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The Nature of Quantum Reality and the Spirit of Copenhagen

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I am working out a quantum theory about it for it is really most tantalizing state of affairs.

—James Joyce, *Finnegans Wake*
(Joyce 2012, p. 149)

Preface

I would like to begin with the endpoint of the history to be traversed by this study, the discovery of the Higgs boson, arguably the greatest event of fundamental physics in the twenty-first century thus far, and, thus far, a culminating event in the history of quantum physics. This discovery has been discussed at all levels and in all media, with photographs of the "events" testifying to the existence of the Higgs boson and of various components, staggering in their complexity, of the Large Hadron Collider (LHC), and the relevant parts of the mathematical formalism of quantum field theory (e.g., "The Higgs Boson," *Wikipedia*; CERN: Accelerated Science: Images). These pictures are well known and easily located on the Web. I only cite the key part of the formalism, the epistemological nature of which will be discussed in Chap. 6:

> In the Standard Model, the Higgs field is a four component scalar field that forms a complex doublet of the weak isospin SU(2) symmetry:
>
> $$\phi = \frac{1}{\sqrt{2}} \begin{pmatrix} \phi^1 + i\phi^2 \\ \phi^0 + i\phi^3 \end{pmatrix}$$
>
> while the field had charge +1/2 under the weak hypercharge U(1) symmetry (in the convention where the electric charge, Q, the weak isospin, I_3, and the weak hypercharge, Y, are related by $Q = I_3 + Y$).
>
> The Higgs part of the Lagrangian is
>
> $$\mathcal{L}_H = \left| \left(\partial_\mu - igW_\mu^\alpha \tau^\alpha - i\frac{g'}{2} B_\mu \right) \right|^2 + \mu^2 \phi^\dagger \phi - \lambda \left(\phi^\dagger \phi \right)^2,$$
>
> where W_μ^α and B_μ are the gauge bosons of the SU(2) and U(1) symmetries, and g and g' their respective coupling constant, $\tau^\alpha = \sigma^\alpha / 2$ (where σ^α are the Pauli matrices) a complete set of generators of the SU(2) symmetry, and $\lambda > 0$ and $\mu^2 > 0$, so that the ground state breaks the SU(2) symmetry. The ground state of the Higgs field (the bottom of the potential)

is degenerate with different ground states related to each other by an SU(2) gauge transformation. It is always possible to pick up a gauge such that the ground state $\phi^1 = \phi^2 = \phi^3 = 0$. The expectation value of ϕ^0 in the ground state (the vacuum expectation value or vev) is then $\phi^0 = \frac{v}{\sqrt{2}}$, where $v = \frac{|\mu|}{\sqrt{\lambda}}$. The measured value of this parameter is $\sim \frac{246\text{GeV}}{c^2}$. It has units of mass, and is the only free parameter of the Standard Model that is not a dimensionless number. Quadratic terms W_μ and B_μ arise, which give masses to the W and Z bosons:

$$M_W = \frac{v|g|}{2}$$

$$M_Z = \frac{\sqrt{g^2 + g'^2}}{2}$$

with their ration determining the Weinberg angle, $\cos\theta_W = \frac{M_W}{M_Z} = \frac{|g|}{\sqrt{g^2 + g'^2}}$, and leave a massless U(1) photon, γ.

("The Higgs Boson," *Wikipedia;* Peskin and Schroeder 1995, pp. 690–700)

Now, what does all this (the photographs of the corresponding events, computer generated images and data, staggering machinery of the LHC, and the mathematics just described) mean? And how is it possible? Without attempting to definitively answer these questions, this study will consider a particular perspective on them, indeed a particular way of asking them, and will suggest partial answers that arise if one adopts this perspective. This perspective is guided by understanding the nature of quantum reality, or the quantum reality of nature, and of quantum theory, from quantum mechanics to quantum field theory, in "the spirit of Copenhagen [*Kopenhagener Geist der Quantenheorie*]," in Heisenberg's memorable phrase, the spirit that guides this study, as indicated by its subtitle (Heisenberg 1930, p. iv). This understanding relates nature and spirit (a relation that we seem unable to do without even when a materialist view of the world is adopted) in a new way. The spirit of Copenhagen, I argue, is defined by three great divorces from the preceding understanding of these relationships between nature and spirit, or, to use a less theologically charged expression, nature and mind (technically, German *Geist* means both), specifically scientific thought in modern physics: reality from realism, probability from causality, and locality from relativity. It is true that the last of these divorces did not shape the rise of the spirit of Copenhagen in the way the first two did, but it became a major part of this spirit nevertheless.

