical transition probabilities" (Heisenberg 1925, *SQM*, p. 268). This use of transition probabilities is yet another decisive step, epistemologically the most decisive step, toward his proper quantum mechanics. This calculation of the transition probabilities from amplitudes is, as I said, essentially equivalent to Born's rule for Schrödinger's wave function, albeit only in this special case. It is, accordingly, not surprising that Born arrived at his rule, specifically in his investigation of collisions in quantum mechanics, and Heisenberg's assumption is itself indebted to the previous work on dispersion by Kramers, Born, and himself. Born begins his paper with a reference to Heisenberg's quantum mechanics, but "has so far been applied exclusively to the calculation of stationary states and vibration amplitudes associated with transitions [between such states]. (I purposely avoid the word[s] 'transition probabilities')" (Born 1926a, p. 52). Born extends the idea to all quantum mechanics—the discovery that justly, although belatedly (in 1954), brought him a Nobel Prize. The right formula itself famously occurs in a footnote (Born 1926a, p. 54,

[10] Technically, this is not true. While the equation itself used by Heisenberg did not involve noncommuting variables, Heisenberg's integration of this equation in effect contained an instance of noncommutative matrix multiplication, as Dirac noted. Thus, Heisenberg could not ultimately avoid noncommutativity even in his original paper (see *MR* 4, p. 129).

note). This extension was crucial and gave the probabilistic character of the quantum mechanical formalism a much greater quantum theoretical generality and field of application. On the other hand, without diminishing Born's contribution, one can understand that Heisenberg and others in Copenhagen at the time were, to Born's chagrin, neither surprised nor, it appears, especially impressed with Born's discovery. Indeed, they appear to value more highly his role in developing Heisenberg's original insights into a full-fledged matrix mechanics. From the present perspective, however, Born's probabilistic interpretation of the wave function was a major move, more so than the Copenhagen crowd gave him credit for, especially because, as was indicated above and as will be discussed in detail in the next two chapters, it gave Schrödinger's wave function the Heisenbergian epistemology.

With these developments in mind, one might appreciate why Bohr invokes "individual processes" in his initial response to Heisenberg's discovery of quantum mechanics and throughout his work on complementarity. Given the failures of the old quantum theory in this respect, at stake was the first *mechanics* of quantum processes, that is, a theory *relating to individual processes* at the quantum level. This possibility of relating the formalism to individual processes, namely motions, defines classical *mechanics*, as opposed to classical statistical physics, although we sometimes speak of (classical) statistical mechanics, usually implying underlying individual mechanical processes. The new quantum mechanics was fundamentally different in that, in contrast to classical mechanics, it did not describe individual quantum processes, but only predicted the outcomes of experiments or *events* involving such processes. By the same token, it was a *probabilistic* theory of these *individual processes and events*, and in this respect had a Bayesian flavor from the outset. I speak of "events" because, given these circumstances, one can, at least in the present, nonclassical, view, speak of these processes as *individual* only in the sense that in each case such a process leads to an individual event of measurement or prediction. The actual physical character of such processes themselves, beyond our description or conception in any event, may be or may not be "individual" in any sense we can give to the term.[11] To the degree that *physical* kinematical and dynamic properties are involved at all, they are only those of certain parts of measuring instruments, where, for example, atomic spectra considered by Heisenberg are registered.

In sum, Heisenberg abandoned the project that defined classical physics: that of describing the behavior of physical objects by means of idealized and mathematized models. He replaced it with the project of using a mathematical formalism to predict the outcome of possible experiments on the basis of certain previous performed experiments. In a proper correspondence with the experimental evidence available at the time (and still in place now), such predictions

[11] As I shall discuss in the Conclusion, in the case of quantum field theory, it becomes especially difficult to think of such processes themselves as individual, even though an outcome is always an individual event in each case and is indeed unique each time.

were probabilistic in nature. Heisenberg was able to pursue his project by introducing an entirely new type of variables, a new calculus, by re-inventing matrices and the rules for their multiplication, which also led to the introduction of noncommutativity into theoretical physics. In his Chicago lectures, Heisenberg describes his new mechanics as "a *calculus* of observable quantities" (Heisenberg 1930, p. 109; emphasis added).[12] This invention of new variables brought physics, mathematics, and philosophy into a new (as against classical physics) type of relationships. Each of the steps just mentioned is remarkable enough. Jointly, they constitute a truly extraordinary accomplishment, rivaling or in any event standing its ground against those of Newton and Einstein.

3.2 The Founding Physical and Philosophical Principles of Heisenberg's Quantum Mechanics

This section considers the key ingredients of Heisenberg's work just outlined as specifically manifest in his famous paper introducing quantum mechanics, "On Quantum-Theoretical Re-Interpretation of Kinematic and Mechanical Relation[ship]s," one of the most important papers of twentieth-century physics (Heisenberg 1925). I begin by offering several qualifications, in part indicated earlier, concerning Heisenberg's insistence on grounding his calculus only in the "quantities which in principle are observable." I shall refer to this grounding principle as the "principle of observable quantities," in parallel with Bohr's "correspondence principle," which was equally crucial for Heisenberg's paper. Were the phrase "the Mach principle" not already used to describe Mach's view of inertia as an effect of distant material bodies, the principle of observable quantities could be called "the Mach principle." Mach was primarily responsible for the introduction of the principle of observable quantities as an epistemological principle of physics. It is via Mach that this principle shaped both Einstein's work on special relativity and Heisenberg's and others' work on quantum mechanics.[13]

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[12] Dirac also saw the development of his q-number formalism (q standing for quantum), inspired by Heisenberg's work, as that of a new calculus. Dirac introduced his q-numbers formally as certain quantities obeying the same rules, most especially those of noncommutative multiplications, that apply to Heisenberg's matrix elements. In order to apply them in practice, one would need to find a suitable representation of such entities, for example, in terms of the matrix scheme as developed by Born, Heisenberg, and Jordan. However, they also allow for other representations. For example, they may be seen as replacing classical wave representations of classical physics in the same way that Heisenberg's matrices replace classical coordinate functions in his mechanics. The approach allows one to connect both Heisenberg's and Schrödinger's theories within the same scheme, which Dirac indeed accomplished shortly thereafter in his transformation theory, independently discovered by Jordan as well.

[13] See the discussion by Mehra and Rechenberg (*MR* 3, pp. 273–290).

The meaning and application of the principle of observable quantities appear to have been somewhat equivocal in Heisenberg's work. It is worth noting first that, as indicated earlier, Heisenberg's *theory* itself, qua theory, was *not founded* only or even primarily on such quantities but on rather complex relationships between observation and theory. This complexity transpires already in Heisenberg's famous opening statement (abstract), "The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon *relationships* between quantities which are *in principle* observable" (Heisenberg 1925, *SQM*, p. 261; emphasis added). "Relationships" is the key word here, and it is also found in the title of the paper, "On Quantum-Theoretical Re-Interpretation of Kinematic and Mechanical *Relation[ship]s*" (emphasis added). Born and Dirac were the first to realize the significance of these *relationships*, as encoded in Heisenberg's kinematical elements as matrix elements. "In principle" is a crucial qualification, too. For, no matter how complicated or theory-laden our processes of observation may be, the quantities in question could, in principle, be *observed* and "kinematically," that is, *mathematically*, *related to each other* by Heisenberg's theory in terms of probabilities of our predictions concerning their values. Moreover, as I said, all the available physical data are only manifested in the measuring instruments involved. The quantities themselves in question were at the time and for quite a while afterward assumed by Heisenberg and others, including Bohr, to be associated (eventually under the constraints of the uncertainty relations) with properties of quantum objects, at least at the time of measurement. However, the epistemological difficulties involved in this association proved to be formidable and eventually led Bohr to abandon this association altogether. Most crucially for the moment, one needed to mathematize such quantities and the relationships between them within a given theory, *analogously* to the ways classical physics uses function and calculus to mathematize classical measurable quantities. This was what Heisenberg accomplished by redefining the corresponding variables in terms of particularly arranged ensembles, namely matrices.

As explained above, these ensembles comprised probability "amplitudes," from which one can derive the probabilities of transitions from one stationary state to another in terms of the numerical outcomes of experiments, such as observable spectra, given a numerical representation. This, again, means that Heisenberg's scheme was that of the *predictions* of the outcome of experiments, in this case manifested as spectra, and the description of the behavior of electrons. In particular, while such stationary states were represented by classical orbits in the old quantum theory (quantum jumps between such orbits had no physical representation), Heisenberg's theory did not contain quantities corresponding to the behavior of electrons in stationary states either; it represented only the probabilities of possible jumps. These connections, established in an ultimately ad hoc manner, amount to a Born-like rule. In other words, even the observable quantities in question are *not* (quantitatively) described by or linked to the *physical* variables described by the formalism but are only *predicted* by the theory or, of course, used by it in order to make predictions.

Once we deal with the transition between two stationary states, rather than with a description of a stationary state of an electron, matrices appear naturally, with rows and columns linked to each state respectively, although this "naturalness" became apparent, or one might say, became *natural*, only in retrospect.

The overall situation just described, its probabilistic aspects included, explains why, rather than referring to actual physical properties or quantities, the term "observables" came to designate these matrices or similar mathematical entities, such as the corresponding operators in a Hilbert space, or still more abstract mathematical entities, such as elements of a certain associative algebra, *C*-algebra*. Given Israel Gelfand's findings and then the theorem of Gelfand, Naimark, and Segal, there is always such a representation of this algebra as an operator algebra in a Hilbert space. This circumstance allows one to use either formalism. Operators are mathematical objects that transform one element of a Hilbert space into another, and such elements themselves are, in this case, functions of variables, similar to the functions, $f(x)$, used in classical physics, except that in quantum mechanics the variables involved are complex quantities ($a + bi$) rather than real ones. This, as noted earlier, is a crucial difference, as is of course the difference in representing observables themselves, kinematic elements, by operators, rather than by functions (again, of real variables) as in classical physics. Thus, the two respective formalisms are essentially different mathematically, and this difference is correlative to the difference between the two types of theory in terms of physics and epistemology. The connections to probabilities and measurements, which are always positive real numbers (less than or equal to 1 in the case of probabilities), are established by Born's square-moduli rule or analogous rules that convert complex quantities into real ones. The term "observable" is, I would argue, somewhat imprecise, and this has often led and still does lead to ambiguities and problems. Analogous problems arise in the case of the "quantum state" terminology, applied to the wave function or the corresponding Hilbert-space vectors. It is worth noting that, unlike in Schrödinger's formalism, in Heisenberg's matrix approach this *type* of mathematical object (the Hilbert-space formalism was developed later) played no role, which fact reflected the difference in their approach between a representation of quantum dynamics itself, which is unobservable, in Schrödinger, and a representation directly linked to observable quantities in Heisenberg. Ultimately, however, Schrödinger's approach only amounts to probabilistic predictions as well, at least in most interpretations. I shall return to these complexities in Chapters 4 and 5. It is crucial, however, that throughout its history quantum theory was dealing with objects not physically observable as such, even in principle—although it took a while to realize this, since initially quantum objects, such as electrons, were presumed to be (classically) observable. Once again, however, what is in principle unobservable or even inconceivable can—and in quantum mechanics does—play a crucial role in what can be observed, conceptualized, and theorized.

This argumentation further supports my point that *dealing* with "quantities which are in principle observable" in terms of the *relationships between them* is not the same as *founding* the theory, qua theory, *only* on these quantities and their relationships. Heisenberg was also working with the available and, from the viewpoint of classical physics, difficult or peculiar data (observable quantities) of quantum physics, such as the Rydberg–Ritz combination rules for frequencies of atomic radiation and the Bohr frequency relations. On these grounds, the principle of observable quantities was well justified insofar as it implied dispensing with the apparently useless baggage of classical physics and those of its observable quantities—in particular, those of orbital motion—that hindered the development of quantum mechanics. The quantum theoretical quantities just mentioned become the central concern of the theory. In this regard, Heisenberg's approach was, again, expressly different from that of Schrödinger, whose scheme was loaded with unobservable classical features, although in this case those of wave (rather than particle) motion. However, Heisenberg's theory, qua theory, was thus *founded* on several *theoretical principles* (physical, mathematical, and philosophical) and was developed through a number of key steps, from finding the new type of physical variables by (re)arranging the observable quantities in question and developing proper rules for mathematically (algebraically) dealing with them to incorporating Bohr's correspondence principle. The principle of observable quantities was one of these principles as well, but not necessarily the most important one, in spite of Heisenberg's emphasis on it—nor perhaps was even the correspondence principle, which guided Heisenberg's work on his new mechanics and to which his theory in turn gave a more precise meaning.

On the other hand, Heisenberg's discovery that the mathematical elements encoding the probabilities of transitions between stationary states could be arranged into two-dimensional arrays, matrices, might well have been the most important step. As I argue here, this arrangement was itself a founding theoretical move. It was an instance of building a new physical concept by combining physics, mathematics, and philosophy (both the phenomenality of this arrangement and the epistemology defined by his suspension of the space–time behavior of quantum objects). It may have been all the more remarkable given that Heisenberg did not at the time appear to be aware of the existence of matrix algebra. He reinvented or reconstituted this algebra from the physics he had to confront. Even if he were aware of the matrix algebra and used it deliberately, however, this move would still be remarkable in its significance, comparable, I argue here, to Newton's use of functions and calculus and to Einstein's use of tensor calculus in general relativity. The significance of this move of Heisenberg is perhaps so obvious that it is rarely, if ever, thought of in terms of a founding *theoretical* concept, in contrast to the noncommutativity of the multiplication in this algebra. This noncommutativity was an automatic result of the Rydberg–Ritz rules for spectra or, again, more accurately and doing more justice to Heisenberg's originality and thought, their algebraization in terms of multiplication in Heisenberg's algebra. Arranging the elements in question into square tables was a founding theoretical move, nevertheless, and arguably the most crucial one to Heisenberg's discovery.

Indeed, Heisenberg's appeal to the in-principle-observable quantities came into play rather late in his work on his new mechanics, when this mechanics itself was nearly in place, largely with the help of the correspondence principle, in turn, as I said, "sharpened" and ultimately given a new form by Heisenberg in the process. This appeal served more to justify than to arrive at the new theory, as Mehra and Rechenberg rightly point out. They also argue, again rightly, that the role of the correspondence principle was more decisive in Heisenberg's work than the role of the principle of observable quantities. They write

> In June 1925, in the process of sharpening the correspondence principle Heisenberg had finally replaced the mechanical orbits by a set or pattern of transition amplitudes. The latter were none other than what he called the 'quantum-theoretical re-interpretation' of the classical Fourier coefficients of the orbits. Thus, the classical mechanical orbits, though abolished as a valid concept in the description of atomic systems, had been transformed into a quantum-theoretical form. This quantum-theoretical form represented the essential outcome of the previous attempts, by using mechanical '*Ersatz*' models, to bring the classical [semiclassical?] results [of Heisenberg and Kramers] on dispersion phenomena into harmony with the behavior of atomic systems. In the process of re-interpretation and reformulation, the correspondence principle had played a much more crucial role than the principle of using only observable quantities in the theoretical description. Indeed, the latter principle made its way into Heisenberg's formalism *a posteriori*. That is, Heisenberg did not establish his new quantum-theoretical scheme on Ernst Mach's positivistic views; he had heard about them before from his friends and collaborators, but they had not guided his work directly and openly. (*MR* 2, pp. 284–285)

As my discussion here indicates, even the principle of observable quantities worked in Heisenberg's argument in a subtler and more qualified way than this comment might suggest. The *language* of in-principle-observable quantities, although not the role of these quantities themselves as discussed above, loses its significance in Heisenberg's subsequent work, beginning with his uncertainty-relations paper (Heisenberg 1927), as his 1929 Chicago lectures show (Heisenberg 1930). While a properly matrix reformulation of his argument in the lectures, as against his original paper, was a contributing factor, the main reason for this refocusing was Einstein's argument that our concepts and theories decide what could be observed (Heisenberg 1971, p. 63). This view impressed Heisenberg and guided his work on the uncertainty relations.

As the exchange with Heisenberg just cited suggests as well, Einstein himself was previously, but not by this point, an adherent of Mach's philosophy, largely responsible for the principle of observable quantities. Einstein's new principle, "the principle of the theoretical definition of observation," as it may be called, appears to have its origin in his work on the general (rather than special) relativity theory. Einstein's insight is significant insofar as it leads to a questioning of the uncritical use of the idea of observation, the idea that has been a subject of much discussion throughout the history and philosophy of science. He argues, in a Hegelian vein, against the empiricist or positivist "philosophical prejudice," which "consists in the belief that facts by themselves can and should yield scientific knowledge without free conceptual construction."[14] He adds, "Such a misconception is possible because one does not easily become aware of the free choice of such concepts, which, through success and long usage, appear to be immediately connected with the empirical material" (Einstein 1949b, p. 47).

It follows that this view and, with it, the *way* that the principle of the theoretical definition of observation was used (as in general relativity) by Einstein himself entail a form of realist or ontological thinking, although the principle can be used otherwise and is in quantum mechanics. This view also tells us that Einstein's realism was complex and indirect or mediated, and specifically conceptually mediated, rather than naïve in the sense of the immediate mapping of reality, unless, as I said, quantum phenomena and quantum mechanics teach us that all realism is ultimately naïve. This complexity and the philosophical nature of this realism result from the mediation by concepts, yet another Hegelian process, in this context defined by a special role of mathematical concepts and of the mathematization of concepts (physical and philosophical), in accordance with the character of modern physics as a mathematical science of nature. Also, again in accordance with Hegel, this mediation has a historical character insofar as these concepts "*appear* to be *immediately* connected with the empirical material" only "through success and long usage," and hence perhaps only appear to be immediately connected to the empirical material, while in fact retaining this mediation hidden in them. At the same time, according to this view, a physical theory should, ideally, enable one at least to approximate physical reality, specifically in terms of physical properties of the objects in question and processes occurring in space–time. At the very least it ought to offer a descriptive idealization of these objects and processes, along with and indeed enabling the predictive capacity of the theory.

This is of course what quantum theory and specifically Heisenberg's quantum mechanics fundamentally question. In other words, as discussed in the Introduction, the role of concepts, mathematical, physical, and philosophical, remains crucial and indeed irreducible in quantum theory as well, including and in particular to enable the theory to serve as the predictive mechanism for quantum experiments. However, it may no longer be possible to use the theory to reach, even approximately and in idealized form, the quantum constitution of nature, to the degree, again, that such terms as "quantum," "constitution," or

[14] Hegel noted that empiricism forgets at the very least that it uses the word "is." In other words, empiricists take this use for granted, as opposed to seeing the concept of "being" as part of our conceptual thinking that shapes our observations. This critique of empiricism in Hegel, or in Einstein, should be distinguished from Kant's argument for a priori forms of cognition (those that are not derived from experience but are pre-given), with which Einstein strongly disagrees. On the other hand, as indicated in the Introduction, apart from the appeal to a priori forms of cognition (Bohr rejects them as well), Kant's epistemology is close to that of Bohr, albeit not as radical, since it remains classical on the present definition. I shall not enter a further analysis of or adjudication between these various positions, apart from noting that, while open to criticism, Kant's conception of a priori involves considerable subtleties and may not be as easily dismissible as it might appear.

"nature" could still apply. Nor might it be possible to develop an alternative of the type Einstein would like, as Schrödinger was the first to attempt to do with an ironic outcome of creating mathematically the same theory for which he and Einstein had these hopes. The mathematical equivalence of the two theories was a warning sign, eventually confirmed by the failure of Schrödinger's physical program. One cannot, again, be certain that either kind of theory is strictly impossible, although Bell's theorem, the Kochen–Specker theorem, and related findings appear to me to make it unlikely, which is, again, not to say that others may not think otherwise, and quite a few indeed do.

These realist or ontological groundings and aspirations of Einstein's position may help to explain why his critical attitude toward quantum mechanics (which was in place by the time he talked to Heisenberg in 1927) was not swayed by his view just discussed. For, while his view applies to all theoretical innovations in physics, including quantum mechanics, which he clearly acknowledged to be a major innovation, the problem for Einstein, as for Schrödinger, was in manifest epistemological implications of the theory. By 1927, Einstein was well aware of the difficulties of establishing quantum mechanics as a realist theory, that is, a complete realist theory, including in view of the locality considerations, although the latter came into the foreground a bit later. They became dominant for Einstein from that point on. In order to articulate the situation more sharply, it may be helpful to briefly reprise here the nature of concepts and of conceptual innovation in quantum mechanics, as considered in the Introduction, vis-à-vis Einstein's and Schrödinger's conceptual realism.

Quantum mechanics does lead to new concepts, such as complementarity in the narrow sense or the matrix variables of Heisenberg's new kinematics, each of which has a complex conceptual architecture, interactively physical, mathematical, and philosophical. However, the architecture and, correlatively, epistemology of these and other key quantum theoretical concepts, such as Bohr's concept of phenomena, are essentially different from those of classical physical concepts. The latter may be seen, as they were by both Bohr and Heisenberg, as representing a suitably mathematizable refinement of our everyday ideas concerning material bodies and their motion. In the sense of descriptive or, to begin with, intuitively visualizable concepts, such as that of a moving object, no other than classical and in this sense "old" concepts are possible. Or at least, such concepts cannot extend beyond a certain enclosure of classical conceptuality, within which the invention of new concepts still takes place, similar to the way it happened previously. The invention of the classical physical concept of motion is itself an example of such an invention, which took a while, with its arguably most significant stage taking place with Galileo and Newton, who gave this concept its mathematical form. While (similarly to Einstein's general relativity) based on mathematical innovations or new applications of mathematical theories in physics, *physically* quantum mechanics appears to rely fundamentally, if not exclusively, on a new use of old physical concepts. A different use of such classical concepts (for example, a necessarily complementary use of some of them) is necessary in quantum mechanics, whereby they are, moreover, applicable only to the measuring instruments involved, although this use itself also involves new physical and philosophical concepts, such as complementarity in the narrow sense. At the same time, it does not appear possible to develop the concepts through which we can describe or even conceive of quantum objects themselves and their behavior, even by way of idealization. This circumstance severs conceptual connections with and hence any possible conceptual approximation of physical "reality" at the level of the ultimate objects considered by the theory.

Accordingly, the nature of quantum phenomena leads to a very different architecture and epistemology of physical concepts. This architecture is interactively physical, mathematical, and philosophical, but it gives a limited role to the kind of ontological and hence descriptive (in terms of mathematical idealization) concepts Einstein had in mind—although these concepts, too, are interactively physical, mathematical, and philosophical. Most especially, whatever new concepts it might invent, such as those of quantum field theory (not something Einstein liked much either), quantum theory offers no description or model of quantum objects and their behavior. As I noted, relativity, special and especially general, already poses considerable difficulties in dealing with its objects. I shall keep these difficulties (or Einstein's reaction to them) aside at the moment. It is worth noting, however, that these difficulties appeared in spite of Einstein's classical-like theory building in creating relativity, and in this respect the situation is similar to that encountered by Schrödinger with his wave mechanics. Heisenberg's approach was not the kind of theory building that Einstein or his predecessors, such as Maxwell, had so effectively used previously and that led Einstein to his belief in the role of classical-like, ontological field theory in fundamental physics.

Indeed, while Einstein's principle of the theoretical definition of observation helped Heisenberg derive the uncertainty relations, it also appears to have contributed to his paper's partial reinstatement of the classical concepts, such as position and momentum, as associated with the motions of quantum objects themselves. Heisenberg's title, *Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik*, is itself indicative, as is the opening part of Heisenberg's paper, which, again, alludes to Einstein's special relativity for a parallel. The English translation of the title, "The *Physical* Content of Quantum Kinematics and Mechanics" (emphasis added), is misleading. It should read instead "on the *representable* (intuitable) content of quantum-theoretical kinematics and mechanics." The main point here is that, as in special relativity, the space–time consideration could apply, although only partially and specifically only at the time of measurement, to the behavior of quantum objects themselves, thus giving the theory its proper representational or intuitively visualizable content [*anschauliche Inhalt*] (Heisenberg 1927, p. 62). Accordingly, Heisenberg's argument is a partial departure from the position adopted in his initial approach, where any such considerations of properties of quantum objects or their behavior were renounced altogether. At least it may be, since it is not altogether clear how far Heisenberg was ready to renounce all quantum-level ontology in his preceding work on quantum mechanics. It is also understandable. His original paper dealt primarily with spectra and thus only indirectly with the behavior of electrons as such, especially in stationary states, while the uncertainty-relations paper was concerned with the behavior of electrons in general, including in stationary states. Bohr, too, from this point on and until his exchange with EPR and even in his reply to them subscribed to this limited, measurement-defined, ontology of quantum properties.

Even apart from his momentous discovery of the uncertainty relations, Heisenberg's argument in his paper is important, especially in showing the transformation of the representational content in question, which, in contrast to classical physics, is now defined by the limitations introduced by the uncertainty relations. Nevertheless, as I shall discuss in Chapter 6, the problems just outlined appear unavoidable in the type of more representational approach adopted by Heisenberg, even with the uncertainty relations in place. By contrast, Heisenberg's initial approach defined new quantum mechanics as a theory of probabilistic predictions concerning the outcome of future possible experiments on the basis of previously performed experiments and not as a theory of a space–time description, especially a causal one, of quantum objects and their behavior. In classical mechanics, predictions are possible because this type of description is possible; in quantum mechanics, predictions are possible in the absence of such a description, and perhaps even because of this impossibility.

3.3 The Correspondence Principle Between Physics and Mathematics

The correspondence principle was introduced by Bohr in the context of the old quantum theory. It states, more or less (there does not appear to be a single defining formulation), that the predictions of both quantum and classical mechanics should coincide in the situations where classical physics could also be used—for example, for the large quantum numbers for an electron in the atom, in other words, when electrons are far from the nucleus. This, as Bohr often stressed, does not mean that the actual behavior of electrons would now change and become classical, but only that it can be sufficiently approximated classically. Heisenberg explains the situation in his uncertainty-relations paper—now, however, in the context of quantum mechanics, which changed the way the principle was used or understood (Heisenberg 1927, pp. 72–76). For Heisenberg's work on quantum mechanics not only crucially depended on and effectively used the correspondence principle but also gave a new, a sharper and more rigorous, meaning to the principle itself. This new meaning emerged by virtue of the fact that Heisenberg's *new kinematics* formally adopted the equations of classical physics while changing the variables to which the equations applied. I shall designate this meaning of the principle, as defined by the formal, mathematical *correspondence* of the equations of classical and quantum mechanics, as "mathematical," in contradistinction to the previous understanding of the principle, which was more physically oriented insofar as the old quantum theory tried to adjust the equations of classical mechanics, while using the same variables—that is, the *old kinematics*.

This "mathematical" reinterpretation of the correspondence principle was decisive to Heisenberg's work and to the development of quantum mechanics, even though the latter, once fully in place, virtually no longer needed the principle itself. Some (Born, for example) saw this modification itself as a move beyond the correspondence principle, as to some degree did Dirac, although Dirac spoke, more cautiously, of the use of the mathematical correspondence in question as "exhausting the content of the correspondence principle" (Dirac 1925, p. 315). Dirac's work was a powerful enactment of Heisenberg's program of using the equations of classical mechanics, now the Hamiltonian equations, while introducing new quantum variables, in his case the so-called q-numbers. As I shall discuss below, there was an argument on this point between Born and Heisenberg in writing the three-man paper on matrix mechanics (Born et al. 1926). Born contended that, unlike the old quantum theory, matrix mechanics was not based on the correspondence principle. Heisenberg had good reasons to argue otherwise, even though and because the use of the principle was now very different: Quantum mechanics became possible because classical mechanics supplied the equations one needed, although it did not supply the variables one needed. This mathematical reinterpretation of the correspondence principle arose from and manifested a radical departure from the descriptively oriented thinking of the old quantum theory, defined by the classical variables used there, toward dealing, via new variables, strictly with probabilities of predictions concerning what is observed in quantum experiments.

On the other hand, while still grounding the correspondence principle in this mathematical correspondence of the equations of classical and quantum theory, Heisenberg's "representational" approach in his uncertainty-relations paper of 1927 and in other later works involves an extension of the principle by relating it to the mental pictures used for the purposes of visualization in quantum mechanics. This extension appears to contrast with Bohr's view of the principle at this stage, defined primarily by its mathematical form. It is worthwhile to consider Heisenberg's representational view of the principle in order to sharpen the epistemological questions involved. This view is expressed at the outset of Heisenberg's important "Appendix" to his Chicago lectures, "The Mathematical Apparatus of the Quantum Theory." As his comments on the correspondence principle show, much more than only mathematics is at stake in this "Appendix"—which is thus also much more than an appendix. Heisenberg still sees the correspondence principle as, in addition to "empirical facts," the main source for "the derivation of the mathematical scheme of the quantum theory." He also adds something else, however: "The correspondence principle, which is due to Bohr, postulates a detailed analogy between the quantum theory and the classical theory appropriate to the mental picture employed. This analogy does not merely serve as a guide to the discovery of formal laws; its special value is that it furnishes the interpretation of the laws that are found in terms of the mental picture used" (Heisenberg 1930, p. 105).

This might be close to the view of the correspondence principle that Bohr adopted in his work on the old quantum theory, to which Heisenberg refers (Heisenberg 1930, p. 105, n. 3), although it is not clear whether Bohr would have subscribed to this particular formulation of the principle even then. In any event, this type of view is no longer found in Bohr's writing following Heisenberg's discovery of quantum mechanics. That includes even the Como lecture, although in other respects the latter, again, retreats from Bohr's views expressed in "Atomic Theory and Mechanics" in the immediate wake of Heisenberg's discovery. Both arguments—that of Bohr in the Como lecture and that of Heisenberg in his uncertainty-relations paper—return to the idea that one can apply physical attributes to quantum objects and processes, even if in a limited way (defined by the uncertainty relations) and only at the time of measurement. This idea in part translates into Heisenberg's view of the correspondence principle as "furnishing the interpretation of the laws that are found in terms of the mental picture used," a view, again, no longer entertained by Bohr by that point. Bohr might have agreed that the application of the principle might depend on whether the classical domain in question is that of wave physics or that of particle physics. That, however, no longer translates into the corresponding mental pictures at the quantum level, since such pictures are no longer possible, except of course heuristically.

Like Heisenberg's works just cited, Bohr's "Atomic Theory and Mechanics" acknowledges the significance of "the visualization of the stationary states by mechanical pictures." Unlike Heisenberg, however, Bohr "strongly emphasize[s]" "the symbolic character of these pictures" and their essential insufficiency (Bohr 1925b, *PWNB* 1, pp. 36, 42–47). As I said, by 1925, even before Heisenberg's introduction of quantum mechanics, but after the collapse of the BKS scheme, Bohr expressed his assessment that quantum processes "cannot be estimated within the ordinary space–time description" (Letter to Heisenberg, April 18, 1925, Bohr 1972–1996, vol. 5, pp. 79–80). In a letter to Born he goes further: "[Quantum experiments] preclude the possibility of a simple description of the physical occurrences [at the quantum level] by means of visualizable pictures ... [S]uch pictures are of even more limited applicability than is ordinarily supposed. This is of course almost a purely negative assertion, but I feel that ... one must have recourse to symbolic analogies to an even greater extent than hitherto. Just recently I have been racking my brain to dream up such analogies" (Letter to Born, 1 May 1925, Bohr 1972–1996, vol. 5, p. 311). As I said, with the collapse of the BKS scheme, Bohr was *ready* for Heisenberg's new mechanics, and he indeed associated his hopes for a breakthrough amidst the stalemate of the old quantum theory with Heisenberg. Heisenberg's theory provided this type of symbolic analogy by its mathematical scheme, and Bohr was to refer to quantum mechanics as symbolic in this sense from this point on. On the other hand, he came to consider any visualization of quantum processes as impossible altogether, opening the way to a nonclassical understanding of quantum phenomena and quantum mechanics. By 1926 he thought that "the possibility of obtaining a space–time picture based on our *usual* conceptions becomes ever more hopeless" (Letter to Slater, January 28, 1926, Bohr 1972–1996, vol. 5, p. 297; emphasis added). Influenced by Schrödinger's wave mechanics, Dirac's transformation theory, and Heisenberg's uncertainty-relations paper, the Como argument was a brief return to causality, but without visualization. As I noted earlier, according to Wittgenstein, no visualization or intuitive conception (*Anschaulichkeit*) of a given process may be possible without causality (Wittgenstein 1985, p. 179). The reverse, however, is not true. Causality is possible without visualization. It can, for example, be mapped mathematically instead. In any event, by 1928–1929 Bohr abandons all causality in his approach to quantum theory.

Bohr refers to the previous use of the correspondence principle, more along the lines of "mental pictures" and Heisenberg's statement under discussion, only in commenting on the *difficulty* of applying mechanical pictures at the quantum level (Bohr 1925b, *PWNB* 1, p. 47). His formulation of the correspondence principle itself is even more careful and, it is worth noting, is linked to "the probabilities of the transition processes." Bohr writes, "The correspondence principle expresses the tendency to utilize in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the postulates [of the quantum theory] and the classical theories" (Bohr 1925b, *PWNB* 1, p. 37). Then, he adds, again, in the context of Heisenberg's use of the principle in his discovery of quantum mechanics, "The whole apparatus of the quantum mechanics can be regarded as a precise formulation of the tendencies embodied in the correspondence principle" (Bohr 1925b, *PWNB* 1, p. 49). These are important statements, and the ideas they advance were used by Heisenberg himself in the three-man paper on matrix mechanics, co-written by him with Born and Jordan, before his paper on the uncertainty relations and the Chicago lecture. According to this paper (in a statement apparently written by Heisenberg), "the new theory . . . can itself be regarded as an exact formulation of Bohr's correspondence considerations" (Born et al. 1926, *SQM*, p. 322). The correspondence principle is seen as "embodying" certain *tendencies* or *principles*, which must be given a precise and—as Bohr says elsewhere, also, as against "furnish[ing] the interpretation of the laws that are found in terms of the mental picture used" invoked by Heisenberg—"*quantitative* formulation of this correspondence consideration" by means of a rigorous quantum theory (Bohr 1927, *PWNB* 1, p. 84; emphasis added).

Heisenberg's new mechanics achieved precisely this formulation. In particular, as I said, if the physical aspects of the correspondence principle were used in the argument for large quantum numbers, the quantum mechanical and classical predictions would coincide. Heisenberg retains this aspect of the principle in his uncertainty-relations paper and the Chicago lectures as well, but, again, supplements them by the view of it as a tool for a possible use of mental pictures for the purposes of interpretation (Bohr 1927, *PWNB* 1, p. 85; Heisenberg 1927, *QTM*, pp. 72–76; Heisenberg 1930, p. 32). For Bohr, the situation is different. It is, at most, only in those regions where one can use classical physics predictively that one might be able to use classical descriptive idealization in terms of mental pictures, such as those of wave or particle motion. However, that does not amount to a mental picture or visualization of the actual motion of electrons, even in these regions, since the processes in question are quantum there as well. From the time of Heisenberg's discovery on, there are no longer any mental pictures applicable to quantum behavior in Bohr's writings, nor any interpretation of quantum laws in terms of such pictures, as invoked by Heisenberg in the passage from the Chicago lectures cited above.

Heisenberg's Chicago lectures remain too tied to a possible (equivalent) description of quantum behavior in either wave or particle terms, which, again, need not (and apparently for Heisenberg does not) imply the wave–particle complementarity. That might be close to Bohr's Como argument, which influenced the lectures, as Heisenberg acknowledged. As I said, however, even in the Como lecture, Bohr's view of the correspondence principle follows his view in "Atomic Theory and Mechanics," based on Heisenberg's original paper on quantum mechanics, rather than the uncertainty-relations paper, to which the Como argument is otherwise indebted. In this view, all mental pictures are abandoned altogether at the quantum level, and the correspondence principle is reinterpreted more mathematically with this renunciation in mind. The Como lecture also extends this view to Schrödinger's theory, although the correspondence principle is mainly connected to Heisenberg's discovery and version of quantum mechanics, beginning with the title of the section "Correspondence Principle and Matrix Mechanics" on this discovery (Bohr 1927, *PWNB* 1, p. 74). The principle is again primarily related to a view of "the quantum theory as a rational generalization of the classical theories" and to Heisenberg's "direct quantum-theoretical transcription of the fundamental equations of classical mechanics," which led to his discovery (Bohr 1927, *PWNB* 1, pp. 70–71). The aid of "the classical pictures" is invoked only when the application of the correspondence principle relates to the cases where classical and quantum mechanical methods are equally applicable. However, since all the processes in question are quantum, the use of classical description is provisional even in this case and—which is my main point here—otherwise not possible at all. Bohr says

> In view of the asymptotic connection of atomic properties with classical electrodynamics, demanded by the correspondence principle, the reciprocal exclusion of the conception of stationary states and the description of the behavior of individual particles in the atom might be regarded as a difficulty. In fact, the connection in question means that in the limit of large quantum numbers where the relative difference between adjacent stationary states vanishes asymptotically, mechanical pictures of electronic motion may be rationally utilized. It must be emphasized, however, that this connection cannot be regarded as a gradual transition towards classical theory in the sense that the quantum postulate [the role of Planck's constant, *h*] would lose its significance for high quantum numbers. On the contrary, the conclusions obtained from the correspondence principle with the aid of classical pictures depend just upon the assumptions that the conception[s?] of stationary states and of individual transition processes are maintained even in this limit. (Bohr 1927, *PWNB* 1, p. 85)

Bohr's "Introductory Survey," again, speaks (in the context of "Atomic Theory and Mechanics") of the correspondence principle, as used especially prior to quantum mechanics, as expressing "our endeavours to utilize all the classical concepts by giving them a suitable quantum-theoretical re-interpretation." He also states, however, that "the detailed analysis of the experimental data from this point of view was, however, destined to show more and more clearly that we did not then possess sufficiently adequate expedients for carrying out a strict description based upon the correspondence principle" (Bohr 1929b, *PWNB* 1, p. 8).

Thus, as Bohr develops his views of quantum mechanics, the correspondence principle, while retaining its significance, becomes—as against Heisenberg's arguments in his uncertainty-relations paper and in the Chicago lectures—less and less linked to the possibility of using any mental pictures in considering quantum processes. It is even used in order to bypass such pictures altogether. In other words, while the principle still serves the purpose of giving classical concepts "a suitable quantum theoretical re-interpretation," it no longer serves the purposes of visualization or any conceptualization, however partial, of the behavior of quantum objects in the classical-like models—wave-like, particle-like, or any other. The impossibility of this visualization becomes one of the central themes of the essays collected in *Atomic Theory and Description of Nature* (*PWNB* 1), where this impossibility is discussed in detail (e.g., Bohr 1927, *PWNB* 1, pp. 98–100, 108), and in all of Bohr's subsequent work.

The correspondence principle now functions in two ways. The first, which I am primarily addressing here, is retained from its functioning in Heisenberg's original work on quantum mechanics and may be seen as (more) "mathematical." The second is a related but more physical and subtler use of it, which might also give the principle its most rigorous physical meaning. I shall address this second use of the correspondence principle in Chapter 9. Here I shall merely state its key point. This use is related to the so-called cut [*Schnitt*] between the two domains, where each description, quantum and classical, would respectively apply. This cut is sometimes known as the Heisenberg cut, or the von Neumann–Heisenberg cut in view of its use by von Neumann, which made the idea prominent (e.g., von Neumann 1932, pp. 418–420). The cut is to some degree arbitrary, but only to some degree, since it can only be made, in Bohr's words in his reply to EPR, "within a region where the quantum-mechanical description of the process concerned is *effectively* equivalent with the classical description" (Bohr 1935b, p. 701; emphasis added). I emphasize "effectively" because, as I said, this equivalence does not mean that these processes themselves are classical. From this viewpoint, it may be more accurate to speak of the "accordance principle" rather than the "correspondence principle." The classical and the quantum mechanical description could be seen as *in accord* with each other rather than as *corresponding* to each other.

The mathematical use of the correspondence principle retains the philosophy of maintaining a close proximity between classical and quantum mechanics, which defined quantum theory from the old quantum theory to Heisenberg's original paper to matrix mechanics and, differently, Schrödinger's work. In his paper, Heisenberg "seeks to construct a quantum-mechanical *formalism* corresponding as closely as possible to that of classical mechanics" (Heisenberg 1925, *SQM*, p. 267; emphasis added). The three-man paper or at least an introduction to it (apparently written by Heisenberg) states

> If one reviews the fundamental differences between classical and quantum theory, differences which stem from the basic quantum-theoretical postulates, then the formalism proposed in the two above mentioned publications [Heisenberg's original paper and Born and Jordan's paper] and in this paper, if proved to be correct, would appear to represent a system of quantum mechanics as close to that of classical theory as could reasonably be hoped. In this context we merely recall the validity of energy and momentum conservation laws and the *form* of equations of motions. . . . This similarity of the new theory with classical theory also precludes any question of a separate correspondence principle outside the new theory; rather, the latter can itself be regarded as an exact formulation of Bohr's correspondence considerations. (Born et al. 1926, *SQM*, p. 322; cited in *MR* 3, p. 102; emphasis added; the translation follows *SQM*)

While a crucial common feature of both the classical and the quantum theories, the conservation laws offer, at least in retrospect, a slim basis for physical proximity between these theories. For these laws can, in both theories, be formulated apart from the description of the physical processes in which the quantities in question are conserved. On the other hand, one could take issue with Mehra and Rechenberg, even if not with Born, as concerns the proximity of these formulations to Bohr's view, which they question. They say, "While, in the introduction, Heisenberg still expressed a cautious attitude and tried to link the new quantum-mechanical theory as tightly as possible with the erstwhile procedures of Niels Bohr, the main thrust and spirit of the three-man paper were quite different" (*MR* 3, p. 102). To be sure, whether the style of the three-man paper was that of Born (as the latter contended) or not, the *character* of the new mechanics was quite different from that of Bohr's work along the lines of the old quantum theory (*MR* 3, p. 103). Heisenberg (who was in Copenhagen at the time) knew what he was talking about, however. Bohr's view of the correspondence principle at this point was fully consistent with the spirit and the letter of the new quantum mechanics, as Bohr's statements from "Atomic Theory and Mechanics," cited earlier (and known to Heisenberg), clearly confirm.

Thus, the correspondence principle continued to serve, to return to Heisenberg's formulation in the Chicago lecture, as "a guide for the discovery of formal laws," new physical laws and new kinds of physical laws, in the same way it served Heisenberg in his discovery of quantum mechanics. On the other hand, now contrary to Heisenberg's contention in the Chicago lectures, the principle no longer "furnishes the interpretation of the laws that are found in terms of the mental picture itself," as cited above. Instead, it serves the program of abolishing and even precluding such pictures, which defines Bohr's ultimate (nonclassical) view of quantum phenomena. Whatever physical description or pictures are used are now applied only to measuring instruments—again, apart from heuristic analogies, which we may use in discussions of quantum objects and processes for the sake of economy and convenience of discourse.

Thus, it is true that with the fully developed matrix mechanics in hand, the role of the correspondence principle changes, as Born and Jordan noted in their paper. The paper itself was designed "to build up the entire [matrix] theory self-independently, without invoking assistance from classical theory on the basis of the principle of correspondence" (Born and Jordan 1925, *SQM*, p. 297; cited in *MR* 3, p. 80). One starts instead by postulating a formal structure, defined by the Hamiltonian equations for certain matrix variables. As I noted, Dirac takes a similar approach. In his first paper on quantum mechanics, inspired by Heisenberg's paper, he goes so far as to say, "The correspondence between the quantum and classical theories lies not so much in the limiting agreement when h⇒0 as in the fact that the mathematical operations on the two theories obey in many cases the same [formal] laws" (Dirac 1925, *SQM*, p. 315). In this view, as Mehra and Rechenberg comment, "any further reference to the analogous classical system, which has previously been used again and again together with the correspondence argument, should be superfluous because the real content of Bohr's correspondence principle had already been fully absorbed in the reformulation prescription" (*MR* 3, p. 80). The contention that "Bohr's correspondence principle had already been fully absorbed in the reformulation prescription" is correct and would apply to Schrödinger's equation, to which this matrix scheme is mathematically equivalent, or to Dirac's scheme, as Dirac himself implied in the paper as well (Dirac 1925, p. 315; *MR* 4, pp. 139–140). As will be seen, Schrödinger's equation may be, and was by Schrödinger, derived in a manner similar to that of Born and Jordan, that is, from the classical Hamilton–Jacobi equation, taken as the classical limit of the theory. The mathematical equivalence of the two mechanics suggests itself from so many different angles!

3.4 "Ensembles of Quantities": From Experiment to Mathematics to Physics

In retrospect and even already in the wake of Born and Jordan's article and the three-man paper of Born, Heisenberg, and Jordan, Heisenberg's discovery of quantum mechanics appears to follow and implement a clear logical scheme. This scheme was formulated by Heisenberg in his Chicago lectures, given in 1929 and published a year later (Heisenberg 1930). The classic books by Dirac (Dirac 1930) and von Neumann (von Neumann 1932) were quick to follow, and the first edition of Weyl's *Theory of Groups and Quantum Mechanics* was published in 1928 and the second in 1931 (Weyl 1928). By that time (remarkably fast), quantum mechanics became the standard theory of nonrelativistic quantum phenomena, which it has remained ever since. As such retrospective impressions often are, this impression concerning Heisenberg's discovery is misleading. Heisenberg's path to quantum mechanics was far from smooth; it required extraordinary capabilities and efforts, which as much gave rise to the logical scheme in question as they implemented or guided this scheme.

This is not to say that there were no guiding ideas and principles in the process; quite the contrary, and, while not sufficient to account for Heisenberg's success and for the process that led Heisenberg to his discovery, they were necessary to that success. As Heisenberg says in the Chicago lectures, "For the derivation of the mathematical scheme of the quantum theory, whether based on the wave or the particle pictures, two sources are available: empirical facts and the correspondence principle" (Heisenberg 1930, p. 105). The discussion in the preceding section explains the role of the correspondence principle. However, it also makes clear that Heisenberg's use of the principle was far from straightforward and was accompanied by considerable complexities and adjustments. The relevance of the empirical facts may appear obvious, and in some respects it is. However, the way Heisenberg related his new theory to these facts was altogether unexpected. It is, I argue here, his original and highly nontrivial approach that led him to his discovery of quantum mechanics—with radical implications for physics, mathematics, and philosophy, and for the relationships between and among all three. I would now like to illustrate this point by considering Heisenberg's paper in more detail, including some of its technical mathematical detail. Readers who are unfamiliar with mathematics may skip this section. The key conceptual points at stake were explained in Section 3.1.

After announcing his intent to found his new mechanics on "relationships between quantities which in principle are observable," Heisenberg begins the paper itself by stating what has not been possible in quantum theory so far and what is possible. He says, "[I]n quantum theory it has not been possible to associate the electron with a point in space, considered as a function of time, by means of observable quantities. However, even in quantum theory it is possible to ascribe to an electron the emission of radiation" (Heisenberg 1925, *SQM*, p. 263). Heisenberg then says, "In order to characterize this radiation we first need the frequencies which appear as functions of two variables. In quantum theory these functions are in the form [originally introduced by Bohr]:

$$v(n, n-\alpha) = \frac{1}{h}\left\{W(n) - W(n-\alpha)\right\}$$

and in classical theory in the form

$$v(n, \alpha) = \alpha v(n) = \alpha h(dW/dn)\text{" (Heisenberg 1925, } \textit{SQM}\text{, p. 263).}^{15}$$

This difference leads to a difference between classical and quantum theories as concerns the combination relations for frequencies, which correspond to Rydberg–Ritz combination rules. However, "in order to complete the description of radiation [in accordance with the Fourier representation of kinematic formulas] it is necessary to have not only frequencies but also the amplitudes"

[15] Some of Heisenberg's notations are modified without affecting his actual formulas.

(Heisenberg 1925, *SQM*, p. 263). These "amplitudes," which in quantum theory are no longer amplitudes of any physical, such as orbital, motions, are to be linked to the probabilities of transitions between stationary states. In fact the frequencies of the orbital motions defining the stationary states played no role in Heisenberg's scheme, that is, they were not represented by his new kinematical elements. This is an important point, to which I shall return in Chapter 6. "The amplitudes may be treated as complex vectors, each determined by six independent components, and they determine both the polarization and the phase. As the amplitudes are also functions of the two variables n and α, the corresponding part of the radiation is given by the following expressions:

Quantum-theoretical:

$$\mathrm{Re}\{A(n, n-\alpha)\mathrm{e}^{i\omega(n, n-\alpha)t}\}$$

Classical:

$$\mathrm{Re}\{A_{\alpha}(n)\mathrm{e}^{i\omega(n)\alpha t}\}."$$

The problem—a difficult and, "at first sight," even insurmountable—is now apparent: "[T]he phase contained in A would seem to be devoid of physical significance in quantum theory, since in this theory frequencies are in general not commensurable with their harmonics" (Heisenberg 1925, *SQM*, pp. 263–264). This is the problem that Heisenberg, who starts this sentence with "at first sight," is about to solve in his customary way, noted earlier, of converting the problem into the possibility of a solution. That is, he proceeds by reshaping the theory or inventing a new theory around the problem that appears to be insurmountable and is insurmountable within the old theory. It is a question of changing the perspective completely, which involves the epistemological moves here considered: those to observable quantities, transition probabilities, and so forth. Most of all, the new theory offers the possibility of rigorous predictions of the outcomes of the experiments, even if at the cost of abandoning the physical description of the ultimate objects considered, which, too, is no longer seen as a problem but as a part of and way to the solution. Heisenberg adds, "However, we shall see presently that also in quantum theory the phase has a definitive significance which is *analogous* to its significance in classical theory" (Heisenberg 1925, *SQM*, p. 264; emphasis added). "Analogous" could only mean here that the way it functions mathematically is analogous to the way the classical phase functions mathematically in classical theory (or analogous in accordance with this form of the correspondence principle), for physically there is no analogy. As Heisenberg explains, if one considers "a given quantity $x(t)$ [a coordinate as a function of time] in classical theory, this can be regarded as represented by a set of quantities of the form

$$A_\alpha(n)\mathrm{e}^{i\omega(n)\alpha t},$$

which, depending upon whether the motion is periodic or not, can be combined into a sum or integral which represents $x(t)$:

$$x(n,\ t) = \sum_{-\infty}^{+\infty}{}_\alpha A_\alpha(n)\mathrm{e}^{i\omega(n)\alpha t}$$

or

$$x(n, t) = \int_{-\infty}^{+\infty} A_\alpha(n)\mathrm{e}^{i\omega(n)\alpha t}\mathrm{d}\alpha\text{''(Heisenberg 1925, }\mathit{SQM}\text{, p. 264).}$$

Heisenberg is now poised to make his most decisive and most extraordinary move. He first notes that "a similar combination of the corresponding quantum-theoretical quantities seems to be impossible in a unique manner and therefore not meaningful, in view of the equal weight of the variables n and $n - \alpha$" (Heisenberg 1925, *SQM*, p. 264). "However," he says, "one might readily regard the ensemble of quantities $A(n,\ n - \alpha)\mathrm{e}^{i\omega(n,\ n\ -\ \alpha)t}$ [an infinite square matrix] as a representation of the quantity $x(t)$" (Heisenberg 1925, *SQM*, p. 264). Heisenberg is on the way to his extraordinary conceptual innovation, but he is not yet quite there. The arrangement of the data into square tables is a brilliant and—in retrospect but, again, only in retrospect (since it also changed our view of what is natural in quantum physics)—natural way to connect the relationship (transitions) between two stationary states, and it is already a great concept. However, it does not by itself establish an *algebra* of these arrangements, for which one needs to find the rigorous rules for adding and multiplying these elements—rules without which Heisenberg cannot use his new variables in the equations of the new mechanics. To produce a quantum theoretical interpretation of the equation of motion, as applied to these new variables, Heisenberg needs to be able to construct the powers of such quantities, beginning with $x(t)^2$. The answer in classical theory is of course obvious and, for the reasons just explained, obviously unworkable in quantum theory. Now, "in quantum theory," Heisenberg proposes, "it seems that the simplest and most natural assumption would be to replace [classical Fourier] equations (3) and (4) by

$$B(n,\ n-\beta)\mathrm{e}^{i\omega(n,n-\beta)t} = \sum_{-\infty}^{+\infty}{}_\alpha A(n,\ n-\alpha)A(n-\alpha,\ n-\beta)\mathrm{e}^{i\omega(n,n-\beta)t}$$

or

$$\int_{-\alpha}^{+\alpha} A(n, n-\alpha)A(n-\alpha, n-\beta)e^{i\omega(n,n-\beta)t}d\alpha\text{''(Heisenberg 1925, }\mathit{SQM}\text{, p. 265).}$$

This is the main postulate, the (matrix) multiplication postulate, of Heisenberg's new theory, "and in fact this type of combination is an *almost* necessary consequence of the frequency combination rules" [Equation (1) cited above] (Heisenberg 1925, *SQM*, p. 265; emphasis added). "Almost" is an important word here. While Heisenberg, to some degree, arrives at this postulate in order to get the combination rules right through a complex process of "guessing" (not the best word here)—by manipulating, among other things, the correspondence principle and the data—the justification or derivation is not strictly mathematical. The rule can only be justified by an appeal to experiment, which is the case even for a fully developed matrix (or wave) theory, and in some respects, it is still a guess, both systematic and lucky (Heisenberg 1930, p. 108). This combination of the particular arrangement of the data and the (re)invention through physics of an algebra of multiplying his new variables is his great invention.

The noncommutativity of Heisenberg's new kinematics may be—and, as discussed earlier, usually is—seen as an equally great physical–theoretical discovery, especially in the context of a fully comprehensive quantum mechanical formalism eventually resulting from Heisenberg's initial insights and arguments. Certainly both Born and especially Dirac immediately recognized its fundamental significance. From the perspective of Heisenberg's process of discovering quantum mechanics, however, this noncommutativity is a consequence of his algebraic multiplication postulate (Heisenberg 1925, *SQM*, p. 266). It automatically follows from the character of the new kinematical elements and of the rules for their multiplication designed to conform to the frequency combination rules. However, these kinematical elements themselves and the rules for multiplying them are in place even for commuting variables, such as (and in particular) the square of a given variable. The latter was all that Heisenberg needed in the particular case of the one-dimensional aharmonic oscillator and its Newtonian equation, the only physical case that he treated in his paper by means of his new kinematics and mechanics. As I noted earlier, this is not quite true, since, as Dirac realized, Heisenberg in fact used the noncommutativity in integrating his equation. Heisenberg himself initially saw the generally noncommutative nature of his new kinematical algebra as a potential hindrance threatening the whole project. He was even glad that he did not give up the whole project in view of this unpleasant feature, never encountered in classical mechanics or the old quantum theory. Eventually, aided by Born's recognition that Heisenberg had reinvented the wheel of the matrix algebra and by Born's expertise in the subject, the problem became a way to the solution in terms of a more comprehensive matrix scheme.

In retrospect, following Alain Connes's way of looking at the situation in his noncommutative geometry, which is an extension of Heisenberg's approach to mathematics, one may note that in the classical model the emitted frequencies can be seen as mathematically forming a commutative group. Then, the observable physical quantities defined by these frequencies form an algebra in the mathematical sense of the term, the "convolution algebra" of this group, Γ, which is also commutative. Given the quantum experimental data, as defined by the Rydberg–Ritz combination rules for frequencies, one no longer deals with a group but with what is called, via Alexandre Grothendieck's algebraic geometry, groupoid Δ, having the composition rule (de facto Rydberg–Ritz composition rule) $(i, j) \times (j, k) = (i, k)$. As Connes explains, "The convolution algebra still has meaning when one passes from a group to a groupoid, and the convolution algebra of the groupoid Δ is none other than *the algebra of matrices* since the convolution product may be written

$$(ab)_{(i,k)} = \Sigma_j \, a_{(i,j)} b_{(j,k)}$$

which is identical with the product rule for matrices" (Connes 1994, p. 38). Connes adds a statement that gives Heisenberg's discovery—including as that of mathematics from physics—its arguably ultimate mathematical expression, at least for now. He says, "On replacing the commutative convolution algebra of the group Γ by the noncommutative convolution algebra of the groupoid Δ, *dictated by experimental results*, Heisenberg replaced classical mechanics, in which the observable quantities commute pairwise, by *matrix mechanics*, in which observable quantities as important as position and momentum no longer commute" (Connes 1994, p. 38; emphasis added).

Connes links these ideas to the measure theory and probabilistic and statistical considerations of the type that Born and then von Neumann (who helped to develop the mathematics of the algebras in question) introduced in quantum mechanics. One has of course to qualify the relationships between what is actually, physically, observable and the mathematical elements involved, to which and only to which the concept of commutation or noncommutation could apply, given the new epistemology of Heisenberg's mechanics, as opposed to that of classical mechanics. These relationships may be given a character of natural *geometrical* mapping vs. the predictive relations between quantum algebra and what we observe in space and time in quantum physics. The quantum mechanical probabilistic considerations are clearly implicated here as well. Born's square-moduli rule or equivalent rules are correlative to the noncommutative nature of the algebra or geometry involved, to the degree that this geometry may be seen as geometry, beyond the formal or, hence, rigorously algebraic, or symbolic or metaphorical nature of its "spaces." As I shall explain in more detail later, the spaces of noncommutative geometry are not spaces or space–times of classical physics or, concomitantly, of our spatial or temporal intuition. They imply a new, *algebraic*, concept of "space".

From the perspective developed in the subsequent work of Born, Jordan, and Heisenberg himself, or independently in the work of Dirac, and adopted by Heisenberg, for example, in the Chicago lectures, one can proceed as follows. One starts with postulating the Hamiltonian equations (via the correspondence principle or even more axiomatically) and the frequencies–amplitudes matrices as variables (accompanied by a Born-like rule for deriving probabilities) and derives quantum mechanics accordingly. By contrast, in Heisenberg's original work, this mathematics was *derived* from physics, without (in contrast to classical physics) mathematically describing any physical process. Instead, this mathematics was used to help a purely predictive capacity of the theory—moreover, in general probabilistically predictive. Thus, this mathematics also helped to make probability the logic, and even Bayesian logic, of this new mathematical science, to paraphrase the title of E.T. Jaynes's book on Bayesian probability, *Probability Theory: The Logic of Science* (Jaynes 2003). His physics led Heisenberg first to a matrix-like arrangement of his new quantum mechanical kinematical elements and then to a multiplication rule, in order to get the degrees of such elements. These rules proved to be those of matrix algebra, with the general noncommutativity of the multiplication of different elements being a consequence of this multiplication rule. At the same time, by divorcing his mathematical formalism, his *algebra*, from a physical description of quantum objects themselves and their behavior, from the *geometry* of such description, Heisenberg's scheme established a new kind of relationship between mathematics and physics, and between both of them and nature.

Chapter 4
From Geometry to Algebra in Physics, with Heisenberg

... a geometrical interpretation of such quantum-theoretical phase relations in analogy with those of classical theory seems at present scarcely possible...

—Werner Heisenberg, "On Quantum-Theoretical Re-Interpretation of Kinematic and Mechanical Relations" (Heisenberg 1925, *SQM*, p. 265)

... a new era of mutual stimulation of mechanics and mathematics has commenced.

—Niels Bohr, "Atomic Theory and Mechanics" (Bohr 1925b, *PWNB* 1, p. 51)

Abstract This chapter considers the new relationships between physics and mathematics that emerge with Heisenberg's discovery of matrix mechanics and its development in the work of Born, Jordan, and Heisenberg himself, and in Dirac's version of the formalism. Taking as its point of departure Einstein's view of "the Heisenberg method" as "a purely algebraic method of description of nature," Section 4.1 examines the shift from geometry to algebra in quantum mechanics as a reversal of the philosophy that governed classical mechanics by grounding it mathematically in the *geometrical* description of the behavior of physical objects in space and time. Heisenberg's matrix mechanics abandons any attempts to develop this type of description and instead offers essentially *algebraic* machinery for predicting the outcomes of experiments observed in measuring instruments. By the same token, a new nonrepresentational type of relationship between mathematics and physics is established, compelling Bohr to speak, in the wake of Heisenberg's discovery, of "a new era of mutual stimulation of mechanics and mathematics." Section 4.2 addresses these relationships and their implications.

4.1 "A Purely Algebraic Method of Description of Nature"

The analysis given in Chapter 3 explains the logic of Heisenberg's approach as grounded in his return to the equations of classical mechanics in considering atomic spectra, which was an unexpected starting point of his journey toward his new

A. Plotnitsky, *Epistemology and Probability*, Fundamental Theories of Physics 161,
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mechanics. These equations, however, were only *formally* those of classical mechanics, since they applied to a very different type of mathematical variables, and given the (matrix) nature of these variables, in the case of Heisenberg's "new kinematics" one can no longer rigorously speak of equations of motion. Accordingly, this "new kinematics" was not a kinematics of motion and, hence, was not a kinematics, properly speaking. It was analogous to classical kinematics only insofar as it related certain mathematical elements of the formalism to the experimental data in question. The equations themselves, however, were instead only used to *predict*, probabilistically, the outcome of experiments, such as the probabilities of the transitions ("quantum jumps") of electrons between stationary states, manifest in the corresponding emissions of light quanta and, thus, in spectra. By the same token, no physical justification for such predictions was offered within Heisenberg's scheme, and the same was true for Born's more general rule for the wave function. This aspect of the theory bothered Einstein from the outset. He referred to the scheme as a certain magical trick, "Jacob's pillow," of Göttingen: it was not, or at least not yet, "the real thing" and "does not really bring us any closer to the secret of the 'old one,' " who, Einstein added in his arguably most famous pronouncement, "at any rate is . . . not playing at dice." In fairness, though, Einstein also says, in the same letter to Born, that "quantum mechanics is certainly imposing" and "says a lot" (A Letter to Born, December 4, 1926, Born 2005, p. 88). In any event, Heisenberg's theory was a grand adieu to physics understood, from Galileo or, again, even Aristotle on, as a science that described physical objects and their behavior, and an equally grand welcome to physics as a theory of "prediction without description." One might also say that, since Heisenberg's discovery of quantum mechanics, the history of fundamental physics, and of the relationships between it and mathematics, has been defined by its oscillations between two main poles—Einstein and Heisenberg. They are of course also primarily, and in Einstein's case, just about uniquely (Schrödinger nearly as important as Heisenberg for quantum theory), responsible for two main theories defining fundamental physics now, relativity and quantum theory, which are largely shaped by Einstein's and Heisenberg's respective ways of doing physics and of using mathematics in physics—geometrical and algebraic.

As indicated earlier, equations of classical mechanics were used in the old quantum theory, for example, the Hamilton–Jacobi equation by Sommerfeld, while retaining classical physical variables and the classical kinematics of motion. The difficulties of the old quantum theory, however, led its practitioners to an unwieldy (partly systematic and partly ad hoc) manipulation of mathematical tools, in describing which one could hardly speak anymore of a coherent scheme of equations of motion analogous to that defining classical mechanics.[1] Initially,

[1] Einstein made an intriguing contribution to this part of the old quantum theory in one of his lesser known papers, published in 1917. It contains penetrating insights into the problems of the Bohr–Sommerfeld quantization scheme, in part anticipating recent developments in the so-called "quantum chaos" (Einstein 1917). The paper was cited by de Broglie and mentioned by Schrödinger as the closest work of the old quantum theory to his wave mechanics. For a discussion of Einstein's paper, see Stone (2005).

following Bohr's 1913 theory of the hydrogen atom, classical mechanical models, such as those using Keplerian orbits, and classical mechanics itself were applied to stationary states, as against the transitions (quantum jumps) between such states. Ultimately, however, it was no longer possible to handle even stationary states by classical means and in particular to apply to them the idea of orbits. By the 1920s, in the old quantum theory there were motions of electrons, that is, one used the idea of motion, but one could no longer have quite the same equations as one did in classical mechanics.

The difficulties of applying classical physical concepts, such as orbits, and models even to stationary states brought some practitioners to doubt their validity in atomic theory altogether. Thus, Pauli persistently criticized their use, including by Heisenberg (in his work preceding quantum mechanics), and at some point he remarked that *numbers* seemed *more real than orbits*. His correspondence with Bohr in 1924 clearly indicates his thinking along these lines, now following "Heisenberg's doubts" concerning "the possibility of definite orbits" (Letter to Bohr, 21 February 1924). Pauli extends his critique to "the kinematic concept of motion of the classical theory" and the correspondence principle, which he rightly thought needed to be clarified, vis-à-vis its use in the old quantum theory, which still related the principle to the idea of motion (Letter to Bohr, 12 December 1924).[2] Pauli, however, was customarily too cautious to renounce a physical description at the quantum level altogether and was ambivalent about many aspects of Heisenberg's paper, including, as I noted, the noncommutativity of his new kinematical variables. He initially thought that this particular feature was highly undesirable and needed to be removed from the theory altogether, which may have been necessary if one wanted to restore the mechanical description of quantum behavior. The fully developed matrix scheme grounded noncommutativity more rigorously. Pauli, however, was even more suspicious of matrix mechanics, which he found too mathematical, too much in "the spirit of Göttingen," and he did not have much confidence in it for quite some time, as stressed by Mehra and Rechenberg (*MR* 3, pp. 166–169; *MR* 4, pp. 140–141, n. 176). While, around the same time as Dirac, he noted, just as Dirac did, that the nonzero difference, $XY–YX$, corresponded to the Poisson bracket in classical mechanics, unlike Dirac, he missed the significance of this fact (*MR* 4, pp. 140–141, n. 176). Even more remarkably he rejected (albeit only at this stage) the probabilistic character of quantum mechanics: "I definitely believe that in the fundamental laws of a satisfactory theory the concept [of] 'probability' should not appear" (Letter to Bohr, 17 November 1925; cited in *MR 6*, p. 173). Later on, Pauli was also to miss discovering the uncertainty relations and complementarity, even though, as his correspondence at the time suggests, he sensed both, but still as *problems*,

[2] Both letters are cited by Mehra and Rechenberg (*MR 2*, p. 266), who offer a useful discussion of Pauli's views of Heisenberg's new mechanics (*MR* 2, pp. 262–273).

rather than, as Heisenberg, ways to *solutions*.[3] It was Schrödinger's theory and, as concerns probability, Born's interpretation of the wave function, that compelled him to accept the matrix version, in part given the mathematical equivalence of both, which Pauli was among the first to discover. Schrödinger's theory was more physical and also causal or so it initially appeared.

Defying Pauli's skepticism, it was, however, precisely mathematics, along with the radical nature of Heisenberg's epistemological ideas, that led Heisenberg to his discovery. It was mathematics against, or even at war with, motion, rather than, as in classical physics, in cooperation with motion, and in this respect, a return to Plato, a philosopher of mathematical form, and away from Aristotle, a philosopher and a physicist of motion. This Platonism was of course only partial and qualified, since, as against Plato, Heisenberg's concerns were with the actual physical world, and in this sense Aristotelian, rather than with the ideal world of mathematical concepts. Plato's position in this respect was, however, more complex as well, and he was certainly concerned with how his ideal forms related to the real world. Also, as will be seen below, Plato's thinking was ontological, although this ontology was mental (albeit exceeding the human mind), and it was, correlatively, geometry that was his mathematical model rather than arithmetic (there was no algebra then), while Heisenberg's mathematical thinking was proto-nonclassical and algebraic, and furthermore, probabilistic. Algebra can serve ontology as well, including a motionless ontology of the Platonist type: things do not really "move" in mathematics. But then, nobody was at the time quite as bold and original as Heisenberg in making such a move. In this sense Bohr's appeal to "emancipation" in the Como lecture is not out of place: "Heisenberg . . . succeeded in emancipating himself completely from the classical concept of motion" (Bohr 1927, *PWNB* 1, p. 70). Bohr, I argue here, was the

[3] It would be difficult to fault Pauli in this regard, let alone to question his achievements as a major figure of twentieth-century physics. It is also worth noting that, while never shy to criticize Bohr, he offered strong support to Bohr's epistemological ideas and helped to propagate them. Nevertheless, some "misses" on Pauli's part, such as those just mentioned, are intriguing. Earlier Pauli missed the potential discovery of spin, virtually contained in his manipulation of quantum numbers that led him to his exclusion principle. He also initially missed the significance of the EPR argument and related arguments of Einstein, which he just about *dismissed*, even though later on he astutely criticized Born's rejoinder to Einstein as missing Einstein's real point (Born 2005, pp. 216–220). In the early 1950s, Pauli did not pursue or publish his findings concerning what became known as the Yang–Mills theory, a non-abelian gauge theory that forms the foundation of the present-day quantum field theory. One might argue that this miss is, again, due to a refusal to see that the theory was a way to the solution of some of the key problems of quantum field theory, in spite of the difficulties the approach contained, which worried Pauli. It is true that the effectiveness of the Yang–Mills theory became apparent only later; and it is also true that in this case Pauli's creativity, including in his use of mathematics, was of the highest order. His other major contributions to quantum theory, such the exclusion principle or his neutrino proposal, for which he was awarded a Nobel Prize, his arguably greatest contributions were not based on the kind of mathematically innovative thinking that defined the work of Heisenberg and Dirac. As is well known, especially from his criticism of Born, Pauli in general distrusted mathematical thinking in physics.

first to accept Heisenberg's discovery as the way of the future for quantum theory. For different (more mathematical) reasons, both Born and Dirac were not far behind, while, as I said, it took longer for Pauli to come aboard. But then, again, Heisenberg, too, was pushed in this direction only under the pressure of the failures of previous methods in which he was well versed and which he had successfully used previously and even made his reputation in doing so. Ultimately, however, he made this move, and did so decisively. As he wrote to Pauli, who continued to invoke orbits even in commenting on Heisenberg's paper and in praising its achievements (although Pauli, again, remained ambivalent concerning Heisenberg's scheme):

> But I do not know what you mean by orbits that fall into the nucleus. We certainly agree that already the kinematics of quantum theory is totally different from that of classical theory (*hν*-relations), hence I do not see any *geometrically-controllable* sense in the statement "falling into the nucleus." It is really my conviction that an interpretation of the Rydberg formula in terms of circular and elliptical orbits (according to *classical* geometry) does not have the slightest physical significance. And all my wretched efforts are devoted to killing totally the concept of an orbit—which one cannot observe anyway—and replace it by a more suitable one. (Heisenberg to Pauli, 9 July 1925; cited in *MR* 2, p. 284; emphasis added)

I shall explain my emphasis and the significance of Heisenberg's appeal to geometry presently. Mehra and Rechenberg, who cite the passage, note the revolutionary nature of this statement. As they write: "It was not so much the renunciation of the mechanical electron orbits in atoms; Heisenberg had arrived at this conclusion gradually during the course of several years. More serious was the radical denial of mechanical models, for they have provided the backbone of his previous calculations. And finally there was the reference to the philosophical guiding principle about using only observable quantities in physical theories" (*MR* 2, p. 284). Mehra and Rechenberg are right to invoke this more general point of "the radical denial of mechanical models." Indeed, the significance of this point is deeper and more general, rather than contained by the more personal fact that such models "have provided the backbone of his previous calculations." The move illustrates Heisenberg's ability and even propensity to such radical departures from his previous approaches or even philosophy. At the same time, as indicated earlier, such departures were compelled by theoretical necessity. Heisenberg made such moves only when the previous means were exhausted. However, the radical nature of Heisenberg's move cannot be overestimated, especially given how far it ultimately took physics and philosophy in renouncing any form of description of or even conception concerning quantum objects and processes themselves.

In this context, Heisenberg's appeal to a renunciation of classical geometry and its capacity to control what "happens" at the quantum level has a deeper significance. In the paper itself, Heisenberg says that "a *geometrical* interpretation of such quantum-theoretical phase relations in analogy with those of classical theory seems at present scarcely possible" (Heisenberg 1925, *SQM*, p. 265; emphasis added). After Heisenberg, there was no geometry left in the mathematical formalism of quantum theory, apart from its metaphorical use, on which I shall comment below. In any event, there is no physical geometry, but only algebra, both in its technical sense and its broader sense of the formalization of our theories by means of symbolic mathematical entities, equations, and so forth. Einstein was to remark later (in 1936) that "perhaps the success of the Heisenberg method points to a purely algebraic method of description of nature, that is, to the elimination of continuous functions from physics. Then, however, we must give up, in principle, the space–time continuum" (Einstein 1936, p. 378). In many respects, this assessment is justified, although one need not take Einstein's disparaging view of this possibility, especially at this point, given that we are accustomed to this possibility. At the time, Einstein was hardly encouraged or encouraging. For he added: "It is not unimaginable that human ingenuity will some day find methods which will make it possible to proceed along such a path. At present however, such a program looks like an attempt to breathe in empty space" (Einstein 1936, p. 378). A few qualifications are in order, however, even as concern "the Heisenberg method" in quantum mechanics.

First of all, Einstein must have had in mind the space–time continuum at the ultimate level of description, and it would be more accurate to say that the classical properties cannot be applied to quantum objects. The role of space–time itself at this level is, at this stage, beyond the scope of the currently available theories, apart from some, for now hypothetical, proposals, such as those of certain theories of quantum gravity (e.g., those arising from the string and brane theories or the so-called loop quantum gravity). One must also keep in mind the complexity of this "algebra," which involves objects that are in themselves not, in general, discontinuous, although certain key elements involved are no longer continuous functions, such as those in classical physics. Some continuous functions, however, are retained as well, since the Hilbert spaces involved are those of such functions, considered as infinite-dimensional vectors in the case of continuous variables such as position and momentum, which are represented by operators. These functions are those of complex (rather than, as in classical physics, real) variables, which is, again, a crucial factor, and the vector spaces that they comprise or associated objects (such as operator algebras) acquire special properties, such as noncommutativity. Indeed, given that it deals with Hilbert spaces, quantum mechanics involves *mathematical* objects whose continuity is denser than that of regular continua such as the (real number) space–time continuum of classical physics or of Einstein's relativity.[4] In contrast to these theories, however, the continuous

[4] I, again, leave aside the question of nature itself. For example, to what degree do the space–times of relativity correspond to the ultimate structure of nature? How realist is such a description, even as an idealization, as opposed to serving as a mathematical tool for correct predictions (in this case, exact rather than probabilistic)? These questions can also be posed in the case of special relativity, or even classical mechanics, where, however, the descriptive idealizations used are more in accord with our phenomenal experience. For some of these complexities, see (Butterfield and Isham 2001) and other articles in (Callender and Huggett 2001).

and differential mathematics used in quantum theory, along with the discontinuous algebraic one, relates, in terms of probabilistic predictions, to physical discontinuities defining quantum phenomena, which indeed appear as discrete in relation to the observable space–time continuum and to each other. At the same time, at least in a nonclassical interpretation, quantum objects themselves are not subject to any mathematical description, continuous or discontinuous. Accordingly, as noted above, there is also an *epistemological* discontinuity or rupture in this respect as well, between what is observed and quantum objects.

It is also worth noting that the papers on matrix mechanics by Born and Jordan, and the three-man paper by Born, Heisenberg, and Jordan developed a differential calculus, "symbolic differentiation," as they called it, for matrices used in quantum mechanics. So did Dirac in his initial papers on quantum mechanics. This differentiation is, it is true, defined more algebraically (more in the style of Leibniz's approach to calculus than that of Newton) by using the commutation rules.[5] If F is a function, such as energy, of a matrix variable, q, such as momentum, then the differentiation of F by p assumes the form $(2\pi i/h)$ $(Fq–qF)$. The procedure is implicit in Heisenberg's initial calculations, which, in this sense, indeed become *calculus*. This calculus is in effect a differential calculus with matrices. His new matrix variables must enter and, thus, recast the differential equations of classical mechanics and their accompanying machinery, such as and in particular the Poisson bracket. Its quantum mechanical analogue is the expression $(2\pi i/h)$ $(pq–qp)$, as Dirac was first to realize, a discovery that had far-reaching implications for quantum mechanics.

Dirac's starting point (in the style of Leibniz) was a quantum mechanical analogue of the rule for the differential of the product of two functions in the standard differential calculus, which differential may of course be seen as a linear operator. He then proceeded to the general law of quantum

[5] Leibniz's algebraic way of thinking concerning calculus or the nature of thought itself is worth noting here, as part of the history of algebraic thinking that eventually led to the "algebraic method of description of nature." Leibniz's work on the systems of linear equations was one of the origins of the idea of the matrix, and his project of universal characteristic anticipates the project of formalization of mathematics, undertaken from the end of the nineteenth century on. This project was given an expected twist in and, in its highest aspiration (of the possibility of formalizing mathematical thinking), was brought to its end by Gödel's incompleteness theorems. Leibniz's view was Platonist insofar as the formalization by a given form of calculus (differential, propositional, or other) was seen as a way of at least approximating the ultimate reality. However, Leibniz's reversal of the hierarchy of geometry and algebra, which governed the preceding thinking in physics and mathematics, or, from Plato on, in philosophy, was a powerful move, including vis-à-vis Descartes's and Newton's geometrical philosophy. Descartes's analytic geometry, for example, still dealt with the ultimately geometrical reality, in part correlatively with the idea of mechanical motion. Descartes had a concept of a "mechanical" curve. Algebra, say, in the Hamiltonian formalism in classical mechanics, allows one to bypass engaging with geometrical aspects of physical reality, even under the assumption of its geometrical nature. Algebra liberates theoretical physics from dealing with these aspects at least in working with equations and doing calculations.

differentiation, defined by the noncommutativity of the multiplication, and to his introduction of the quantum mechanical Poisson brackets, defined by this noncommutativity. The mathematical linearity of quantum mechanics is manifest in the features under discussion as well, and it is worth noting that the essential point of differential calculus is to replace a given differential function with a linear function, which is possible infinitesimally. The linearity of the quantum mechanical formalism is a crucial feature, and it is so much taken for granted now, that the contemporaneous novelty of this feature and the significance of its discovery, in the case of Schrödinger's equation, by Léon Brillouin, are nearly forgotten.[6]

The features of quantum theory just mentioned have far-reaching implications. In particular, for a mathematically informed reader (this paragraph can be skipped by others without creating difficulties in following my main argument), one might note the relationships between the symmetry groups involved, as Lie groups, and their Lie algebras, and their infinite-dimensional (Hilbert-space) representations. Sophus Lie, who was the first to use the term "Poisson brackets," developed his theory in part by considering the group of infinitesimal contact transformations in classical, Hamiltonian mechanics. As against Heisenberg's original paper (which considered only a Newtonian equation for an anharmonic oscillator), Dirac wanted to develop a Hamiltonian framework for quantum mechanics. He succeeded in doing so, independently of Born and Jordan (who also extended Heisenberg's ideas to the Hamiltonian form of quantum mechanics), and by using a different and ultimately more effective scheme of q-numbers. For the moment, the particular Poisson brackets of a given quantum theory, such as quantum mechanics, may be seen as commutators in the Lie algebra, whose Lie group is a symmetry group of the theory. In the case of the standard quantum mechanics, the group is the so-called Heisenberg group, and the corresponding Hilbert space is that of an irreducible infinite-dimensional representation, while the corresponding Lie algebra is defined by the quantum mechanical Poisson brackets introduced by Dirac. According to the theorem of Stone and von Neumann, there is only one equivalence class of such representations, suitably parametrized by orbits, with q and p as parameters, with $(q, p) \in \mathbf{R}^{2n}$, and with a (nonzero) h as a constant. One derives the actual quantum mechanics by assigning h the value of Planck's constant. (The equivalence of Heisenberg's and Schrödinger's scheme is automatic.) By the same theorem, the finite-dimensional irreducible representations are in fact derived from the infinite-dimensional class by putting h equal to zero (and by taking q and p as fixed), a kind of "correspondence principle." Of these there is, again, only one equivalence class of one-dimensional representations (over complex numbers and, hence, two-dimensional over real numbers). The union of these, with now, again, $(q, p) \in \mathbf{R}^{2n}$, can be seen as a classical phase space with q and p as coordinate and

[6] See, however, Mehra and Rechenberg's discussion of the subject (*MR* 6, pp. 20–36).

momentum variables, and thus gives us classical mechanics. These connections to group theory and, hence, to symmetry became quickly apparent, as the appearance of Weyl's *Theory of Group and Quantum Mechanics* (Weyl 1928) in 1928 testified, and they have been germane to quantum theory ever since. Their significance in quantum field theory and all of elementary particle physics is truly momentous. It may also be noted that Heisenberg's group has other important uses in mathematics.

It follows then that it is not merely a matter of using algebra, as opposed, say, to mathematics that involves differential calculus, such as that of Riemannian or pseudo-Riemannian manifolds of Einstein's relativity, as Einstein's statement might suggest.[7] In short, there is no real mathematical difference between Heisenberg's algebraic and Schrödinger's differential methods, although they initially appeared to be different in this regard and were products of two very different philosophies, reflected in Einstein's comment. Indeed, this comment may be best seen as a comment on algebra as a form of philosophy of physics, since the mathematical equivalence of both schemes had been established for a decade by the time of Einstein's comment. Besides, Einstein speaks of Heisenberg's "*method*," not his product.

The ideas of Born, with whom Einstein corresponded extensively on the subject, and specifically the argument of Born and Jordan's paper on the matrix mechanics might have been one of the sources of Einstein's remarks, and they were among the sources for Schrödinger's opposition of his physically continuous wave mechanics to the (physically) discontinuous matrix mechanics. Both Einstein and Schrödinger thought that the physical and the mathematical continuity or discontinuity are somehow correlative to each other, which is understandable given their representational and realist views of mathematical models in physics. By contrast, the quantum mechanical formalism separates its mathematics, continuous or discontinuous, from either the description of the behavior of quantum objects or the description of the irreducibly discontinuous observed phenomena defined by the effects of the interactions between quantum objects and the measuring instruments upon the latter. According to Born and Jordan, matrix mechanics is "an essentially *discontinuous* theory." They say

> Therein lies the big difference between this and the previously adopted semiclassical methods of determining the stationary states. The classically calculated orbits merge into one another continuously; consequently the quantum orbits selected at a later stage have a particular sequence right from the outset. The new mechanics presents itself as an essentially *discontinuous* theory in that herein there is no question of a

[7] Einstein's broader thoughts on the subject are another matter, however. Einstein pondered the question of geometry in the context of special and general relativity, including in relation to the constitution of our measuring instruments (rods and clocks), throughout his life (e.g., Einstein 1921, 1949a), and his thinking on the subject and its implications would require a treatment beyond my scope here. Cf., (Brown and Pooley 2001) and also Heisenberg's comments (Heisenberg 1989, pp. 82–83).

> sequence of quantum states defined by the physical process, but rather of quantum numbers which are indeed no more than distinguishing indices which can be ordered and normalized according to any practical standpoint whatsoever (e.g., according to increasing energy W_n). (Born and Jordan 1925, p. 879; *SQM*, pp. 300–301; cited in *MR* 3, 83; emphasis added; the translation follows *SQM*)

It is clear that in question here is, again, a *physical*, and *not mathematical*, discontinuity. In other words, it is not so much that quantum mechanics is "an essentially *discontinuous theory*," but rather that it is a theory, partly continuous and partly discontinuous, that relates to phenomena that are essentially discontinuous.

Quantum mechanics is defined mathematically by a very complex and subtle form of algebra, including in its technical sense of matrix algebra, or algebras of Hilbert-space operators, C^*-algebras, and so forth. The *language* and certain *conceptions* of geometry, such as that of Hilbert *spaces*, and certain forms of geometrical intuition may of course apply to this algebra, and were applied to it throughout the history of these concepts, for example, in quantum mechanics by Dirac, and to some degree by Born. This application of the geometrical language and conceptions is useful, in particular, in establishing the relationships between operator algebras and Hilbert spaces. These relationships are manifest, in particular, in Stone's representation theorem (which allows one to construct a "space" from an algebraic structure by identifying certain entities associated with this structure with points and subspaces of this space) and then in the theorems of Gelfand and of Gelfand, Naimark, and Segal, mentioned earlier. These theorems enable us to construct a Hilbert space from a given Banach algebra, which is an algebraic structure: the "points" of this space correspond to the so-called maximal ideals of this algebra. This procedure allows one to traffic between different ways of representing and handling "observables" (operators) and "quantum states" (state vectors) in the mathematical formalism of quantum mechanics (physical states are, again, a separate issue).

These findings have also had major implications for algebraic geometry, from the 1930s to Grothendieck's work and beyond, which uses the same type of construction, and then to Connes' noncommutative geometry introduced in the 1980s, which, as I said, takes Heisenberg's discovery of quantum mechanics as its point of departure. Noncommutative geometry emerges through various mathematical developments just mentioned and their relationships, with far-reaching implications for modern physics, specifically quantum field theory. Via these developments and related mathematical theories, such as, among others, K-theory, Galois's theory, algebraic field theory, elliptical curves and Riemann's ζ-function, and fractal geometry, and the relationships among them, noncommutative geometry extends to and was in part developed in conjunction with quantum field theories.[8] This geometry is called noncommutative because,

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[8] On these connections see Pierre Cartier's article (Cartier 2001). Both Connes and Cartier hold strongly Platonist views. Cartier's Platonism also interestingly manifests itself in Cartier's discussion of physics from Kepler to Bohr in his "*Kepler et la musique du monde*" in the special issue, *Nombres*, of *La Recherche* on numbers (Cartier 1995).

unlike the algebras with commutative multiplication (rings) used in algebraic geometry, the algebras from which the spaces of noncommutative geometry are constructed are defined by the noncommutative nature of multiplication in them.

At the same time, however, these geometries must, as geometries, be seen as *symbolic* even if one leaves aside the fact that when used in quantum theory they, in contrast to the mathematics used in classical physics or relativity, have no descriptive role to play as concerns the physical processes involved. Ultimately they are all forms of *algebra*, as against the more standard forms of geometry, such as Euclidean or even non-Euclidean geometry, or that of manifolds in general, although certain questions concerning the properly geometrical nature of such objects, in contrast to those of Euclidean geometry, can be posed in the latter two cases as well. The symbolic character of such geometries arises in part by virtue of the infinite-dimensional nature of some of those "spaces" or also the fact that they are defined over complex numbers. In what sense, apart from certain essentially *algebraic* properties, may such spaces be *seen* as spaces, in particular in the sense of visualizing, imagining, or intuiting them? The subject is complex and is, as yet, at early stages of investigation in cognitive psychology. It is almost certainly true, however, that, when we visualize such objects, we visualize only three- (and perhaps mostly two-) dimensional configurations and supplement them by algebraic structures and intuitions, as Feynman observed in describing visual intuition in thinking about quantum objects (cited in Schweber 1994, pp. 465–466). These problems arise already for spaces of finite dimensions, once the number of dimensions is more than three, or for spaces of any dimensions, beginning with the complex plane itself, defined over number fields other than that of real numbers, such as those used in complex analysis or algebraic geometry. The latter indeed arrives at such esoteric concepts as spaces without points. This type of argument was made by Robert Langlands in a related mathematical context (Langlands 1990). Langlands refers to quantum physics as a parallel case, where, as I argue here, one has confronted this situation all along. It follows from the preceding remarks that in quantum mechanics we confront a certain double unrepresentability or inconceivability. We relate to something (quantum objects and processes) that is phenomenally inconceivable, is beyond phenomenal intuition (*Anschaulichkeit*) at the physical level, by something (such as the infinite-dimensional Hilbert spaces) that is phenomenally inconceivable at the mathematical level, although the algebraic properties of such objects are well defined.[9] In other words, two ultimately inconceivable domains, physical and mathematical, could be linked, with the help of our experimental technology, within a single theory to enable us to predict the outcomes of experiments, at least in probabilistic terms, which feature may be due to these epistemological conditions.

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[9] The actual architecture of nature may, again, be still something else, thus making the relationships considered here tri-partite, which, however, further amplifies my point here.

By contrast, in classical physics, "algebra" (such as that of differential calculus or partial differential equations of mathematical physics) can be given a proper, even if, again, idealized, meaning as well, and, by the same token, can be linked to the geometrical representation of classical physical processes, as it was done from Galileo and Newton on. Classical physics is defined by this link, the program that culminated in Lagrangian and Hamiltonian mechanics, which were subjected to a quantum mechanical reinterpretation in matrix mechanics or Schrödinger's wave mechanics, for which the Hamiltonian formalism was a starting point as well. Born, Heisenberg, and Jordan commented on this situation in the three-man paper as follows: "In the further development of the theory, an important task will lie in the closer investigation of the nature of this correspondence [i.e., that of the correspondence principle] and in the description of the manner in which *symbolic quantum geometry* goes over into the intuitively visualizable [*anschaulich*] classical geometry" (Born et al. 1926, *SQM*, p. 322; translation modified). We are as yet far from finished with this type of investigation, especially as concerns the *physical* nature of this transition, still an unresolved issue. My main point here is that the situation has far-reaching consequences for our understanding of the relationships between algebra and geometry (in nearly every sense of these terms, and one can of course add analysis and topology to the mix) in physics, defined by these relationships from Galileo or even Aristotle on, or even in mathematics itself.

In particular, it changes the nature of our thinking and even intuition concerning spatiality. To some degree, the practice of using the Hilbert-space formalism and bra- or ket-vector notation in explaining "what is actually going on" in our experiments even by the experimentalists may be seen as a manifestation of this change. This way of seeing the situation and "what is actually going on" also change our understanding of what it means to offer a description of a physical process, as (differently) N. David Mermin and Serge Haroche appear to suggest (Mermin 1998a; Haroche 2001; Haroche and Raimond 2006). Such a change would, however, be consistent with nonclassical epistemology, provided that space–time behavior and classical-like physical concepts can only refer to the effects of the (quantum) interactions between indescribable quantum objects and certain, classically described, parts of measuring instruments. In sum, one cannot see bra- or ket-vectors as representing physical processes, however, indirectly, but only as mathematical entities enabling probabilistic predictions.

Heisenberg's stroke of genius, I argue here, was his discovery of new mathematical elements for his kinematics of quantum mechanics and of the rules of their proper manipulation, specifically of their multiplication, in short the discovery of a new type of physical variables. The character of these elements and of their multiplication was to define everything else, mathematically (in particular the noncommutativity of this multiplication), physically, and epistemologically. Finding these elements was, I also argue, itself a founding theoretical move, and this arrangement of the relationships between observable quantities in infinite matrices of complex, rather than real, numbers (numbers never observable as such) is already a form of *theory*, not of observation of nature, which does not arrange anything in this way, except through us. At least in this respect, Heisenberg's theory indeed defined what is observed and how, in accordance with Einstein's argument that our concepts and theories decide what could be observed, but with an outcome quite different from the one Einstein would have preferred.

Still, Heisenberg reinvented matrix algebra through *physics*, albeit by abandoning the physics of *motion* at the quantum level and by replacing it instead with the probabilities of predictions concerning the data found in manifest (and hence, physically classical) phenomena, in this case, spectra. In this respect of proceeding from physics to the invention of mathematics, the process was different from applying a given mathematical theory, or even developing one already available, say, in the way Riemannian differential geometry was, to some degree, developed by Einstein's relativity. Most of this mathematical development was really done by mathematicians, such as Elie Cartan and Hermann Weyl, following Einstein's work on the physics of relativity. This pattern is also found in the mathematical developments in the fields related to quantum theory, or elsewhere, for example, in the mathematical developments linked to the string theory, in which, however, physicists (mathematically) participated as well, Edward Witten in particular. The distinction itself is not always so easily maintained, although the necessity of rigorous mathematical proof still defines the disciplinarity of mathematics, but no longer physics. Nor was it only or even primarily a matter of introducing a given new mathematical object to help both the mathematics and the physics of a given theory (such as Dirac's delta function in quantum mechanics or spinors in quantum electrodynamics). Instead it was the process of *developing* a new mathematical scheme *from physics*, analogous to the way calculus was developed from classical physical processes—analogous but, again, not identical, since calculus was used descriptively in classical physics. In this sense, Heisenberg's work would be even more momentous, as momentous as that of Newton, at least in physics, and it would be in mathematics, too, were it not for the fact that the corresponding mathematics already existed. As indicated earlier, however, Heisenberg's scheme has led to some new mathematics.

From this perspective, the comparison between Heisenberg and Newton may be made in a somewhat different and stronger form. Heisenberg's approach in fact involves a new differential calculus for matrices, itself defined in terms of commutators, that is, through this noncommutativity, which becomes fundamental and serves, via von Neumann's C^*-algebras, as the basis for Connes' noncommutative geometry, for now the farthest-reaching mathematical generalization of these ideas. The analogue of integration is established by the so-called Dixmier trace, "a general tool designed to treat in a classical manner data of quantum-mechanical nature," and in this respect, a mathematical correlative of Bohr's physical concept of phenomenon and of the complementarity of certain phenomena (Connes 1994, p. 21). In other words, just as Newton linked classical mechanics and calculus, or as Einstein linked gravity and Riemann's tensor calculus in general relativity, Heisenberg linked quantum mechanics and matrix calculus.

The difference was, again, in the epistemology of these connections. In particular, Newton was able, in *Principia*, to present his mechanics in terms of geometry rather than calculus, in part, as he explained, to assure a geometrical *demonstration* of his theorems, also in the direct sense of showing something by means of phenomenal visualization, rather than in terms of the "algebra" of calculus (Newton 1687). As I have indicated, the geometrical or pictorial visualization, intuition [*Anschaulichkeit*] of the equations of calculus is already problematic, even though Newton in his version of calculus, as opposed to that of Leibniz, certainly tried and, by and large, failed to overcome the difficulties involved. However formal it might have been (given that his thinking itself was based on calculus), his return to Euclid and geometry in *Principia* may well be a reflection of, and Newton's reflection on, this situation. In fact, it was algebra that was to become the main means of rigorous demonstration in mathematics. For the moment, the question of pictorial, geometrical visualization was crucial for the development of both Heisenberg's and Schrödinger's versions of quantum mechanics; and, as I argue here, with Bohr and Heisenberg, quantum mechanics may be telling us that no "geometry" of quantum objects and processes may ever be possible in quantum theory. The fact that we have "algebra" that enables us to predict, even if only probabilistically, what can happen in quantum experiments might have to suffice and is miraculous enough.

4.2 "A New Era of Mutual Stimulation of Mechanics and Mathematics"

I began my discussion of Heisenberg's discovery in Chapter 3 with Bohr's comment on its radical epistemological nature in the last section, "The Development of a Rational Quantum Mechanics," of "Atomic Theory and Mechanics." Bohr closes the section and the article with a post script on the role of mathematics in quantum mechanics, a role that he argues to be as significant as it was in all *modern physic*s, from Galileo on, and on the new type of relationships between mathematics and physics that make quantum mechanics depart from all *preceding physics*, from Aristotle on. His comments might be unexpected, given the subsequent trajectory of his thought, especially his insistence on the defining role of measurement rather than on the central significance of mathematics in quantum mechanics. The measuring instruments came to replace "the mathematical *instruments*," which he invokes here, in playing "an essential part," even *the most* essential part, in his interpretation of quantum phenomena and quantum mechanics from the Como lecture on Bohr (1925, *PWNB* 1, p. 51; emphasis added). In 1925, however, in the immediate wake of Heisenberg's discovery, Bohr writes

> It will interest mathematical circles that the *mathematical instruments* created by the higher algebra *play an essential part* in the rational formulation of the new quantum mechanics. Thus, the general proofs of the conservation theorems in Heisenberg's theory carried out by Born and Jordan are based on the use of the theory of matrices, which go back to Cayley and were developed especially by Hermite. It is to be hoped that a new era of mutual stimulation of mechanics and mathematics has commenced. To the physicists it will at first seem deplorable that in atomic problems we have apparently met with such a limitation of our usual means of visualization. This regret will, however, have to give way to thankfulness that mathematics in this field, too, presents us with the tools to prepare the way for further progress. (Bohr 1925, *PWNB* 1, p. 51; emphasis added)

Bohr may well have been too optimistic as concerns the physicists' attitude. There is no question that there is much "thankfulness that mathematics in this field, too, presents us with the tools to prepare the way for further progress." On the other hand, the discontent with "the limitation" in question has never subsided and is still with us now. Einstein led the way. He did not find satisfactory or even acceptable either this state of affairs as concerns physics or this type of use of mathematics in physics. Schrödinger was quick to join, with many, indeed a substantial majority of, physicists and philosophers to follow. However one views the situation on that score, one has to appreciate Bohr's carefulness and precision in this statement, beginning with stressing the *essential* nature of the mathematics in question for quantum mechanics, especially for the rigorous proof of the conservation theorems. (Heisenberg only proved their application in the particular case of the one-dimensional anharmonic oscillator and only to the first order of approximation.) Bohr favors the use of the word essential in the direct sense of being fundamental, of pertaining to the essence of a given situation. It is also significant that Bohr specifically speaks of "a new era of mutual stimulation of *mechanics* and mathematics," rather than physics and mathematics, although Heisenberg's discovery redefines the relationships between them as well. At stake are *individual* quantum processes and events. The mathematical science, both descriptive and predictive, of these processes in classical physics is mechanics. It is, correspondingly, classical mechanics that is reconfigured as quantum mechanics, but as a theory that is only able to predict, in general in probabilistic terms, such events as effects of certain processes without describing these processes.

Most crucial, accordingly, are the new relationships, enabled by a certain algebra (in either sense), in the absence of visualizable and, hence, again, also geometrical description at the quantum level, unless it is a symbolic geometry in turn, as considered in the preceding section. It is this situation that defines the "new era" in question. Mathematics becomes even more important for physics. It allows us to predict the outcomes of quantum experiments in the absence of any knowledge or even conception concerning the actual (independent) physical behavior of quantum objects themselves. Also, as I argue here, different as it is from that found in classical physics, this conjunction of mathematics and physics enables one to create new physical concepts (at the intersection of physics, mathematics, and philosophy) and ensures the compatibility between the disciplinary requirements of quantum mechanics as physics and a new epistemology, ultimately nonclassical epistemology. In other words, perhaps ironically, Heisenberg's epistemology and then nonclassical epistemology open greater possibilities for the use of mathematical concepts in physics. For, it follows that one's choice of a mathematical scheme under these conditions becomes relatively arbitrary in the sense that one need not provide any descriptive physical justification for it, but only need to justify it by its capacity to make proper predictions. It is true that the actual developments of the mathematical formalism of quantum mechanics extended (via the correspondence principle) from the descriptively justified formalism of classical mechanics. One can, however, also start directly with the Hilbert-space or C^*-algebra formalism. This is an important part of the new way of "mutual stimulation of mechanics [or physics in general] and mathematics."[10]

Bohr's passage under discussion also appears to have been influenced and perhaps even prompted by a letter from Born concerning matrix mechanics. Bohr received it shortly before completing his article, although the key mathematical considerations of Born's letter could have already been familiar to Bohr from his brother Harald. As I explained in the Introduction, Harald Bohr made major contributions in this area of mathematics, and he spent some time in Göttingen and his work was greatly admired there. Bohr had continuous exchanges on these subjects with him. Born used some of Harald Bohr's work on aperiodic functions in his later work (with Norbert Wiener) on quantum mechanics and operator theory (Born and Wiener 1926). It is true that Born's letter was written before the three-man paper was completed and only stated the program of Born's arguably most important contribution to the paper, the use of Hilbert's principal axis transformation in determining the eigenstates of the energy matrix. (I shall explain this concept in Chapter 5.) Born and Jordan's paper was finished by then and was known to Bohr, and Heisenberg was mostly in Copenhagen during his work on the three-man paper. The mathematics involved must have been discussed in Copenhagen at the time. Born's letter traces the history of matrix and invariant theory to which Heisenberg's discovery gave an unexpected significance, especially for Born in view of his own

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[10] It is peculiar and in a way remarkable that in commenting on Bohr's passage in their introduction to Volume 3 ("The Formulation of Matrix Mechanics and Its Modifications, 1925–1926") of their treatise and even in building this introduction around it, Mehra and Rechenberg miss the epistemological meaning and implications of Bohr's statement. They see the passage as indicating primarily or even exclusively that "the unsolved problem[s?] of atomic theory would still require the use of appropriate mathematical tools" (*MR* 3, p. 4). This point is of course trivially correct, but it is hardly germane to Bohr here. They say nothing about Bohr's statement that "in atomic problems we have apparently met with such a limitation of our usual means of visualization," again, manifestly crucial to this statement and to all of Bohr's thinking. This instance is not unsymptomatic of Mehra and Rechenberg's reading of Bohr in their treatise, which often misses the deeper and more radical aspects of Bohr's thought.

trajectory as a theoretical physicist, which helped him to perceive Heisenberg's formulation in terms of matrix algebra.

According to Born: "for me the possibility of this formulation has a very personal attraction. Since my student days I have been haunted by an *idée fixe*, namely that all essential laws of physics must find their adequate formulation as invariants of linear substitutions" (Born to Bohr, October 10, 1925).[11] Born then traces the subject both in his own scientific education and life, and in general, from Cayley, Frobenius, and Hermit to Felix Klein's program (of formulating mathematical and physical theories in terms of transformation groups and invariant theory) and Hilbert's work implementing this program in his theory (with Fredholm) of integral equations. All this leads Born to a reflection on infinite-dimensional Hermitian quadratic forms and infinite-dimensional matrices, which Heisenberg unknowingly rediscovered and which Born recognized in Heisenberg's paper and developed in his own work on matrix mechanics. "Therefore," Born then says, "I am now so happy that Heisenberg's rules fit exceedingly beautifully into my *idée fixe*, and that one can express quantum mechanics directly in the form of an eigenvalue problem; thereby one can, it seems to me, perceive all theoretical possibilities quite well" (Born to Bohr, October 10, 1925).

Born was even more perceptive than he appears to have realized at the time. This view of quantum mechanics as a form of eigenvalue problem was how Schrödinger was to develop his wave mechanics, as the title of his first paper, "Quantization as an Eigenvalue Problem," on the subject was to announce (Schrödinger 1926a). Schrödinger, moreover, proceeded via Fredholm and Hilbert's work and Klein's analysis of the mechanical–optical analogy through which Hamilton discovered his equations of classical mechanics. These are the same equations that lead to both forms of mechanics. The relationships between and the equivalence of both formulations emerge through many junctures. This is not altogether surprising, given the mathematical history of these developments, mentioned above, especially Hilbert's work, in part as presented in Courant and Hilbert's *Methods of Mathematical Physics*. The book was read by all the principal actors involved, including Heisenberg and Schrödinger. Both Hilbert's work and Courant and Hilbert's book are often mentioned but rarely sufficiently credited either historically or conceptually, with only few exceptions (e.g., Corry 2004). Bohr did not miss its crucial significance and commented on it in his important, including in the context of the present discussion, late essay, "Mathematics and Natural Philosophy" (1956). As he says: "In this work, invaluable to every student, a lucid exposition is given of logical generalizations that not only have proved to be of extreme fertility in the explorations of multifarious problems within the domain of classical physics but have also shown themselves to be equally inspiring for the elucidation of the

[11] The letter is cited and discussed, from a rather different perspective than the one offered here, by Mehra and Rechenberg (*MR* 3, pp. 118–121).

novel problems with which modern developments in physical science have confronted us" (Bohr 1956, *PWNB* 4, p. 166).

Born, in his letter, also commented in a self-deprecating vein: "I am sure that such a one-sided method of viewing things [i.e., from the mathematical perspective that he outlined] might appear to you as being quite ridiculous, because the formalism is rather secondary for the actual physical relationships. I am, however, aware of the fact that I lack that physical intuition which you and Heisenberg possess; and all that I can contribute to advance the thing consists in putting the physical regularities into a mathematical scheme" (Born to Bohr, 10 October, 1925). One might do well to disregard this unnecessary self-deprecation. Born had plenty of physical intuition, which, for example, led him to his interpretation of the wave function, and besides, "putting the physical regularities into a mathematical scheme" is what theoretical physicists do, and this process requires much physical intuition. The statement is, however, misconceived on other counts as well. Bohr's argument concerning the new mechanics and its mathematics in "Atomic Theory and Mechanics" clearly shows the opposite of what Born contends, as does Heisenberg's work, which, while not mathematics, is fundamentally mathematical even though and because it proceeds from physics, as explained above. Indeed, with Heisenberg, physical intuition itself essentially depends on mathematics, at one end, and epistemology, at the other, rather than on the kind of physical intuition that governed classical physics, since so much is altogether inaccessible to this last kind of intuition in quantum theory. Born's statement almost seems to be addressed to Pauli, who made this type of criticism of Born a few months previously, criticism in turn quite misplaced and reflecting, at the time (this was to change), an insufficient grasp of what Heisenberg had accomplished and how he accomplished it on Pauli's part (Born 1978, p. 218).

First of all, even if Bohr would have seen it as preferable for mathematics to be derived from physics, mathematics and this particular mathematics would by no means be seen, then or ever, as secondary by Bohr. As he was to say in "Mathematics and Natural Philosophy": "For anyone who through the years has been concerned with the difficulties and paradoxes in quantum physics, it is indeed a deep satisfaction that logical order should be attained to such degree by means of the subtle methods offered by mathematical science" (Bohr 1956, *PWNB* 4, p. 169). Much of this article is also devoted to his interpretation of quantum mechanics and the role of measuring instruments there. Bohr's argument thus is motivated and defined by the connections between measuring instruments and "mathematical instruments" in quantum physics, including in jointly resolving the difficulties and paradoxes in question. We recall that in his Chicago lectures, Heisenberg argued that "it is not surprising that our language [or concepts] should be incapable of describing the processes occurring within atoms, for . . . it was invented to describe the experiences of daily life, and these consist only of processes involving exceedingly large numbers of atoms." He also noted that "it is very difficult to modify our language so that it will be able to describe these atomic processes, for words can only describe things of which we can form mental pictures, and this ability, too, is a result of daily experience." The same argument is often made by Bohr. Heisenberg added "Fortunately, mathematics is not subject to this limitation, and it has been possible to invent a mathematical scheme—the quantum theory [e.g., quantum mechanics]—which seems entirely adequate for the treatment of atomic processes" (Heisenberg 1930, p. 11).

Indeed, mathematics now becomes in a certain sense primary, even though, as I discussed in the Introduction, quantum mechanics cannot be reduced to mathematics and, as against classical physics, it contains an irreducible non-mathematical remainder, since no mathematics can apply to quantum objects and processes themselves. But then, nothing else, physics or philosophy, for example, can apply either. The key intuition was that there could be no physical intuition that could possibly apply to quantum processes themselves, as against using mathematics to predict the outcome of experiments. In other words, this situation required the kind of physical–mathematical intuition displayed by Heisenberg or later by Born himself in interpreting Schrödinger's wave function. This intuition, however, if not itself mathematical, depends fundamentally on the role of mathematics, even as it redefines the relationships between mathematics and physics, and transforms the nature of theoretical physics.

Bohr's elaboration in question shows his profound understanding of this situation. Although it may appear to announce a program that is more Heisenbergian than Bohrian and that is different from the one Bohr came to follow later, by taking this view one underestimates subtler complexities of Bohr's later views as concerns the significance of mathematics in quantum mechanics. It would be more cogent to argue, as I do here, that Bohr's views of quantum mechanics, from "Atomic Theory and Mechanics" to "Discussion with Einstein" and beyond, are defined by the essential roles of both measuring and mathematical *instruments* in their reciprocal relationships in quantum physics. The very appeal to "instruments" is hardly casual. Apart from the fact that such choices of expression are rarely casual in Bohr, the point is consistent with Bohr's general view of mathematics (e.g., Bohr 1954, *PWNB* 2, p. 68). It suggests that mathematics is a form of technology, a technology of thought, rather than something absolute or ideal along Platonist lines. "Mathematics and Natural Philosophy" makes this reciprocity of the two technologies especially apparent. It is, however, manifest throughout Bohr's writings. The significance of measurement is of course crucial for Bohr and is central to this study as well.

Here, however, I would like to emphasize the role of mathematics in quantum mechanics, following both Bohr and Heisenberg (in his earlier work, in contrast to somewhat stronger Platonist tendencies in his later thinking). I qualify this assessment, because, as I said, Platonism in Heisenberg or elsewhere, including in Plato, is not a simple matter, and Heisenberg's later arguments concerning the role of mathematics in physics make us all the more aware of this complexity. In all of Heisenberg's work and in his philosophy of physics, the primary role of mathematics is defined by its indispensability in the constitution of physical concepts, but also along with and in interaction with the experimental or physical and philosophical architecture of these concepts. For the moment, I primarily speak of the physical part of this architecture defined by the physical phenomena involved, for example, via measuring instruments, and of its physical and epistemological part as concerns the corresponding aspects of the interpretation of these phenomena. In particular, I have in mind the impossibility, stressed by Bohr, of isolating quantum objects and their behavior from measuring instruments and, hence, of considering quantum objects and processes as separate phenomena. Philosophical, including phenomenal, conceptual construction would be involved in all three components—mathematical, physical, and epistemological–philosophical.

Accordingly, I speak of a kind of return to Plato, as against Aristotle, in Heisenberg's approach to quantum mechanics only in the sense of the abandonment of the classical concept of physical motion as applicable to quantum objects and their behavior. By the same token, one also abandons the use of mathematical models of motion, which have grounded classical physics from Galileo on. The mathematical grounding of modern physics goes beyond Aristotle, but, as discussed in the Introduction (and with qualifications offered there), it is overshadowed or at least shadowed by Aristotle's thought as concerns the role of motion in physics, which equally defines classical physics and relativity. As explained above, the project of the classical program may also be seen as geometrical, a view that lies at the origins of modern physics in Galileo and Newton. For Galileo, physics was a subset of Euclidean geometry, defined by virtue of his introduction of mass (hence, physics defined this subset), and it was presented in his work in terms of geometrical figures, as, importantly, idealized representations of nature. Algebra was inevitably involved as well, as was of course the case in Newton or Descartes, helped by his invention of analytic geometry.[12] De Broglie's theory and then, as against Heisenberg's algebraic approach, Schrödinger's wave mechanics were among the attempts to continue this project, which the subsequent developments of quantum theory appear to have made difficult, if not impossible, to realize.

By contrast, seen from the present perspective, quantum mechanics and then higher level quantum theories form a new mathematical science of nature, a science without a theoretical representation of motion or anything else at the quantum level. This science does, however, not replace the mathematics of motion with a Platonist mathematical model of nature based on an immutable reality, a reality without motion. Instead, and here lies, again, the radical nature of Heisenberg's revolution, mathematics only serves the purposes of predictions concerning certain phenomena, defined by the impact of quantum objects upon the world we observe, without describing the behavior of these objects themselves. To this behavior, accordingly, the concept of motion or possibly even change may not apply. But neither could the concept of "standing still." We certainly register the effects of change between measurements, and even

[12] For a discussion of Galileo in this context, see (Plotnitsky and Reed 2001).

primarily such effects, although there are certain quantum effects of permanence, such as the von Neumann effect of repeated measurement, the quantum Zeno effect, or the quantum "watched-kettle" effect. This makes it rather less likely to think that things stand still in the quantum level, especially if one assumes the theory and nature to be local.[13] However, no concept of change may still be applicable at the quantum level.

The role of these effects of change might, however, have been one of the reasons why, in his later thinking, specifically in considering the concept of elementary particle, Heisenberg was compelled to invoke certain *dynamic* properties of matter itself on the quantum level and even the possibility of representing these properties mathematically ("What is an Elementary Particle?" Heisenberg 1989, p. 79). Heisenberg sees this (ontological) view as Platonist, in opposition to Democritean atomism, which he wants to abandon, just as Bohr did especially in his post-EPR thinking. What each offers or aspires to achieve instead appears to be quite different, however. Expanding upon and radicalizing Heisenberg's original approach to quantum mechanics through nonclassical epistemology, Bohr replaces the Democritean doctrine with his new epistemological, rather than physical, atomism of the individual phenomena (e.g., Bohr 1949, *PWNB* 2, pp. 32–33). By contrast, Heisenberg appears to want to give a certain mathematical or, at least ideally, mathematizable non-Democritean ontology to the elementary constitution of nature. It is also of some interest that in considering quantum probability, Heisenberg now tends to speak in terms of *propensities* of quantum objects, reflected in the mathematical formalism of quantum mechanics. Propensity is an Aristotelian ontological concept (*potentia*), which does not appear to appeal to or to have been used by Bohr.

While, however, Heisenberg does speak of certain, yet unknown, dynamics, he does not invoke *motion* and thus, on this point, bypasses Aristotle along with Democritus. This is not surprising, since from the 1930s on, his primarily model becomes that of quantum field theory and the virtual particle formation, introduced by Dirac. This model makes it difficult to speak of physical motion on the model of classical physics. In particular, this kind of "motion" deprives a given particle, say, an electron, of its fixed identity. Instead the particle is transformed by the process into another particle or a set of particles in the process of its motion (e.g., a positron, a photon, an electron–positron pair, etc.), or in the present view, the corresponding observable and measurable effects, coupled to a particular mathematical formalism. The allowable, or forbidden,

[13] Cf., Julian B. Barbour's concept of "Platonia," an underlying reality without change and motion (Barbour 1999), the idea apparently originating with Parmenides, who inspired Plato. Barbour's conception appears to originate in the idea that it does not appear possible by means of quantum theory to describe the *motion* of the ultimate constituents of nature. From a nonclassical viewpoint, however, while this is true, it does not follow that everything "stands still" at that level, since, as just explained, the latter concept would not apply any more than that of "motion" (or "object" and "quantum") to quantum objects.

transitions and the probabilities of such transitions (the theory is probabilistic, just as quantum mechanics is) are rigorously specified by the theory. Accordingly, Heisenberg's invocation of motion may be seen as consistent with Bohr and the present view insofar as something must "happen" at the quantum level to lead to changes in quantum phenomena we observe and to the very emergence and constitution of these phenomena. In a nonclassical view, however, this "happening" or this "dynamics," or this "something," is beyond any concept we can possibly form, including those of happening (or not happening), dynamics, or somethingness, or, again, "particle" or "wave" or field, or, when it comes to probabilities, of propensity of objects. Indeed, Heisenberg still maintains that all such concepts are only applicable at the level of observed phenomena even in his later works (Heisenberg 1962, pp. 51–58). This is not inconsistent with his later mathematical ontology, since mathematics, in principle, does not depend on such concepts. On the other hand, a mathematical ontology is inconsistent with nonclassical epistemology, which precludes any form of ontology. Nor, in a nonclassical view, are there propensities to the behavior of quantum systems themselves. There only degrees of expectations and corresponding probabilities defined by the overall experimental setup of a performed actual measurement and a possible measurement with a predicted outcome (Bohr 1938, *PWNB* 4, p. 101). The mathematics of quantum theory defines these probabilistic expectations (no other appear possible) in the practice of quantum physics by enabling us to make better predictions concerning what is thus observed in measuring instruments or other macro-objects under the impact of quantum objects. But this is also all that this mathematics does for us, and no other mathematics appears to be able to do more.

This is a revolutionary change from classical physics or relativity. However, quantum physics, as a mathematical science of nature, had to make this change in order to be able to deal with quantum phenomena mathematically. It is remarkable and, again, by definition inexplicable, that the mathematics of quantum mechanics enables us to make correct predictions concerning quantum experiments without our being able to know anything about quantum objects themselves, or even being able to conceive of what they are and how they behave. While we may be lucky that nature allows our mathematics to do so and to have this mathematics, itself a gift of nature, it was still necessary to discover this mathematics and this way of using it in physics. Heisenberg was able to accomplish both. This is why his discovery of quantum mechanics was so momentous and why it earned him a place alongside Aristotle, Galileo, Newton, Maxwell, and Einstein. At this magnitude of achievement, this list, while not exhausted here, may not be that much longer.

Chapter 5
Schrödinger's Waves: Propagation and Probability

This is an extremely funny thing. The contrast between light-quanta and wave radiation can be traced even in the atom as: (a) single orbiting electrons; or (b) a standing vibration of the whole atomic region. In the interpretation (a) [we have]: the Hamiltonian partial differential equation; the separation of variables; and the quantization in the well-known manner. In the interpretation (b) [we have]: the wave equation; the separation of variables *in the old, well-known sense that the unknown function is assumed to be, e.g., the product of a function of [radial coordinate]* r*, of a function of [the angle]* θ*, and a function of [the angle]* ϕ*; searching for the "normal vibrations." The frequencies emitted [by atoms] appear as frequency differences, i.e., as beat frequencies of the normal frequencies. Something must be hidden behind that. I hope to formulate the results soon in an organized way.*

—(Erwin Schrödinger, A Letter to Wilhelm Wien, January 8, 1926)

[The ψ*-function] is now the means for predicting the probability of measurement results. In it is embodied the momentarily-attained sum of theoretically based future expectation, somewhat as laid down in a* catalogue.

—Erwin Schrödinger, *"Die gegenwärtige Situation in der Quantenmechanik"* [The Present Situation in Quantum Mechanics] (Schrödinger 1935a, p. 158)

*If two separate bodies, each by itself known maximally, enter the situation in which they influence each other, and separate again, then there occurs regularly that which I [call] entanglement [*Verschränkung*] of* our knowledge *of the two bodies.*

—Erwin Schrödinger, *"Die gegenwärtige Situation in der Quantenmechanik"* [The Present Situation in Quantum Mechanics] (Schrödinger 1935a, p. 161)

Abstract This chapter offers a reassessment, from the perspective of this study, of Schrödinger's work, especially of his concept idea of quantum waves, as it extended from Max Born's 1926 interpretation of Schrödinger's wave or ψ-function in terms of probability to Bohr's complementarity and

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then to the viewpoint of modern-day quantum information theory, especially in its Bayesian version. Section 5.1 gives a general introduction to the subject of quantum waves. Section 5.2 discusses those aspects of the old quantum theory and Heisenberg's matrix mechanics that help place Schrödinger's work in its proper historical context. Sections 5.3, 5.4, and 5.5 consider Schrödinger's wave mechanics as a wave theory of subatomic processes. I close with a discussion of the concepts of quantum state, quantum entanglement, and quantum information, via the cat-paradox paper. These themes will be developed in Chapters 6, 7, and 8.

5.1 Quantum Waves and Quantum Probability

The present view of quantum mechanics, including its nonclassical character and its Bayesian orientation, was anticipated by Schrödinger in his cat-paradox paper, "*Die gegenwärtige Situation in der Quantenmechanik*" [The Present Situation in Quantum Mechanics] (Schrödinger 1935a), which, however, also expressed a deep discontent with this situation. Inspired by EPR's paper and probably further motivated by Bohr's reply, which Schrödinger did not find convincing, the paper aimed to express doubts concerning the quantum mechanical "doctrine," primarily as established by Bohr and Heisenberg, and even to repudiate it, at least as a desirable way to do physics. Schrödinger appears to have had some suspicions that, even though "born of distress," the doctrine might have been forced upon us by nature itself. EPR's paper, however, offered a new hope that the latter might not be the case and that nature was kinder to us than the "doctrine" claimed. Schrödinger eventually grew more skeptical, more so than Einstein, as concerns our ability to approach, even in principle, the ultimate constitution of nature. Ironically, this skepticism brought his epistemological position closer to that of Bohr and Heisenberg, who, as we have seen, expressed similar reservations all along. Their assessment of the situation was of course different, since Bohr and Heisenberg saw it as a reflection of how far nature allows human thinking and knowledge to reach and, concomitantly, as a new way of doing physics, a view that Schrödinger was unwilling to accept.

However, that deeper skepticism on Schrödinger's part was to come later. In 1935, he appears still to hope that a theory of quantum phenomena that accords more with what he saw as "the classical ideal" in physics might be developed. Einstein's ideas concerning the EPR-type experiments and what Schrödinger believed to be insufficient counterarguments by Bohr and others, again, rejuvenated his hopes after a decade during which, while his formalism thrived, his philosophical ideas suffered a major setback in the developments of quantum theory, which, as concerned its philosophical aspects, followed Bohr, Heisenberg, and their supporters. Schrödinger's anticipation of the argument of a Bayesian type for quantum mechanics as a probabilistic theory of *individual* processes and events (rather than a statistical theory of ensembles, on the model of classical statistical physics, one manifestation of this "classical ideal") is related to this epistemological problematic. Schrödinger subscribed to the Bayesian view of probability, although, as explained earlier, this view as such is not necessarily in conflict with an epistemologically classical view of physics. In any event, the ψ-function becomes a means for predicting the probabilities of measurements associated with individual quantum events, and is seen by Schrödinger as, in his remarkably Bayesian phrase, "an expectation *catalogue*" concerning such events (Schrödinger 1935a, *QTM*, p. 158). Technically, the ψ-function only gives us "probability amplitudes," and we need rules, such as Born's rule, to form catalogues of probabilities themselves. The ultimate problem of quantum mechanics in Schrödinger's view was its inability to account for the quantum-level ontology, and at the time he believed, as did Einstein, that a more complete and classical theory of quantum phenomena could be found.

This chapter itself is, however, devoted to a more general discussion of Schrödinger's thinking—physical, mathematical, and philosophical. The power of his philosophical thinking may be more manifest in his later more philosophical meditations on biology (where his scientific thinking was of course remarkable as well) or on the nature of human mind and consciousness (Schrödinger 2006). However, this power is quite apparent throughout his work on wave mechanics and in his later attempts to think through the complexities of quantum mechanics, as in his cat-paradox paper. Even if the key philosophical ideas or ideals that guided his work might not have succeeded in quantum theory (although not everyone agrees with this claim either), they remain important, and his exploration of nonclassical ideas is valuable as well, even though and sometimes because he was critical concerning them. This chapter, thus, also continues this book's general exploration of the relationships among physics, mathematics, and philosophy in quantum physics. Schrödinger's work reveals some among the deepest and most significant aspects of these relationships.[1]

Even though Planck's discovery puts into question the unconditional application of the classical electromagnetic theory as a wave theory to quantum phenomena of radiation, the *idea* and *language* of waves have retained their significance for quantum theory from the old quantum theory to quantum mechanics to quantum field theory (where the idea became transformed into

[1] For Schrödinger's collected papers on wave mechanics, see Schrödinger (1928), and for his other important papers on quantum mechanics, see Schrödinger (1995). This chapter cannot pretend and does not aim to offer a comprehensive treatment of Schrödinger's thought, which, it may be observed, was given rather less attention in literature than that of Bohr and Heisenberg, or Einstein (even as concerns his engagement with quantum theory). Arguably the best available study is Bitbol (1996). On the other hand, in Mehra and Rechenberg's treatise Schrödinger is given more space than any other founding figure (*MR* 5), and my analysis in this chapter is indebted to their discussion of Schrödinger. As in earlier chapters, however, I am less in accord with and, at several key junctures, shall contest their philosophical argumentation.

the concept of quantum field). On the other hand, giving a *physical* content to the concept of waves applied to the behavior of quantum objects posed major and perhaps ultimately insurmountable difficulties. In most interpretations of quantum mechanics, especially those following "the spirit of Copenhagen," the idea that quantum behavior is wave-like is abandoned, and, in some among these interpretations, such as that of Bohr (in any version) or the one adopted here, it is rigorously precluded. We recall, however, that in a nonclassical interpretation, the same is also true as concerns the concept of particle. Heisenberg in particular noted in his Chicago lectures that, even if one assumes—as, again, up to a point, both Bohr and Heisenberg did—that one could apply either (classical) concept, "wave" or "particle," at the quantum level, one could only do so "with certain limitations" and never in its entirety (Heisenberg 1930, p. 47). As explained earlier, a particle is an idealization even in classical mechanics, but of a different kind, which indeed enables the application of the physical concept of "particle." In quantum mechanics, the concept cannot apply to quantum objects in full measure in view of the uncertainty relations or in a nonclassical view, at all, any more than can the concept of wave. On the other hand, apart from the fact that there are the uncertainty relations for waves as well (Heisenberg 1930, pp. 48–52), *physically*, all observable individual quantum phenomena appear to be particle-*like* by virtue of being individual and discrete (discontinuous from each other). They never appear to be individually wave-*like*, insofar as any wave-*like* quantum phenomena are in principle decomposable into a set of discrete quantum phenomena, such as the dot-like traces of collisions between quantum objects and the silver bromide screen in the double-slit experiment.[2] This circumstance is one of the reasons why the wave–particle complementarity does not appeal to Bohr. The situation also presented an ultimately insurmountable difficulty for Schrödinger's program of his wave mechanics, since his wave ontology could not properly account for such discrete effects.

Under these conditions, in nonclassical and most other interpretations of quantum mechanics, the significance of the concept of "wave" and (to the degree such a concept can be given this sense) this concept itself is defined by Born's probabilistic interpretation of the wave function. Born himself states the case as follows: "[T]he motion of particles follows the probability law but the probability itself propagates [in a wave-like manner] according to the law of causality" (Born 1926b, p. 804; also Born 1949, p. 103). This conceptual or symbolic, or metaphoric, (re)incarnation of the idea of waves, as opposed to physical waves, becomes part of the conceptual architecture of quantum mechanics—at least, again, in the present and most other interpretations, since some other interpretations of quantum mechanics or alternative versions of quantum theory, such as Bohmian mechanics, do retain the concept of

[2] As explained earlier, such traces are dot-like only at a low resolution, which "masks" a very complex physical object, composed of millions of atoms, and particle-like only in the sense that a classical object idealized as a particle would leave a similar trace. See, again (Ulfbeck and Bohr 2001) and (Bohr et al. 2004).

physical waves. This architecture is parallel and even isomorphic to that of Heisenberg's matrix mechanics. One might say that Born gave Schrödinger's wave mechanics a Heisenbergian epistemology and, thus, allowed one to interpret both versions of quantum mechanics in the same way (mathematically, both are, again, equivalent). In a nonclassical view, quantum probability laws apply to the outcomes of experiments, defined by the interactions between quantum objects, rather than to the motions of those objects, which circumstance requires a qualification and, in the first place, a more rigorous interpretation of Born's statement.

Born's formulation, especially his statement to the effect that "the motion of particles follows the probability law," is somewhat vague, which, however, is, again, not surprising given that it was really the first attempt to understand the wave function in these terms. What is this law? Does Born mean that one can only make probabilistic predictions, concerning, say, a future position of a particle, in accordance with his rule, by using Schrödinger's equation? Or does he mean something more complex, specifically as concerns some possible connections between the wave-like propagation of probability and the waves in the configuration or phase space mapped by Schrödinger's equation? On the other hand, the second part of Born's formulation, while stated peculiarly and metaphorically (one cannot, again, speak of a *propagation*, causal or not, of probability otherwise than metaphorically), is essentially correct. It can also be inflected so as to bring it into accord with the nonclassical view adopted here. The situation would appear as follows. By means of Schrödinger's equation, quantum mechanics predicts, and *predicts exactly*, the *probabilities* of the outcomes of certain experiments on the basis of certain previously performed experiments, which experiments can be rigorously specified and set up. The *probabilities* themselves are determined and, again, determined *exactly* by Born's square-moduli rule, applied to the wave function or alternative mathematical procedures, for which it does not appear possible to provide a proper physical justification. Thus, these procedures remain ad hoc. As noted earlier and as will be discussed in more detail later, the situation contains further nuances as concerns the nature of the experimental arrangements involved and of the actual experiments performed. In particular, there are no waves or propagation of probabilities either, but only certain patterns of probabilities, whose physical manifestations are *always discrete*, although sometimes, as in the case of the interference pattern observed in the double-slit experiment, these patterns can be wave-*like* in their phenomenal appearance, which is to say correlated rather than random. From this viewpoint, one can discard the idea of waves and the difficulties, such as those just mentioned, associated with it in quantum theory altogether. For example, one need not worry about drawing proper conceptual connections between the wave-like propagation of probability and waves in the configuration or phase space mapped by Schrödinger's equation, which waves, as will be seen below, can only be symbolic in any event.

Beyond explicating an important part of quantum mechanics and reiterating the significance of Born's idea, the argument of this chapter allows one to reassess, from the nonclassical perspective of this study, Schrödinger's contribution and his thinking as thinking in terms of and indeed *in* waves. By offering this reassessment, this chapter also becomes a tribute to Schrödinger's work, even though some of his, in the present view, most significant philosophical ideas emerge in part against the grain of his thought and his ideal of physical theory, the "classical ideal," as he, again, called it in his cat-paradox paper (Schrödinger 1935a, *QTM*, p. 152). As I argue, the significance of this paper lies well beyond the cat-paradox—for which it is most famous, to the point that it is rarely discussed apart from the cat-paradox itself and references to the term "entanglement" [*Verschränkung*], the concept defined there and now equally famous. The paper was inspired by the EPR experiment and EPR's argument concerning this experiment, through which EPR de facto introduced entanglement, without discussing it in these terms. In particular, even though Schrödinger expressly doubted quantum mechanics, at least in the interpretation similar (it need not be quite as radical) to the one adopted here, "the doctrine born of distress," he offers a remarkably cogent exposition of the doctrine itself.

This exposition, as I said, also anticipates the Bayesian approach to quantum information theory. The wave function (cum Born's rule) provides a "catalogue" of expectations or probabilities for the outcomes of quantum experiments. Such catalogues, which are, it is worth reiterating, always discrete, include those involved in the EPR type of experiments and quantum entanglement, which Schrödinger discusses in these terms. An entanglement is a correlation of expectation catalogues. Born's interpretation of the wave function, at least as originally offered, expressly refers to the probabilities of individual observable events. In other words, the probabilities in question apply, along Bayesian lines, to individual quantum systems or events rather than ensembles of such systems of events. Born's rule or equivalent rules, such those based on von Neumann's projection postulate or related conceptions and refinements (e.g., the Lüders postulate), for predicting such probabilities became subject to various ensemble-type interpretations subsequently.[3] We do of course need many repeated experiments of the same type to verify the reliability of Bayesian predictions based on the wave function and these rules. However, as explained in Chapter 2, in contrast to classical physics we cannot improve our individual predictions, regardless of how often we repeat the experiment with the same (classically defined) initial conditions of measuring instruments.

Schrödinger's understanding of the situation in Bayesian terms is not surprising. As noted above, Schrödinger was a follower (one of the relatively few) of the Bayesian approach, which defines probability in terms of degrees of expectation or belief concerning individual events, as opposed to Einstein, who was a frequentist (a more common position). In Einstein's view, quantum mechanics should be seen, by analogy with classical statistical physics, as a

[3] The investigation of these rules and their interpretations would require a separate discussion, which is beyond my scope and is not germane for my argument in this study.

statistical theory of ensembles of events, a view that Schrödinger came to question on other grounds as well, such as the fundamental difference between the mathematical formalism of quantum mechanics and classical statistical physics. On the other hand, Einstein thought that quantum mechanics failed to describe, in the way classical mechanics does in its proper domain, individual processes and events. Accordingly, quantum mechanics is, for Einstein, incomplete, or, for the reasons to be explained in later chapters, if complete, then nonlocal. By contrast, in Bohr's view, quantum mechanics is a probabilistic theory even in the case of individual processes and events, as opposed to only certain composite mechanically complex events, defined by sequential (as in the case of a coin toss) or parallel (as in classical statistical physics) multiplicities of primitive individual events. (Actually, in the case of a coin toss we deal with both types of complexity as well.) At the nonclassical limit, this view implies the impossibility of an underlying description of such processes.

In a nonclassical view, quantum events are observed in classical domains but result from the impacts upon those domains of quantum objects and processes—or, again, entities the existence of which we are compelled to infer and which we are compelled to idealize as quantum objects and processes on the basis of such events. Schrödinger's wave function mathematically encodes this situation, in particular the *irreducibly* probabilistic character of the predictions involved even in the case of primitive individual processes and events. This is, again, an experimental fact, given that "one and the same experimental arrangement may yield different recordings" of the outcomes (Bohr 1954; *PWNB* 4, p. 73). The specific character of this *encoding* gives the ultimate significance to quantum waves and Schrödinger's wave mechanics as a "wave kinematics" of probabilities analogous to the new kinematics of Heisenberg's matrix mechanics (both forms of kinematics are, again, mathematically equivalent). The difference is that this was a starting point for Heisenberg, while it was Born who brought Schrödinger's mechanics to this point. Heisenberg himself said as much later on: "The probability function [the wave function] obeys an equation of motion as the coordinates did in Newtonian mechanics; its change in the course of time is completely determined by the quantum-mechanical equation [Schrödinger's equation], but it does not allow a description in space and time" (Heisenberg 1962, p. 49). Although Heisenberg's analogy with Newtonian mechanics is suitable, by stating that Schrödinger's equation "does not allow a description in space and time" this formulation also tells us that one cannot rigorously speak here of an equation of motion, any more than in the case of Heisenberg's version. In other words, Heisenberg is careful not to speak of Schrödinger's equation as describing a causal evolution of a given quantum system itself, which argument is often found (as will be discussed in the next chapter) in literature on quantum mechanics, including in Bohr's Como lecture. Once Schrödinger's equation "does not allow a description in space and time," the impossibility of such a causal evolution follows automatically, a point that was made by Schrödinger himself in his cat-paradox paper.

In this view, the mathematical concept of "quantum state" in terms of ψ-function, cum Born's rule, enables us to establish a catalogue of expectations, in terms of probabilities, for future measurements on the basis of a given measurement and only for such future measurements. Quantum waves may then be seen as the "waves" of such expectations, if one *hypothetically* follows possible changes in the situation of a given object, such as an electron in an atom, and performs the corresponding measurements along the way. In reality, as discussed earlier and as Schrödinger explained in his cat-paradox paper, we cannot track the same quantum object in the way it is possible to do in classical physics. We can only perform a sequence of experiments, each of which corresponds to a discrete event, on different electrons and atoms to make estimates concerning the probabilities of finding each electron in a given region (Schrödinger 1935a, *QTM*, pp. 154, 158–159). We only deal with a discrete series of events. This discreteness appears to be uncircumventable in quantum physics and defines it as *quantum*.

5.2 "The Wave Radiation Forming the Basis of the Universe"

Discovered a few months after Heisenberg's matrix mechanics, Schrödinger's wave mechanics aimed at offering, and initially appeared to be able to offer, a theory that would be realist and causal and thus would conform to the "classical ideal." It was expected to be able, just as classical mechanics did, both to *describe* the physical processes at a subatomic level (as wave-like processes) *and* to *predict*, on the basis of this description, the outcomes of the experiments involving these processes.

Schrödinger was aware of Heisenberg's and related work on matrix mechanics and of the successes of the theory. However, apart from his discontent with the mathematical difficulties of the matrix version and with its epistemological features, which his wave mechanics (he thought) would avoid, his path, first, to his wave equation for the electron and to his more ambitious program for a wave quantum mechanics was different. He proceeded primarily from Louis de Broglie's ideas concerning matter waves and related work by Einstein. In 1923, de Broglie conjectured that the same type of wave–particle duality that Planck and Einstein discovered in the case of radiation would apply to other elementary constituents of nature, such as electrons. The conjecture was soon experimentally confirmed by the discovery of electrons' diffraction in crystals, although the presence of these wave-like aspects of electrons' behavior did not imply that electrons could now be treated merely as waves, any more than photons could be treated merely as particles. Both aspects of quantum behavior were equally unavoidable, but they manifested themselves in different and indeed mutually exclusive circumstances. These difficulties were to haunt Schrödinger's wave theory, and ultimately proved to be irresolvable. In any event, Schrödinger's discovery may be seen as independent from that of Heisenberg, and it has been always viewed as such. Almost from the outset, it was a deliberate alternative to the matrix version of the theory. The existence of the new mechanics *might* (the evidence is skimpy and the situation complex) have provided some guidance and help to Schrödinger, especially in its fully developed matrix version, presented, with Born, as an eigenvalue problem, the type of approach eventually adopted by Schrödinger as well. In addition, Weyl, who at some point assisted Schrödinger in his work on his equation, was familiar with the development of matrix mechanics.[4] On the other hand, alternative trajectories were also available to Schrödinger and, as is well documented, followed by him, extending from Fredholm's and Hilbert's work in integral equations (Weyl made several contributions in this area as well), and Courant and Hilbert's *Methods of Mathematics Physics*, discussed earlier (Courant and Hilbert 1991). These qualifications do not of course undermine the significance and originality of Schrödinger's contribution, even leaving aside the fact that he was the first to write down the equation, which now bears his name, that is one of the great achievements of twentieth-century physics.

While Schrödinger's hopes concerning the descriptive capacity of his theory did not materialize, on the predictive side the theory was spectacularly successful. So of course was matrix mechanics, which, however, was much more cumbersome to work with mathematically and, as a result, was overtaken by Schrödinger's version in the practice of theoretical physics. Indeed, the experimental predictions of both versions coincided exactly, and both, again, were quickly shown to be mathematically equivalent. The epistemology of physical description in both versions proved to be a more complex issue. We cannot conceive of an entity that is simultaneously both a particle and a wave, or, to begin with, simultaneously continuous and discontinuous. At most, we can try to combine both types of entities in a given physical dynamics, such as a continuous motion of a discrete object, or the pilot wave of de Broglie's and then Bohm's versions of quantum theory. The mutual incompatibility of the particle and the wave pictures poses no problem for classical physics, since the latter handles waves and particles by means of two separate types of physical theories, even though both types use continuous mathematics, as manifest in the equations of motion for both types of objects. This motion itself is assumed to be continuous in both cases as well. The difference is defined by the nature of the *objects* considered, continuous in the case of waves and discontinuous in the case of particles. The classical wave theory of light, culminating in Maxwell's electrodynamics and then special relativity theory, eventually replaced Newton's optics, which was a corpuscular theory and with which the wave optics had competed for a while. Both, however, were strictly alternative treatments of light, and once one type of framework was adopted, the other was excluded.

Similarly, Schrödinger, in developing his wave mechanics, aimed to avoid particles altogether at the fundamental level of description, where everything

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[4] On Weyl's assistance to Schrödinger, see Mehra and Rechenberg (*MR*5, pp. 484–485).

would be handled in terms of wave processes, which could, under certain circumstances, produce particle-like effects at the level of observation. This approach, if successful, would resolve the conceptual difficulties arising from the necessary appeal to both the wave and the particle pictures within the same theoretical framework. It would also restore causality to the underlying wave processes, on the model of Maxwell's field theory.

Bohr's early approach to complementarity was shaped by the apparently necessary use of mutually exclusive conceptions, such as those of particles and of waves, and the corresponding physical theories within the same theoretical framework, which is in part why Schrödinger's theory had a certain, *qualified* appeal to Bohr. Even at this stage of his thinking, however, Bohr was aware of the difficulties of applying the idea of waves to quantum objects themselves, even by way of complementarity; and, as I argue here, he avoids using the wave–particle complementarity. Influenced by Schrödinger's approach and the Dirac–Jordan transformation theory (which brought both versions of quantum mechanics together), Bohr did briefly subscribe to the idea that the independent behavior of quantum objects is in some sense causal and that the lack of causality in the observable quantum phenomena was due to the "disturbance" of this behavior unavoidably introduced by measurement. I shall discuss this view on the part of Bohr, shared by many others and even prevalent through the history of quantum theory, in the next chapter. Bohr's ultimate solution to the dilemma of whether quantum objects are particles or waves—or his "escape" from the paradoxical necessity of seeing them as both—is that they are neither. This view is perfectly legitimate and consistent, once either type of feature or, again, either type of effects (particle-*like* or wave-*like*) is transferred to the level of measuring instruments and made mutually exclusive, as explained earlier.

The case became easier (following the development of quantum mechanics) from Born's probabilistic interpretation of the wave function on. Rather than coming as a monumental surprise (in part in view of the way "amplitudes" worked in Heisenberg's scheme), Born's probability interpretation of the wave function was, for Bohr, merely the final nail in the coffin for the idea of the wave-like physical behavior, individual or collective, of quantum objects. All quantum mechanical phenomena became reconceived in terms of certain discrete individual effects upon measuring instruments, with wave-like features physically appearing only as patterns by way of an accumulation of large numbers of such effects or phenomena. This is one of the main reasons why Bohr shuns the idea of the wave–particle complementarity.

Prior to Heisenberg's introduction of quantum mechanics, however, and even following it, the situation appeared along the lines of Schrödinger's comment in a letter to Wilhelm Wien. The letter just about announced the birth of Schrödinger's equation. Schrödinger wrote

> This is an extremely funny thing. The contrast between light-quanta and wave radiation can be traced even in the atom as: (a) single orbiting electrons; or (b) a standing vibration of the whole atomic region. In the interpretation (a) [we have]: the Hamiltonian partial differential equation; the separation of the variables; and the quantization

> in the well-known manner. In the interpretation (b) [we have]: the wave equation; the *separation of variables* in the old, well-known sense that the unknown function is assumed to be, e.g., the product of a function of [radial coordinate] r, of a function of [the angle] θ, and a function of [the angle] φ; searching for the "normal vibrations." The frequencies emitted [by atoms] appear as frequency differences, i.e., as beat frequencies of the normal frequencies. Something must be hidden behind that. I hope to formulate the results soon in an organized way. (Erwin Schrödinger to Wilhelm Wien, 8 January, 1926; cited *MR* 5, p. 465; translation slightly modified)

It is worth noting that this insight, too, suggests that, while mutually exclusive, the wave and particle "pictures" (in whatever interpretation) may be better seen as *equivalent*, rather than *complementary* in Bohr's sense, a view also transpiring in Heisenberg's Chicago lectures, which invoke both pictures as equally possible *visualizations* (Heisenberg 1930, pp. 13, 62–65). Either visualization could only be symbolic rather than representational from Bohr's perspective, especially in his post-Como thinking, and the ultimate nature of subatomic processes is neither wave-like nor particle-like, any more than it is anything-else-like. More generally, as I argue here, the idea of the wave–particle complementarity is ineffective and, given the massive qualification it requires, may be misleading. In any event, it is not really found in Bohr, even in the Como lecture, or in Heisenberg. It should, again, be kept in mind that either concept, as developed and used in classical physics, could apply in quantum theory only partially, the circumstance reflected in the uncertainty relations. The latter exist for both particles and waves, and they can be derived for particles from the wave considerations, à la de Broglie, as Bohr did in the Como lecture, or without them (Heisenberg 1930, pp. 13–19, 48–52; *PWNB* 1, pp. 57–61). Heisenberg, more generally, sees the situation as requiring a critique of each concept—particle and wave—and of other concepts of both particle and wave theories (as derived from classical physics, which uses them uncritically). He pursues these critiques by using one theory against the other and organizes his main argument in the Chicago lectures accordingly (Heisenberg 1930, p. 47). In any event, in quantum physics we only deal with *discrete* multiplicities of (sometimes correlated) phenomena, in a nonclassical view, moreover, as juxtaposed to quantum objects, which are placed beyond the reach of quantum theory or our thought itself.

Schrödinger was aware of these complexities, and he realized, in particular, that his quantum-*level* wave theory could not be the same as the classical wave theory, of the type Maxwell's electrodynamics was. *Quantum* would technically be a misnomer, since the theory was to be continuous, and this should of course be kept in mind whenever I use the term "quantum" in Schrödinger's wave mechanics. It only applies to the level of description. Schrödinger's realization of the unavoidable difference between a wave quantum-level mechanics and the classical wave theory made the task that he set for himself all the more difficult. Schrödinger's description just cited contains traces of the old (semi-classical) quantum theory, as the practitioners of matrix mechanics had by then just about given up on orbiting electrons. Schrödinger never liked the probabilistic view of quantum mechanics either, including eventually Born's interpretation of the wave function. He saw it (correctly) as an extension of Bohr's atomic theory and then Heisenberg's approach, and as such in essential conflict with the classical ideal. As he wrote to Bohr: "What is before my eyes, is only one thesis: one should not, even if a hundred trials fail, give up the hope of arriving at the goal—I do not say by means of classical pictures, but by logically consistent conceptions—of *the real structure of space-time [quantum-level] processes*. It is extremely probable that this is possible" (Schrödinger to Bohr, October 23, 1926, cited in *MR* 5, p. 828; emphasis added). The probabilistic and indeed Bayesian content of the last sentence is worth noting here.

Apart from the fact that this "bet" proved to be too optimistic in retrospect, the immense difficulties of this program were clearly apparent even at the time. The main difficulty was the ultimately discrete character of all observable quantum phenomena. Also, as, again, stressed by Bohr, it was far from obvious whether *conceptions* of space–time processes at the quantum level are possible apart from classical or classical-like pictures, especially a picture that would be accessible to our phenomenal intuition and thus could be used in the corresponding *anschaulich* theory, as Schrödinger also appears to have hoped these processes would be. As I have discussed, visualizability, rather than intuition or intuitiveness, is the common translation of *Anschaulichkeit* in this context, although the German combines both (as for that matter sometimes does the English "intuition" or "intuitiveness"). Such expressions as pictorial visualization, pictorial representation, and mental pictures are used as well, specifically by Bohr and Heisenberg. Schrödinger sensed these deeper difficulties of quantum mechanics for his view of nature and physics even at the time, a suspicion that he expressed more fully and with greater justification in 1935 in the cat-paradox paper. It is also clear from the available record of the published articles, correspondence, and notebooks that Schrödinger aimed to replace the difficult mathematics and disagreeable epistemological features of the matrix version with the *picture* of waves as representing the ultimate nature of physical reality, with particles appearing only as surface effects. He was thinking, just as Einstein was, in terms of an approximation or idealization of physical reality through the mediation of ontologically aimed concepts. This approach would be in accord with what he defined in his cat-paradox paper as the classical ideal in physics, which he saw (again, correctly) as being in conflict with the "doctrine" of quantum mechanics, at least if understood in the spirit of Copenhagen.

Given the preceding history of quantum theory leading to Heisenberg's discovery of matrix mechanics, one could easily have been skeptical concerning the suitability of Schrödinger's physical program for theorizing quantum phenomena. The problem of discreteness and individuality of quantum phenomena had never found an adequate resolution in Schrödinger's program, which aimed to dispense with such quantum concepts. It was quickly noted at the time, by Heisenberg in particular, that by virtue of viewing the charge density as a classical source of radiation, Schrödinger's approach was in conflict with Planck's radiation law, with which Heisenberg's approach was consistent. The difficulties

Schrödinger's approach faced ultimately proved insurmountable and compelled Schrödinger to abandon his project of wave mechanics. Nevertheless, Schrödinger's thinking of the ultimate nature of the physical world in terms of waves or *in* waves is significant, including as an instructive, even if unsuccessful, attempt to relate continuity and discontinuity in terms of underlying continuity. At some point, Schrödinger thought of the possibility of some kind of intermediate objects, which would be neither particles nor waves, and yet would *combine* some aspects of both. This is not an easy task and perhaps is ultimately impossible, given that the wave and particle properties are, in general, incompatible.[5] As noted earlier, at the pre-quantum mechanics stage of his thinking concerning the difficulties of quantum theory, Bohr, too, was trying to find some pictures of what is going on at the quantum level. However, he abandoned such attempts in the wake of Heisenberg's discovery of quantum mechanics, or indeed already in the wake of the collapse of the BKS proposal, which compelled him instead to try to "dream up" proper *symbolic* analogies. This task was greatly facilitated by Heisenberg's symbolic quantum mechanics, which ultimately led Bohr to nonclassical epistemology, whereby no intuition concerning what happens at the quantum level is possible. By contrast, Schrödinger, with Einstein, would prefer our thought to be able to reach farther, even if not quite all the way, in our description of the underlying physical reality.

The history of Schrödinger's thinking, leading to his wave quantum mechanics, is of much interest and importance, although I can address here only the most essential junctures of this history. The most definitively established and, arguably, most significant influence was de Broglie's work on matter waves. This work was based on his idea of attributing a wave character to particle-like objects, such as electrons, following Einstein, who was compelled, conversely, to ascribe a particle character to wave-like objects, such as light. Einstein also thought that some form of "fusion" of both conceptions would be necessary in quantum theory. However, he was reluctant to accept Bohr's solution of this problem through complementarity, which, again, abandoned both conceptions at the quantum level, rather than fused them, as Einstein thought to be desirable.[6] This was an ambitious aim, thus far accomplished, as in Bohm's theory, only at the cost of nonlocality, equally unacceptable to Einstein, who, as is well known, was not satisfied with the initial version of Bohm's theory, offered in 1952. Several among Einstein's other ideas, including those in turn based on de Broglie's work, and Einstein's general relativity theory also stimulated Schrödinger's thinking in (terms of) waves. The shift toward a

[5] The idea is by no means dead. Thus, J. S. Bell was an ardent supporter of quantum "beables," mostly along the lines of de Broglie–Bohm's thinking (e.g., Bell 1987, p. 173).

[6] It is not clear to what degree Einstein realized that Bohr's approach was not grounded in wave-particle complementarity, especially in Bohr's post-EPR writings. Einstein does not appear to have been at ease with the concept of complementarity (in the narrow sense) and says at one point that he was "unable to attain …the sharp formulation … [of] Bohr's principle of complementarity" (Einstein 1949b, p. 674).

preference for the wave theories in both cases, that of Einstein and that of Schrödinger, is itself noteworthy, and the influence of Einstein's work on Schrödinger's ideas concerning his wave mechanics is unquestionable, just as is his influence upon Schrödinger's earlier predilection for quantum discontinuity or statistical considerations. This change is an intriguing subject in its own right, which cannot, however, be addressed here. Schrödinger also did important work in acoustical theory and other subjects, including vibrations of a string, where he dealt with waves and with representations of the physical phenomena, mathematically encoded in a particular way (wave equations) that needed to be thought of in terms of waves. In any event, it becomes clear even upon a relatively cursory examination that Schrödinger arrived at his wave mechanics in a series of complicated moves and oscillations between different views (physical, mathematical, and philosophical).

The problem or set of problems that Schrödinger posed might be described as follows. Could one find a *wave*-type equation that would describe the physical processes at the quantum level, such as the behavior of an electron in the atom, and that would enable us to predict the results of quantum mechanical experiments? What would the (wave-like) character of the processes corresponding to this equation be, given the peculiar features of quantum physics—in particular, discreteness and indeterminism—which correspond to the outcome of experiments and which, accordingly, must be retained? Can a wave-like theory solve then outstanding problems of quantum theory? Would such a solution allow us to capture, at least by way of an idealized approximation, the ultimate reality of nature in a wave-like picture? While, especially in retrospect, Schrödinger's program appears to have been bound to fail, the very possibility of these questions reveals a number of deeper aspects of modern physics, classical and quantum, and of the relationships among physics, mathematics, and philosophy, which are crucial to quantum mechanics and debates concerning it. In particular, as I indicated above, Schrödinger oscillated between offering a merely intuitive (*anschaulich*) wave theory and offering a realist, even an approximately realist one. He thought, however, that matrix mechanics was deficient either way, a contention that, as will be discussed in the next chapter, was challenged on the account of intuitive visualizablity by Heisenberg in his uncertainty-relations paper, essentially by arguing that matrix mechanics is not any less visualizable than Schrödinger's wave mechanics (Heisenberg 1927, p. 82n.). Schrödinger's realist and causal agenda as concerns the fundamental processes of nature was there from the outset and, it appears, had always remained on his mind. One must, again, keep in mind that Schrödinger was mindful of the approximate or idealized character of all our physical theories, as he explained it in his cat-paradox paper.

Schrödinger's article on the Bose–Einstein gas theory, following Einstein's investigation and immediately preceding his work on his wave mechanics, makes a very strong statement of this agenda, thus making this work a major juncture on his road toward his theory. This statement anticipates his subsequent grasp, as well as his dislike, of the essence of quantum mechanics in its

Heisenbergian form, as expressed in his statements following his introduction of his wave mechanics and then his cat-paradox paper in 1935. In 1925, in the immediate wake of de Broglie's and Einstein's work (using Satyendra Nath Bose's new statistical counting procedures for a quantum gas, with the existence of the Bose–Einstein condensate as one of the outcomes), his hopes for a wave theory as a proper alternative to Heisenberg's approach were high. He especially guards, even by a peculiar appeal to "our natural feeling," against applications, such as that used by Einstein, of the Bose–Einstein statistics (as something essentially and irreducibly quantum) "to the motion of gas molecules." He says "Our natural feeling rightly guards against accepting this new statistics as something primary, which cannot be further explained" (Schrödinger 1926a, p. 95; cited in *MR* 5, p. 435). This is of course the view that Bohr was compelled to adopt and that defines the argument of this study. As against his low expectations or skepticism concerning the new quantum thinking of Bose or (clearly on his mind as well) that of Heisenberg, Schrödinger's hope for "gain[ing] a deeper insight in the real nature of the new theory" lay in his belief in the power, tested over and over again, of classical statistical methods. He advocated "keeping the old statistical methods [entailing the underlying causality of individual processes], tested by experience and logically well-founded," as part of a new quantum theory (Schrödinger 1926a, p. 95; cited in *MR* 5, p. 435).

This view combines his classical philosophy of physics with his earlier welcoming of statistical approaches to quantum mechanics, such as that of Bohr, Kramers, and Slater (again, abandoned by 1925 because of its other problems, especially the conflict with conservation laws, which was a major concern for most physicists but did not appear to trouble Schrödinger that much). The latter theory could still be grounded in the classical-like, including causal, nature of the underlying individual processes, and, as I said, was the last-ditch attempt on Bohr's part to keep the semi-classical approach to quantum phenomena alive. The change, Schrödinger argues, might and even will be required "but *at a point* [of the theory] where one can do so without *sacrificium intellectus*" or without relying on a lucky guess, as was done (presumably) by Bose in his theory or by Heisenberg in the case of matrix mechanics. In order to do so, first, "one must construct the picture of the gas simply according to *that* picture of cavity radiation, which does *not yet* correspond to the extreme light-quantum concept," in other words, a classical picture. "Then the natural statistics—say, via the suitable Planck method of the state sum—will yield Einstein's gas theory" (Schrödinger 1926a, p. 95; cited in *MR* 5, p. 435). What will make this statistics natural is that it will do so without making use of the unmotivated (by proper mechanical considerations) counting procedure of Bose. This is to be accomplished, logically, through de Broglie's idea of matter waves. Schrödinger says: "This means nothing more than taking seriously the *undulatory theory* of the moving corpuscle proposed by de Broglie and Einstein, according to which the latter [i.e. the corpuscle] is nothing more than a kind of 'white crest' on the wave radiation forming the basis of the universe" (Schrödinger 1926a, p. 95; cited in *MR* 5, p. 435). The appeal to "the *undulatory* *theory* of the moving corpuscle" encapsulates both Schrödinger's vision of quantum theory and the difficulties it will face. "*The basis of the universe*," no less! The phrase is as remarkable for its philosophical ambition as for a physical one.

The success of Schrödinger's paper itself was mixed but not inconsiderable, in retrospect also because it was mixed. While there was a problem with the Bose–Einstein condensate (which was a major drawback, although not apparent at the time, since the condensate was not yet experimentally observed), his argument established the discrepancy, rather than a strict equivalence, between the physical behavior according to the wave and to the corpuscular theories. The resulting wave theory itself was complicated as well, especially as concerns a possible representation of molecules and light quanta in three-dimensional space by the superposition of plane waves, which had to be accomplished by the theory. It became clear that "the universal radiation, whose 'signals' or perhaps singularities should be the corpuscles, ... constitutes an essentially more complicated phenomenon than, say, the wave radiation of Maxwell's theory." Schrödinger also noted that "one *cannot*, of course, achieve within the classical wave laws that the 'model of a light-quantum' thus constructed—which by the way extends over *many* wavelengths in all (space) directions—also remains together *at all times*. The object rather dissipates after going through a focus into steadily extending space dimensions" (Schrödinger 1926a, p. 101; cited in *MR* 5, p. 442).

In sum, Schrödinger's classical agenda of deriving quantum effects from the underlying classical situation is already in place here, with, however, a wave-theoretical twist. The presence of this agenda, I hasten to add, does not diminish the enormous significance and radical nature of Schrödinger's step from the recognition of the significance of de Broglie's waves to his nonrelativistic equation for the electron. It is worth noting that, as some of his references in the paper (including to Maxwell) suggest, by that time Schrödinger had already worked on and still continued to hope for a relativistic equation. Indeed he wrote one, now known as the Klein–Gordon equation, which eventually proved unworkable for a nonrelativistic electron. The (free) relativistic electron is described by Dirac's equation, discovered in 1928. One can write Schrödinger's equation for the electron in the hydrogen atom in the quantum electrodynamical regime, but one cannot solve it exactly. In any event, Schrödinger's philosophy compelled him to postulate the underlying causal dynamics of fundamental constituents and processes in nature. This dynamics then may under certain circumstances lead to probabilistic outcomes in predicting actual situations, similarly to the way it happens when we use probability and statistics in classical physics.

This philosophy is in sharp and, as just explained, deliberate contrast ("our natural feeling rightly guards against accepting this new statistics as *something primary, which cannot be further explained*") to the irreducibly probabilistic character of primordial individual processes or events, or otherwise physically unmotivated statistical counting, such as that of Bose, or, as became clear in retrospect, that of Planck. Apart from possibly altogether giving up, late in his life, a hope that physics would ever be suited to approach the ultimate constitution of nature, Schrödinger never gave up on this idea or ideal. It was the *classical ideal*, as he, again, saw it in the cat-paradox paper in order to juxtapose it to the *nonclassical* (in either sense) "doctrine" defining quantum mechanics in the spirit of Copenhagen, "the doctrine ... born of distress" (Schrödinger 1935a, *QTM*, pp. 153–154). By then, in 1935, the mathematics of the doctrine would include his equation, no longer offering any hope for an alternative to the "doctrine." In 1926, however, there was a crucial twist insofar as it appeared possible to provide the underlying causality to quantum phenomena by means of wave rather than particle motion of quantum objects, even though the observed phenomena in question would appear to correspond to particles, such as electrons or photons. In other words, along with probabilistic effects and in part correlatively, particles, too, are *effects* of the underlying "universal radiation," "the wave radiation forming the basis of the universe."

Of course, in contrast to Heisenberg's matrix mechanics based on "quantities which in principle are observable," Schrödinger's causal "wave radiation forming the basis of the universe" was unobservable, perhaps in principle unobservable. As will be seen below, Sommerfeld was to *observe* this immediately, upon receiving the manuscript of Schrödinger's first paper on his wave equation, marveling nevertheless at the coincidence of the prediction obtained by the two theories (Sommerfeld to Schrödinger, February 3, 1926, cited in *MR* 5, p. 502). As will be seen in the next chapter, however, while quantum waves were by and large (but, again, not altogether) abandoned by quantum theory, the idea itself of the underlying unobservable causality of independent, *undisturbed-by-observation,* quantum processes has continued to persist and still does.

Schrödinger's next step toward his equation is captured by a passage in a letter to Wien, cited above, concerning the apparent equivalence of the two descriptions, in terms of the particle and the wave motion. It is important to keep in mind that, as Schrödinger stresses, unlike de Broglie's propagating waves, his original (time-independent) equation was written for a standing wave, "a standing vibration of the whole atomic region." Solving this time-independent equation gives one the hydrogen spectrum in a much more immediate and mathematically easier way than matrix mechanics did, a feature—also found in his mathematical program in general—that assured the immediate success of his approach vis-à-vis the matrix one. In addition, while in Schrödinger's "picture" there are no particles at the ultimate level, in de Broglie's case, a wave would be accompanying the particle in question, in this case an electron, in the manner of a pilot or ghost wave. De Broglie originally spoke of it as a "fictitious" wave. Einstein had already proposed something along these lines earlier, and although he ultimately rejected his ideas to that effect (in their specific form, they led to several problems), it served as an inspiration for de Broglie. Einstein's idea inspired Born as well in his work on the probability interpretation of the wave function. While the idea of physical waves propagating in space was abandoned altogether, Born conceived his probability wave as a kind of pilot or ghost wave of potentialities or propensity (tendency), a notion going back to Aristotle that, as noted in the preceding chapter, was eventually adopted by Heisenberg, but never by Bohr. Einstein, in following de Broglie's ideas, specifically invokes the possibility of connecting "with every process of motion an undulatory field, . . . whose physical nature is so far still in the dark—by the phenomena of motion corresponding to it" (Einstein 1925, p. 10, cited in *MR* 5, p. 558).

In Schrödinger's wave mechanics, then, there were only waves at the level of the ultimate constitution of nature, with "particles" seen as certain singularity-like surface effects. He, again, sometimes stopped short of claiming that these waves actually represent the ultimate reality of nature, as opposed to providing an intuitively accessible (*anschaulich*) theory sufficiently approximating, as an idealization, this reality. The theory would also possess a powerful predictive, even if, in terms of underlying reality, insufficient descriptive power—insufficient, but not completely absent in the way it was in Bohr's or Heisenberg's view of quantum mechanics. It also allowed one, as did Dirac's q-number scheme, but not the matrix scheme, to offer a quantum mechanical representation of stationary states. We recall that the old quantum theory was dealing, with limited success, with either (in the case of electrons) particles and their motion, or (in the case of radiation) an uneasy coexistence of a particle-like description in terms of photons—a product of, in Bohr's words, "Einstein's unfailing intuition"—and a wave-like description. As Bohr stressed on the same and several other occasions: "Notwithstanding its fertility, the idea of the photon implied a quite unforeseen dilemma, since any simple corpuscular picture of radiation would obviously be irreconcilable with interference effects, which present so essential an aspect of radiative phenomena, and which can be described only in terms of a wave picture. The acuteness of the dilemma is stressed by the fact that the interference effects offer our only means of defining the concepts of frequency and wave-length entering into the very expressions for the energy [$E = h\nu$, where ν is the frequency] and momentum [$P = h\sigma$, σ-wave length] of the photon" (Bohr 1949, *PWNB* 2, p. 34).

In other words, our formalism and numerical considerations depend on the concepts and numbers derived from the conceptually incompatible considerations. It is no wonder that Schrödinger wanted to dispense with particles altogether. As it happened, however, from the present perspective at least, this was not sufficient. One needs ultimately to dispense with both—particles *and* waves. De Broglie's theory, which tried, by contrast, to *combine* both descriptions in the case of a single moving object, contains the same type of problem, which Bohm's theory solved at the cost of nonlocality. In Heisenberg's theory there would be no waves, but at the cost, unacceptable to Schrödinger (or de Broglie and Einstein), of renouncing any description of physical processes concerning electrons in space–time, eventually leading Bohr to a nonclassical epistemology of quantum theory. Waves would either be used symbolically, in conjunction with Born's probability interpretation of the wave function, or metaphorically, in relation to certain wave-*like* effects comprising multiple discrete individual phenomena in certain circumstances, as exemplified by the appearance of the interference pattern in the double-slit experiment.

5.3 Schrödinger's Equation

There are well-known and *relatively* straightforward paths to Schrödinger's equation, and Schrödinger indicated some of them in his initial article. However, these paths may only be seen as relatively straightforward. First of all, they require complex manipulations of the formulas and concepts involved. Secondly, they depend on certain assumptions justified only by their correspondences with experiments, rather than by mathematical derivations, the point rightly stressed by Heisenberg (Heisenberg 1930, p. 108). Perhaps the most natural or the most expected way, at least *historically*, to derive Schrödinger's equation is via de Broglie's formulas for phase waves associated with particles, which were crucial to Schrödinger's thinking and which he used in a derivation found in one of his notebooks. This is also one of the most common paths followed in textbooks on quantum mechanics. One proceeds roughly as follows. De Broglie's formula for the speed of the phase wave of an electron, adjusted for the speed of the electron in the electric field of a hydrogen nucleus, is inserted into the classical relativistic wave equation for the wave function, ψ. Since de Broglie's formula conveys both the particle and the wave aspects of the behavior of quantum objects, the nature of the equation changes. Unfortunately, the resulting equation, usually known as the Klein–Gordon equation (although Schrödinger appears to be the first to have written it), does not work for a relativistic electron because the predictions one makes by using it are in conflict with experimental results. One needs Dirac's equation to make correct relativistic predictions. However, if, in the procedure just described, one drops terms that are small at the nonrelativistic limit, which is easily done mathematically, one arrives at a different equation. This is, in essence, what Schrödinger appears to have done initially, as his notebooks indicate. The resulting equation, which is Schrödinger's equation, happens to offer correct predictions of experimental results in the nonrelativistic case.

In terms of theoretical justification, the situation is far more complicated. These complications arise not only and not so much because it is a nonrelativistic treatment of an object that ultimately needs to be treated relativistically, although this is not unimportant. Mehra and Rechenberg even speak in this latter context of "a decisive *spiritual* step forward—and to some extent a sacrifice of the intellect (the hydrogen atom had eventually to be treated in a relativistic manner!)," on Schrödinger's part, "although," they add, "it could be performed easily on the mathematical level" (*MR* 5, pp. 462–463; emphasis added). In addition, as noted above, de Broglie's waves are not the same as Schrödinger's standing wave, which his (time-independent) equation describes. In this respect, too, the procedure, while it works mathematically, is hardly rigorous conceptually, although one might, in principle, also see Schrödinger's approach as giving a different meaning to de Broglie's concept while retaining his formulae. Perhaps most significant, however, is the following problem. One derives the *right* nonrelativistic equation from a *wrong* relativistic one, at a nonrelativistic limit. Accordingly, Schrödinger's (nonrelativistic) equation becomes more of a guess, albeit a correct guess, which would need to be justified otherwise or perhaps even derived otherwise. This is what Schrödinger, aware of these difficulties, attempted to do and, in some measure, accomplished in his published paper, which derives his equation differently. Eventually, Dirac showed Schrödinger's equation to be the nonrelativistic limit of his equation as well, which straightened out the difficulty. This was an important point of Dirac's argument concerning his equation, since by that time Schrödinger's equation was established as the correct nonrelativistic equation for the electron, regardless of derivation. In any event, while Schrödinger's equation gave correct experimental predictions at the nonrelativistic limit, the situation was clearly more complex and uncertain in terms of the theoretical argumentation concerning the equation or its interpretation.

These circumstances may explain why Schrödinger chose a different way to present the derivation of his equation in his paper introducing the equation. His more ambitious program for wave mechanics began to be laid out in his second paper. It is sometimes argued that, by presenting this different derivation, Schrödinger unnecessarily complicated and obscured the case, and that he might have done so in order to further distance his program from that of de Broglie. There are some reasons for either contention, although it is difficult to be certain whether either is true. In any event, either is only of historical interest. One the other hand, one might argue that there were deep conceptual reasons for this change, since this new derivation was also more essentially representative of his program of wave mechanics. It is worth reiterating that, as conceived by Schrödinger, the underlying physical picture of standing waves and strictly vibrational processes in the atom was different from de Broglie's view of these processes (as involving both particles and waves and/in their joint *propagation* in space–time). Schrödinger's approach was also different mathematically, beginning with the idea of using a wave equation to describe the behavior of electron phase waves in atoms, as against de Broglie's direct geometrical treatment of the motion of electrons.

Schrödinger arrived at his view of the problem of quantization as an eigenvalue problem, treated, via his equation, by variational methods, with atomic spectra derived accordingly. If one has an equation, such as Schrödinger's equation, defined by an action of an operator, in this case the energy operator H, upon a vector variable x, then by transforming this variable into $H(x)$, it may be possible to find a vector X such that $H(X) = EX$, where E is a number. X is then called an eigenvector and a is called an eigenvalue or eigennumber. In the case of the time-independent (standing wave) Schrödinger's equation for the hydrogen atom, the eigenvalues E_n are possible energy levels (corresponding to the stationary states) of the electron in the hydrogen atom, which is what gave the equation its truth and beauty, to put it poetically. The time-dependent Schrödinger's equation has a greater generality and in principle can be written for any quantum mechanical situation. It is, again, a separate question, what, if anything, the equation (or the time-independent Schrödinger's equation) *describes*. We do know, however, what it can predict.

I shall not discuss the calculus of variations, since—although one of the most important mathematical tools in physics (from classical physics to relativity to quantum theory and beyond)—it would require a technical explanation and is not essential for my argument. A few basic points may be in order, however. The calculus of variations deals with such problems as finding the shortest curve (a "geodesic") connecting two given points on a given surface, such as that of the sphere—hence its significance for general relativity. The latter considers spaces curved by gravity, as manifested in curving the paths of light in such spaces, but still in such a way that light always follows the shortest possible path there. This principle is an extension of Fermat's principle in optics, introduced by Pierre Fermat in the seventeenth century. The principle states that light always follows the shortest path in the corresponding medium and hence is defined by the material of the medium. Fermat was not thinking in terms of curving space by gravity, but rather of how light travels in different mediums, say, water or thicker liquids. There is a corresponding principle in mechanics, the principle of least action (the quantity defined as the integral of the momentum over a given distance traveled by a body), apparently stated first by Pierre Louis Maupertuis in 1774 and mathematically developed by Leonhard Euler around the same time, and then by Joseph Louis Lagrange and Sir William Rowan Hamilton. Leibniz had had related ideas earlier, and the fact that he was a contemporary of Fermat suggests that this thinking in physics appears to have emerged then. Philosophically, the idea that nature follows the most efficient ways possible can be traced much earlier. Schrödinger's alternative derivation of his equation via the so-called mechanical–optical analogy takes advantage of the fact that the analogy translates the principle of least action into Fermat's principle, and vice versa.

As is clear from his notebooks, Schrödinger's alternative derivation was linked to and possibly motivated by the fact that some of the predictions based on his wave equation coincided with those of Bohr's and Sommerfeld's semiclassical theory, which used the standard, classical-like, Hamiltonian approach to quantum mechanics. Schrödinger explained these relationships in a way that led him to the derivation of his equation found in his first paper. He did so by replacing—without a real rigorous theoretical justification from first principles (a justification never achieved)—the mechanical Hamilton–Jacobi equation $H(q, \partial S/\partial q) = E$ with a wave equation by substituting $S = K \ln \psi$ (where K is a constant that has a dimension of action). This radical step led him, via a mechanical–optical analogy, to the equation

$$\Delta\psi + \left(\frac{2m}{K^2}\right)(E - V)\psi = 0$$

and then to the right equation for the nonrelativistic hydrogen atom,

$$\Delta\psi + \left(\frac{2m_e}{K^2}\right)\left(E + \frac{e^2}{r}\right)\psi = 0.$$

Here $K^2 = h^2/4\pi^2$, h is the Planck constant, and m_e the electron mass. The Hamilton–Jacobi equation considered by Schrödinger was already used in the old quantum theory, specifically by Sommerfeld and Paul Epstein, as well as (with proper quantum mechanical adjustments) by Born, Jordan, and Heisenberg in matrix mechanics and by Dirac in his version of quantum mechanics. Schrödinger's key step—made via the mechanical–optical analogy and the connections between the principle of least action in mechanics and Fermat's principle in optics, as just explained—was to give to S the wave form by putting $S = K\, ln\, \psi$. In the case of the hydrogen atom, one thus also replaces the deterministic mechanics of the particle motion with amplitudes and then probabilities, although Schrödinger did not realize this at the time. He aimed at a (wave-like) deterministic picture, but ultimately arrived elsewhere.[7]

These are, historically and conceptually, far-reaching connections, which, as we have seen, were equally crucial earlier for the matrix version of Heisenberg, Born, and Jordan's, or Dirac's versions of quantum mechanics. The latter also retain the formal structure of Hamiltonian equations, but expressly change the variables to which these equations apply. Accordingly, if one follows the derivation of Schrödinger's equation given in his first paper, one is led nearly directly to the mathematical connections and, ultimately, mathematical (even if not physical or epistemological) equivalence between their versions and that of Schrödinger. It took only a few months to establish this equivalence, which was done first by Pauli and then, almost immediately and independently, by Dirac and Schrödinger himself. One can also extend these connections to Schrödinger's derivation of his equation and Heisenberg's procedure of deriving his matrix mechanics, and, with it, to Bohr's correspondence principle, which grounded Heisenberg's argument, as considered earlier. In other words, both derivations may be seen as ways of using the correspondence principle in its mathematical form (as defined in Chapter 3)—explicitly in the first case and implicitly in the second, since Schrödinger himself appears to have seen his wave mechanics as a way to bypass the use of the correspondence principle. Dirac saw his q-number program along somewhat similar lines, and an analogous case can be made for his program; as noted in Chapter 3, he does invoke the mathematical form of the correspondence principle, which is what is really at stake in my argument at the moment. In all of these cases, one recovers classical equations and variables and hence can make the same predictions by classical means in the region of large quantum numbers—in other words, in the case of Schrödinger's

[7] Once can hardly miss yet another parallel here, that between his formula for S and his fellow-Viennese Ludwig Boltzmann's famous formula for entropy (inscribed on Boltzmann's gravestone), which could have suggested at least certain formal connections between waves and entropy and hence between information and probability.

scheme, once one can ignore quantum waves, whether one interprets them physically (as Schrödinger would want) or probabilistically.

Schrödinger's starting point and his philosophy that defined it were quite different from those of Heisenberg, who renounced the actual description of quantum processes themselves or, earlier, Bohr, who offered no physical explanation (which he thought hopeless) to the electron's quantum jumps between stationary states. Schrödinger, by contrast, hoped that such an explanation would be found and, moreover, that it would be classical-*like* in nature, although, as was indicated earlier, it was bound to be different from a classical wave propagation, such as that of Maxwell's theory. One might even say that, in some respects, Schrödinger guessed his equation more than Heisenberg did his formalism, since Heisenberg derived his new matrix formalism from the nature of the experimental data, having abandoned the search for the space–time physical description of subatomic processes as unproductive and perhaps impossible. Schrödinger, by contrast, believed that a proper descriptive and descriptively classical-like argument for and the corresponding derivation of his equation would be found, in part in view of the apparently more intuitively (*anschaulich*) natural shape of his wave equation. As it happened, eventually this greater intuitive naturalness was only apparent, and both formalisms would reflect the difficulties of achieving a classical-like description of the behavior of the electron. Initially, however, and even when this equivalence was established, finding such a space–time wave description of quantum processes seemed possible, and Schrödinger used his new derivation of his equation as a justification for this view.

This apparently (but, it follows, only apparently) strange parallel with Heisenberg's approach becomes even more pronounced if, against Schrödinger's philosophy, one suspends the physical significance of quantum waves. If one does so, Schrödinger's approach to deriving his equation as just explained is almost self-evidently equivalent to that of Heisenberg (who knew of Sommerfeld's semiclassical work, using the Hamilton–Jacobi equation, although his trajectory to matrix mechanics was different). This equivalence becomes even more manifest if one considers the matrix mechanics in its full-fledged form rather than in the nascent form of Heisenberg's first paper. For—especially, again, if one does not think in terms of physical waves—Schrödinger's procedure is very similar. It replaces the action function S of the classical Hamilton–Jacobi equation with a rather different type of variable. This variable is seen as a wave function, "amplitude," in part by virtue of the optical-mechanical analogy used by Hamilton and others. (As I shall explain, however, this analogy may also be misleading.) However, this amplitude is a *probability* amplitude, as its "entropic" relation to action in Schrödinger's substitution formally suggests; and, as we have seen, the same type of probability amplitudes were involved in Heisenberg's matrix elements, arising there from Fourier's representation of motion. Accordingly, once we move to Schrödinger's equation via Hamilton–Jacobi mechanics rather than via de Broglie's scheme, one can, especially with Born's probabilistic interpretation of the wave function in hand, also perceive the deeper conceptual relationships between Heisenberg's and Schrödinger's schemes, rather than only a more or less straightforward mathematical equivalence. Born's interpretation de facto develops these relationships, using the more general character of Schrödinger's equation rather than only the transition probabilities involved in Heisenberg's argument.

As a bonus, the connection to the Hamilton–Jacobi mechanics also gives us Bohr's correspondence principle in its mathematical form for large quantum numbers, that is, a coincidence of the results of the quantum mechanical and classical treatment, when the latter can be used. As we have seen, one might conversely use the principle in this form, as Heisenberg did, to justify the use of the classical mechanical equations in the first place. This connection to the correspondence principle is, again, amplified by the difference between the two derivations of Schrödinger's equation in question. In the first derivation, via de Broglie, one starts with a classical electrodynamics problem and the corresponding (wave) equation, which is then transformed into a nonrelativistic one by neglecting certain (nonrelativistically) small magnitudes. In the second derivation, one starts with the equations of classical mechanics (which are then recovered exactly when one applies the correspondence principle) and replaces the variables with a wave function by putting $S = K ln \psi$. At least in the case of the actual wave equation for quantum systems (i.e., when h enters the coefficients involved), this formula related to probabilities, although this relation has, again, no physical justification. It is ad hoc. In Schrödinger's initial view, for large quantum numbers the vibrations of the universal radiation would be negligible. This would justify the correspondence principle, although it would not give it a special role of the kind it played in Heisenberg's initial approach.

The argument just offered is Heisenbergian in spirit, or is in the spirit of Copenhagen rather than in the spirit of Schrödinger's thinking. If anything, this argument may be seen as proceeding against Schrödinger's philosophy and his view of the wave function at the time, although eventually he, again, accepted that the wave function might not work along the lines of his initial thinking. Indeed, even though he did not offer any such theory in his paper, Schrödinger suggested, strongly, that the wave function of the hydrogen atom could be understood as representing some "*vibration process* in the atom, which would more nearly approach reality than electronic orbits, the real existence of which is being very much questioned to-day. I originally intended to found the new quantum conditions in this more intuitive [*anschaulich*] manner, but finally gave them the above neutral mathematical form, because it brings more clearly to light what is really essential" (Schrödinger 1926b, pp. 371–372, cited in *MR* 5, p. 533). It is worth reiterating that by then the practitioners of matrix mechanics had effectively given up on orbits, or on any picture of atomic processes. This was not something that Schrödinger was willing to entertain, at least at that point.

Although Schrödinger's program received a quick enthusiastic reception, most notably on Einstein's part, and for different reasons on the part of Bohr, Born, and Pauli from the Copenhagen–Göttingen side, the skepticism toward the program emerged ever earlier, in part (but not exclusively) in view of

Heisenberg's matrix theory and its epistemological implications. Thus, according to Sommerfeld, writing to Schrödinger: "The difference of the points of departure between your and Heisenberg's approaches is peculiar in the light of the same results. Heisenberg starts from the epistemological postulate not to put more into the theory than can be observed. You put in all kinds of possible frequency processes, node lines and spherical harmonics. After our epistemological knowledge has been sharpened by relativity theory, the large, unobservable ballast in your presentation also seems to be suspicious for the time being" (Sommerfeld to Schrödinger, February, 3 1926, cited in *MR* 5, p. 502).

Schrödinger defended his approach on this last point, but interestingly, seeing Sommerfeld's objection as concerning "possibly unnecessary assumptions" rather than "unobservable ballast." This is a crucial difference (missed or at least not commented upon by Mehra and Rechenberg), arising in part from Schrödinger's view of the situation in terms of trajectories of motions, "the electron orbits with their loops," as represented by the "the fundamental equations of mechanics" (Schrödinger to Sommerfeld, February 20, 1926, cited in *MR* 5, p. 502). Orbits and all of the classical mechanical concepts of motion were, again, abandoned by Heisenberg in view of the problems, by then just about insurmountable, that they posed. As Schrödinger was aware, his scheme by no means resolved these problems at the time or later when his time-dependent equation was introduced, although he did hope to avoid them by reconceptualizing all these processes in terms of wave motion. The above quotation from his paper clearly suggests both his awareness of these problems and his hope of resolving them by appealing, as just mentioned, to some "*vibration process* in the atom, which would more nearly approach reality than electronic orbits, the real existence of which is being very much questioned today" (Schrödinger 1926b, p. 371, cited in *MR* 5, p. 533). His theory was never able to fulfill this promise. Thus, Sommerfeld's point was not really countered by Schrödinger's subsequent papers, in which he promised to provide a more general foundation of the theory (Schrödinger to Sommerfeld, February 20, 1926, cited in *MR* 5, p. 502). Sommerfeld's observation was astute; and as will be discussed in the next chapter, certain aspects of the problem he detected in Schrödinger's approach, specifically those related to the causality of independent (unobserved) quantum processes, were to persist well beyond Schrödinger's program for his wave mechanics.

5.4 Wave Mechanics Between Optics and Mechanics

Schrödinger pressed on with his program in his second paper, which, as Mehra and Rechenberg note, "establishe[s] the foundations and the definite outlines of what was later [in his next communication] called 'wave mechanics' " (*MR* 5, p. 533). The program was, as I said, to be enacted through the mechanical–optical analogy, accompanying, since Hamilton's work, the Hamilton–Jacobi framework for classical mechanics, the connections to which Bohr, too, refers on a number of occasions (e.g., Bohr 1929b, *PWNB* 1, p. 9). Schrödinger wanted to "throw more *light* on the *general* correspondence which exists between the Hamilton-Jacobi differential equation of a mechanical problem and the 'allied' *wave equation*," that is, Schrödinger's equation (Schrödinger 1926c, p. 13; cited in *MR* 5, p. 533; emphasis on "light" added). The mathematical procedures used in the first paper are now declared "unintelligible" and "incomprehensible," from, one presumes, physical and conceptual viewpoints. It is crucial to keep in mind that—as is clear from his second paper and his other papers on wave mechanics and from his accompanying statements and correspondence—Schrödinger was aware that his wave mechanics would have to be different from previous wave theories. Accordingly, he anticipated that some changes of the physical concepts involved were likely to be necessary, in part in order to preserve the classical-like (causal and realist) character of the theory. It is worth citing Schrödinger's notebook comments written in preparation for his second paper:

> The somewhat dark connection between the Hamiltonian differential equation [$H(q, \partial S/\partial q) = E$] and the wave equation [$\Delta\psi + (2m/K^2)(E - V)\psi = 0$] must be clarified. This connection is not new at all; it was, in principle, already known to Hamilton and formed the starting point of Hamilton's theory, since Hamilton's variational principle has to be considered as *Fermat's principle* for a certain wave propagation in configuration space, and the partial differential equation of Hamilton as *Huygens' principle* for exactly this wave propagation. Equation [$\Delta\psi + (2m/K^2)(E - V)\psi = 0$] is nothing but—or better, just a possible—wave equation for exactly this wave process. These things are generally known, but perhaps I should recall them at this point.[8]

The situation is more complicated than Schrödinger makes it sound, especially if one takes into account subtler aspects of these connections, as they apply to the quantum mechanical, instead of the classical, case. These connections became even darker as quantum mechanics developed, in spite of its successes as a physical theory. They still are dark and may remain dark for a long time to come, possibly forever. The analogy itself was well known to and was explored by Schrödinger, both in his work on his wave mechanics and in his earlier studies, specifically that of tensor-analytical mechanics and Hertz's physics. The analogy may best be described via Felix Klein's argument, which Schrödinger cited, and he knew the argument itself before he encountered Klein's discussion of the case. One may express the main idea as follows: *Every Hamiltonian system of classical dynamics, such as that of one or an ensemble of particles, can be considered in terms of the motion of a wave front in a suitably chosen medium, although, in general, in a higher-dimensional space, rather than in a three-dimensional one in which actual physical processes occur.* The last qualification is crucial, and I shall explain its significance below. The genealogy of the analogy extends from Pierre–Simon de Laplace, who considered the propagation of light in terms of corpuscular theory. From Hamilton

[8] *Archive for the History of Quantum Physics* Microfilm No. 40, Section 6, p. 1 (cited as Notebook II, p. 1 in *MR* 5, p. 543).

on, the analogy involves the relationships among geometrical optics, wave optics, and mechanics, and the workings of respective variational principles there—Fermat's principle in optics and the principle of least action in mechanics. It is worth citing Klein, however, since his statement expresses the key aspects of the analogy. According to Klein

> Hamilton met with the concepts of the emission theory, according to which the determination of the light ray passing through an arbitrary inhomogeneous (though isotropic) medium is a special case of a usual mechanical problem dealing with the motion of a mass point; we may immediately add the remark that this specialization [of the mechanical problem] is not an essential one, that is, one rather may reduce, *by implying higher-dimensional spaces*, each mechanical problem to determining the path of a light ray in a suitable medium. (Klein 1892, pp. 35–36, cited in *MR* 5, p. 517; emphasis added)

I interrupt the quotation to note that Klein reverses Hamilton's optical-mechanical analogy into a mechanical–optical analogy, or more accurately, reverses Hamilton's translation of an optical problem into a mechanical one, offering instead a translation of a mechanical problem into an optical one. This reversal played a key role in Schrödinger's work on his wave mechanics. Moreover, as I shall discuss below, this type of translation involves a shift, if not a misplacement, of the physical situation into a mathematical one, in view of using spaces of higher dimensions, which must be handled with caution even in classical physics. The situation is much more complex in quantum mechanics. Schrödinger anticipated this greater complexity from the outset of his work on wave mechanics. However, the situation proved more difficult than he hoped. To resume Klein's passage

> And now *Hamilton's discovery, according to which the integration of the differential equations of dynamics is related to the integration of a certain partial differential equation of the first order*, simply rests on the fact that Hamilton, following the great physical movement of his time, embarked on deriving the results of geometrical optics, known on the basis of the emission theory, from the point of view of the undulation theory. Hamilton's integration theory of the differential equations of dynamics is, in the first place, nothing other than a general analytic formulation of the well-known relations in physics between a *light-ray* and a *light-wave*. In the light of the thus given point of departure, one may understand the unnecessarily special form, in which Hamilton published his theory and which was then generalized by Jacobi. In investigating systems of rays, Hamilton first had in mind quite practical problems of instrument theory. Hence, he operated exclusively with light waves that are emitted from single *points*. Jacobi's generalization then amounts to the fact that *one may use for the definition of the ray other arbitrary light waves as well*. Starting from special waves, one constructs in optical theory the general ones—as is well known—with the help of the so-called Huygens' principle; this construction represents an exact [physical] equivalent for the analytical process, by which one obtains in the theory of partial differential equations of first order the general solution by starting from any [particular] "complete" solution. (Klein 1892, pp. 35–36, cited in *MR* 5, p. 517)

As just indicated, Klein's representation of the situation is shifted away from Hamilton's own view of his work and is a clear and deliberate generalization of Hamilton's argument. This, however, is not that crucial in the present context.

On the other hand, the mathematical character of the situation—Klein's main focus, as against but also in relation to the possible physics involved, classical and especially quantum—is a crucial issue. To appreciate its significance, it is worth following briefly Schrödinger's thinking concerning these relationships, and more generally the relationships between mathematics and physics, on the one hand, and between physical theory and physical reality, on the other. As Schrödinger writes to Sommerfeld:

> The ψ-vibrations are naturally not electromagnetic vibrations in the old sense. Between them some *coupling* must exist, corresponding to the coupling between the vectors of the electromagnetic field and the four-dimensional current in the Maxwell-Lorentz equations. In our case the ψ-vibrations correspond to the four-dimensional current, that is, the four-dimensional current must be replaced by something that is derived from the function ψ, say the four-dimensional gradient of ψ. But all this is my fantasy; in reality, I have not yet thought about it thoroughly. (Schrödinger to Sommerfeld, February 20, 1926, cited in *MR* 5, p. 542)

Schrödinger does, however, close his second paper with a speculative conclusion, which envisions a wave mechanics:

> *We know today, in fact, that our classical mechanics fails for very small dimensions of the path and for very great curvatures.* Perhaps this failure is in strict analogy with the failure of geometrical optics, i.e. "the optics of infinitely small wavelengths," that becomes evident as soon as the obstacles or apertures are no longer great compared with the real, finite, wavelength. Perhaps our classical mechanics is the *complete* analogy of geometrical optics and as such is wrong and not in agreement with reality; it fails whenever the radii of curvature and dimensions of the path are no longer great compared with a certain wavelength, to which, in q-space, a real meaning is attached. Then it becomes a question of searching for an undulatory mechanics, and the most obvious way is the working out of the Hamiltonian analogy on the lines of undulatory optics. (Schrödinger 1926c, pp. 496–497, cited in *MR* 5, p. 559)

The physical analogy with optics, thus, should be carefully distinguished from the Hamiltonian one. The latter type of analogy only gives one the wave function in a q-space; this function then should be related to some actual physical (vibrational) process. Related considerations are found throughout the paper. Schrödinger writes, for example, "We *must* treat the matter strictly on the wave theory, i.e., we must proceed from the *wave equation* and not from the fundamental equations of mechanics, in order to form a picture of the manifold of the possible processes" (Schrödinger 1926c, p. 506, cited in *MR* 5, p. 569). Schrödinger also develops a reversed argument to the effect that the true mechanical processes in nature are represented by the *wave processes* in q-space and not by the motion of *image points* in that space. This argumentation reflects both the possibilities (and hopes) for wave mechanics and the potential difficulties involved as well as Schrödinger's awareness of these difficulties. Schrödinger offers a number of observations concerning the nature of quantum processes as reflected in his equation and comes close to the probabilistic interpretation of it, but never quite gets there. Perhaps he could not, given his overall philosophy and agenda. For it follows from his equation, even apart from wave mechanics, that it is difficult and perhaps impossible to speak of the path of an electron in the atom, in accordance with the ideas of classical mechanics. In this respect Schrödinger was, as he acknowledged, in agreement with the views of Bohr, Heisenberg, Born, Jordan, and Dirac. However, he was reluctant to accept the kind of suspension, let alone prohibition (this view came later, though), of any description of the underlying quantum behavior ("reality"), which ideal of classical physics he was not about to surrender.[9] He was more ready to give up his equation—as he reportedly said to Bohr at some point, he was sorry to have discovered it. Eventually he gave up *on* it as reflecting the ultimate workings of nature.

5.5 Quantum Mechanics Beyond Mechanics and Optics

What is one to make of Schrödinger's work on wave mechanics, then, beyond acknowledging, as one must, his discovery of this equation as one of the great achievements of twentieth-century physics? One cannot, I think, hope for an easy answer, given, on the one hand, the labyrinthine complexity and contortions of his project, unavoidable given the difficulties of what Schrödinger wanted to achieve, and on the other, the many interpretive decisions involved in assessing the case. Such decisions would concern Schrödinger's own thinking, the nature of his equation, wave mechanics, quantum mechanics, classical physics, relationships between physics and mathematics, the history of quantum mechanics and of classical physics, and many other things. As I suggested earlier, similarly to Planck in the case of his black body radiation law, Schrödinger largely, albeit not altogether (which is also true in Planck's case), guessed his equation, rather than derived it from first principles. According to Mehra and Rechenberg

> In the beginning of the new atomic theory there stood a wave equation for the specific example of the hydrogen atom. Schrödinger had essentially guessed its structure and form [in his first paper]; its derivation or—more adequately—connection with the dynamical equations of the old quantum theory of atomic structure (working with the Hamilton-Jacobi partial differential equation) had been rather artificially forced. This was soon felt by Schrödinger himself. ... Did the new formulation of the foundations of undulatory mechanics lead in a less arbitrary and artificial way to wave equations that described atomic systems and processes, notably the successful non-relativistic hydrogen equation? ... Was the wonderful analogy, between Hamiltonian mechanics in higher-dimensional non-Euclidean spaces and the undulatory optics, just highbrow idealistic decoration and useless for the practical purposes of atomic theory? (*MR* 5, pp. 571–573)

Mehra and Rechenberg are right to reject "so pessimistic a view of [Schrödinger's] achievement," suggested by the last question (*MR* 5, p. 573). They do not, however, explore the deeper complexities of the situation or the

[9] See Mehra and Rechenberg's discussion (*MR* 5, pp. 570–571).

optical-mechanical analogy of Klein and Schrödinger as reflecting these complexities, as indicated in the preceding section. I would like to pursue the subject further in order to give a clearer picture of the situation. It is worth restating the main point of this analogy in classical mechanics. *Every Hamiltonian system of classical dynamics, such as that of one or an ensemble of particles, can be considered in terms of the motion of a wave front in a suitably chosen medium, although in general in a higher-dimensional space, rather than in a three-dimensional one in which actual physical processes occur.*

The approach thus defined is powerful and effective in developing the mathematical formalism of classical mechanics. However, it may also be misleading, even in the case of classical physics. For the optical (wave or geometrical) part of the argument is a mathematical generalization of the actual physical propagation of light in the three-dimensional space. It is, one might say, a *metaphor*. The "space" or "medium" of propagation, or for that matter the "light" itself in question, does not physically exist or have a proper physical meaning. It is not a physical space or a physical light. Even for the mechanical systems with only three degrees of freedom, in which case the configuration space is three-dimensional, one should not think that one could interpret the physical motion of a given mechanical (particle) system in optical, wave-like, terms, even though the relevant predictions concerning both "systems" would coincide. It is a mathematical, algorithmic coincidence, which to some degree misled Schrödinger in the case of certain simple quantum systems, where one finds an analogous (but, since the physics is now quantum, not identical) coincidence, as was often noted, including by both Bohr and Heisenberg (e.g., Bohr 1927, *PWNB* 1, pp. 76–77).

Schrödinger was, again, aware of the difficulties of attributing reality to the waves in the configuration or phase space, and thought of such waves "as something real *in a sense*, and the constant *h* universally determined their frequencies or their wave lengths" (Schrödinger to Wien, February 22, 1926, cited in *MR* 5, p. 536). He was also careful to caution against using his analogy as that between mechanics and physical or undulatory optics, as opposed to that between mechanics and geometrical optics (Schrödinger 1926c, p. 495; cited in *MR* 5, p. 558). In other words, one must find a relationship and, hopefully, a classical-like *correspondence* between these waves in the phase or configuration space and some physical vibrations—correspondence, but not identification. Schrödinger continued to believe in his program in a certain general sense, that is, insofar as he seemed to have thought, with Einstein and on the model of general relativity, that (classical-like) fields should be seen as primary and perhaps ultimate physical entities anyhow.[10] As we have seen, Schrödinger, in his letter to Sommerfeld—admittedly written at earlier stages of his thinking on wave mechanics (but speaking of essentially the same

[10] As is clear from the last section of his cat-paradox paper, Schrödinger was as suspicious of quantum field theory as of quantum mechanics (Schrödinger 1935a, *QTM*, p. 167).

conception)—even invoked the word "fantasy," which he hoped to make into a theory at the time (Schrödinger to Sommerfeld, February 20, 1926; cited in *MR* 5, p. 542). Accordingly, Schrödinger appears to have been thinking along these more cautious lines of relationships between these "waves" and actual physical vibrations corresponding to them (perhaps indirectly) rather than in terms of identifying both. Such identification would, as I said, not be rigorous even in the case of classical physics. In classical physics, however, one can relate this (metaphorically) "optical" machinery to actual (causal) mechanical processes, say, the motions of particles in space and time, properly described by the Hamiltonian equations for the system. In other words, this machinery relates to an idealized causal model, which both offers a good *descriptive* approximation and ensures correct *predictions* for many physical processes in nature.

As it happened, however, in quantum physics no descriptive physically causal model, to which the optical machinery can relate in this way, appears to be possible. One may, accordingly, have to, and in the present view must, content oneself with at most the symbolic or metaphorical "optical space," where the wave function supposedly propagates, as only related to the outcome of experiments in terms of *predictions,* which are, moreover, generally probabilistic, without describing the physical processes that lead to these outcomes. In other words, this metaphorical optical space does not in any way correspond or relate to idealized (descriptive *and* predictive) models of the kind that we can create in classical physics and that one can then relate, as idealizations, to actual quantum objects and processes. Indeed, as I have explained earlier, the type of (in the case of continuous variables) infinite-dimensional space over complex numbers used in quantum mechanics, too, can be called space only by analogy and metaphorically. This is so even in the case of discrete quantum variables (since the latter require Hilbert spaces over complex numbers, which are non-visualizable in phenomenally spatial terms), or indeed even in the case of classical physical systems with phase spaces of higher dimensions. In quantum theory, our predictions are enabled by the mathematical machinery that appears to represent nothing beyond itself. In particular, it does not represent physical processes in space and time.

One can, as Born did, speak—again, metaphorically—of a wave-like propagation of probabilities, in this case by relating this "propagation" more rigorously to what physically occurs and is observed in measuring instruments, but still with much qualification. As explained earlier, a rigorous assignment of probabilities according to Born's rule requires a very different way of looking at the situation, as Schrödinger makes clear in his cat-paradox paper. In particular, rather than dealing with continuous quantum-level processes in space and time, we still deal with a discrete "spectrum" (another fitting metaphor here) of experimental situations or phenomena, for each of which we have a given probability, provided by the wave function cum Born's rule. We also deal with a very different, in turn always discrete, sense of tracking the behavior of quantum systems, and the different concept of repeating the "same" experiment, which can apply only to the same state of measuring instruments but not to the quantum objects involved, or even in general to the same quantum objects.

Quantum phenomena (whether they are related to continuous or discrete variables) and quantum mechanics make us confront the essentially probabilistic character of quantum predictions. These predictions are, in quantum mechanics, enabled by the particular structure of the Hilbert spaces involved, as *complex* Hilbert spaces (mathematically essential to quantum mechanics of both continuous and discrete variables), cum Born's or similar rules. These rules have no rigorous mathematical or physical justification, apart from "that's the way nature is." This justification, which may be called "experimental," is not discountable, and classical physics from Galileo and Newton on is ultimately justified accordingly, with the crucial difference that there our predictions are defined and, in this sense, justified by the descriptive character of our mathematical models. We do of course try to develop our theories so as to provide such further justifications, to the degree we can, when, for example, we use the quantum vs. classical theory of light, or general relativity as against Newton's theory of gravity. In any event, the corresponding quantum mechanical Hamiltonian equations (we can, accordingly, no longer speak of equations of motion) become, in Heisenberg's terms, a new kinematics and a new form of relationships between a mathematical formalism and the experimental data to which it relates. This argument applies to whatever of the mathematically equivalent formalisms one prefers: that of Heisenberg's matrix mechanics, Schrödinger's wave equation, Dirac's Hamiltonian q-numbers, von Neumann's Hilbert-space formalism, C^*-algebra formalism, and so forth. Borrowed from classical physics, where it relates to representation of motion, the term "kinematic" is, again, misleading here, just as is the term "*wave* equation." Heisenberg, however, never intended to relate his kinematical elements to motion, as against Schrödinger's program. His "new kinematics" was truly new, in the sense of establishing the relationships between mathematics and physics, as discussed in Chapter 4.[11]

In sum, the mathematics of quantum theory (in whatever form) relates to experimentally verifiable probabilities of transition from one particular physical situation to another such situation, both established by using measuring devices or equivalent macro-objects.[12] It does not appear to—and in the present view *does not*—describe or even relate to any idealized models of quantum

[11] As emphasized throughout this study, these considerations do not mean that an alternative formalism for such predictions is not possible, which, however, does not bear on my argument here, since Schrödinger's formalism is that of the standard quantum mechanics.

[12] It may be noted that, in this view, the problem of time-reversibility of physical processes—or, rather, their description (there is no actual physical evidence thus far that any actual physical processes are time-reversible)—found in classical physics or relativity does not arise in quantum mechanics. One might see (and many do) the time-dependent Schrödinger's equation as, mathematically, time-reversible, although this reversal involves a complex conjugation of variables. This fact already poses difficulties (since in principle such a procedure implies that the resulting equation refers to a different "time-reversed" quantum system than the original one), customarily disregarded by those who argue the case. In any event, in the present view, Schrödinger's equation does not offer any physical description but only, in each case, a probabilistically predictive algorithm concerning the outcome of future experiments.

processes, which are, nevertheless, responsible for the existence of these situations and the connections between them. Accordingly, quantum mechanics is no longer a *science of the motion* of the ultimate objects it considers, quantum objects. To some degree, it becomes a way, a very specific and particular way, of *handling* information, essentially probabilistic in character, from one experimental set-up to another. It is more appropriate to speak of *correlating* information between different setups, for part of this particular nature of quantum information processing is that, although the information itself (quantum data) is no different from classical information, one cannot rigorously speak of *transmitting* information in the way it is done by means of classical physics. Once this information is generated by quantum means, it could of course be transmitted by classical means, through classical channels. It follows that correlational relations between quantum phenomena or measuring devices also enable us to create and convey information in a new way. This possibility is inherent in the nature of quantum data itself and hence does not depend on quantum mechanics or any quantum theory. The informational configuration thus established serves as the basis for quantum information theory, which—given the present analysis, unsurprisingly—has a significant role to play in the foundational research in quantum theory.

However, the nature and scope of quantum mechanics, let alone those higher level quantum theories, cannot be contained by quantum informational concerns. These theories are essentially concerned with the nature and the effect of quantum objects and processes, even though they (at least in the present interpretation) deal with and use them as irreducibly inaccessible to knowledge and hence information, and even to thought itself. This quantum mechanics and, quite possibly, any rigorous theory of quantum data are, in the present view, only predictive and informational in the sense just explained, rather than descriptive of the processes responsible for the emergence of these data. Nevertheless, from quantum mechanics to quantum field theories and beyond, quantum theory remains *physics*, a mathematical science of *nature*, although irreducibly different epistemologically from classical physics, even though it also deals, in terms of probabilistic predictions, with phenomena that are describable by means of classical physics. Unlike quantum mechanics, however, classical physics cannot predict the numerical aspects of these phenomena, including correlations between some of them. Neither theory appears to be able to describe the ultimate constitution of these phenomena or the nature of these correlations. Thus, quantum mechanics does relate to certain essential features of nature, even if irreducibly indirectly, through the effects of quantum objects manifested in quantum phenomena.

While this view retains Schrödinger's mathematics, it reverses the vision that initially grounded and guided Schrödinger's physical program, defined by the idea of "the wave radiation forming the basis of the universe." Schrödinger's equation does not describe any physical waves (actual or idealized in terms of models), as Schrödinger initially hoped it would. Instead, quantum probabilistic predictions—enabled by Schrödinger's equation and Born's or equivalent rules for deriving probabilities from quantum amplitudes—may be physically linked to a set of discrete individual phenomena, corresponding to certain wave-*like* correlational patterns. From this perspective, a certain Hamiltonian mechanical–optical analogy or translation of a mechanical kinematic into an undulatory one could be maintained in quantum mechanics or, to begin with, in classical mechanics, and can indeed be given a rigorous form. This can be done if one sees the "optics" involved only as predictive machinery in either case, classical or quantum. In classical mechanics, however, this machinery is also accompanied by descriptive (realist and causal) idealized models. By contrast, in quantum physics, such models do not appear to be—and in the present view, are not—possible, and the "optics" remains strictly predictive. The quantum mechanical Hamiltonian equations map no motion in space and time, and, at least in a nonclassical interpretation, only predict probabilities of the outcome of experiments, staged as physical situations defined by physically classical observable phenomena, such as those manifested in measuring instruments. From the mathematical standpoint, the transition from "mechanics" to "optics" is different, too, given that it works via complex Hilbert spaces and the corresponding (infinite-dimensional) representation of Lie groups and Lie algebras. One can, however, establish an *analogy* (now the term is right) with the classical case, as George W. Mackey, for example, did long ago (Mackey 1963).

As discussed in Chapter 4, initially Heisenberg and his coworkers (Born and Jordan) saw their matrix quantum mechanics as, in their words, a "truly *discontinuous* theory," while Schrödinger saw his wave mechanics as, in his words, "a step from a classical point-mechanics towards a *continuum theory*," which would represent quantum reality accordingly (Schrödinger 1926b, p. 735, *MR* 5, p. 641). As it happens, while at first sight they do look different (one as a theory of a more discontinuous type, the other of a more continuous type), both theories depend on continuous mathematics, and each is mathematically transcribable into the other. Neither, however, may mathematically represent—again, even as an idealization—any physical or phenomenal reality, continuous or discontinuous, and, as I have argued here, such a reality at the quantum level may not be representable by any means.

Ultimately, it appears, we can dispense with the idea of waves even at the phenomenal or metaphorical level, but we cannot dispense with discontinuities. These discontinuities reveal the epistemological abyss of the quantum, but, again, only do so through patterns of probability. The only continuity left is that of the mathematics of quantum theory that allows us to predict both quantum discontinuities and quantum correlations, without telling us how they come about and thus making the abyss of the quantum part of the theory. This continuity, however, may only be seen as *mathematically* wave-*like,* insofar as it deploys the mathematics analogous to—or generalizing that used to describe—physical waves, which is why we still speak of Schrödinger's equation as a wave equation. Just as is the case in Heisenberg's matrix mathematics, and it is indeed the same mathematics, rigorously we deal only with the algebra rather than the geometry of waves. As Heisenberg's matrix theory (as discussed in Chapters 3 and 4) taught us, quantum theory appears to be "a purely algebraic," or more accurately, an *essentially* algebraic, way of describing nature, as Einstein said of Heisenberg's theory—and Schrödinger's theory proved to be no different, against Schrödinger's and Einstein's hope.

5.6 The Ends of the Wave Function: From Quantum States to Entangled Knowledge

Schrödinger's article "*Die gegenwärtige Situation in der Quantenmechanik*" [The Present Situation in Quantum Mechanics], the cat-paradox paper, may be seen as Schrödinger's last word on quantum mechanics, at least in the sense that, apart from a few even more pessimistic intimations, his subsequent interventions do not appear to reach beyond the argument offered there.[13] And what a last word it was! The cat paradox itself, which made it famous and gave it its folklore name, is only one of its contributions. Among others are (1) an exceptionally lucid exposition of the classical ideas and ideals in physics; (2) an equally lucid exposition of quantum mechanics and the (Copenhagen-like) "doctrine" that accompanies it, the doctrine "born of distress"; and finally, (3) the introduction, via an analysis of the EPR argument, of the concept of "entanglement" (Schrödinger 1935a, *QTM*, pp. 153–154).

It would not be possible to consider the paper in detail here without venturing too far beyond the limits defined by the particular project of this study; accordingly, I would like to focus on those parts of the paper that are especially pertinent to this project. In particular, I shall not address Schrödinger's analysis of the EPR experiment, my subject in Chapters 8 and 9, where I shall comment on some of Schrödinger's points concerning the experiment in the context of the Bohr–Einstein exchange on the subject. Also, I shall only briefly discuss Schrödinger's concept of entanglement, which, I would argue, is often misunderstood, beginning (this has been often noted) with the translation of his *Verschränkung* as "entanglement." There is not much to be done about this now, beyond trying to explain—even if by way of *an* interpretation, one among several possible interpretations—what Schrödinger had in mind, which is what I shall sketch here. It is worth stressing, however, that Schrödinger's discussion of entanglement deploys the concept of the wave function as a catalogue of expectations throughout. Finally, I shall not properly consider the cat paradox, which has been extensively covered in literature—so extensively indeed and in so many different interpretations that it would be difficult to suggest a definitive treatment. Much as the paradox is discussed (often at the expense of other key arguments of Schrödinger's analysis), it may not ultimately be as important for understanding quantum phenomena as it is often claimed to be, and the context

[13] The paper appeared in German in three parts in 1935 in *Die Naturwissenschaften* and was accompanied by two related articles in English (Schrödinger 1935b, 1936).

in which the paradox is introduced is often missed (Schrödinger 1935a, *QTM*, p. 157). However, a few remarks concerning the paradox might be in order, because (apart from giving due consideration to the aspects of Schrödinger's thought that the paradox reflects) these remarks relate to my argument concerning the nature of quantum states. According to Schrödinger

> One can even set up quite ridiculous cases. A cat is penned up in a steel chamber, along with the following diabolical device (which must be secured against direct interference by the cat): in a Geiger counter there is a tiny bit of radioactive substance, *so* small, that *perhaps* in the course of one hour one of the atoms decays, but also, with equal probability, perhaps none; if it happens, the counter tube discharges and through a relay releases a hammer which shatters a small flask of hydrocyanic acid. If one has left this entire system to itself for an hour, one would say that the cat still lives *if* meanwhile no atom has decayed. The first atomic decay would have poisoned it. The ψ-function of the entire system would express this by having in it the living and the dead cat (pardon the expression) mixed or smeared out in equal parts. (Schrödinger 1935a, *QTM*, p. 157)

Now, Schrödinger adds a further elaboration that is rarely discussed or given proper attention, which provided a further context for his thought experiment. He says: "It is typical of these cases that an indeterminacy originally restricted to the atomic domain becomes transformed into macroscopic indeterminacy, which can then be *resolved* by direct observation. That prevents us from so naively accepting as valid a 'blurred model' for representing reality" (Schrödinger 1935a, *QTM*, p. 157). A blurred model is defined by a view of the ψ-function as "an imagined entity that images the blurring of all variables at every moment [unless a measurement intervenes] just as clearly and faithfully as the classical model [images] its sharp numerical values" (Schrödinger 1935a, *QTM*, p. 156). In other words, the problem arises if one sees the ψ-function as representing, *imaging*, the independent behavior of quantum systems. Schrödinger aimed for this type of representation initially but had given up on the idea by this point, in part in view of the difficulties such imaging represents given certain observable features of quantum phenomena, in particular those manifested in the uncertainty relations. This was not something he counted on in the initial pursuit of his wave mechanics, although his equation itself of course reflects these features.

Without pursuing the subject of the cat paradox further here, from the present perspective Schrödinger is right to see the ψ-function—as a possible imaging of quantum reality—as a problem. If, on the other hand, quantum states are seen (as in this study), as defined by the ψ-function, strictly as mathematical entities encoding and enabling expectation catalogues concerning the outcome of possible experiments—rather than as describing the behavior of quantum systems, in particular between experiments—Schrödinger's thought experiment presents no problem. No objects, micro or macro, *physically* exist in a superposition of actual physical (quantum) states. (I leave aside the difficulties of physically considering the cat as a quantum object, which are not germane to my point here.) Superpositions of quantum states (considered as strictly mathematical entities) are mathematical relations between probability amplitudes, which relate only to our probabilistic expectations concerning possible experiments. Given the information that we have, our expectations concerning either outcome of an actual experiment of this type would *generally* be 50 percent. Technically, in the Bayesian view, such expectations depend on the prior and hence may be different, even if one believes in quantum mechanics.

Schrödinger's argument for the classical ideal and his discontent with quantum mechanics are, I would contend, defined precisely by the situation considered in this study, which is also reflected in the view of quantum states just stated and to be considered in more detail below. That is, they are defined by a confrontation between a classical-like (realist and causal) epistemology, in however complex a form it is entertained, and nonclassical or near nonclassical epistemology. Schrödinger rejects even the arguments offered at earlier stages of Bohr's and Heisenberg's thinking, and the paper deals with the (proto-nonclassical) stage of Bohr's or Heisenberg's argument when one can attribute single properties to quantum objects under the constraints of the uncertainty relations. Schrödinger, at this point at least, appears to think, along the lines of Einstein's argument, that the EPR experiment shows that quantum mechanics is incomplete, or else nonlocal.

Even as Schrödinger rejects the "doctrine," he presents it, including in juxtaposition to the classical ideal in physics, with cogency, lucidity, and precision rarely found, especially at the time, among the expositions of quantum mechanics and complementarity by friends and foes alike. Most especially, this exposition is defined by his view of the ψ-function (no longer a *wave*-function, apart from "waves" of probabilities à la Born) as a catalogue of expectations, with important further qualifications as to the nature of the repetition of the experiments involved, as discussed elsewhere in this study, especially in Chapter 2 and in Chapters 8 and 9. To briefly recapitulate here, we can never track the same quantum object in the way it is possible to do in classical physics. To make our estimates, for each region in which there is a nonzero probability for a given object to be found, after a given initial measurement is performed we need to repeat the experiment from the very beginning, ab ovo, on different objects, with the same initial conditions of measuring instruments (Schrödinger 1935a, *QTM*, pp. 154, 158–159). It is, again, the essential aspect of quantum phenomena, correlative to the uncertainty relations, that if we repeat the experiment with the same initial set-up and try to locate the second object in the same region, it will in general be found in a different point of this region, which makes our predictions irreducibly probabilistic. These probabilities themselves are properly given by quantum mechanics, which allows us to compile a catalogue of expectations for finding a given object, such an electron in an atom, in a given region on the basis of previously performed experiments. I argue in this study, in the Bayesian spirit, that these expectations concern *individual* quantum processes (one can, again, be concerned with them even though they are inaccessible) and *individual* experimental events defined by them, rather than only statistical collectivities.

The concept of quantum state is defined accordingly, that is, in accord with the *spirit* of Copenhagen cum the *spirit* of Bayesian quantum information theory. The concept is also defined more generally, including epistemologically (rather than only mathematically, as a Hilbert space or equivalent object), and has been subject to significant recent exchanges and controversy around quantum information theory.[14] If by a "quantum state" we refer to a state vector (or a density operator) in a Hilbert space (or a suitable alternative mathematical object, such as a linear form, or part of the algebra of observables associated with a given quantum system, via the Gelfand–Naimark–Segal representation theorem), then, from the present perspective, we can see a quantum state as follows.

A quantum state is part of the mathematical machinery that enables a rigorous (numerical) assessment of our probabilistic expectations concerning the outcomes of certain experiments to be performed on the basis of the data obtained in certain previously performed experiments. Other parts of this machinery include other abstract mathematical elements pertaining to that Hilbert space (or, again, some alternative mathematical machinery) and numerical data, of determinate or probabilistic character, obtained from those previously performed experiments. These data also constrain what kind of predictions concerning the objects under investigation we can make, especially once some and not other experiments are performed, which circumstance becomes especially crucial in situations of the EPR type. The crux of the question is whether, to what degree, and in what sense such mathematical objects correspond to actual physical states of the quantum systems considered—for

[14] See, again, Fuchs (2001), (2003); Caves et al. (2007). The program these works pursue is based on the so-called POVMs (Positive Operator Value Measures), rather than on the type of measurements considered in this study, which correspond to ideal cases defined by wave functions or "pure states." In von Neumann's version of the formalism, the latter measurements are associated with "projection operators" or "projectors" in the corresponding Hilbert spaces. For those who are unfamiliar with Hilbert spaces, this operation is analogous to a projection of vectors in a two-dimensional plane onto a coordinate axis defined by a given vector. POVMs are analogous to density operators or matrices corresponding to the so-called "mixed states," as opposed to "pure states," which are vectors rather than matrices. Unlike a mixed state, a pure state cannot be represented by a mixture (convex combination) of other states. Pure states correspond to ideal experiments (which presuppose ideally functioning instruments and the absence of outside interferences in the course of such experiments). In such cases, one could always assign a definite probability to a given outcome, but, again, only a *probability*, rather than have it strictly determined as in classical mechanics. Actual quantum experiments require using mixed states and density operators, or POVMs, for predicting their outcomes. These qualifications do not affect my argument here or elsewhere in this study. In the present view, all quantum states are mathematical objects that, supplemented by Born's or equivalent rules, enable our expectation catalogues concerning the outcomes of quantum experiments. Indeed, it is crucial that this view is applied here *even* to the (ideal) case of pure states, since it could be more expected for mixed states and in fact appears less "distressing" (more "realist" and "objective"). This understanding of pure states corresponds to the view that idealized causal models of the type used in classical mechanics are impossible in considering quantum phenomena.

example, in the way they correspond in classical physics, in which the concept of (physical) state originates and from which it initially comes to quantum mechanics. The situation involves considerable subtlety. It has been subject to much debate and has led to major difficulties and even confusion, including (as will be seen) in EPR's article and related arguments by Einstein. In the view adopted here, the answer to this question is negative. That is, quantum states do not correspond to any physical states of quantum objects or of anything else (such as, and particularly, the physical states of measuring instruments) at all, although they serve as mathematical instruments for predicting, in general probabilistically, certain measurable quantities corresponding to the classical physical states of the measuring instruments involved.

On the other hand, closer to recent arguments in quantum information theory, quantum states can be seen as "mental." Or more accurately, they relate or even correspond to our probabilistic expectations concerning the experiments in question, which expectations are always mental. The probabilistic data defining the expectations—just as the data, already obtained, on which it is based—can of course physically exist otherwise, say as an array of numbers written down somewhere after the corresponding measurements are recorded and the corresponding calculations are performed. It is nevertheless meaningless to speak of such expectations apart from our mental processing of them and hence apart from someone holding such expectations in one's mind (possibly in one's unconscious).

In some respects, this is true in classical physics as well. In that case, however, we can—again, at least in principle and in idealized models—rigorously correlate such states of mental expectation and physical states of the system itself in question, by at least in principle being able to disregard or compensate for the role of the measuring instruments involved. In other words, classical systems can be tracked "without disturbing them appreciably," as Bohr put it by way of the starting point of his Como argument (Bohr 1927, *PWNB* 1, p. 53), and our classical interventions that perturb classical systems are still, in principle, traceable and calculable by means of classical mechanics. Neither this type of correlation nor this type of compensation appears to be possible in quantum physics, and both are strictly impossible in a nonclassical interpretation. Indeed these two impossibilities are themselves correlative. The irreducible role of measuring instruments makes mathematically defined quantum states strictly a mathematical tool of our expectations, and nothing else. They do not relate either to the motion of quantum objects themselves or to any properties of these objects at the time of their interactions with measuring instruments (e.g., collisions with the screen in the double-slit experiment). Instead, as just indicated, quantum states are used to relate to measurements and predictions concerning observations pertaining strictly to certain parts of measuring instruments impacted by their interaction with quantum objects. In other words, quantum states relate physically to no properties of quantum objects at any point, before, during, or after their interaction with measuring instruments. They relate, in terms of probabilistic predictions, only to certain classical physical states of macro-systems (such as measuring instruments) described by means of classical physics, without in any way relating to any dynamics, classical or quantum, responsible for these classical states. There is no physical evolution at any level, quantum or classical, to which quantum states relate, but they, as mathematical objects, enable us (once we enter the relevant numbers into the formalism) to make correct probabilistic predictions concerning the outcome of the interactions between quantum objects and measuring instruments, manifested in the classical physical properties of certain parts of these instruments. The mathematics of quantum theory—quantum states as mathematical objects included—enables these predictions but describes nothing physically involved in the situation, and it deprives us of any possible knowledge about the ultimate nature of the physical processes that led to the outcome of the experiments concerning which we make these predictions.

These considerations bring me, in closing, to Schrödinger's concept of entanglement and to one of its central and usually missed features, defining it virtually in terms of our predictive knowledge or information, our expectation catalogues—defined by (entangled) mathematical quantum states or state vectors—concerning possible future experiments, rather than in terms of the actual *physical* state of a given quantum system. According to Schrödinger's definition, "If two separate bodies, each by itself known maximally [according to the laws of quantum mechanics], enter a situation in which they influence each other, and separate again, then there occurs regularly that which I [call] *entanglement* of *our knowledge* concerning the two bodies" (Schrödinger 1935a, *QTM*, p. 161; translation modified: emphasis on "our knowledge" added). As my added emphasis indicates, Schrödinger at the very least directs our attention to an *entanglement* of *our knowledge* concerning quantum objects involved. He also, indeed in the first place, speaks of an "entanglement of *predictions*" (Schrödinger 1935a, *QTM*, p. 161; emphasis added). Thus, he at least directs our attention to the possibility that quantum states are *states of knowledge* or of expectations concerning the outcome of possible experiments—"expectation catalogues," or catalogues of possible bets, whose basis is knowledge obtained in previously performed experiments. Entangled quantum states, too, are states of knowledge, expectations, or bets in this sense. In the present view, they could only concern the effects of the interactions between quantum objects and measuring instruments, or some other part of the macroscopic world, rather than quantum objects themselves, which remain inaccessible to any knowledge or, again, even conception. As I argue in this study, our quantum mechanical predictions are defined by this knowledge, which concerns the physical states of our measuring instruments. In the present view, it does not and, it appears, cannot concern quantum objects themselves or their behavior, even though no quantum experiments and, accordingly, no quantum predictions would be possible apart from the unknowable and inconceivable quantum objects and their behavior, including their (quantum) interaction with measuring instruments.

This argument takes us, *with Schrödinger's help,* far from wave mechanics as Schrödinger initially conceived of it. Or one might say that with expectations, probabilities, or bets and their correlation patterns, we may have reached the end of Schrödinger's wave in the double sense of the word "end," the termination and the goal, however initially unexpected by Schrödinger, however far from his initial bet. Or perhaps we have reached yet another intermediate station in our long, seemingly unending quantum theoretical journey, just for the moment, before we find ourselves at sea again. In the meantime, both the mathematics and the physics of quantum theory, and in more complex ways, its philosophy, continue to survive and sometimes thrive. They might be around for quite a while, and debates concerning them are likely to continue for as long as they are around.

Chapter 6
Bohr's Como Argument: Complementarity and the Problem of Causality

Abstract This and the following chapter consider Bohr's new *ways* (plural) of thinking about quantum phenomena and quantum theory in terms of complementarity that emerged in the wake of Heisenberg's discovery of quantum mechanics and then of the uncertainty relations. Bohr's thought on these subjects underwent several changes even at the earlier stages to be discussed in these two chapters, and then was further refined in the wake of EPR's argument. The first change is the shift from Bohr's pre-complementarity view of quantum theory in the wake of Heisenberg's 1925 discovery of quantum mechanics to his view following Schrödinger's wave mechanics, Dirac's and Jordan's transformations theory, and Heisenberg's uncertainty relations—developments that were instrumental to the invention of complementarity, introduced in the Como lecture in 1927. This change will be discussed in this chapter. The second change, discussed in Chapter 7, was marked by Bohr's rethinking of the question of causality in quantum theory and occurred under the impact of his exchanges with Einstein in 1927. Section 6.1 gives an introductory discussion of the developments of Bohr's thought and of the concept of complementarity. Sections 6.2 and 6.3 offer an analysis of the Como lecture. Section 6.3 also critically examines some of the problematic implications of the Como argument, in particular those concerning quantum causality. Section 6.4 considers, from the same critical perspective, the arguments concerning quantum causality offered—in part following Bohr's Como argument—by Dirac, Heisenberg, and von Neumann.

6.1 Complementarity: Between Concepts and Experiments

I would like to begin by qualifying that by the "Como version" of complementarity, I refer to the version found in Bohr's article, "The Quantum Postulate and the Recent Development of Atomic Theory," based on the lecture given in Como but published in this version in 1928 (Bohr 1927, *PWNB* 1, pp. 52–91). As is well known, the article deviates from the lecture; it has a complicated history of writing and publication and appears to have never been quite

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finalized by Bohr to his satisfaction.[1] Almost nothing ever appears to have been—at least prior to his late articles, written in the 1950s (which, however, appear to present previously developed arguments)—but in this case the lack of finalized argument is especially apparent. In part, this incompleteness reflects the general essay-like quality often found in Bohr's writings, which require one to consult Bohr's other articles to "complete" Bohr's argument; and it is symptomatic that Bohr never wrote a book offering a fully sustained presentation of complementarity. In this case, however, as against most subsequent works, one also encounters substantive problems that require significant corrections, or ideas that are unworkable and were to be abandoned altogether, rather than only those that could be more fully developed or further refined. Bohr's subsequent writings remedy most of these problems, by changing, in some respects significantly, his concepts and arguments. There is nothing wrong with or surprising about these changes, especially given the complexity of the subjects Bohr had to confront. Bohr, however, rarely, if ever, indicates these changes; he tends to present and perhaps see his writings on complementarity as a certain unity composed of consistent parts (represented in his different articles). However, this is not always the case, although it may be seen as nearly the case if one subtracts part of the Como argument from the trajectory of thought mapped by his essays.

As I have stressed from the outset of this study, Bohr has several versions of complementarity, forming a spectrum of interpretations of quantum phenomena themselves and quantum mechanics. This spectrum may be divided into two or possibly three primary subsets and (suppressing less essential differences within each) two or three versions of complementarity. Although not altogether strict or unequivocally determined, this division is, I would argue, not arbitrary, either historically or conceptually.

Historically, it is shaped by Einstein's critique of quantum mechanics, roughly from the time of Bohr's introduction of complementarity and culminating in his arguments of the EPR type (Einstein was developing them into the late 1940s), and by Bohr's exchanges with Einstein. It is, as I said, intriguing, although difficult, to speculate how Bohr's thinking concerning complementarity would have developed apart from Einstein's arguments. The Como argument was not subject to this pressure, with, I would argue, mixed results. It is a

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[1] It would, accordingly, be difficult to argue that the published version is the definitive statement of Bohr's views at the time. However, it appears be at least as definitive as any available to be analytically treated as a text; and it was so treated by Bohr himself, for example, in his "Introductory Survey" to *Atomic Theory and the Description of Nature* (*PWNB* 1, pp. 9–15). This collection gives 1927 as the article's date, which is the main reason why I use this date here as well, although this version was not published before 1928. I shall put aside the difference between German and English versions, which are interesting, but ultimately not germane here. For the history and preliminary drafts of the article, beginning with the draft of the lecture (it is not clear to what degree Bohr's actual presentation followed it), and useful commentary by J. Kalckar, see Volume 6 of Bohr's collected works (Bohr 1972–1996, vol. 6).

momentous contribution on many counts, which also has a special appeal in being defined by the introduction of new ideas rather than by clarifications and adjustments in response to criticism, as is characteristic of Bohr's later works. On the other hand, it is, arguably, the most problematic of Bohr's arguments concerning complementarity. This is, again, understandable given that it represents a first confrontation with difficult problems, of the kind that takes—and had taken Bohr—years to think through. In such situations, a critical pressure of the kind Einstein was able to exert on Bohr helps and often leads to new ideas, as it happened in Bohr's case.

Conceptually, the transition from the earlier to the later version or versions is especially marked by a shift from a more philosophical to a more empirical viewpoint, or more accurately, a shift in balance between philosophical and empirical aspects of complementarity. Complementarity in the narrow sense, introduced—although, I shall argue, not rigorously developed—in the Como lecture, is retained in and is crucial to Bohr's subsequent works, where it also developed more rigorously. The complementary features or phenomena themselves considered by Bohr also change in accordance with this shift. Following Bohr's exchange with EPR, although not quite in Bohr's reply itself to EPR, these phenomena, and all quantum phenomena, are also given a nonclassical interpretation, based in Bohr's concepts of phenomena and atomicity. It should be stressed, however, that even when defined in more philosophical terms (as in the Como lecture), complementarity in the narrow sense reflects the specific character of certain observed phenomena as *quantum* phenomena, in particular as defined by the role of Planck's constant h and the uncertainty relations.

The earlier versions of complementarity, those prior to the Warsaw lecture of 1938, "The Causality Problem in Atomic Physics," including the one Bohr uses in his reply to EPR, allow one to associate certain physical properties with quantum objects, at least at the time of measurement. By virtue of the uncertainty relations, however, at most only one half of the properties that define the behavior of classical physical systems could be available for this association at any given point—for example, either a position or a momentum, but never both simultaneously. Bohr appears to have always been ambivalent as concerns this type of association, even when it is seen as possible only at the time of measurement. But he also viewed the idea and language of "creation" of physical properties of quantum objects by measurements with as much suspicion as those of "disturbance" of (preexisting) properties by measurements, ultimately seeing both notions as inapplicable (Bohr 1938, *PWNB* 4, p. 104; Bohr 1949, *PWNB* 2, p. 64; Bohr 1954, *PWNB* 2, p. 73). I shall explain the reasons for this view in more detail later, in the context of his thinking concerning the EPR experiment, which deepened these questions and made them more urgent. Bohr's concept of phenomenon equally dispenses with both notions. In the post-EPR version of complementarity, based on this concept, an association of physical properties with quantum objects is never possible even as concerns single such properties (rather than only simultaneous conjugate properties, governed by the uncertainty relations) at any point—before, during, or after a measurement. All physical properties considered are now only those of certain parts of the measuring instruments involved and are, as physical properties, described by classical physics. The attribution of such properties is still constrained by the uncertainty relations. The application of the uncertainty relations, however, is now transferred into this classical domain of physical description. In other words, first, one can never properly define both conjugate classical properties and the corresponding measurable quantities, such as those position and momentum, for the relevant part of the measuring instrument in the same experimental arrangement. Second, and more radically, one cannot associate any physical properties, single or joint, with this object itself at any point, before, during, or after measurement.

Bohr's revisions of his interpretation are not always sufficiently taken into account or adequately considered by commentators, friends and foes alike, although the subject has received more attention in more recent studies of Bohr.[2] The significance of these revisions motivates the approach adopted by this study, which devotes separate chapters to at least three such versions—the Como version of 1927, the intermediate version of (approximately) 1929, and, finally, the post-EPR version, crystallized by late 1930s. The first one is discussed in this chapter and the final one in Chapter 10. Chapters 8 and 9 offer an analysis of the Bohr–EPR exchange around the EPR type of experiments, as the main impetus for and a transition to Bohr's ultimate version of complementarity. The version used in his reply is by and large the intermediate version of 1929 to be discussed in Chapter 7; it is marked by the introduction of the complementarity "between the space–time description and the laws of the conservation of energy and momentum" (Bohr 1929a, *PWNB* 1, p. 94).

It would, however, be equally misleading to disregard the continuities in Bohr's thought or the significance of the Como argument, beginning with the introduction of a conceptual *framework*, the first of its kind, for understanding and interpreting quantum mechanics, even if not quite the framework of such an interpretation itself. The most significant innovation was the introduction of the concept of complementarity in the narrow sense, even if without giving it a fully rigorous definition. Several other contributions in the Como lecture are also important as well, both in their own right and for Bohr's subsequent work. Bohr's title concept of the quantum postulate is crucial to all of his thinking. So is his argument for the irreducible role of measuring instruments in the constitution of quantum phenomena, which intimates Bohr's concept of phenomena that grounds his nonclassical version of complementarity. This view of measuring instruments also leads to yet another important argument offered in the Como lecture. Bohr argues that both concepts, particle and wave, are

[2] These changes in Bohr's views have been used to criticize Bohr's argument or "the spirit of Copenhagen," for example, by Mara Beller, as part of her advocacy of David Bohm's hidden-variables approach (Beller 1999). As I have argued previously, Beller's argument appears to me unconvincing; it appears to miss some among the most essential aspects of Bohr's thinking at all stages (Plotnitsky 2002a, pp. 254–255, n. 33).

abstractions, derived from the effects of the interactions between quantum entities and measuring instruments upon the latter. As a result, a more rigorous understanding of the wave and the particle aspects of quantum physics becomes possible. Bohr's elementary derivation of the uncertainty relations (directly from de Broglie's formulas rather than, as in Heisenberg, from the formalism of quantum mechanics or via the γ-ray-microscope thought experiments) is noteworthy as well. It is especially important insofar as, along with the argument in Heisenberg's paper itself, Bohr's derivation further helped to establish the uncertainty relations as an experimental fact, a quantitative law of nature, reflecting a feature of quantum phenomena that cannot be circumvented. A view that it is possible to do so had been adopted by Einstein in most of his criticism of quantum mechanics and became one of the targets of Bohr's counterarguments. Instead, the fact that one can derive the uncertainty relations from quantum mechanics shows that it properly accounts for this particular aspect of quantum phenomena as well. While Heisenberg's uncertainty-relations paper essentially makes this point as well, Bohr's derivation and analysis of the uncertainty relations in the Como lecture sharpens it. Finally, Bohr was also able to establish proper interconnections between the features of quantum phenomena just mentioned, which enabled him to offer a coherent overall framework for understanding quantum mechanics already in the Como lecture, and eventually to develop a full-fledged interpretation of it. These contributions make the article a major statement on quantum theory in spite of problematic aspects of Bohr's argumentation there.

Quite a few things were to be left behind as well. Apart from the more radical epistemology of Bohr's post-EPR version of complementarity (in the broad sense) and several refinements or corrections of the argument offered in the Como article, the key differences between the Como and the post-EPR versions, or already the intermediate version of 1929, are the following. Arguably, the most crucial one concerns the question of causality, linked to the conceptual significance of Schrödinger's wave mechanics and the Dirac–Jordan transformation theory in the Como argument vis-à-vis their mathematical significance in Bohr's subsequent arguments. The Como argument attributes causality to the independent ("undisturbed") quantum processes, viewed as described by Schrödinger's equation, while the lack of causality and determinism is due to the disturbance introduced by measurements. This concept is abandoned in Bohr's subsequent arguments. By the same token, wave mechanics itself loses its significance nearly altogether in Bohr's post-Como thinking, although the reinterpretation of quantum waves in terms of probabilities and correlations retains its crucial role. This thinking, I argue, returns Bohr to Heisenberg's initial views, which shunned the wave picture, welcomed by Bohr in the wake of Schrödinger's introduction of his wave equation.

The significance of Schrödinger's mechanics for the Como lecture does not amount to the significance of the wave–particle complementarity. The status of this complementarity is complex even in the Como version, if indeed one can even speak of this complementarity there or elsewhere in Bohr. Bohr, I argue, not only does not invoke but also does not appear to think in terms of the wave–particle complementarity even in the Como lecture, let alone in his subsequent work, although the Como lecture may be suggestive of the idea and appears to have been responsible for its emergence and proliferation. Nor ultimately does Heisenberg, who, as discussed in Chapter 4, speaks instead of both "pictures" (particle and wave) as equally possible (Heisenberg 1930, pp. 13, 62–65). Bohr does relate the wave and the particle features—or, again, more accurately, the wave-*like* and the particle-*like* features—of quantum phenomena to the concept of complementarity in the Como lecture (e.g., Bohr 1927, *PWNB*1, pp. 55–57) and elsewhere. These relations, however, do not amount to the wave–particle *complementarity*.

Consider, for example, the following passage in "Discussion with Einstein," which may appear to contradict my claim here: "Under these circumstances [of the necessity to use complementary phenomena in quantum theory] an essential element of ambiguity is involved in ascribing conventional physical attributes to atomic objects, as is at once evident in the dilemma regarding the corpuscular and wave properties of electrons and photons, where we have to do with contrasting pictures, each referring to an essential aspect of empirical evidence" (Bohr 1949, *PWNB* 2, p. 40). Thus, he speaks of the *dilemma*, arising, moreover, by virtue of "an essential element of ambiguity . . . in ascribing conventional physical attributes to atomic objects," but *not* of the *complementarity* of the wave and the particle descriptions. Nor is the term complementarity used here. The same may be said concerning Bohr's similar earlier statement to the effect that "the well known dilemma between the corpuscular and undulatory character of light and matter" is "avoidable only by means of the viewpoint of complementarity" (Bohr 1937, *PWNB* 4, pp. 87–88). It is, again, indicative that Bohr does not invoke the wave–particle complementarity here either. Of course, the resolution of this dilemma in Bohr's framework involves certain complementary phenomena arising under different, mutually exclusive, circumstances, such as those of the double-slit experiment. These phenomena, however, are also individual, discrete (and in this, but again, only in this sense, particle-*like*), or, when collective, composed of discrete individual phenomena. Indeed, Bohr immediately, in the same paragraph, moves to the complementarities involving the space–time coordination and the application of the momentum and energy conservation laws, which are correlative to the complementarities of the measurements of position and momentum, or time and energy, in the Compton effects (Bohr 1949, *PWNB* 2, p. 40). Now, he does use the term "complementary phenomena," at this point clearly in his post-EPR sense of something manifested in measuring instruments, as opposed to atomic objects in the case of the dilemma of the corpuscular and wave properties of these objects. The textual proximity of the two statements may suggest the wave–particle complementarity. However, reading what Bohr says on his terms and properly considering the concepts he actually uses compels one to conclude otherwise. I have already offered in this study several major reasons why Bohr did not, and why one might not want to, speak in terms of the wave–particle complementarity, and a few more reasons will be given in this chapter as well. To put it strongly, there has never been, at least for Bohr, wave–particle complementarity, which may, again, be seen as one of the greatest fictions of the history of quantum theory.

As I said, the Como version may be seen as more philosophical, while the subsequent versions are more empirical, beginning with the key complementary features or phenomena with which Bohr is concerned—although, as I argue, all versions of complementarity are positioned between physics and philosophy and among physics, mathematics, and philosophy. In particular, the Como version is defined by the complementarity of the space–time coordination and the claim of causality, which, Bohr adds, "[symbolize] the idealization of observation and definition respectively" (Bohr 1927, *PWNB* 1, p. 55). This qualification gives this complementarity a more philosophical conceptual architecture by coupling it to the idealizations in question, that of observation and that of definition, although, as I shall explain, the definition in question is also and even primarily mathematical. This complementarity, however, also has a crucial physical dimension by virtue of the juxtaposition with classical physics that it establishes by splitting causality and the space–time coordination and, thus, by redefining the relationships between experiment (observation) and theory (definition). These relationships appear to reflect Bohr's rethinking, in terms of complementarity, of the relationships between "the principle of observable quantities" of Heisenberg's paper introducing quantum mechanics and "the principle of the theoretical definition of observation," due to Einstein, shaping Heisenberg's paper on the uncertainty relations. Once again, there is no invocation of the wave–particle complementarity in the lecture. Instead Bohr relates the wave-*like* and the particle-*like* features of quantum phenomena to the complementary of the space–time coordination and the claim of causality (e.g. Bohr 1927, *PWNB* 1, pp. 55–57).

This complementarity disappears from Bohr's subsequent arguments, following his initial exchanges with Einstein on quantum mechanics in 1927. Bohr's approach becomes centered, first, on the complementarity of the application of spatial–temporal description and the conservation laws, and, correlatively, on the mutual exclusivity of the corresponding experimental arrangements (e.g., those suited for position or, conversely, momentum measurements), linked to Heisenberg's uncertainty relations. The experimental considerations involved in this type of thinking are significant in the Como version as well but are subordinated there to the complementarity of the space–time coordination and the claim of causality. This reorientation defines the key works following the Como lecture, especially Bohr's 1929 article "The Quantum of Action and the Description of Nature" (Bohr 1929a), to be discussed in the next chapter. The article, written almost immediately in the wake of Bohr's first exchanges with Einstein on quantum mechanics, enacts the transition in question, and as offering an intermediate version of complementarity, still in place in Bohr's reply to EPR. His ultimate version is, again, developed later on and is first apparent in

"Complementarity and Causality" (Bohr 1937) or in the Warsaw lecture, "The Causality Problem in Atomic Physics" (Bohr 1938).

6.2 The Quantum Postulate: Discontinuity and Irrationality

Bohr appears to have formulated the concept of the quantum postulate in the Como lecture.[3] Underscoring the centrality of this concept for Bohr's argument there is the fact that it serves as Bohr's title concept, indeed the single physical concept of the title, "The Quantum Postulate and the Recent Development of Atomic Theory." The significance of the concept has never diminished in Bohr's work (although the phrase itself became infrequent in his writings), even as other concepts, beginning with complementarity, moved to center stage and changed the conceptual architecture of the quantum postulate itself. Bohr's work on complementarity may also be seen as a lifelong effort to give the proper meaning to "the quantum postulate" and, thus, to "Planck's discovery of the *elementary quantum of action*," the phrase Bohr uses throughout his works beginning with his 1929 article, "The Quantum of Action and the Description of Nature" (Bohr 1929a, *PWNB* 1, p. 92; emphasis added). Although Bohr often speaks of the quantum postulate as *Planck's* quantum postulate, his interpretation or, again, conception of it makes the postulate his own. Associating the postulate with Planck is of course well justified. From the point of Planck's discovery of his black body radiation law, quantum discontinuity, reflected in the quantum postulate, becomes a defining part of our understanding of the ultimate constitution of nature. In "Atomic Theory and Mechanics" (Bohr 1925b, *PWNB* 1, pp. 25–51), completed in the wake of Heisenberg's introduction of quantum mechanics, there is no "quantum postulate" yet, and the only relevant feature involved is discontinuity. Bohr speaks of Planck's discovery as "demand[ing] an element of *discontinuity* of atomic processes quite foreign to the classical theory" (Bohr 1925b, *PWNB* 1, p. 28). By the time of the Como lecture, however, the quantum postulate assumes a central position in Bohr's thinking. It is seen by Bohr as expressing the very essence of quantum theory, which, Bohr says, "is characterized by the acknowledgment of a fundamental limitation in the classical physical ideas when applied to atomic phenomena." "The situation thus created," he adds, "is of a peculiar nature, since our interpretation of the experimental material rests essentially upon the classical concepts" (Bohr 1927, *PWNB* 1, p. 53). These statements reveal the complexity of Bohr's view on the role of the classical concepts (not the same as "classical physical ideas," which

[3] Bohr uses the phrase "the quantum postulates" (in plural) on earlier occasions, including in "Atomic Theory and Mechanics" (Bohr 1925b), in referring to basic postulates of his atomic theory of 1913. I would argue, however, that, as used in the Como lecture, the phrase designates a new concept.

refers to the ways these concepts are to be used) as essential and yet limited and, as concerns quantum objects and their behavior, even inapplicable.

The quantum postulate itself reflects or symbolizes this situation. According to Bohr: "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" (Bohr 1927, *PWNB* 1, p. 53). Bohr's hesitation ("or rather") is worth noting, as is his appeal to the "essential" nature of these features or to the role of the classical concept, clearly used here in the same direct sense of being fundamental and irreducible as in his appeal to "a *fundamental* limitation in the classical physical ideas." The essential discontinuity and the essential individuality are, again, to be eventually reinterpreted in terms of Bohr's concepts of phenomenon and atomicity and connected by making this discontinuity or discreteness refer to that between individual phenomena in Bohr's sense. The kernel of the idea is found here as well, however. Bohr's appeal to the symbolic nature of Planck's quantum of action, *h*, is in part due to his view of all quantum mechanical formalism as symbolic—a view he developed following Heisenberg's introduction of matrix mechanics and applied to Schrödinger's wave theory as well. This formalism is symbolic by virtue of the fact that, while formally analogous to classical mechanics, it does not describe the physical behavior of quantum objects or any physical processes in space and time, but only serves to predict the outcomes of experiments. Correlatively, Planck's quantum of action is symbolic insofar as it should not be seen as merely implying the strictly discrete, particle-like, nature of quantum objects, as opposed to their wave-like character, since both of these aspects are seen as pertaining only to certain effects registered in measuring instruments. Bohr elaborates as follows, via a link to relativity (ever on his mind, as positioned between classical and quantum physics):

> This 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 emphasized 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 [between a quantum object and?] the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena [objects?] 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 which point the concept of observation involving the quantum postulate with its inherent "irrationality" is brought in. (Bohr 1927, *PWNB* 1, pp. 53–54)

The last point clearly registers the irreducible role of theoretical, including mathematical, concepts in shaping our observation when they enter a physical theory; and, unlike the role of measuring instruments, which is irreducible only in quantum physics, our theoretical concepts are equally irreducible in both quantum and classical physics or in relativity. Einstein's "principle of the theoretical definition of observation," discussed in Chapter 3, is thus given a properly stratified rendition here. The passage as a whole contains several ingredients of Bohr's subsequent conceptions and of his nonclassical epistemology of quantum phenomena, although the key distinction between the (unobservable and inconceivable) quantum objects and the (observable) phenomena in Bohr's sense is not yet in place. As I indicated in the text by my question marks, the passage even appears to indicate some confusion or at least imprecision in expression in this respect.

An emphasis on—and the very notion of—"disturbance" contributes to the problems of Bohr's argument in the Como lecture. At one level this emphasis is essential, insofar as in the case of quantum phenomena the interference of measurement cannot be avoided and, hence, no independent behavior of quantum objects can be observed. On the other hand, the concept of disturbance is problematic insofar as it may imply the possibility of a (specific) conception of the undisturbed behavior of quantum objects. This concept, too, is to be abandoned by Bohr in his later work, along with that of "creation" of the attributes of atomic objects by measurements (Bohr 1938, *PWNB* 4, p. 104; Bohr 1949, *PWNB* 2, p. 64; Bohr 1954, *PWNB* 2, p. 73).

I am less worried by Bohr's appeals to the "inherent 'irrationality' " of the quantum postulate or to the "choice" on the part of atoms or nature later in the article or elsewhere in his early work. The notion and the very language of irrationality have often been used against Bohr by his critics and have troubled even many of his advocates, in my view often as a result of misunderstanding Bohr's thinking.[4] Apart from dissociating quantum mechanics and his own views from any "mysticism foreign to the spirit of science," beginning with his initial response to Heisenberg's introduction of quantum mechanics, Bohr stresses the *rational* character of quantum mechanics throughout his work (Bohr 1925b, *PWNB* 1, p. 48; Bohr 1949, *PWNB* 2, p. 63). The "irrationality" invoked here and elsewhere in his earlier writings is clearly *not* any "irrationality" of *quantum mechanics* itself, which Bohr, again, sees as a rational theory, a *rational* theory of something that may, in a certain sense, be *irrational*—that is, inaccessible to rational thinking or thinking in general.

Bohr's point is misunderstood in part by virtue of overlooking (beginning with Bohr's quotation marks) the difference between the rationality of a theory

[4] These concerns and some of this misunderstanding are exhibited, for example, by J. Kalckar in introducing Bohr's work on complementarity in Volumes 6 and 7 of Bohr's collected works, devoted to complementarity (Bohr 1972–1996, vols. 6 and 7).

and the irrationality of what this theory (rationally) deals with. Bohr's invocation of "irrationality" appears to be based on an analogy with irrational numbers, reinforced perhaps by the role of complex numbers and specifically the square root of –1, *i* (algebraically, an irrational magnitude). The ancient Greeks, who discovered the (real) irrationals, could not find an arithmetical, as opposed to geometrical, form of representing them. The Greek terms were "alogon" and "areton," which may be rendered as "incommensurable" and "incomprehensible," both equally fitting in the present context, that is, as referring to quantum objects and processes. The problem was only resolved in the nineteenth century, after more than 2000 years of effort. It remains to be seen whether the quantum mechanical "irrationality" will ever be resolved in this way, that is, by discovering the way to mathematically or otherwise represent quantum objects and processes, assuming that one sees it as a problem. In Bohr's view, the quantum postulate implies the existence of a certain boundary ("cut"), up to a point (but only up to a point) arbitrarily placed, between the "rational"—comprehensible and measurable, or observable and conceivable—classical world and the "irrationality" of the quantum postulate. The existence of this "cut" may itself be seen as correlative to the irreducibility of complex numbers in the formalism, the algebra, of quantum mechanics. Quantum mechanics and complementarity are rational forms of understanding this situation, and Bohr's concepts of phenomena or atomicity are the ultimate embodiment of Bohr's understanding of quantum phenomena and, hence, of the quantum postulate.

The point concerning this rationality is brought home in Bohr's 1929 "Introductory Survey" to his *Atomic Theory and the Description of Nature* (*PWNB* 1). As he says, "A conscious resignation in this respect [the impossibility of carrying forward a coherent causal description of atomic phenomena] is already implied in the form, *irrational from the point of view of the classical theories*, of those postulates,... upon which the author [Bohr] based his application of the quantum theory to the problem of atomic structure" (Bohr 1929b, *PWNB* 1, p. 7; emphasis added). Bohr offers another helpful elaboration there. It follows his comments on complementarity as separating certain features that "are united in the classical mode of description." In so doing, complementarity helps us to avoid the contradiction and "irrationality" (apparently unavoidable if one retains a classical physical and epistemological viewpoint). Then he adds

> Moreover, the purpose of such a technical term is to avoid, so far as possible, a repetition of the general argument as well as constantly to remind us of the difficulties which, as already mentioned, arise from the fact that all our ordinary verbal expressions bear the stamp of our customary forms of perception, from the point of view of which the existence of the quantum of action is an irrationality. Indeed, in consequence of this state of affairs, even words like "to be" and "to know" lose their unambiguous meaning. In this connection, an interesting example of ambiguity in our use of language is provided by the phrase used to express the failure of the causal mode of description, namely, that one speaks of a free choice on the part of nature. Indeed, properly speaking, such a phrase requires the idea of an external chooser, the existence of which, however, is denied already by the use of the word nature. We here come upon a fundamental feature in the

> general problem of knowledge, and we must realize that, by the very nature of the matter, we shall always have last recourse to a word picture, in which the words themselves are not further analyzed. (Bohr 1929b, *PWNB* 1, pp. 19–20)[5]

The passage reminds one of, and further explains, Bohr's exchange with Hoffding, cited in the Introduction: "Where can the photon be said to be? To be, to be, what does it mean to be?" Bohr's example is not accidental. While he and others, in particular Dirac (who even spoke of an electron as having a "free will"), used expressions like "a free choice on the part of nature," Bohr's use of it must be considered in accordance with this comment. It is true that this was written after the Como lecture and that this particular comment refers primarily to Bohr's article, "The Quantum of Action and the Description of Nature" (Bohr 1929a). Later on, Bohr abandoned this mode of expression and other aspects of his Como idiom (such as, again, that of "disturbance"), made unnecessary by his refinement of complementarity, thus implicitly acknowledging certain problems of the Como argument.

Earlier in the paragraph, just cited, from "Introductory Survey" to *Atomic Theory and the Description of Nature*, Bohr, now referring to the Como lecture as well, invokes the crucial role of the "mathematical symbolism" in helping to make quantum mechanics a rational theory. With Heisenberg's new algebra (as opposed to the geometry of classical mechanics or of the old quantum theory), the irrationality in question could now be handled rationally, just as certain developments in mathematics led to rational means of handling irrational numbers. By contrast, geometry served as that rational means of handling the irrational in ancient Greece, since the arithmetic could not then make sense of such entities. There is a double irony in this juxtaposition. On the one hand, all measurements are still carried out in rational numbers. On the other hand, the mathematical formalism of quantum theory involves a highly developed form of algebra, which, however, also includes calculus, unknown to the ancient Greeks (at least not before Archimedes) but the main tool of classical mechanics, which extends the Euclidean thinking to the Newtonian one. Bohr's persistent appeal to the symbolic nature of quantum mechanics, alongside its rational nature, supports the argument just offered. Algebra is our ultimate means of handling the mathematical irrational. Quantum mechanics, too, is essentially an algebraic and symbolic theory of the irrational, insofar as it does not offer a (geometrical) description of quantum objects and processes.

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[5] Later on, Bohr commented on the phrase "choice of nature" as follows in accordance with his post-EPR views: "Needless to say, such a phrase implies no allusion to a personification of nature, but simply points to the impossibility of ascertaining on accustomed lines directives for the course of a closed indivisible phenomenon" (Bohr 1954, *PWNB* 2, p. 73).

6.3 Complementarity and Causality

Complementarity in the narrow sense is introduced in the Como lecture as the immediate consequence of the quantum postulate and the epistemology that it implies, especially the fact of the irreducible "disturbance" of the observable phenomena by the act of observation. This concept, again, eventually evolved into that of the irreducible role of measuring instruments in the constitution of quantum phenomena. Bohr's preferred term became "interference," which is more rigorous and precise than "disturbance," especially given his later views. It no longer suggests an assumption of independent, especially causal, quantum processes—describable by the quantum mechanical formalism—that would then be disturbed by measurement, a defining and problematic assumption of the Como lecture. Bohr argues there as follows:

> This situation has far-reaching consequences. On one hand, the definition of the state of a physical system, as ordinarily understood [i.e., in classical physics], claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space–time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively. 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. (Bohr 1927, *PWNB* 1, pp. 54–55)

This task will, again, take Bohr a while to complete, ultimately along different lines of thought than those stated here and developed in the article. Major breakthroughs notwithstanding, it is, I argue, far from being accomplished in the Como lecture, as Bohr, who was never happy with it, realized with the help of Einstein's criticism of quantum mechanics (it did not concern the Como lecture as such) offered shortly thereafter. It would in my view be difficult to maintain that the article offers a fully consistent argument. On the other hand, one might argue, as I do here, that it contains a number of problems. In particular, the complementarity of the space–time coordination and the claim of causality was to disappear quickly from Bohr's writings (I remove Bohr's hyphen in coordination). It was not complementarity, however, but causality that was a problem. Although triggered by Schrödinger's wave theory, the idea that the independent quantum behavior is causal, while the lack of causality and determinism manifested in quantum phenomena is due to the disturbance introduced by observation, appears to originate, via the transformation theory, with Dirac, who introduced it sometime in 1926 while at Bohr's Institute in Copenhagen (Dirac 1927). As I shall discuss below, this view of quantum behavior has been and continues to be pervasive in foundational arguments concerning quantum theory. By contrast, Bohr quickly came to realize the difficulties of sustaining this contention, which, as will be seen presently, is not adequately, if at all, supported by Bohr's argument in the lecture. Already his next publication on the subject abandons the idea and the complementarity of the space–time coordination and the claim of causality, thus giving the Como argument barely a yearlong life span. By the time of his reply to EPR, he speaks of "a *final renunciation* of the classical ideal of causality" (Bohr 1935b, p. 697; emphasis added) and eventually of complementarity as "a rational *generalization* of the . . . ideal of causality" (Bohr 1949, *PWNB* 2, p. 41; emphasis added). The latter is obviously quite different from maintaining causality, even if only as complementary to something else. It is true that the Como lecture also speaks of "a consistent theory of atomic phenomena, which may be considered as a rational generalization of the causal space–time description of classical physics" (Bohr 1927, *PWNB* 1, p. 87). Besides a significant nuance in the formulation itself, however, it still refers to the complementarity of the space–time coordination and the claim of causality (vis-à-vis their unity in classical physics), which complementarity is manifestly absent in Bohr's later writings. I shall now discuss Bohr's Como argument in more detail in order to show why this complementarity is problematic.

I would like to offer, first, a few remarks on Heisenberg's uncertainty relations, which will help clarify the nature of the problem. While Bohr overtly grounds his argument in the quantum postulate, it is clear that he also follows and develops Heisenberg's argument in his uncertainty-relations paper, and indeed *revises* this argument by bringing causality into the picture. This revision was in turn to influence Heisenberg's subsequent thinking as manifested, for example, in the Chicago lectures. According to Heisenberg, "*All concepts which can be used in classical theory for the description of a mechanical system can also be defined exactly for atomic processes in analogy to the classical concepts*. The experiments which provide such a definition themselves suffer from an indeterminacy introduced purely by the observational procedures we use when we ask of them the simultaneous determination of two canonically conjugate quantities. The magnitude of this indeterminacy is given by [the uncertainty] relation [$\Delta q \Delta p \cong h$]" (Heisenberg 1927, *QTM*, p. 68; Heisenberg's emphasis). One might note in passing the argument for the use of classical concepts, which will become crucial to Bohr, for a while, from the Como argument to his reply to EPR, exactly as it is used by Heisenberg here, and ultimately only as applicable to measuring instruments and no longer to quantum objects themselves, as here. The main point for the moment is that the disturbance introduced by measurement leads to the impossibility, reflected in the uncertainty relations, of establishing both variables in question simultaneously. Thus, it also makes it impossible to maintain the causal connections between quantum events in the way this is done in classical physics, where causality is possible because we can, at least in principle, properly define both variables at any point. This is why Bohr argues that while the union of "the space–time co-ordination and the claim of causality" characterizes the classical theories, it is no longer possible in quantum theories, where these two idealizations (we deal with idealized models in both cases) become complementary.

Now, on the account of this last claim Bohr's position is different from that of Heisenberg in the uncertainty-relations paper, because, unlike Heisenberg, Bohr does attribute causality to the undisturbed quantum behavior, even if by way of a model, which is why I say that Bohr revises Heisenberg's argument. Heisenberg is cautious not to speak of the causality of the undisturbed behavior of quantum systems at this point—and is indeed resolute in speaking against it later in the paper. His formulation itself may or may not imply causality at this level, since it only says that "*all concepts which can be used in classical theory for the description of a mechanical system can also be defined exactly in atomic processes in analogy to the classical concepts.*" On the one hand, as I just noted, such a definition—that is, that of "*all concepts*" involved—is what assures causality in classical physics. On the other hand, even though he speaks of "disturbance," which is ultimately problematic, Heisenberg does not say that such concepts can be defined simultaneously for an undisturbed system. In closing his article, he regards "the presumption[s] that behind the perceived statistical world [of quantum observations] there still hides a 'real' world in which causality holds" as "fruitless and senseless . . . speculations" (Heisenberg 1927, *QTM*, p. 83). Heisenberg obviously rejects, just as Bohr does, causality at the level of observation in view of the uncertainty relations, which establish "the final failure of causality" (ibid.). This strong formulation—especially coupled to the one, cited above, against the existence of a hidden causal reality behind the observed statistical world—appears to imply that Heisenberg also rejects that causality applies at the quantum level. Indeed, he adds in a Bayesian vein, "[quantum] physics ought to describe only the *correlation* of observations" (ibid.; emphasis added).

One might argue that Heisenberg's claim concerning "the final failure of causality" is too strong. However, this claim is tempered, first, by the fact that it refers only to quantum processes and causality still applies in the classical domain. Second, it is tempered by the conditional nature of his argument, since, as is clear from his paper, it is made under the assumption that quantum mechanics is a correct theory of nature. It is of some interest that he says earlier in the paper—clearly referring to Dirac's paper on the transformation theory, discussed earlier (Dirac 1927)—that "one can say, if one will, with Dirac, that the statistics are brought in by our experiments" (Heisenberg 1927, *QTM*, p. 66). However, Heisenberg's meaning appears to differ from Dirac's, insofar as

Heisenberg refers primarily to the necessity of statistical considerations even in classical experiments, rather than, as Dirac does, to the causal nature of the independent quantum processes.

In a way, Heisenberg suggests here, along Kantian lines, that the ultimate constitution of nature may be beyond us in any event, and hence we cannot even know whether it is causal or not. His position is closer to that of Born (who never accepted that the independent quantum behavior is causal) or to Bohr's post-Como position than to that taken by Bohr in his Como argument or that of Dirac in his paper on the transformation theory cited by Heisenberg (Dirac 1927). Part of the reason is Heisenberg's much stronger position in advocating matrix mechanics and arguing against Schrödinger's wave approach than that taken, on both counts, by Dirac and Bohr. Neither Heisenberg nor Bohr of course accepted Schrödinger's philosophical view, since, against Schrödinger's desideratum, both still saw the role of observation and its noncausal intervention as irreducible in principle, rather than only in practice. Nevertheless, Schrödinger's theory appears to be significantly responsible for bringing causality back into quantum theory after the exile it suffered following Heisenberg's discovery of matrix mechanics. In his subsequent arguments, influenced by Bohr's Como argument, Heisenberg, too, invokes "causality," but more ambiguously and, it appears, ultimately only in a certain mathematical sense, to be explained presently, rather than in any physical sense.

I shall now consider how this new argument for causality in quantum mechanics works or, as the case may be, does not work in Bohr. First of all, the idealization of both observation and definition invoked by Bohr is primarily mathematical, and, in the case of definition, it refers to the mathematical formalism of quantum mechanics, such as Schrödinger's equation, which reflects the causality in question, even if not directly representing it. While Bohr's statement "*The very nature of the quantum theory thus forces us to regard the space–time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description*" introduces the concept or rather the idea of complementarity, Bohr does not rigorously define what he means by "complementary" in the lecture. Hence, complementarity is not yet a rigorous concept there but merely an idea, which needs to be developed into such a concept. Specifically, it is not clear what complementary means here beyond "exclusive." Eventually, Bohr defined complementarity more rigorously in the sense explained from the outset of this study. It designates (a) a mutual exclusivity of certain phenomena, entities, or conceptions; yet (b) the possibility of applying each one of them separately at any given point; and (c) the necessity of using all of them at different moments for a comprehensive account of the totality of phenomena that we must consider. As I stressed in introducing the concept, parts (b) and (c) of this definition are just as important as part (a), and disregarding them often leads to misunderstanding, which is quite common, and the Como argument may have led to some of these misunderstandings.[6] These aspects of the concept are not stated in the Como lecture, although (c) appears to be implied and appears to be the reason Bohr sees the space–time coordination and the claim of causality not only as "mutually exclusive" but also as "complementary." On the other hand, as will be seen presently, both (b) and (c) pose difficulties in the case of this particular complementarity. However, the definition itself just given is very general and allows for other complementary configurations, such as that of the (exact) position and momentum measurements or those (there are technically two complementarities here) of "the space–time description and the laws of the conservation of energy and momentum" (Bohr 1929a, *PWNB* 1, p. 94), each correlative to the uncertainty relations. In these cases all three features—(a), (b), and (c)—rigorously apply.

Now, if the independent behavior of quantum objects is, as Bohr maintains, mutually exclusive with observation, it follows that this behavior is unobservable. How and in what sense, then, could one speak of "the claim of causality" concerning this behavior, at least in physical terms, especially given that one needs something like the classical model to have physical causality? One might speak of "mathematical causality," or better, "determination," in quantum theory, whereby the equations of quantum mechanics *determine* the relevant *mathematical* object, say, a wave function in the case of Schrödinger's equation (the mathematical part of the "idealization" invoked by Bohr), at any time once it is known at a given time. Unlike in classical physics, however, where the same mathematical determination obviously holds as well, this determination does not translate into a physical causality even when the system in question is undisturbed by measurement, or at least it is not clear how to accomplish such a translation.

Bohr, however, appears at least to imply a certain physical causality here, without, again, supplying the requisite mechanism for this kind of translation. In particular, he says that while the space–time coordination and the claim of causality are mutually exclusive in quantum physics, the classical theory is characterized by the unity of both. In classical physics, however, the claim of causality is physical, and the equations of classical physics map physically causal processes. Hence, it appears that some physical causality, now as mutually exclusive with the space–time coordination, is implied by Bohr in quantum theory. At the same time, even if we grant that the space–time coordination

[6] An instructive example is Leonard Susskind's concept of the black hole complementarity, which he extrapolates from the wave–particle complementarity to the mutual exclusivity of the physical picture inside vs. outside a black hole (Susskind 2006, pp. 334–336). While this concept may be more consistent with the Como argument, it is clearly different from Bohr's ultimate understanding of the concept, as applied, for example, to the position and momentum measurement. For, in contrast with the possibility, always available, of performing either a position or a momentum measurement associated with the same quantum object (part (b) of Bohr's concept as delineated above), in Susskind's black hole complementarity no such alternative is obviously available. Bohr's concept, again, always refers to the role of the observer or of an instrument *outside* quantum objects and to the mutually exclusive and yet both possible at any given point, which is not the case in Susskind's concept.

and claim of (physical) causality are mutually exclusive and thus cannot apply at the same point, it is not clear why this is true as concerns the idealizations of "observation" and "definition," in terms of the formalism of quantum mechanics—for, we do appear to be able and even required to apply the formalism and, hence, the idealization of definition to the result of observations in order to make predictions concerning the outcome of experiments. It follows that if the claim of causality in question is merely that of mathematical determination, then it is not really mutually exclusive with the space–time coordination.

What might be ascertained is that, unlike in classical physics, in quantum physics the independent behavior (causal or not) of the quantum system and the observation *are mutually exclusive*, in view of the irreducible "disturbance" in question affecting the behavior of quantum systems in any act of observation or measurement on which our predictions concerning this behavior are based. Even the slightest possible observational "interference" (which is, again, a better term than "disturbance")—say, by a single photon—would be sufficient, as Heisenberg explained in his paper on the uncertainty relations (Heisenberg 1927, *QTM*, p. 65). It does, then, follow that this independent behavior of quantum systems is mutually exclusive with observation and hence is unobservable. In classical physics, the independent behavior of a given system is not mutually exclusive with observation because the disturbance or interference in question can, at least in principle, be neglected or compensated for. Hence, this behavior can be considered independently and happens to be causal, and the formalism of classical mechanics maps this behavior—at least, again, in the case of idealized models. Bohr is thus right to say that "*the union ... of the space–time coordination and the claim of causality ... characterizes the classical theories*." No such models appear to be possible in quantum theory. The overall situation just outlined compels Bohr to speak of the irreducible role of measuring instruments in quantum physics (in contrast to classical physics). But, again, in what sense, then, could one speak of the independent quantum behavior as causal? Or, to begin with, in what sense could one meaningfully claim that the formalism of quantum mechanics describes this independent behavior, especially since Bohr insists on the symbolic character of this formalism, including Schrödinger's equation? As we have seen, Schrödinger—whose earlier thinking concerning wave mechanics was shaped by the idea of a causal evolution of an electron (on a wave model) —made the point in his cat-paradox paper, "if a classical [physical] state does not exist at any moment, it can hardly change causally" (Schrödinger 1935a, *QTM*, p.154).[7]

[7] A causal quantum-level *behavior* is a more rigorously established feature in certain alternative interpretations of quantum mechanics, such as the many-worlds interpretation, or alternatives to quantum mechanics itself, in particular in Bohmian theories. The latter maintains not only the underlying causality of the independent quantum behavior but also the view that the probabilistic predictions of the theory (which coincide with those of the standard quantum mechanics) arise only due to our observational interference. No undistorted *description* of independent quantum behavior is possible, and the uncertainty relations are still valid, which also makes the theory ontological, as well as causal—again, at the cost of nonlocality. In the many-worlds interpretation the observational disturbance plays no role.

The problem just discussed is far from inconsequential and is found elsewhere and is indeed pervasive in the physics community, although Bohr's Como argument is not the only source of the view in question. For one thing, as I said, this view appears to originate with Dirac, who advanced it, via the transformation theory, in his influential 1927 paper "The Physical Interpretation of the Quantum Dynamics" (Dirac 1927), completed while in Bohr's institute in Copenhagen in 1926. The paper had a major impact on Heisenberg's thinking and his paper introducing the uncertainty relations, where, however, Heisenberg still maintained a strong position against the quantum-level physical causality. Both papers influenced Bohr's thinking at the time, as did Schrödinger's wave mechanics. However, while Schrödinger's program also aimed at a causal theory of quantum processes, and while it had influenced most of the arguments concerning quantum causality in question, his view of the situation, as discussed in Chapter 5, was quite different. The concept of disturbance of quantum processes by observation played no role in his argumentation, and he hoped that the causal behavior of quantum systems would, at least in principle, be accessible. Bohr appears to have been the first to formulate expressly the idea of the juxtaposition, as complementarity, of "causality" with "observation" ("disturbance") in the Como lecture. This juxtaposition (the idea of complementarity was yet to come) was only implied, albeit difficult to miss, in Dirac's paper. However, Dirac adopts Bohr's language and, I would argue, way of thinking, while not mentioning complementarity (the concept that he tends to shun in general) in his *The Principles of Quantum Mechanics*, published in 1930 and republished in four subsequent editions, the last in 1958 (Dirac 1958). Thus he says there, "[W]e must [in view of the nature of quantum phenomena and quantum mechanics] revise our ideas of causality. Causality applies only to a [quantum] system which is left undisturbed. If a system is [quantum-level] small, we cannot observe it without producing a serious disturbance and hence we cannot expect to find any causal connexion between the results of our observations" (Dirac 1958, p. 4).[8] This is close to Bohr's Como argument and the complementarity of the space–time coordination (defining observation) and the claim of causality as defined there. Also adopting Bohr's view, while following his language more expressly, are Heisenberg in his Chicago lectures, given in 1929 and published in 1930 as well (Heisenberg 1930), and J. von Neumann in his *Mathematical Foundations of Quantum Mechanics*, published in 1932 (von Neumann 1932). Given their prominence and impact, these works might well have been especially responsible for the prevalence of this view, and I shall consider their argument for it in the next section.

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[8] It is not really a matter of the "smallness" of a given quantum system, since a quantum system could be a large one, but only of the "smallness" of its ultimate quantum constituents. Large quantum systems cannot be observed as *quantum systems* without using classically described measuring instruments and hence without these systems being "disturbed," in the way we observe classical systems, by disregarding the role of Planck's constant, h, which we cannot do in considering quantum phenomena. Indeed, classical systems, at least in Dirac's (or Bohr's) view, ultimately have quantum constitution as well, which is unavailable to classical observation.

I am not saying that the claim that the independent behavior of quantum objects is causal is necessarily incorrect, although it is of course in conflict with the nonclassical view of quantum theory adopted here. However, even after an extensive perusal of foundational literature on quantum mechanics, I have not been able to find an adequate argument in support of this view, or even a suitable explanation of recurring formulations of it. As I noted at the outset of this study, there is no way to survey the literature on the subject in a truly comprehensive way, given that the ever-expanding proliferation of books and articles on the foundations of quantum mechanics has by now reached astronomical proportions. The classic major treatments discussed or cited in this chapter should be sufficiently representative for the case I would like to make. Of course, not everyone subscribes to this view. It has also been at least de facto, if not always expressly, challenged in several instances, beginning with Heisenberg in his introduction of the matrix quantum mechanics. As we have seen, even in his paper on the uncertainty relations, influenced by Dirac's transformation theory and his paper just mentioned (Dirac 1927), Heisenberg strongly argues against quantum causality at any level, including that of independent ("undisturbed") quantum processes. It is, accordingly, of particular interest that he adopts this view, at least to some degree, in the Chicago lectures, clearly under the impact of Bohr's Como argument. It is also worth noting that Born appears to have never subscribed to this view. More recently, this view was challenged—again, at least de facto—by those who pursue the Bayesian approach to quantum information theory, which is close to the nonclassical view of quantum theory adopted in this study. And of course, this view was challenged and rejected by Bohr in his thinking following the Como lecture.

Indeed, although leaving space for this type of view, even Bohr's argument in the Como lecture is less definitive in this regard than other arguments just mentioned, especially given his view of Schrödinger's wave mechanics as *symbolic* and his discussion of it in the lecture (Bohr 1927, *PWNB* 1, pp. 73–80). In particular, Bohr argues for the physical and epistemological (rather than only mathematical) equivalence between wave and matrix mechanics, and, for the reasons explained in Chapter 3 and to be further discussed below, matrix mechanics was not conducive to ascribing causality to independent quantum processes. As he says, "In fact, wave mechanics, just as the matrix theory, on this view 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" (Bohr 1927, *PWNB* 1, p. 75; emphasis added). From this perspective, Schrödinger's equation or any other partly mathematical machinery of quantum mechanics cannot be seen as describing or even referring, however indirectly, to the causal behavior of independent quantum systems themselves.[9] As a "mechanics," quantum

[9] Attributing to Bohr the view that it is possible to establish such relations, even as a form of direct mapping of the independent space–time behavior of quantum objects, is not an uncommon misunderstanding of Bohr's thinking, including in his later works, in the case of which this claim is especially problematic.

mechanics, in either form, is only a *symbolic* theory. That is (as, again, explained in earlier chapters), it assumes the formal structure of classical mechanics, say, in its Hamiltonian form, but only serves as a set of algorithms for the predictions, in general probabilistic, concerning the outcomes of certain possible experiments on the basis of certain previously performed experiments. While this formulation more reflects Bohr's subsequent argument than the one offered in the Como lecture, the latter oscillated between both views under consideration. Thus, Bohr's overall argument in the lecture tends to undermine his argument for the causal character of the independent behavior of quantum objects and the possibility that Schrödinger's equation maps this causal behavior unless measurement intervenes, which contention usually grounds the arguments of this type.

These remarks further illustrate my argument that, as Bohr's elaborations in the Como lecture introduce new concepts and make major steps forward, they also reflect difficulties and even confusions, inevitable in this process (as a passage toward the new), which, rather than a fully worked-out scheme, these elaborations appear to represent. Ultimately, the contributions outweigh the problems, beginning with the idea of complementarity but by no means ending there. Before returning to the question of quantum causality in the next section, I would like to close this section by summing up some of the contributions of the lecture, beyond the idea of complementarity. Its momentous significance in Bohr's work and beyond is self-evident, and my summary of other contributions of the lecture will not be able to bypass it either.

First of all, the concept of the quantum postulate as only symbolized by Planck's quantum of action (h)—rather than merely reflecting the discontinuous nature of atomic objects (Democritean atoms)—is a crucial point, especially as a step in introducing a new form of "atomicity" in physics. Second, the irreducible role of measuring instruments and its radical consequences are apparent, in particular in Bohr's argument that "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 [i.e., classical macrosystems]. Nevertheless, these abstractions are . . . indispensable for a description of experience in connection with our ordinary space–time view" (Bohr 1927, *PWNB* 1, pp. 56–57). It is worth noting that Bohr's full sentence contains a qualification "according to the view taken above," which suggests that one is dealing with an interpretation here (Bohr 1927, *PWNB* 1, p. 56). As explained above, this formulation in principle still leaves space for attributing such properties to quantum objects (at the time of measurement), which is short of Bohr's ultimate, nonclassical view, but it is a radical step, nonetheless, and certain near nonclassical implications are derived from it already in the Como lecture. Thus, Bohr says

> On the whole, it would scarcely seem justifiable, in the case of the interaction problem, to demand a visualization 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. (Bohr 1927, *PWNB* 1, p. 77)

This critical view of particles or wave-like radiation as independent physical processes is further amplified by his argument for the symbolic nature of quantum mechanics. As he says

> The symbolic character of Schrödinger's method appears not only from the circumstance that its simplicity, similarly to that of the matrix theory, depends essentially upon the use of imaginary arithmetic quantities. But above all there can be no question of an immediate connection with our ordinary conceptions because the "geometrical" problem represented by the wave equation is associated with the so-called co-ordinate 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, just as the formulation offered by matrix theory, involves a neglect of the finite velocity of propagation of the forces claimed by relativity theory. (Bohr 1927, *PWNB* 1, pp. 76–77)

It is important that Bohr addresses here, via the symbolic nature of both theories, their *physically* analogous nature, their mathematical equivalence being already established by that time. This argumentation is not that far away from his ultimate view, whereby no properties of any kind are attributed to quantum objects themselves, and hence one no longer would speak of any knowledge or even conception concerning them but only of certain effects of their interactions with measuring instruments upon those instruments. In this view, we only need to use some of the classical-like abstractions in question in their application to measuring instruments, which we can do, since these instruments are now described by means of classical physics. Rather than in terms of the quantum postulate, in Bohr's later work this argument is developed in terms of effects and phenomena, or, one might say, by interpreting the quantum postulate in these new terms. In addition, yet a further move is at stake, that of rigorously prohibiting an attribution of even single physical properties to quantum objects and motion, rather than only certain joined such properties as constrained by the uncertainty relations.

It is also worth noting the appearance of certain key terms and concepts that are to remain with Bohr throughout his writings on complementarity, such as "a renunciation as regards the causal space–time coordination of atomic processes," "ambiguity of definition" of variables at the quantum level, or "distinguishing sharply." "Renunciation" becomes a very prominent and resonant word in Bohr's writing, from his next major contribution, "The Quantum of Action and the Description of Nature" (Bohr 1929a), to his reply to EPR, and beyond. In his reply to EPR, Bohr argues that "the very existence of the quantum of action entails . . . 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" (Bohr 1935b, p. 697).

The argument that once "in order to make observation possible we permit certain interactions with suitable agencies of measurement, … an unambiguous definition of the state of the [quantum] system is naturally no longer possible" (Bohr 1927, *PWNB* 1, p. 54) will eventually translate itself into the "essential ambiguity" Bohr locates in the EPR argument. It is, he will argue, no longer possible in considering quantum phenomena to unambiguously ascribe any variables or properties, single or joint, to quantum objects and processes. In "Discussion with Einstein," he describes his counterargument to EPR and, in effect, his later version of complementarity as aiming "to bring out the *essential ambiguity* involved in a reference to physical attributes [even single such attributes] of objects when dealing with *phenomena* where no *sharp distinction* can be made between the behavior of the *objects* themselves and their interaction with the measuring instruments" (Bohr 1949, *PWNB* 2, p. 61; emphasis added). At the same time, as against the Como lecture, the *distinction* between the unobservable quantum *objects* and observable *phenomena*, defined through the effects of the interaction in question upon the measuring instruments, is now *sharply* defined analytically, although they cannot be sharply distinguished experimentally within the wholeness or indivisibility of phenomena in Bohr's sense. It remains crucial for Bohr that, indescribable or even unthinkable as quantum objects may be, their independent existence manifests itself in measuring instruments, in part via the epistemological discontinuity between them and the observable phenomenal effects—for which effects quantum objects are nonetheless responsible.

Finally, from the Como lecture on, the parallel with relativity is aimed to amplify his and Heisenberg's point, discussed earlier, that classical physics may itself be seen as derived from classical theories elsewhere and also, in part correlatively, as a refinement of everyday experience and language. As such it is no longer applicable to physical objects at both scales, those of relativity (the large magnitude of c, the speed of light) and quantum physics (the small magnitude of h, Planck's constant)—a major general point as concerns the nature of the physical world and of our relation to it, and even our place in it.

In sum, although significant refinements and even major changes are introduced as Bohr develops his thinking concerning complementarity, the Como lecture establishes the basic framework or matrix for this thinking and a kind of blueprint for the exposition of the subject. Most crucially, it introduces some of Bohr's key concepts, beginning with complementarity itself (in the narrow sense) and prepares grounds for rigorously developing the fundamental architecture (physical, mathematical, and philosophical) of these concepts and for the introduction and developments of his other key concepts, such as phenomena and atomicity.

6.4 Quantum Causality in Dirac, Heisenberg, and von Neumann

As I said, Dirac, Heisenberg, and von Neumann follow Bohr's argument in, respectively, *The Physical Principles of the Quantum Theory, The Principles of Quantum Theory* (the Chicago lectures), and *Mathematical Foundations of Quantum Mechanics*, all published around the same time (1930–1932). These works, along with Weyl's *Theory of Groups and Quantum Mechanics* (Weyl 1928), were the most important early books on quantum mechanics. It is worth noting that two of these books were written by physicists who were among the founding figures of quantum mechanics, and two others by leading mathematicians of the time who contributed to the mathematics of quantum theory. This fact further testifies to the essential role of mathematics in both quantum physics and the philosophy of quantum theory, as is clear from Heisenberg's Chicago lectures, the most philosophical and philosophically sophisticated among these works. Quickly becoming classics, these books shaped our thinking concerning quantum mechanics from then on and still continue to do so. As such, they might well have been most responsible for the propagation of the argument concerning quantum causality that I question here. Weyl's book does not really address the problematic in question and hence will not be discussed here. It does, however, appear to take a position similar to those taken in the other books just mentioned concerning causality (Weyl 1928, p. 54), even though it expresses skepticism concerning Schrödinger's wave approach (p. 48).

I would like to consider Dirac's arguments first because, as I said, Dirac held the type of view in question already in his paper "The Physical Interpretation of the Quantum Dynamics," completed in 1926 while in Copenhagen and published in 1927 (Dirac 1927). Thus, Dirac's argument for this view precedes both Bohr's Como argument and Heisenberg's work on the uncertainty relations, influenced by this paper, without, in Heisenberg's case, subscribing to this view, which Heisenberg only adopted, still ambivalently, in the wake of Bohr's Como argument. It is difficult to say whether Dirac's paper influenced Bohr or was, conversely, influenced by Bohr's thinking, and the definitive answer is not that important at the moment. What is important is that, stimulated by his transformation theory (which rigorously connected both Heisenberg's and Schrödinger's versions of quantum mechanics), Dirac saw his argument for the causality of the behavior of the "undisturbed" quantum system as compatible with Heisenberg's matrix mechanics. This is also true for Bohr's Como argument, influenced by the transformation theory, both directly and via Heisenberg's paper on the uncertainty relations. Initially, matrix mechanics avoided this view by virtue of dealing only with discontinuous and acausal transitions between quantum states ("quantum jumps"), rather than dealing with the evolution of an electron in an atom in the way Schrödinger's (time-dependent) equation or Dirac's q-number formalism allows one to do.

Now, according to Dirac, "One can suppose that the initial state of a system determines definitively the state of the system at any subsequent time. . . . The notion of probabilities does not enter into the ultimate description of mechanical processes; only when one is given some information that involves a probability ... can one deduce results that involve probabilities" (Dirac 1927, p. 641). The last proposition is correct. The question is whether what precedes it is true and in what sense, which hinges on the concept of "quantum state," as discussed in Chapter 5. Dirac's statement and those of others to the same effect cause no problem if one sees a quantum state only as a mathematical object (say, a vector in a complex Hilbert space) that, via Born's or an equivalent rule, enables one to predict the probabilities in question, rather than as a physical concept, especially if conceived on the model of classical mechanics. The formalism of quantum mechanics allows for an unequivocal determination of a state vector as a mathematical object under a given mathematical transformation, say, under an action of an energy operator. This determination need not translate into a physical description and could only reflect how we make our predictions along the lines considered and with the qualifications given in Chapter 5. Dirac, however, evidently implies some physical causality by invoking "the ultimate [quantum-level] description of mechanical processes," seen as causal here, or by the title phrase of his paper, "the *physical interpretation* of the quantum dynamics" (emphasis added). Dirac's similar argument in *The Principles of Quantum Mechanics* should, accordingly, not be surprising. However, the influence of Bohr's Como argument is now palpable. Dirac says

> [W]e must revise our ideas of causality. Causality applies only to a system which is left undisturbed. If a system is [quantum-level] small, we cannot observe it without producing a serious disturbance and hence we cannot expect to find any causal connexion between the results of our observations. Causality will still be assumed to apply to undisturbed [quantum] systems and the equations [those of quantum mechanics] which will be set up to describe an undisturbed system will be differential equations expressing a causal connexion between conditions at one time and conditions at a later time. These equations will be in close correspondence with the equations of classical mechanics, but they will be connected only indirectly with the results of observations. There is an unavoidable indeterminacy in the calculation of observational results, the theory enabling us to calculate in general only the probability of our obtaining a particular result when we make an observation. (Dirac 1958, p. 4)

It follows that the results of observation in space–time and the determination, including that of time itself, and causality of the connections between conditions at one time and conditions at a latter time are mutually exclusive or *complementary*. Thus, the complementarity of space–time coordination and the claim of causality is de facto used by Dirac and used exactly in the way it was by Bohr in the Como lecture, with the statement itself nearly paraphrasing Bohr's formulation, even if without referring to Bohr or complementarity, which is not mentioned in the book. Even though Dirac was involved in editing the English version of the Como lecture, he, as I indicated above, never especially liked and almost shunned the idea of complementarity. It is true that by the time Dirac published his book, the view in question was not uncommon. Also, in his skepticism concerning complementarity, Dirac appears to have had in mind primarily the wave–particle complementarity, rather than that of the space–time coordination and the claim of causality. On that score his suspicion would be justified, except that Bohr, too, was suspicious of the wave–particle complementarity, as he was by then of that of the space–time coordination and the claim of causality. Dirac does not appear to have considered or at least commented on Bohr's post-Como work, although this work was available by the time the book was published in 1930, let alone by the time of subsequent revised editions, the last of which appeared in 1958.

Be that as it may, Dirac's statement just cited clearly carries the same difficulties as Bohr's Como argument. The "conditions at one time and conditions at a later time" that are claimed to be causally connected by differential equations of quantum mechanics cannot be ever physically determined and are, in principle, unobservable, since any observation, by definition, destroys the presumed causal connection in question. On what basis, then, can one claim the causality of this connection, and in what sense—beyond, again, the mathematical determination of variables involved by the differential equations in question? However, just as Bohr does, Dirac appears to imply the physical causality of independent quantum behavior by saying that "causality will still apply to undisturbed [quantum] systems," rather than only to the equations of quantum mechanics. These equations, Dirac says, are "connected only *indirectly* to the results of observation." One presumes that "indirectly" means in terms of probabilistic predictions, given Dirac's important final sentence. But what are these equations connected *directly* to in physical terms? On the next page, Dirac himself inadvertently suggests that his contention could be questioned along these lines, and perhaps the rigor of his thinking compels him to do so. He says, "A question about what will happen to a particular photon under certain conditions is not really very precise. To make it precise one must imagine some experiment performed having a bearing on the question and inquire what will be the result of the experiment. Only questions about the results of experiments have a real significance and it is only such questions that theoretical physics has to consider" (Dirac 1958, p. 5). This sounds closer to the post-Como Bohr, who no longer thinks of the independent ("undisturbed") behavior of quantum systems. However, once one makes this statement, it becomes difficult to sustain the claim that "causality will still apply to undisturbed [quantum] systems."

Heisenberg's Chicago lectures, *The Physical Principles of the Quantum Theory*, published in the same year (1930) and shaped as much by Bohr's Como argument as by his own work on the uncertainty relations, give Bohr's scheme an elegant diagrammatical form:

<table>
<tr><th colspan="3">CLASSICAL THEORY
Causal Relationships of Phenomena Described in Terms of Space and Time
QUANTUM THEORY</th></tr>
<tr><td>Either</td><td></td><td>Or</td></tr>
<tr><td>Phenomena described in terms of space and time</td><td rowspan="2">Alternatives related statistically</td><td>Causal relationship expressed by mathematical laws</td></tr>
<tr><td>But</td><td>But</td></tr>
<tr><td>Uncertainty principle</td><td></td><td>Physical description of phenomena in space–time impossible</td></tr>
</table>

(Heisenberg 1930, p. 65)

The meaning of "Alternatives related statistically" appears to be that, although our predictions concerning quantum experiments are enabled by certain mathematical laws, defined "causally" by the formalism of quantum mechanics, these predictions are nevertheless only probabilistic or statistical. Heisenberg adds, "It is only after attempting to fit this fundamental complementarity of space–time description and causality into one's conceptual scheme that one is in a position to judge the degree of consistency of the methods of quantum theory (particularly of the transformation theory)" (Heisenberg 1930. p. 65). The statement itself is opened to questioning along the lines of the preceding discussion. Heisenberg does, however, pinpoint a crucial aspect of the situation and the main difficulty of Bohr's Como scheme by invoking "causal relationships expressed by mathematical laws," rather than physical causality, which is difficult to assume given that a "physical description of phenomena in space–time is impossible"—*any physical description*, let alone a causal one. This contention is not surprising, since, as we have seen, Heisenberg strongly argued against introducing physical causality into the quantum mechanical situation in his uncertainty-relations paper. "There exists a body of exact mathematical laws," he also says here, "but these cannot be interpreted as expressing simple relationships between objects existing in space and time" (Heisenberg 1930, p. 64). Heisenberg's later statement, cited in Chapter 5, corroborates this view: "[The wave function] obeys an equation of motion as the co-ordinates did in Newtonian mechanics; its change in the course of time is completely determined by [Schrödinger's equation], but it does not allow a description in space and time" (Heisenberg 1962, p. 49). By the same token, one cannot rigorously speak of "an equation of motion" either, and Heisenberg is clearly aware of this. Heisenberg's qualification, again, compels us to ask, What is, then, the meaning of causality under these conditions? Which relationships—and between which elements—are causal and are, as such, expressed by mathematical laws of quantum theory? Heisenberg does not answer this question either, any more than do Dirac, Bohr, and von Neumann. However, by posing this question—by, on the one hand, invoking "causal relations expressed by mathematical law" and, on the other hand, stating that these laws "cannot be interpreted as expressing simple relationships between objects existing in space and time"—Heisenberg invites and even necessitates the kind of analysis undertaken here.

I would like now to extend this analysis by considering von Neumann's more mathematical argument for the causal character of the independent behavior of quantum systems. According to von Neumann

> *[O]n the one hand,* a state ϕ is transformed into the state ϕ' under the action of an energy operator H in the time interval $0 \leq \tau \leq t$:
>
> $$\frac{\partial \phi_\tau}{\partial t} = -\left(\frac{2\pi i}{h}\right) H\phi_\tau (0 \leq \tau \leq t), \tag{1}$$
>
> so if we write $\phi_0 = \phi$, $\phi_t = \phi'$, then

$$\phi' = e^{-(2\pi i/h)tH}\phi \tag{2}$$

which is *purely causal*. A mixture U [U is a statistical operator] is correspondingly transformed into

$$U' = e^{-(2\pi i/h)tH}Ue^{(2\pi i/h)tH} \tag{3}$$

Therefore, *as a consequence of the causal change of* ϕ *into* ϕ', the states $U = P_{[\phi]}$ go over into the states $U' = P_{[\phi']}$

$$U \rightarrow U_t = e^{-(2\pi i/h)tH}Ue^{(2\pi i/h)tH}$$

On the other hand, the state ϕ —which may measure a quantity with a pure discrete spectrum, distinct eigenvalues and eigenfunctions $\phi_1, \phi_2, \ldots$—undergoes in a measurement *a non-causal change* in which each of the states $\phi_1, \phi_2, \ldots$ can result, and in fact does result with the respective probabilities $|(\phi, \phi_1)|^2, |(\phi, \phi_2)|^2, \ldots$. That is, the mixture

$$U' = \sum_{n=1}^{\infty} |(\phi, \phi_n)|^2 P_{[\phi_n]} \tag{4}$$

obtains. More generally, the mixture U goes over into

$$U' = \sum_{n=1}^{\infty} |(U\phi_n, \phi_n)|^2 P_{[\phi_n]} \tag{5}$$

Since the states go over into mixtures, the process is not causal.

The difference between these two processes $U \rightarrow U'$ is a very fundamental one: aside from the different behaviors in regard to the principle of causality, they are also different in that the former is (thermodynamically) reversible, while the latter is not. (von Neumann 1932, pp. 417–418; emphasis added)

While having a more rigorously mathematical form, epistemologically and conceptually, this set of propositions and von Neumann's argument supporting them largely follow those of Bohr, Dirac, and Heisenberg, as just discussed. Throughout the book, von Neumann's elaborations on the subject are marked by similar ambivalences concerning the possibly physical nature of the causality invoked here vis-à-vis the relationships between mathematical entities (e.g., vectors and operators in Hilbert spaces), which express the laws linked to physical (here quantum) objects or phenomena. These ambivalences also clearly reflect the ambiguity of the term "state," which appears in the book to designate both a certain mathematical concept (a vector in a complex Hilbert space) and a certain physical concept (on the model of classical physics), without properly explicating the relationships between these concepts. These difficulties haunt von Neumann's analysis throughout. Thus, on the one hand, his mathematical formulation here and related formulations found in the book appear to suggest that he has in mind primarily a certain mathematical "causality" or determination (e.g., von Neumann 1932, p. 357). So do other arguments in the book, specifically to the effect that the laws of nature may not be and the ultimate (quantum) constitution indeed cannot be *causal*, at least if quantum mechanics is correct. The theory would have "to be [proven] objectively false, in order that another description of the elementary processes than the statistical one be possible" (von Neumann 1932, p. 325). On the other hand, von Neumann does not expressly say here or elsewhere in the book that this causality is only mathematical, and, since the noncausal measurement process corresponding to Equation (4) is physical, it is difficult to avoid an implication that so is the causal process corresponding to Equation (2). His claim that the process $U \rightarrow U_t = e^{-(2\pi i/h)tH}\, U\, e^{(2\pi i/h)tH}$ (the states $U = P_{[\phi]}$ go over into the states $U' = P_{[\phi']}$, where U is a statistical operator), which is "*a consequence of the causal change of* ϕ *into* ϕ'," is "thermodynamically reversible" also suggests, in view of the generally physical nature of the latter concept, that this process and the causality itself in question may be physical in nature, as does his analysis of "measurement and reversibility" in Chapter 6 of the book.

Now, leaving aside the independent evolution of mixtures (to which the present argument could be easily extended), if the *independent evolution* of "a [quantum] state ... under the action of an energy operator," $\phi' = e^{-(2\pi i/h)tH}\,\phi$ (Equation (2)), is assumed to represent or relate to any physical process, this process is not observable, and hence the *assumption* that the evolution of a quantum state is "purely causal" appears to be nothing other than an assumption. Equations (1) and (2), as describing the (causal) transformation of the state vector, are not given a rigorous physical content in von Neumann's analysis. As I argue, it may not be possible to do so, as opposed to the case of the noncausal process defined by measurement, represented by Equation (4). Coupling the overall scheme to Schrödinger's equation, with a wave function ψ (which can also be connected to a ket-vector in a Hilbert space, $|\psi>$), enables one to make a prediction concerning the outcome of a future measurement, at time t, on the basis of a measurement performed at time t_0.[10]

By means of measuring a certain physical quantity, represented by the corresponding operators (say, Q, in the case of a coordinate measurement), we can give the necessary numerical specification to the formalism involved, as reflected at the time of measurement by a given state vector, say, $\phi = \phi_0$, at t_0. We can consider this measurement as an initial measurement in a given experiment. It does not follow, in view of the uncertainty relations, that we (fully) *know* the *physical* state of the system at this point, especially if we understand "physical knowledge" on the model of classical physics, and von Neumann's mathematical argumentation here and in the book properly reflects this situation. Nevertheless, this type of specification obtained in a measurement allows

[10] I shall bypass the technical details of the procedure, found in any standard treatment of quantum mechanics. See, for example, Feynman's lucid exposition (Feynman et al. 1977, vol. 3, 16.4–16.16). Some of the problems of relating the formalism to the independent behavior of quantum objects are found in his exposition as well. Feynman, however, avoids speaking of causality in the way von Neumann does, although he does speak of Schrödinger's equation as deterministic, but, it appears, only in the mathematical sense.

us to predict—using the formalism, say, again, of Schrödinger's equation, and Born's rule or von Neumann's projection postulate (Equation (4))—the probability that a future position will be within a given region at t, if a measurement is performed at that time. We do need Schrödinger's equation to make proper predictions, but we need not assume that it relates to any causal evolution of the object in question, any more than do Equations (1) or (2). On the other hand, given that the exact position measurement will, in view of the uncertainty relations, make the value of the momentum at t_0 entirely undetermined, no prediction concerning future momentum measurements is possible at all. Accordingly, a measurement only allows us to obtain "partial" information as specified by the uncertainty relations, at most one half of the information that we can obtain in classical physics. The quantum information (in either sense) in question could of course be called "partial" only in the sense of this comparison with classical physics, since rigorously this is as much information as is ever available, and hence, this information is complete. From this perspective, quantum mechanics can be seen as a complete theory. It gives us as complete knowledge concerning quantum phenomena as possible by allowing us, on the basis of measurements performed, to make the corresponding specifications of quantum states, which specifications enable us to make as good predictions as are possible in all experimental situations (thus far available) within the proper scope of quantum mechanics. It is true that these predictions are in general only probabilistic in nature.[11] However, no other predictions appear to be possible, given that repeating the *identically prepared* experiments in general leads to different recordings of their outcomes, the fact stressed throughout this study and reflected in von Neumann's Equation (4), within his ensemble interpretation, grounded in the overall situation just outlined (von Neumann 1932, pp. 206–209). Accordingly, quantum mechanics provides as complete knowledge of the state of a quantum system as possible within his ensemble scheme, even though "only statistical statements can be made on the values of the physical properties involved," as von Neumann indeed argues (e.g., von Neumann 1932, p. 207).

In von Neumann's scheme (under the assumption that the quantum mechanical formalism does relate to some physically causal process), the physical situation would be, roughly, as follows. It is important to keep in mind that, unlike in classical statistical mechanics, in the standard quantum mechanics we still expressly deal experimentally with individual quantum systems.[12] Accordingly, even in an ensemble approach, we deal with sequences of identically prepared individual experiments upon quantum objects, such as electrons or photons, each considered identical in turn. In this respect, the situation is

[11] Rigorously speaking, in dealing with repeated experiments, even the specification of ϕ_0 is ultimately statistically based, which leads to further complexities that I shall put aside here, since they do not affect and may indeed be used to amplify the present argument.

[12] For an (epistemologically) classical ensemble approach and for a more general discussion of the frequentist view of probability, see Khrennikov (2009a, b).

similar to a sequence of coin tosses, with the crucial difference that the probability distribution of multiple events may, in quantum events, be different depending on the experimental setup. The dice become "loaded" under certain conditions, as in the case of the setup of the double-slit experiment in which the interference pattern is observed. If we consider an emission, one by one, of electrons from a source in the double-slit experiment in this setup, in von Neumann's approach each individual process is assigned a state vector, ϕ, which is transformed according to Equation (2). This individual description, however, applies only if no observation is made, in this case an observation defined by the trace of the collision between an electron and the screen. In other words, this description, again, refers only to the individual physical process, causal or not, which is in principle unobservable, and hence in this case only to something that takes place before the collision. It follows that the *physical* states with which ϕ and $\phi' = e^{-(2\pi i/h)tH}\phi$ would be, respectively, associated are unobservable as well. Either state may in general be different in each individual process in question, from the emission of each object to the collision between it and the screen. (In the alternative, no-interference, setup of the double-slit experiment, an event of each photon's passing through a slit may be considered as an act of measurement, which affects our estimations of the probabilities concerning where each photon will hit the screen.) The initial *classical* physical state of the source—and hence, mathematically, the wave function, as a catalogue of probabilities—would be specified identically for each emission, which would, accordingly, allow us to establish the same catalogue of probabilities for each emission. However, that does not guarantee that the *quantum* physical state of the emitted object, if it could be known directly (in the way that is possible in classical physics), would be the same at any point in each individual run of the experiment. The dispersion of traces on the screen tells us that the state of each object right before its collision with the screen is different. Quantum mechanics gives correct predictions for each distribution, which is remarkable and fortunate for us, if as mysterious as quantum behavior itself, since we do not know *why* quantum mechanics works.

In von Neumann's scheme, the measuring process "disturbs" the system by discontinuously and noncausally changing its physical state as it evolved just prior to the measurement. (When we deal with an emission of an object, there is no prior evolution.) The measurement is assumed to be instantaneous, that is, it "must be carried through in so short a time that the change of [state or mixture] is not yet noticeable," for otherwise the time–energy uncertainty relations will make the whole scheme inapplicable (von Neumann 1932, pp. 352–354). In other words, the time of the interaction between the object and the measuring apparatus is assumed or, one might say, idealized to be zero. By the same token, if one repeats the measurement instantaneously in the same sense and in the same setting, the result will always be the same. This ideal "repetition" in the same setting should of course be distinguished from a repeated quantum experiment in a new, even if identically prepared, setting. Quantum mechanics properly predicts the observed statistical (ensemble) distributions, arising in von Neumann's scheme, as in other cases under discussion here, only due to the disturbance of each (causal) individual quantum process introduced by measurement; and, in von Neumann's interpretation, it predicts only them rather than the probabilities of individual quantum events.

In a Bayesian view, such as the one adopted in this study, an individual wave function is, in Schrödinger's language, a probability catalogue of possible future measurements to be performed in that individual system, without assuming that the quantum mechanical formalism relates to the independent behavior of quantum objects and without assuming this behavior to be causal. It is instructive to discuss briefly how this approach compares to that of von Neumann. Let us consider, first, what happens if—given that whatever physical process (causal or not) to which von Neumann's Equation (2) corresponds or relates is unobservable—we suspend the assumption of any such correspondence (or causality) altogether. We can still use the same numerical specification of the initial situation by a measurement of a given physical quantity linked to the corresponding operator—say, again, a coordinate (Q), at a given point t_0— and use the same formalism to make the same predictions for what happens at a later point t, by means of our catalogue of probabilities for a future experiment compiled with the help of the wave function. Now, however, only Equation (4) would have a physical content by virtue of reflecting this catalogue of probabilities. The only role of the rest of the formalism, in particular Schrödinger's equation, is that it gives us the probabilities for our predictions concerning an outcome of the corresponding experiment at time t by using the data from the initial measurement at t_0. This role, which may be seen as involving what I call mathematical causality, is of course crucial. The equations themselves, however, have no physical content of their own. They only acquire physical content once they are combined with the procedure governed by Equation (4). Mathematically, the difference from the approaches based on the physical causality of the independent quantum-level behavior, such as those considered here, is trivial, if any. Epistemologically, however, the difference is fundamental.

We do not, accordingly, need to interpret the quantum mechanical situation in terms of ensembles, as von Neumann does and as he appears to think necessary, perhaps because of his assumptions concerning the causality of the independent evolution of individual quantum systems, although one can also pursue an ensemble interpretation of quantum mechanics apart from this assumption. Instead, one sees quantum mechanics as a theory of (or helping) our estimate concerning individual events associated with quantum phenomena, in the present view as manifested in measuring instruments. Our probabilistic assessment of the outcome of experiments may involve previously obtained data based on statistical samples. Nevertheless, the wave function and the relevant equations, cum Born's rule or some equivalent rule, allow us to make our probabilistic estimates, our *bets*, individually on the basis of the previously obtained data. Indeed, as I discussed in Chapter 5, Born's interpretation of the wave function, as originally offered, refers to the probabilities of individual observable events, in accordance with Schrödinger's equation. Born's and related rules became subject to various ensemble-type interpretations, such as that of von Neumann, only subsequently. Born's own position appears ambivalent in this respect. On the other hand, as I noted earlier, Born is not ambivalent as concerns causality: For him there is no physical causality of any kind in quantum mechanics. He never subscribed to the view that the independent quantum behavior is causal, although he appears to be more open to the idea that quantum behavior can somehow be described in (noncausal) spatio-temporal terms by the formalism of quantum mechanics.

Consider, along the lines explained in Chapter 5, the evolution of a given individual quantum system, say, an electron, to which the corresponding Schrödinger's equation *relates*, after the initial situation is mathematically specified by a measurement (say, again, that of the position of an electron), but only in predictive terms, without *describing* this evolution in physical terms. One might speak of certain *potential* or *virtual* probabilities for each given subsequent point t_n at which a measurement *could be* performed. The sequence of possible measurements is always discrete without allowing us to relate, in the manner of classical physics, to any "in-principle-observable" continuous motion of the object in question. We can compile a catalogue of probabilities of predicting the outcome of each experiment possibly to be performed at each such point. In von Neumann's view, this propagation of probabilities is still tied to the transformation of the state under the action of the energy operator, with a potential implication of the physical causality of the independent individual quantum process itself. Born, by contrast, suggests no implications of this kind, although his view implies a certain mathematical causality or determination of probability, which refers to a catalogue of our expectations concerning certain future possible events. Any act of measurement discontinuously resets both the future evolution of the system and the propagation of virtual probabilities following this measurement. The process starts anew, ab ovo, with each new measurement, which erases the outcome of the previous measurements as meaningful for future predictions concerning the system (Schrödinger 1935a, *QTM*, p. 154). Assuming, for the moment (this is not always the case), that we deal with the same quantum object, only the last measurement in any such sequence is meaningful for the subsequent predictions. Each measurement changes the expectation catalogue, or rather creates a new one. Given these circumstances, the "propagation" (of which Born speaks metaphorically) of virtual probabilities can be defined mathematically but is physically fictitious, since it does not relate to any sequence of actual physical events concerning which we make our predictions. After a predicted measurement is performed—say, at a point t_1—it may not be possible to use the object for any subsequent predictions at all. A new initial experiment and a new sequence of potential future measurements will have to be set up, and even if we can specify the initial setup identically at a *new* t_0', the outcome of the measurement at $t_1'(t_1 - t_0 = t_1' - t_0')$ will, in general, not be the same.

Of course, one can also pursue, as von Neumann does, an ensemble interpretation of quantum mechanics and even take the position that nature in fact disallows us even probabilistic, let alone causal, laws concerning individual phenomena. Pauli appears to have taken this type of position as well. He argues that individual quantum events in general are not "comprehended by laws," although it is possible that he refers only to causal laws. He says, "The theory predicts only the *statistics* of the results of an experiment, when it is repeated under a given condition. Like an ultimate fact without any cause, the *individual* outcome of a measurement is, however, in general not comprehended by laws" (Pauli 1994, p. 32).

There are several reasons why a Bayesian view might be preferable. First of all, as explained above, the quantum mechanical formalism and Born's or equivalent rules do allow us to predict the probabilities of individual quantum events. This view allows one to maintain the initial Heisenbergian sense of quantum mechanics as a proper quantum equivalent of classical mechanics as a theory of individual physical processes and events. It is true that this mechanics is not a mechanics in the sense of *describing* individual quantum processes, in the way classical mechanics is. One might regret—as Einstein and many others did (and still do)—that only a probabilistically predictive, rather than descriptive, theory is as far as we can reach in quantum physics. On the other hand, it might be nature itself that disallows alternatives of the kind Einstein wanted. Quantum mechanics has remained the standard theory and no descriptive alternative has taken hold for nearly a century now (there has been no dearth of proposals).[13] Nor need one necessarily regret this situation, and not everyone does, beginning with Bohr and other founding figures, such as Heisenberg, Born, Pauli, and Dirac.

Second and most pertinent to the present discussion, von Neumann's ensemble interpretation *may* allow for and *perhaps* assumes the underlying physical causality of individual quantum processes, even though these processes and, hence, this causality remain inaccessible. I qualify this statement because, as I also argue, von Neumann's argument may be seen as ultimately ambivalent or unresolved on this point. In particular, as I noted earlier, von Neumann argues, in his ensemble framework, that *if quantum mechanics is correct*, the laws of nature at the ultimate level of its constitution would have to be noncausal, and "[no other description] of the elementary processes than the statistical [is] possible" (von Neumann 1932, p. 325). The main reason for this, as von Neumann shows, is that "all quantum ensembles have dispersion, even the homogeneous [ones]" (von Neumann 1932, p. 323). Composed of identical

[13] Of course, apart from the fact that arguably a majority of physicists and philosophers of quantum theory are dissatisfied with the apparent lack of descriptive capacity in quantum mechanics, the number of physicists who subscribe to alternative views, such as the many-worlds interpretation, is not negligible. (Those subscribing to Bohmian theories are a small minority.) It is also true that certain other approaches to quantum mechanics claim a greater descriptive capacity for quantum mechanics, although, as I noted in the Introduction, these claims may be questioned (e.g., Plotnitsky 2006b, pp. 84–85). The point here is that non-classical interpretations are logically and experimentally consistent.

individual particles, the latter paradigmatically represent the standard case of quantum measurements, and their dispersion reflects that identically prepared quantum experiments lead to different outcomes. What von Neumann's argument shows is that quantum mechanics properly reflects this situation, which is why he argues that if the theory is correct, "no other description of the elementary processes than the statistical [is] possible." The question is whether this argument is consistent with the *possibility* that the undisturbed individual quantum processes, as described by the evolution of the state vector under the action of the energy operator, are causal. I, again, speak of "possibility" because von Neumann only speaks of the causal evolution of the state vector itself and only appears to suggest or imply the causality of the corresponding physical process. I cannot offer here a detailed analysis of von Neumann's argument, which takes up nearly half of his book—an analysis that might be necessary in order to settle this question. I shall, instead, take a different approach, which allows one to make the epistemological argument against causality that I would like to make without definitively answering this question.

Let me reiterate, first, that—even leaving aside that von Neumann does not expressly qualify his claim—it does appear that several aspects of his argument indicate that he allows for a possibility that independent individual quantum processes may be causal. *First* and most significantly, there are statements to that effect, such as (as noted earlier) those concerning the "thermodynamical" reversibility of the causal processes in question, which is a physical concept. *Second*, since measurement is a physical process, von Neumann's emphasis throughout on the idea of disturbance by measurement in bringing noncausality into the laws of nature suggests this possibility as well, even though he makes qualifications that could be used against this possibility. Among these qualifications are, in particular, his discussion of the question of the "cut" between the classical and quantum domains via the psycho-physical parallelism (von Neumann 1932, pp. 418–420) and his argument that the noncausal nature of the measuring process is not due to "any incomplete knowledge of the state of the observer" (von Neumann 1932, pp. 437–439). *Third*, finally, by restricting his analysis of noncausal changes due to measurement strictly to ensembles, rather than individual quantum processes, von Neumann appears to allow for a possibility that independent individual quantum processes may be causal. As discussed above, the Bayesian view, which also accords with Bohr's post-Como and especially post-EPR view, allows one to bypass this implication more easily.

It might still be possible, however, that von Neumann does not ultimately subscribe to the view that the independent physical behavior of individual quantum systems is causal but only to a certain mathematical causality or determination of the quantum state as a mathematical object with the formalism of quantum mechanics. While it would, in my view, be difficult to deny the presence of ambiguities in von Neumann's position on this point, it may not be necessary to resolve them to make the main conceptual point of the present analysis, as against a rigorous interpretive or historical claim. This point is that the claims concerning the causal nature of independent quantum processes—or, more generally, concerning the describability of these processes by the formalism of quantum mechanics—are not adequately supported and are not justified by any experimental evidence or by the theoretical, including mathematical, structure of this formalism. On the other hand, especially at this point, there are good reasons to argue against this possibility.[14] Accordingly, if von Neumann does subscribe to this claim, as do, again, many others, his position is difficult to support. If, however, von Neumann has in mind only mathematical causality, then his position is closer to the one taken here, which denies any other causality both to quantum processes and to the connections between observable quantum phenomena. The present approach remains different insofar as it sees quantum mechanics, along Bayesian lines, as a probabilistic theory of individual quantum events, rather than, as von Neumann does, as a statistical theory of ensembles.

6.5 A Brief History of Quantum Causality

Given the preceding argument—beginning with its starting point, recognized by the authors discussed here, that the independent ("undisturbed") physical behavior of quantum objects is unobservable—what, then, is *the basis* for the view that this behavior is *physically*, rather than only *mathematically*, causal? That is, given that there does not appear to be a sufficient conceptual argument for it, how did this view *come about*? Both the history of modern physics (beginning with Galileo and Newton) and the history of quantum theory (before and after quantum mechanics or in the process of its development) have played their roles in the emergence of this view. The nature and effectiveness of classical physics led to powerful philosophical imperatives or—since there are other forces (philosophical and cultural) that have been at work shaping them before the rise of modern physics—reinforced and stimulated such imperatives. Arguably the most powerful of them is the idea and the ideal—"the classical ideal," as Schrödinger, again, called it in his cat-paradox paper—of describing or at least approximating by way of idealized models the independent, and presumably causal, behavior of individual physical systems. This ideal, as Schrödinger also noted on the same occasion (Schrödinger 1935a, *QTM*, p. 152), was radically challenged by quantum theory.

It is not possible to trace this history here. The invention of Schrödinger's wave quantum mechanics, however, and the key philosophical differences between the latter and Heisenberg's matrix quantum mechanics appear to be most responsible for the turn or return to causality in quantum theory. Heisenberg's theory was based on a suspension of the causal view of the independent behavior of quantum objects, as in principle unobservable. By contrast,

[14] I especially have in mind Bell's and related theorems, such as the Kochen–Specker theorem, discussed in Chapter 8. I shall, however, refrain from making definitive claims concerning the situation, given the debate and controversies surrounding them.

Schrödinger's wave mechanics was based on this view, and, initially, the hope was that the intervention of measurement could, at least in principle, be neglected, just as in classical mechanics. In particular, in Heisenberg's initial formulation of quantum mechanics the (continuous) orbital motion of electrons (retained in the old quantum theory along with discontinuous and noncausal quantum jumps from one orbit to another) and the *orbital* frequencies of this motion were seen as, if existent at all, in principle unobservable. Hence, these frequencies were excluded from his new mechanics and its formalism altogether, based on the relationships between the "quantities which in principle are observable" (Heisenberg 1925, *SQM*, p. 263). In Heisenberg's scheme there were no mathematical variables corresponding to these frequencies, unlike in the full-fledged matrix mechanics as developed by Born and Jordan, Dirac's q-number formalism, and Schrödinger's scheme. Only the *transition* "frequencies"—corresponding *physically* to quantum jumps and *mathematically*, but *not* physically, to classical frequencies—were retained, along with quantum "amplitudes" corresponding, mathematically, to classical amplitudes in Fourier's representation of classical motion.

The key physical point was that, instead of relating to physical motion of quantum objects, Heisenberg's new kinematical elements referred to, and properly predicted, the probabilities of discontinuous and—given that we can only estimate their probabilities—noncausal transitions (quantum jumps) from one quantum energy level to another. Accordingly, as discussed in detail in Chapter 3, in Heisenberg's scheme one does not deal with the evolution of a quantum object, such as an electron in the atom, either physically or mathematically. There is no transformation of the state under the action of the energy operator, and hence no physical evolution, however related to this transformation. There are only stationary states of an electron at certain energy levels, each numerically determinable by a measurement, and the probabilities, calculated from the matrix formalism, of the discontinuous transitions from a given state to other possible states, the transitions physically manifested in spectra. It is true that—given that it is mathematically equivalent to Schrödinger's scheme or Dirac's q-number scheme—matrix mechanics can be adjusted to incorporate, *mathematically*, the general transformation of the state under the action of the energy operator. It is also true, however, that one can use the transformation theory, along with Born's probabilistic interpretation of the wave function as part of the theory, to reinterpret Schrödinger's scheme along more Heisenbergian lines of dealing only with the probabilities of measurement outcomes, without referring to any independent behavior of quantum objects. Thus, Born—who, again, never accepted the view that the independent behavior of quantum systems is causal—returned quantum mechanics in its Schrödingerian form to its Heisenbergian epistemological roots.

My main point is that this type of mathematical machinery was not part of Heisenberg's initial thinking, since his mathematics only reflected, in terms of probabilistic predictions, physically discontinuous and acausal transitions ("jumps") from one state to another, from one set of quantum numbers to another. In other words, Heisenberg's mathematics, including its matrix and hence noncommutative nature, came from what can actually be observed and not from what happens between observations. It did not reflect any conjectural thinking concerning what happens at the *unobservable* quantum level—how electrons actually, mechanically, "behave," especially while in stationary states. Nor was Heisenberg trying or aiming to integrate (as Dirac was a bit later) stationary states into the overall scheme in quantum mechanical mathematical terms, to find quantum mechanical mathematical analogues for the corresponding classical quantities. Hence the profound physical difference not only from Schrödinger's original approach but also from the kind of thinking, influenced by Schrödinger, concerning quantum causality is in question here.

Schrödinger's wave scheme, then—especially once the time-dependent Schrödinger's equation was introduced—and, around the same time, Dirac's q-number scheme allowed one also to integrate the orbital (rather than only the transition) frequencies, that is, the quantum analogues of orbital frequencies, into their quantum scheme. On the one hand, this was a major theoretical advance, amplified by Born's probabilistic interpretation of the wave function, which generalized Heisenberg's rules for the probabilities of transitions of electrons between the energy levels. Dirac's and Jordan's transformation theory allowed one to combine both with a single scheme. On the other hand, this mathematical representation and the transformation theory were also conducive to the view that the independent physical behavior of quantum systems, as reflected mathematically in the transformation of a state vector, within the formalism, is causal.

This shift is, again, particularly interesting in the case of Bohr and Heisenberg, given their sharply different previous positions, defined by the matrix version of quantum mechanics. Heisenberg's case is especially revealing in this respect because, as discussed earlier, Heisenberg's paper introducing the uncertainty relations was also intended as a critique of Schrödinger's approach and of applying the idea of causality to the behavior of quantum objects, whether disturbed by observation or not. It also strongly defended matrix mechanics against Schrödinger's attack on it on account of its abstractness and lack of visualization [*Anschaulichkeit*], including, one might add, a causal visualization. Heisenberg wrote, "Schrödinger describes [matrix] quantum mechanics as a formal theory of frightening, indeed repulsive, abstractness and lack of visualizability. Certainty one cannot overestimate the mathematical (and in that sense also [intuitive]) mastery of the quantum-mechanical law that Schrödinger's theory has made possible. However, as regards questions of physical interpretation and principle, the popular view of wave mechanics, as I see it, has actually deflected us from exactly those roads which were pointed out by the papers of Einstein and de Broglie, on the one hand and by the papers of Bohr and by [matrix] quantum mechanics on the other hand" (Heisenberg 1927, *QTM*, p. 82, n.). As its title "*Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik* [On the intuitable content of quantum-theoretical kinematics and mechanics]" stated, Heisenberg's paper offered also an argument for a different form of visualization possible in quantum mechanics (in whatever form), which was limited by the uncertainty relations, and, correlatively, physically noncausal. In that respect, as I explained, his position was different from those of Bohr (in the Como lecture), Dirac, and von Neumann, and closer to that of Born.

And yet, Heisenberg's paper also appealed to the idea of "disturbing" the *independent* behavior of quantum objects by an act of observation in his γ-ray-microscope thought experiment. As I said, Born, too, appears to think that quantum behavior could somehow be described in (noncausal) spatio-temporal terms. While Bohr, famously, corrected some of Heisenberg's argumentation even before the paper was published, he at this point did not critique the ingredients under discussion here and indeed adopted them in his own discussion of the experiment in the Como lecture. As I said, as he moved toward his nonclassical view, Bohr rejected the use of the term "disturbance" as implying that there is some in principle describable or even conceivable (let alone causal) independent quantum behavior, which is then disturbed by observation. He spoke instead of our *interfering* with quantum objects and thus affecting their subsequent behavior—and, hence, our probabilistic predictions concerning future experiments—or changing our expectations concerning the outcome of these experiments, as a Bayesian quantum theorist would have it. Again, nothing could any longer be said about the independent behavior of quantum objects themselves. That one cannot under these conditions speak of any quantum-level causality, again, follows automatically. Bohr, I argue here, moves to this type of view (it took, again, a while to crystallize it) shortly after advancing his Como argument under the impact of his exchanges with Einstein in 1927. This move is clearly manifested in the first article on the subject published, in 1929, after the Como lecture, "The Quantum of Action and the Description of Nature," to be discussed in the next chapter. At least for Bohr himself, the history of the claim of causality in quantum theory was finished and a new history was about to begin—that of quantum theory in which any description or conception (let alone a causal one) of quantum objects and their behavior was rigorously prohibited, "*in principle* excluded" (Bohr 1949, *PWNB* 2, p. 62).

Chapter 7
From Como to Copenhagen: Renunciations

Abstract This chapter is a discussion of a new approach to complementarity, developed by Bohr shortly after the Como lecture. This approach was marked by Bohr's rethinking of the question of causality in quantum theory following his initial exchanges on quantum mechanics with Einstein and was based on a new form of complementarity in the narrow sense, between the space–time description and the laws of conservation of energy and momentum. While this change was, in part, a return to Bohr's 1925 view, it also initiated the way of thinking that ultimately led Bohr to his nonclassical version of complementarity.

The Como version of complementarity, discussed in the preceding chapter, proved to be short-lived. One might say that it died virtually on Bohr's train ride from Como to Copenhagen. While this is, obviously, a factual exaggeration, especially since Bohr spent some time in reworking the lecture for publication, it symbolizes a quick transition from the spirit of Como to the spirit of Copenhagen guiding Bohr's thought concerning quantum phenomena and quantum mechanics. I, again, distinguish "the *spirit* of Copenhagen" (Heisenberg's phrase) from *the* Copenhagen interpretation, which does not exist as a single interpretation, even in Bohr's own case. In any event, within a year or so, even before the Como lecture was published, Bohr was on his way to an essentially different approach, partly shaped by his initial exchanges on quantum mechanics with Einstein in 1927 (Bohr 1949, *PWNB* 2, pp. 47–52). The approach was developed in several works completed shortly after the Como lecture, most especially in the 1929 article "The Quantum of Action and the Description of Nature." It was Bohr's first published work on complementarity after his exchanges with Einstein just mentioned and it was clearly shaped by them, as Bohr indeed notes (Bohr 1949, *PWNB* 2, p. 52). Originally written for the issue of *Die Naturwissenschaften* in celebration of the 50th doctoral anniversary of Max Planck, the article not only represents a key step in the development in Bohr's thought, including as concerns the role of probability in quantum theory, but also offers a new version of complementarity in the broad sense of the interpretation of quantum phenomena and quantum mechanics.

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This version is based on a new form of complementarity in the narrow sense, "between the space–time description and the laws of conservation of energy and momentum" (Bohr 1929a, *PWNB* 1, p. 94). This complementarity is not found in the Como lecture, which, as discussed in Chapter 6, was based on the complementarity of the space–time coordination and the claim of causality, which is problematic, as Bohr came to realize sometime in 1927–1928. This contention is supported by the fact that "The Quantum of Action and the Description of Nature" abandons this complementarity and, with it, the idea of the causal independent behavior of quantum objects. Bohr's new complementarity is also that of different experimental arrangements, where the corresponding measurements and applications of physical laws become mutually exclusive, in accordance with, and as an interpretation of, Heisenberg's uncertainty relations. This particular complementarity is also essentially linked to the irreducibly probabilistic character, established *on the experimental grounds*, of our predictions concerning quantum phenomena, even and in particular those related to individual quantum events. By the same token, quantum processes themselves are now seen as noncausal in all circumstances and are ultimately placed beyond any possible description or even conception, although this more radical view was, again, developed by Bohr only by the late 1930s. In any event, causality no longer applies to our understanding of quantum processes, and, hence, it cannot be part of any given complementarity, as against the conservation laws, which can be fully maintained by quantum theory apart from causality, to which they are linked in classical physics.

The version of complementarity in the broad sense offered in "The Quantum of Action and the Description of Nature" may be seen as an intermediate or transitional one between the Como version and Bohr's ultimate, nonclassical version. This version is, however, much closer to the latter than it is to the Como version, although it still contains some problematic remnants or echoes of the Como argument, including as concerns causality. In particular, Bohr says there that "any attempt at ordering in space–time leads to a break in the causal chain" (Bohr 1929a, *PWNB* 1, p. 98). This statement echoes the Como argument for the complementarity of the space–time coordination and the claim of causality. Bohr's surrounding elaboration, however, appears to leave space for reading the statement in a conditional sense, that is, to the effect that such would be the case if one assumes that the classical view holds in quantum mechanics (Bohr 1929a, *PWNB* 1, pp. 97–98). I shall not insist on either reading, because it is possible that Bohr's Como ideas were still affecting his thinking. In any event, it is hardly in doubt that the article essentially departs from the Como argument and sets Bohr on his way to his nonclassical thinking. Accordingly, while lesser known and rarely cited, it might be seen as one of the crucial works in Bohr's oeuvre, next in its significance perhaps only to his reply to EPR.

Bohr briefly changed his terminology from "complementarity" to "reciprocity" in the article, perhaps in view of the problems of the Como lecture and the kind of response it received in the quantum physics community. Apart from those close to Bohr, such as Heisenberg and Pauli, and a few others, such as

Paul Ehrenfest, the reception of Bohr's article was relatively cool. The lecture did not appear to most, including those who responded favorably to Bohr's epistemological argument, to introduce a new physics. This, as I argued in Chapter 6, is not true, although it may depend on how one understands physics. Nor did the article offer new mathematical tools that would help one to deal with the still outstanding problems of quantum mechanics or a new exposition or a new version of the mathematical formalism of quantum mechanics. This assessment was correct, although Bohr of course did not aim at doing either. The difficulty and, as he himself later called it (referring to his reply to EPR), "inefficiency" of Bohr's exposition, including as against the extraordinary efficiency of mathematics, was a factor as well (Bohr 1949, *PWMB* 2, p. 61). The deeper philosophical implications of his argument took a while to absorb and required subsequent interventions by Bohr before they took hold. Eventually they did, although they were often accepted dogmatically or uncritically, rather than properly thought through. The new physics of Bohr's argument was difficult to appreciate or even to perceive apart from the epistemological and philosophical considerations. Many, on both sides (pro and contra Bohr's argument), were also troubled by the radical epistemological implications of the argument. These worries have never subsided, while Bohr's epistemology only became more radical as his thinking concerning quantum physics and complementarity developed.

The term "reciprocity" appears to be in part derived by Bohr from the uncertainty relations, which entail "reciprocal" (mutually influencing) relations between the quantities involved. Bohr used the term earlier, shortly before Heisenberg's introduction of quantum mechanics, in considering certain features of the old quantum theory that were related to the problems that Heisenberg's new mechanics were to solve in his article on collisions (Bohr 1925a). The meaning of the term was different, however, or even opposite. What he saw, at the time, as reciprocal processes were the processes analogous to those considered in classical physics, while he saw atomic processes, such as those of collisions, that appeared to defy classical analogies as "irreciprocal." A proper quantum mechanics would be able to treat both kinds of processes within the same scheme.[1] Bohr quickly returned to the language of complementarity, which is more precise, especially once he refined his definition. His important, although never published, lecture of 1931, "Space–Time Continuity and Atomic Physics" (Bohr 1972–1996, vol. 6, pp. 361–370), given at the University of Bristol, uses the language of "complementarity" and further sharpens Bohr's argument.

The argument developed in these articles also gave Bohr a platform for his response to EPR. In referring to his earlier publications in his reply to EPR,

[1] This earlier article by Bohr was, accordingly, indicative of the insurmountable difficulties of the old quantum theory. It is also worth reiterating the significance of the analysis of collision and dispersion phenomena for the development of quantum mechanics, in particular in Heisenberg's discovery of quantum mechanics and Born's interpretation of the wave function.

Bohr says, "I shall therefore be glad to use this opportunity to explain in somewhat greater detail a general viewpoint, conveniently termed 'complementarity,' which I have indicated on various previous occasions" (Bohr 1935b, p. 696). While this is not altogether wrong, it is not quite accurate either. In particular, this statement, accompanied by a reference to Bohr's 1934 collection, *Atomic Theory and the Description of Nature* (*PWNB* 1), cannot, I would argue (for the reasons explained in Chapter 6), apply to the Como lecture. It can, however, *nearly* apply to "The Quantum of Action and the Description of Nature," contained in that volume, or to Bohr's "Introductory Survey" (Bohr 1929b) there (and in the final article included in the volume that also dates from 1929). His reply to EPR does, however, introduce new refinements, eventually leading Bohr to a nonclassical epistemology. Most crucial of these changes is, again, that a relatively *arbitrary renunciation* of a causal and, ultimately, any description of atomic processes is to be replaced with an *unavoidable renunciation*, that is, by the view that such a description is rigorously precluded. Bohr uses the phrase "unavoidable renunciation" in the Warsaw lecture (Bohr 1938, *PWNB* 4, p. 105), where he introduces his concept of phenomenon, and with it his nonclassical interpretation of quantum phenomena and quantum mechanics.

The shift and, to some degree, return to the position Bohr took in response to Heisenberg's discovery of quantum mechanics in "Atomic Theory and Mechanics" (1925), also included in the volume, are confirmed by Bohr's "Introductory Survey" (Bohr 1929b). The latter adopts this post-Como approach and partly recasts the Como lecture itself in these new terms by stressing those aspects of the Como argument that were retained in his new approach, and, notably, without ever mentioning the complementarity of the space–time coordination and the claim of causality. The survey invokes instead our being forced "step by step to forego a causal description of the behavior of individual atoms in space and time" and "the renunciation of causality in the quantum-mechanical description" (Bohr 1929b, *PWNB* 1, p. 4). Accordingly, the Como argument may be best seen as only one of these steps.

It is worth considering the setup and tracing the main gradient of "The Quantum of Action and the Description of Nature," as against the Como lecture, to better understand and appreciate the meaning and significance of Bohr's new approach. First, the language of "the quantum postulate" is supplemented by that of "the elementary quantum of action," to be used from now on throughout Bohr's work (Bohr 1929a, *PWNB* 1, p. 92). In his "Introductory Survey," Bohr notes, refining his argumentation and language as against the Como lecture

> The application of [mechanics and the electromagnetic theory] to atomic problems was destined to reveal a hitherto unnoticed limitation that found its expression in Planck's discovery of the so-called quantum of action, which imposes upon individual atomic processes an element of discontinuity quite foreign to the fundamental principles of classical physics, according to which all actions may vary in a continuous manner. The

quantum of action has become increasingly indispensable in the ordering of our experimental knowledge of the properties of atoms. (Bohr 1929b, *PWNB* 1, p. 4)

From the outset, Bohr (this view continues the Como argument) defines quantum mechanics as a "symbolic" theory, with which the history of quantum theory "has reached a temporary climax." The climax is seen as temporary because of the remaining problems of interpretation and because of quantum electrodynamics, which was introduced by that time primarily in the work of Dirac and posed new problems of its own. For the moment, although radically different from classical mechanics in its key physical, mathematical, and epistemological features, quantum mechanics is argued to be "a natural generalization of the classical mechanics with which in beauty and self-consistency it may well be compared" (Bohr 1929a, *PWNB* 1, p. 92). Bohr qualifies his advocacy of the new theory in an important elaboration. It places quantum theory within the history of our attempts (such as in the kinetic theory of gases), "to accomplish [a causal space–time] description also in the case of phenomena, which, in our immediate sense impressions, do not appear as motions of material bodies," in contrast to those considered in classical mechanics (Bohr 1929a, *PWNB* 1, pp. 92–93). Bohr says

This goal [of developing quantum mechanics] has not been attained, still, without a *renunciation* of the causal space–time mode of description that characterizes the classical physical theories which have experienced such a profound clarification through the theory of relativity. In this respect, the quantum theory may be said to be a disappointment, for the atomic theory arose just from the attempt to accomplish such a description also in the case of phenomena which, in our immediate sense impressions, do not appear as motions of material bodies. From the very beginning, however, one was not unprepared in this domain to come upon a failure of the forms of perception adapted to our ordinary sense impressions. We know now, it is true, that the often expressed skepticism with regard to the reality of atoms was exaggerated; for, indeed, the wonderful development of the art of experimentation has enabled us to study the effects of individual atoms. Nevertheless, the very recognition of the limited divisibility of physical processes, symbolized by the quantum of action, has justified the old doubt as to the range of our ordinary forms of perception when applied to atomic phenomena. Since, in the observation of these phenomena, we cannot neglect the interaction between the object and the instrument of observation, the question of the possibilities of observation again comes to the foreground. Thus, we meet here, in a new light, the problem of the objectivity of phenomena which has always attracted so much attention in philosophical discussion. (Bohr 1929a, *PWNB* 1, pp. 92–93; emphasis added)

The contrast with the Como argument is striking. The complementarity of the space–time coordination and the claim of causality is gone, along with causality itself. What replaces them as the grounding feature of quantum epistemology is the crucial *distinction*, which is to shape Bohr's argumentation from this point on, between the unobservable quantum *objects* and observable *phenomena*, defined through the *effects* of the interactions in question upon the measuring instruments. It is worth noting that, under these conditions, it is still in principle possible to speak of certain properties of quantum objects, unobservable but ascertainable through these effects, which view Bohr, again, eventually abandons. One can especially note the language of *effects*, which was in turn to become decisive for Bohr's later works. If "the wonderful development of the art of experimentation has enabled us to study the effects of individual atoms," it has also limited us to the observation or descriptive study of these effects (predicted by means of the quantum theory) and only to them. As quantum objects, atoms themselves cannot be studied or even rigorously described or, in Bohr's ultimate view, conceived of as quantum objects. Nor for that matter can be any entity, considered as a quantum object, which, again, can be macroscopic in scale (in the sense that some of its macroscopic aspects require quantum theory to be accounted for), although their quantum nature is defined by their ultimate microscopic constitution. This nature may be more pointedly manifest via certain effects, as in the case of Josephson's junctures, or not, as in the case of more conventional macro-objects, from everyday objects to planets to stars to galaxies to the universe itself. All these entities, beginning with atoms (whatever they are), do exist and exist as quantum objects.[2] They can, in this view, only be *observed*, however, as classical objects, while, as quantum objects, they remain unobservable and indescribable, or even inconceivable. Their quantum nature could only manifest itself in certain effects they produce in measuring instruments.

The parallel with relativity may be linked to this more radical epistemology, although, as in the Como lecture, it serves primarily to argue that classical physics may be seen as a refinement of everyday experience and language, "the forms of perception adapted to our ordinary sense impressions." These forms, however, are no longer applicable to physical objects conforming to either relativistic or quantum regimes. The argument is amplified in Bohr's commentary, cited earlier, introducing the article in "Introductory Survey," where he argues "that *all* our ordinary verbal expressions bear the stamp of our customary forms of perception, from the point of view of which the existence of the quantum of action is an irrationality" (Bohr 1929b, *PWNB* 1, p. 19). As we have seen, the point was also made by Heisenberg in the Chicago lectures (Heisenberg 1930, p. 11). "Discussion with Einstein" gives Bohr's arguably most refined expression of this idea: "The peculiar individuality of the quantum effects presents us, as regards the comprehension of well-defined evidence, with a novel situation unforeseen in classical physics and irreconcilable with conventional ideas suited for our orientation and adjustment to ordinary experience [on which classical physics is based]. It is in this respect that quantum theory has called for a renewed revision of the foundation for the unambiguous use of elementary concepts as a further step in the development which, since the advent of relativity theory, has been so characteristic of modern science" (Bohr 1949, *PWNB* 2, p. 62).

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[2] As noted earlier, there are arguments, such as that by Leggett for "microrealism" (Leggett 1988), that question to what degree macro-objects could in fact be described by quantum mechanics.

In "The Quantum of Action and the Description of Nature," this point, and most other points just discussed, is linked to Heisenberg's uncertainty relations, the main quantitative law of quantum mechanics (Bohr 1929a, *PWNB* 1, p. 95). The final sentence of Bohr's passage in question at the moment ("Thus, we meet here, in a new light, the problem of the objectivity of phenomena which has always attracted so much attention in philosophical discussion") refers, among other things, to Kant, but also indicates a possible move to a more radical argument. This argument concerns in particular placing quantum objects and processes even beyond the limits of Kant's things-in-themselves, both perceptually or conceptually or experimentally–technologically (the technologies of our bodies included). In other words, the statement moves from a Kantian epistemology of things-in-themselves as unknowable but thinkable (and, at least in principle, given some specifiable ontology) to nonclassical epistemology as the epistemology of the unthinkable. Bohr retains objectivity at the level of effects of the interactions between quantum objects and measuring instruments upon the latter and eventually at the level phenomena in his sense, which, as he was to note later, ensures the possibility of "unambiguous communication" (Bohr 1954, *PWNB* 2, p. 67). This argument, developed throughout his later writings, also secures the disciplinary continuity between classical and quantum physics as mathematical experimental sciences of nature (Bohr 1954, *PWNB* 2, pp. 67–69; Bohr 1935b, p. 700).

For the moment, these considerations lead Bohr to, correlatively, both the irreducibly probabilistic nature of quantum mechanics and the irreducibility of certain complementary features (and both are in turn correlative to the uncertainty relations), but now, as against the Como lecture, defined in terms of certain experimentally defined phenomena. To address Bohr's reconfiguring of complementarity, first, according to Bohr (he, again, replaces "complementarity" with "reciprocity")

> [W]e know now that for material particles as well as for light different conceptual pictures are necessary to account completely for the phenomena and to furnish a unique formulation of the statistical laws which govern the data of observation. The more clearly it appears that a uniform formulation of the quantum theory in classical terms is impossible, the more we admire Planck's happy intuition in coining the term "quantum of action" which directly indicates a renunciation of the [continuous] action principle, the central position of which in the classical description of nature he himself has emphasized on more than one occasion. This [Planck's] principle symbolizes, as it were, the peculiar reciprocal symmetry [complementary] relation between the space–time description and the laws of the conservation of energy and momentum, the great fruitfulness of which, already in classical physics, depends upon the fact that one may extensively apply them without following the course of the phenomena [of the processes resulting in the appearance of phenomena?] in space and time. It is this very reciprocity [complementarity] which has been made use of in a most pregnant way in the quantum-mechanical formalism. As a matter of fact, the quantum of action [*h*] appears here only in relations in which space–time co-ordinates and momentum–energy components, which are canonically conjugate quantities in the Hamiltonian sense, enter in a symmetrical and reciprocal manner [linked to Heisenberg's uncertainty relations]. In addition, the analogy between optics and mechanics, which has proved to be so fruitful for the

recent development of the quantum theory, depends intimately upon this reciprocity. (Bohr 1929a, *PWNB* 1, p. 94)

I shall return to the uncertainty relations, considered by Bohr in his next paragraph, presently. I have discussed the analogy between optics and mechanics earlier in considering Schrödinger's work. Bohr, too, primarily refers to Schrödinger, along with de Broglie, as he also does in the Como lecture and elsewhere in his works, as in his "Introductory Survey" (Bohr 1929b, *PNWB* 1, p. 9). The main point is the shift to a new form of complementary configurations, those defined by kinematical variables, on the one hand, and conservation laws, on the other hand, as against the complementarity of the space–time coordination and causality, dominant in the Como lecture. This shift is accompanied by a more Heisenbergian epistemology, with a further radicalization yet to come in the wake of the EPR argument. Until then, including in the article under discussion, a reference to the properties of quantum objects is still seen as possible, under the constraints of the uncertainty relations. Still, Bohr makes a decisive move with his argument in the article.

This argument is developed in his Bristol lecture, "Space–Time Continuity and Atomic Physics," helpfully accompanied by Bohr's return to the language of complementarity. As Bohr says there, "We have thus either space–time description or description where we can use the laws of conservation of energy and momentum. [These descriptions] are *complementary* to each other" (Bohr 1931, Bohr 1972–1996, vol. 6, p. 369). That is, as his discussion in the lecture makes clear, both types of description are necessary for a comprehensive account of the quantum mechanical situation. By presenting its argument via the double-slit experiment, the lecture also follows a more physical approach, rather than as in "The Quantum of Action and the Description of Nature," which makes a more general philosophical argument. In this respect it is closer to and anticipates Bohr's reply to EPR, although this proximity is in part due to Bohr's earlier discussion with Einstein, as is apparent from comparing the lecture to Bohr's account of the events in his "Discussion with Einstein." Most of Einstein's criticisms of quantum mechanics have been shaped by the type of thinking that led him to his argument of the EPR type. This proximity between Bohr's argument at this stage and that in his reply to EPR is defined in particular by the emphasis on the mutual exclusivity of the experimental arrangements involved, which now becomes a crucial part of Bohr's interpretation and is a reinterpretation of Heisenberg's uncertainty relations, to which he moves next. Before I consider this part of Bohr's argument, however, I would like to stress a few other key points transpiring in the passages cited thus far.

First, in reading these passages one also notes a subtle but important use of the history of quantum theory, from Planck's discovery on, and of thinking through the gradual road to the realization of the impossibility of "a uniform formulation of the quantum theory in the classical form." It may also be noted that while Bohr clearly has in mind a broader sense of self-consistency and harmony, especially as concerns the mathematical formalism of the theory and its relation to experiment, "uniform" may also be here juxtaposed to complementary. The question of, first, the great difficulties and, then, the impossibility of applying classical-like formulations and especially of classical-like visualizable [*anschaulich*] mechanical pictures in quantum physics and the protracted history of dealing with this situation are central themes of the volume, from "Atomic Theory and Mechanics" (1925) on, and in part unite it. As we have seen, the point led to Heisenberg's discovery of his matrix mechanics, initially in contrast to Schrödinger's path to his wave mechanics. Equally important is Bohr's point that "the great fruitfulness of [the laws of conservation of energy and momentum], already in classical physics, depends upon the fact that one may extensively apply them without following the course of the phenomena in space and time." In accordance with Bohr's ultimate view, while retaining this formulation, one could further speak of the processes that may, in the event of a performed measurement, lead to the appearance of the phenomena in question.

This view of the conservation law suggests the main complementary relation in question and the uncertainty relations almost by itself, that is, once one is in possession of matrix mechanics, as developed by Heisenberg himself, Jordan, Born, and Dirac, whereby Planck's quantum of action, h, appears in the commutator

$$PQ - QP = \frac{h}{2}\pi i.$$

Here, however, I primarily have in mind the following fact, discussed in Chapter 3 in the context of Heisenberg's discovery of quantum mechanics, to which it was crucial. Throughout the history of the quantum theory leading to this discovery, one would persistently encounter the circumstance that certain key quantum mechanical relations, such as Kramers's and, then, Heisenberg and Kramers's dispersion formulas, did not depend on the use of mechanical pictures (such as those of electrons orbiting the nuclei of the atoms). In particular, on the one hand, certain mathematically formulated concepts and laws, such as the concepts of momentum and energy and the laws of their conservation, could be carried over from classical physics directly. On the other hand, the mathematical formulation of these and other concepts (such as amplitudes of oscillations) needs to be given a quantum theoretical reinterpretation, and in the case of the concepts in question a new physical content.

The situation gave a new complexity to Bohr's correspondence principle as "express[ing] our endeavours to utilize all the classical concepts by giving them a suitable quantum-theoretical re-interpretation" (Bohr 1929b, *PWNB* 1, p. 8). The term reinterpretation is also used by Bohr, including on the same occasion (Bohr 1929b, *PWNB* 1, p. 18), and, as we have seen, by Heisenberg in his work on matrix mechanics. This term may, however, not be fitting here, since these mathematical concepts, such as matrix elements (or operators in a Hilbert space), do not have any interpretation as representing physical objects or phenomena, including those that are observed in measuring instruments, which are represented by classical physics. As Bohr says, in speaking of one of the most crucial quantum mechanical terms, "the term 'probability amplitude' for the amplitude function of the matter waves is part of a mode of expression which, although often convenient, can, nevertheless, make no claim to possessing general validity" (Bohr 1929b, *PWNB* 1, p. 17). Its validity is only specific to quantum mechanics. On the other hand, once we neglect the quantum of action, as in the case of large quantum numbers for electrons in atoms, these formulas convert themselves—or rather (the procedure is not mathematical or physical, since the processes in question are still quantum) can be replaced by the classical formalism. In other words, the application of the correspondence principle, which was so important to Heisenberg, involves a complex reciprocal, two- or multi-directional, traffic between the quantum and the classical descriptions, the mathematical and the physical concepts (those used in classical physics and those used in quantum physics), or the impossibility of physical concepts, and so forth. In the process, as discussed in Chapter 3, the principle itself was developed and sharpened throughout Bohr's and Heisenberg's work, from using it in the context of mechanical models of atomic behavior (such as electrons' orbits in the old quantum theory) to quantum mechanics. There it takes what I called its "mathematical" form, defined by formally adopting the equations of classical physics in quantum mechanics, but changing the variables to which these equations apply ("new kinematics"). This use of the correspondence principle is correlative, at least in Heisenberg's work, to a renunciation of all mechanical models as applicable to the (independent) behavior of quantum objects.

In "The Quantum of Action and the Description of Nature," Bohr moves next to the role of the classical (physical) concepts and the uncertainty relations, now as immediately correlative to the complementarity of the space–time description and dynamical conservation laws. He notes first, "It lies in the nature of physical observation, nevertheless, that all experience must ultimately be expressed in terms of classical concepts, neglecting the quantum of action. It is, therefore, an inevitable consequence of the limited applicability of the classical concepts that the results attainable by any measurement of atomic quantities are subject to an inherent limitation" (Bohr 1929a, *PWNB* 1, pp. 94–95). This is an important formulation, especially given that Bohr's appeal to the significance of classical concepts is often misunderstood. I have commented on the subject earlier and shall further discuss it later, but a few key points should be reiterated in the context of the article under discussion, in order to grasp the import of Bohr's argument more fully.

First, this formulation clearly states the following point, which, as I noted, is often missed by commentators on Bohr. Although indispensable, classical concepts are never sufficient for a proper quantum mechanical account, as is shown by Heisenberg's uncertainty relations to which Bohr proceeds from this point in the article itself. Second and by the same token (this point is also usually missed by commentators), in Bohr's ultimate nonclassical view, quantum objects or their quantum interactions with measuring instruments are never subject to description in terms of classical physical or any other concepts.[3] Bohr's earlier view, including in this article, again, allows an attribution of certain properties to quantum objects at the time of measurements (under the constraints of the uncertainty relations) but not independently. In his ultimate view, any such description can only apply to these measuring instruments, onto which the physical application of the uncertainty relations is now transferred, or, again, to the classical parts of the instruments, since these instruments are seen as having a quantum stratum through which they interact with quantum objects. This interaction is irreversibly amplified to the classical level, say, as manifest by a spot left on a silver screen (e.g., Bohr 1954, *PWNB* 2, p. 73). Finally and, again, correlatively, classical physics is seen as a refinement of our everyday perception and thinking, which, effective as they are in classical physics or, to a more limited extent, in relativity, may not be suitable for the quantum scale of nature, no matter how far this refinement may reach in terms of physics. Mathematics is a different story, even though, ultimately, the development of mathematics may also be seen in terms of such a refinement of our thought (e.g., Bohr 1954, *PWNB* 2, p. 68). From this viewpoint, the significance and limitation of classical concepts appear in a new light. While by referring in the passage in question to "classical concepts" Bohr clearly also means classical physical concepts, the fact that he omits the term "physical" may not be accidental. For, as I discussed earlier, all concepts we can form, all our perception and thinking, are classical from this viewpoint, and quantum objects and processes are nonclassical in the sense of being inaccessible to our perception and thinking. According to Bohr, the uncertainty relations epistemologically reflect this situation. As he says

> A profound clarification of this question was recently accomplished with the help of the general quantum-mechanical law, formulated by Heisenberg, according to which the product of the mean errors with which two canonically conjugate mechanical quantities may be simultaneously measured can never be smaller than the quantum of action. Heisenberg has rightly compared the significance of this law of reciprocal uncertainty for estimating the self-consistency of quantum mechanics with the significance of the impossibility of transmitting signals with a velocity greater than that of light for testing the self-consistency of the theory of relativity. In considering the well-known paradoxes which are encountered in the application of the quantum theory to atomic structure, it is essential to remember, in this connection, that the properties of atoms are always obtained by observing their reactions under collisions or under the influence of radiation, and that the above-mentioned limitation on the possibilities of measurement is directly related to the apparent contradictions which have been revealed in the discussion of the [quantum] nature of light and of material particles. (Bohr 1929, *PWNB* 1, p. 95)

[3] It is worth keeping in mind that, in the present view, the term "quantum objects" applied to the objects of quantum mechanics (cum a nonclassical interpretation of it), rather than to the ultimate constituents of nature, which quantum objects idealize as irreducibly inaccessible.

These contradictions are, however, only apparent—or, more accurately, quantum mechanics allows us to offer a non-contradictory theory of the data that this situation defines, even though, if one speaks of it as a *physical* theory, this may only be possible if the formalism (cum quantum phenomena themselves) is accompanied by a suitable interpretation. Of course, in practice one can ignore the physical contradictions in question and work with the mathematical formalism to get correct predictions of the experimental results. To some degree, most physicists have often taken this approach, which should not be seen only in negative terms. For, as I argue here, as only a predictive rather than descriptive theory, quantum mechanics also radically changes the relationships between mathematics and physics and does not require a classical-like descriptive justification of the mathematical formalism it uses.

Bohr's argumentation, indicated in this passage, takes significant steps, especially as against the Como argument, in resolving the contradictions in question, although it does not quite reach the level of his post-EPR works. As indicated in Chapter 6, Heisenberg's famous γ-ray-microscope thought experiment, alluded to here, is part of the problem, even though Bohr had straightened out some of the problems of Heisenberg's analysis of this experiment earlier. Heisenberg's thought experiment is helpful insofar as it reflects the fact that the role of the agencies of observation cannot be neglected or compensated for and, thus, *irreducibly* shape any observable phenomena in quantum physics, and that, while, as observed phenomena, all such phenomena are *classical*, they are the effects of the *quantum* interaction between quantum objects and measuring instruments. As discussed earlier, however, the experiment may also be misleading insofar as it suggests that one could speak, on the classical model, of the "undisturbed" independent behavior of quantum objects, especially as described by the formalism of quantum mechanics, before these objects are "disturbed" by an experiment. These implications of Heisenberg's argument troubled Bohr all along, even though he retained for a while the language of "disturbance," which, as we have seen, he qualified even in his earlier works and eventually abandoned.

Bohr inches toward his later views by noting that any observation in quantum theory is essentially linked to a *reaction* of quantum objects upon other quantum objects, eventually to be seen by Bohr as the quantum aspects of measuring instruments. This transition is also suggested by the following statement, building on the passage just cited and alluding to and refining a similar point in the Como lecture. He says, "At the conclusion of [the Como lecture], it was pointed out that a close connection exists between the failure of our forms of perception, which is founded on the impossibility of a strict separation of phenomena and means of observation, and the general limits of man's capacity to create concepts, which have their roots in our differentiation between subject and object" (Bohr 1929a, *PWNB* 1, pp. 95–96). Along with and in part through our classical perceptual and conceptual limitations, the wholeness or indivisibility of phenomena in Bohr's later sense is clearly intimated here, and, as we have seen, even in the Como argument itself. On the other hand, the statement that "the properties of atoms are always obtained by observing their reactions under collisions or under the influence of radiation" stops short of his post-EPR view of the situation. It suggests that the quantum mechanical measurement or even formalism still refers to the properties of quantum objects themselves at the time of measurement, as opposed to the effects ("irreversibly amplified" to the classical level) of their interactions with measuring instruments upon those instruments.

Thus, Bohr's path toward a more rigorous and deeper understanding of quantum phenomena and quantum mechanics and toward a more lucid exposition of his view was, perhaps unavoidably, difficult. Part of the difficulty, as Bohr noted in "Introductory Survey" and on many other occasions, is our customary, classical forms of perception and expression. This language and thought are especially difficult to negotiate when we respond to the classically shaped arguments of others, such as Einstein. It is also true that Bohr cannot always completely control his texts. Nobody can do it completely. These difficulties require literally a lifetime effort, building upon, refining, and correcting the previous argumentation, where criticism, such as that of Einstein, becomes especially helpful.

That said, however, I do think that Bohr, with Heisenberg's help, was much closer to the nonclassical view of his post-EPR works in his immediate response to Heisenberg's discovery of quantum mechanics and to matrix mechanics itself in "Atomic Theory and Mechanics," or even in his still earlier thinking following the collapse of the BKS proposal. Then, by the time of his invention of complementarity, Schrödinger's wave mechanics, Dirac's transformation theory, and Heisenberg's paper on the uncertainty relations "detoured" Bohr. Apart from the fact that some aspects of Bohr's epistemological thinking that emerged with Heisenberg's discovery remained in place in these works as well, the detour was far from unproductive, beginning with Bohr's discovery of complementarity. Nevertheless, Bohr's initial work on complementarity may be argued to contain significant problems, which appear to have been sensed by Bohr, who, as I said, was never happy with the Como lecture. These problems were remedied, first in "The Quantum Postulate and the Description of Nature" and related works in the late 1920s and early 1930s, and then in Bohr's reply to EPR and related works of the late 1930s, which brought his thinking to nonclassical epistemology.

Bohr makes further steps in this direction in "Introductory Survey," including in discussing "The Quantum Postulate and the Description of Nature" there, and still closer in "The Atomic Theory and the Fundamental Principles Underlying the Description of Nature," the final essay of *The Atomic Theory and the Description of Nature*, and "Space–Time Continuity and Atomic Physics" (both works date to 1931). In these works he also returns to the language of complementarity. Although not ultimately that decisive in this respect, the language of reciprocity is not helpful, and Bohr's appeal to it, as "perhaps ... more suitable," exhibits a certain hesitation as well (Bohr 1929a, *PWNB* 1, p. 95). Nevertheless, "The Quantum of Action and the Description of Nature" is a major step in the development of Bohr's thought on complementarity. That his "Introductory Survey" devotes the largest space to it is indicative of its significance as well, and Bohr's partial recasting of its argument in terms of complementarity helps one to see its significance (Bohr 1929b, *PWNB* 1, pp. 15–21). His elaborations concerning the relationships among the quantum mechanical formalism, classical concepts, probability, the uncertainty relations, complementarity, the role of measuring instruments, and the distinction between them and quantum objects are especially significant. They are also often cited, although far less often adequately interpreted, especially the philosophical reflection of the final sentence of the passage to be cited. I shall only cite a part of Bohr's long paragraph:

> [T]hese [quantum] phenomena belong, indeed, to a domain in which it is essential to take into account the quantum of action and where an unambiguous description [of quantum objects and processes] is impossible. ... [O]nly with the help of classical ideas is it possible to ascribe an unambiguous meaning to the results of observation. We shall, therefore, always be concerned with applying probability considerations to the outcome of experiments which may be interpreted in terms of such conceptions. Consequently, the use made of the symbolic expedients will in each individual case depend upon the particular circumstances pertaining to the experimental arrangement. Now, what gives to the quantum-theoretical description its peculiar characteristic is just this, that in order to evade the quantum of action we must use separate [i.e., complementary] experimental arrangements to obtain accurate measurements of the different quantities, the simultaneous knowledge of which would be required for a complete description based upon the classical theories, and, further, that these experimental results cannot be supplemented by repeated measurements. In fact, the indivisibility of the quantum of action demands that, when any individual result of measurement is interpreted in terms of classical conceptions, a certain amount of latitude be allowed in our account of the mutual action between the object and the means of observation. This implies that a subsequent measurement to a certain degree deprives the information given by a previous measurement of its significance for predicting the future course of the phenomena. Obviously, these facts not only set a limit to the *extent* of the information obtainable by measurements, but they also set a limit to the *meaning* which we may attribute to such information. We meet here 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 [i.e., the quantum character of their ultimate constitution] but only to track down, so far as it is possible, relations between the manifold aspects of our experience. (Bohr 1929b, *PWNB* 1, pp. 17–18)

The last sentence might be referring to Galileo who, in order to establish his two "new mathematical sciences of nature" (that of the motion of material bodies and that of their resistance to fracture) abandoned any concern with the inner constitution of material bodies, which would not be possible to mathematize at the time (Galileo 1991; Plotnitsky and Reed 2001). One might of course argue that one could, in principle, go further into "the real essence of phenomena," which physics has done throughout its history, including in quantum theory. In fact, as I argue here, quantum mechanics and Bohr himself in fact do so as well. However, quantum mechanics does appear to establish certain irreducible limitations upon our capacity to reach the ultimate quantum constitution of nature. In this regard, this elaboration is a bridge to Bohr's post-EPR perspective, which will further qualify this argumentation, along the lines just suggested. There are still a few glitches as well. Thus one cannot really speak of "the future *course* of the phenomena," only of the *features*, numerical or other, of possible future phenomena, always individual and discrete, defined by possible future measurements. Quantum mechanics makes Bohr's tasks difficult and his work on it hard, and we are lucky to have him help us, sometimes even in moving against him. The article contains some extraordinary philosophical elaborations, especially as concerns the question of visualization [*Anschaulichkeit*] or, as the case may be, unvisualizability of the quantum processes, as Bohr, again, moves to a more radical epistemology. These elaborations clearly amplify the argument just presented, and some of them were already referred or alluded to and others will be discussed later in this study. I would like instead to conclude this chapter with Bohr's analysis of the probabilistic character of quantum mechanics in the article, which is yet another crucial contribution of it.

The irreducibly probabilistic nature of our predictions concerning quantum phenomena is, again, seen by Bohr as an experimental given, and as arguably the single most important postulate of quantum physics, since the same experimental arrangements, which we can control in the manner of classical physics, may yield "different recordings" of their outcomes (Bohr 1954, *PWNB* 2, p. 73). This fact is, however, now seen or *interpreted* as an unavoidable consequence of the irreducible role of measuring instruments in the formation of quantum phenomena. I invoke an interpretation here, since one can also see this view as an interpretation of the probabilistic nature of quantum mechanical predictions.

Christopher A. Fuchs speaks, in the context of quantum information theory in its Bayesian form, of this type of argument as "a Paulian idea" (Fuchs 2001, 2003). It would be difficult to deny the point a Paulian *flavor*, at least at a certain, later juncture of his thinking on the subject, and Fuchs indeed appears to see it more as implicit or implied in, rather than stated by, Pauli. On the other hand, it would be even more difficult to argue that the argument does not originate in Bohr; and it is, it appears, adopted by Pauli from Bohr, beginning at least in the Como lecture, where it is, again, dominated by the complementarity of space–time coordination and the claim of causality. Indeed, as noted in Chapter 4, in 1925, Pauli *definitively* stated, "I definitely believe that in the fundamental laws of a satisfactory theory the concept [of] 'probability' should not appear" (Letter to Bohr, November 17, 1925, cited in *MR 6*, p. 173). This was to change, as were his other misgivings concerning quantum mechanics. In any event, this "idea" appears in the form just stated in "The Quantum of Action and the Description of Nature."[4] In the passage cited above, Bohr argues that "in the observation of these [quantum-mechanical] phenomena, we cannot neglect the interaction between the object and the instrument of observation," and he links the situation to the Kantian question of the relation between objectivity and subjectivity (a crucial point for Pauli and for Fuchs in the context of quantum information theory). Then Bohr says, giving the case its proper history, "This being the state of affairs, it is not surprising that, in all rational applications of the quantum theory, we have been concerned with essentially statistical problems. Indeed, in the original researches of Planck, it was, above all, the necessity for modifying the classical statistical mechanics which gave rise to the introduction of the quantum of action" (Bohr 1929a, *PWNB* 1, p. 93).

The nature of this modification is, again, one of my main subjects here, and I shall continue to address it throughout the remainder of this study. My main point at the moment is the essential link between the irreducible role of the measuring instruments in forming any given phenomena and the irreducibly probabilistic character of the theory predicting such phenomena, and more specifically, the latter as an unavoidable consequence of the former. The argument is found throughout Bohr's writing but appears to be introduced in this form in this article. This link is no longer supported by the kind of physical explanation and justification that we use in classical physics, or, at the ultimate level, it appears by any other possible explanation and justification we can offer or even imagine. Here and, especially, in Bohr's post-EPR work, the lack of causality is an automatic consequence of his argument to the effect that any description or analysis of quantum objects and processes is no longer possible, in other words, his argument for the nonclassical epistemology of quantum phenomena and quantum mechanics.

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[4] The article has Paulian connections, as does a later article, "On the Notions of Causality and Complementarity" (Bohr 1948, *PWNB* 4, pp. 141–148), in many ways preparatory to "Discussion with Einstein," with which Bohr's nonclassical program is more or less brought to its completion (*PWNB* 2, p. 34). Pauli was involved in Bohr's work on the article under discussion at the moment, and he was an editor of the issue of *Dialectica*, in which the second article was originally published (*Dialectica* n. 2 [1948], pp. 312–319). He especially praised "The Quantum of Action and the Description of Nature" for the philosophical nature of its argumentation, which he thought, rightly, enabled Bohr to say something new, and indeed Bohr did something new there, including, as Pauli perhaps perceived as well, something new concerning the nature of using the mathematics of probability in physics. On Pauli's exchanges with Bohr on this article, see (Bohr 1972–1996, vol. 6, pp. 193–195). Indeed Bohr worked on the statistical aspects of quantum mechanics around that time (Bohr 1972–1996, vol. 6, pp. 191–192). As he states in his letter to Pauli, "The fact is that during recent months I have worked rather diligently on an investigation of *the statistical problems* in quantum theory. I really feel that I can now present *the question of observation* substantially more clearly" (Bohr 1972–1996, vol. 6, p. 193; emphasis added). Bohr thus links the statistical problems of quantum theory and the question of observation. He also notes that the change from "complementarity" to "reciprocity" was a mistake (Bohr 1972–1996, Vol. 6, p. 195).

"The Quantum of Action and the Description of Nature" is a crucial step toward this argument. It took Bohr at least another ten years, to the Warsaw lecture of 1938 to finally cross this threshold and then some to arrive at his final destination on this road, a journey more or less completed by the time of his 1949 "Discussion with Einstein" (Bohr 1949). This destination was perhaps reached in part because Einstein was no longer there to make Bohr continue his journey. Could it have been continued? Perhaps. Bohr had, however, reached quite far already and no significantly new ideas and arguments are found in his articles beyond "Discussion with Einstein," although there are some further refinements, which was an interminable process for Bohr. He appears to have needed Einstein to reach where he finally arrived. But he also, and to begin with, needed Heisenberg and, differently, Schrödinger to reach this destination or destiny, for, to return to his final statement on the subject, it was his life.

Chapter 8
Can Quantum-Mechanical Description of Physical Reality Be Considered both Complete and Local?

I am forcing myself these days with all my strength to familiarize myself with the mysticism of nature and am attempting to prepare myself for all eventualities, indeed even for the assumption of a coupling of quantum processes in separated atoms. However, the costs of this assumption are so great that they cannot be estimated within the ordinary space-time description.
—Niels Bohr, A Letter to Heisenberg, April 18, 1925 (Bohr 1972–1996, vol. 5, pp. 79–80)

I cannot seriously believe in [quantum mechanics] because the theory cannot be reconciled with the idea that physics should represent a reality in time and space, free from spooky actions at a distance.
—Albert Einstein, A Letter to Born, March 3, 1947 (Born 2005, p. 155)

Abstract This chapter offers an analysis of Bohr's exchanges with Einstein concerning the completeness and, then, locality of quantum mechanics, most especially Einstein, Podolsky, and Rosen's article and Bohr's reply, both published under the same title—"Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?"—in 1935. EPR's article introduced a thought experiment, the *EPR experiment*, and offered a particular argument concerning it, *EPR's argument*, which led EPR to conclude that quantum mechanics is incomplete, or else nonlocal in Einstein's sense of entailing instantaneous physical connections, forbidden by relativity. This argument and these conclusions were questioned in *Bohr's counterargument*, which offers a different analysis of the *EPR experiment* and derives different conclusions concerning its meaning and implications. After a general introduction given in Section 8.1, Sections 8.2 and 8.3 focus on those parts of EPR's and Bohr's arguments, respectively, that concern the completeness of quantum mechanics. Section 8.4 discusses the question of locality in both arguments. I offer a close overall reading of Bohr's reply in Chapter 10 where I also discuss Einstein's arguments that address the statistical nature of quantum mechanics from the perspective of the EPR-type experiments.

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8.1 Correlations, Completeness, and Locality

Bohr's statement, used as my first epigraph, captures the core of the dilemma posed by the EPR experiment. What makes the statement remarkable is that it was made in 1925. In 1925! This is not only 10 years before EPR's article introducing the EPR experiment, or even before Bohr's earlier exchanges with Einstein in 1927, but also before (albeit by only a few months) Heisenberg's discovery of quantum mechanics, based on "abandoning the ordinary space-time description." The immediate context of the statement is the collapse of the Bohr, Kramers, and Slater (BKS) theory, in view of the recently obtained (by Hans Geiger and Walther Bothe) experimental evidence in favor of the exact validity of the energy conservation law. As noted earlier, the BKS theory was based on the statistical rather than exact conservation of energy, which unsurprisingly did not endear it to most major players at the time—Einstein, Dirac, and Pauli among them. Schrödinger, by contrast, was sympathetic. The theory also represented the last-ditch attempt on Bohr's part to retain, at least partially, the space–time description of quantum processes in the manner of the old quantum theory. Heisenberg, the recipient of the letter where the statement occurs, did not address the question of "a coupling of quantum processes in distant atoms" in his work on his new quantum mechanics (then underway). He was, however, willing to pay the great costs invoked by Bohr, which would resolve the problem of "a coupling of quantum processes in separated atoms" anyhow.[1] Heisenberg's new theory was defined by the absence of the space–time description, while preserving the exact energy conservation. Indeed, demonstrating the validity of the latter from his formalism was a major success of the new theory. At the same time, the theory retained what proved to be a lasting contribution of the BKS theory: the probabilistic nature of our predictions concerning the outcome of individual quantum events.

In any event, Bohr was already contemplating, *in the first place*, the possibility that *quantum phenomena* could exhibit the features found in the situation of the EPR type, and, only *secondarily*, as a consequence, the assumption "that [quantum processes] cannot be estimated within the ordinary space–time description," the cost that ultimately came to define his epistemology of quantum theory. It took Einstein's interventions from 1927 on, especially the EPR experiment, to induce Bohr to rigorously develop this epistemology, ultimately in

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[1] In responding to the situation, Heisenberg said that he "*would … plead for a dualistic theory: everything must be described both in terms of the wave theory and in terms of light-quanta*" (Letter to Kronig May 8, 1925; cited by *MR* 6, p. 163). While Mehra and Rechenberg, who cited both letters, might be right to stress the significance of Heisenberg's "perceptive and prophetic remark," they are less justified in seeing the situation itself in terms of the wave–particle complementarity (*MR* 6, p. 163). Heisenberg, too, views these two descriptions in terms of equivalence rather than complementarity. Mehra and Rechenberg's discussion of complementarity appears to miss this point and, more generally, the complexity of the relationships between Bohr's concept and the wave–particle dilemma (*MR* 6, pp. 163–198).

its nonclassical form, sometime around 1938. As earlier in this study, I distinguish the *EPR experiment*, the thought experiment EPR propose, and *EPR's argument*, their analysis of this experiment and the conclusions they derive, which Bohr challenged. Unlike Einstein, Bohr never appears to have thought—either at the time of this statement or following EPR's and related arguments by Einstein—that the EPR experiment would imply the nonlocality of quantum phenomena or, correspondingly, of a proper (complete) theory, such as quantum mechanics that would account for it. His use of the word "coupling" might suggest this implication, but the epistemology contemplated by the statement allows one to avoid it. As he said later: "Certainly the issue [raised by the EPR experiment] is of a very subtle character and suited to emphasize how far, in quantum theory, we are beyond the reach of pictorial visualization" (Bohr 1949, *PWNB* 2, p. 59), which is to say, beyond estimating it within the ordinary space–time description. But, while he argued that the EPR experiment is adequately accounted for by quantum mechanics, he never said that the situation implies the nonlocality of either quantum phenomena, such as those observed in the EPR experiment, or of quantum mechanics. Just as did Einstein, Bohr saw nonlocality as unacceptable, although Einstein and others thought (mistakenly) that Bohr allowed for it in order to argue that quantum mechanics is complete. As already this earlier statement makes clear, for Bohr the cost of "the assumption of a coupling of the processes in separated atoms" is the impossibility of "the ordinary space–time description" and, correlatively, the irreducibly probabilistic nature of all quantum predictions, collective or, more crucially, individual. While this last point is not stated here, it is suggested by Bohr's accompanying and later work. In sum, this is the same position as he took in his reply to EPR.

Although Bohr was, thus, not unprepared, EPR's paper was still a surprise, "a bolt from the blue," as Léon Rosenfeld famously described it (Rosenfeld 1967, p. 114). There is some debate as to the nature of the surprise—in particular, whether Bohr was surprised by the argument itself or by the fact that Einstein would still be making this type of argument, following their previous exchanges. I shall discuss these earlier exchanges from the vantage of the EPR experiment below. It may also be recalled that, while contained in and grounding the *EPR experiment*, the concept of quantum entanglement, which was new, was not formulated by EPR and was introduced by Schrödinger later. Be that as it may, a reassessment, now with quantum mechanics and complementarity in hand, of this situation defined by Bohr's 1925 statement in question, as restaged by EPR, eventually led Bohr to his nonclassical version of complementarity. The conceptual architecture of complementarity had to be further refined in turn, ultimately leading Bohr to new concepts, such as phenomena and atomicity, and to a fully nonclassical interpretation of quantum phenomena and quantum mechanics. EPR's argument and Bohr's reply mark one of the most decisive points of this trajectory.

The argument of Bohr's reply proved difficult to formulate. As he said in "Discussion with Einstein": "Rereading these passages, I am deeply aware of the inefficiency of expression which must have made it very difficult to appreciate the trend of the argumentation aiming to bring out the essential ambiguity involved in a reference to physical attributes of objects when dealing with phenomena where no sharp distinction can be made between the behavior of the objects themselves and their interaction with the measuring instruments" (Bohr 1949, *PWNB* 2, p. 61). In part because of these difficulties, it is a matter of interpretation whether Bohr's reply offered a fully developed counterargument to EPR's argument, and it has been disputed that he did. Bohr's case was only partially helped by his "hope," expressed on this later occasion, "that the present account of the discussion with Einstein in the foregoing years . . . may give a clearer impression of the necessity of a radical revision of basic principles for physical explanation in order to restore logical order in this field of experience," argued for in his reply to EPR (Bohr 1949, *PWNB* 2, p. 61). The arguments of this and other later essays do offer useful clarifications to his reply. However, these clarifications can only serve as supplements, and still one needs to undertake a reading of Bohr's reply itself to take advantage of them.

I shall argue that, at the very least, Bohr's reply supplied just about all the ingredients necessary for and came very close to formulating such a counterargument; and my aim here is to offer as complete a formulation of Bohr's counterargument—or, at least, of a counterargument based in Bohr's reply—as possible. This formulation will, hopefully, make clearer why Bohr sees EPR's and related arguments by Einstein as insufficient to make the case for either the incompleteness or, alternatively, the nonlocality of quantum mechanics. I also believe that there are still lessons to be learned from the exchange, in particular as concerns the irreducibly probabilistic character of our predictions concerning quantum phenomena. The argument of this chapter shows that this fact is relevant even when such predictions can be made—as they are in the EPR experiment—with certainty, with probability equal to unity. This probabilistic aspect of the EPR experiment does not appear to have received sufficient attention in the literature. In any event, I am unaware of other works offering the type of argument to be offered here, apart from previous brief communications along these lines by the present author (e.g., Plotnitsky 2006a). It might, however, be useful, before I proceed, to reflect briefly on the current stage of the debate concerning quantum phenomena and quantum mechanics, especially given that this stage is largely defined by the EPR and related problems.

As noted from the outset of this study, the debates concerning quantum mechanics, quantum phenomena, and nature itself at the quantum level of its constitution appear to be interminable. These debates still involve the complexities and mysteries that became apparent at the inception of quantum physics with Planck's discovery of the quantum of action in 1900, and we have received many offerings from the apparently inexhaustible store of quantum mysteries since then, with more undoubtedly yet to come. The existence of statistical correlations between certain specifically prepared spatially separated quantum events is among the most profound of these mysteries, and it defines the current state of the debate concerning quantum theory. These correlations can be ascertained experimentally, regardless of any theory.[2] Quantum mechanics, however, properly predicts the corresponding numerical data, and this capacity is reflected in the concept, remarkable in its own right, of "entangled states" in the formalism of the theory.

These correlations are often known as the EPR correlations or, given the role of J. S. Bell's work in the development of the subject, the EPR–Bell correlations. At most, EPR's article has only de facto introduced quantum entanglement and correlations. However, neither term was invoked and neither concept considered in EPR's article or in Bohr's reply, or in subsequent contributions by either Einstein or Bohr, focused, as they were, on the questions of completeness and locality of quantum mechanics. Both articles, as it were, spoke about correlations without speaking of them, and Bohr's reply did so no less and perhaps more than EPR's article (Mermin 1998a, p. 765, n. 3). The term "entanglement" [*Verschränkung*] was introduced by Schrödinger in his analysis of the EPR experiment in its immediate wake. The subject of correlations took center stage with Bell's work, now assembled in (Bell 1987), and his theorem, based on David Bohm's version of the EPR experiment for spin measurements and discovered in the mid-1960s. Most of the related developments deal with Bohm's version of the experiment, since, while the original thought experiment proposed by EPR is an idealized experiment that cannot be actually performed in a laboratory, Bohm's version can be and has been. Among the most prominent of these developments are the Kochen–Specker theorem or more recent theorems of D. M. Greenberger, M. Horne, and A. Zeilinger and L. Hardy, and, from the experimental side, Alain Aspect's experiments and related experimental work, such as that of Anton Zeilinger and his group (Greenberger et al. 1989, 1990; Hardy 1993; Aspect et al. 1982). The advent of quantum information theory during the last two decades gave this problematic further prominence and significance.

The EPR correlations are essentially quantum (classical phenomena do not exhibit them) and, like all quantum mechanical regularities, they are statistical. As discussed throughout this study, it is a defining feature of quantum phenomena that identically prepared quantum experiments (as concerns the state of the measuring instruments involved) in general lead to different outcomes. This circumstance makes the recourse to probability irreducible in estimating the outcomes of individual experiments or events. It also makes quantum mechanics—at least, again, in Bohr's or the present nonclassical interpretation—a probabilistic theory in fundamental terms, rather than only in practical terms, which would reflect the incompleteness of our knowledge due to the great complexity of the behavior of certain systems, behavior that is ultimately causal but is not traceable as such. At the same time, however, series of certain quantum events (but again, only of some certain quantum events) are

[2] For an elegant discussion of this aspect of quantum formalism see N. D. Mermin's argument (Mermin 1998a) to which I shall return in Chapter 10.

correlated, as against analogous series of spatially separated random events found in classical physics, as is to be expected following Einstein's relativity and the principle of local causality it introduced. It is, accordingly, not surprising that the shift of attention to the subject of entanglement and correlations has brought more sharply into focus the question of locality, as against that of the completeness of quantum mechanics.[3] The currently prevalent view appears to be that quantum mechanics is complete within its proper scope. The completeness of quantum mechanics was, as the identical title of the two papers states, the primary focus of the Bohr–Einstein initial exchange concerning the EPR experiment. As will be seen, however, the question of locality was at stake in this exchange as well. As do later arguments by Einstein (e.g., Einstein 1936, 1948, 1949a, b), EPR's argument contends that quantum mechanics is *either incomplete or nonlocal*. Einstein had maintained this view until the end of his life, as my epigraph citing his 1947 letter to Born indicates: "I cannot seriously believe in [quantum mechanics] because the theory cannot be reconciled with the idea that physics should represent a reality in time and space, free from spooky actions at a distance" (Letter to Born, March 3, 1947, Born 2005, p. 155). The phrase "spooky actions at a distance" has been famous ever since. Since, however, Einstein saw locality as impermissible, he never accepted that quantum mechanics could be regarded as a complete theory.

As will be discussed in Chapter 10, Einstein admitted that quantum mechanics could be considered local if seen as a theory of ensembles, loosely

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[3] As throughout this study, I use the term "local" in the sense of the compatibility with (special) relativity. One encounters a number of conceptions of locality or nonlocality in the discussions of the EPR-type situations. Other terms, such as nonseparability, are also used, including early on by Einstein himself (Letter to Schrödinger, 19 June, 1935, cited in *MR* 6, p. 740). Such alternative terms are often given various meanings as well. Some of them are compatible with my argument in this chapter, while others would require further qualifications. Indeed, there have been arguments that quantum nonlocality or instantaneity, if true, is different in nature than that which special relativity inherently prohibits given its particular structure. These qualifications, however, are not essential for my argument, which involves locality only in the sense just defined, although the present argument implies that EPR's and related arguments are not sufficient to demonstrate any "spooky" *physical action* at a distance in related experiments. It should also be noted that, especially in Bohr's interpretation—that is, insofar as all measurable physical properties or quantities considered are those pertaining only to the measuring instruments involved—locality may be viewed as a classical macro-feature of the overall "quantum-mechanical description of physical reality." In this case, one can use this phrase without further qualifications, since it is now applied to the classical description of, in the EPR situation, the two sets of measuring instruments involved. Insofar as one does not ascribe any physical properties to quantum objects and processes themselves, except at the time of measurement (as in Bohr's reply to EPR) or at all (as in Bohr's ultimate view), this view does not reintroduce realism at the quantum level, as locality might do otherwise (cf., Chiao and Garrison 1999). Further qualifications might be needed in the case of quantum field theory. Quantum mechanics, however, is not a relativistic theory, since it deals only with the energy levels where relativistic effects can be neglected. It only needs to be *compatible* with relativity within its limits, just as, within its limits, classical physics needs to be.

on the model of classical statistical physics. This view, however, still leaves it incomplete either by virtue of not offering a complete description of the individual quantum systems (since at this level the EPR argument still applies) or by offering no such description at all. Bohr agrees that quantum mechanics does not describe individual quantum processes in space and time as independent of their interaction with measuring instruments in the way classical mechanics does. This is what Einstein wanted a proper theory to do, although not on the lines of classical mechanics, but instead on the model of general relativity as a field theory (e.g., Einstein 1949a, pp. 83–85). Bohr argued, however, that it does not necessarily follow that quantum mechanics is incomplete, since, while quantum mechanics only provides probabilistic estimates concerning outcomes of individual quantum experiments, nature may not allow us to do more. In other words, the probabilistic nature of quantum-mechanical predictions concerning single individual events is not incompatible with the completeness of quantum mechanics because this character of our predictions is unavoidable given the nature of these phenomena and the experiments we can perform concerning them. Nor, he further argued, is the situation incompatible with the locality of quantum mechanics or of quantum phenomena themselves. It follows that quantum mechanics is, at least thus far, as complete as this nature appears to allow any theory of these phenomena and experiments to be, and this nature also appears to allow for the locality of quantum mechanics or, again, of these phenomena and experiments themselves. Or at least, again, EPR's and Einstein's arguments do not prove otherwise.

On the other hand, there exist interpretations of quantum mechanics, such as Bohr's complementarity, that are local. Accordingly, unless some arguments or some experimental data to the contrary become available (which thus far does not appear to be the case), such interpretations allow one to see quantum mechanics as, within its scope, a complete and local theory accounting for quantum phenomena. As Bohr's says in the abstract of his reply, "a *viewpoint* termed 'complementarity' is explained *from which* quantum mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands of completeness" (Bohr 1935b, p. 696; emphasis added).[4] He also speaks of a "viewpoint" in the first paragraph of the article itself and then in the passage—which conceptually closes his counterargument and is the most famous passage of his reply—on the essential ambiguity he locates in EPR's use of their criterion of reality (Bohr 1935b, pp. 696, 700). On all occasions just cited, Bohr omits the word "reality." His formulations displace the "quantum-mechanical description of physical *reality*" of EPR's and his title into "quantum-mechanical description of physical *phenomena*." Bohr also omits the term "reality" yet again when he states that "the argumentation of the mentioned authors does not justify their conclusion that quantum-mechanical description [!] is essentially incomplete" (Bohr 1935b, p. 700).

[4] Here and throughout his reply, Bohr uses the term 'phenomena' in its conventional sense, rather than in Bohr's special sense, introduced later. Accordingly, in this chapter I shall use the term in this conventional sense as well, unless specified otherwise.

These omissions and displacements can hardly be seen as accidental and may have been deliberate, although they would be as significant if they were unconscious.

In his reply to EPR, he does not go quite as far as his later thinking and allows for an attribution of certain properties to quantum objects at the time of measurements under the constraints of the uncertainty relations, or even, with certain crucial qualifications, on the basis of predictions with certainty and hence independent of measurements.[5] At least, Bohr allows for the latter kind of attribution as part of his *counterargument* to EPR, since, as will be seen presently, EPR ground their argument in the assumption that we can do so. Bohr shows their argument is still insufficient even under this assumption. Bohr's *argument for complementarity* itself, given in his reply, virtually amounts to the impossibility of this assumption, in view of the qualifications just mentioned, especially those that have to do with the question of locality. Bohr's aim is, again, also to offer a comprehensive "viewpoint" of the quantum mechanical situation, the EPR experiment included, as an adequate response to an "entirely new situation as regards the description of physical phenomena" with which nature confronts us in quantum physics. From this viewpoint quantum mechanics appears as both a complete and a local theory of quantum phenomena.

From Einstein's *viewpoint*—again, even if considered in terms of Bohr's "tamer" epistemology adopted in his reply—such a theory could not be considered a complete one.[6] It is not altogether clear whether Einstein (who appears to have misread Bohr's reply) followed Bohr to the ultimate limits of his epistemology in place by the time of his "Discussion with Einstein," first published in the so-called Schilpp volume (Schilpp 1949) and at least ostensibly on Einstein's radar by that time. In his comments on the EPR experiment there, he appears to have in mind only Bohr's argument in his reply to EPR. However, it is difficult to think that Einstein would have found appealing a view that the actual properties and behavior of the ultimate physical objects considered by a theory are inaccessible to the theory or inconceivable altogether—and even prohibit such access or conception.

In any event, in all of his arguments of the EPR type, Einstein assumes that quantum phenomena and quantum mechanics allow us to assign physical properties to quantum objects themselves on the basis of predictions with certainty. The reason for this assumption is that, in the EPR-type experiments, these predictions are made on the basis of the data obtained from measurements performed on other

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[5] Some commentators do attribute to Bohr, including in his post-EPR thinking, the view that "measurement reveals the objective, pre-existing value of an observable" (Murdoch 1987, p. 107), which attribution is problematic from the perspective of the present reading of Bohr.

[6] This rejection also shows that one's philosophical view influences one's concept of completeness of a physical theory. EPR's (sufficient) criterion of reality allows for a broader range of views, and Einstein's critique of quantum mechanics, while conditioned by his philosophical views, is not determined by these views alone. Hence, as Bohr realized, it is not sufficient to reject Einstein's arguments merely on metaphysical grounds. One has to offer a rigorous counterargument, which may involve a critique of certain philosophical assumptions underlying Einstein's argument, if these assumptions are inapplicable to quantum phenomena.

quantum objects, spatially separated from those concerning which we make the predictions in question. Accordingly, it appears to be possible to assign these properties independently of any measurement performed on the objects themselves, or, in EPR's and Einstein's language, without in any way disturbing these objects. On this basis, EPR and Einstein in his other communications on the subject argue that it is possible to ascertain that quantum objects independently possess *both* conjugate measurable quantities of the type used in classical mechanics, such as the position and the momentum of a quantum object. If correct, this argumentation would de facto, even though not in actual measurements, allow one to circumvent the uncertainty relations. Since, however, it is never possible to ascribe definite values to both such quantities by means of the formalism of quantum mechanics, which contains the uncertainty relations, quantum mechanics must be incomplete.

Bohr contested this argumentation by arguing that EPR do not show that it is in fact possible to establish that a given quantum object can be seen as simultaneously possessing both conjugate quantities. By the same token, the uncertainty relations cannot be circumvented even de facto, as EPR think they could be, rather than only by way of direct measurements. Bohr pursues his critique of EPR's argument by rigorously re-examining EPR's assumptions, most especially their conception of physical reality (and the criterion of reality based on this conception) as applicable to quantum phenomena, including those found in the EPR experiment. This criterion states, "*If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity*" (EPR 1935, *QTM*, p. 138). Bohr argues that the nature and the constitution of quantum phenomena, as defined by the irreducible role of measuring instruments, disallow the *way*—the *unqualified* and hence ambiguous way—this criterion *is used* by EPR, and thus disallow the single most important part of the logic on which EPR base their argument. In other words, what EPR see as possible, but not covered by quantum mechanics, may not be possible by virtue of the experimentally established facts. EPR's argument becomes insufficient by virtue of the "reality" of quantum phenomena, which cannot be interpreted in accordance with EPR's assumptions. At least, EPR did not demonstrate that they can, and hence, it follows, they did not show quantum mechanics to be either incomplete or nonlocal. The same type of criticism applies to Einstein's subsequent arguments of the EPR type. I am not saying that local interpretations other than that of Bohr or alternative local theories of quantum phenomena that allow for a stronger quantum-level ontology are impossible, or that EPR's argument and related arguments by Einstein are the only possible arguments against the completeness or locality of quantum mechanics. Bohr's counterargument to EPR does not prove that such interpretations or theories or such arguments are impossible, although subsequent developments, such as Bell's and related theorems, appear to create major obstacles for such projects. I am arguing, however, that EPR's and Einstein's related arguments are insufficient for making the case against quantum mechanics they aim to make.

Einstein appears to have thought that Bohr "came nearest to doing justice to the problem" posed by the EPR experiment, even though he did not think that Bohr had solved it. On the other hand, he also appears to have misread Bohr's argument as allowing for a nonlocal character of quantum mechanics and preserving its completeness at this cost—as he qualified, "translated into my own way of putting it" (Einstein 1949b, Schilpp 1949, p. 681). Beginning with bypassing the role of quantum measurement, on which Bohr based his argument, and ending with seeing nonlocality as acceptable to Bohr in order to preserve the completeness of quantum mechanics as concerns individual quantum systems (rather than seeing it as a statistical theory of ensembles of quantum systems), Bohr's argument is lost in Einstein's "translation." This translation reads Einstein's own logic of the relationships between locality and completeness in quantum mechanics into Bohr's very different logic.[7] Bohr, by contrast, argues that "the singular position of measuring instruments in the account of quantum phenomena"—which makes it impossible to consider quantum objects apart from their interactions with these instruments—disables Einstein's argumentation on both counts: completeness and locality. Einstein, Bohr argues, does not show that quantum mechanics is deficient as concerns what is, in principle, possible to know about quantum phenomena or quantum objects. At the same time, "together with the relativistic invariance of the uncertainty relations," this role of measuring instruments "ensures the compatibility between [the] argumentation [of his reply]... and all exigencies of relativity theory" (Bohr 1935b, p. 701, n.). Both facts are crucial, since together they allow Bohr to maintain the locality of quantum phenomena and quantum mechanics and of complementarity as their

[7] On a later occasion, in presenting a similar argument, he characterizes Bohr's position by stating that for Bohr "there is no reality independent of the probable subject." He adds that while "[he does] not believe... that this concept, though consistent in itself, is here to stay,... it is the only one which does justice to the mechanism of the probabilistic quantum theory" (Letter to Born, December 3, 1953, Born 2005, p. 205). I assume that Einstein means here "reality" at the quantum level, since otherwise the statement is flatly wrong, to whatever stage of Bohr's thinking it refers (and it is not clear whether Einstein distinguishes different positions taken by Bohr at these stages). Bohr clearly retains reality in Einstein's sense and objectivity (in the sense that the experimental evidence concerned is independent of a given subject) at the classical level of measuring instruments. At the quantum level, too, Bohr only maintains that there is *no specific form of reality* or *specific ontology* that one can assign to this "reality": there is no concept (including that of reality) through which we conceive of it. Indeed, given this inconceivability, this view automatically implies a strict independence of this "reality" of any human subject. Moreover, in his reply to EPR, to which Einstein appears to refer here, Bohr, again, allows for an assignment of certain properties to quantum objects themselves at the time of measurement. However, Einstein might only mean that, if quantum mechanics is complete, in the case of the EPR experiment, which involves a pair of spatially separated quantum objects (S_1, S_2), "a probable subject's" decision concerning which of the two alternative measurements to perform on the first EPR object, S_1, determines the corresponding reality of the state of the second, S_2. This would make quantum mechanics nonlocal, which he believes—again, mistakenly—is acceptable to Bohr. He also says, however, that "one need not become involved with Bohr's interpretation"—again, as *interpreted* by Einstein—if one merely sees quantum mechanics as a theory of ensembles. In this case it is, again, incomplete, but now by virtue of not providing a "complete law for the complete description of the single system which [law] determines its development in time" (Letter to Born, December 3, 1953, Born 2005, p. 205). On the latter point, Einstein is correct. The question is, again, whether nature allows for such a description, which, if it does not, redefines the meaning of "completeness."

interpretation, or *through* complementarity as their interpretation. Quantum mechanics correctly predicts all the data in question, the EPR correlations included, without requiring one to make claims concerning either nonlocal connections between distant events or the properties of quantum objects or their behavior.

Ironically, any classical-like complete theory of the type Einstein wanted and thought possible by virtue of his arguments of the EPR type that would predict these data now appears to have to be nonlocal in view of Bell's theorem and related findings (related to discrete variables). The situation would be similar to Bohmian hidden variables quantum mechanics, where nonlocality is an explicit consequence of the formalism (in any version of it available so far). Bell's theorem tells us that any classical-like (hidden variables) theory, reproducing the statistical predictions of quantum mechanics, would be nonlocal, a finding further amplified by related theorems, such as the Kochen–Specker theorem, all of which, in the present view, appear to support Bohr's argument.[8] This argument itself did not go as far as

[8] There has been much debate and controversy concerning Bell's theorem and related theoretical and experimental findings, specifically as regards how tight these arguments are, and sometimes as regards what they actually demonstrate. The subject has generated an immense body of literature, reflecting a nearly uncontainable multitude of views, as have EPR's article and Bohr's reply, to begin with, whether in their own right or in the context of Bell's theorem. It is, as I said, impossible to even minimally survey this landscape, even as concerns the pertinent theoretical work. There is a very large body of related experimental work on quantum correlations as well, most of which, such as that mentioned earlier in this chapter, appears to confirm the *fact* of the EPR–Bell correlations. Physicists, including experimentalists, are now much more cautious about the concept of "experimental confirmation" in physics, especially in cases, like this one, that have great subtlety (cf., for example, Salart et al. 2008). Interpreting these findings is of course a different matter and is part of the debate in question. For earlier, but still instructive, exchanges concerning EPR's article and Bohr's reply, see Mehra and Rechenberg's discussion, which refers to some more recent ones as well, and references (*MR* 6, part 2, pp. 713–759). For reasonably representative selections of articles on Bell's theorem, see Cushing and McMullin (1989), Kafatos (1989), and Ellis and Amati (2000). One of the most persistent and unfortunate (mis)conceptions of Bell's theorem or Bell's view of the situation—or, again, of the EPR experiment—is that it in fact implies nonlocality of quantum mechanics. I shall not try to counter this contention further here. It represents a minority view (a sometimes vocal minority), especially prominent among those who support or are sympathetic to Bohmian theories, for obvious reasons, since it makes one of the most undesirable features of these theories in effect a feature of nature, even if never manifested in actual experiments. I might note that, while sympathetic to Bohm's approach in general, Bell himself never subscribed to this view and only saw it as one of the possible, if highly undesirable, alternatives, and indeed, while he was hardly sympathetic to Bohr's views, he saw them as less problematic (Bell 1987, p. 155). Compelled by the view, again, shared by Einstein and Bohr, that nonlocality is not permissible, the present understanding of Bell's theorem is consistent with that articulated by many commentators. See, in particular, Mermin's essays on the subject collected in *Boojums All the Way Through* (Mermin 1990) or his argument in Mermin (1998b), Gottfried (2000), Bertlmann and Zeilinger (2002), and Zeilinger et al. (2005), or, with a different valuation of this interpretation, Bell's articles on the subject in Bell (1987). See Shimony (2004) and Held (2006) for helpful introductory discussions of Bell's and the Kochen–Specker theorems, respectively. There are by now many proofs of both theorems, and some of the more recent proofs of the Kochen–Specker theorem are considerably simplified vis-à-vis the original proof, which is quite cumbersome. See, for example, Peres (1993, pp. 196–201) and for an especially elegant proof (Cabello 2009). From the present perspective, both theorems confirm what follows from the experimental considerations discussed in this chapter (they apply to discrete variables as well).

rigorously proving the impossibility of local classical-like theories compatible with the data of quantum mechanics, but it is, I argue, sufficient for a critique of EPR's argument. Bohr did not mathematically demonstrate—in the way, for example, Bell's and the Kochen–Specker theorems do (again, for discrete variables)—that the numerical data in question in quantum mechanics are logically incompatible with a joint assignment of certain (complementary) properties to a given quantum object.[9] What he showed, however, is that, contrary to EPR's contention, quantum phenomena, including those of the EPR type, disallow us to rigorously contemplate an experimental assignment of such properties simultaneously or, as I shall explain, even separately to *the same quantum object*. This demonstration, accordingly, disables their particular argument, even though, as I said, not all conceivable arguments. In this regard, the contention, made sometimes, that Bohr did not properly, in mathematical terms, respond to EPR's mathematical arguments is beside the point. This contention is not justified technically either, since Bohr commented on the mathematics involved, relatively briefly but sufficiently, by explaining why their deduction easily follows from the formalism of quantum mechanics (Bohr 1935b, pp. 696–697, n.). There is nothing wrong with *EPR's mathematics* or the *EPR experiment*; it is their physics—the conclusions they draw from this mathematics and this experiment—that Bohr found objectionable. Bohr also shows that his interpretation of quantum phenomena and quantum mechanics as complementary allows one to adequately, if not uniquely, ground quantum mechanics as a complete and local theory of these phenomena.[10]

8.2 Can Quantum-Mechanical Description of Physical Reality Be Considered Complete? EPR's Argument

EPR base their argument on two key criteria. One is a *necessary* criterion of completeness: for a theory to be considered complete, "*every element of the physical reality must have a counterpart in the physical theory*" (EPR 1935, *QTM*, p. 138). The criterion of completeness must be necessary for the type of argument they aim to make. On the other hand, with this criterion in hand, they need only a *sufficient* criterion of reality to demonstrate the incompleteness of quantum mechanics, at least under the condition of locality. EPR formulate

[9] Since it is consistent with attributing some (but, again, not all) properties in quantum mechanics to a quantum object *at the time of measurement*, Bell's and the Kochen–Specker theorems only require the epistemology of the type Bohr adopts in his reply. They do not entail nonclassical epistemology, which is, however, self-evidently consistent with both theorems, since all quantities involved are manifested only in the measuring instruments involved.

[10] Bohr's interpretation cannot guarantee that these phenomena or nature at whatever level cannot one day be proved to be nonlocal. But then relativity cannot guarantee this either. As things stand now, there is no experimental evidence that contradicts relativity, but there is much evidence to support it, which makes most physicists disinclined to accept nonlocal theories, such as those of the Bohmian type, for example.

their criterion of reality as follows: "*If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity*" (EPR 1935, *QTM*, p. 138). Although not always expressly stated, both criteria are retained in Einstein's subsequent arguments on the subject, even though the main focus of his analysis shifts from the completeness of quantum mechanics to its locality. The completeness of quantum mechanics is still at stake in these arguments, since quantum mechanics is argued by Einstein to be either incomplete or if complete, then nonlocal—the alternative considered, briefly but importantly for their overall argument, in EPR's article as well. Accordingly, while Einstein's subsequent arguments may express his way of looking at the situation more faithfully, these arguments are not essentially different from that of EPR. As I noted, these arguments also consider quantum mechanics as a local statistical theory of ensembles, the subject to be considered in Chapter 10. These considerations, however, do not essentially change the present argument either.

EPR's argument for the incompleteness of quantum mechanics (by their first criterion) depends on the assumption, taken by EPR to be self-evident, that their second criterion, their criterion of reality, applies as straightforwardly in quantum physics as it does in classical physics (EPR 1935, *QTM*, p. 139). This assumption becomes the main target of Bohr's counterargument. In his famous phrase, the criterion contains "an essential ambiguity ...*when it is applied to quantum phenomena*" and, hence, to "the actual problems with which we are ... concerned [in the EPR experiment]" (Bohr 1935b, pp. 696–697; emphasis added). EPR's argument is ingenious and, as Bohr noted later, "remarkable for its lucidity and apparently incontestable character," but, as he clearly implies here, only *apparently* incontestable (Bohr 1949, *PWNB* 2, p. 59). I shall now outline the essential logic of this argument.

It may indeed *appear* that the criterion of physical reality applies in the case of quantum phenomena and quantum mechanics as well, *without any further qualifications* (which Bohr will argue to be necessary), just as it does in classical mechanics. As EPR say, "Regarded not as a necessary, but merely as a sufficient, condition of reality, this criterion is in agreement with classical as well quantum-mechanical ideas of reality" (EPR 1935, *QTM*, p. 139). The reasons for this *apparent* applicability or apparently unqualified applicability of the criterion to quantum phenomena are as follows. It is only a joint *simultaneous measurement* or *simultaneous prediction* of two conjugate quantities involved in the quantum-mechanical description that is impossible in view of the uncertainty relations. The value of a single variable can always be measured with any degree of accuracy within the capacity of our measuring instruments, while the uncertainty relations are not affected by this capacity, but only by the value of Planck's quantum of action, h. Hence in the idealized case this value can be considered precisely measurable, which fact is crucial to both

EPR's argument and Bohr's reply.[11] The value of a single variable can also be *predicted exactly* without performing a measurement on a quantum object in certain (idealized) circumstances, such as those of the EPR experiment. The latter deals with two quantum objects, S_1 and S_2, forming an EPR pair (S_1, S_2), that have previously been in interaction but are then spatially separated. Once S_1 and S_2 are separated, one can establish (with certainty) *both* the *distance between* the two objects and *the sum of their momenta* (the corresponding Hilbert-space operators commute as well). It can be easily shown that, with the "states" (in the sense of formalism) of the two objects thus entangled and with these quantities in hand, by *measuring* either the position or, conversely, the momentum of S_1, one can *predict exactly* either the position or the momentum for S_2 without physically interfering with S_2 by measurement. In Bohr's words, "By means of [their] interesting example . . . [EPR] proceed to show that in quantum mechanics, just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation" (Bohr 1935b, p. 696). This possibility, according to Bohr is "an immediate consequence" of what he calls "the transformation theorems of quantum mechanics," which reflect the fact that quantum mechanics offers "a coherent mathematical formalism covering automatically any procedure of measurement" (Bohr 1935, pp. 696–697n., 699).

EPR thought that these facts enable one to argue for the incompleteness of quantum mechanics, or else, again, its nonlocality, which they see as impermissible. In Bohr's words, "According to their criterion, [EPR] therefore want to ascribe an element of reality [pertaining to S_2] to each of the quantities represented by such variables. Since, moreover, it is a well-known feature of the present formalism of quantum mechanics that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables, they consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed" (Bohr 1935b, p. 696). In other words, in EPR's view,

[11] As I noted, unlike the Bell–Bohm version of the EPR experiment for spin (at stake in Bell's and related theorems), the actual experiment proposed by EPR, dealing with continuous variables, cannot be physically realized, since the EPR-entangled quantum state is non-normalizable. This fact, however, does not affect the fundamentals of the case, which can be considered in terms of the idealized experiment proposed by EPR. There are experiments (e.g., those involving photon pairs produced in parametric down conversion) which statistically approximate the idealized entangled state constructed by EPR for continuous variables, rather than for discrete variables (e.g., Ou et al. 1992; Howell et al. 2004; D'Angelo et al. 2004). I cannot consider these experiments, but they appear to be consistent with the present argument. They also reflect the fact that the EPR experiment is a manifestation of correlated events for identically prepared experiments with EPR pairs on the model of the Bell–Bohm version of the EPR experiment, to which the argument of this chapter applies with only minimal adjustments.

quantum mechanics is a *correct* theory, insofar as it allows us to predict with certainty *some* among the physical quantities ("elements of reality") associated with the phenomena in question. Thus, *so EPR and Einstein contend* (the contention contested by Bohr), it also enables us to ascertain, at least in the EPR-type situation, the existence *of all* such quantities in accordance with EPR's criterion of reality. By the same token, however, quantum mechanics is incomplete by virtue of providing no means for predicting and, hence, determining all of these quantities, whose physical reality we are thus able to ascertain, according to them.

EPR themselves sum up their argument and convey their logic more cumbersomely, which cumbersomeness might have contributed to Einstein's dissatisfaction with the article. They describe their deduction in their argument is as follows: "Previously we have proved that either (1) the quantum-mechanical description of reality given by the wave function is not complete or (2) when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality" (EPR 1935, *QTM*, p. 141). This "either or" leads EPR to their logical deduction that quantum mechanics is incomplete, given their conclusion that it is in fact possible to assign such a simultaneous reality to both such quantities. As they say, "Starting then with the assumption that the wave function does give a complete description of the physical reality, we arrived at the conclusion that two physical quantities, with noncommuting operators, can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete" (EPR 1935, *QTM*, p. 141). Before closing, they offer an important comment on the nonlocality of quantum mechanics as, they admit, a possible, but to them unacceptable, alternative, which argument I shall discuss in Section 8.4. In the absence of this unacceptable alternative, EPR can now close their argument: "While we have thus shown that *the wave function does not provide a complete description of the physical reality*, we left open the *question* of whether or not such a description exists. We believe, however, that such a theory is possible" (EPR 1935, *QTM*, p. 141; emphasis added).

EPR's conclusion is hardly hesitant as concerns their view of the incompleteness of quantum mechanics. It is true that Einstein eventually expressed a certain dissatisfaction with the article itself, written in English, it appears, largely by Podolsky, with Rosen contributing most of the mathematical part of the argument. Much has been made of this dissatisfaction in favor of Einstein's subsequent arguments, which Einstein preferred. Perhaps, however, too much was made of it, even leaving aside that his known comments appear to be mostly to the effect that the essential point of his argument was obscured, as opposed to his subsequent arguments, which were cleaner and less cluttered mathematically. This is not a monumental objection, which, moreover, clearly concerns an exposition and not the argument itself. In sum, the same essential argument—that quantum mechanics is either incomplete or nonlocal, and given that nonlocality is not permissible, it must be seen as incomplete—manifestly remains in place in Einstein's subsequent writings on the subject. However dissatisfied Einstein might have been with EPR's article itself, he never renounced this claim that quantum mechanics is incomplete under the assumption of locality. He added the possibility that quantum mechanics is local as a statistical theory of ensembles, as opposed to individual quantum systems, which, however, he did not see as a way out of the dilemma, since a proper description of individual quantum systems would still be lacking. Accordingly, Bohr in his reply had to address primarily the question of completeness of quantum mechanics, since, like EPR or Einstein in his related arguments, he took locality as axiomatic, and the key elements of Bohr's reply are applicable to all of Einstein's arguments on the subject. Bohr's analysis of the EPR experiment shows that neither EPR's argument for the incompleteness of quantum mechanics nor their alternative reasoning leading them to its nonlocality is sufficient to make their case. I shall in the next section explain Bohr's reasoning in his critique of EPR's argument for the incompleteness of quantum mechanics. I shall discuss the question of locality in Section 8.4.

8.3 Can Quantum-Mechanical Description of Physical Reality Be Considered Complete? Bohr's Argument

Bohr's argument in his reply is based on the analysis of, as Kant would have it, the conditions of possibility of quantum phenomena, most especially the irreducible (as against classical physics) role of measuring instruments in their constitution. Bohr argues that an examination of this role is lacking in EPR's article, even though they themselves "emphasize" that "the extent to which an unambiguous meaning can be attributed to such an expression as 'physical reality' cannot of course be deduced from a priori philosophical conceptions, but . . . must be founded on a direct appeal to experiments and measurements" (Bohr 1935b, p. 696).

It may be helpful to briefly consider first why EPR's criterion of reality automatically applies in classical mechanics. The main reason is that any object under investigation is presumed to possess the properties in question at any given point. The quantitative value of each such property may change in the course of the dynamic evolution of the object, and, unlike the equations of quantum mechanics, the equations of classical mechanics track these changes (thus also providing an idealized description of the systems considered). Classical mechanics makes predictions on the basis of this tracking. Whatever "disturbance" a measurement might introduce could, in principle, be neglected or compensated for, which is a crucial point for all of Bohr's thinking from the Como lecture on. This also means that one need not distinguish between objects and phenomena. By the same token, the value of both conjugate variables could be established simultaneously and, in principle, within the same experimental arrangement, which ensures the causal and deterministic character of classical mechanics. It is equally crucial that it is also possible, in principle, to *exactly* repeat a given experiment on an *identically prepared* object or even on the same object.

These conditions are no longer obtainable in the case of quantum phenomena. For, as emphasized throughout this study, it is a well-established experimental fact that in this case we can control only the preparation of measuring instruments and never the outcomes of the corresponding experiments, which outcomes are, in general, different, thus making the difference between quantum objects and quantum phenomena irreducible. It is, Bohr argues, by virtue of these circumstances, not sufficiently considered by EPR, that their application of their criterion of reality to quantum phenomena and quantum mechanics contains an essential ambiguity. It is possible to remove this ambiguity by qualifications that establish the proper conditions of using EPR's criterion of reality in the case of quantum phenomena. In effect, Bohr offers these qualifications. They concern the mutual exclusivity of the *two* measuring arrangements suitable and always necessary for predicting alternative quantities in question, as against classical physics, where this can be done, at least in principle, in a single experimental arrangement. These qualifications, however, also make insufficient EPR's *argument* and, by implication, Einstein's later arguments concerning the lack of completeness or, alternatively, locality of quantum mechanics. Once this ambiguity is removed by virtue of adding these qualifications, it can be shown that every element of reality that can be rigorously ascertained for a well-defined quantum phenomenon does have a counterpart in quantum mechanics, since the latter can predict this element. The probability of such a prediction is not always equal to unity, although sometimes it is, as in the EPR experiment (EPR, too, use quantum mechanics to make EPR predictions). The number of elements of reality for a given quantum objects thus ascertained could only be half of the number that EPR deem possible to ascertain. Thus, one could claim either an element of reality corresponding to a position measurement or an element of reality corresponding to a momentum measurement, but never both. The situation is, thus, strictly in agreement with the uncertainty relations, which become uncircumventable. As I have explained, EPR do aim to de facto circumvent them by arguing that both variables could be assigned to a given quantum object, although both can never be established simultaneously by either a measurement or a prediction, which makes the uncertainty relations applicable in practice. In sum, from Bohr's viewpoint, contrary to EPR's contention, what quantum mechanics can predict appears to be as much as nature allows us to predict, which makes quantum mechanics as complete as a theory of these phenomena could be.

According to Bohr, this difference between quantum and classical phenomena arises by virtue of the following interrelated circumstances, which, as discussed in Chapter 7, are also correlative to the irreducible probabilistic character of our quantum predictions:

(1) The first circumstance is *the necessity of discriminating in each case between quantum objects and measuring instruments (or phenomena)*. As Bohr says, "this necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a *principal distinction between classical and quantum-mechanical description of physical phenomena*" (Bohr 1935b, p. 701).
(2) The second is *the irreducible role of measuring instruments in the constitution of quantum phenomena*, which reflects the necessity of discrimination stated in (1).
(3) The third is, as a consequence, *the impossibility, even in principle, of rigorously considering the behavior of quantum objects independently of their interaction, their "finite [quantum] and uncontrollable interaction," with the measuring instruments* (Bohr 1935b, pp. 697, 700). Nobody has even *seen* a quantum object, say, a photon, independently (i.e., so that the role our observational technology in what is observed could be neglected in the way it can be in classical physics): we only observe impacts of such objects on measuring instruments.

It might appear that in the case of the EPR experiment the role of measuring instruments is no longer essential. This apparent possibility to dispense with this role was one of the main reasons why Einstein conceived of the experiment, in part in view of his previous exchanges with Bohr, where, as I shall discuss below, Bohr was able to use the role of measuring instruments to counter Einstein's criticism. As explained in the preceding section, the EPR experiment involves an (EPR) *pair* of objects (S_1, S_2), which allows for predictions (with certainty) about S_2 without performing a measurement on it by performing a measurement on S_1, and hence without involving an interaction between S_2 and any measuring instrument which would "in any way disturb" S_2. Given this possibility, EPR argue *first*, on the basis of their criterion of reality, that the quantities thus *predicted* may be attributed to quantum objects themselves even at the time of the prediction (rather than of a measurement), and hence that one need not disturb these objects by measurement in determining these quantities. *Second*, they argue that, since one can by means of such a procedure predict *either one or the other* of the two conjugate measurable quantities defining the behavior of physical systems (say, either the position or the momentum of the object), quantum objects independently possess *both* of these quantities, even though we can never predict both of them *simultaneously*. *Third* and finally, they argue, given that quantum mechanics provides no means for giving definite value to *both* such quantities, it must be incomplete. The only alternative, as they see it, is that quantum mechanics is nonlocal, since, as will be seen in the next section, assuming quantum mechanics to be complete, the state of a given quantum object could be determined by a measurement performed on another, spatially separated, quantum object.

Bohr counterargues that even in the case of the EPR experiment, the role of measuring instruments and the necessity of discriminating between them and quantum objects remain irreducible. For, even in this case, this role entails limitations concerning the *types* of measuring arrangements used in determining one or the other quantity in question, even if one does so in terms of *prediction* rather than measurement in accordance with EPR's criterion of reality. In the nonclassical version of his interpretation, any attribution of properties to quantum objects at any time (between measurements or during a measurement) becomes ambiguous, including that of any single property, rather than only a joint attribution of complementarity properties. As I noted, however, Bohr does not need this more radical epistemology for his counterargument to EPR. His *weakly ontological* position, held by him at the time in any event, is sufficient to respond to EPR. This position allows for an assignment, in accordance with the uncertainty relations, of one of the two conjugate measurable quantities (such as a position) to a given quantum object itself *at the time*—and, hence, on the basis—of measurement. This is sufficient to offer an interpretation of quantum phenomena and quantum mechanics that allows one to see quantum mechanics as a complete theory of these phenomena, while both quantum phenomena and quantum mechanics may be maintained to be local. Let me reiterate that such an interpretation is necessary for the overall case for complementarity Bohr wants to make in his reply. As concerns EPR's argument itself, Bohr assesses it in its own terms (whereby it is possible, on the basis of EPR's criterion of reality, to assign certain properties to quantum objects themselves even on the basis of predictions with certainty, rather than only measurements) and shows that they do not demonstrate what they claim to be possible, namely, that both properties in question can be assigned.

Bohr's critique of EPR's argument does, however, contain or imply the provision that EPR's assumption is valid only if these predictions are *in principle verifiable*. This is not always the case in quantum physics, since an alternative measurement—say, that of momentum—on a quantum object for which a prediction regarding its position was made would erase the possibility of ever verifying this prediction. In classical physics this difficulty does not arise, since we assume that we can assign to the object both quantities simultaneously in all circumstances. This is one of the qualifications of EPR's criterion that quantum phenomena require and that is not considered by EPR.[12] As will be seen in the

[12] It may be added that, although EPR consider quantum objects themselves, independently of their interactions with measuring instruments, their criterion of physical reality and logic could be applied to the quantities associated with the measuring instruments involved. Even in this case, however, one could still argue that, without proper qualifications, the use of the criterion would be ambiguous, and hence that the corresponding argument would not demonstrate either the incompleteness of quantum mechanics or, alternatively, its nonlocality, or the nonlocality of quantum phenomena.

next section, the qualification that our prediction should be in principle verifiable is especially crucial for ensuring the locality of quantum phenomena and quantum mechanics. This qualification virtually amounts to the argument that an assignment of any measurable quantity associated with quantum objects requires a measurement, rather than only a prediction, even if this prediction is exact.

EPR's main point is, again, that *both* conjugate physical quantities involved in the quantum mechanical formalism can be ascribed *to the same quantum object*, albeit by means of two *separate* procedures, rather than simultaneously within the same procedure. This possibility, they argue, allows one to conclude that quantum mechanics is either incomplete, since it does not allow for such an assignment, or else nonlocal. The main *counter*point, which disables this conclusion, is, I shall argue, that even such a separate attribution of the second quantity in question *to the same quantum object* by means of two separate procedures is never possible, once an attribution of the first one is made. This is, arguably, the single most crucial physical point behind—or, since Bohr does not expressly state it in this form, implied in—Bohr's argument in his reply, and it appears to be missed by most commentators on the exchange. Indeed, I am unaware of any commentary that addresses it. It is, however, essential in order to understand Bohr's thinking concerning the EPR experiment. As will be seen, EPR are not entirely unaware of this obstacle, when they remark in closing that their criterion of reality might not be "sufficiently restrictive," and say that "one would not arrive at [their] conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted.* On this point of view, since either one or the other, but not both simultaneously, of the quantities P and Q can be predicted, they are not simultaneously real." However, they see this restriction as implying the nonlocality of quantum phenomena or of "reality" (EPR 1935, *QTM*, p. 141). As I argue here, the situation concerning the application of their criterion to quantum phenomena is somewhat different as concerns qualifications this application requires. By the same token, nonlocality need not follow.

EPR's and Bohr's arguments do share the same initial assumption, taken as axiomatic by each. Either in the standard or in the EPR case, one can set up a quantum mechanical experiment in two alternative ways so as to predict (using quantum mechanics) *either* one *or* the other of the two conjugate measurable quantities associated with a given quantum object. In the EPR case, we can do so with probability equal to unity and without physically interfering with ("disturbing") the object itself at the time of this prediction. I shall call this "*assumption A*."

From the possibility of the alternative covered by *assumption A*, EPR conclude that *both quantities can be assigned* to the same quantum object, even though it may not be possible to do so simultaneously by means of quantum mechanics, which may be designated as "*inference E*" (for Einstein). Accordingly, quantum mechanics is incomplete (unless one allows for nonlocality), and EPR express a hope that a future theory would enable us to offer a more complete description of physical reality (EPR 1935, *QTM*, p. 141).

Bohr argues *inference E* to be impermissible, in view of the *conditions* under which any possible *association* of any measurable physical quantity with a quantum object can take place, most crucially the irreducible role of the measuring instruments in any such association. This role is irreducible even when such an *attribution* is made by means of predicting this quantity with certainty, in accordance with EPR's criterion of reality. Bohr assumes the possibility of such an attribution (under the constraints of the uncertainty relations) *for the purposes of his counter-argument*, provided, again, that such a prediction is in principle verifiable. In his own view at the time, an attribution of a given single property to a quantum object itself is possible at least on the basis of and at the time of measurement, as against his later view, according to which no such attribution is ever possible at all. However, Bohr argues that, given the irreducible role of measuring instruments, even if one operates under the assumption of the EPR criterion of reality, a realization of the two alternative situations of measurement in question, which are necessary for the respective (verifiable) predictive assignment of these quantities, would *always* involve *two incompatible experimental arrangements*. It follows that, once one of these two quantities is thus established, it is never, in principle, possible to establish the second one; there is no conceivable experiment that would allow us to do so. For doing so would inevitably involve, in the standard case, *two different quantum objects* of the same type, such as electrons or photons, or in EPR's case, two EPR pairs, whose behavior, rather than only their initial preparation (defined by the same state of measuring instrument), will be *fully identical* in following the course of the experiment. This, however, is not possible, I shall designate this inference from *assumption A* as *inference B* (for Bohr).

As discussed, in the case of the standard quantum experiment, in considering the quantum eraser experiments in Chapter 2, the reason for this impossibility is a well-established experimental fact, stressed throughout this study, that identically prepared quantum systems (in the sense of the state of measuring instruments) do not behave identically. Indeed, as discussed in Chapter 6, at no point can one assume that two quantum objects can be in the same physical state, even though the two experiments concerning them are identically prepared. These circumstances entail the irreducibly probabilistic nature of all quantum predictions, regardless of the theory used to make them. This situation does not appear to be sufficiently taken into account by EPR, whose argument tacitly depends on the possibility of identically repeating the EPR experiment. However, whatever prediction one has made in an already performed EPR experiment, say, for the pair (S_{11}, S_{12}), one would require a *different EPR pair* (S_{21}, S_{22}) for the measuring procedure, performed on S_{21}, necessary to make the alternative EPR prediction concerning S_{22}. This is also necessary if one wants to repeat the experiment in order to make the same prediction, in which case the outcome can never guaranteed to be the same and is usually different. This explains my double notations (S_{n1}, S_{n2}) in labeling the objects concerned. Accordingly, there is no physical situation in which one can ever assign both conjugate or complementary quantities to *the same object*—either simultaneously (the uncertainty relations) or separately in two possible alternative experiments—or to two fully identically behaving *objects*. No experiment will allow us to justify the validity of such an assignment. Bohr does not appear to make this last point, at least expressly, in this form, although it is a consequence of his argument that a realization of the EPR experiment or related experiments contemplated by Einstein would *always* involve *two incompatible experimental arrangements*.[13]

One can diagrammatically represent the situation as follows (for the continuous variables, although one can easily extend the argument to discrete variables as well). Let X and Y be two complementary variables in the Hilbert-space formalism ($XY - YX \neq 0$) and x and y the corresponding physical measurable quantities ($\Delta x \Delta y \approx h$); ($S_1$, S_2) is the EPR pair of quantum systems; and p is the probability of the prediction concerning a given variable for S_2, via the wave function on the basis of the measurement performed on the corresponding variable for S_1. The objects and quantities involved become doubled to (S_{12}, S_{21}) and (S_{21}, S_{22}), and correspondingly to (X_{11}, X_{12}), (X_{21}, X_{22}), and (Y_{11}, Y_{12}), (Y_{21}, Y_{22}) in the present view *(inference B)* of the situation, as against EPR's or Einstein's view *(inference E)*. Then,

The EPR experiment (EPR's and Einstein's view):

$$\begin{array}{ccc} S_1 & & S_2 \\ X_1 & \psi_1 \text{ (with } p = 1) \rightarrow & X_2 \\ Y_1 & \psi_2 \text{ (with } p = 1) \rightarrow & Y_2 \end{array}$$

This view represents *inference E*, whereby both X_2 and Y_2 could be assigned to S_2, making quantum mechanics incomplete (or if complete, nonlocal).

The EPR experiment (Bohr's and the present view):

$$\begin{array}{ccc} S_{11} & & S_{12} \\ X_{11} & \psi_1 \text{ (with } p = 1) \rightarrow & X_{12} \\ S_{21} & & S_{22} \\ Y_{21} & \psi_2 \text{ (with } p = 1) \rightarrow & Y_{22} \end{array}$$

This view represents *inference B*, which allows one to maintain both the completeness and the locality of quantum mechanics—or at least to argue that EPR did not prove otherwise, since their argument depends on the *inference E*, which is not experimentally obtainable.

[13] As I noted, the same argumentation would apply if one adjusted the situation in accordance with Bohr's ultimate view, in which case our predictions would be made concerning the data to be registered in the measuring instruments involved. There are no experiments that would allow us to make two separate predictions of the two conjugate variables *associated* with the same quantum object (S_2) as variables pertaining to these instruments, since the two arrangements necessary to do so are incompatible.

As I said, Bohr himself does not explain the situation in terms of the two different objects and EPR pairs necessary in order to make the second EPR prediction. This, however, is at least an implication of his argument, given his insistence (apparently lost on Einstein) in his reply and elsewhere 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" (Bohr 1949, *PWNB* 2, pp. 57, 60; Bohr 1935b, p. 699). In any event, the implication is automatic, in view of the mutual exclusivity in question, which is, again, due to the irreducible role of the measuring instruments.

That this mutual exclusivity of the two experimental arrangements involved—specifically in the EPR experiment—cannot be avoided needs to be argued, of course, and I shall properly explain Bohr's argument to that effect in the next chapter. That such is the case should, however, be expected and the argument itself easily surmised from the discussion given in this study thus far, beginning with the analysis of the double-slit experiment in Chapter 2. Indeed, as will be seen, Bohr's argument amounts to a straightforward examination (such as the one offered in Chapter 2) of the double-slit experiment, with only insubstantial additional considerations in the case of the EPR experiment. The main point is that, given the mutual exclusivity of the measuring arrangements involved, the second quantity in question cannot even in principle ever be assigned to the *same quantum object*, *once one such quantity is assigned.* This holds true even if one accepts EPR's criterion of reality and allows that such an assignment is made on the basis of a prediction, because once an experiment enabling one to make the first prediction is performed, the first object, S_{11}, is no longer available to make the second prediction in question. A new EPR pair would be needed to "repeat" the experiment, ab ovo, to get to the stage of the prediction in question, and, as I shall explain presently, such a prediction can never be guaranteed to be (and in general is not) properly coordinated with the first. For any given *single* quantum object, including the second object of the EPR pair, one can, in principle, predict or measure only *one* of the quantities involved—and *once one is predicted*, *never* the other. There is no conceivable experiment that would allow us to circumvent this doubling and its implications. This doubling is a manifestation of the irreducible individuality of all quantum phenomena, the "*individuality* completely foreign to classical physics" (Bohr 1935b, p. 697). Each quantum event is each time unique, singular and unrepeatable, which makes quantum mechanics—or, it appears, any theory that could predict such events—irreducibly statistical, including as concerns each such event individually.[14] Accordingly, while

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[14] One can make such an alternative determination for (i.e., measurements on or predictions concerning) the same quantum object sequentially. This possibility, however, does not change the present argument, since, as discussed in Chapter 2, it amounts to a change of the experimental setup, which makes the value of the first variable in the sequence completely undetermined at the time the second variable is determined. The first determination becomes irrelevant as concerns the properties or behavior of the object, once the second determination is made. This deprives us of the possibility of any classical-like repetition of the same experiments as concerns the behavior of quantum objects or the classical-like tracking of quantum objects.

EPR's reasoning of considering, counterfactually, the alternative determinations of variables to the same object applies *in the case of a single classical object*, it cannot, contrary to EPR's contention, be even meaningfully contemplated *in the case of a single quantum object*.[15]

Nor, correlatively, is it possible (in the way it is in classical mechanics) to coordinate the two experiments in question on two different objects so as to make it possible to consider both as rigorously identically prepared *objects*, or EPR pairs. This is, again, because in dealing with quantum phenomena, we can only control our instruments in the same way, but never the behavior of the quantum objects and, consequently, the outcomes of the experiments defined by this behavior, which thus become subject to probabilistic estimates only. In the EPR case, we can predict with probability equal to unity the first quantity in question—say, the value of the position variable—for the second object, S_{12}, of a given EPR pair (S_{11}, S_{12}). We can then predict the second quantity—the value of the momentum variable—for the second object, S_{22}, of, unavoidably, *another, "identically prepared"* EPR pair (S_{21}, S_{22}). However, we cannot coordinate these predictions in such a way that they could be considered as pertaining to two identically prepared objects in the way this could be done in classical physics. This is not possible since the necessary intermediate measurements would, in general, give us different data. Were we to repeat the measurement and the prediction of the first pair of quantities, those of the position variables for respectively S_{21} and S_{22}, we could still make our prediction with the probability unity, but, as I said, the outcome would, in general, not be the same as in the case of the first pair (S_{11}, S_{12}).[16] In other words, we *can predict* the outcome of a given EPR experiment with probability equal to unity but we *cannot repeat* such an experiment with probability equal to unity for the values of the corresponding outcomes. We can only coordinate such measurements and predictions statistically, and thus establish the EPR correlations (for continuous or discrete variables), as J. S. Bell realized, in the case of discrete (spin) variables

[15] These considerations and the present discussion as a whole, as well as the discussion of the double-slit and other experiments in Chapter 2, clearly bear on the question of counterfactual statements in considering quantum phenomena, such as those of the EPR type. This question—especially germane to Bell's or the Kochen–Specker theorem and related subjects—is beyond my scope. However, the analysis given here does imply certain limits upon the application of counterfactual logic to quantum phenomena in view of the irreducible role of measuring arrangements in determining the physical quantities considered. We cannot reason in the way we do in classical physics concerning the ultimate outcome of an experiment on the basis of what would have happened if a certain measurement that was not actually performed had been performed. Only actual measurement can affect and allows us to predict what will happen. For helpful discussions of counterfactual reasoning, including in the context of Bell's theorem or the Kochen–Specker theorem, see the works cited in Note 9, especially Mermin (1990) and Peres (1993).

[16] Schrödinger appears to have a sense of this aspect of the EPR situation in his letter to Einstein in the wake of EPR's paper (Schrödinger to Einstein, June 7, 1935, cited in *MR* 6, pp. 739–740). His cat-paradox paper, too, appears to reflect this sense (Schrödinger 1935a).

and Bohm's version of the EPR experiment.[17] This does not help EPR, since their argument—either for the incompleteness or, alternatively, for the nonlocality of quantum mechanics—de facto presupposes exact, rather than statistical, coordination of such variables as belonging to the same or an *identically prepared* object of the same or an identically prepared EPR pair. Quantum mechanics correctly predicts these statistical outcomes and hence corresponds to what obtains experimentally, while, as this argument shows, what EPR argue for *(inference E)* does not obtain experimentally. Accordingly, EPR's argument cannot be seen as demonstrating the incompleteness of quantum mechanics, and *it fails* to do so *because of the irreducible nature of quantum probability*, even it deals only with predictions with certainty. Indeed, as will be seen, Einstein ultimately accepted the validity of this type of argument, but he still saw quantum mechanics as incomplete (if now local), since it provides no description of the behavior of individual systems. It is as if in classical physics we would have only statistical mechanics without standard classical mechanics. The essence of Bohr's and the present argument is that in the case of quantum phenomena only probabilistic predictions are possible even in the case of individual quantum systems, even in idealized cases. Nature does not appear to allow us to do more, and hence, as at least as things stand now, it does not allow us to do more than quantum mechanics delivers.

Bohr's argument concerning "the essential ambiguity" of EPR's use of their criterion—and specifically that this ambiguity concerns EPR's expression "without in any way disturbing a system"—has caused some confusion (Bohr 1935b, p. 700). While Bohr's famous paragraph on the subject needs a careful overall reading (to be offered in the next chapter), Bohr's meaning in this particular clause poses no special difficulties for the present analysis. For, once one conjugate quantity in question is established (even on the basis of a prediction, in accordance with EPR's criterion of reality) for S_{12}, we cannot ever establish the second quantity involved without measuring and hence *disturbing* S_{12}. We can establish such a quantity only for a different quantum object, S_{22}, via a different EPR pair (S_{21}, S_{22}), by a different measurement on S_{21}—and these two determinations cannot be coordinated so as to assume that both quantities could be associated with the same object of the same EPR pair.

[17] As I indicated in Note 12, the argument given here could be nearly automatically transferred to Bohm's version of the EPR experiment and spin variables. In this case, too, any assignment of the alternative spin-related quantity to the *same* quantum objects becomes impossible, once one such quantity is assigned. An assignment of the other would require an alternative type of measurement, mutually exclusive with the first, on the first object of a given pair, and hence, at least, another fully identically behaving EPR–Bohm pair, which is, again, not possible or, in any event, cannot be guaranteed. Nothing else than statistical correlations between such assignments is possible, which is of course fully consistent with the fact of the Bell–EPR correlations themselves, which are statistical in any event (cf., Mermin 1990, pp. 107–108). The argument concerning locality given in the next section could be automatically transferred to the case of discrete variables as well.

The coordination of such events can only be statistical. Thus, the ambiguity in question indeed relates to the clause "without in any way disturbing the system," which requires a qualification here explained (but not provided by EPR), if one wants to apply it rigorously in the EPR situation. As Bohr says,

> In the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of [sacrificing] other such elements [for a given quantum object], but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum. Any remaining appearance of arbitrariness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. (Bohr 1935b, p. 699)

The *physical* meaning of the uncertainty relations and, thus, the use of Heisenberg's formula itself, $\Delta q \Delta p \cong h$, are adjusted by Bohr in accordance with this view in the wake of EPR's argument. As he argued in "Complementarity and Causality" (1937), echoing EPR's argument and by way of "warn[ing] against a misunderstanding likely to arise when one tries to express the content of Heisenberg's ... indeterminacy relation ... by such a statement as 'the position and momentum of a particle cannot simultaneously be measured with arbitrary accuracy:' "

> According to such a formulation it would appear as though we had to do with some arbitrary renunciation of the measurement of either the one or the other of the two well-defined attributes of the object, which would not preclude the possibility of a future theory taking both attributes into account on the lines of the classical physics. From the above considerations [following those of his reply to EPR] it should be clear that the whole situation in atomic physics deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to the object. On the contrary, the proper rôle of the indeterminacy relations consists in assuring quantitatively the logical compatibility of apparently contradictory laws which appear when we use two different experimental arrangements, of which only one permits an unambiguous use of the concept of position, while only the other permits the application of the concept of momentum defined as it is, solely by the law of conservation. (Bohr 1937, *PWNB* 4, p. 86)

Coming as it does following the Bohr–EPR exchange, this statement, nevertheless, nearly amounts to a counterargument to EPR on its own accord—in one paragraph, or rather, with the uncertainty relations introduced in 1927, a decade of thinking and a paragraph.

As I have said, Bohr was not unprepared to respond to EPR by his previous thinking concerning quantum phenomena, again, as early as 1925 and even before the introduction of quantum mechanics, and especially by his previous exchanges with Einstein. The experiments discussed in these exchanges show that the conditions of the EPR *type*—and hence the argument offered here—apply to *all* quantum predictions, although there are some inessential differences between the standard and the EPR case. The reasons for this applicability are as follows.

In any quantum measurement, we actually *predict* the value of either the position or the momentum associated with a quantum object *after* the object has already left the region of its interaction with the measurement apparatus, which interaction defines the data on the basis of which we make the prediction (Bohr 1949, *PWNB* 2, p. 57). Hence, we make this prediction ("at a distance") without *any further* interference with the quantum object under investigation, whether we assume that this quantity is already defined by this prediction, in accordance with EPR's criterion, or that it is yet to be defined by a subsequent measurement, in accordance with Bohr's ultimate view (Bohr 1949, *PWNB* 2, p. 57). In this sense—and, from the present view, only in this sense of the impossibility of a local tracking of quantum processes (rather than in the sense of any instantaneous physical influence between spatially separated events)—quantum mechanics is, as I said, nonlocal. As explained in Chapter 2, were we to interfere with this object after that initial interference, the new interference would erase this prediction as meaningful. The object and that (quantum) part of the measuring instruments that originally interacted with it become entangled. Or, again, more accurately, our actual and possible knowledge concerning them becomes entangled, analogously to our knowledge concerning the EPR objects. In other words, our knowledge of the state of the apparatus obtained by measurements becomes entangled with our possible knowledge (now provided by the wave function or density matrix, which define the mathematical "quantum state") concerning the outcome of a specified future measurement performed on this object. To cite Roger Penrose: "A measurement, after all, merely consists of the quantum state under consideration becoming entangled with a more extended part of the physical universe, e.g., with a measuring apparatus" (Penrose 2001, p. 290). In the present view (not that of Penrose), "quantum state" here would, again, only refer to the mathematical object—the state vector, defined by the wave function or density matrix—enabling our expectation concerning the experiment in question.[18]

In the EPR situation, which involves two quantum objects rather than a single object and a measuring apparatus, the case is a bit more complicated (given that we need to involve additional measurements on the first EPR object), but it is not fundamentally different (Bohr 1935b, pp. 699–700). The fact of the previous interaction between the objects involved, S_1 and S_2, brings the EPR and the standard case closer together. A certain quantum part of the instruments involved appears in the same role as the first object, S_1, of an EPR pair (S_1, S_2), which has previously interacted with the second object, S_2. The registered measured data relate to this part of the instrument in a way similar to the data obtained by performing the final measurement (there are, as will be seen, certain intermediate stages) on S_1. By the same token, a prediction we make on the basis of this data will concern an object (the one under investigation) separated from

[18] See (Haroche 2001) and (Haroche et al. 1997) for elegant experimental illustrations of this point.

this part of the instrument. Conversely, in the case of the EPR experiment, the first object, S_1, may be seen as analogous to—or may be treated as—a quantum part of a measuring instrument, although one would still need to add a proper classical part to it (which is essentially done by performing a measurement on it, since this measurement involves a measuring apparatus). In this case, as against that of the standard measurement, we are no longer concerned with predictions concerning S_1 but only with those concerning S_2, which is what makes the analogy in question possible. Bohr came to realize this analogy at least by the time of the Warsaw lecture, as is clear from his arguments from that point on (e.g. Bohr 1938, *PWNB* 4, pp. 101–103; Bohr 1949, *PWNB* 2, p. 60).

Of course, in the standard case, as opposed to that of the EPR experiment, a prediction in question—concerning, say, the location of a collision between the object and the silver bromide screen—will not, in general, have a probability equal to unity, but some other probability. In order to get a probability equal to unity, one would need to perform some intermediate measurements, just as one would in the EPR case. Such additional measurements are, in general, not possible in the case of the standard quantum measurement, since the quantum part of the apparatus, with which the object under investigation interacts, cannot be sufficiently defined to create a traceable EPR situation. It is only possible to treat the whole apparatus involved as a quantum object and thus reproduce the EPR case—which, however, would require another measuring apparatus (Bohr 1938, *PWNB* 4, p. 104; Bohr 1949, *PWNB* 2, p. 60).

These differences, however, do not affect my main point at the moment. All our predictions concerning quantum objects, that is, concerning their future interactions with measuring instruments, are defined by their previous interactions with other quantum objects, whether a part of the quantum strata of measuring instruments or one of the objects of the EPR pair. By the same token, these predictions are defined only by a measurement performed on this other object, which is, at the time of the measurement enabling the prediction, spatially separated from the object under investigation. That interaction, as it were, endows the object with which we interfere in a measurement with all the potential information necessary for such a prediction, information that we acquire through this measurement.

Moreover, as discussed in Chapter 2, we can always (in a delayed-choice manner) make our plans and set up one arrangement or another after this interaction has already taken place (Bohr 1935b, pp. 698–699; Bohr 1949, *PWNB* 2, p. 57). The situation thus becomes more strictly parallel to that of the EPR case, in which we decide, after the interactions between S_1 and S_2 took place, whether to measure the position or, conversely, the momentum associated with S_1 and hence which prediction to make concerning S_2, and make the corresponding, mutually exclusive, experimental arrangements. In either case, once this determination has been made, any possibility of making an alternative prediction concerning the other conjugate variable associated with this second object, or again, any quantum object concerning which we make a prediction, is unavoidably precluded. Accordingly it is, in principle, impossible to speak of an alternative determination concerning the same object. Such alternative determinations could only concern two different quantum objects, just as in the EPR experiment.

Bohr appears to have realized the key aspects of the quantum mechanical situation in question in EPR's argument in his earlier exchanges with Einstein in the standard case of quantum mechanical measurements. In his reply, too, before proceeding to his argument concerning the EPR experiment itself and in order to ground this argument, he offers a detailed analysis of the standard case. His account in "Discussion with Einstein" (reflecting the first set of these exchanges during the Solvay conference in Brussels in October 1927) suggests this development of his thinking. On the other hand, the language of "effects" he uses in his account appears to reflect his post-EPR thinking, which inflects most of Bohr's later writings, even when technically referring, as on this occasion, to his earlier discussions with Einstein. As Bohr says,

> This point [of the double-slit experiment] is of great logical consequence, since it is only the circumstance that we are presented with a choice of *either* tracing the path of a particle *or* observing interference *effects*, which allows us to escape from the paradoxical necessity of concluding that the behavior of an electron or a photon should depend on the presence of a slit in a 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 behavior of atomic objects and their interaction with the measuring instruments which serve to define the conditions under which the phenomena occur. (Bohr 1949, *PWNB* 2, pp. 46–47; emphasis on "effects" added)

Of course, in order to observe interference effects we would need many repeated trials, but, as explained earlier, our probabilistic prediction for any single particle would be affected by what kind of arrangement we make as well. The situation acquires further features relevant to the EPR experiment in the discussion of the famous photon-box experiment proposed by Einstein and subsequent (refined) arguments by Einstein, leading him to his arguments of the EPR type. What makes Einstein's new argument especially significant is the shift from the question of a possible *measurement* of both conjugate variables to that of a possible *prediction* of both. Originally, the argument was formulated as follows, according to Bohr:

> Einstein proposed the device ..., 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. (Bohr 1949, *PWNB* 2, p. 53)

Einstein apparently accepted Bohr's counterargument showing, by using Einstein's own general relativity, that one cannot really circumvent the uncertainty relations in this way (*PWNB* 2, pp. 53–55). The tightness of this counterargument has been questioned sometimes, although the counterargument appears, at least to the present author, to be sufficiently tight. Be that as it may, Bohr's counterargument is crucial for the development of Bohr's thought, both in its own right and as part of the sequence it forms with the next stages of the exchange, proceeding via experiments of the EPR type and discussed in this chapter. Accordingly, of principal interest here is Bohr's essential logic of differentiating between those parts of measuring instruments and [quantum] objects under investigation and his conclusion. He summarizes this logic as follows: "The discussion, so illustrative of the power and consistency of relativistic arguments, thus emphasized once more the necessity of distinguishing, in the study of atomic phenomena, between the proper measuring instruments which serve to define the reference frame and those parts which are to be regarded as objects under investigation and in the account of which quantum effects cannot be disregarded" (Bohr 1949, *PWNB* 2, pp. 55–56). His conclusion is that, in accordance with the (time–energy) uncertainty relations (again, seen as an experimental given), "a 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" (Bohr 1949, *PWNB* 2, p. 55). Two mutually exclusive arrangements would be required for an exact measurement of each variable, with an obvious implication that the same logic would apply to any pair of complementary variables. Hence, as I argue here, an assignment of such quantities would require at least two *identically* behaving quantum objects, and there is no experimental arrangement that can ever guarantee such a behavior in identically prepared experiments. This fact disables considering the variables in question as pertaining to the same objects and prohibits any coordination of such events other than statistical.

Anticipating EPR's argument and criterion of reality, Einstein's follow-up argument shifts the questioning toward *predictions* rather than measurements, which, in Bohr's words, "might seem to enhance the paradoxes [i.e., the conflict with the uncertainty relations] beyond the possibilities of logical solution." According to Bohr:

> Thus, 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. (Bohr 1949, *PWNB* 2, p. 56; emphasis on "predictions" added)

Bohr also observes that “paradoxes of the kind contemplated by Einstein are encountered also in such simple arrangements [as those used in the double-slit type of experiments]. In fact, after a preliminary measurement of the momentum of the diaphragm, we are in principle offered the choice, when an electron or a photon has passed through the slit, either to repeat the momentum measurement or to control the position of the diaphragm and, thus, to make predictions pertaining to alternative subsequent observations” (Bohr 1949, *PWNB* 2, p. 57). He adds a comment that anticipates the delayed-choice experiment: “It may also be added that it obviously can make no difference, as regards observable effects obtainable by a definite experimental arrangement, whether our plans of constructing or handling the instruments are fixed beforehand or whether we prefer to postpone the completion of our planning until a later moment when the particle is already on its way from one instrument to another” (Bohr 1949, *PWNB* 2, p. 57).

While Bohr acknowledged that “once more Einstein’s searching spirit has 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,” he could not agree with “the trend” of Einstein’s argument (Bohr 1949, *PWNB* 2, pp. 56–57, 59). The reasons for Bohr’s problems with this “trend” should be clear in view of the preceding discussion. For, the experiments in question would always have involved two mutually exclusive arrangements and, hence, two different quantum objects, again, just as they do in the EPR case (where we need two different EPR pairs of quantum objects). Accordingly, Einstein’s argumentation does not adequately correspond to the situation that obtains in quantum phenomena. As Bohr says,

> [W]e 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 transfer by a 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 prediction 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. (Bohr 1949, *PWNB* 2, p. 57)

Self-evidently, these two mutually exclusive or complementary situations of measurement cannot apply to the same quantum object. Accordingly, the situation and the arguments, both that of Einstein and that of Bohr, are analogous to those of the EPR case, as Bohr indeed says: “[W]e are here [in the EPR case] dealing with problems of just the same kind as those raised by Einstein in previous discussions” (Bohr 1949, *PWNB* 2, p. 59).

It is true that in the EPR case the predictions in question are enabled by the measurements performed on a different—rather than, as in the argument just considered, the same—quantum object, which has previously been in interaction with the object under investigation. This is what allows EPR to speak, in accordance with their criterion, of predictions "without in any way disturbing the system" and, moreover, predictions with certainty (with probability equal to unity), in an apparent contrast with the standard situation. The very progression of Einstein's argumentation—from the measurement to the prediction of the conjugate quantities associated with a given object in the standard situation, and then to the predictions concerning such quantities associated with a separate quantum object via the EPR experiment—is important as well. The EPR situation, however, is essentially analogous to the standard one. The standard predictions are always made concerning the object—analogous to the second object, S_2, of a given EPR pair—which, at the time of the prediction, is spatially separated from the measuring devices, whose quantum stratum plays the role analogous to that of the first object, S_1, of the pair, upon which we perform a given EPR measurement. Hence, Bohr's argument applies in both situations, and in his reply he considers a particular (double-slit) measuring arrangement for the EPR experiment, which makes this parallel especially apparent. There are, again, differences concerning the probabilities involved, but they do not affect the essential points in question. In sum, although ingenious, the EPR contrivance of making such predictions on the basis of performing a measurement on a different quantum object so as not to interfere with the object in question does not change the essential aspects of the situation and hence does not help their case.

8.4 Can Quantum-Mechanical Description of Physical Reality Be Considered Local?

Thus far I have considered only the question of completeness of quantum mechanics. One must still consider EPR's and Einstein's argument for the nonlocality of quantum mechanics, which I shall do now. EPR's argument concerning this alternative reveals analogous problems to those discussed in the preceding section, and thus can be similarly shown to be insufficient once one properly considers the nature of quantum phenomena and specifically the irreducible role of measuring instruments in their constitution.

In closing their article, EPR acknowledge that they did not demonstrate that one could ever *simultaneously* ascertain by measurement or prediction both quantities in question for the same quantum object, such as S_1 or S_2 in their experiment. However, they see this qualification as implying the nonlocality in the EPR situation and hence as unreasonable. According to them,

> One could object to this conclusion [that the quantum-mechanical description of physical reality given by wave functions is incomplete] on the grounds that our criterion of reality is not sufficiently restrictive. Indeed, one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted.* On this point of view, since either one or the other, but not both simultaneously, of the quantities

> *P* and *Q* can be predicted, they are not simultaneously real. This makes [in the EPR case] the reality of *P* and *Q* depend upon the process of measurement carried out on the first system, which does not disturb the second system in any way. No reasonable definition of reality could be expected to permit this. (EPR 1935, *QTM*, p. 141)

It is worth noting first that the last sentence, one of the most often quoted (but not always in its proper context) sentences of EPR's article, is not an indictment of quantum mechanics as a physical theory, as is sometimes thought, although it does carry the implication of its incompleteness. It is an indictment of nonlocality as a possible view of reality, including in those cases when it is accepted in order to maintain the completeness of quantum mechanics. In Einstein's subsequent arguments this position usually serves as a starting point, from which he proceeds to argue, along the lines of EPR's argument, that under this assumption quantum mechanics cannot be considered complete by his criteria, again, roughly of the EPR type.[19]

Now, nonlocality does indeed follow, if, along with the completeness of quantum mechanics, one assumes, as EPR do, that the measurement, say, of P, on S_1 *fixes the physical state* or reality of S_2 by "a spooky *action* at a distance," rather than allows for "a spooky *prediction* at a distance" by fixing *the possible conditions* of such a prediction. It is true that in this latter view, taken by Bohr and adopted by the present study, quantum mechanics becomes a strictly predictive, and moreover, only probabilistically predictive theory concerning our knowledge regarding a future state of a given system, or even only of a measuring apparatus impacted by that system, which is hardly acceptable to Einstein either. The question is, again, whether nature allows us to do more. For the moment, EPR reason that, if quantum mechanics is assumed to be complete, a given measurement—say, that of position, Q, on S_1—and the wave function, cum Born's rule, give us as much knowledge as possible concerning the physical state of S_2, in this case leaving the momentum for S_2 completely unknown. This is correct (leaving aside for the moment the question whether this knowledge concerns the *actual physical state* of S_2 or the best knowledge we can have concerning its *possible state*). Since, however, we can also perform an alternative measurement, that of momentum, P, and use the wave function and Born's rule to predict the momentum for S_2, now with the position of S_2 left completely unknown, the state of S_2 is different. Accordingly, the state of S—established, again, without in anyway disturbing it—depends on the measurement performed on the spatially separated system S_1. Hence, quantum mechanics is, if complete, nonlocal. Under EPR's assumption, an alternative measurement—say, in the first case (when Q is predicted), of P on S_2—would discontinuously change this fixed state, although EPR do not examine this last eventuality, which is, however, an automatic consequence of their argumentation, as Einstein appears to have realized later (e.g., Einstein

[19] His earlier arguments in his exchanges with Bohr and his initial conception of the EPR experiment, as he suggested to Rosenfeld in 1933, appear to have been centered on nonlocality as well (Rosenfeld 1967, p. 114; cited in *QTM*, p. 137).

1949a, p. 81). So did, more immediately, Schrödinger in his cat-paradox paper (Schrödinger 1935a, pp. 164–165). It may be noted that, in this case, the situation reflected in the uncertainty relations is left intact (at the cost of nonlocality), rather than being de facto, albeit not in practice, circumvented as it is in EPR's preceding argument.

Their key point here, however, is that the physical reality of S_2 depends, as it does under this assumption, on whether we measure P or Q of S_1, and it is differently determined accordingly. This entails nonlocality, assuming that quantum mechanics is complete. Or, as Einstein argued later, one is left with a paradoxical situation insofar as (assuming that quantum mechanics is complete) two mutually incompatible states could be assigned to the same quantum system. The paradox, Einstein said, is resolved by assuming that the state of S_2, as defined by the wave function, is determined by a measurement performed on a spatially separated object, S_1, or, more strongly, "by denying altogether that spatially separated entities posses independent real states," either of which assumptions he saw as "entirely unacceptable" (Einstein 1949a, p. 81). Yet another alternative would, again, be an assumption that quantum mechanics is a theory of ensembles, rather than individual systems, which would still make it incomplete. I shall discuss Einstein's subsequent arguments below and this last alternative in Chapter 10.

The reasoning just explained is why EPR argue that, if quantum mechanics *is* complete by their *criterion of completeness ("every element of the physical reality must have a counterpart in the physical theory")*, then the physical state of a system, here S_2, could be *determined* by a measurement on a spatially separate system, S_1, in violation of locality. By the same token, their *criterion of reality* no longer applies in its original ("not sufficiently restrictive") form, and becomes modified so as to state "that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted.*" If quantum mechanics is local, then (they believe) their preceding argument, based on their criterion of reality, shows that it is incomplete. This logic is a bit convoluted, and Einstein streamlined it in his later arguments, but it is correct if their argument concerning the incompleteness of quantum mechanics works. In other words, EPR and Einstein in his later argument accept the possibility of argument of the type offered in the preceding section to the effect that in the case of quantum phenomena the quantities in question could not be established simultaneously. If such is the case, then quantum mechanics could be seen as complete. However, unlike Bohr or the present author, they then conclude, by reasoning along the lines just explained, that in this case quantum mechanics is nonlocal. Einstein, as I said, thought that Bohr in fact accepted the alternative of locality vs. completeness, and retained completeness by allowing for nonlocality, which would of course also imply that Bohr accepted that part of EPR's argument that was based on their initial less restrictive criterion of reality (Einstein 1949b, p. 681). However, Einstein misread Bohr, who, again, only allows for a spooky *prediction*, and *not action*, at a distance, and hence sees quantum mechanics as local.[20] As Bohr stated in his reply, for him "the singular position of measuring instruments in the account of quantum phenomena . . . together with the relativistic invariance of the uncertainty relations . . . ensures the compatibility between [the] argumentation [of his reply] and all exigencies of relativity theory" (Bohr 1935b, p. 701; 701n.; also Bohr 1958, *PWNB* 3, p. 3). This compatibility enables Bohr's critique of EPR's argument for the incompleteness of quantum mechanics—and hence the argument given in the preceding section—without sacrificing locality.

There is a subtle difference between determining the state *by a prediction* and doing so *on the basis of a prediction*. In Bohr's own view, physical states cannot be seen as properly determined (even when we have predicted them exactly) unless either the actual measurement is made (later on, but not in his reply, this becomes the necessary condition of any such determination for Bohr) or there is a possibility of actually *verifying* the prediction. In other words, it must be possible to perform an actual measurement that would yield the predicted value.[21] This condition in turn becomes a necessary qualification of EPR's criterion of reality in the case of quantum phenomena. Hence, even though it is impossible ever to *make* both predictions in question for the same quantum object, the question of the *verification* of an EPR's prediction, or any quantum mechanical prediction, remains important. For, if one assumes the validity of EPR's criterion in its original (unrestrictive) form, the measurement of the alternative quantity, Q, on S_2 would automatically disable any possible verification of the original prediction, by virtue of the mutual exclusivity of the two measuring arrangements involved. In other words, once this alternative measurement is performed, the original prediction becomes meaningless as in principle unverifiable; it is erased for any meaningful purposes—for example,

[20] On a later occasion (in 1953), in his letter to Born, Einstein does speak of "expectations" at a distance. He describes "the paradox" of nonlocality in the sense "that a measurement carried in *one* part of space determines the *kind* of *expectations* for measurements carried out later in *another* part of space (coupling part of systems far apart in space)" (Letter to Born, December 3, 1954, Born 2005, p. 205). The paradox itself, however, appears to be the same as just mentioned, namely, that (if quantum mechanics is complete) two mutually incompatible states could be assigned to the same individual quantum system, S_2, depending upon which measurement we perform on S_1. Einstein also notes there that this paradox is eliminated if quantum mechanics is a theory of ensembles, the point to be discussed in Chapter 10 (Letter to Born, December 3, 1954, Born 2005, p. 205). The term "expectations," perhaps motivated by these statistical considerations, may appear to bring Einstein's argument closer to the one offered here. However, Einstein's argument remains different. He clearly still has the implications of nonlocality in mind, if one considers individual quantum systems; he believes that two such expectations could concern the same object and argues for nonlocality on this basis. Indeed, if Einstein took a position similar to the one taken here, there would be no paradox.

[21] We are here, again, within the limits of Bohr's earlier thinking, which allows an assignment (under the constraints of the uncertainty relations) of certain single measurable quantities to quantum objects themselves, as against Bohr's later thinking, which suspends such an assignment.

for any further predictions concerning either object, S_1 or S_2. As Bohr notes in the Warsaw lecture, clearly with EPR's argument in mind (which he discusses about a page earlier), the same type of logic applies even if we actually know, say, the momentum of a quantum object, as already established by measurement. As he says,

> [It] must be remembered that any well-defined phenomenon involves the combination of several comparable measurements. The significance of this point is strikingly exemplified by the case, often discussed, of the possible determination of the position of a particle with known momentum by the spot produced by its impact on a photographic plate. Far from meeting any contradiction with the uncertainty relations, we have clearly here to do with a measuring arrangement which is not suited to define a phenomenon involving *a test of predictions* as regards the location of the object. In conformity with the uncertainty relations, the *knowledge* of its momentum prevents in fact any unambiguous connection between this object and the frame of reference with respect to which the position of the photographic plate is defined. (Bohr 1938, *PWNB* 4, p. 103; emphasis added)

In other words, the position thus measured is incompatible with the object as defined by the known momentum. If, conversely, we consider this object to be defined by the position, our knowledge of this momentum is erased and is no longer relevant for the new definition, and hence, the known value of the momentum for the purposes of any new prediction possible on the basis of this new definition. That is, the particle whose position is established by the collision with the screen is no longer a particle with a known momentum, since there is no way to verify in principle its momentum at the time of collision. As discussed in Chapter 2, this fundamental "erasure" of the object's previous state by a new measurement for the purposes of any subsequent predictions concerning this object is the main difference between classical and quantum measurement or behavior. This erasure is also correlative to the impossibility of repeating an identically prepared experiment with the same or a properly correlated outcome—the impossibility in question in this chapter. An object with a known momentum may be a legitimate concept in quantum physics, just as it is in classical physics, but it is meaningful only insofar as a given measurement enables (as Bohr puts it in his reply to EPR) a corresponding "correlation between its behavior and some instrument" (Bohr 1935b, pp. 699–700). As explained above and as Bohr clearly implies in this statement, this correlation is found in classical physics as well. The difference is that measuring a conjugate quantity, that of the object's position, still allows us, at least in principle, to measure the momentum simultaneously—and hence to ascertain both qualities, simultaneously. This is never possible in the case of quantum phenomena, since, as I argue here, an alternative measurement always erases our previous predictions altogether, making irrelevant the information that enabled us to make those predictions. The EPR experiment makes this situation all the more striking, though without preventing one from maintaining the locality of quantum phenomena and quantum mechanics.

This erasure of quantum predictions by an alternative measurement might have been yet another reason why Bohr was ultimately compelled to see any measurable quantities in question in quantum theory as determined only through measurement and never through prediction, and as pertaining only to the (classical) physical aspects of measuring instruments involved or to phenomena in his new sense. The impossibility of ever verifying an actual spatial–temporal attribution of even a single property to quantum objects themselves (as opposed to measuring instruments) and even at the time of measurement (rather than only independently) appears to have been another key factor. On both counts, however, the possibility of, in principle, verifying a given prediction appears to be crucial to Bohr's thinking concerning the quantum mechanical situation. As quantum experiments often do, the EPR experiment forces us to pay more careful attention and rethink the nature of our most basic concepts. For what does it mean to predict something? Such a concept is only meaningful when a prediction is in principle verifiable. If there is no possibility in principle to verify a prediction, the application of the concept cannot be considered meaningful, as is the case when a measurement of the alternative quantity to that of the predicted one is performed in either a regular case or the EPR one.

As I noted earlier, Einstein's later arguments of the EPR type are sometimes viewed as offering a stronger or even a different case against quantum mechanics, especially by focusing more sharply on the question of nonlocality (e.g., Einstein 1936; Einstein 1948, in Born 2005, pp. 166–170, 204–205, 210–211; Einstein 1949a, pp. 77–85; and, in commenting on Bohr's reply, Einstein 1949b, Schilpp 1949, pp. 681–682). It is difficult to disagree that these arguments streamline EPR's argument, in part by this shift of focus. One the other hand, they still make their case in terms of the alternative between locality and completeness along the lines of EPR's argument, as just considered. I would, accordingly, argue that these arguments do not really make a substantively different case. Einstein, too, never claimed otherwise, but thought that these arguments bring into sharper focus the essential features of EPR's argument. The main point here is that these arguments of Einstein never consider—any more than EPR's argument does—that the alternative determinations of one or the other complementary conjugate variable for the second distant object of an EPR pair can never, in principle, be established for the same quantum object. It is true that these arguments add analyses of quantum mechanics as a theory of ensembles, but as I shall explain in Chapter 10, this possibility is equally problematic for Einstein.

For the moment, with the discussion just given in hand, one can easily see why Bohr argues in his reply—clearly with EPR's alternative between locality and completeness in mind—that it is not a question of a physical or, as he says, "mechanical" influence of the measurement on S_1 upon the physical state of affairs concerning S_2.[22] As he says: "Of course there is in [the EPR case] no

[22] "Mechanical" is a precise term, insofar as it refers to an influence of one individual object or system upon another.

question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage [the last critical stage of the measurement in question on S_1] there is essentially the *question of an influence on the very conditions which define the possible types of predictions* [by making a measurement on S_1] *regarding the future behavior of the system* [S_2]" (Bohr 1935b, p. 700; also Bohr 1935a, *QTM*, p. 144). In other words, contrary to the common surmise (beginning with that of Einstein), the "influence" invoked here should not be seen as an influence, *at a distance*, of the measurement performed on S_1 upon the spatially separated situation of S_2, although this measurement defines our predictions concerning the corresponding future measurement on S_2. It might be that Bohr's choice of the word "influence" was not the best and was in part responsible for a common confusion concerning his point on the part of commentators. His meaning, however, is clear from his elaboration itself and from the preceding analysis from which this conclusion is derived, to be discussed in detail in Chapter 9. This analysis is not always carefully considered by Bohr's readers either, which does not help in clearing the confusion concerning Bohr's elaboration. The "influence" in question is defined by strictly determining, *fixing*, the overall experimental conditions in the EPR situation, by the particular measurement on S_1 as mutually exclusive for each physical quantity in question, which, accordingly, strictly determines the kind of quantity that can be predicted for S_2. In other words, this influence pertains to *the conditions of the measurement on* S_1 and, correspondingly, *the prediction concerning* S_2, the only possible prediction by virtue of this measurement. It is an influence upon the determination of one possible experimental setup, as opposed to the other possible setup. We can never combine the two setups. Once one is in place and defines the measurement on the first object, any determination of the second quantity becomes impossible. An alternative arrangement, which would make this type of *determination* possible, would inevitably involve a different quantum object. Our decision, which follows one or the other of the two possible determinations in question, *influences* what kind of prediction concerning S_2 is possible, even though we do not interfere with it, while Bohr clearly takes the requirements of relativity and, hence, of locality as axiomatic, just as Einstein does. This is why, as I have stressed from the outset, the alternative choice part of complementarity in the narrow sense is as important the mutual exclusivity of the two alternative possibilities in question, and, as will be seen in the next chapter, it is crucial to Bohr's counterargument to EPR.

It is also suggested sometimes that EPR's closing elaboration on locality anticipated Bohr's reply, or that Einstein's subsequent arguments along these lines adequately respond to Bohr's argument, which, as I suggested, might have been Einstein's view, given the way in which he read and, I argue, misread Bohr's reply. In particular, this suggestion is based on the fact that EPR admit that their argument depends on their criterion of reality and would not work if this criterion is suspended, as it is, it is also suggested, in Bohr's reply, or, again, by EPR themselves in the end of their article. It should be apparent from the preceding analysis that it is difficult to sustain this argument, whether as concerns Bohr's counterargument in his reply or as concerns the kind of arguments Bohr offers via his concept of phenomenon later, which arguments do indeed suspend EPR's criterion altogether (rather than restrict it in the way EPR do in closing their article). Neither of these arguments appears to be anticipated either by EPR's statement in question or by Einstein's later arguments. However, it might be useful to comment on the subject further, since it allows one to bring the nature of the two types of argumentation in question—that of Einstein and that of Bohr—into sharper relief.

Again, the main reason for my reservations concerning the claim that EPR anticipate Bohr's counterargument is that from a *possible* impossibility of ascribing both quantities in question to the same quantum object (which impossibility would be actualized if quantum mechanics were complete), EPR deduce the nonlocality of quantum mechanics or of quantum phenomena. They find this inadmissible, and, accordingly, reject that impossibility and, with it, the completeness of quantum mechanics. The same type of deduction is invariably found in all of Einstein's arguments concerning the EPR type of experiments. Bohr's arguments in his reply and (via his concept of phenomenon) in later works are quite different. The qualifications of EPR's criterion offered or suggested in his reply, like his suspension of this criterion in his later work, are accompanied by the argument, discussed here, that allows one to both maintain the locality of quantum mechanics (or of quantum phenomena) and to preserve the completeness of quantum mechanics. Or again, at least these qualifications allow one to show that EPR's or Einstein's later arguments are insufficient to demonstrate otherwise on either count.

Now, as indicated earlier, Einstein speaks of the view defined by the assumption (at least de facto) of nonlocality as that of "the physicist B" (perhaps also standing for Bohr), while "the physicist A" may be standing for Albert Einstein (Einstein 1949a, p. 79). He then proceeds to the same deduction of nonlocality from this assumption and, since nonlocality is impermissible, to argue that quantum mechanics must be seen as incomplete, at least as a theory of "individual single systems" (Letter to Born, January 15, 1954, Born 2005; also Einstein 1949a, p. 81). The question of individual systems vs. ensembles in quantum theory is, again, important, but it does not affect my point at the moment, which concerns Einstein's and Bohr's handling of individual quantum systems. The article contains Einstein's arguably final statement on the subject, strongly "assert[ing]" (Einstein's word) that quantum mechanics, as a theory of individual systems and processes, is either incomplete or else nonlocal. Nowhere in Bohr's arguments concerning EPR's experiment does one find any evidence of, or allowance for, such a deduction; nowhere does the assumed completeness of the quantum mechanical account logically lead to the nonlocality either of quantum mechanics itself or of quantum phenomena. Contrary to Einstein's supposition, Bohr accepts neither nonlocality nor Einstein's argument concerning the very alternative of locality and completeness—which, Einstein claims, "the physicist B[ohr]," who maintains the completeness of quantum mechanics, should accept along with "the physicist A[lbert]" (Einstein 1949a, p. 81). Bohr takes an assumption of locality for granted, just as Einstein does—but, unlike Einstein, he argues that quantum mechanics may be seen as both local and complete or at least, again, that Einstein did not prove otherwise. Bohr and Einstein share a commitment to locality, but not to realism or ontology.

Bohr, I argue here, offers an entirely different analysis of the EPR experiment. He does contend that the application of EPR's criterion of reality (which he, again, accepts for the purposes of his counterargument) requires qualifications vis-à-vis its use by EPR in their argument in order to avoid an essential ambiguity found in that use. However, these qualifications are altogether different from those contemplated by EPR in the end of their article and are based on the experimental facts or the nature of quantum phenomena, in this case *in the conventional sense* of "phenomenon." These qualifications are retained in Bohr's later arguments concerning the EPR experiments based on his concept of phenomena, although certain aspects of this concept, including the suspension of any specifiable ontology from quantum objects, are an interpretation. It is worth noting, however, that even in the restricted form of their criterion under discussion, EPR do not suspend their criterion as such (i.e., an element of reality is still assigned if a corresponding *prediction* can be made with certainty); they merely qualify it by a restriction on its simultaneous application. In Bohr's later view, this type of prediction-based criterion is suspended altogether, and moreover, any physical quantities pertain strictly to certain parts of measuring instruments and never to quantum objects themselves. This is not something that EPR and Einstein had ever contemplated. Bohr's arguments allow him to avoid the type of implication of nonlocality argued for by EPR and by Einstein in his subsequent arguments. In sum, it does not appear that either EPR's article or any of Einstein's subsequent arguments on the subject ever anticipate any of Bohr's arguments. Nor, again, does Einstein adequately adhere to Bohr's counterargument in his commentary on it, in part translating it into his "own way of putting it" (Einstein 1949b).

It is true that neither in his reply to EPR nor elsewhere in his discussions of the EPR experiment—such as (in terms of his concept of phenomenon) in the Warsaw lecture (1938) or "Discussion with Einstein" (1949)—does Bohr *directly* address the question of locality, as perhaps he should have done. Part of the reason appears to be that (in EPR's article in particular) it is the question of completeness that is the main focus of Einstein's argument, and this question is raised in all of Einstein's arguments, in contrast to more recent discussions (following Bell's theorem), which usually assume quantum mechanics to be complete and center solely on locality. Bohr, especially in his reply to EPR, responds primarily to this question of completeness of quantum mechanics, which appeared to have posed greater concerns at the time (again, the situation changed with Bell's theorem). Given that all of Einstein's arguments on the subject have essentially the same logic as EPR's argument (apart from Einstein's added consideration of quantum mechanics as a theory of ensembles), it is not surprising that Bohr did not feel a need to reply to them separately, beyond a few clarifications added in the Warsaw lecture and in "Discussion with Einstein." Developing a more sustained critique of these arguments might have compelled him to address locality more directly. It is clear, however, that Bohr is well aware of the question of locality. It would be highly surprising were it otherwise, given that some of his exchanges with Einstein directly involve Bohr's arguments based on relativity and that most of his writing addresses the question of the epistemological positions of relativity and quantum mechanics vis-à-vis classical physics or vis-à-vis each other. As I argue here, he takes the requirement of relativity and hence locality as axiomatic, just as Einstein does. He expressly asserts in his reply "the compatibility between [his] argument and *all exigencies* of relativity theory" (Bohr 1935b, p. 701n.; emphasis added). Bohr's reply to EPR makes clear that the locality of quantum mechanics is assured by the irreducible role of the measuring instruments and is in no way undermined by EPR's or Einstein's argument.

The irreducible role of measuring instruments, of rods and clocks, is what assures the structure of relativity theory, special or general. In both cases, relativity and quantum mechanics, the mathematical formalism of the theory coherently covers the situation of measurement, as Bohr notes at the same juncture of his reply in closing his argument on a parallel between the two theories (Bohr 1935b, pp. 701–702). Bohr must have felt great frustration (there is evidence he did) that he failed to successfully convey to Einstein this significance of measuring instruments in the constitution of quantum phenomena, which Einstein clearly bypassed in his comments on Bohr's reply and throughout his arguments (Einstein 1949b, p. 681). However, that might have been Einstein's problem, rather than Bohr's.

Chapter 9
Essential Ambiguity and Essential Influence: Reading Bohr's Reply to EPR

Abstract This chapter offers a detailed reading of Bohr's reply to EPR, which is one of the most important, and one of the most difficult and controversial of Bohr's works. The present reading is arguably the first ever at this level of detail; and, in addition to supplementing the analysis given in Chapter 8, it aims to clarify some of the difficulties, acknowledged by Bohr himself, that the article customarily poses for its readers.

9.1 Framing the Argument

There are several reasons for undertaking the task of a detailed reading of Bohr's reply to EPR, even though Chapter 8 is nearly sufficient to make my case concerning Bohr's argument there.

First, a major part in Bohr's reply is his argument for the unavoidably mutually exclusive or complementary nature of the two measuring arrangements involved in any measurement or prediction concerning conjugate physical quantities associated with quantum objects. This argument needs to be carefully considered to complete the argument of Chapter 8, and doing so requires a close reading of the corresponding elaborations on Bohr's reply.

Second, as indicated in Chapter 8, Bohr realized that in order to make a rigorous case for the completeness and locality of quantum mechanics, it is not sufficient to show the problems of EPR's argumentation. One also needs to offer an interpretation of quantum phenomena and of quantum mechanics as a complete (i.e., properly covering the available experimental evidence) and local theory of quantum phenomena. The detailed reading of Bohr's reply allows one to offer a more sustained explanation of this part of Bohr's argument.

Finally, Bohr's reply confronts its readers with major difficulties. It is arguably the most difficult work in Bohr's oeuvre, and it has been the subject of considerable frustration on the part of many of its readers and of much controversy. These difficulties are, first, those of (to return to Bohr's language) the "inefficiency of expression" in conveying the epistemological complexities of his argument (Bohr 1949, *PWNB* 2, p. 61). The article requires a particular kind of

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reading that is not customarily required by physical texts, which may be one of the reasons a detailed reading of Bohr's article as whole has not as yet, to my best knowledge, been offered by commentators on Bohr. One of the contributing factors is Bohr's minimal overt reliance on the mathematical formalism of quantum mechanics as against most foundational works, physical and philosophical, on quantum mechanics. Bohr's discussion of the formalism in his reply is contained in a single footnote. This is, again, not to say that Bohr underappreciated the significance of the mathematical formalism of quantum mechanics, including specifically in considering the EPR experiment, since EPR's predictions and quantum correlations, in general, are enabled by this formalism. Quite the contrary; indeed, Bohr's footnote is mathematically substantive and its content essential to his argument. Instead, as I noted from the outset of this study, he appears to have thought that the key problems (such as those at stake in the EPR experiment) posed by quantum phenomena and quantum mechanics require a deep and subtle understanding and interpretation that involve but go beyond the mathematical formalism of the theory. The task is difficult, and Bohr recognized and commented on these difficulties and the imperfections of his reply, without, however, renouncing anything in his argument.[1]

To begin in the beginning (or almost, since there is still the title, on which I shall comment presently), Bohr's abstract says, "It is shown that a certain 'criterion of physical reality' formulated in a recent article with the above title ['Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?'] by A. Einstein, B. Podolsky and N. Rosen contains an essential ambiguity when it is applied to quantum phenomena. In this connection a viewpoint termed 'complementarity' is explained from which quantum-mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands of completeness" (Bohr 1935b, p. 696). Two different answers are, then, given to the question posed by the title of the two

[1] It is true that, as noted in Chapter 6, Bohr does not appear to readily acknowledge such mistakes in the case of complementarity, as against renouncing his reservations concerning the idea of the photon or abandoning the BKS proposal. In these cases, however, Bohr gave up his views because the experimental evidence against them became available. In the case of complementarity we deal with interpretation of the same evidence and the same theory, which is a more subtle matter, and Bohr might have seen his revisions as refinements rather than changes or corrections, perhaps not always justifiably. In any event, Bohr appears to have firmly believed (in this case justifiably, in my view) in the essential correctness of his counter-argument to EPR, whose argument he continued to view critically throughout his life (cf. Bohr 1962). By a strange coincidence, the article also had "suffered" a rather peculiar history of reprinting. Thus, the version reproduced in (Wheeler and Zurek 1983) has the order of pages mixed (page 149 should preceed page 148), and the version reproduced in *PWNB* 4 contains significant errors, which seriously inhibit Bohr's meaning. At one point, it substitutes "causal" for Bohr's "rational," which literally destroys Bohr's whole point (*PWNB* 4, p. 75). There are other errors as well. For these reasons, as earlier in this study, I cite here from the original version, which is, however, also directly and, for a change, correctly reproduced in Bohr (1972–1996, vol. 7, pp. [292]–[298]).

articles, negative by EPR and affirmative by Bohr, which also explains why Bohr decided to use the same title, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" The emphasis on phenomena, rather than reality (at the quantum level), becomes apparent here and in the initial elaborations of Bohr's reply, and it is to remain part of Bohr's argument there and of his thinking concerning quantum phenomena, quantum mechanics, and complementarity from this point on. The two sentences of Bohr's abstract define the two main parts of Bohr's argument, as discussed in Chapter 8.

The first part, the *critique* or *counterargument* part, is aimed to demonstrate the insufficiency of EPR's argument for the incompleteness or else nonlocality of quantum mechanics in EPR's own terms, by showing that the application of their criterion of reality to quantum phenomena, including specifically those found in the EPR experiment, is ambiguous. This is to be accomplished by using the uncertainty relations, the role of the measuring instruments, complementarity (in the narrow sense), and the unavoidably probabilistic or statistical nature of our predictions concerning quantum phenomena as correlative *experimental facts* in order to argue that this application requires qualifications not offered by EPR. On the other hand, once these qualifications are introduced, they, while allowing one to apply EPR's criterion unambiguously, disable EPR's argument on both counts—completeness and locality.

The second part of Bohr's reply is aimed to argue that, from the viewpoint of complementarity based on the experimental circumstances just mentioned, "quantum-mechanical description of physical phenomena *would seem* to fulfill, within its scope, all rational demands of completeness," as well as locality. In other words, Bohr argues that a local interpretation of quantum mechanics as a complete theory of quantum phenomena is possible. Accordingly, as explained in Chapter 8, unless some arguments or experimental data to the contrary become available, such an interpretation allows one to see quantum mechanics as, within its scope, a complete and local theory accounting for quantum phenomena. In this respect, his reply goes beyond what he announced concerning his reply earlier in his note in *Nature*, which referred only to the problems of EPR's argument (Bohr 1935a). As I said, Bohr's exposition of his "viewpoint" in his reply may not quite amount to a full-fledged interpretation of quantum phenomena and quantum mechanics, but rather supplements the argumentation of his previous works, assembled in *PWNB* 1, to which he refers (Bohr 1935b, pp. 696, 696n.). The interpretation of his reply is by and large equivalent to the one offered in Bohr (1929a) and Bohr (1929b), as discussed in Chapter 7, but, as I said, it departs from that of the Como lecture found in the same volume. This is, again, an important point, which should be kept in mind in order to avoid confusions concerning Bohr's argument. Bohr's reply allows for the attribution, *at the time of measurement*, of certain measurable quantities to quantum objects themselves (as opposed to only to certain parts of measuring instruments), under the constraints of the uncertainty relations. This view, while not impossible, carries with it certain difficulties, which made Bohr ambivalent concerning such an attribution and which he only resolved in his nonclassical version of complementarity. However, these difficulties do not affect Bohr's critique of EPR's argument. Moreover, the latter assumes the attribution of such quantities to be possible on the basis of *predictions* (with probability equal to unity), rather than measurement, in accordance with the EPR criterion of reality. Bohr argues that the applications of the criterion under this assumption requires qualifications, not offered by EPR, which makes the use of their criterion ambiguous in quantum physics, as against that of classical physics. Bohr summarizes EPR's argument as follows:

> By means of [the EPR experiment] … [EPR] proceed to show that in quantum mechanics, just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation. According to their criterion the authors therefore want to ascribe an element of reality to each of the quantities represented by such variables. Since, moreover, it is a well-known feature of the present formalism of quantum mechanics that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables, they consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed. (Bohr 1935b, p. 696)

It is worth registering Bohr's careful language in saying that "it is possible under suitable conditions to predict the value of *any given variable pertaining to the description of a mechanical system* from measurements performed entirely on other systems which previously have been in interaction with the system under investigation" (emphasis added). That is, such predictions are indeed equally possible in classical and quantum mechanics. As we have seen, however, this does not mean that other things EPR argue for are equally possible in classical and quantum mechanics—in particular, that one could apply their criterion of reality in both cases in the same way. Hence, as Bohr says in the first paragraph of his reply, "the trend of their argumentation … does not seem to [him] adequately to meet the actual situation with which we are faced in atomic physics," that is, the situation defined by the nature of quantum phenomena and the corresponding experimental data (Bohr 1935b, p. 696). This, again, includes the uncircumventable role of the uncertainty relations, or, more accurately, of the situation concerning the simultaneous attribution of conjugate physical quantities to quantum objects that the uncertainty relations reflect. EPR, by contrast, deem that one can de facto, albeit not in any actual measurement, circumvent this situation via the EPR experiment and that the inability of quantum mechanics to do so reflects its incompleteness. A bit later, Bohr offers another strong statement: "Such an argumentation, however, would hardly seem suited to affect the soundness of quantum-mechanical description, which is based on a coherent mathematical formalism covering automatically any procedure of measurement like that indicated" (Bohr 1935b, p. 696).

Bohr adds a footnote on what he calls the transformation theorems of quantum mechanics, which allow one to move from one set of quantum mechanical variables to another and from which the possibility of the EPR predictions follows automatically as a special case (Bohr 1935b, pp. 696–697n., 699n.). Both this footnote and the statement just cited clearly indicate (again, against common contentions to the contrary) the significance of the mathematical formalism of quantum mechanics for Bohr's argument here and for Bohr's thinking concerning quantum theory in general.

Bohr develops his argument by examining the role of measuring instruments in quantum mechanics, both in general and specifically in the EPR situation, taking a clue from EPR and turning the tables on them. As he says, "The extent to which an unambiguous meaning can be attributed to such an expression as 'physical reality' cannot of course be deduced from a priori philosophical conceptions, but—*as the authors of the article cited themselves emphasize*—must be founded on a direct appeal to experiments and measurements" (Bohr 1935b, p. 696; emphasis added). However, as Bohr shows along the lines considered in Chapter 8, EPR's criterion of reality and their argument subtly involve "a priori philosophical conceptions" and "the customary viewpoint of natural philosophy," which has "an *essential* inadequacy . . . for a rational [!] account of physical phenomena of the type with which we are concerned in quantum mechanics" (Bohr 1935b, pp. 696–697).

Thus, Bohr argues that, beginning with its philosophical groundings, EPR's argument is inadequate to deal with quantum phenomena. Quantum mechanics conforms to or, given its lack of descriptive capacity (possibly not allowed by nature for any theory of quantum phenomena), adequately responds to nature through our interactions in it. (Ultimately, these interactions are still part of nature, of course.) In other words, rather than, as EPR argue, showing quantum mechanics to be wanting as a physical theory, the *EPR experiment* confirms that it is nature—or, one might say, "reality"—that resists the application of EPR's criterion of reality and the logic of their argument. By contrast, Bohr is able to offer a *viewpoint*, that of complementarity, which avoids the kind of ambiguity that EPR's or, it appears, any classical-like view inevitably brings with it, once one tries to apply it to quantum phenomena. From this viewpoint, "quantum mechanics within its scope would appear as a completely rational description of physical phenomena, such as we meet in atomic processes," and would also "seem to fulfill, within its scope, all rational demands of completeness" (Bohr 1935b, p. 696), unless, again, some rigorous arguments or new data will show otherwise. According to Bohr, EPR's argument does not accomplish this, while the EPR experiment is perfectly consistent with both completeness and locality of quantum mechanics.

Bohr proceeds as follows. First, he argues his point concerning this role of measuring instruments, interpreted in terms of complementarity (in the narrow sense). Second, he considers the EPR experiment from this perspective and shows the essential ambiguity in question, along the lines sketched earlier. He ends this part of his analysis with his famous elaboration on the essential ambiguity of EPR's criterion—an elaboration often misunderstood, in part, as I said, by virtue of being read separately from the analysis on which it is based. This elaboration by and large ends his counterargument to EPR and defines complementarity as experimentally based, which allows one to see quantum mechanics as both complete, within its scope, and local. Bohr then considers the question of temporality in the experimental situations of quantum mechanics. He shows that this question is subject to the same kind of treatment as the question of spatial coordination, given that the complementarity of the position and the momentum measurements is parallel to that of the time and the energy measurements, with the corresponding uncertainty relations for each pair. Then he discusses the issue of discriminating between those parts of the physical system in question that are to be treated as quantum objects and as measuring instruments, respectively. Finally, he comments on the relationships between the epistemology of quantum mechanics and that of relativity, specifically general relativity. He adds a footnote explaining why his overall argumentation allows for the locality of quantum mechanics, that is, "the compatibility between the argumentation outlined in the present article and all exigencies of relativity theory" (Bohr 1935b, p. 701n.).

9.2 Measurement and Complementarity

Bohr first addresses the nature of quantum phenomena (in the conventional sense of the terms) and argues that it is defined by the irreducible role of measuring instruments in their constitution. Consequently, in contrast to the situation that obtains in classical physics (where one can identify objects and phenomena and disregard measuring instruments), quantum objects cannot be considered apart from their interaction with measuring instruments. He further argues, however—and this point grounds his overall argument in his reply—that this circumstance is correlative to the mutual exclusivity of the measurements of or predictions concerning the conjugate physical quantities in question in the EPR experiment or in any quantum experiment, and he first considers the case of the standard quantum measurement. As discussed in Chapter 8, it is the latter point that exposes the insufficiency of EPR's argument. It disables any assignment of the second quantity in question (say, momentum) to the same quantum object if the first quantity (position) is assigned, even if this assignment is made on the basis of a prediction in accordance with EPR's criterion of reality. For this prediction is made on the basis of a measurement performed on the first object, S_1, of the EPR pair, and an alternative prediction would require an alternative measurement on S_1, which is never possible, if the first measurement is made. Nor is it possible to repeat the experiment in such a way that this alternative measurement could be exactly, rather than only statistically, correlated with the position measurement obtained in the first experiment. If we repeat the same measurement, the outcome will be different as well. Thus, it is not possible to sustain EPR's argument that the object in fact even possesses both quantities as "elements of reality."

As also discussed in Chapter 8, EPR agree that any actual measurement or prediction itself allows one to assign only one of these quantities, and hence that both quantities cannot be simultaneously measured or simultaneously predicted (for S_2 by making the same measurement on S_1). By the same token, they admit that it is possible that their criterion does not apply without a modification in the case of quantum phenomena, in which case one could also see quantum mechanics as complete. However, they see this circumstance as implying nonlocality in the case of the EPR experiment. According to Bohr, however, this conclusion can be avoided as well, if one properly takes into account the irreducible role of measuring instruments in the constitution of quantum phenomena.

In his reply, Bohr considers quantum measurement in terms of "the finite and uncontrollable interaction" between quantum objects and measuring instruments (Bohr 1935b, pp. 697, 700). He speaks of "*the finite interaction between object and measuring agencies* [as] conditioned by the very existence of the quantum of action" and [as] defined by "the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose" (Bohr 1935b, p. 697). The "finite" also means quantum and as such not subject to the continuous treatment of classical physics, and, thus, also manifested only in the corresponding effects observed. At this stage, this concept still retains the view adopted by Bohr at this point (or by EPR) that certain properties could be attributed (under the constraints of the uncertainty relations) to quantum objects themselves, which is sufficient for Bohr's counterargument to EPR. Nevertheless, the concept represents a significant step in the development of Bohr's epistemology, defined by "the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude toward the problem of physical reality," which quantum phenomena appear to demand from us (Bohr 1935b, p. 697). Although still germane, this concept becomes less prominent in Bohr's later writings, once Bohr sees the situation in terms of his concept of phenomenon, which suspends any attribution of physical properties (even single ones and even at the time of measurement) to quantum objects.

Bohr's initial elaborations, which concern the standard case of quantum mechanical measurement, use the double-slit experiment, following upon his previous exchanges with Einstein, as discussed in Chapter 8. Bohr examines the case of a quantum object (a "particle") passing through a slit in a diaphragm as an initial stage of a given measurement situation, which can concern either a position or a momentum measurement and, then, a prediction concerning the object in question—a stage already subject to the uncertainty relations. As he says,

> Even if the momentum of this particle is completely known before it impinges on the diaphragm, the diffraction by the slit of the plane wave giving the symbolic representation of its state will imply an uncertainty in the momentum of the particle, after it has passed the diaphragm, which is the greater the narrower the slit. Now the width of the slit, at any rate if it is still large compared with the wave-length, may be taken

> as the uncertainty Δq of the position of the particle relative to the diaphragm, in a direction perpendicular to the slit. Moreover, it is simply seen from de Broglie's relations between momentum and wave-length that the uncertainty Δp of the momentum of the particle in this direction is correlated to Δq by means of Heisenberg's general principle
>
> $$\Delta p \Delta q \approx h.$$
>
> (Bohr 1935b, p. 697)

Thus, once ascertained, the passing of the object through a diaphragm makes it no longer possible to take into account its previous state, defined by its *known* momentum, for future predictions concerning this object, once the object has passed through the slit. Bohr's appeal to "the *symbolic* representation of [the particle's] state" (emphasis added) is significant on several counts, beginning with its indication that the measurement de facto performed here gives us only expectations concerning some future possible measurements. These expectations concern *individual* events and are defined by quantum amplitudes and the rules, such as Born's rule, for quantum probabilities. The situation is correlative to the uncertainty relations, connected with "the plane wave giving the *symbolic* representation of [the particle's] state." This plane wave represents the probabilistic nature of our quantum predictions *even in the case of individual quantum experiments*. Arguing that the initial state in question is subject to the uncertainty relations is an important point. It indicates, along the lines of Chapter 8, the difference in the outcomes of repeated experiments, even if identically prepared, and the impossibility of other than statistical coordination of the conjugate variables considered, because they pertain to different quantum objects, in contrast to the situation that obtains in classical mechanics. The full significance of this point becomes immediately apparent in Bohr's reply.

Bohr appeals to this individuality in the next paragraph. Bringing together quantum individuality and probability is, I argue in this study, an essential aspect of all Bohr's arguments for complementarity. In Bohr's later view, all other uses of physical concepts—such as "particle" (which, as we have seen, was already argued to be an "abstraction" even in the Como lecture)—as these concepts relate to quantum objects are viewed as "symbolic" as well; only the variables and quantities associated with the diaphragm can be considered in physical terms. Even if one allows for a less radical approach in Bohr's reply, certain limitations concerning the assignment of both conjugate quantities to the same quantum object are irreducible, and Bohr's argument is based on these limitations and their implications for our predictions concerning the measurable quantities considered in quantum mechanics.

As the first stage of this argument, Bohr shows that the (exact) measurements or predictions of the position and the momentum for a given quantum object are incompatible, thus making it impossible to even contemplate an assignment of both for the *same object*, if one such quantity is assigned—either on the basis of measurement or on the basis of prediction, and either in the standard or in the EPR case. If at some future point of this object's "life" such an assignment of the other quantity is made, it erases the previous assignment of the first quantity either as concerns the physical state of the object at the time of this new assignment or for the purposes of any meaningful prediction concerning the object. Thus, as explained in Chapter 8, if we measure the conjugate variable (say, momentum) other than the one (position) that was predicted for the second object of a given EPR pair, the prediction becomes meaningless. There is no possible experiment to assess the value of the originally predicted variable, and any repeated preparation of a new EPR pair will give a different outcome that can never be coordinated with the first on the model of classical physics.

Bohr observes next that "obviously the uncertainty Δp is inseparably connected with the possibility of an exchange of momentum between the particle and the diaphragm" (Bohr 1935b, p. 697). We, again, keep in mind that Bohr here allows for the assignment of properties to quantum objects themselves, *at the time of measurement* (and hence involving some measuring instruments) and *as subject to the uncertainty relations*. Accordingly, while this statement would need to be further qualified if one considered the situation in terms of Bohr's later argument via his concept of phenomenon, under Bohr's assumptions in his reply this statement is legitimate. Bohr, we recall, also starts the paragraph with the conditional "if the momentum of this particle is completely known before it impinges on the diaphragm," which can only be so on the basis of some previous measurement (either on this or on some entangled object). The significance of this measurement or knowledge for our predictions concerning the particle will be erased once the particle passes through the diaphragm. This passing is a new measurement, and only this measurement is meaningful for the purposes of our predictions concerning any future events from this point on.

Bohr states next "the question of principal interest" for his counterargument, which also reveals the fuller significance of his appeal to the uncertainty relations at this initial stage of the experiment. He says, "[T]he question of principal interest for our discussion is now to what extent the momentum thus exchanged can be taken into account in the description of the phenomenon to be studied by the experimental arrangement concerned, of which the passing of the particle through the slit may be considered as the initial stage" (Bohr 1935b, p. 697; also Bohr 1949, *PWNB* 2, pp. 42–43). This stage will define future predictions concerning the particle, while any preceding knowledge concerning it (e.g., its previously known momentum) is no longer of any use for the purposes of such predictions from this point on.

Bohr now considers two alternative—and, as he is about to prove, mutually exclusive—experimental arrangements, one suited for predictions concerning the particle's position and the other for predictions concerning its momentum. These arrangements represent the situations that obtain in any quantum mechanical measurement or prediction, including in the EPR case, although for the moment Bohr addresses only the standard case. It is worth noting that Bohr is thorough in showing that each arrangement precludes the other. In considering the first case, that for predictions concerning the position of the particle, Bohr argues as follows:

> Let us first assume that, corresponding to usual experiments on the remarkable phenomena of electron diffraction, the diaphragm, like the other parts of the apparatus,—say a

> second diaphragm with several slits parallel to the first and a photographic plate,—is rigidly fixed to a support which defines the space frame of reference. Then the momentum exchanged between the particle and the diaphragm will, together with the reaction of the particle on the other bodies, pass into this common support, and we have thus voluntarily cut ourselves off from any possibility of taking these reactions separately into account in predictions regarding the final result of the experiment,—say the position of the spot produced by the particle on the photographic plate. The impossibility of a closer analysis of the reactions between the particle and the measuring instrument is indeed no peculiarity of the experimental procedure described, but is rather an essential property of any arrangement suited to the study of the phenomena of the type concerned, where we have to do with a feature of *individuality* completely foreign to classical physics. In fact, any possibility of taking into account the momentum exchanged between the particle and the separate parts of the apparatus would at once permit us to draw conclusions regarding the "course" of such phenomena [of the process leading to such phenomena?],—say through what particular slit of the second diaphragm the particle passes on its way to the photographic plate—which would be quite incompatible with the fact that the probability of the particle reaching a given element of area on this plate is determined not by the presence of any particular slit, but by the positions of all the slits of the second diaphragm within reach of the associated [symbolic] wave diffracted from the slit of the first diaphragm (Bohr 1935b, pp. 697–698).

Bohr's case is made easier if we consider "the limiting case of . . . infinitely narrow slits," which, in the EPR experiment, will correspond to the idealized case of the wave function considered by EPR (Bohr 1935b, p. 699n.). Then, the position of the particle is determined exactly, and its momentum, in view of the uncertainty relations, is completely undetermined. In any event, we cannot take the exchange of momentum between the particle and the first diaphragm into account in the arrangement suited for determining its position. Were we able to do so, we would be able to determine the course of the particle in the way we can in classical physics and, hence, determine "through what particular slit of the second diaphragm the particle passes on its way to the photographic plate." Then, however, our predictions would be incompatible with the conditions of the double-slit experiment, and the probabilities and the corresponding patterns observed there—in this case, the interference pattern that would be observed in this arrangement, once the experiment has been repeated a sufficient number of times. Bohr's appeal to the individuality of the phenomena under consideration and, as a consequence, of all quantum phenomena is, again, worth noting. Apart from its significance in its own right, it anticipates Bohr's concept of phenomena, which takes the inseparability of the behavior of quantum objects from their interaction with the measuring instruments *to the point of the impossibility of assigning any properties to quantum objects themselves even at the time of measurement*. Even at this earlier stage of his thinking, however, Bohr can still say that one cannot meaningfully *consider* the behavior of quantum objects apart from their interaction with the measuring instruments, which is part of the individuality in question. For, while an assignment of one of the properties in question is possible even by means of a prediction, it always requires a measurement, even if on a different quantum object, as in the

EPR case, and it also requires that a measurement that can verify such a prediction remains in principle possible. Once such verification is no longer possible, the prediction becomes meaningless.

Bohr's argument essentially follows the argument developed in the preceding chapter. Given the phenomena in question, one cannot assign, along the lines of EPR's thinking, the values of both complementary physical quantities associated with the *same quantum object*, even if one does so on the basis of predictions, in accordance with their criterion of reality. If one quantity is assigned, there is no experiment or set of experiments that would allow assignment of the other. Thus, in the case of the position measurement in question here, "the momentum exchanged between the particle and the diaphragm will, together with the reaction of the particle on the other bodies, pass into this common support," which would prevent us from "any possibility of taking these reactions separately into account in predictions regarding the final result of the experiment,—say the position of the spot produced by the particle on the photographic plate." After it passes through the slit of the first diaphragm, however, there will be an uncertainty in this momentum, Δp, regardless of the setup considered, which is numerically known from the uncertainty relations, $\Delta p \Delta q \approx h$, since the uncertainty of the position, Δq, is assumed to be the width of the slit. This is a nice touch on Bohr's part. It shows that the uncertainty relations are also a source of positive knowledge. Most crucially, Bohr's argument presents the uncertainty relations as a law of nature, reflecting the essential features of quantum phenomena coherently covered by quantum mechanics, rather than something that can be de facto (even if not in an actual measurement) circumvented and would thus reveal quantum mechanics to be incomplete, as EPR want to argue.

As noted earlier, Bohr observed that [this] known uncertainty of the momentum Δp is "inseparably connected with the possibility of an exchange of momentum between the particle and the diaphragm." This observation serves as a setup for considering "to what extent the momentum thus exchanged can be taken into account in the description of the phenomenon to be studied by the experimental arrangement concerned, of which the passing of the particle through the slit may be considered as the initial stage" (Bohr 1935b, p. 696). This question was raised by Einstein in his earlier exchanges with Bohr concerning the double-slit experiment. Bohr here reprises his earlier counterarguments, which, as discussed earlier, Einstein apparently accepted but which he wanted to circumvent by his reasoning of the EPR type (Bohr 1949, *PWNB* 2, pp. 42–47). Bohr's argument at this juncture is, again, that there is no possibility in principle of taking this exchange of momentum into account for the predictions for which the arrangement in question is suited, such as and in particular those concerning "the position of the spot produced by the particle on the photographic plate." If we could do so, certain key features of the double-slit experiment and, by implication, of quantum phenomena, in general, would be effectively circumvented, in conflict with the experimental evidence that supports them. That includes, in particular, correct probabilistic descriptions concerning such phenomena.

It follows that any arrangement suited for properly predicting the outcome of a position measurement in the case of quantum phenomena precludes taking into account the momentum exchange in question and hence precludes any possible predictions concerning the momentum measurements associated with the same object. An alternative arrangement is necessary in order to make those predictions. Bohr now proceeds to argue that any such alternative arrangement is in turn incompatible with any possibility of predicting the outcome of a future position measurement associated with the object considered, in view of the impossibility of "control[ing] . . . the space-time coordination" of the test bodies involved. He argues for a reciprocal mutual exclusivity: Each arrangement in question excludes the other. He says,

> By another experimental arrangement, where the first diaphragm is not rigidly connected with the other parts of the apparatus, it would at least in principle be possible to measure its momentum with any desired accuracy before and after the passage of the particle, and thus to predict the momentum of the latter after it has passed through the slit. In fact, such measurements of momentum require only an unambiguous application of the classical law of conservation of momentum, applied for instance to a collision process between the diaphragm and some test body, the momentum of which is suitably controlled before and after the collision. It is true that such a control will essentially depend on an examination of the space-time course of some process to which the ideas of classical mechanics can be applied; if, however, all spatial dimensions and time intervals are taken sufficiently large, this involves clearly no limitation as regards the accurate control of the momentum of the test bodies, but only a renunciation as regards the accuracy of the control of their space-time coordination. This last circumstance is in fact quite analogous to the renunciation of the control of the momentum of the fixed diaphragm in the experimental arrangement discussed above, and depends in the last resort on the claim of a purely classical account of the measuring apparatus, which implies the necessity of allowing a latitude corresponding to the quantum-mechanical uncertainty relations in our description of their behavior. (Bohr 1935b, p. 698)[2]

This passage, again, makes one appreciate the importance of Bohr's shift from the complementarity of the space–time coordination and the claim of causality in the Como lecture of 1927 to that of the space–time coordination and the application of the conservation laws in "The Quantum of Action and the Description of Nature" in 1929. It is this complementarity that is correlative to the mutually exclusive nature of the two measuring arrangements discussed by Bohr here. It is also clear that, given the nature of the position and momentum measurements described here, the uncertainty relations can rigorously apply only to certain parts of measuring arrangements, and only through them to quantum objects, to the degree this latter application is possible at all. (It is, again, no longer possible in Bohr's nonclassical view.) The further significance of this point becomes apparent in the next paragraph. Bohr writes,

> The principal difference between the two experimental arrangements under consideration is, however, that in the arrangement suited for the control of the momentum of the first diaphragm, this body can no longer be used as a measuring instrument for the same purpose as in the previous case, but must, as regards its position relative to the rest

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[2] Bohr adds a note on the essential equivalence of his thought experiment to actual experiments, such as those related to the Compton effect (Bohr 1935b, p. 698n.).

> of the apparatus, be treated, like the particle traversing the slit, as an object of investigation, in the sense that the quantum mechanical uncertainty relations regarding its position and momentum must be taken explicitly into account. In fact, even if we knew the position of the diaphragm relative to the space frame before the first measurement of its momentum, and even though its position after the last measurement can be accurately fixed, we lose, on account of the uncontrollable displacement of the diaphragm during each collision process with the test bodies, the knowledge of its position when the particle passed through the slit. (Bohr 1935b, p. 698)

In other words, the uncontrollable (by virtue of its quantum origin) displacement of the diaphragm leads, as concerns its position, to a quantum mechanical-like rather than classical behavior of the diaphragm in the sense that the uncertainty relations apply to it as well. Hence, in considering the quantities in question we are, again, dealing with two mutually exclusive arrangements and not with a single arrangement, as all of Einstein's arguments of the EPR type assume. This crucial point is, as we have seen, repeatedly stressed by Bohr in "Discussion with Einstein" (e.g., Bohr 1949, *PWNB* 2, pp. 57, 60). Bohr's reply explains the physics behind this point in greater detail than any other of his works. Bohr concludes, "The whole arrangement is therefore obviously unsuited to study the same kind of phenomena as in the previous case" (Bohr 1935b, p. 698). Bohr closes by indicating the relationships, considered in Chapter 2, between this second arrangement and the interference effects. He says, "In particular it may be shown that, if the momentum of the diaphragm is measured with an accuracy sufficient for allowing definite conclusions regarding the passage of the particle through some selected slit of the second diaphragm, then even the minimum uncertainty of the position of the first diaphragm compatible with such a knowledge will imply the total wiping out of any interference effect—regarding the zones of permitted impact of the particle on the photographic plate—to which the presence of more than one slit in the second diaphragm would give rise in [the] case [when] the positions of all apparatus are fixed relative to each other" (Bohr 1935b, p. 698).

Thus, in dealing with quantum phenomena, any arrangement suited for properly predicting the outcome of a momentum measurement precludes predictions concerning the position measurements associated with the same object. For the possibility of such predictions would be incompatible with the key features of the double-slit experiment or other quantum phenomena that the double-slit experiment paradigmatically represents (Bohr 1935b, p. 698n.). Hence, this possibility would be in conflict with experimental evidence. It, again, follows that it is never possible to predict both quantities for the same quantum object, either simultaneously (in view of the uncertainty relations) or otherwise, once one of these quantities has been assigned. Bohr adds the paragraph on the delayed-choice-like considerations of the type discussed earlier in conjunction with "Discussion with Einstein" (Bohr 1935b, pp. 698–699; Bohr 1949, *PWNB* 2, p. 57). Bohr concludes this part of his analysis as follows:

> [I]n the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of [sacrificing] other such elements, but with a rational discrimination between

> essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum. Any remaining appearance of arbitrariness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. In fact, the renunciation in each experimental arrangement of the one or the other of two aspects of the description of physical phenomena,—the combination of which characterizes the method of classical physics, and which therefore in this sense may be considered as *complementary* to one another,—depends essentially on the impossibility, in the field of quantum theory, of accurately controlling the reaction of the object on the measuring instruments, i.e., the transfer of momentum in [the] case of position measurements, and the displacement in [the] case of momentum measurements. (Bohr 1935b, p. 699)

A certain arbitrariness may be found in our setting up of one experimental arrangement or the other, and this *possibility*, in practice, of doing either is, again, as essential to Bohr's concept of complementarity (in its narrow sense) as the impossibility, in principle, of ever doing both simultaneously. By the same token, however, there is also no possibility of arbitrarily, at will, selecting such quantities and associating them with a given quantum object, since, in contrast to classical physics, such an association is not possible apart from a specified experimental arrangement. The choice one can make could concern only the type of experimental arrangement one makes. Once chosen, this arrangement would then automatically determine one and only one of the two conjugate variables, say, that of position, that could be assigned to the same quantum object. In order to make an alternative assignment, that of momentum, at the same time, the *complementary* arrangement would have to be made, and these assignments exclude each other. The choice in question also "fixes" the quantum object to which such a variable could be assigned within the phenomenon, in this case a predicted phenomenon, while the alternative arrangement and the corresponding phenomenon (say, that associated with the momentum measurement) would inevitably involve a different quantum object—and vice versa. This is what Bohr refers to as "*an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system*" (Bohr 1935b, p. 700).

Hence, it would indeed be difficult to consider quantum objects independently of their interactions with the measuring instruments, and Bohr's concept of phenomena is nearly inherent in and arises from these considerations. Each such alternative arrangement in fact involves two phenomena—one actual (defined by the measurement performed) and one potential, a phenomenon-to-be (to be defined by a measurement yet to be performed). As explained earlier, one can subsequently, after the prediction in question (say, that of the position) is made, perform an alternative complementary *measurement* (that of the momentum). In so doing, however, one would (discontinuously) change the situation into a different, alternatively defined and mutually exclusive, set of phenomena, which would disable any possible assignment of the position variable to the object at the time of this second measurement.

In his reply thus far Bohr has considered only the standard case, rather than the EPR case, of quantum measurement and prediction. But, as discussed earlier and as Bohr is about to argue, the EPR experiment does not change the essential features of the situation. To reprise briefly the argument of the preceding chapter, both EPR and Bohr proceed from *assumption A*—that of the possibility of an assignment of either one such complementary quantity or the other to the same quantum object—but to two different inferences. EPR proceed from this assumption to *inference E*—that *both* quantities can, in principle, be assigned to the same quantum object, even though it may not be possible to do so simultaneously. Bohr, by contrast, proceeds to *inference B*—according to which a realization of the two alternative situations of measurement in question, which are necessary for the respective assignment of these quantities, would, in contrast to classical physics, involve two incompatible, mutually exclusive, experimental arrangements. Thus, they will involve *two* quantum objects, which could never be prepared or behave identically as objects, even though we can identically prepare the measuring instrument involved in the corresponding experiment. Bohr, again, does not state this point in this form, but it immediately follows from his conclusion concerning the mutual exclusivity of the measuring arrangements involved. The difference between these two alternative inferential logics is defined by Bohr's argument for the irreducible role of measuring instruments in any possible handling of quantum objects as an uncircumventable experimental feature of quantum phenomena. In the EPR case, we can predict (in an idealized experiment) with probability equal to unity the first quantity in question (say, that related to the position variable) for the second object of a given EPR pair, and then predict the second (that related to the momentum variable) for the second object of another, "identically prepared," EPR pair. We cannot, however, coordinate these predictions in such a way that they could be viewed as pertaining to the same pair of quantum objects, since all the necessary intermediate measurements could and, in general, would give us different data. The generally statistical character of our predictions concerning such phenomena becomes irreducible, regardless of the theory we use. At the same time, however, as Bohr notes next, just before he discusses the EPR experiment: "Just in this last respect [of the renunciation in each experimental arrangement of the one or the other of two aspects of the description of the physical phenomena] any comparison between quantum mechanics and ordinary statistical mechanics,—however useful it may be for the formal presentation of the theory,—is essentially irrelevant. Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way" (Bohr 1935b, p. 699).

This is a crucial point, including in the context of Einstein's arguments concerning the statistical aspects of quantum mechanics to be discussed in detail in Chapter 10. Briefly, Einstein does allow for the possibility that quantum mechanics offers a correct, as well as local, account of quantum phenomena in terms of statistical ensembles. However, this still leaves quantum mechanics incomplete insofar it does not offer a theory of individual processes and events, since EPR's logic still applies at this level, and the completeness of quantum mechanics in this regard would entail nonlocality. Bohr here responds in advance to this reasoning of Einstein, which compares quantum mechanics and ordinary statistical mechanics essentially rather than formally. For Bohr, quantum mechanics remains a probabilistic theory of even individual quantum events. However, perhaps no theory could do better, given the nature of quantum phenomena.

9.3 Restaging the EPR Experiment

With his analysis of the standard case of quantum measurement in hand, Bohr is ready to address the EPR experiment and offer his critique of EPR's argument. The analysis Bohr offered in the earlier elaborations of his reply, as just considered, de facto exposes the essential difficulty or ambiguity he locates in EPR's application of their criterion of reality to quantum phenomena. The setup for one or the other of the two possible types of complementary measurements we perform in the EPR experiment on the first EPR particle (S_1) establishes—*fixes*, in accordance with EPR's criterion of reality—our determination concerning the second EPR particle, S_2. This "fixing" disables an alternative determination, *ever*, concerning S_2—unless, as I explained, we perform an alternative type of *measurement* on it. Such a measurement will, however, annul the determination that this first setup establishes. Either way, only one such determination is in principle possible for a given object. Hence, in accordance with the argument given in Chapter 8, we can only assign without ambiguity both quantities in two different, mutually exclusive, specified arrangements—and, hence, to two different objects and never to a single object, as EPR argue. Nor could these two assignments ever be properly coordinated to assume that a given object could be assigned both quantities. These facts reveal the essential ambiguity of EPR's application of their criterion of reality to quantum phenomena. There is no experimental arrangement that could allow us to avoid the indeterminacy of the second variable for the same object, once the determination of the first variable is made. However, it is worth following Bohr's discussion of the EPR experiments in his reply in terms of a specific experimental arrangement "reproduc[ing] [at least in principle]… the particular quantum-mechanical state of two free particles, for which [EPR] give an explicit mathematical expression" (Bohr 1935b, p. 699).

This elaboration has seldom been discussed and, when discussed, is rarely given adequate treatment. In this case, the commentators are not altogether to blame, though one might fault their unwillingness to engage in a more extended discussion of Bohr's overall argument, without which it is difficult to consider this passage properly. It does appear that Bohr has insufficiently clarified some points of this elaboration. Perhaps for that reason, this particular way of representing the EPR experiment disappears from Bohr's arguments concerning it, and one need not depend on this particular "staging" of the EPR experiment. Instead, from the Warsaw lecture on, Bohr considers the EPR experiment in terms of the interaction between a given quantum object and a measuring instrument, which appears in the role of the first object, S_1, of a given EPR pair, (S_1, S_2), as discussed in the preceding chapter (Bohr 1938, *PWNB* 4, pp. 101–104; Bohr 1949, *PWNB* 2, p. 60). This view is also accompanied by—and very likely has emerged from—Bohr's rethinking of complementarity in terms of his concept of phenomenon.

Bohr's preceding analysis, as just discussed, is important for understanding this passage, which transposes this analysis into the situation of the EPR experiment, as Bohr makes clear in his statement opening this discussion of the EPR case. He says: "The last remarks apply equally well to the special problem treated by Einstein, Podolsky, and Rosen, . . . which does not actually involve any greater intricacies than the simple examples discussed above" (Bohr 1935b p. 699). "The last remarks" refers to Bohr's conclusion of his analysis of the standard case of quantum measurement, cited earlier. I repeat the remark because of its singular importance: In "the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrificing other such elements, but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum" (Bohr 1935b, p. 699). These arrangements, again, unavoidably require two quantum objects or, in the case of the EPR experiments, two EPR pairs, to be realized. Bohr says next:

> The particular quantum-mechanical state of two free particles, for which they [EPR] give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation [and are infinitely narrow, in the idealized limiting case mathematically represented by EPR], and through each of which one particle with given initial momentum passes independently of the other. If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction; while of course the conjugate quantities, i.e., the difference of the components of their momenta, and the sum of their positional coordinates, are entirely unknown. In this arrangement, it is therefore clear that a subsequent single measurement 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; at least if the wave-length corresponding to the free motion of each particle is sufficiently short compared with the width of the slits. As pointed out by [EPR], we are therefore faced at this stage with a completely free choice whether we want to determine the one or the other of the latter quantities by a process which does not directly interfere with the particle concerned. (Bohr 1935b, p. 699)

It is important that, as throughout Bohr's reply, his argument here is made in terms of physical bodies (such as diaphragms and their supports or, in the language of Einstein's relativity, rods and clocks) establishing the frame of reference and the corresponding marks, actual or possible, rather than in terms of "position" in some (absolute) empty space. It is this determination by means of measuring rods to which Bohr refers by "the idea of space location." Any such location is a phenomenon defined by the presence of measuring rods, actual or possible, following Einstein's ideas in relativity—the point stressed by Bohr in the end of his reply and in most of his works on complementarity. The predictions in question concerning the second particle depend on a measurement performed upon the first particle, and as such involve the use of rods and, as Bohr explains later, ultimately clocks as well (Bohr 1935b, pp. 700–701). These predictions also depend on the formalism of quantum mechanics, supplied by the numerical data obtained in measurements.

In the EPR experiment, a prediction (say, that of a future position of the second particle) is possible by virtue of further data and hence further measurements, and then further elements of formalism, such as those used by EPR or the transformation theorems invoked by Bohr. The additional elements concern the difference between the positional coordinates of (or associated with) the two particles or the sum of their momenta or, again, the corresponding measurement data in the preceding stage of the experimental procedure. But these data, too, require measuring instruments, such as measuring rods, implied even by the very term "coordinate," which is one of the key functions of Bohr's diaphragm here. Apart from such a measurement, as at least in principle possible, it is impossible to determine (and even meaningless to speak of) either a coordinate of either particle or the difference between those coordinates. The situation is simplified in the case of Bohm's version of the EPR experiment for spin variables, since we do not have to worry about measuring the distance between two objects. Bohr now is ready to conclude that performing (after both particles pass the slits) one of the two possible measurements in question (say, that of momentum) on the first object, S_1, will automatically disable any possible attribution of an alternative conjugate variable, either for the first, S_1, or for the second object, S_2, of the pair. A different set of arrangements and, again, a different EPR pair would be necessary to do so. Bohr expressly states this conclusion in the next paragraph, which I shall discuss presently. First, a qualification is in order.

It might appear from Bohr's elaboration just cited that he accepts that the position or, conversely, the momentum of the second, spatially separated, particle could be *definitively* established or fixed by the measurement of either the position or the momentum of the first particle within the *single* experimental arrangement or *single* phenomenon. Some commentators argue that he does. By the same token, as these commentators also argue, while the complementarity in the sense of the mutual exclusivity of the respective arrangements would be retained, nonlocality would follow, even though Bohr never invokes nonlocality and, I argue, maintains the contrary (Bohr 1935b, pp. 700–701n.).[3] As I explained, Einstein takes a similar view of Bohr's position. Such is not the case,

[3] Among these commentators are B. d'Espagnat (d'Espagnat 1989, pp. 94–95, 255), J. Faye (Faye 1991, pp. 181–182), H. Folse (Folse 1987), J. Honner (Honner 1987, pp. 125–141), D. Murdoch (Murdoch 1987, p. 194), and H. Stapp (Stapp 1989, p. 162). Folse, however, appears to depart from this view in his subsequent commentaries (e.g., Folse 2002).

however, in part in view of the fact that, as the passage just cited indicates and as the next paragraph explains, two interactions between the first object, S_1, and the overall arrangement (several measuring devices are used) are involved in each case of measurement on this object, that of position or that of momentum. The first measurement or at least interaction with a measuring device takes place when this particle passes through one of the two slits in the diaphragm, while the other object, S_2, passes through the other slit. The second and most crucial measurement is that of the position or the momentum of S_1 itself, after it passes the first slit. This measurement would involve an additional measuring device, as Bohr explains in the next paragraph in parallel with his preceding discussion concerning the standard case (Bohr 1935b, pp. 699–700). This sequence of two procedures within the same overall arrangement or set of measuring devices (different for the position and momentum measurement), considered by Bohr earlier in the article, pertains only to S_1 and not to S_2. Thus, this sequence is that of single measurements or predictions pertaining to a single object. The situation is consistent with Bohr's later concept of phenomenon and may be discussed in these terms, as some of the commentators mentioned in Note 3 do. In accordance with this later concept, however, the sequence in question must be seen as involving two phenomena, defined by this sequence, rather than one phenomenon that would comprise all devices involved in the overall arrangement. Any prediction concerning S_2 inevitably involves yet another possible future phenomenon, as opposed to being part of one overall phenomenon, as these commentators argue. For, in accordance with Bohr's concept of phenomena, only a measurement and never a prediction determines measurable quantities concerned, as, moreover, quantities belonging only to measuring instruments.

The term "determine" and the expression "automatically determine" may be the main source of the confusions just indicated, since they may suggest that the position (or, conversely, the momentum) of the second particle would be established or fixed absolutely, that is, regardless of any measuring procedure performed upon it at a later stage. I do not think that this is what Bohr means or how one should read the passage, except, again, to the extent that such a prediction may be considered as in principle verifiable. If, however, as discussed in Chapter 8, the verification of a given prediction is in principle impossible, which becomes the case if the alternative measurement is made, the prediction no longer determines the quantity in question. I am inclined to give Bohr the benefit of the doubt as concerns the consistency of his argument, in view of his overall logic and given what he says in the next paragraph, in which he shifts his terminology from "determination" to "prediction." "Determine" here may be read as "determine our *predictions* (with any desired accuracy)" concerning the position or the momentum of S_2, which predictions should, again, be in principle verifiable. Bohr certainly was aware that one could perform an alternative measurement, say, momentum, on the second particle, thus changing the situation of measurement (phenomenon) to that of the complementary type and thereby defeating any physical determination of the position established by our measurement on the first particle. Bohr's elaboration in the next paragraph and his earlier analysis in the article support this view, although he would still need to have explained his use of "determine." The term may also have to do in part with the mathematical formalism of quantum mechanics, which indeed *determines* such variables and quantities in the sense of *predicting* them. The use of the term appears also related to the fact that Bohr's main target is the ambiguity of EPR's application of their criterion of reality to quantum phenomena, even if one accepted their criterion itself. As I argue here, even the determination by means of predictions, along the lines of EPR's criterion, is sufficient for Bohr's counterargument, once one exposes that both such determinations are never possible for the same quantum object since they cannot be accomplished apart from the involvement of measuring instruments. In this sense, "determine" would mean determine in one of the two mutually exclusive setups and not the other, and, hence, also unless an alternative *measurement* on S_2 (as the object under investigation) is performed, since, as explained in Chapter 8, this act irrevocably alters the setup.

Bohr's next paragraph clarifies his argument further and immediately leads to his conclusions concerning the essential ambiguity of the EPR criterion of reality and hence the problem of their argument. As he says,

> Like the above simple case of the choice between the experimental procedures suited for the prediction of the position or the momentum of a single particle which has passed through a slit in a diaphragm, we are, in the "freedom of choice" offered by the last arrangement, just concerned with a *discrimination between different experimental procedures which allow of* [for?] *the unambiguous use of complementary classical concepts*. In fact to measure the position of one of the particles can mean nothing else than to establish a correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference. Under the experimental conditions described such a measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with respect to this space frame when the particles passed through the slits. Indeed, only in this way we obtain a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus. (Bohr 1935b, pp. 699–700)

Bohr's syntax here is strained. On the other hand, Bohr's customary carefulness is noteworthy. In particular, in defining the position measurement, he rightly speaks of "a correlation between the *behavior* [of the object in question] and some instrument." According to Bohr's ultimate interpretation, which this formulation anticipates, a position cannot be attributed to the object anymore than any other physical property, although, as will be seen in the next chapter, it is still possible to speak of a "correlation" here. Even within the scheme of his reply, however, the concept cannot be used apart from considering the role of the measuring instrument through which, and only through which, any reference to "position" is possible. Bohr uses the same type of locution in speaking of "the idea of momentum in predictions regarding the behavior of [an object]," that is, its interaction with an instrument we use to measure a change in momentum of the corresponding part of the apparatus under the impact of its interaction with a quantum object. It is worth keeping in mind that the instrument in question here is an additional measuring device (added to the diaphragm with the two slits used at the first stage of the overall experiment).

The mutual exclusivity of the two alternatively specified arrangements and of the position or the momentum measurements upon S_1 and the corresponding predictions concerning S_2 now follows automatically, and is, again, analogous to the standard situation of quantum measurement. As Bohr says,

> By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles and therefore have lost our only basis for an unambiguous application of the idea of momentum in predictions regarding the behavior of the second particle. Conversely, if we choose to measure the momentum of one of the particles, we lose through the uncontrollable displacement inevitable in such a measurement any possibility of deducing from the behavior of this particle the position of the diaphragm relative to the rest of the apparatus, and have thus no basis whatever for predictions regarding the location of the other particle. (Bohr 1935b, p. 700)

This deduction is a straightforward application of Bohr's argument that one is not dealing here with a single specified arrangement but with two different, mutually exclusive such arrangements. It is, again, not simply that, in dealing with the same quantum object, we can only select making either one measurement on S_1 and, correspondingly, one prediction concerning S_2, or making the other measurement and its corresponding prediction—a limitation which is taken into account by EPR. By fixing the measuring arrangement in one way or the other, as suited to one complementary situation or the other, we also fix the object or the EPR pair we deal with. In order to perform an alternative prediction and measurement we would need to prepare the whole situation anew, ab ovo, beginning with a new pair of objects.

It is also clear that, as noted earlier, the EPR type of "determination" of the final position of S_2 "at the critical final stage of the measuring procedure" amounts to a prediction of such a position (with probability equal to unity) only if the corresponding measurement is to be performed or could in principle be performed and verified. It is a prediction of a possible "correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference." For the moment, it is assumed, in accordance with EPR's view and in order to counter their argument, that it is this type of measurement—that of the position, rather than a momentum measurement—that *will be* performed, thus making this determination unambiguous. (A momentum measurement on the second particle, again, amounts to a change of the setup.) When the measuring device used to measure the position of S_1 in the final measurement in the overall sequence is fixed, the same arrangement will "provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with respect to this space frame when the particles passed through the slits" and "a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus" (Bohr 1935b, p. 700, paraphrased). *In this arrangement*, however, in order to make sure that the EPR experiment is actually enacted, the fact that S_2 has indeed passed through the slit could only be properly confirmed with a separate measurement verifying the EPR prediction, that is, the prediction enabled by the final measurement performed on S_1. Otherwise—that is, if any attempt to verify the fact of the passing of S_2 through the slit is made (which could only be done by an act of measurement involving one device or another)—S_2 would be displaced or destroyed, which would automatically disable any predictions concerning it by means of a measurement on S_1. But then, the same considerations pertain to S_1, to begin with, if we want to ensure the physical possibility of the situation!

It would, accordingly, be inaccurate, although tempting, to speak of either object passing through the slit as an act of measurement on that object, although the fact that each did pass through its respective slit in this overall arrangement is essential for properly enacting the EPR experiment. The only proper measurement in this sequence—prior to the final critical stage of measurement upon the first particle ultimately enabling the EPR prediction—may be said to be the measurement of the distance between the two particles passing through the slits or, simultaneously, the measurement of the sum of their momentum. This simultaneity is possible, and these variables themselves commute and, hence, are not subject to the uncertainty relations. Bohr's arrangement physically enacts this situation.[4]

The situation of the momentum measurement and prediction concerning S_2 is entirely analogous to that of the position measurement and prediction as concerns the character of all phenomena involved (either in the conventional sense or in Bohr's sense of the term). It is clear that this situation is inevitably mutually exclusive to that of the EPR position measurement and prediction, just as it is in the standard case. The diaphragm (which is now used to measure

[4] If one wants to describe this situation in terms of phenomena in Bohr's sense, it would appear as follows. All phenomena that occur in this setup (and the same is true for the complementary setup designed for momentum measurements) are associated with S_1, and any phenomenon associated with S_2 would necessarily involve a separate local measuring arrangement and require an actual measurement to be performed upon it. S_2—especially once it passes through the slit, which is when we are specifically concerned with it in the EPR context—is never part of any phenomenon in Bohr's sense, unless some local measurement directly associated with it is performed. Some phenomena involved pertain to the apparatus itself, including the measurement (entirely classical) of the distance between the slits, and these may be seen as the *preconditions* of the final (EPR) measurement, performed at the next stage on the first object. It is the latter measurement that makes the prediction concerning the final position of S_2 possible, which is strictly in accordance with Bohr's definition of phenomenon as relating to a single registered measurement. It is also clear that temporal considerations are important here and further indicate that it would be inaccurate to include the final (predicted) position of S_2, measured or not, in the same phenomenon with the final positional measurement on the first object, thus defined strictly locally in physical terms. Hence, it is difficult to agree with those commentators cited above (Note 3), who argue that Bohr sees both particles of a given EPR pair as linked to a single phenomenon (in Bohr's sense) and sees the overall situation as nonlocal.

the combined initial momentum of both objects, necessary for the EPR predictions) and an additional part of the overall apparatus (necessary to measure the momentum of the first particle, in the final critical stage of the experimental procedure) are loose relative to the support. Therefore, it is impossible to use the diaphragm to measure the position of (or associated with) object S_1 and hence to predict the position of (or associated with) object S_2. It, again, follows that in order to perform both of these measurements and to be able to make both of the predictions in question, one needs two mutually exclusive experimental setups and, by the same token, *two* preparations of the experiment. These two preparations involve two different EPR pairs, which cannot be properly coordinated so as to be able to think of the quantities in question as pertaining to the same quantum object—the second object, S_2— of a single EPR pair.

9.4 Essential Ambiguity and Essential Influence

One can now understand Bohr's contention that "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'" (Bohr 1935b; p. 700; emphasis added). (I shall explain my emphasis on Bohr's "now" momentarily.) With this sentence Bohr enters the most famous elaboration of his reply. It was seen as crucial by Bohr himself, who, as noted earlier, cited it and commented on the difficulties of writing and reading it in "Discussion with Einstein" (Bohr 1949, *PWNB* 2, p. 61). It is, I would argue, also the most misunderstood (especially, again, as concerns the nature of the "influence" Bohr appeals to), even by sympathetic commentators, primarily because Bohr's preceding argumentation is not adequately considered. Bohr's "now" is important, and it means "after the preceding analysis." Both this "now" and this analysis are often disregarded by those who comment on the passage, which is sometimes taken for Bohr's *argument* rather than *the conclusion derived* from an argument, or is considered on the basis of experimental facts and theoretical arguments other than Bohr's argument in his reply. I would like, accordingly, to offer a reading of this passage in relation to Bohr's preceding argument.

I shall divide Bohr's elaboration, here designated as **[B]** (for Bohr), into key units (**[Bn]**). It is, I shall argue, crucial for understanding Bohr's points in this elaboration how one parcels and groups these units, and I shall suggest a grouping different from that used in most readings.[5] I shall also cite Bohr's

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[5] Murdoch (Murdoch 1987, pp. 170–171) is an exception. However, his reading of Bohr's reply is essentially different from the one offered here and, in my view, does not offer an adequate analysis of Bohr's argument. In particular, Murdoch maintains, close to the lines of Einstein's reading of Bohr's reply, that Bohr "held no strong view on the question of locality," and hence, would in principle allow for nonlocality (Murdoch 1987, p. 185). Murdoch also reads Bohr's concept of phenomena as applied to the EPR experiment along these lines (Murdoch 1987, p. 194).

parallel elaboration, designated as **[b]**, from his initial short reply to EPR in *Nature*, divided into parallel units (**[bn]**). One might see **[b]** as revised by Bohr into **[B]** and, accordingly, not consider it germane. One might, however, also see it as helpful in elucidating Bohr's meaning in **[B]**, especially since both texts were written just about simultaneously. According to Bohr,

> **[B1]** 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."
>
> **[B2]** 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.
>
> **[B3]** 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.*
>
> **[B4]** 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.
>
> **[B5]** 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 object and the measuring instruments in the field of quantum theory.
>
> **[B6]** 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. (Bohr 1935b, p. 700; Bohr's emphasis)
>
> **[b1]** I should like to point out, however, that the named criterion contains an essential ambiguity when it is applied to problems of quantum mechanics.
>
> **[b2]** It is true that in the measurements under consideration any direct mechanical interaction of the system and the measuring agencies is excluded,
>
> **[b3]** but a closer examination reveals that the procedure of measurements has an essential influence on the conditions on which the very definition of the physical quantities in question rests.
>
> **[b4]** Since these conditions must be considered as an inherent element of any phenomenon to which the term "physical reality" can be unambiguously applied, the conclusion of the above mentioned authors [EPR] would not appear to be justified. (Bohr 1935a, *QTM*, p. 144)

As we have seen, yet another version of **B[1]** and **b[1]** appears earlier in his reply:

> **[B1a]** ... a criterion of reality like that proposed by the named authors contains—however cautious its formulation may appear—an essential ambiguity when it is applied to the actual problems with which we are here concerned. (Bohr 1935b, p. 697)

The invocation of "ambiguity" in **B1** and Bohr's claim that this ambiguity arises "as regards the meaning of the expression 'without in any way disturbing a system' " is often taken in conjunction with **B3** (or **b1** in conjunction with **b3)**. This reading would imply that, according to Bohr, there is (contrary to EPR's claim) some form of "disturbance" or "influence" of our manipulations of S_1 on the second, spatially separated, object, S_2, of the EPR pair (S_1, S_2). As must be apparent from the analysis given here, it is difficult to argue that this is what Bohr has in mind either in **B1** or in **B3**. There appears to be little, if anything, to support such a view either in **B3** itself or in the extended elaboration in question or in Bohr's preceding argument in his reply. As I said, the latter is clearly important, as the opening phrase "from *our point of view* we now see" of **B1** and "as appears from the preceding discussion" in **B5** indicate by stating that Bohr has just offered an argument demonstrating that EPR's criterion is ambiguous. These nuances are usually ignored by commentators, along with Bohr's preceding argument itself. In order to properly understand the meaning of **B1**, one must consider other propositions in Bohr's reply concerning what can and cannot be unambiguously done in quantum theory. Once one does so, it becomes apparent that the "ambiguity" here need not be read as stating that, contrary to EPR, there is in fact a "disturbance" in making the EPR predictions.

Instead, as I argue here, this ambiguity has to do with the conditions under which—in the EPR situation or in the case of quantum phenomena in general—an unambiguous meaning can be assigned to the terms and concepts, such as EPR's "elements of reality," that define the situation in question. These are the conditions of the interaction between quantum objects and measuring instruments, ultimately leading Bohr to his concept of phenomenon, but in place in his reply as well. Once one conjugate quantity in question is established (even on the basis of a prediction, in accordance with EPR's criterion of reality, via the corresponding measurement on S_{11}) for S_{12}, we cannot ever establish the second quantity involved without measuring and hence, in this sense but only in this sense, without *disturbing* it. We can only establish such a quantity for a different quantum object, S_{22}, via a different EPR pair (S_{21}, S_{22}), by a different measurement on S_{21}. Nor can these two determinations be coordinated so as to assume that both quantities could be associated with the same object of the same EPR pair. Thus, the ambiguity in question clearly relates to the clause "without in any way disturbing the system," which, accordingly, requires a qualification if one wants to apply it rigorously in the EPR situation, since, as explained in detail in Chapter 8, we can never determine both conjugate quantities for the second object of any EPR pair, without disturbing this object, but only one of these quantities. Bohr, again, *agrees* with EPR that we cannot allow for any "disturbance," physical or other, of anything that is spatially separated from actual physical measurement.

One might say that the reading of Bohr's elaboration and the key terms and concepts involved in it turns on how one reads Bohr's "But" in **B3** or **b3,** in particular as defined by two "sequences," **Sequence 1** and **Sequence 2**. **Sequence 1** is from **B2**—"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"—to **B3**—"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.*" **Sequence 2** is from **B2**—"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," thus taking locality as an axiom—to **B3 and B4**—"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*," **and** "since these conditions constitute an inherent element of the description of any phenomenon to which the term 'physical reality' can be properly attached ['unambiguously applied'], we see that the argumentation of the mentioned authors does not justify their conclusion that quantum mechanical description is essentially incomplete." That is, **B3** and **B4** must be considered together!

It becomes clear that Bohr does not speak of an ambiguity as concerns the expression "without in any way disturbing the system" because there is a physical (mechanical) disturbance or influence between the two spatially separate physical situations in question. Instead, it is a question of our manipulations of—our "influence" on—S_1. Bohr refers to his argument that, given the mutually exclusive conditions of measurements and predictions in EPR's case, the two alternative predictions in question can be made only in two mutually incompatible experimental setups and, it follows, only for two different quantum objects. Neither these conditions nor this inescapable fact are specified by EPR, thus making their criterion ambiguous and their argument, based on the view that this can be done for the same quantum object, unsuitable for demonstrating either the incompleteness or the nonlocality of quantum mechanics. It is, again, this situation itself—or, one might, again, say, "reality"—that, while thus coherently covered by quantum mechanics, stands in the way of EPR's criterion of reality, which need not necessarily correspond to nature, and then of their argument. This, then, is why Bohr says that the EPR criterion of physical reality contains "an [essential] ambiguity as regards the *meaning* of the expression 'without in any way disturbing the system' " (Bohr 1935b, p. 700). It is, as we have seen, an obvious consequence of Bohr's interpretation (earlier or later) that in any quantum mechanical measurement, the language of "disturbance" cannot apply in the sense that there are any undisturbed properties of quantum objects that are then disturbed by measurement. Nor, however, does one need to conclude—if one considers the situation in terms of Bohr's concept of phenomenon—that because we cannot speak of the properties of objects before experiments establishing them are performed (and in Bohr's later view, we cannot do so even then), the two quantum systems in question form an inseparable nonlocal whole. These quantum systems or the physical situations of measurement are seen as spatially separated, and whatever can in fact be ascertained concerning them in space–time—that is, any phenomena associated with them—would be spatially separated. Indeed, as I have said, the concept of locality can be given a proper meaning only in this classical domain—which of course need not mean that the quantum domain must be assumed to be non-local in view of the EPR experiment.

It is thus clear that the "influence" in Bohr's statement does not refer to any physical influence upon the system associated with S_2 (concerning which we make predictions but upon which we do not perform measurements) because of our interference with S_1. Instead, it refers to the physical influence upon—a *fixing* of—the measurement-prediction situation defined by the particular setup of measuring instruments physically associated with and only with S_1 (upon which we do perform measurements and with which we, hence, interfere). As a result, this influence determines—it "influences"—what kind of predictions we can or cannot make concerning the second object. It defines the conditions of one or the other of the two—always irreducibly mutually exclusive—situations in the EPR-type experiments or indeed in all quantum mechanical predictions. If the apparatus is set up for measuring one complementary quantity for the measuring system associated with the first particle, then within this setup the other variable cannot be *unambiguously defined*—we are absolutely precluded from doing so—for either S_1 or S_2. That is, such is the case, again, unless we independently perform an alternative measurement on S_2—which, however, amounts to a change of setup and disables any possible determination of the initial variable or quantity for it. The influence Bohr has in mind is thus clearly that upon the conditions of an unambiguous definition of the quantities in question, and, as I said, in this sense "influence" may not be the best term to use here. These conditions physically affect only S_1, where direct physical measurements are performed, although the prediction and the possibility of definition themselves also concern S_2, which is not physically influenced, and nor is anything that can be associated with it. This is why, while Bohr says that there is "*an influence on the very* ***conditions*** *which* ***define the possible types of prediction*** *regarding the future behavior of the system*," he never says that anything disturbs, interferes with, or even influences S_2. The former need not—and in Bohr's argument does not—entail the latter.

In sum, Bohr questions the conditions of the unambiguous applicability of EPR's criterion of reality and the adequacy of their argumentation, given the nature of the phenomena in question, while remaining in agreement with EPR's locality requirement. Accordingly, and given that quantum mechanics properly responds to the situation, EPR's conclusion that it is incomplete by the EPR criteria need not follow. Hence, Bohr concludes,

> **[B5]** On the contrary this [quantum-mechanical] 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.

> **[B6]** 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. (Bohr 1935b, p. 700; Bohr's emphasis)

In other words, interpreting quantum phenomena and quantum mechanics in terms of complementarity allows one to give a proper scientific grounding to the physical laws—reflected in and encoded on the formalism of quantum mechanics—that otherwise appear mystical rather than scientific. Of course, it does so at the cost of suspending certain *epistemological* principles. These principles may appear to some, such as Einstein, equally basic; some may even regard them as physical principles. Perhaps chief among them is the possibility of offering a realist and preferably causal description of the objects and processes considered by the theory—at least, again, by way of idealized models. However, even if nature may allow for a more complete theory of the type Einstein wants, EPR do not demonstrate, as they appear to think they do, that nature does so. Nor (as I explained in Chapter 8) does it follow, as an alternative, that quantum mechanics is nonlocal, as EPR contend. On the other hand, as Bohr is about to point out in his final footnote (to be discussed presently), all the phenomena in question and Bohr's argument itself are fully consistent with relativity. This makes quantum mechanics a local theory, at least as things stood then and as they still stand now—unless a different argument for its nonlocality is offered, which thus far does not appear to be the case.

The argument of Bohr's reply is, thus, defined by the two uses of the term "essential" in Bohr's writing, where it recurs often—an *essential ambiguity* of EPR's criterion and argument vis-à-vis the *essential influence* of the conditions of measurement on the very definition of all the physical quantities associated with quantum phenomena. The essential ambiguity of EPR's application of their criterion to the quantum mechanical situation arises from the insufficient consideration of these conditions, defined by the *essential role of measuring instruments* in determining all quantum phenomena, including those in question in the EPR experiment. This essential role leads to "an *essential influence* on the conditions on which the very definition of the physical quantities in question rests"—an influence missed by EPR (Bohr 1935a, *QTM*, p. 144). This omission makes the application of their criterion of reality essentially ambiguous. The removal of this ambiguity is, I argue here, possible and is in effect enacted by Bohr in his reply. This removal, however, disables the logic of EPR's argument and renders this argument insufficient to make the case EPR want to make.

9.5 From Temporality to Relativity

Before Bohr offers his final footnote on the compatibility of quantum mechanics and relativity, he considers two other subjects that amplify and allow him to properly complete his argument. The first is the role of time in those quantum phenomena where this role could no longer be neglected, while it was not essential to his argument thus far; and the second is the question of the difference, sometimes known as the "cut," "between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation" (Bohr 1935b, pp. 700–701).

As might be expected from his previous exchanges with Einstein, discussed in Chapter 8, while the temporal dimensions of the EPR situation are important, they do not introduce significant new difficulties. The reason for this is that the type of reasoning Bohr already offered in his reply would also apply to them, now by considering the complementary nature of time measurement and the application of the law of conservation of energy.[6] Bohr writes,

> Just as in the question discussed above of the mutually exclusive character of any unambiguous use in quantum theory of the concepts of position and momentum, it is in the last resort this circumstance [the mutually exclusive control of time-measurement and the application of the law of conservation of energy] which entails the complementary relationships between any detailed time account of atomic phenomena on the one hand and the un-classical features of intrinsic stability of atoms, disclosed by the study of energy transfers in atomic reactions on the other hand. (Bohr 1935b, p. 701)

Bohr ends his reply (almost: a punch line on locality is yet to come) with the discussion of the "necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a *principal distinction between classical and quantum-mechanical description of physical phenomena*." For, while "in classical physics the distinction between object and measuring agencies does not entail any difference in the character of the description of the phenomena concerned," in quantum physics it does (Bohr 1935b, p. 701; also Bohr 1949, *PWNB* 2, p. 50; Bohr 1958, *PWNB* 3, p. 3).

This question is often referred to as the question of the "cut" [*Schnitt*] between the quantum and the classical, the term more commonly used by Heisenberg and von Neumann than Bohr, and, for that reasons, also known as the Heisenberg or Heisenberg–von Neumann cut. Bohr's statement just cited may suggest that, while parts of measuring instruments are described by means of classical physics, the independent behavior (in space and time) of quantum objects is described by means of the quantum mechanical formalism. As discussed in Chapter 6, while this view of the situation is not uncommon, it is not that of Bohr, at least not after he revised the Como argument, which gravitates

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[6] There are well-known difficulties concerning the time variable in quantum mechanics, since it is not defined as a Hilbert-space operator and cannot be related to the energy operator H in the way the position and momentum operators are related, $PQ - QP = ih/2\pi$. Indeed it is not possible to construct an operator T such that $HT - TH = ih/2\pi$ for a physically meaningful H. These difficulties do not affect Bohr's argument, discussed in Chapter 8, concerning Einstein's photon-box experiments, to which he refers in the note (Bohr 1935b, pp. 701–702n.). Cf. also (Peres 1993, pp. 413–415).

to this type of view. In the statement just cited, Bohr obviously does say that *parts* of measuring instruments are described by means of classical physics, again, with this crucial qualification that this description only concerns parts of measuring instruments. He does not say, however, and does not mean (there is no evidence to conclude otherwise) that the independent behavior of quantum objects is described by quantum-mechanical formalism. As we have seen, beginning with the immediate aftermath of Heisenberg's discovery of quantum mechanics, Bohr rejects the view that the quantum mechanical formalism can unambiguously refer to quantum objects and processes in terms of space–time concepts. Apart, again, from a brief and ambivalent retreat from this view in the Como lecture, he had maintained this position throughout his works, certainly in his reply to EPR.

As Bohr points out next, "it is true that the place within each measuring procedure where this discrimination [between the object and the measuring instrument] is made is in both [classical and quantum] cases largely a matter of convenience" (Bohr 1935b, p. 701). Largely, but not completely, at least in quantum physics! Bohr brings into consideration the transformation theorems, which he invoked from the outset of his reply. The latter mathematically ground the EPR argument and, according to Bohr, "perhaps more than any other feature of the formalism contribute to secure its mathematical completeness and its correspondence with classical mechanics" (Bohr 1935b, p. 696n.). He then says, "By securing its proper correspondence with the classical theory, these theorems exclude in particular any imaginable inconsistency in the quantum mechanical description, connected with a change of the place where the discrimination is made between object and measuring agencies. In fact it is an obvious consequence of [Bohr's] argumentation that in each experimental arrangement and measuring procedure we have only a free choice of this place within a region where the quantum-mechanical description of the process concerned is effectively equivalent with the classical description" (Bohr 1935b, p. 701).

This last point conveys the deeper physical meaning of Bohr's correspondence principle, as he understands it in his post-Como works, and couples it to the mathematical meaning of the principle, defined by the fact that in this region the equations of classical mechanics would give the same predictions as those of quantum mechanics. What makes this point especially important, however, is that quantum objects are always on the other side of the "cut" and may even be rigorously defined accordingly. So are, as quantum objects in their own right, those parts of the measuring instruments through which the latter interact with the quantum objects under investigation, or of course the overall quantum constitution of these instruments or other macro-objects. At one end, then, by virtue of their classical nature, the individual effects in question can be isolated materially and phenomenally (in the usual sense)—we can perceive and analyze them as such—once an experiment is performed. They cannot be separated from the process of their physical emergence by our even conceiving of, let alone analyzing, this process. This impossibility will define the indivisible wholeness or "atomicity" of phenomena in Bohr's sense, as developed in his later works. By contrast, at the other end, quantum objects and processes can never be isolated, either materially (from the measurement process and measuring instruments) or mentally insofar as we cannot, even in principle, conceive of what actually happens at that level or how. They can only be seen as cut off *from our thought*, in the epistemological sense of being inaccessible to it. Hence, Bohr speaks of "the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or 'individuality' [atomicity], characterizing the elementary processes" (Bohr 1949, *PWNB* 2, p. 34). Complementarity allows one to do precisely this and, thus, to deal with "regularities beyond the grasp of [classical] description" (Bohr 1949, *PWNB* 2, p. 41).

This argumentation is, however, fully consistent with that for the necessity of using "the classical frame of concepts" at the level of the effects defining quantum phenomena, and this use is crucial in enabling complementarity to achieve the task of handling such regularities. As I have argued from the outset, classical concepts are necessary already in order to establish the peculiar features of these effects and their quantum origins, which are, at the same time, beyond their grasp. This is a difficult point to confront and understand, as Heisenberg noted in his commentary on EPR's paper, written before Bohr's reply. This commentary was centered on the idea of the "cut," and specifically the possibility of shifting it, again, up to a certain point. As he wrote to Bohr at the time:

> Why the possibility of shifting the "cut" is so particularly important in my opinion, I can most simply explain thus: You say correctly that "all elements of *description* are defined classically and yet the classical theory leaves no room for quantum-mechanical laws." This statement appears to physicists used to think formally as a plain contradiction, as I know for instance from talking to *Herr* von Laue. Hence I thought it to be important to stress the property of the formalism which ensures that no contradiction arises here, and this, it seems to me, lies in the possibility to shift the cut. If this were not so, simply two categories of physical systems—classical and quantum-mechanical ones—would exist, and one could *never* apply classical concepts to the latter. That's how von Laue sees the situation. I believe that then it might be very difficult to argue against the hope of a later causal supplement. (Heisenberg to Bohr, 29 September 1935; cited in *MR* 6, pp. 733–734; emphasis on "description" added)

As discussed in the Introduction, Heisenberg (rightly) sees this situation as at the core of the Copenhagen interpretation (as he understand it), which "starts from the fact that we describe our experiments in the terms of classical physics and at the same time from the knowledge that these concepts do not fit nature accurately. The tension between these starting points is the root of the statistical character of quantum theory" (Heisenberg 1962, p. 56). He also argues, as he does in his commentary of EPR's paper, that, far from being paradoxical, the situation in fact ensures the consistency of the overall "Copenhagen" scheme, and on this point his own and Bohr's views are pretty much the same. I shall leave aside Heisenberg's own view of the cut, which appears to be somewhat different from that of Bohr. Bohr's reply to EPR, however, was clearly influenced by Heisenberg's commentary (which was not published as an article) and by this correspondence, since, as just explained, these considerations and their rigorous connections to the formalism of quantum mechanics, explored by Heisenberg, are crucial to Bohr's reply. In particular, Bohr's statement, cited earlier, to the effect that complementarity "provides room for new physical laws" (Bohr 1935b, p. 700) appears to echo or respond to Heisenberg's point that "the classical theory leaves no room for quantum mechanical laws." Reciprocally, Bohr's complementarity viewpoint was crucial to Heisenberg's argument, including as concerns the impossibility of applying visualizable concepts, or the statistical character of quantum mechanics as different from that of classical statistical physics. As Heisenberg says, "Hence the possibility of different, complementary contexts of observation, unknown in the classical theory, becomes responsible for the occurrence of statistical laws" (Heisenberg in Pauli 1979–1999, vol. 2, p. 418; cited in *MR* 6, p. 733). As we have seen, Bohr had maintained this view since 1928.

The key point at the moment is that classical systems, such as those of measuring instruments, may at certain points need to be treated quantum mechanically, in particular, as we have seen, insofar as their behavior may, in certain circumstances, be constrained by the uncertainty relations. (The ultimate constitution of classical system may, again, be quantum, but this fact is not relevant here [Bohr 1949, *PWNB* 2, p. 51].) By contrast, certain (elementary) quantum mechanical systems can never be treated classically, while certain (macro) quantum mechanical systems may be treated both classical and quantum mechanically. In this respect the cut may not be as arbitrary as may appear from this passage, although not from Heisenberg's argument as a whole. Heisenberg admits later in his discussion that, in Mehra and Rechenberg's words, even in the context of measurement, "the cut cannot be shifted so arbitrarily that certain measuring devices operating like atomic systems (e.g., nuclear systems measuring the neutron flux) are described by classical theory" (*MR* 6, p. 732; Pauli 1979–1999, vol. 2, pp. 413–414). One would still need some classically describable device to register the actual results of such a "measurement" (and the term itself could only apply then). In any event, Heisenberg is right in his main argument for the classical *description* of all the experimental effects in question and yet the impossibility of comprehending the quantum mechanical laws by means of classical theories.

Both Bohr's and Heisenberg's analyses of the "cut" amplify the point that it is the nature of quantum phenomena and of the relationships between the classical and the quantum strata in the constitution of quantum phenomena that stands in the way of the EPR argument. On the other hand, quantum mechanics coherently covers this situation, by virtue of the very same transformation theorems that enable EPR predictions. Bohr now adds as a kind of postscript on the relationships between quantum mechanics and relativity.

> Before concluding I should still like to emphasize the bearing of the great lesson derived from general relativity theory upon the question of physical reality in the field of

> quantum theory. In fact, notwithstanding all characteristic differences, the situations we are concerned with in these generalizations of classical theory present striking analogies which have often been noted. Especially, the singular position of measuring instruments in the account of quantum phenomena, just discussed, appears closely analogous to the well-known necessity in relativity theory of upholding an ordinary description of all measuring processes, including a sharp distinction between space and time coordinates, although the very essence of this theory is the establishment of new physical laws, in the comprehension of which we must renounce the customary separation of space and time ideas. (Bohr 1935b, p. 701)

Bohr moves from temporality to relativity, a theory that radically reshaped our ideas concerning time, and he does so via the problem of the cut, which deals with the space, physically, in between classical and quantum physics, while relativity may be seen as located between both in conceptual terms. And via relativity, Bohr now moves to locality. Apart from the power of the epistemological parallel between the two theories, extended through the rest of the elaboration and giving the article a fitting closure, also as a letter to Einstein, Bohr's most important and most resistant addressee, Bohr's footnote here adds a punch-line point on the locality of quantum mechanics. Bohr does this almost in passing, and perhaps the point should have been given more prominence. It is decisive, nevertheless. Bohr says, "Just this circumstance [the singular position of measuring instruments in the account of quantum phenomena], together with the relativistic invariance of the uncertainty relations of quantum mechanics, ensures the compatibility between the argumentation outlined in the present article and all exigencies of relativity theory" (Bohr 1935b, p. 701n.).

All exigencies of relativity theory! It is an intriguing (although hardly surprising) fact that some of the key discoveries in physics or many other fields in science and beyond were introduced by way of a footnote, and quantum mechanics is no exception. Born's interpretation of the wave function is the most remarkable example. Quantum mechanics can be considered as both a local and a complete theory of quantum phenomena, those in question in the EPR experiment included, even if ultimately at the cost of the impossibility of any description of quantum objects and processes (or quantum reality) themselves. That, however, may be the cost extracted from us by nature, and appears to be, at least for now. It is, again, not inconceivable (although not certain) that we—that is, those of us who see it as a cost—will pay a still greater cost in the future. On the other hand, some of us may see this cost as an investment, but into a different portfolio of thought and knowledge.

Chapter 10
Mysteries Without Mysticism, Correlations Without Correlata, Epistemology Without Ontology, and Probability Without Causality

Abstract This chapter considers the nonclassical version of Bohr's interpretation of quantum phenomena and quantum mechanics as complementarity, developed by him in terms of his concepts of phenomena and atomicity following his exchange with EPR, and the view of probability arising from this interpretation or, more generally, from the nonclassical epistemology of quantum theory. Section 10.1 offers a commentary on "quantum mystery." Section 10.2 addresses the nature of quantum correlations as "correlations without correlata." Section 10.3 is a discussion of quantum epistemology as epistemology without ontology via the irreducible role of measuring instruments in the constitution of quantum phenomena, and Bohr's concepts of phenomena and atomicity. Section 10.4 considers quantum probability as a nonclassical form of Bayesian probability. It includes a discussion of Einstein's arguments concerning the incompleteness of quantum mechanics as, in his view, only a statistical theory of ensembles rather than a theory describing individual quantum processes.

10.1 Mysteries Without Mysticism

My argument in this chapter takes advantage of the four concepts of my title, which reflect different but interrelated aspects of the quantum mechanical situation as it appears from the nonclassical perspective of this study. I would like to emphasize that these are *philosophical concepts* in the sense defined in the Introduction, and accordingly they have their particular architecture, which this chapter delineates as well. It might be useful to begin by offering a preliminary summary of each concept.

The first concept reflects the mysteriousness of quantum phenomena—which is, as we have seen, often invoked in discussing them—but liberates this concept from, in Bohr's words, any "underlying mysticism foreign to the spirit of science" and thus makes quantum mysteries into "mysteries *without* mysticism" (Bohr 1949, *PWNB* 2, p. 63). This is accomplished by means of nonclassical thinking about quantum phenomena, which is

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reflected in my third concept, that of "epistemology without ontology," that is, without any specifiable quantum-level ontology, which concept defines nonclassical epistemology. Quantum physics is mysterious only insofar as no final explanation of how quantum phenomena come about or what quantum objects ultimately are and how they behave appears to be possible, but not because there is some "mystical" agency (modeled on theology) in charge of the situation.

The second concept defines quantum correlations (such as those of the EPR type) as "correlations without correlata." This expression is courtesy of N. David Mermin (1998a). However, here this concept will be brought closer to "the spirit of Copenhagen" and of nonclassical thinking than Mermin's conception thus designated. While Mermin is sympathetic to Bohr in advancing this conception, he also seeks to depart from Bohr in his view of quantum mechanics, especially as concerns the irreducible role of measuring instruments in the constitution of quantum phenomena. In the present view, however, it is this role that deprives quantum correlations of their *ultimate* correlata and, by doing so, grounds the present concept of "correlations with correlata," in the following sense, defined by the nonclassical epistemology of quantum phenomena. While such phenomena themselves may be seen as the (physical) correlata of the correlations in question, that which is ultimately responsible for the emergence of these phenomena and these correlations—what we refer to as quantum objects and processes—is beyond our reach and hence cannot be given any specifiable ontology. In other words, in the present view, to argue for correlations without correlata is the same as to argue for "epistemology without ontology" at the quantum level.

"Epistemology without ontology" is my third concept here. This epistemology is essentially nonclassical epistemology, which is developed by Bohr in his later works in terms of his two correlative (with correlata) concepts of phenomena and atomicity.

Finally, the fourth concept, that of "probability without causality," reflects the unavoidably probabilistic nature of our predictions concerning quantum phenomena even in dealing with individual quantum events and the apparent impossibility of ascribing causality to the quantum processes responsible for these events. This concept unavoidably arises in a nonclassical view of the quantum situation and is linked here to the concept of correlations without correlata. These two concepts jointly define the character of quantum probability by connecting the irreducible randomness of individual quantum events to the correlational order that quantum phenomena exhibit in certain circumstances, but crucially not in all circumstances, as exemplified in the double-slit experiment. I relate this situation and, thus, the concept of probability without causality to the Bayesian view of probability, in turn considered in nonclassical terms, since this view can be operative in classical situations as well.

Quantum mechanics and higher level quantum theories, or the constitution of nature according to these theories, are often viewed as strange and mysterious by their friends and foes alike. One could cite numerous statements that testify to this widely held perception and display a broad spectrum of valuations of this mysteriousness itself. One of the most eloquent expressions of this mysteriousness is found in Mermin's article, "Spooky Actions at a Distance: Mysteries of the Quantum Theory," included, along with several other helpful essays on the subject, in his *Boojums All the Way Through: Communicating Science in a Prosaic Age* (Mermin 1990). The phrase "spooky actions at a distance" is of course courtesy of Einstein, who did not believe that the quantum problem or enigma was really solved by quantum mechanics because, in his view, if considered as a complete theory of individual quantum processes, quantum mechanics implied "spooky actions at a distance"—the mysterious correlations between distant events. As will be seen, in his view, only a theory of continuous fields (modeled on Maxwell's electrodynamics) could properly solve it or, in any event, would be an acceptable solution to him. For the reasons explained in this study, many, Bohr among them, would not find any *physical* "action at a distance" there or in other quantum phenomena. Einstein's view was tempered by his statistical qualifications to be considered later in this chapter, although, as he said, in the same letter to Born and elsewhere, he still did not believe in quantum mechanics. Instead, one might speak, as I do in this study, of *spooky predictions at a distance*, insofar as there is not and does not appear to be a proper physical justification for these predictions. Accordingly, most would see the EPR situation as "spooky," strange, albeit not surprising by the standard of quantum mechanics or the world according to it.

Thus, confounding our "prosaic age," the EPR and other quantum experiments reveal that, in Mermin's words, "the world behaves in a manner that is exceedingly strange, deeply mysterious, and profoundly puzzling" (Mermin 1990, p. 126). Quantum phenomena may be a particularly spectacular example (although relativity is nearly as spectacular) of the fact that, as Feynman said, "nature's imagination far surpasses our own" (Feynman 1965, p. 162). To risk a strong claim, nothing in the preceding human history, in our conscious thinking and imagination or in our dreams, was able to come up with or prepare us for what these phenomena show us. In Wheeler's words, "What could one have dreamed up out of pure imagination more magic—and more fitting—than this?" (Wheeler 1983, *QTM*, p. 189). One may even wonder: "More fitting" than what or fitting to what, apart from nature itself, to which we can respond with thought that is capable of giving a physical and philosophical meaning to this situation? Our thought, this study suggests, might not be able to offer this response without making the inconceivable, the unthinkable, part of this meaning. Our thought and imagination can, however, lead us to this thinking via the unthinkable as part of physics as a mathematical science of nature, which is what happened in Heisenberg's discovery of quantum mechanics.[1]

There are exceptions, not so much to this view of quantum phenomena as in this sense mysterious, which is difficult to avoid, but to the use of this dramatic way of describing the quantum mechanical situation—Bohr, notably, among them, as Mermin observes in the same article and in his review of Bohr's philosophical works (Mermin 1990, pp. 114, 136–139). As witnessed throughout this study, Bohr's style of expression is ponderous and tedious, and almost deliberately dry in aiming to avoid any poetic appeal. Instead it is marked by its reflection of and response to the seriousness of the problems at stake as requiring the most rigorous thinking and discipline. There are good reasons for Bohr's reticence and caution. The problem is not so much strangeness or mysteriousness, on which science often depends at least at certain stages, but rather the *mysticism* that is sometimes attached to this mysteriousness. Throughout his writings, Bohr aimed, in his words, to "clear up ... misunderstandings" concerning any "underlying mysticism foreign to the spirit of science" that could be associated with quantum mechanics (Bohr 1949, *PWNB* 2, p. 63; also Bohr 1937, *PWNB* 4, p. 83). It may be noted that at an earlier point Bohr did speak, in a letter to Heisenberg, cited earlier, of "mysticism of nature" (Letter to Heisenberg, April 18, 1925, cited in *MR* 6 p. 163). This statement occurs in the wake of the collapse of the BKS proposal, the last attempt on Bohr's part to retain the space–time description of quantum processes in quantum theory, and hence to solve the quantum problem in a (more) classical way. Bohr's phrase appears to refer to the likely impossibility of such a description, invoked in the letter as well, along with the equally troubling implication "of a coupling of quantum processes in separated atoms," as discussed in Chapter 8. It is equally

[1] It may, in some respects, be true that, as the particle physicist Sydney Coleman (reportedly) said, "if thousands of philosophers spent thousands of years searching for the strangest possible thing, they would never find anything as weird as quantum mechanics" (Randall 2005, p. 117). On the other hand, philosophers, beginning with the pre-Socratics, or poets, have spent thousand of years exploring things that are pretty weird, or in any event, quantum mechanical-like, as Bohr appears to have realized when he spoke of "the epistemological situation here encountered, which *at least in physics* is of an entirely novel character" (Bohr 1937, *PWNB* 4, p. 87; emphasis added). This means that, at least in some respects, this situation might have been previously encountered elsewhere. Human affairs, the main source of philosophical and poetic thinking (although, from the pre-Socratics or indeed Homer on, physics has also served as such a source), are often nearly as strange as and sometimes stranger than quantum physics. Have not Shakespeare, Dostoyevsky, Joyce, Kafka, and Beckett, or Freud and Nietzsche, if not already Plato and the pre-Socratics, told us as much? One need not search for the strangest possible things: One cannot avoid them. It is true that the strangeness of quantum phenomena leaves room for rigorous, mathematically defined laws, without, it appears, leaving room for explaining what is behind these laws. This may be strange. But is it that much stranger than quite a few other things? Is not our capacity to think of these laws, or for that matter to think, just as strange or mysterious? On the other hand, it does need to be seen as mystical.

significant, however, that the letter precedes Heisenberg's discovery of, in Bohr's words, a "*rational* quantum mechanics" (Bohr 1925, *PWMB* 1, p. 48; emphasis added). The latter retained quantum mystery and even possibly made it unavoidable, but deprived it of mysticism, no longer invoked by Bohr from that point on. Quantum mystery is the behavior of quantum objects, which we cannot know and conceive of, and the ways they produce their (often strange or mysterious) effects upon the world we can know and conceive of. Quantum mechanics nevertheless predicts these effects flawlessly, without (unlike classical physics) describing the behavior of quantum objects, and thus by *rationally* responding to quantum mystery. The equations of quantum mechanics respond to quantum mystery but, by the same token, do not correspond to the behavior of quantum objects.

In this view, quantum mystery is a mystery without mysticism. First of all, one does not presuppose any mystical agency governing these processes, say, in the manner of the so-called mystical or, as it is also known, negative theology. Negative theology, too, disallows an assignment of any possible attributes to such an agency except by way of negation ("it is *not* this," "it is *not* that," and so forth, including "it is *not* anything that could be designated as 'it' "), but, unlike nonclassical epistemology, it presupposes such a mystical agency. In the case of quantum phenomena, such a "mystical" agency need not be seen as divine or otherwise spiritual, although such views are found in the literature on quantum mechanics, especially, but not exclusively, when one applies quantum theory in the cosmological sense, as in considering the early Universe.[2] However, such an agency may also be seen as physical (possibly subject to some mathematical architecture) but conceived on this theological model. It is also possible to see this agency as ideal and specifically mathematically defined in the absence of theological determination of this agency, and such a view—a form of mathematical *Platonism* (which should be distinguished from Plato's own view)—is, too, found sometimes. Either view may be called, to borrow Heidegger's term, ontotheological, that is, defined by an ontology that is not in itself theological, but is based on the model of theology, positive or negative (mystical). "Ontotheological" thinking is a particularly strong form of "ontological" or "classical" thinking.

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[2] Cf. Susskind's remarks concerning the subject and his astute observation that modern (materialist) cosmology begins with Darwin and Wallace (Susskind 2006, pp. 5–8, 17–19). Darwin's "cosmology" is essentially linked to chance and may be seen as epistemologically nonclassical. If the processes responsible for the origin of the Universe and its early (pre-Big Bang) history are quantum as they are generally assumed to be, in the nonclassical view, such processes would be beyond our knowledge or conception. This may be the best available scientific alternative to the theological view of the origin of the Universe. Some of the available (classically described) effects or traces of these processes, many of which are manifest at cosmic scales and in classical objects such as stars and galaxies, may be of entirely new types and require new mathematics, possibly extending beyond that of current quantum field theories, defining the standard model of particles and forces, or even beyond string and brane theories.

Classical and specifically ontotheological views and the agendas they define are persistent across the history of twentieth-century physics (or of course earlier) and are still dominant. Such arguments may be based physically, mathematically, philosophically, or aesthetically, and they usually proceed along rationalist, rather than theological, lines, although theological agenda and arguments are, again, not absent either in this landscape. Among recently prominent examples of the works advocating such views are those by Brian Greene (Greene 2000, 2004) and by Roger Penrose, from his *Emperor's New Mind: Concerning Computers, Minds, and the Laws of Physics* (Penrose 1989) to *The Road to Reality: A Complete Guide to the Laws of the Universe* (Penrose 2005). The last title, beginning with the first definite article—*the* road to reality, no less!—is telling. Penrose's is, it is true, only "*a* guide," but it is *the* Road (with a capital R), nevertheless. It is of some interest and indicative of the diversity just mentioned, that while Greene sees his agenda as consistent with quantum mechanics, Penrose sees his as in conflict with it and hopes that quantum mechanics will be proven deficient even within its own scope and will be replaced by a more realist theory. Penrose is not alone in this view, although he takes it farther than most, and, as are many others, he is inspired by Einstein.

Indeed, as I said, it is the view advocated, following Bohr, in this study that is an exception. In the present view, moreover, unlike perhaps that of Bohr, any nonclassical interpretation of quantum phenomena or quantum mechanics is itself seen as idealization, which relates—via measurements and (in predictive terms) quantum mechanics—to experimental *facts*. "Fact" is a complex denomination, since these facts are, as discussed earlier, constitutively shaped by our theories and philosophical views, even if one leaves aside a complex interplay of social, cultural, and political determinations.[3] These facts,

[3] The nature and the character of scientific knowledge, beginning with the concept of "fact," has been subject to a massive reconsideration in recent decades, beginning with the work of Ludwik Fleck, Thomas Kuhn, Imre Lakatos, and Paul Feyerabend, and then their followers, especially in the constructivist social studies of science and related trends. The literature on these subjects is, again, massive in turn. To give a few pertinent and often cited references, for more standard post-Kuhnian philosophical approaches, see Cartwright (1983) and (Hacking 1983), and in the context of the relationships between classical and quantum physics (Cartwright 1999, pp. 177–233). For more "adventuresome" treatments, see Galison (1997), Latour (1999), (Hacking 2000), (Smith 2006), and closer to the present discussion, (Plotnitsky 1997). Both (Galison 1997), which deals with fundamental physics, and Latour (1999) depart from more strictly *social*-constructivist views found in the earlier in the constructivist science studies, especially that of the followers of the so-called "strong program" of David Bloor and his school. As this study argues, (the construction of) the unconstructible appears to be just as unavoidable in our, equally unavoidable, construction of scientific facts. It is worth reiterating that, with the qualifications just given (which also apply in classical physics), the complementary character of certain quantum phenomena, along with the uncertainty relations and the probabilistic nature of our predictions, is seen here as experimental fact. By contrast, any nonclassical interpretation of these phenomena is, in the present view, an interpretation, one among possible interpretations. Bohr's position in this regard, again, appears to be ambivalent, although he does not appear to foreclose this view.

moreover, pertain to a particular and limited scope of quantum mechanics (as a nonrelativistic theory) and reflect the experimental findings as these stand now. Accordingly, the overall understanding, experimental and theoretical, of the situation could change, and arguments of the type presented in this study could be abandoned or modified by developments they cannot anticipate. Indeed, this understanding is likely to change, given the present state of our knowledge and our physical theories, and the direction of this change—for example, whether it will take us toward deeper mysteries (without mysticism) or away from mysteries—is not certain. It is unclear how many such changes await us even in the near future, and there is no special reason to assume (although many do) that there is some single overall fundamental physical architecture to nature or reality, which we may or may not be approaching in our theories. Thus, we do not know whether the so-called standard model, which does not include gravity, will or can be further unified. Does it need to be? The hope that our theories may be able to approach the ultimate architecture of nature persists, even though it has been persistently questioned on multiple grounds, scientific, philosophical, and cultural, from the early twentieth century on. Our theories are many and diverse, and they appear to describe or predict diverse and often incompatible (as in general relativity and quantum mechanics) idealized physical phenomena or forms of reality.

Considered nonclassically, quantum mechanics retains most essential features that characterize classical physics as a *mathematical–experimental* science of nature, although, as we have seen, some, beginning with Einstein or Schrödinger, would take a different position as to which among such features, physical or epistemological, should be considered essential, and would include ontology or reality or causality among them. Nevertheless, the nonclassical view of the situation offered by Bohr via complementarity, or the one adopted here, maintains the character of quantum mechanics as, in Bohr's language, a *rational* science of nature in the absence of any physical description or even conception of quantum objects and processes. In the present view, moreover, this conception of quantum objects and processes must be seen as an idealization of whatever happens in nature at that level. That is, quantum mechanics in a nonclassical interpretation conforms to all the standard disciplinary requirements defining modern mathematical sciences of nature, from Galileo on. It provides as rigorous theoretical and experimental knowledge as classical physics does, and it rigorously relates (for example, by means of the uncertainty relations) what can and what cannot be known. It involves experimental verification, logical arguments, and mathematical formalisms. In short, it fulfills all the major requirements of modern physics and modern scientific inquiry in general, not the least insofar as it provides, as Bohr argues in his reply to EPR, the pathway to the discovery of new physical laws, the primary business of all physics.

Indeed, as indicated in Chapter 9, according to Bohr, beyond being merely consistent with these requirements, complementarity enables their fulfillment. As Bohr explains in his reply to EPR: "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" (Bohr 1935b, p. 700; emphasis added). In other words, without a concept-like complementarity (in the narrow sense), it would be difficult to bring the laws of quantum physics and quantum mechanics itself in accord with the basic principles of science. As Bohr writes in "Unity of Knowledge," published in 1954, by which time his nonclassical view had fully crystallized:

> A most conspicuous characteristic of atomic physics is the novel relationship between phenomena observed under experimental conditions demanding different elementary concepts for their description. Indeed, however contrasting such experiences might appear when attempting to picture a course of atomic processes on classical lines, they have to be considered as complementary in the sense that they represent equally essential knowledge about atomic systems and together exhaust this knowledge. The notion of complementarity does in no way involve a departure from our position as detached observers of nature, but must be regarded as the logical expression of our situation as regards objective description in this field of experience. The recognition that the interaction between the measuring tools and the physical systems under investigation constitutes an integral part of quantum phenomena has not only revealed an unsuspected limitation of the mechanical conception of nature, as characterized by attribution of separate properties to physical systems, but has forced us, in the ordering of experience, to pay proper attention to the conditions of observation. (Bohr 1954, *PWNB* 2, p. 74)[4]

Thus, by a skilful combination of understanding quantum phenomena and quantum mechanics (as a coherent experimental–mathematical theory of these phenomena) and properly interpreting both (such as via complementarity), one can ensure the status of quantum mechanics as an objective science of nature. This is possible notwithstanding the nonclassical epistemology of the situation and, at least to a large degree, the subjective nature of our quantum predictions by virtue of their probabilistic character even in the case of individual events.[5] Objectivity here also and even primarily means "unambiguous communication"

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[4] The appeal to "detached observers" brings to mind both Einstein and Pauli, as representing two contrasting views of the situation. For Einstein, the quantum mechanical observer is not sufficiently detached and, hence, not sufficiently objective, which Bohr counterargues here. In his view of the situation, the observers are as detached vis-à-vis measuring instruments as they are in classical physics, thus ensuring the objectivity of the scheme. On the other hand, the measuring instruments involved in experiment are *not* "detached" from quantum objects, since the latter cannot be extracted from the closed "envelopes" of phenomena, envelopes that cannot in principle be open. For Pauli, the quantum-mechanical observer is still too detached, at least for a successful approach to quantum field theory. Curiously, in "Discussion with Einstein," Bohr invokes Einstein's "*detached* attitude," which left "a deep impression" on him (Bohr 1949, *PWNB* 2, p. 36; emphasis added).

[5] I qualify my statement, in accordance with my discussion in the final section of Chapter 5, in view of the complexity of any "subjectivity," always shaped by inter-subjective factors and objective elements, such as measurement data.

or the unambiguous character of our statements, to begin with. As Bohr says on the same occasion:

> Returning to the much debated question of what has to be demanded of a physical explanation, one must keep in mind that classical mechanics had already implied the renunciation of a cause for a uniform motion and furthermore that relativity theory has taught us how arguments of invariance and equivalence must be treated as categories of rational explanation. Similarly, in the complementary description of quantum physics, we have to do with a further self-consistent generalization which permits the inclusion of regularities decisive for the account of fundamental properties of matter, but which transcends the scope of deterministic [or realist] description. The history of physical science thus demonstrates how the exploration of ever wider fields of experience, in revealing unsuspected limitations of accustomed ideas, indicates new ways of restoring logical order. (Bohr 1954, *PWNB* 2, p. 74)

It is difficult to disagree with Bohr's overall point, and Einstein, whose criticisms are clearly on Bohr's mind, might have agreed with most of this statement. One can, however, grant a peculiarity to the epistemological situation arising in quantum physics and understand Einstein's apprehension on these grounds. From Einstein's perspective, by suspending an even idealized physical description of individual quantum objects and their behavior, let alone by arguing that an analysis of this behavior is rigorously impossible, quantum mechanics almost becomes no longer theoretical physics, understood by Einstein as the project of developing this type of (mathematical) description. Quantum mechanics appears instead as a theory reflecting a fundamentally inexplicable, "spooky," form of information processing between measuring devices, to adopt the language of quantum information theory.

It is important, however, that, as I have argued throughout this study, Bohr's or the present view does not limit quantum theory by quantum informational concerns, even though any description or conception of quantum processes themselves is, in Bohr's defining phrase, "*in principle* excluded" (Bohr 1949, *PWNB* 2, p. 62). One might say that the peculiar *architecture* of (bits of) classical information encoded in quantum phenomena itself carries "information" concerning this exclusion. It is not a question of arbitrarily renouncing the analysis of quantum objects or their behavior, along positivist lines, or of being concerned only with a different (such as that of the EPR type) form of information processing. Quantum mechanics, in this interpretation, *is fundamentally concerned* with the behavior of quantum objects (say, electrons in atoms), just as classical physics is concerned with the behavior of classical objects (say, planets moving around the sun). Hence, in this view, while quantum mechanics may be used in quantum information theory and treated as a form of information theory, it is seen here as a physical theory in the same sense as classical physics is. However, a nonclassical interpretation of the theory and, again, of quantum phenomena themselves arrives at the conclusion that any knowledge or conception concerning such objects and their behavior is no longer possible. Accordingly, the theory is thereby limited to knowledge (classical in nature) and predictions concerning the effects of such behavior manifested in measuring instruments through the interaction, itself ultimately quantum in nature, between quantum objects and these instruments. On the other hand, it is concerned with *new configurations* of quantum *phenomena*, as defined by these effects, and new physical, including statistical, laws that describe and relate them. This knowledge may well be unlimited, in spite of and sometimes, as in the case of the uncertainty relations, because of the irreducible limits upon our knowledge and thought concerning quantum objects and their behavior.[6]

At stake in the Bohr–Einstein debate and, following it, most debates concerning quantum physics, is what our physical theories offer us. Do they, as Einstein hoped, offer us a way to achieve certain, even if approximate or idealized, descriptions of nature and thus a connection between mind and nature, on the model of classical physics? Or, do they, as Bohr came to accept, enact a radical break or discontinuity with nature by telling us, as quantum theory in a nonclassical interpretation does, that such models are "*in principle* excluded"? The mystery without mysticism or the epistemology without

[6] Given these considerations, it would be difficult to conclude, as some authors have done in the context of quantum information theory, that quantum mechanics is only about information (Clifton et al. 2003; Bub 2005). In the words of one of these authors, Jeffrey Bub, it is (only) "about quantum information" and manipulation of information, not about the mechanics of "waves" or "particles" (Bub 2005). One might argue instead that quantum mechanics is a form of information processing rather than physics only if one thinks of physics on the model of classical physics. As here explained, quantum mechanics is, fundamentally, about quantum objects, even though and because, in a nonclassical interpretation, while these objects are productive of a particularly configured information (which is, qua information, classical), no information concerning them as independent entities can possibly be obtained. Quantum information theory is not the same as quantum mechanics, however much it may contribute to our understanding of the foundations of quantum mechanics. For the significance of quantum information theory for quantum foundations see Peres (1993), Fuchs and Peres (2000), Fuchs (2001, 2002, 2003), (Pitowsky 2006) Caves et al. (2007), D'Ariano (2007), and, in the context of Bohr and Heisenberg, Plotnitsky (2002b, 2006b). For (technical) comprehensive introductions to the subject, see Jaeger (2007) and Mermin (2007). For a more visionary and generally more accessible introduction to the grounding ideas of quantum information theory see Wheeler (1990). Bub's views may not be surprising given that much of his previous work is devoted to Bohmian mechanics, to which he appears to remain more sympathetic, as a theory that is about physical processes in space and time and as such is a better platform for a future quantum theory as a physical theory. Bohm was his dissertation supervisor, and there exists a version of the theory known as the Bohm–Bub hidden-variables theory. Bub's position appears to me to be different from and conceptually less subtle than that of the Bayesian quantum information theorists just cited, on whose ideas his argument in question depends. Bub's article also appears to be insufficiently attentive to the complexities of the connections between quantum information theory and Bohr's view, which Bub invokes but which he appears to misunderstand, in part for the reasons just explained. Bub has commented on Bohr previously on several occasions, including in the context of the EPR exchange, but he appears, at least to the present author, to essentially misunderstand much of Bohr's thought. For example, like several other authors mentioned in Chapter 9, he appears to believe that the measurement on S_1 and the corresponding prediction concerning S_2 form a single phenomena in Bohr's sense, which, as I argue in Chapter 9, need not and, in the present reading does not, follow, as Bub thinks, from Bohr's definition of his concept of phenomenon (Bub 1999, pp. 192–198).

ontology of quantum physics includes us as creatures that may never be able to grasp the ultimate (quantum) constitution of nature—and yet are able to create theories that can predict, even if only probabilistically, the impact of this constitution on what we can grasp. How is this possible? This is perhaps the most complex question this study can invoke, including as an alternative to a theological understanding of the ultimate nature of things—but it can only invoke the question. For even meaningfully posing this question is well beyond its scope, and perhaps still beyond the scope of science, although perhaps no longer beyond its horizon.

10.2 Correlations Without Correlata

The expression "correlations without correlata" was introduced by Mermin in his argument, based on the EPR–Bell experiment (for discrete variables), for what he calls, by way of a tongue-in-cheek allusion to the Copenhagen interpretation, the "Ithaca Interpretation of Quantum Mechanics" (Mermin 1998a). The present concept of "correlations without correlata" is, however, different from that of Mermin. It derives from Bohr's argument for the constitutive, irreducible role of measuring instruments in quantum mechanics, while Mermin believes that measurement should only place a secondary, auxiliary role there. Bohr's view eventually led him to his concept of phenomenon as defined effects of these interactions upon certain classically describable parts of measuring instruments. Bohr's definition also includes the specification of each arrangement, determined by the type of measurement or prediction we want to make. We always have a free choice as concerns what kind of experiment we want to perform, in accordance with the very idea of experiment, which defines classical physics as well. Unlike in classical physics, however, implementing our decision concerning what we want to do will allow us to make only a certain type of prediction (for example, that concerning a future position measurement) and will unavoidably exclude the possibility of certain other, specifically the complementary, types of prediction (in this case, that concerning a future momentum measurement). At the same time and by the same token, Bohr's concept of phenomenon precludes any description or even conception of quantum objects themselves and their behavior, which behavior is, nevertheless, responsible for the emergence of these effects.

As Bohr says in his reply to EPR, "in fact to measure the position of … [a particle] can mean nothing else than to establish a *correlation* between its behavior and instrument rigidly fixed to the support which defines the space frame of reference" (Bohr 1935b, pp. 699–700; emphasis added). This statement could apply in both classical and quantum physics. In classical physics, however, such a correlation allows one to ascertain the actual position of the measured object as well as that of the measuring instrument involved. Thus, one can also ascertain the two *classical* correlata of this correlation, at least ideally. In fact, in classical mechanics it is always possible (again, at least in principle) to ascertain simultaneously the values of all four variables or four correlata—both the position and the momentum of the object considered, and both the position and the momentum of the corresponding part of the measuring apparatus involved. In the case of a prediction on the basis of a measurement, there are eight such correlated measurable quantities. That the determinate values of both such conjugate variables can, first, be assumed and, second, be determined by measurement makes classical mechanics, first, ontologically realist; second, ontologically causal; and third, epistemologically deterministic. That is, it allows one to determine all of its objects and processes as knowable, directly or by way of approximation or idealized models, or at least as conceivable or thinkable, on the model of Kantian things-in-themselves.

By contrast, according to a nonclassical view of quantum phenomena as phenomena in Bohr's sense, physical quantities obtained in quantum measurements such as those defining the physical behavior of certain (classically described) parts of measuring instruments can no longer be assumed to represent the corresponding properties of quantum objects ("elements of [quantum-level] reality"). This statement applies, in accordance with Bohr's post-EPR view and his concept of phenomena, even in the case of any single such property, rather than only to the simultaneous joint attributions of certain properties, in accordance with the uncertainty relations. As we have seen, Bohr's earlier view allows for this type of attribution at the time of measurement. Even this weaker view, however, is self-evidently not sufficient to define the physical state of an object on the model of classical physics, which requires a well-defined, unambiguous, determination of both conjugate variables for its objects considered at any point independently of measurement. At the moment, I do speak of the *physical* state of a given object (which is bound by the uncertainty relations) in contradistinction to the quantum theoretical concept of state defined via the mathematical formalism of quantum theory, as discussed in Chapters 4 and 6. In Bohr's nonclassical version of complementarity, an attribution *even of a single property* to any quantum object is *never possible—before, during, or after measurement.*

It is not difficult to surmise Bohr's reasons for moving to nonclassical epistemology, given the preceding discussion in this study. The conditions that experimentally obtain in the case of quantum phenomena (in the usual sense) only allow us to rigorously specify measurable quantities that are, in principle, available only for measuring instruments and never for quantum objects themselves. Even in the case when we do not consider the momentum of a given quantum object and thus need not worry about the uncertainty relations, neither the exact *position* of this object itself nor the actual time at which this "position" is established is ever available and hence in anyway verifiable (e.g., Rovelli 1998). These properties, assuming they could be defined, are lost in the quantum abyss of the measurement process—in, to return to the wording of Bohr's reply, "the finite and uncontrollable interaction" between quantum objects and measuring instruments—and hence it is rigorously impossible to speak of such attributes or verify this attribution. However, this process leaves, as its effect, a mark in measuring instruments, which can be treated as a part of a sufficiently permanent and objective record. These recordings we *can* think and speak about, use in our work, communicate, and so forth.

It took the EPR experiment for Bohr to reach his nonclassical view, which appears to be expressed for the first time in his 1937 article "Complementarity and Causality," where he says, "[T]he whole situation in atomic physics *deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to the object*... The renunciation of the ideal of causality in atomic physics which has been forced on us is founded logically only *on our not being any longer in a position to speak of the autonomous behavior of a physical object*, due to the unavoidable interaction between the object and the measuring instruments which in principle cannot be taken into account, if these instruments according to their purpose shall allow the unambiguous use of the concepts necessary for the description of experience. In the last resort an artificial word like 'complementarity' which does not belong to our daily concepts serves only briefly to remind us of the epistemological situation here encountered, which at least in physics is of an entirely novel character" (Bohr 1937, *PWNB* 4, pp. 86–87; emphasis added). The statement, again, makes clear that complementarity is a specific physical–philosophical concept, "which does not belong to our daily life" and which is nonclassical, although its architecture contains epistemologically classical components as well. There is no qualification here or in his subsequent writings to the effect that such "inherent attributes" may still be meaningful if their assignment is constrained by the uncertainty relations (e.g., *PWNB* 2, pp. 40, 51, 61). The uncertainty relations are still applicable, of course, but they now apply only to the corresponding (classical) variables of suitably prepared measuring instruments, impacted by their interactions with quantum objects. We can either prepare our instruments so as to measure a change of momentum of certain parts of those instruments or arrange them so as to locate a spot impacted by a quantum object, but never do both together. The physical character of the uncertainty relations as applicable only to certain parts of measuring instruments is brought out by Bohr in the following elaboration:

> [A]n adequate tool for a complementary way of description is offered precisely by the quantum mechanical formalism which represents a purely symbolic scheme permitting only predictions, on lines of the correspondence principle, as to results obtainable under conditions specified by means of classical concepts. It must here be remembered that even in the indeterminacy relations [$\Delta q \Delta p \cong h$] we are dealing with an implication of the formalism which defies unambiguous expression in words suited to describe classical physical pictures. Thus, a sentence like "we cannot know both the momentum and the position of an atomic object" raises at once questions as to the physical reality of two such attributes of the object, which can be answered only by referring to the conditions for the unambiguous use of space-time concepts, on the one hand, and dynamical conservation laws, on the other hand. While the combination of these concepts into a single picture of a causal chain of events is the essence of classical

mechanics, room for regularities beyond the grasp of such a description is just afforded by the circumstance that the study of the complementary phenomena demands mutually exclusive experimental arrangements. (Bohr 1949, *PWNB* 2, pp. 40–41)

The comment ostensibly refers to the Como lecture but is clearly inflected by Bohr's post-EPR thinking and language, in particular his argument concerning and expressing "an essential element of ambiguity ... [always] involved in ascribing conventional physical attributes to atomic objects" (again, even single such attributes) (Bohr 1949, *PWNB* 2, p. 40). The argument itself, as stated here, is much closer to Bohr's argument in his reply to EPR, again, given a nonclassical inflection. Bohr's 1954 article "Unity of Knowledge" expressly describes the situation and the uncertainty relations, via his concept of phenomenon. Bohr writes,

In particular, the impossibility of a separate control of the interaction between the atomic objects and the instruments indispensable for the definition of the experimental conditions prevents the unrestricted combination of space-time coordination and dynamical conservation laws on which the deterministic description in classical physics rests. In fact, any unambiguous use of concepts of space and time refers to an experimental arrangement involving a transfer of momentum and energy, uncontrollable in principle, to fixed scales and synchronized clocks which are required for the definition of the reference frame. Conversely, the account of phenomena which are characterized by the laws of conservation of momentum and energy involves in principle a renunciation of detailed space-time coordination. These circumstances find quantitative expression in Heisenberg's indeterminacy relations which specify the reciprocal latitude for the fixation of kinematical and dynamical variables in the definition of the state of a physical system. In accordance with the character of the quantum mechanical formalism, such relations cannot, however, be interpreted in terms of attributes of objects referring to classical pictures, but we are here dealing with the mutually exclusive conditions for the unambiguous use of the very concepts of space and time on the one hand, and of dynamical conservation laws on the other. (Bohr 1954, *PWNB* 2, pp. 72–73)

In addition, as explained in Chapter 8, both variables involved in the uncertainty relations cannot be seen as pertaining to the same quantum object, which, by the same token, implies the statistical nature of the uncertainty relations themselves. As the mathematical definition of the variables involved (whether we speak of matrix elements, q-numbers, or operators in Hilbert spaces) is precise mathematically, Bohr's "definition" of the physical variables and quantities involved (in terms of the classical physical variables pertaining to certain parts of measuring instruments) is precise physically. While the type of idealization used in classical physics is no longer possible in this interpretation, it is replaced by a different type of idealization, whereby quantum objects themselves cannot be ascribed any physical properties—again, even single such properties and even in measurement, rather than only independently—or for that matter any other attributes, such as "quantum" or "object." Indeed, the concept of "object" is itself derived from classical physics and its philosophical and epistemological genealogy, from Plato and Aristotle and even the pre-Socratics on, and it has its theological history as well, especially as regards a view of nature as an object, for example, an object created by God.

Thus, the concepts of "mysteries without mysticism" (or "epistemology without ontology") and "correlations without correlata" become correlative, and all three are correlative to the uncertainty relations, now, again, in turn dealing rigorously only with correlations between quantum events, as discussed in Chapter 2. The impossibility of, in Bohr's language, *unambiguously* specifying the quantum-level correlata of any possible quantum mechanical correlations is correlative to quantum mystery (without mysticism) in the sense that beyond certain limits an analysis of quantum phenomena "is *in principle* excluded." As the "properties" of quantum objects and of their behavior (if one can in turn speak here in terms of "properties" or "behavior," any more than of "quantum" or "objects"), the ultimate correlata of quantum correlations are irreducibly beyond the limits of physical description or our knowledge and thought, in general. Hence, one can indeed speak, as Mermin does, of "correlations and only correlations," without ever being able to speak about the *quantum-level correlata* of those correlations. These correlations give us, in Bell's words, what is "speakable" in quantum mechanics. Their *quantum* correlata are, in the present view, unspeakable, unthinkable, and inconceivable. The observable and hence "speakable" correlata of quantum correlations—that is, correlata manifested in quantum phenomena, such as those at stake in Bell's theorem—become in turn correlative to this epistemology, and as such are correlations without ascertainable *quantum-level* correlata. The ultimate constitution of nature itself may, I argue here, be at an even further remove from our grasp than this idealization, if inexorably affecting our interaction with nature. It may be unspeakable even as unspeakable, unthinkable even as unthinkable, inconceivable even as inconceivable, and so forth. This is not a situation that Bell himself saw as desirable any more than did Einstein, although both appear to have accepted it as logically possible.[7]

10.3 Epistemology Without Ontology

Bohr's nonclassical version of complementarity and his concepts of phenomena and atomicity had crystallized by the time of "Complementarity and Causality" (Bohr 1937) and the Warsaw lecture, "The Causality Problem in Atomic Physics" (Bohr 1938). The architecture of the concept of phenomena has already been traced in this study and was in part sketched in the preceding sections of this chapter. This section brings together and develops these various threads, with a particular emphasis on Bohr's crucial (and often misunderstood) concept of the classical description of measuring instruments.

The grounding premise of Bohr's concept of phenomena is that all quantum phenomena (in the usual sense) are constituted by certain, sometimes correlated, *recorded* effects of the interactions between quantum objects and

[7] For Bell's comment on this possibility, see Bell (1987, p. 155).

measuring instruments, "practically irreversible amplification effects," such as a click of a photodetector or a blackening of a grain of a photographic emulsion (Bohr 1949, *PWNB* 2, p. 51). The very language of effects becomes persistent in Bohr's post-Como and especially post-EPR writings. These effects are physically described in terms of classical physics, but they cannot be predicted by its means and require an alternative theory, such as quantum mechanics. Quantum objects and their behavior themselves are, again, placed beyond any knowledge and conception. This concept of phenomena is Bohr's ultimate interpretation of his argument for "the *impossibility of any sharp separation between the behavior of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear*" (Bohr 1949, *PWNB* 2, pp. 39–40). The term "phenomenon" itself now refers only to *registered* observations or already *measured* quantities. In other words, by definition, it refers only to what has already happened and not to what may happen, even if the latter possibility corresponds to a rigorous prediction. Phenomena in Bohr's sense *enable predictions* but *do not involve predictions* in their definition and constitution as phenomena. Such predictions are, again, in general probabilistic and, hence, never guarantee a given outcome, which is itself a good reason for referring only to already registered events in defining phenomena. In Bohr's words,

> I advocated the application of the word *phenomenon* exclusively to refer to the observations *obtained* under specified circumstances, including an account of the whole experimental arrangement. In such terminology, the observational problem is free of any special intricacy since, in actual experiments, all observations are expressed by unambiguous statements referring, for instance, to the registration of the point at which an electron arrives at a photographic plate. Moreover, speaking in such a way is just suited to emphasize that the appropriate physical interpretation of the symbolic quantum-mechanical formalism amounts only to predictions, of determinate or statistical character, pertaining to individual phenomena appearing under conditions defined by classical physical concepts. (Bohr 1949, *PWNB* 2, p. 64; emphasis added)

One might prefer to speak of *an* (rather than *the*) appropriate interpretation. The key features of Bohr's concept are clearly apparent here—in particular, the point that the term refers "to the observations *obtained* under specified circumstances" (emphasis added), and hence, only to already registered phenomena, as just explained. The concept allows Bohr to resolve some of the difficulties and ambiguities of his own previous arguments and to bring greater clarity to his argument for complementarity. As he also notes at the same juncture:

> I entered more directly on the questions of terminology. In this connection I warned especially against phrases, often found in the physical literature, such as "disturbing of phenomena by observation" or "creating physical attributes to atomic objects by measurements." Such phrases, which may serve to remind [us?] of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like "phenomena" and "observations," just as "attributes" and "measurements," are used in a way hardly compatible with common language and practical definition. (Bohr 1949, *PWNB* 2, pp. 63–64; also Bohr 1938, *PWNB* 4, p. 104; Bohr 1954, *PWNB* 2, p. 73)

Bohr is commenting on Einstein's views "as regards the incompleteness of the quantum mechanical mode of description" (Bohr 1949, *PWNB* 2, p. 63). It is difficult to fault Bohr on any aspects of this statement—*as this statement relates to his view at the time, his post-EPR view of the situation*. Given the preceding discussion in this study, however, it is also clear that Bohr's own earlier arguments sometimes suffer from insufficient clarifications in this respect, which his concept of phenomenon helps to remedy.

Bohr's insistence on the indispensability of classical physical concepts in considering the measuring instruments is a subtle and often misunderstood issue. This misunderstanding is, again, often due to insufficient attention to the architecture of Bohr's concepts and insufficiently careful reading of Bohr's elaborations on the subject. In particular, as I have stressed throughout, the classical description in question concerns only certain strata of measuring instruments, while other strata of their constitution, specifically those that are responsible for their interactions with quantum objects, are seen as quantum. Thus, on the one hand, "although, of course, the existence of the quantum of action is ultimately responsible for the properties of the materials of which the measuring instruments are built and on which the functioning of the recording devices depends, this circumstance is not relevant for the problems of adequacy and completeness of the quantum mechanical description in its aspects here discussed" (Bohr 1949, *PWNB* 2, p. 51; also Bohr 1937, *PWNB* 4, p. 88).[8] On the other hand, the dependence itself of the constitution and functioning of the recording devices on the quantum of action is crucial, since it enables the

[8] As Bohr notes on the second occasion here cited, the situation changes once we move to quantum field theory, where the quantum constitution of measuring instruments might need to be taken into account (Bohr 1937, *PWNB* 4, p. 88). The observation must have been prompted in part by Bohr's work with L. Rosenfeld on measurement in quantum field theory (Bohr and Rosenfeld 1933) in response to L. Landau and R. Peierls's argument. The latter argument, contested by Bohr and Rosenfeld, concerned a possible inapplicability of the uncertainty relations in quantum field theory, a relevant subject in the present context. This exchange occurs only two years prior to Bohr's exchange with EPR. Rosenfeld assisted Bohr in writing his reply to EPR, although one must be cautious in relating their respective interpretations of quantum mechanics, which are quite different. My point here is that Bohr's work on complementarity also reflects and is shaped by Bohr's thinking concerning quantum field theory, from his introduction by Dirac, while, as noted earlier, working in Bohr's institute in Copenhagen, and his discussion of the subject with Pauli and Heisenberg, both of whom made decisive contributions to the field. Bohr's comment just cited also appears to be influenced by Pauli's argument that the quantum-mechanical observer is still too "detached" from quantum objects (see Note 4 above). Bohr's view is also consistent with that of some of the founders of modern (i.e., renormalized) quantum electrodynamics, in particular Julian Schwinger (see Schweber 1994, p. 366) and Freeman Dyson (Dyson 1949, p. 1755). Finally, these considerations have deep connections, often on Bohr's mind, to the question of the problem of the physical constitution of the measuring instruments, rods and clocks, in special and then general relativity, which troubled Einstein all his life. Significant though it is in its present context and for a deeper understanding of the ultimate constitution of nature (e.g., quantum gravity), I can only mention the subject here. For an illuminating discussion, see Brown and Pooley (2001).

quantum interaction between these devices and quantum objects, without which there would be no quantum data. Earlier, in his reply to EPR Bohr speaks of "the purely classical *account*" rather than "description" of measuring apparatuses involved, which is a better choice of term (Bohr 1935b, p. 697; emphasis added). The account we use is indeed that of classical physics, with the qualifications just made. The description is both classical, at one end, and quantum, at the other end—where, however, no actual physical description may be possible, any more than a physical description of the quantum objects concerning which (or, again, concerning whose future interactions with measuring instruments) our predictions are made. This point is important, even though these quantum strata do not enter either predictive or descriptive aspects of Bohr's interpretation. According to Bohr,

> [W]e must recognize above all that, even when the phenomena transcend the scope of classical physical theories, the account of the experimental arrangement and the recording of observations must be given in plain language, suitably supplemented by technical physical terminology. This is a clear logical demand, since the very word "experiment" refers to a situation where we can tell others what we have done and what we have learned. However, the fundamental difference with respect to the analysis of phenomena in classical and in quantum physics is that in the former the interaction between the objects and the measuring instruments may be neglected or compensated for, while in the latter this interaction forms an integral part of the phenomena. The essential wholeness of a proper quantum phenomenon finds indeed logical expression in the circumstance that any attempt at its well-defined subdivision would require a change in the experimental arrangement incompatible with the appearance of the phenomenon itself. (Bohr 1954, *PWNB* 2, p. 72)

The demand or logic in question may require further analysis, as suggested by Bohr's qualification "suitably supplemented by technical physical terminology" here and by his other commentaries, considered earlier, on classical physics as a refinement of everyday concepts and language. The main point at the moment is Bohr's argument concerning the nature of quantum phenomena in his sense—that is, as indivisible or closed phenomena—or their atomicity in his sense, developed by the time of this statement just cited. Bohr then introduces his concept of "closed phenomenon," via a summary of his analysis of the uncontrollable interactions between quantum objects and measuring instruments resulting in complementarity and, correlatively, the uncertainty relations (Bohr 1954, *PWNB* 2, pp. 72–73). This concept does not contain new epistemological features, but it usefully clarifies the situation, and the elaboration is worth citing:

> [O]ne sometimes speaks of "disturbance of phenomena by observation" or "creation of physical attributes to atomic objects by measurements." Such phrases, however, are apt to cause confusion, since words like phenomena and observation, just as attributes and measurements, are here used in a way incompatible with common language and practical definition. On the lines of objective description, it is indeed more appropriate to use the word phenomenon to refer only to observations obtained under circumstances whose description includes an account of the whole experimental arrangement. In such terminology, the observational problem in quantum physics is deprived of any

> special intricacy and we are, moreover, directly reminded that every atomic phenomenon is closed in the sense that its observation is based on registrations obtained by means of suitable amplification devices with irreversible functioning such as, for example, permanent marks on a photographic plate, caused by the penetration of electrons into the emulsion. In this connection, it is important to realize that the quantum-mechanical formalism permits well-defined applications referring only to such closed phenomena. Also in this respect it represents a rational generalization of classical physics in which every stage of the course of events is described by measurable quantities.
>
> The freedom of experimentation, presupposed in classical physics, is of course retained and corresponds to the free choice of experimental arrangements for which the mathematical structure of the quantum mechanical formalism offers the appropriate latitude. (Bohr 1954, *PWNB* 2, p. 73)

As noted in the Introduction, quantum physics gives us a truer sense of this freedom insofar as our experiments define what can happen, as opposed to classical physics, where our experiments merely track what happens in any event. The indispensability of classical physical concepts involves only the description of certain parts of measuring instruments. In considering the quantum part of measuring instruments, through which they interact with quantum objects, classical physical concepts are not only dispensable but are rigorously inapplicable, just as are any other concepts, if, again, we can have representational concepts other than classical. In the present view, we cannot, since this view defined all representation as classical. The quantum interactions between quantum objects and measuring instruments are, however, capable of producing classically describable effects upon these instruments. More accurately, these interactions are capable of initiating the process that ultimately leads to these changes, whereby these interactions are "amplified" to the classical level, compelling Bohr to speak of "practically irreversible amplification effects."[9] As he says,

[9] This concept may be linked to the decoherence approach to quantum theory, which is, however, a separate subject that cannot be addressed here. I have discussed these connections previously (Plotnitsky 2006b, pp. 40–44). See Zurek (2003) for an informative discussion of decoherence in the set of contexts relevant to my argument here, and for a recent comprehensive treatment see Schlosshauer (2007). Zurek's epistemological position in this article and throughout his work on decoherence is different from the one adopted here. In particular, he appears to assume, along the lines discussed in Chapter 6, that the independent evolution of a quantum system is in fact causally (or in his language "deterministically") mapped by Schrödinger's equation, without offering sufficient, if any, grounds for this assumption, which, accordingly, retains the problems discussed in Chapter 6. Schlosshauer's study takes a similar view. It also appears to underappreciate the complexity of Bohr's argumentation, including on the wave–particle dilemma (e.g., Schlosshauer, pp. 61–63), although Schlosshauer deserves credit for realizing the complexity of using either concept, "particle" or "wave," in quantum physics. He also tends to see Bohr's view in overly positivistic terms. In any event, from the present perspective, a quantum interaction between quantum objects and measuring instruments leaves only an "irreversibly amplified" classical trace once the experiment is performed. The numerical data associated with this trace can be probabilistically predicted using the formalism of quantum mechanics. There are, however, no *physical state described by this formalism*, which "decoheres" into this trace.

> It is just arguments of this kind which recall the impossibility of subdividing quantum phenomena and reveal the ambiguity in ascribing customary physical attributes to atomic objects. In particular, it must be realized that—besides in the account of the placing and timing of the instruments forming the experimental arrangement—all unambiguous use of space-time concepts in the description of atomic phenomena is confined to the recording of observations which refer to marks on a photographic plate or to similar practically irreversible amplification effects like the building of a water drop around an ion in a cloud-chamber. (Bohr 1949, *PWNB* 2, p. 51)

This elaboration, again, makes apparent that, even though this passage refers to earlier discussions with Einstein in 1927–1929, by this time (1949) Bohr sees the quantum mechanical situation itself in nonclassical terms defined by his closely linked concepts of phenomena and atomicity. Every registered quantum event is defined, as in classical physics, by a correlation between the quantum object under investigation and the corresponding measuring instrument. Unlike in classical physics, however, this "correlation" is without ascertainable quantum-level correlata, pertaining either to the object or to the quantum part of the instrument. Indeed, this correlation without correlata is not even that between these two quantum entities.

The (classically) measured quantity only establishes a correlation between the object and a certain classical stratum of the measuring instrument involved, since the measuring process unavoidably and irreversibly amplifies the interaction in question into a classical-level effect. In other words, this correlation has one classical correlatum and no quantum-level correlata.

By the same token, "ascribing customary physical attributes to atomic objects," even single attributes (rather than only conjugate ones in view of the uncertainty relations), is ambiguous. Once we deal with both a measurement and a prediction (based on this measurement), and once the second measurement, corresponding to this prediction, is performed, the outcomes may correlate as well and may be given classical correlata defined by the corresponding outcomes of these two measurements. This situation defines the wholeness or indivisibility of phenomena in Bohr's sense. This feature leads Bohr to his concept of "atomicity," which applies at the level of phenomena, rather than referring to indivisible physical entities, atoms, found in nature itself, conceived on the lines of the old Democritean doctrine of atomicity. These two concepts, phenomenon and atomicity, are effectively equivalent.

It is crucial that that Bohr qualifies and indeed makes part of his definition of the concept of phenomenon that such effects must be considered *under rigorously specifiable experimental conditions* and that *this specification must itself be seen as part of the phenomenon*. The constitutive role of these conditions can never be eliminated in considering quantum measurement or predictions (including, as in the EPR experiment, when these predictions concern objects that are *physically* unaffected by the measurements enabling them), in the way that role can, in principle, be eliminated in classical physics. Thus, if seen independently of the quantum mechanical context of its appearance, each mark on the screen in the double-slit experiment would be perceived in the same way or as the same phenomenon in the sense of the philosophical phenomenology, from Kant to Husserl. Such a mark would appear the same regardless of the difference in the physical conditions and, hence, outcome ("interference" or "no interference") of the double-slit experiment. According to Bohr's understanding, however, each mark is, or is part of, a *different individual* phenomenon depending on these conditions, which are mutually exclusive in the case of complementary phenomena and are defined by each phenomenon uniquely in any circumstances.

Thus, in the double-slit experiment, rather than dealing only with two phenomena, each defined by a different multiplicity of spots on the screen, we deal with two distinct multiplicities of individual phenomena, each defined by a spot on the screen. Each is an individual phenomenon in Bohr's sense and depends on a different set of conditions of the experiment. One of these sets will lead to the emergence of the interference pattern, "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" (Bohr 1949, *PWNB* 2, pp. 45–46; emphasis added). The other will not. Each spot must be seen as *a different individual phenomenon* defined by the conditions in which the event occurs, while two different patterns, "interference" and "no interference," pertain to *two sets* of different individual phenomena. Individually, we will be able to make exact predictions in the first case concerning the momentum measurement and, in the second, concerning the position measurement associated with each event and, physically, pertaining to the measuring apparatus. Neither is sufficient by itself to determine the course of the future events and where the object hits the screen exactly, but only to determine the probability that it will do so within a certain region, calculated differently in each setting. In either setting, however, there is always a nonzero probability for a given object to hit the screen outside the predicted range. While, thus, a given single event does not allow us to establish in which setting it had occurred, one can still see quantum mechanics as a probabilistic theory of individual quantum events, along Bayesian lines, rather than as only that of ensembles of events, along frequentist lines. Any quantum phenomenon is always unique and unrepeatable—singular—and as such is incompatible with any other actual situation of measurement. These individual situations themselves are not all essential in the same way as are properly complementary situations, correlative to the uncertainty relations. This necessity gives such measurements a special role in quantum mechanics and complementarity. The irreducible individuality of each phenomenon is essential, however.

Far from being a matter of convenience, the distinction between two multiple-spot phenomena and two multiplicities of different individual spot-like phenomena in the double-slit experiment is essential, including, as we have seen, in countering Einstein's arguments of the EPR type. The statistical qualifications given above (e.g., that a single trace on the screen may in fact be meaningless vis-à-vis any arrangement) do not diminish this point, but instead reinforce it. Given this type of view, first, no paradoxical properties—such as simultaneous possession of contradictory wave-like and particle-like attributes on the part of quantum objects themselves—are involved, which allows one to dispense with the wave–particle complementarity. Second, we should never mix considerations that belong to complementary experimental setups in analyzing a given experimental outcome even when dealing with a single spot on the screen, as we could, in principle, do in classical physics. This is not an uncommon problem, including, as we have seen, in some of Einstein's arguments.

Bohr's concept of phenomenon enables him to transfer to the level of observable configurations manifested in measuring instruments all the key features of quantum physics—discreteness, discontinuity, individuality, and atomicity (indivisibility)—previously associated with quantum objects themselves. As I said, this transfer also led Bohr to his concept of "atomicity," in its original Greek sense of an entity that is not divisible any further. Both concepts emerged at about the same time and are more or less equivalent. "This novel feature of atomicity in the laws of nature," was, according to Bohr, "disclosed" by "Planck's discovery of the quantum of action." Bohr sees this discovery as "supplementing in such unexpected manner the old [Democritean] doctrine of the limited divisibility of matter," although Planck's atomicity, too, was originally understood on these classical lines (Bohr 1938, *PWNB* 4, p. 94). I cite Bohr's initial formulation in the Warsaw lecture, but virtually identical formulations are found throughout his subsequent writings (e.g., Bohr 1949, *PWNB* 2, p. 33 and Bohr 1958, *PWNB* 3, p. 2). One might speak of "Bohr's atom," radicalizing, with Bohr, his initial ideas of his 1913 theory of the hydrogen atom, to which the term "Bohr's atom" is sometimes applied. Like Bohr's concept of phenomenon, so too is this concept of "atomicity" defined in terms of certain *individual* effects of quantum objects upon the classical world, as opposed to Democritean atoms of matter itself. Accordingly, "atomicity" now refers to physically complex and hence *physically* subdivisible entities. It no longer refers to single physical entities, whether quantum objects themselves or even point-like traces of physical events. Bohr's "atoms" are thus conceived of as individual phenomena in his sense, rather than as indivisible atomic quantum objects, to which one cannot ascribe atomic physical properties any more than any other kind of properties.[10]

[10] Bohr's conception of atomicity and its significance have rarely been properly examined. Among very few exceptions are Folse (1985, 1987) and Stapp (2007). Folse's interpretation of this concept or of Bohr's concept of phenomenon, and of Bohr's epistemology in general, is different from the one proposed here. In particular, he attributes to Bohr a new argument that quantum mechanics is nonlocal, which is, as I argue here, difficult to sustain. Folse's more recent commentaries on the subject no longer invoke nonlocality (Folse 2002, p. 93). The philosophical genealogy of this concept is not easy to trace, although one can find a few parallels, in particular, with A. N. Whitehead's conception of atomicity ("drops of experience") in *Process and Reality* (Whitehead 1929). It is unclear whether Whitehead's ideas were familiar to Bohr or had impact on his thought, although this is not inconceivable, given that at the time Whitehead's work had considerable impact (which subsided in the second half of the twentieth century). Stapp sees a conceptual parallel between Whitehead's and Bohr's conceptions of atomicity.

Any attempt to "open" or "cut through" a phenomenon can only produce yet another closed individual phenomenon, a different "Bohr's atom" or set of such "atoms," leaving quantum objects themselves irreducibly inaccessible inside phenomena. As Bohr says, "In fact, the individuality of the typical quantum effects finds its proper expression in the circumstance that any attempt of subdividing the phenomena will demand a change in the experimental arrangement introducing new possibilities of interaction between objects and measuring instruments which in principle cannot be controlled" (Bohr 1949, *PWNB* 2, p. 40). It is at this point that Bohr offers his most rigorous single definition of complementarity: "Consequently, evidence obtained under different experimental conditions cannot be comprehended within a single picture, but must be regarded as *complementary* in the sense that only the totality of the phenomena exhaust the possible information about the objects" (Bohr 1949, *PWNB* 2, p. 40). As a result, again, only probabilistic estimates concerning the outcome of our experiments can be given, since it is impossible, even in principle, to reach the level of objects themselves, where one could encounter anything causal. It does not follow that one necessarily would find anything causal there! That is why this and related arguments by Bohr "recall the impossibility of subdividing quantum phenomena and reveal the ambiguity in ascribing customary physical attributes to atomic objects" (Bohr 1949, *PWNB* 2, p. 51). For the same reasons, Bohr, as noted above, also speaks of phenomena as "closed" (Bohr 1954, *PWNB* 2, p. 73).

Each phenomenon becomes *individual*, each—every (knowable) effect conjoined with every (unknowable) process of its emergence—unique and unrepeatable. Some of them can be clustered insofar as they refer to the "same" quantum entities, whether "individual" (for example, elementary particles) or collective (for example, more or less stabilized composites of quarks and gluons, such as protons or neutrons). Reciprocally, however, this is the only way to define and identify such entities, individually or collectively. Thus, along with the quantum atomicity as *indivisibility*, the quantum atomicity as *individuality* is now also understood as the individuality, and ultimately the uniqueness, of each phenomenon. By the same token, each phenomenon is discrete, that is, discontinuous in relation to every other such phenomenon, as are, for example, those associated with each trace ("dot") on a silver screen left by a collision between a quantum object and the screen. Finally, another general characteristic of "atomicity," quantum *discontinuity*, is now also different from discontinuity at the level of quantum objects. It is redefined, nonclassically, as the irreducible inaccessibility of quantum objects themselves, the impossibility of applying either of these concepts (continuity or discontinuity) or any conceivable concept to their "relation" (another inapplicable concept) to the manifested effects of their (quantum) interaction with measuring instruments, which is responsible for these effects.

It is worth reiterating that under these conditions, one cannot assign a physical significance to the atomistic idea of *particles* at the level of quantum objects, any more than to the idea of *waves*. This circumstance is far less often commented upon or even recognized than the latter, in part because the suspension of the idea of particle as applicable to quantum objects is far less common even among the proponents of the Copenhagen view. This suspension, however, is epistemologically crucial, and it ultimately extends to a suspension of the attribution of any conceivable features to quantum objects. Particles qua particles disappear in this interpretation—only *Bohr's atoms* remain, although the term "particle" could be provisionally used and sometimes was by Bohr for the sake of convenience and economy of discourse, or for strategic reasons (for example, in dealing with arguments, such as that by EPR, that appeal to the concept).[11]

Bohr's atomicity is, thus, a new philosophical and, *qualitatively*, physical concept, which in and of itself does not involve any mathematics. It can, however, be related, *quantitatively*, to the experimental data and mathematical formalism of quantum mechanics, insofar as the latter can predict these data, configured in terms of this concept. By the same token, the scheme is linked to probability in a way radically different from that of the old quantum theory, where the use of probability was linked to the classical, Democritean view of elementary particle as the indivisible units of matter itself.

10.4 Probability Without Causality

As explained at the outset of this study, the irreducible role of randomness, on the one hand, and probability and correlations, on the other, in defining quantum phenomena and our predictions concerning them is reflected in a particular way in a nonclassical interpretation of quantum phenomena and quantum mechanics. The genealogy of the nonclassical thinking concerning probability in quantum theory begins with Heisenberg's introduction of matrix mechanics as a theory dealing, via probability amplitudes, with the probabilities of transitions between stationary states of electrons in atoms, and then Born's probabilistic interpretation of the wave function. This thinking is ultimately integrated into a rich physical, mathematical, and philosophical concept, in which nonclassical epistemology, the wave function, and probability (along Bayesian lines) are joined together within a single conceptual architecture. This architecture is defined by suspending causality even at the level of the individual processes and events involved, as against the view adopted in classical statistical physics, where the behavior of the individual entities comprising

[11] Cf., however, the arguments offered in (Ulfbeck and Bohr 2001; Bohr et al. 2004), which target the use of the idea of particles by quantum physicists, including, in my view mistakenly, Niels Bohr. In particular, they disregard Bohr's argument that the concept of particle, just as that of wave, is an abstraction and (this is crucial) an abstraction of a different kind than in classical physics. Secondly, they do not appear to be sufficiently attentive to the difference between Bohr's language in his own arguments and in his analyses of arguments by others, who use the term particle.

the multiplicities considered statistically is assumed to be causal. Given, however, that we deal with quantum phenomena as phenomena in Bohr's sense and their nonclassical constitution, our predictions no longer concern individual quantum objects and processes, which are placed beyond any description or conception; they concern only the effects of such processes manifested in the measuring instruments involved.

By contrast, the *mathematical* probability theory deployed for estimating the probabilities involved is not *essentially* different from that used in classical physics or elsewhere. There are practical differences, encountered already by Planck, which concern the counting procedures involved and which are reflected in the (admittedly peculiar) procedure of using probability amplitudes and Born's or related rules to count them, rather than by working only with probabilities themselves.[12] It is also true that the quantum mechanical situation requires delicate handling when we link it to particular mathematical theories and conceptions of probability. Nevertheless, quantum chance is still chance (a random occurrence) and quantum probability is still probability (an estimate of how likely an occurrence of a given event is), just as they are in classical physics. Bohr did not think that any special mathematical form of probability was necessary for quantum mechanics, any more than any special form of quantum logic, of which he was even more suspicious (Bohr 1958, *PWNB* 3, pp. 5–6). From his viewpoint, we have well-defined individual physical phenomena and rigorous rules for predicting them, based on the standard logic or the standard mathematics of probability.

One the other hand, part of the epistemological complexity of the quantum situation may be linked to the epistemological nature of probability theory and its own dependence on interpretation. Accordingly, the Bayesian view of probability holds particular interest for the foundations of quantum mechanics from

[12] Recently, Christopher A. Fuchs and coworkers have pursued, in the context of quantum information theory, a project of bypassing "amplitudes" and establishing quantum theory as working directly with probabilities in the case (it is worth qualifying) of discrete variables. See Fuchs and Schack (2009a, b) and reference there. For an earlier attempt along these lines see Glauber (1963). The project, which involves substantial mathematical difficulties, is still underway. It has considerable foundational, as well as practical, significance by virtue of suggesting that while quantum theory may not be able to avoid probabilities, it may dispense with amplitudes. This would enable the theory to relate more directly to the experiments, as opposed to a more artificial machinery of the present version of quantum mechanics. The mathematics of the two approaches is equivalent, which suggests that, although we may be able to avoid amplitudes, we might need the mathematics that involves complex numbers. The argument of Fuchs and coworkers expressly depends on Hilbert spaces (in this case of finite dimensions) over complex numbers and would not work with Hilbert spaces over real numbers. This fact as such does not prove that a real-number quantum mechanics (which could, accordingly, not be mathematically equivalent to the standard version) is impossible, but it does indicate that it might be difficult to develop it. I am grateful to Christopher Fuchs for the discussions concerning the subject and for directing my attention to the importance in this context of Roy Glauber's earlier work just cited, which brought him a Nobel Prize in 2005.

this perspective, as the foundational work on the subject in foundational research in quantum information theory has shown. The relationships between different interpretations of probability theories and different interpretations of quantum mechanics are significant, and they have led to stimulating debates concerning the mathematical aspects of quantum probability. There are numerous arguments regarding what kind of probability theory—such as frequentist, Bayesian, Kolmogorovian, contextual—is best suited to quantum theory, including quantum statistical physics, which deals expressly with quantum multiplicities.[13] These arguments unavoidably involve the question of interpretation of quantum mechanics or other quantum theories. I shall, however, put these arguments aside, given that my main concerns are physical and epistemological, although mathematics remains part of the conceptuality in question. They do not affect my main argument concerning the particular, nonclassical, set of interpretations of quantum mechanics considered here and the *physical* character of chance and probability such interpretations entail. This argument would apply regardless of which mathematics of probability is used, or how this *mathematics* is interpreted, and my appeal to the Bayesian approach is motivated by physics and epistemology as well, as defined by nonclassical interpretations. In other words, although the relative merits or disadvantages of the Bayesian vs. frequentist view are part of the debate concerning the epistemology of the concept of probability and its mathematization, the issue does not essentially affect my argument. As discussed in Chapter 6, quantum mechanics can be interpreted along frequentist lines, as it was by von Neumann or Pauli, among others, and nonclassical frequentist interpretations are possible. Nonclassical epistemology, however, appears to more readily invite the Bayesian view, which facilitates nonclassical epistemology by virtue of relating to the fact that even the primitive individual processes and events cannot be considered causally.

Now, in considering the relationships between epistemology and probability in quantum theory, the question is not whether a theory of quantum phenomena can avoid probability, in the way classical mechanics does. The probabilistic character of our predictions concerning these phenomena is unavoidable, since, to return to Bohr's formulation, "one and the same experimental arrangement may yield different recordings" of their outcomes (Bohr 1954, *PWNB* 2, p. 73). It is, again, possible to speak of "one and the same experimental arrangement" here, since, unlike the outcomes in question, we can control the measuring instruments involved, given that the parts of these instruments relevant for setting up our experiments can be described classically. Accordingly, the situation has Bayesian implication given that, under these conditions, the probabilistic character of such predictions will also concern primitive individual quantum events. For, unlike in the case of certain classical individual events, such as a coin toss, in the case of quantum phenomena it does not appear

[13] On some of these issues, see Khrennikov (2009b).

possible—and in a nonclassical view, it is in principle impossible—to subdivide these phenomena into entities of different kinds, concerning which our predictions could, even in principle, be exact. As discussed in the preceding section, any attempt at subdividing the phenomena will require the use of an experimental setup that will reconstitute the phenomena of the epistemologically same type (it could be different physically), concerning which we could, again, only make in general probabilistic predictions.

Accordingly, rather than the question of whether quantum phenomena entail a probabilistic theory predicting them (since they manifestly do), the question is whether there is or is not an underlying classical-like, causal dynamics ultimately responsible for such events. If this kind of underlying dynamics exists, it would imply that a classical-like account of such events could eventually be developed, as Einstein hoped with the model of classical statistical physics in mind, although it cannot be guaranteed that it would. By contrast, quantum phenomena appear to disable the underlying assumptions of classical statistical physics (or accounts of individual classical phenomena that we cannot sufficiently track), based on the causality of the primitive individual processes involved. In other words, in quantum theory it is difficult to assume that randomness and, hence, the necessity to use probability merely arise in view of our inability to access the underlying *causal* dynamics determining the behavior of the system considered. The fact that probabilistic predictions of these theories are correct is, again, mysterious (without mysticism), and we might just be lucky to be able to make them.

This situation makes chance and probability irreducible even and in particular in considering the behavior of individual quantum objects or individual events or phenomena in Bohr's sense—the only entities to which any quantum mechanical predictions can apply in Bohr's or the present interpretation. In a nonclassical view, the random character of quantum events and the necessary "recourse to probability laws" do not arise by virtue of some hidden underlying mechanical complexity of the type found in classical statistical physics or elsewhere. Instead, they are due to "the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or 'individuality,' characterizing the elementary processes"—in other words, the features captured by Bohr's concepts of phenomenon and atomicity (Bohr 1949, *PWNB* 2, p. 34). This understanding of quantum probability is Bayesian, expressly in the present view and de facto in that of Bohr. The reverse is, again, not true, since one can use a Bayesian approach along epistemologically classical lines. I shall return to this later (1949) statement by Bohr below. First, I shall consider another statement by Bohr, which opens the Warsaw lecture, where Bohr's concepts of phenomena and atomicity were introduced and where they were related to the question of causality and probability from the outset, as Bohr's title, "The Causality Problem in Atomic Physics," indicates. Bohr writes,

> The unrestricted applicability of the causal mode of description to physical phenomena has hardly been seriously questioned until Planck's discovery of the quantum of action,

> which disclosed a novel feature of atomicity in the laws of nature supplementing in such unsuspected manner the old doctrine of the limited divisibility of matter. Before this discovery statistical methods were of course extensively used in atomic theory but merely as a practical means of dealing with the complicated mechanical problems met with in the attempt at tracing the ordinary properties of matter back to the behaviour of assemblies of immense numbers of atoms. It is true that the very formulation of the laws of thermodynamics involves an essential renunciation of the complete mechanical description of such assemblies and thereby exhibits a certain formal resemblance with typical problems of quantum theory. So far there was, however, no question of any limitation in the possibility of carrying out in principle such a complete description; on the contrary, the ordinary ideas of mechanics and thermodynamics were found to have a large field of application also to proper atomic phenomena, and above all to offer an entirely sufficient basis for the analysis of the experiments leading to the isolation of the electron and the measurement of its charge and mass. Due to the essentially statistical character of the thermodynamical problems which led to the discovery of the quantum of action, it was also not to begin with realized, that the insufficiency of the laws of classical mechanics and electrodynamics in dealing with atomic problems, disclosed by this discovery, implied a shortcoming of the causality ideal itself. (Bohr 1938, *PWNB* 4, pp. 94–95)

Bohr, thus, makes a strong historical claim, which he elsewhere extends to the history of philosophy as well. This extension is not surprising, given the fundamental relationships between philosophy and classical physics, and, as noted earlier, philosophical conceptuality may in turn be seen as a particularly refined form of everyday concepts and language.[14] The problematic in question is expressly at work in Kant or, in his confrontation with Kant concerning the relationships between conceptuality and reality, in Hegel, and in a long line of thinkers following and quite a few preceding them. The Kant–Hegel confrontation, in which classical physics played a major shaping role, defines the history of modern philosophy, as much as the Bohr–Einstein confrontation defines the history of quantum mechanics, and, in many respects, in parallel fashion. As discussed earlier, Bohr is closer to Kant, and Einstein closer to Hegel, insofar as Bohr and Kant give at least human concepts considerably less power than do Hegel and Einstein in determining the ultimate nature of reality.

This view makes both Kant and, more radically, Bohr deeply question the limits of applicability of the idea of causality, in Kant's case extending Hume's critique of causality. As Nietzsche—who offered one of the deepest and most radical philosophical critiques of causality—said, "Let us recall, … *Kant's* tremendous question mark that he places after the concept of 'causality'—without, like Hume, doubting its legitimacy altogether. Rather, Kant began cautiously to delimit the realm within which this concept makes sense (and to this day we are not done with this fixing of limits)" (Nietzsche 1974, p. 305). Nietzsche's parenthesis is still valid. It may indeed be more precise to say that questioning the limits of our knowledge and thought in dealing with the

[14] The character of this refinement is, again, different from that found in physics, in part by virtue of the mathematical nature of modern physics, although not disconnected from it, since the genealogy of both could be traced to Aristotle and Plato, or even the pre-Socratics.

ultimate nature of things is reciprocal with questioning and fixing the limits of causality: Both are part of the same critical philosophical analysis. While Kant forbade the use of "the game of probability" in critical philosophy, especially metaphysics (foundations of a theory should be certain, he believed, using geometry as an example), he understood very well the role of probability outside metaphysics. Kant, however, believed that the ultimate nature of the world is causal, and he would not see his view as a bet. It might have been, however—just as all our theories of the world (such as quantum mechanics in any interpretation) might be, no matter how logical, including mathematical, their foundations (as they are in quantum theory).

In Bohr's view, especially when it reaches its nonclassical stage, concepts—including any concept of ontology or reality, or causality—have no such power, while remaining indispensable including in any practice of this epistemology, since we reach and work at this limit through them. According to Bohr, "[E]ven in the great epoch of critical [i.e., post-Kantian] philosophy in the former century, there was only a question to what extent *a priori* arguments could be given for the adequacy of space-time coordination and causal connection of experience, but never a question of rational generalizations [such as complementarity] or inherent limitations of such categories of human thinking" (Bohr 1949, *PWNB* 2, p. 65). That this statement occurs in "Discussion with Einstein" is hardly coincidental, and it appears to serve a deliberate placement by Bohr of his debate with Einstein in relation to the history of philosophy. Even the more radical philosophical questionings of causality, such as those by Hume or Kant, are those of our epistemological capacity to perceive the underlying causal world, which would be presupposed at the ultimate level as inaccessible to us. It is only with Darwin's evolutionary theory and Nietzsche's philosophy that this (classical) view of causality and chance begins be questioned. It is worth noting that Darwin's and Nietzsche's thought is contemporaneous with the emergence of the kinetic theory of gases and thermodynamics—both statistical theories, which, however, unlike Darwin and Nietzsche, still assume the ultimate underlying causality.[15] In physics (one is compelled to agree with Bohr), this more radical questioning of causality does not appear before quantum physics comes on stage. However, by extending the questioning of causality to, at least for now, its limit, quantum theory also revealed the deeper nature of the problem of causality, or again, reality. This is why Bohr speaks of complementarity as "a rational generalization of the very ideal of causality" and as re-delimiting the scope within which the idea of causality applies, rather than simply abandoning causality (Bohr 1949, *PWNB* 2, p. 41). I would now like to formulate the quantum mechanical concept of chance advanced here in more general philosophical terms.

It is worthwhile first to summarize the *classical* understanding of chance. Classically, chance or, indeed (given that, classically, the ultimate reality is

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[15] Cf. Kuhn's comments on Darwin (Kuhn 1962, pp. 171–172).

always causal, is necessity), the *appearance* of chance is seen as arising from our insufficient and perhaps, in practice, unavailable knowledge of a total configuration of forces involved and, hence, of a lawful necessity, postulated behind an apparently lawless chance event. If this configuration becomes available, or if it could be made available in principle (it may not be available in practice), the chance character of the event would disappear. It is the *presupposition* of the existence of this configuration that is essential for and defines the classical view of chance and causality. On this point *classical* ontology and *classical* causality come together; or rather, this presupposition brings them together. The situation is similar to that of Oedipus in Sophocles's *Oedipus the King*, who eventually discovers in the course of his investigation that the chance events that happened to him were inevitable, arising from the architecture of fate preordained by gods. Subtle and complex as they may be, *all scientific* and *most philosophical* theories of chance and probability prior to quantum theory and many beyond it, and most philosophical theories of chance from the earliest to the latest, are of the type just described.[16]

I qualify my statement because, beginning with the pre-Socratics as well, there emerged three main conceptions of ontology or reality, defined by this confrontation between chance and necessity or causality. The first ontology, the classical ontology of chance, as just considered, defines chance as only apparent or illusory, while at the ultimate level of reality necessity and order rule. The second ontology is, conversely, defined by the rule or un-rule of chance, which makes all necessity, causality, or order only apparent or illusory. This ontology may be called the Jocasta ontology, since it was arguably first, or in any event most powerfully, expressed by Jocasta, Oedipus's mother and wife in *Oedipus the King*: "Fear? What should a man fear? It's all chance, chance rules our lives. Not a man on earth can see a day ahead, groping through the dark. Better to live at random, best we can" (ll. 1068–72, Sophocles 1984, p. 215). This view is proved illusory in the play, since the lives of the play's characters are ultimately ruled by fate. However, this view was contemplated by the ancient Greeks. The third ontology is that of the interplay of chance and necessity, which appears to have been introduced by Democritus and developed his followers, such as Lucretius, as the atomist ontology of nature.

Nonclassical epistemology, as epistemology without (specifiable) ontology, introduces a fourth alternative. Although one can find earlier intimations, this epistemology appears to emerge only following Kant, especially in Nietzsche and Darwin, in whose work one finds the first scientific manifestation of this view, and in Bohr, with whom it receives a rigorous scientific articulation. Such an articulation is not found in Darwin, although, in philosophical terms, it is offered by Nietzsche. The corresponding concept of chance is what I call nonclassical chance.

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[16] For an account of this history (consistent with Bohr's view), see Hacking (1984).

Nonclassical chance is irreducible not only in practice but also in principle. There is no knowledge in principle available to us, now or ever, that would allow us to eliminate chance and replace it with the picture of necessity behind it. Nor, however, can one postulate a causal dynamics behind the chance events in question as unknown, or even unknowable, but existing as outside our engagement with it, and hence at least thinkable in the manner of Kant's things-in-themselves. This qualification, correlative to the radical suspension of at least thinkable ontology or reality at the ultimate level of description, is, again, crucial, both in defining the nonclassical view of chance and because, in view of Bell's and related theorems, it does not appear possible to assume this type of ontology in the case of quantum phenomena. (There are, again, debates concerning this claim.) As I just explained, some forms of the classical understanding of chance allow for this type of assumption, including in quantum mechanics, as discussed in Chapter 6. In such interpretations, the independent behavior of quantum objects is considered causal, and the manifest lack of causality and the probabilistic character of quantum mechanical predictions is brought about by the disturbance of this behavior by observation. Nonclassical chance that we encounter in quantum physics in a nonclassical interpretation is not only unexplainable in practice and in principle but is also irreducible in practice and in principle—to any physical causality, knowable or unknowable, thinkable or unthinkable. Accordingly, in the case of the individual events considered one can indeed speak, with D. Bohm, of a certain "irreducible lawlessness" (Bohm 1995, p. 73). Once again, however, this is true only in the case of individual quantum events, since in certain circumstance collectivities of quantum events exhibit a certain correlational order, manifest in quantum probability, the origin of which is equally unexplainable.[17]

Any element defining a quantum event or phenomenon could, however, only be correlated with its experimental preparation, say an "emission" from a source, in statistical terms—that is, on the basis of other observations and measurements that one can perform in similar circumstances. No given preparation can guarantee that a single emission, corresponding to the observed

[17] A qualification may be due here. While not completely free of chance and chaos, thought and knowledge (in-*form*-ation) are defined by form and order, which fact appears to block thought from accessing the ontology defined by absolute chance. Accordingly, just as in the case of nonclassical epistemology, the absolute randomness of the ultimate constitution of nature can only be postulated or argued for by means of certain effects of this constitution upon what we can think and know. In the case of nonclassical epistemology, however, one cannot see the ultimate nature of things in terms of absolute chance or chaos, any more than in terms of any given conception of order, or of any given combination of both. The cases of other forms of ontology mentioned here are different, since there it is only a question of difference between knowledge and the nature of things, accessible to one degree or another, or inaccessible, while both are defined either strictly by necessity and order or by the interplay of necessity and chance. The Democritean interplay of chance and necessity appears to be unavoidable at the phenomenal level of thought and knowledge, which are defined by this interplay.

event, has actually taken place, since any given quantum-like trace could be a product of some other random quantum (or possibly classical) event, unrelated to any arrangement we made. No emission from a quantum source is ever assured, regardless of preparation, even though the instruments involved can, in a preparation for an emission, be controlled just as they can be in classical physics. No event itself involving quantum objects themselves can be so controlled, however, and, hence, such an event is, in general, not repeatable. The irreducible nature of quantum probability, as manifested in our expectations concerning the outcome of individual events, is defined by the irreducible individuality, atomicity, of all quantum phenomena, and the view of quantum objects and processes as inaccessible to our thought and knowledge.

Bohr appears to have stated his nonclassical view explicitly for the first time in 1937, in "Complementarity and Causality." He said, "The renunciation of the ideal of causality in atomic physics which has been forced on us is founded logically only on our not being any longer in a position to speak of the autonomous behavior of a physical object, due to the unavoidable interaction between the object and the measuring instruments which in principle cannot be taken into account, if these instruments according to their purpose shall allow the unambiguous use of the concepts necessary for the description of experience" (Bohr 1937, *PWNB* 4, p. 87). As, however, Bohr notes, in "Discussion with Einstein:" "[I]t is most important to realize that the recourse to probability laws under such circumstances is essentially different in aim from the familiar application of statistical considerations as practical means of accounting for the properties of mechanical systems of great structural complexity. In fact, in quantum physics we are presented not with intricacies of this kind, but with the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or 'individuality,' characterizing the elementary processes" (Bohr 1949, *PWNB* 2, p. 34). One should perhaps refer to the indivisibility *and* individuality of phenomena, restricting us to probabilistic estimates even as concerns the outcome of individual quantum events, something that Einstein refused to entertain as a viable starting point for a future development of quantum theory.

Bohr's and Einstein's epistemological positions may be seen as correlative to their respective views of probability in quantum physics, and thus also in terms of juxtaposition between Heisenberg's and Einstein's ways of doing physics, including as concerns the role of mathematics in physics. The question of locality of quantum mechanics is implicated in the statistical aspects of the EPR situation, addressed in Einstein's later arguments of the EPR type, which I would like to consider now. It is worth recalling, first, the key relevant points of my argument in Chapters 8 and 9. EPR's argument is made insufficient by the nature of quantum phenomena as defined by the irreducible role of measuring instruments in their constitution, which makes it impossible to consider the behavior of quantum objects independently of their interaction with these instruments. The application of EPR's criterion of reality becomes ambiguous by virtue of the lack of the qualifications of this criterion required by these conditions, not adequately considered by EPR. “Since,” Bohr concludes, “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 argument of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete” (Bohr 1935b, p. 700).

These conditions and, hence, the completeness of quantum mechanics are also essentially linked to the irreducibly probabilistic nature of our predictions concerning quantum events, in particular, individual quantum events, by virtue of the impossibility, as against classical physics, of securing the *identical outcomes* of the *identically prepared experiments*. This circumstance may not appear relevant in the EPR situation, since the predictions in question can be made with probability equal to unity. As I argued in Chapter 8, however, it is crucial there, and is merely obscured by the fact that the EPR predictions are made with certainty. The role of these statistical considerations appears to have been underappreciated by EPR, although it did not escape Einstein’s attention in his later arguments. EPR’s and these later arguments by Einstein for the incompleteness, or else nonlocality, of quantum mechanics subtly depend on the proper coordination of the outcomes of the key measurements involved in two repeated EPR experiments on two different, but identically prepared, EPR pairs. This type of coordination is possible in considering classical phenomena or (this difference is not relevant in classical physics) objects. It is, however, not possible in considering quantum phenomena, including those of the EPR type, in view of the impossibility of securing the identical outcomes of the identically prepared experiments. It follows that quantum mechanics, which adequately covers what is possible as concerns these phenomena, may be seen as a complete, as well as (since an analogous counterargument to EPR applies in the case of locality) local, theory of quantum phenomena. At least, EPR or Einstein in his later arguments has not demonstrated otherwise. Bohr, accordingly, contends that the situation entails “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,” while allowing for the locality of quantum phenomena and quantum mechanics (Bohr 1935b, p. 697). It is this joint imperative that ultimately led Bohr to his nonclassical view, expressed in the statement that ultimately defines his response to EPR’s and related arguments by Einstein. He says, “[W]e are not dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with a recognition that such an analysis is *in principle* excluded” (Bohr 1949, *PWNB* 2, p. 62).

Bohr specifically responds, in 1949, to Einstein’s argument made in 1936, entitled “Physics and Reality.” This argument is described by Bohr as follows: “[T]he quantum-mechanical description is to be considered merely as a means of accounting for the average behaviour of a large number of atomic systems” as against “the belief that it should offer an exhaustive description of the individual phenomena” (Bohr 1949, *PWNB* 2, p. 61). This argument persists throughout Einstein’s life; apart from using it in the Schilpp 1949 volume

(Einstein 1949a, b), Einstein invokes it, for example, in his letters to Born as late as 1954, the last year of his life (Born 2005, pp. 166–170, 204–205, 210–211). In Einstein's own words, he saw "the belief that it should offer an exhaustive description of individual phenomena" as "logically possible without contradiction," but found it "so very contrary to his scientific instinct that [he could not] forego the search for a more complete conception" (Einstein 1936, p. 375; Bohr 1949, *PWNB* 2, p. 61).[18] It is not altogether clear how far Einstein perceived the nonclassical implications of this belief. He might have seen this belief along the lines of Bohr's reply to EPR, whereby, in accordance with the uncertainty relations, one of the two conjugate quantities is assumed ascribable to a quantum object the time of a measurement. On the other hand, as we have seen, he also misread Bohr by assuming that his position allows for nonlocality. In any event, Bohr's nonclassical view was not quite yet in place at the time. By contrast, Bohr's 1949 comments, in response to Einstein's statement, implies a nonclassical view of the situation, as a probabilistic theory of individual events, and it appears to suggest that Einstein rejects this view as well, which would not be surprising.

The question (in either eventuality) is on what grounds a search for an alternative may be deemed successful, especially if the EPR-type experiment may be shown not to offer such grounds. For a mere *rejection* of given argumentation (such as that of Bohr) and a desideratum for an alternative, "more complete conception" (on Einstein's definition) do not in themselves constitute a demonstration of either logical or experimental deficiency of this argumentation, even though, as Bohr acknowledges, Einstein's "attitude might seem well balanced in itself" (Bohr 1949, *PWNB* 2, p. 62). According to Bohr, "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" (Bohr 1949, *PWNB* 2, p. 57). Ostensibly, this statement refers to Einstein's earlier arguments, but, by this time, in 1949, Bohr clearly has all of Einstein's arguments in mind, including those of the EPR type. In particular, the comment concerning "exhaust[ing] the possibilities of observation" must have been made with EPR's argument in mind. Bohr follows this sentence with his point that grounded discussion of the EPR experiment in Chapters 8 and 9, that "we must realize that in the problem in question we are

[18] As indicated earlier, technically, in a nonclassical regime, we cannot consider a given "quantum system" either as "individual" or as "multiple," any more than any other terms. We can, however, speak of individual phenomena as individual effects of the interaction between a given "system" and some measuring apparatus. This qualification does not affect the argument pursued at the moment, since the individuality Einstein has in mind on the atomic level is correlative to individual, rather than multiple, effects. "Ensembles of [quantum] systems" to which Einstein refers comprise multiple effects manifested in measurable quantities.

not dealing with a *single* specified experimental arrangement, but are referring to *two* different, mutually exclusive arrangements" (Bohr 1949, *PWNB* 2, p. 57; Bohr's emphasis).

There are further subtleties to Einstein's 1936 assessment that the belief in question is "logically possible with contradiction... but [is] so very contrary to [his] scientific instinct." It is true that this belief refers, as Bohr says, to the argument that "the quantum-mechanical description is to be considered merely as a means of accounting for the average behavior of a large number of atomic systems." But it also refers to the belief that one can derive the inner workings of individual quantum systems—quantum mechanics as a statistical theory of ensembles. It replies to the following rhetorical question: "Is there really any physicist who believes that we shall never [in the quantum-mechanical way] get an inside view of these important alterations [due to individual perturbations] in the single system, in their structure and their causal connections, and this regardless of the fact that these single happenings have been brought so close to us, thanks to the marvelous invention of the Wilson [cloud] chamber and the Geiger counter?" (Einstein 1936, p. 375). Einstein's question is not unreasonable. It is not easy to believe that it is possible to ever get an inside view, also literally in the sense of visualization [*Anschaulichkeit*], of the inner workings of individual quantum systems themselves from quantum mechanics as a predictive theory. It is, by definition, in principle impossible in Bohr's interpretation, even in the version found in his reply to EPR and more radically in his nonclassical version of it. One is indeed likely to need a different theory in order to achieve this, if quantum phenomena will ever allow us to do so. Einstein believed that such a theory should be possible in view of the EPR experiment, while Bohr argued—including by way of exposing the problems of EPR's and other arguments by Einstein concerning this experiment—that such a theory might not be possible. This view appears to be supported by Bell's theorem and related developments, or at least, thus far, it is not contradicted by either theoretical arguments or experiments. As I have stressed throughout this study, definitive claims are difficult in this domain. At the very least, however, one could argue, with Bohr, that Einstein did not demonstrate the contrary in his argument of the EPR type.

Einstein's post-EPR arguments do allow for the possibility that quantum mechanics could be regarded as a statistical theory of ensembles, in which case it could also be seen as local. In this view, quantum mechanics would be *analogous* to classical statistical physics. According to Einstein, if one regards the wave function as relating to "many systems, to 'an ensemble of systems,' in the sense of statistical mechanics," then "the paradox" arising in view of EPR's argument is eliminated (Einstein 1936, p. 375; Einstein 1948; Einstein 1949a, p. 81; Letter to Born, December 3, 1953, Born 2005, p. 205; Letter to Born, January 12, 1954, Born 2005, p. 211). "The paradox" appears to refer to nonlocality, since Einstein uses the term in this context in other works just cited in the sense that, if one assumes that quantum mechanics is complete, it is possible in the

EPR case to conclude by performing two alternative measurements on S_1 that S_2 possesses two incompatible states (e.g., Einstein 1949a, p. 81).

Einstein's statistical alternative, however, still leaves quantum mechanics incomplete by virtue of its inability to provide a properly exhaustive physical description of the behavior of individual quantum systems of the kind classical mechanics does, including for the individual constituents (such as molecules of a gas) of the systems considered in classical statistical physics. At most, quantum mechanics, again, provides an incomplete description of such systems along the lines of EPR's argument, since one must maintain locality at this level, although "Physics and Reality" makes a stronger statement: "[W]hat happens to a single system" is also "*entirely* eliminated" (Einstein 1936, pp. 375–377; emphasis added). In other words, on this view, quantum mechanics provides no account of individual quantum systems at all. On subsequent occasions, Einstein appears to assume that quantum mechanics gives an *incomplete* description of individual systems on the lines of EPR's article, which, again, allows one to avoid nonlocality. As he says, "[O]ne can safely accept the fact ... that the description of the single system is incomplete, if one assumes that there is no corresponding complete law for the complete description of the single system which [law] determines its development in time" (A Letter to Born, December 3, 1953, Born 2005, p. 205). In this case, "The statistical character of the ... theory would ... follow necessarily from the incompleteness of the description of the [individual] systems in quantum mechanics" (Einstein 1949a, p. 81).

Thus, from Einstein's point of view, the introduction of quantum mechanics, while an important step, kept the quantum riddle—the riddle posed by Planck's discovery and the old quantum theory—essentially intact. A proper mechanics is lacking either way: If quantum mechanics is a complete description of individual quantum systems, it is nonlocal, which is unacceptable; it may be considered local if it is merely a statistical theory of ensembles, in which case, however, there is either an incomplete description of individual quantum systems or no such description at all. By the same token, quantum mechanics cannot be seen as providing a proper starting point for a future theory, of the type Einstein wanted, of the ultimate constitution of nature. For—given that the theory must, in view of the locality considerations, be assumed to be incomplete—"there would no longer exist any ground for the assumption that a future foundation of physics must be based upon statistics." He adds in a strong elaboration, effectively his credo, which may, again, be seen in terms of juxtaposition between Heisenberg's and Einstein's physics, perhaps, thus, the defining confrontation of twentieth- and now twenty-first-century theoretical physics:

> It is my opinion that the contemporary quantum theory represents an optimal formulation of the relationships, given certain fixed basic concepts, which by and large have been taken from classical mechanics. I believe, however, that this theory offers no useful point of departure for future development. This is the point at which my expectation deviates most widely from that of contemporary physicists. They are

> convinced that it is impossible to account for the essential aspects of quantum phenomena (apparently discontinuous and temporally not determined changes of the state of a system, simultaneously corpuscular and undulatory qualities of the elementary carriers of energy) by means of a theory that describes the real state of things [objects] by continuous functions of space for which differential equations are valid. They are also of the opinion that in this way one cannot understand the atomic structure of matter and radiation. They rather expect that systems of differential equations, which might be considered for such a theory, in any case would have no solutions that would be regular (free from singularities) everywhere in four-dimensional space. Above everything else, however, they believe that the apparently discontinuous character of elementary processes can be described only by means of an essentially statistical theory, in which the discontinuous changes of the system are accounted for by continuous changes of the probabilities of possible states. (Einstein 1949a, p. 83)

Einstein, thus, again questions "a purely algebraic method of description of nature, that is, . . . the elimination of continuous functions from physics" and thus "giv[ing] up, in principle, the space-time continuum" which "*perhaps* the success of the Heisenberg method points to" (Einstein 1936, p. 378; emphasis added). Unlike in this *slightly* more cautious earlier assessment (as we have seen, he described the method as "an attempt to breathe in empty space" even in 1936), he now appears to reject this "method" and the corresponding philosophy altogether. These are Einstein's "experiences with the theory of relativity," a continuous field theory, that "determine [his] expectations," since, "in [his] opinion, these equations are more likely to tell us something more *precise* than all other equations of physics" (Einstein 1949a, p. 83). These expectations, this passage further suggests, would also appear to entail an introduction of new concepts, presumably of the field-theoretical type, as against those of classical mechanics, the point which I addressed in the Introduction and to which I return presently. I leave aside the problem of singularities, already apparent by then, which was never resolved in the way Einstein wanted even in general relativity, although it hardly helps Einstein, and by now we know (Einstein of course did not) that the situation involved quantum considerations as well (Hawking's radiation).

Could extending these equations or this type of approach work in quantum theory? Believing that it could would indeed fly in the face of then contemporary (or the present) expectations of most (albeit not all) "contemporary physicists," given the developments of quantum electrodynamics along the lines of "a purely algebraic method of description of nature." This method just then enjoyed a new success with the accomplishment of the renormalization program in the work of Tomonaga, Schwinger, Feynman, and Dyson. Einstein must have been aware of these developments, although the approach could have hardly encouraged him, and he appears never to have put much trust into quantum field theory. His own program even for bringing together classical electromagnetism and gravity remained unfulfilled, although it was primarily responsible for the dream of grand unification in physics—which, however, has been primarily pursued, apart from gravitation, by means of quantum theory. It is, as I said, possible that the ultimate unification of all forces of nature, if ever achieved, will be closer to Einstein's vision than to quantum field-theoretical approach. However, the reverse is also possible.

For the moment, one need not conclude, as Einstein does here and in his related articles, that quantum mechanics, if, again, local, could be seen only as "*an essentially statistical theory of ensembles*." It may instead be seen *an essentially probabilistic theory of individual quantum systems* or more accurately *individual quantum phenomena*, rather than only of ensembles of such phenomena. (It is that too, since it also offers statistical accounts of quantum multiplicities, including by way of quantum statistical theory, to the development of which Einstein made major contributions himself, via Bose's theory [Einstein 1925].) I qualify my statement because (in this respect Einstein is right) it does not, at least in the present view, offer a description of or even predictions concerning the individual independent behavior of quantum systems themselves, considered apart from their interaction with the measuring instruments. This point applies even in the less radical version of Bohr's argument offered in his reply to EPR. In nonclassical interpretations, quantum systems and their behavior are left altogether beyond all description or conception even at the time of measurement. Either way, however, this argument, as against Einstein's frequentist view, makes quantum mechanics a probabilistic theory even in regard to predicting individual quantum phenomena. A similar view defines the Bayesian approach to quantum information theory, which Einstein's arguments of the EPR type, here considered, help to articulate and sharpen (Caves et al. 2007).

Einstein speaks of the wave function as referring to " 'an ensemble of systems,' in the *sense* of statistical mechanics" (Einstein 1936, p. 375; emphasis added). The term "sense," the *sense* of this sense, requires qualification. We recall that Einstein admitted in 1953 that he could finally contemplate that God played dice, but could not fathom why he would gamble "according to definite rules" of quantum mechanics, which are indeed hardly those of dice playing (*QTM*, p. 8). On the other hand, it is unlikely that Einstein had classical statistical mechanics and its models in mind as a rigorous possibility. He was, we recall, the first to show that Planck was wrong in assuming such a model in the case of his law and, hence, that his derivation of his law was wrong, even though the law itself was correct. Later on, Einstein made a major contribution—via Bose's work (which re-derived Planck's law and did it properly)—to quantum statistics (Einstein 1925). This statistics requires a very different type of counting, manifested in any quantum experiments (such as the double-slit experiments) or in the correlations manifested in experiments of the EPR type, and it is incompatible with the physical assumptions of classical statistical physics. Einstein is more likely to have had in mind that we needed new concepts to develop a proper theory of quantum objects and processes, the point that, as discussed in the Introduction, he made on several occasions, as did Schrödinger. Indeed, Einstein thought along these lines (especially with a field theory in mind) even before quantum mechanics, and, accordingly, saw (in contrast to Heisenberg's "method") de Broglie's theory and then Schrödinger's wave mechanics as holding a great promise, albeit unrealized.[19]

Hence, Einstein, or again, Schrödinger, resisted Bohr's idea that we did not need new concepts and only needed classical concepts—an idea that, as discussed in the Introduction, they appear to have misunderstood. To recapitulate it briefly here, Bohr's point is double. First, classical ("old") physical concepts suffice at the level of measuring instruments to handle all the available physical evidence in question in quantum theory. Second, no refinement in our thinking (which led to the concepts of classical physics or may be responsible for whatever physical concepts we may have) might allow us to reach quantum objects and processes. A nonclassical epistemology of quantum mechanics—or, at the very least, the nearly nonclassical epistemology of it found in Bohr's reply to EPR—follows. Neither, however, could satisfy Einstein or Schrödinger. For Bohr, on the other hand, it is this type of epistemology that is responsible for the probabilistic character of all our predictions concerning quantum phenomena. Hence, as Bohr says in his reply to EPR: "[A]ny comparison between quantum mechanics and ordinary statistical mechanics,—however useful it may be for the formal presentation of the theory,—is essentially irrelevant. Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way" (Bohr 1935b, p. 699).

Thus, against Einstein's view, it appears to be nature that makes chance and probability irreducible in our interaction with it at the quantum level of its constitution, at least as far the experimental data in question reflects this constitution. This circumstance would make any theory of such processes probabilistic even in the case of individual quantum phenomena and events—in other words, only as complete as quantum mechanics is. The kind of physical description Einstein has in mind cannot be altogether excluded, and Bohr was cautious in this regard, as suggested by his persistent use of such locutions as "would seem" and "would appear." It *would not seem* likely, however, that this kind of physical description will be found, given the experimental circumstances and data in question—especially, again, in view of such subsequent developments as Bell's and related theorems and the corresponding experimental findings—any more than a return to a classical rather than relativistic view of gravitation or electrodynamics (special relativity) is likely. Indeed, Bohr sees quantum mechanics as "no more than a first step in the necessary generalization of the classical mode of description" in our encounter with deeper features of

[19] Einstein, as I said, was not enamored of Bohm's theory (in its initial 1952 version, the only one available before Einstein's death), in part on account of its nonlocality, but also, it appears, given its too close proximity to quantum mechanics, whose predictions Bohmian theories reproduce. This makes them no more complete than quantum mechanics, unless we find some hidden variables for which they or, better, some local theory would account, while quantum mechanics would not, which has not been the case thus far.

nature in its quantum constitution, and he refers to quantum field theory as potentially requiring "a still more radical renunciation" in this regard (Bohr 1937, *PWNB* 4, p. 88).

Is "a still more radical renunciation" by future quantum theories inevitable? It would be difficult to make a long-term guess, and, as stressed from the outset, I prefer to err on the side of caution. As I shall explain in the Conclusion, however, higher level quantum theories may take us farther on the road anticipated by Bohr here, and he clearly has quantum electrodynamics in mind in making this statement. For the moment, this interpretation is, at least, good enough—and in the view of this particular author, at the very least as good as any alternative and better than most. Again, one cannot deny that "skepticism about the necessity of going so far in renouncing customary demand as regards the explanation of nature phenomena," invoked by Bohr in 1949, continues to be widespread and indeed dominant (Bohr 1959, *PWNB* 2, p. 63). However, one might take a more positive view: that we are lucky that quantum mechanics and higher level quantum theories enable us to make rigorous predictions concerning quantum phenomena, even if without allowing us to obtain classical-like knowledge—or even to form a conception—concerning the (ultimate?) constituents of nature that we call "quantum." This remarkable fact has enabled the disciplinary practice of quantum mechanics, Heisenberg's physics, as physics and as a disciplinary continuation of classical physics. It is conceivable that, as we move on, physics will confront randomness or strangeness in nature or in our interaction with nature that is beyond any possible regularity. At that point, physics as a mathematical science of nature, as we know it now, is likely to stop, and either other forms of knowledge of nature will take over or we will reach an absolute limit in this respect. We may also continue to be lucky or may arrive at the point when physics will become epistemologically more classical again, as Einstein hoped and as so many others continue to hope—a hope that is difficult and perhaps impossible to defeat.

Quantum theory has lived an extraordinarily successful, although not always easy, century—from Planck's black body radiation law to Einstein's photon to Bohr's atom to Heisenberg's and Schrödinger's quantum mechanics to Dirac's quantum electrodynamics to quantum field theory and beyond. However, given that our current theories are manifestly incomplete, and given what we have learned from that century, it remains hard to predict where we may arrive even in the short run. So, we had *better* stay tuned. Although one should not make too much of the common etymology of *bet* and *better*, there is no harm in using it on occasion, and I shall take advantage of it here. The irreducible, nonclassical chance and correlations without correlata, and with them mysteries without mysticism, epistemology without ontology, and probability without causality may be worth a *bet*. It might even be our *best bet*.

Chapter 11
Conclusion: "The Mere Touch of Cold Philosophy"

While aware that many and even most are inclined to bet or at least to hope otherwise, I nevertheless suggested in closing the previous chapter and, with it, my main argument in this book that the nonclassical epistemology of quantum theory may be worth a *bet* and that it might even be our *best bet*. I would like to stress this is only a bet, indeed a *possible* bet, which, accordingly, cannot be certain. First of all, apart from the fact that even quantum mechanics may be proven to be wrong by new data, the book's argument, my main justification for this suggestion, only concerns *an* interpretation of quantum phenomena and quantum mechanics, both of which, as I qualified throughout, could be and have been interpreted otherwise. The uncertainty of such bets becomes even greater once one moves beyond the scope of quantum mechanics, in view of the fact that our higher level fundamental theories are incomplete or contain unresolved problems, even in the case of quantum field theories within the purview of the standard model. Nevertheless, my bet does reflect a broader assessment on my part that, while based on my assessment of quantum mechanics, extends to higher level quantum theories and fundamental theories in physics in general, which I am inclined to think or, again, to bet will ultimately need to be quantum (there are alternative views on this point). A physical theory need not be quantum to be nonclassical, and, as noted earlier, nonclassical interpretations of classical physics and especially relativity, even special relativity, are not out of the question. In the case of relativity, such an interpretation may even be necessary, although nonclassical aspects of relativity appear to be more likely to be found in phenomena (such as those related to the behavior of photons) that are ultimately due to the quantum constitution of matter. In any event, quantum theory is my main concern here, and I would like, by way of conclusion, to sketch a further argument for the significance of nonclassical epistemology there. This argument is not definitive, although my discussion of quantum field theory is reasonably grounded and could be developed into a full-fledged argument. The latter, however, is beyond my scope here, and I shall only address the most essential aspects of quantum field

A. Plotnitsky, *Epistemology and Probability*, Fundamental Theories of Physics 161,
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theories that support my argument for nonclassical epistemology.[1] As throughout this study my argument is governed by my understanding of how the relationships among physics, mathematics, and epistemology (and hence, philosophy) work in quantum theory from Heisenberg on: the mathematics of quantum theory enables us to properly predict the outcome of quantum experiments without being able to offer a mathematized description of quantum objects and their behavior. These predictions are in general probabilistic, which fact, however, corresponds to the experimentally established character of quantum phenomena themselves. How these predictions are possible remains mysterious, but this mysteriousness is, in Bohr's words, free of any "mysticism foreign to the spirit of science" (Bohr 1949, *PWNB* 2, p. 63).

Quantum field theories, such as quantum electrodynamics, deal with quantum processes occurring at high speed and energy, in considering which the effects of *special* relativity must be taken into account. In quantum mechanics such effects can be neglected because the speed of the quantum objects considered is slow vis-à-vis the speed of light in the vacuum, *c*, and hence the energy levels of the processes considered by the theory are low. According to special relativity, *c* limits the speed of all physical processes in nature. There are dissents from this view, despite the fact that special relativity is an experimentally well-confirmed theory. As discussed earlier, quantum mechanics is *consistent* with special relativity, unlike, say, Bohmian mechanics, although, as we have seen, there is dissent on this point as well, in particular, by way of interpreting Bell's and related theorems as implying the nonlocality of quantum mechanics or nature, even though this nonlocality is never manifest in actual experiments. A quantum theory of gravity corresponding to Einstein's *general* relativity does not as yet exist. String and brane theories offer a hope for such a theory, possibly by way of unifying all currently known forces of nature within a single scheme, and there are alternative proposals as well.

Quantum field theory could be equivalently formulated either in terms of "particles" or in terms of "fields," a concept developed, classically, by Faraday and Maxwell in the nineteenth century in order to account for electromagnetism. These two concepts are parallel to the concepts of "particles" and "waves" in quantum mechanics. We recall that in developing quantum mechanics, first Heisenberg and then Born and Jordan formally adopted the equations of

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[1] I have considered quantum field theory along similar lines in more detail elsewhere (Plotnitsky 2006b, pp. 119–142), although the present discussion departs from and, on several points, corrects this earlier analysis, and has a different emphasis. The philosophy of quantum field theory is a difficult subject, still only sporadically explored in the literature. Although the technical literature is vast, one can think of barely a handful of books devoted to the philosophy of quantum field theory, as against hundreds of books and uncountable articles on the philosophy of quantum mechanics. One might mention in particular Paul Teller's *Quantum Field Theory: An Interpretive Introduction* (Teller 1995), arguably the *first* (a remarkable fact in itself) systematic philosophical book-length treatment. See also Haag (1996), Cao (1999), Kuhlmann et al. (2002), and Kuhlmann (2006), a useful encyclopedia article. For historical accounts, see also Schweber (1994) and the more technical Weinberg (2005). Feynman (1985) is arguably the best non technical book on quantum electrodynamics.

classical mechanics (for "particles"), most generally, in their Hamiltonian form, but gave them a new mathematical and physical content by using different (matrix) variables to which the equations themselves applied. Mathematically, the formalism amounted to that of the infinite-dimensional Hilbert spaces over complex numbers. Schrödinger's ("wave") formalism is mathematically the same, and one can combine both approaches with the same Hilbert-space scheme. Physically, the most crucial point was that quantum mechanics only predicts, in general probabilistically, the outcomes of quantum experiments, but does not describe quantum objects and their behaviors. In developing his quantum electrodynamics, in which both electrons and photons were considered as "particles" (idealized as dimensionless points, assigned a mass, in the case of electrons), Dirac adopted the same approach, which he also used earlier in his own version of quantum mechanics. He developed a more complicated Hamiltonian (Hilbert-space) formalism applied to the type of matrix or operator variables analogous to those used in quantum mechanics. By contrast, in their version of quantum field theory developed a bit later, Heisenberg and Pauli used the field picture. It was the first quantum *field theory* in the current sense, since, as a particle theory, Dirac's theory could be seen as a form of *mechanics*. Heisenberg and Pauli's approach was similar to that of Schrödinger, but, unlike Schrödinger, Heisenberg and Pauli suspended the physical picture of wave propagation from the outset. They used the equations analogous to Maxwell's famous equations, the (wave) equations of classical electromagnetism. The variables used in these new equations were, again, different from those used in classical electrodynamics, and, as those of quantum mechanics, only predicted the probabilities of the outcomes of experiments. Later on Heisenberg, Enrico Fermi, and Hideki Yukawa introduced a quantum field theory of nuclear forces.

These developments inaugurated quantum field theory as a theory of particles and fields, which it has remained ever since, allowing one to use either picture or variously combine both, depending on one's need or preference. The complementarity of particles and fields is sometimes invoked as well, which, however, I shall bypass here, by and large for the same reasons I have in this study avoided the wave–particle complementarity, and speak instead of two equivalent *mathematical* models, used only for *predicting* the probabilities of certain observable effects of the interactions between quantum objects and measuring instruments. Quantum field theory is conceptually different from quantum mechanics because it contains the concept of virtual formation (creation and annihilation) of particles, which introduced new complexities. (I shall here adopt the particle picture.) These differences initially emerged in the so-called second quantization, which was introduced by Jordan and which replaced a one-body or one-degree-of-freedom problem with a many-body problem, technically, the infinitely many-body or infinite-degrees-of-freedom problem. Thus, in Pais's words, "the hydrogen atom can no longer be considered to consist of just one proton and one electron. Rather it contains infinitely many particles. . . . A transition from one [energy] level to another must therefore mean that particles with energy *hv* are either made or else disappear" (Pais

1986, pp. 325, 333). This conception was eventually extended to and developed mathematically for all fundamental forces (except gravitation) and came to define the standard model.

As I have indicated earlier in this study, quantum field theory appears to be open to a nonclassical interpretation and even to contain features that are epistemologically more radical than those found in quantum mechanics and the quantum phenomena within its scope. Bohr certainly saw quantum field theory as confirming his key ideas concerning the nonclassical epistemology of quantum physics and possibly as taking us even farther along this gradient.[2] He described Dirac's quantum electrodynamics, the first quantum field theory, as "a most striking illustration of the power and fertility of the general quantum-mechanical way of description" and as reflecting "new fundamental features of atomicity" (Bohr 1949, *PWNB* 2, p. 63). The term "atomicity" is used here in Bohr's sense, correlative to that of phenomena, and thus implies that quantum field theory conforms to and, via its "new fundamental features," possibly extends the nonclassical epistemology of quantum mechanics. I shall now explain, in nontechnical ("naïve") terms, the main reason for this assessment.

Suppose that one arranges for an emission of an electron, at a given high energy, from a source and then performs a measurement at a certain distance from that source. Placing a photographic plate at this point would do. The probability of the outcome would be properly predicted by quantum electrodynamics. But what will be the outcome? The answer is not what our classical or even our quantum-mechanical intuition would expect.

Let us consider, first, what happens if we deal with a classical physical object in the same type of arrangement, say, a small ball that hits a metal plate. In classical mechanics we, again, can deal directly with the objects involved, rather than with their effects upon measuring instruments. The place of the collision could, at least in an idealized representation of the situation, be predicted exactly by classical mechanics, and as explained earlier, we can repeat the experiment with the same outcome on an identical or even the same object. By contrast, if we deal with an electron as a quantum object in the quantum-mechanical (low-energy) regime we cannot predict the place of collision exactly or exactly repeat the experiment on the same electron. Indeed, there is a nonzero probability that we will not observe such a collision at all. Quantum mechanics, however, gives us proper estimates concerning the probabilities of such events.

Once, however, the process occurs at a high energy and is governed by quantum electrodynamics, the situation is different. We can still predict, as well as in quantum mechanics, the probabilities of the outcome or rather the *outcomes* (plural), since they may be different. However, one might find, in the corresponding region, not only an electron (or nothing), as in quantum mechanics, but also other particles: a positron, a photon, an electron–positron

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[2] I shall use the nonclassical interpretation of quantum mechanics adopted in this study in my discussion from now on, and shall not qualify my statements as concerns other interpretations, while bearing in mind that they are possible.

pair. Once we move to still higher energies or different domains governed by quantum field theory a panoply of possible outcomes becomes much greater. What is referred to by the absence of a single ground state in quantum field theory is a manifestation of the situation, first discovered by Dirac via his famous relativistic equation for a free electron, introduced in 1928. The equation (although it took a few years to realize this fact) also "contained" the positron, eventually predicted by Dirac and discovered by Carl D. Anderson in 1932. Just as does quantum mechanics, quantum field theory rigorously predicts which among such events can or cannot occur, and with what probability. And as in quantum mechanics, it can only predict such probabilities or, sometimes, statistical correlations between quantum events.

Thus, in quantum field theory an investigation of a particular type of quantum object irreducibly involves not only *other particles of the same type* but also *other types of particles*, conceivably all existing types of particles. This qualification is crucial, since the identity of particles within each type is strictly maintained in quantum field theory, as it is in quantum mechanics. In either theory one cannot distinguish different particles of the same type, such as electrons. Accordingly, one can never be certain that one encounters the same electron in the experiment just described even in the quantum-mechanical situation, although the probability that it would in fact be a different electron is low in the quantum-mechanical regime in comparison to that in the regime of quantum electrodynamics. In quantum field theory, it is as if instead of identifiable moving objects and motions of the type studied in classical physics, we encounter a continuous emergence and disappearance, creation and annihilation, of particles from point to point, theoretically governed by the concept of *virtual* particle formation. The corresponding operators, that is, the operators used to predict the probability of such events, are called the creation and annihilation operators. This view takes us even beyond quantum mechanics. For, while the latter questions the applicability of such classical concepts as objects (particles or waves) and motion, and possibly all concepts, at the quantum level, it still preserves the identity of quantum objects at the level of effects upon measuring instruments.

The introduction of these operators and, with them, a new mathematical formalism (although still of a Hilbert-space type, but involving more complex Hilbert spaces) was a momentous event in the history of quantum physics, comparable to that of Heisenberg's introduction of his matrix variables. To return to Bohr's assessment of Dirac's theory, cited earlier, and prompted by these considerations: "Dirac's ingenious quantum theory of the electron offered a most striking illustration of the power and fertility of the general quantum-mechanical way of description. In the phenomena of creation and annihilation of electron pairs we have in fact to do with new fundamental features of atomicity, which are intimately connected with the non-classical aspects of quantum statistics expressed in the exclusion principle, and which have demanded a still more far-reaching renunciation of explanation in terms of a pictorial representation" (Bohr 1949, *PWNB* 2, p. 63). It is not clear how much further one can renounce "explanation in terms of a pictorial representation," once such a representation or any representation or even conception is already renounced altogether, as it is in a nonclassical interpretation of quantum mechanics. Bohr's main point, however, is that Dirac's quantum electrodynamics and, following it, quantum field theory makes a return to classical-like epistemology more unlikely. Heisenberg held similar views. Indeed, he saw Dirac's theory as an even more radical revolution than quantum mechanics was. In reflecting on the situation in the early 1970s, Heisenberg spoke of Dirac's discovery of antimatter as "perhaps the biggest change of all the big changes in physics of our century ... because it changed our whole picture of matter" (Heisenberg 1989, pp. 31–32). In other words, the theory introduced more radical forms of multiplicity, if not that of unknowability or inconceivability, into our understanding of physical phenomena, in either sense, since Bohr's concept of phenomena is as applicable here as in quantum mechanics.

Quantum field theory has made remarkable progress and has acquired a much richer content and structure since its introduction and especially in the second half of the last century, as manifest perhaps most famously in the electroweak unification and the quark model of nuclear forces. Many predictions of the theory, from quarks to electroweak bosons and the concept of asymptotic freedom, to name just a few, were spectacular, and, since its introduction, the field has garnered arguably the greatest number of Nobel Prizes in physics. It was also quantum field theory that led to string and then brane theories, the current stratosphere of theoretical physics.[3] However, the essential epistemological points just considered remain in place. I would like to illustrate this by briefly considering Feynman diagrams, arguably the most important practical tool of the present-day quantum field theory, which help physicists navigate through the difficulties (these are, again, immense) of calculations.

For example, the following diagram represents the annihilation and then the creation of an electron and a positron via a virtual photon (represented by a wavy line), with another virtual photon emitted by an electron later (Fig. 11.1).

At any point represented in this diagram, another virtual process (similar to the emission of a virtual photon γ_2 depicted here) may occur and hence another diagram may be inserted into it. Although different events are in principle possible and their possibility defines the situation, only some of them can be registered in actual experiments. Those particles that are registered by observations are considered "real particles," while those that are not are considered "virtual particles." Every virtual or actual event or transition can be represented by a Feynman diagram.

Feynman diagrams are, however, only diagrams—pictures that help us to heuristically visualize the situation. So is ultimately the "picture" of the creation and annihilation of particles or of virtual particle formation, or, again, the

[3] For an elegant and efficient (in two pages!), but highly technical, discussion of these connections, see Zee (2003, pp. 38–40). For a philosophical treatment, see Kuhlmann (2006).

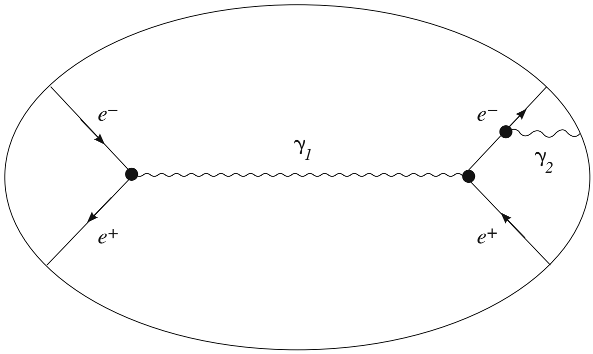

Fig. 11.1

particle "picture," in the first place. What actually happens at the level of such processes themselves we can no more know or even conceive of, let alone visualize, than we can in quantum mechanics, and as in quantum mechanics, the formalism of quantum field theory only enables us to predict the probabilities of and correlations between events. To accomplish this task in quantum field theory, however, one needs both new concepts, such as those of the creation and annihilation of particles and of the virtual particle formation, and a new mathematical machinery. As explained earlier, rigorous physical concepts contain this machinery as part of their architecture.

The new complexities of quantum field theory appear to be deeply connected to the question of measurement. We recall that in the quantum mechanical regime, in Bohr's words, "although, of course, the existence of the quantum of action is ultimately responsible for the properties of the materials of which the measuring instruments are built and on which the functioning of the recording devices depends, this circumstance is not relevant for the problem of the adequacy and completeness of the quantum-mechanical description" (Bohr 1949, *PWNB* 2, p. 51). As, however, Bohr noted already in 1937, once we move to quantum field theory, the quantum constitution of the measuring instruments might need to be taken into account. He says,

> On closer consideration, the present formulation of quantum mechanics in spite of its great fruitfulness would yet seem to be no more than a first step in the necessary generalization of the classical mode of description, justified only by the possibility of disregarding in its domain of application the atomic structure of the measuring instruments themselves in the interpretation of the results of experiment. For a correlation of still deeper laws of nature involving not only the mutual interaction of the so-called elementary constituents of matter but also the stability of their existence, this last assumption can no longer be maintained, as we must be prepared for a more comprehensive generalization of the complementary mode of description which will demand a still more radical renunciation of the usual claims of so-called visualization. (Bohr 1937, *PWNB* 4, p. 88)

Bohr's view here can be related to Pauli's argument, mentioned in Chapter 10, that in quantum mechanics, as against quantum field theory, the observer is still too "detached" or even "too completely detached" (Pauli 1994, p. 132). "The atomic structure of the measuring instruments" may make it impossible to avoid this "detachment," since this structure appears to be in principle unobservable. This assessment is supported by the view of the founders of modern (i.e., renormalized) quantum electrodynamics, in particular, Julian Schwinger and Freeman Dyson.[4] Dyson gave the situation a properly modern connection to the formalism of quantum field theory. He writes,

> We interpret the contrast between the divergent Hamiltonian formalism [which imposes the necessity of renormalization] and the finite *S*-matrix as a contrast between two pictures of the world, seen by two observers having a different choice of measuring equipment at their disposal. The first picture is of a collection of quantized fields with localizable interactions, and is seen by a fictitious observer whose apparatus has no atomic structure and whose measurements are limited in accuracy only by the existence of the fundamental constants c and h. This ["ideal"] observer is able to make with complete freedom on a sub-microscopic scale the kind of observations which Bohr and Rosenfeld employ ... in their classic discussion of the measurability of field-quantities. The second picture is of [a] collection of observable quantities (in the terminology of Heisenberg) and is the picture seen by a real observer, whose apparatus consists of atoms and elementary particles and whose measurements are limited in accuracy not only by c and h, but also by other constants such as α [the fine-structure constant] and m [the mass of the electron]. (Dyson 1949, p. 1755, cited in Schweber 1994, pp. 547–548)

In sum, the divergencies in question are due "to the fact that the Hamiltonian formalism is based upon an idealized conception of measurability," which disregards the atomic constitution of measuring instruments (Dyson 1949, p. 1755). This is quite possible, since, as we have seen, the Hamiltonian formalism was brought into quantum mechanics from classical mechanics via the correspondence principle and then transferred by Dirac into quantum electrodynamics, where the correspondence considerations of the type used by Heisenberg are no longer applicable, as Bohr suspected immediately (*PWNB* 4, p. 56). Bohr's reasons were, again, similar to those of Dyson, as Bohr's elaboration cited above suggests (Bohr 1937, *PWNB* 4, p. 88). It remains to be seen whether

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[4] The renormalization procedure is difficult technically, and its mathematical legitimacy is not as yet a fully resolved issue. Roughly speaking, the procedure might be seen as manipulating infinite integrals that are divergent and, hence, seen as mathematically illegitimate. At a certain stage of calculations, these integrals are replaced by finite integrals through artificial cut-offs that have no proper mathematical justification and are performed by putting in, by hand, experimentally obtained numbers that make these integrals finite, which removes the infinities from the final results of calculations. (See Teller, pp. 149–168.) These calculations are experimentally confirmed to a very great degree. Indeed, quantum electrodynamics is the best experimentally confirmed theory in our possession. The renormalization of quantum electrodynamics was accomplished in the 1940s by Tomonaga, Schwinger, and Feynman, which brought them a shared Nobel Prize in 1965, with some contributions by others, especially Dyson, or earlier Hans Bethe and Hendrik Kramers. The Yang–Mills theory was eventually shown to be renormalizable by Martinus Veltman and Gerardus t'Hooft in the 1970s (eventually bringing them their Nobel Prize as well). This allowed a proper development of the standard model of all forces of nature, except for gravity, which is, again, not given its quantum form as yet, although it is expected to be ultimately quantum by most physicists.

any formalism could overcome the physical and epistemological dimensions of the situation described by Dyson here. While this may not be impossible, we might need to be even luckier than we have been thus far. The ideal observer, as defined by Dyson, reflects our capacity to observe or conceptualize anything, given the technology we have, the perceptual machinery of our body and the conceptual machinery of our mind, responsible for our mathematics or our thought, to begin with. The understanding is also suggested and even implied by Bohr and Rosenfeld's discussion of measurement in quantum field theory (Bohr and Rosenfeld 1933). Indeed, since, on their and the present view, the data in question in quantum field theory is actually observed or measured as classical physical data, any actual observer is in fact a classical observer. However, and this is of course what is crucial here, this observer is now a classical observer of quantum-field-theoretical (rather than quantum-mechanical) effects of the interactions between quantum objects and measuring instruments, which are able to detect these effects. In this regime, as opposed to the quantum mechanical one, these instruments must indeed be seen, as Dyson says, as "consist[ing] of atoms and elementary particles and whose measurements are limited in accuracy not only by c and h, but also by other constants such as α [the fine-structure constant] and m [the mass of the electron]." While still enabling our predictions, which are, again, probabilistic (just as they are in quantum mechanics), the mathematical formalism of quantum field theory, arising from but moving beyond both classical and quantum mechanics, does not appear to be able to free itself from divergencies, except, thus far, by means of renormalization. Whether string and brane theories will allow us to do so and by what epistemological means (classical or nonclassical) remains to be seen.

For the moment, quantum theories appear to be well in accord with nonclassical epistemology and to give it new features. Quantum field theory is a new set of, in Bohr's words, "mathematical instruments," coupled to new measuring instruments and, as a result, new types of configurations of phenomena or "atoms" in Bohr's sense, reflected in the concept of virtual particle formation. What we predict is derived from the multiplicity of possible configurations of mathematical entities, such as symmetry groups (associated with different elementary particles, whose physical identities are beyond all knowledge and conception). The outcomes of what we measure and we predict are manifest in certain effects or phenomena observed in measuring instruments, "atomic" entities, *associated* with photons, electrons, positrons, quarks, anti-quarks, and so forth, or their combinations. Momentous as more recent developments in quantum field theory have been, experimentally and mathematically, they, I would contend, have not changed the epistemological situation just outlined, at least not in the classical direction. If anything they move us even farther along the nonclassical trajectory. In sum they have, yet again, justified Bohr's assessment that "the introduction of still further abstractions into the formalism will be required to account for the novel features revealed by the exploration of atomic processes of very high energy" (Bohr 1958, *PWNB* 3, p. 6).

As I have qualified from the outset of this study, this does not mean that the future development of quantum theory, for example, quantum gravity or further unifications (even partial ones), will necessarily conform to nonclassical epistemology. Even in the case of quantum mechanics, the spectrum of different expectations is, again, wide—from the hopes that the theory might be proven incorrect even within its proper scope to more classical views of it (or of quantum phenomena) to the possibility of more classical alternative theories of quantum phenomena. Nevertheless, there does not appear anything thus far that substantively contradicts a nonclassical view of quantum mechanics or, as the preceding discussion of quantum field theory would suggest, quantum theory in general. On the other hand, there appears to be much to support such a view. Once things become quantum, their epistemologically nonclassical character seems to show up; and although one cannot be certain either, there is thus far no physical reason to assume that it will be otherwise in the case of string or brane theories, quantum gravity, or quantum cosmology.

First of all, although there have been some attempts to argue that they can help the case for ontological or realist views of fundamental physics, such concepts as strings or branes can hardly be considered rigorously as anything other than mathematical abstractions, analogous to particles as represented by dimensionless points in the standard quantum mechanics or quantum field theory. As things stand now, such objects cannot correspond to the actual objects in nature, on the one hand, and yet, on the other, cannot be rigorously considered as anything else in theoretical terms, as became clear in the case of the electron even before quantum theory. Were the electron to have any spatial volume, it would be torn apart by the negative electric forces of its charge, and yet we know that it is stable. To return to Bohr's defining statement in the Como lecture (which, on this point, did define Bohr's view throughout, including his nonclassical view), "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 . . . indispensable for a description of experience in connection with our ordinary space-time view" (Bohr 1927, *PWNB* 1, pp. 56–57; emphasis added). Thus far at least, the same applies to strings and branes. Indeed, these objects correspond to the scales so far removed from those of our measuring instruments that no interaction between them is possible, and whatever effects strings or branes, assuming they exist, might have could only be highly indirect, mediated by many more strata than effects generated by quantum objects, as considered in quantum mechanics and quantum field theories. Of course, as I said, such objects and processes, for example and in particular those that took place at the earliest stages of the Universe, apparently defined by them, can have classically observed macro and indeed cosmic effects on an enormous scale, such as the distribution of matter in the Universe.

As suggested earlier, technically these abstractions are not altogether indispensable, albeit useful and, given the history of physics and human nature, difficult to avoid. In principle, however, we can consider, say, the double-slit experiment and develop the mathematics for predicting the outcomes correctly. This is of course not how it happened or, given the preceding history of physics, classical and quantum, could have happened, but an alternative, more mathematically defined, way of doing or even discovering new quantum theories is not in principle inconceivable.

Let us imagine that the data in question in quantum mechanics, for example, as manifest (including quantitatively) in the double-slit experiment, were given to a mathematician on the cutting edge of mathematics at the time, say, a doctoral student of Hilbert, somebody like von Neumann or Amy Noether, but unfamiliar with quantum or even classical physics. This mathematician would then have been asked to develop a mathematics that would enable one to *predict* these data. This would of course have been a formidable problem, especially in the absence of physics, although, as things actually happened, physics (as it existed then) was almost more inhibiting than helpful. The main difficulty would have been of course the existence of two mutually exclusive patterns depending on the corresponding set-up, since either distribution by itself would not have been difficult to handle. The mathematician needed not to think in terms of describing mathematically some moving classical-like objects, particles, that hit the screen, but only in terms of two different patterns that are produced. The mathematician then only needs to find the mathematics that predicts them in terms of two different probability distributions. (There are no waves either.) The mathematician would have had to have made two extraordinary guesses—one truly extraordinary and, with the first guess in hand, the other somewhat less so. The first guess is that one needs a Hilbert space over complex numbers, which is difficult, but for a doctoral student of Hilbert it would not have been impossible. The second guess would have been Born's rule, and while not easy, it would be almost natural because probabilities are real numbers, and when moving from complex to real numbers, the moduli of complex numbers is the most obvious way to do so. One would of course need a square moduli, but that could be figured out by trial and error, and Born's first guess was just moduli, too. Indeed, Hilbert's students could have thought of von Neumann's projection postulate as well. Finally, Planck's constant could have been established from the data as well, at least in principle.

This fable is not that far from how Heisenberg made his discovery of quantum mechanics, since to a large degree he had to suspend, to "forget," classical physics or the old quantum theory to arrive at his new mathematical scheme. His physics was defined by finding this predictive mathematics from the available data. Of course he had to reinvent the wheel of (in effect) Hilbert space formalism, and in his case, the already available equations of classical mechanics, equations of motion, and the correspondence principle were exceptionally helpful. Still, his process was deeply mathematical in this sense, and the crucial point was, again, that while he adopted the form of classical equations, he did not use them as equations of *motion*, and he guessed the new variables that were necessary to predict the data. This is in part why he made his statement, cited in the Introduction: “It is very difficult to modify our language so that it will be able to describe these atomic processes, for words can only describe things of which we can form mental pictures, and this ability, too, is a result of daily experience. Fortunately, mathematics is not subject to this limitation, and it has been possible to invent a mathematical scheme—the quantum theory [e.g., quantum mechanics]—which seems entirely adequate for the treatment of atomic processes” (Heisenberg 1930, p. 11). Indeed, it may not be the only such a scheme.

In other words, “particles” are not only phenomenological but also and, in the context of quantum theory, primarily mathematical abstractions or essentially linked to mathematical abstractions, such as Hilbert spaces or symmetry groups, a point that shaped Heisenberg’s view throughout his life, as his late essays, such as “What Is an Elementary Particle?,” especially indicate, again, in the context of the quantum field–theoretical approach to the elementary particle theory (Heisenberg 1989, pp. 71–88). “Strings” or “branes” are no different in this respect, which is of course not to say that such new abstractions, (heuristically) phenomenal or (substantively) mathematical, are not helpful or indeed necessary. Quite the contrary, since they enable new mathematical machineries which we need to account for physical phenomena that physics needs to account for, from micro quantum phenomena (there is still plenty of work to be done there, even apart from gravity) to the early Universe and cosmology (dark energy in particular). The point here is the same as the one made by Bohr concerning Dirac’s theory, which introduced new mathematical abstractions into quantum theory in order to establish quantum field theory. String and brane theories suggest remarkable possibilities in this regard, thus far mostly along mathematical lines, but with potentially crucial physical consequences. There is some debate concerning this potential, including vis-à-vis other available alternatives, some of which have a more realist flavor (such as the loop quantum gravity), but this debate does not affect my argument at the moment. In the present view, the mathematical abstractions of quantum theory describe only themselves. But they enable us to make proper predictions, albeit in quantum theory only probabilistic ones (the nature of our abstractions, such as the use of complex numbers, reflects this fact), concerning the outcomes of relevant experiments. This is, again, not the kind of approach that would appeal to Einstein or Schrödinger, and one might be sympathetic to their view that physics should offer more. The question is, again, whether nature, including our nature as human animals, allows physics to achieve more in the quantum domain.

I add “our nature,” because, as I suggested from the outset of this study, classical thinking may reflect the essential workings of our neurological machinery born with our evolutionary emergence as human animals and, in part, enabling our survival.[5] In other words, as the product of this machinery, our *thinking qua thinking* is classical, even in non-nonclassical situations, since the entities, such as quantum objects, that make a theory considering them nonclassical are, by definition, beyond the capacity of our thought and are ultimately unthinkable even as unthinkable. We are only compelled to infer the existence of such entities from certain configurations of their effects upon what we can think and know, inevitably through classical theoretical thinking: This inference is a product of our thought which takes us beyond our thought. These objects exist and these configurations arise (due to these objects, our nature as particular biological and neurological, thinking, creatures, and our technology), because, as Bohr and Heisenberg argue, it is not clear why nature at the ultimate level of its constitution should conform to our intuition, concepts, or language. These were, to return to Heisenberg's assessment, "invented to describe the experiences of daily life, and these consist only of processes involving exceedingly large numbers of atoms" (Heisenberg 1930, p. 11). One might add that they are also products of a long preceding evolutionary and then cultural history, where our survival and success depended on them and created them in very different settings than those of nature at the ultimate micro, or macro, level of its constitution. It might, accordingly, be stranger if nature, at so vastly different scales (micro and macro), did conform to them than if it did not.

"The book of nature" and "reading the book of nature" are among the most persistent tropes of the intellectual history of modernity. For modern physics, it is a book written in the language of mathematics or, more accurately, containing a philosophy written in the language of mathematics, as Galileo famously said, speaking of the characters of this language as "*geometrical figures*," and thus setting in motion the project of modern physics as a mathematical science of nature (Galileo 1966, pp. 183–184; emphasis added). This writing may, however, be ours rather than that of nature (except, again, insofar as we are nature, too), as quantum theory and its *algebraic* language, beginning with Heisenberg's discovery of both, tell us in a new way. Indeed, quantum phenomena may be telling us that nature may not allow us to "write" its ultimate constitution in the language of mathematics or possibly in any language, or even to conceive of this constitution by any means that are or will ever be available to us. Nature does, however, allow us to *write*, to *create* something new by way of experimenting with it in quantum physics. It allows us to create quantum physics—its experimentation, its physics, its mathematics, its philosophy, through which

[5] I might note in passing that this argument is not related to the so-called "anthropic principle." The latter, roughly, states that our theories, especially our cosmological theories, must take into account not only the existence of life but also the existence of human life and the fact of human intelligence, which implies certain constraints upon our theories. The principle can be given further specificity and has indeed functioned in a number of different ways. See (Susskind 2006) for a useful discussion in the context of his theory of the cosmic landscape.

our thought reveals itself as capable of *constructing* the concept of nature as that which is *un*-constructible. How close to nature, to the *nature* of nature, this concept brings us is difficult to say, but it tells us something new about it. Mathematics remains as indispensable for quantum physics as it has ever been for physics: just as classical physics, physics is still physics because it is both experimental and mathematical. Indeed, it is more indispensable than ever because now our staging of experiments *defines*, rather than merely follows, what happens and because mathematics, which, as Heisenberg said, is not subject to the limitations of classical language and mental pictures, enables us to predict, even if only probabilistically, what will happen in these experiments. We might be lucky that nature allows our mathematics to do so by linking randomness and probability (rather than leaving our encounter with quantum events to absolute chance), even if without allowing us to know or even imagine how this is possible.

I would like to close with John Keats, who wrote in *Lamia*:

> . . . Do not all charms fly
> At the mere touch of cold philosophy?
> There was an awful rainbow once in heaven:
> We know her woof, her texture; she is given
> In the dull catalogue of common things.
> Philosophy will clip an Angel's wings,
> Conquer all mysteries by rule and line,
> Empty the haunted air, and gnomed mine—
> Unweave a rainbow . . .
> (*Lamia*, Part II, ll.229–238)

Here, since Keats has both Descartes and Newton in mind, philosophy is also natural philosophy or what we now call science, specifically physics. Keats may be right about fleeing charms. The remainder of the passage, however, has proven to be, at least for now, among the least prophetic of Keats's insights. Keats, were he alive, would have delighted in this failure, even though he might not want "cold philosophy" to ever arrive, however slowly, where poetry gets so quickly and with so much charm on its wings, "wings of Poesy," invoked by Keats in *Ode to a Nightingale* (l. 33). The ultimate unweaving of the rainbow does not appear to be possible. The rainbow is ultimately un-unweavable. This is what physics and its cold philosophy appear to have revealed to us as they arrived at relativity and quantum physics, the ultimate theory (at least for now) of light. Light cannot ultimately be catalogued in terms of common or any other things, that is, reduced to the known, simple constituents, for example, of the kind that the Democritean atomism (or classical optics, linear or wave) would envision. As we have seen, if one wants to use this language and one might, with Schrödinger, quantum mechanics compels us to speak of a very different, and hardly common, type of catalogues, catalogues of our expectations concerning future measurements, provided by Schrödinger's wave function. Accordingly, the ultimate nature of light cannot be *envisioned*. Its mystery, but now mystery without mysticism, cannot be conquered by rule or line, the great instruments of cold philosophy, of its algebra and geometry.

Cold philosophy may, even after quantum theory, still be reluctant to heed Hamlet's advice to Horatio upon the appearance of his father's ghost in Shakespeare's play: "This is wondrous strange!/ And therefore as a stranger give it welcome./ There are more things in heaven and earth, Horatio/ Than are dreamt of in your philosophy" (*Hamlet*, I, V, ll. 165–168). It is, however, the responsibility of "cold philosophy," part of its commitment to the maximal possible rigor, to *confront* the unknowable and the unthinkable, and, as quantum mechanics taught us, even to make them part of our thought and knowledge. Doing so need not prevent but might instead enhance thought and knowledge, and allow us to use our "rules" and "lines," our algebras and geometries, and to invent, dream up, new ones. Whatever our algebras and geometries might create in this confrontation might still only be a dream, yet another dream of cold philosophy. In the same poem, also a poem about dreams, Keats says, "Real are the dreams of Gods" (*Lamia*, Part I, l. 126). But we are not gods, and our dreams, at least our scientific, philosophical, and poetic dreams, are rarely, if ever, lucky enough to be real. On the other hand, as quantum theory tells us, some of these dreams may be, and are strange enough to be, more real than we think or expect reality to be. In any event, in science, philosophy, and poetry alike, we can only bet on our dreams. But then, on which dreams? It may not always be a matter of choice either.

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Note: Due to the frequency of their occurrence in the book the names of Bohr, N.; Einstein, A.; Heisenberg, W.; and Schrödinger, E. are not indexed. Their key ideas and contributions are indexed in the Subject Index.

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