I shall comment on these three "divorces" and define the corresponding concepts below, and discuss them in detail in Chap. 1 and elsewhere in this study. For the moment, the spirit of Copenhagen has its history in the preceding understanding of nature and mind, and their relationships. This history extends even as far as the pre-Socratics, and I shall address some of these more distant historical connections later in this study. However, the most significant historical trajectory of this study prior to the birth of quantum theory, inaugurated by Planck's discovery of the quantum of action, h, begins with scientific modernity. There is no modernity other than scientific, because modernity is defined, partially but decisively, by the rise of modern, mathematical-experimental, sciences of nature.

Consider John Milton's description of chaos in *Paradise Lost*. This description and Milton's poem itself were written in the aftermath of the rise of mathematical-experimental science, at that stage physics and astronomy, with Copernicus, Kepler, and Galileo; and the poem was a response to a different world that emerged with and because of this rise, a world to which we now refer as the world of modernity. Milton's stated aim in writing the poem is "to justify the ways of God to man," with "justify" referring to both the nature and the justness of these ways (Milton 2004, p. 3, *Paradise Lost*, Book I, ll. 25–26). But why would one have needed such a book? Don't we already have the Bible that should do so? Well, not exactly, or rather the Bible, Milton realized, was no longer sufficient to do so. As is clear from Milton's references in *Paradise Lost* to post-Copernican astronomy, and Galileo's and Boyle's physics, Milton acutely realized that the world he lived in, the world of modernity, was defined by, in Galileo's words, "new mathematical sciences of nature," which brought mathematics and experiment together (Galilei 1991). "Modern science," M. Heidegger says, "is experimental because of its mathematical project" (Heidegger 1967, p. 93). The world, as envisioned by Milton, was post-Copernican and post-Galilean. R. Boyle has already conducted his famous experiments on the properties of air and the existence of the vacuum, and Newton, Milton's equally famous fellow Cambridge graduate, was soon to appear on this stage and to shape the thinking of modernity even more decisively. (Both Boyle and Newton had major alchemical and theological interests, and left voluminous writings on these subjects.) It was no longer the world of the Bible, and Milton reread or re-envisioned the Bible as consistent with this new world. For Milton, God created the world as understood by modern science and (they are, again, inseparable) scientific modernity. This world called for a new justification of the ways of God to man, assuming that this justification is possible, given that this new world compelled some to deny this possibility, or the existence of God in the first place. The question of this justification or its possibility, which is still with us, is well outside the scope of this study. But Milton's argument for the extraordinary complexity of this world, which, however it came about, requires the utmost reach of and may ultimately be beyond human thought, is relevant to this project. This complexity and this relevance are shown, for example and in particular, by Milton's description of chaos in the poem:

> Before their eyes in sudden view appear
> The secrets of the hoary Deep—a dark
> Illimitable Ocean without bound,
> Without dimension: where length, breadth, and height,
> And time, and place, are lost; where eldest Night
> And Chaos, ancestors of Nature, hold
> Eternal anarchy, amidst the noise
> Of endless wars, and by confusion stand.
> For Hot, Cold, Moist, and Dry, four champions fierce,
> Strive here for maistrie, and to battle bring
> Their embryon atoms: they around the flag
> Of each his faction, in their several clans,
> Light-armed or heavy, sharp, smooth, swift or slow,

> Swarm populous, unnumbered as the sands
> Of Barca or Cyrene's torrid soil,
> Levied to side with warring winds, and poise
> Their lighter wings. To whom these most adhere,
> He rules a moment: Chaos umpire sits,
> And by decision more embroils the fray
> By which he reigns: next him, high arbiter,
> Chance governs all. Into this wild Abyss,
> The womb of Nature, and perhaps her grave,
> Of neither Sea, nor Shore, not Air, nor Fire,
> But all these in their pregnant causes mixed
> Confus'dly, and which thus must ever fight,
> Unless th' Almighty Maker them ordain
> His dark materials to create more worlds—
>
> (Milton 2004, p. 20, Paradise Lost, Book II, 890–916)

The physical universe in this view is, thus, chaos, unless order emerges from it, and this happens continuously, too, even if, generally, without giving this order stability. Milton's description is presciently close to the understanding of the ultimate constitution of nature arising from quantum theory, arguably more so than Lucretius's atomism in *De Rerum Natura* (Lucretius 2009), commonly claimed to be the main precursor of modern atomic theory (along with Leucippus and Democritus, and then Epicurus, on whose ideas Lucretius relies) and one of Milton's sources. Boyle's experiments were undoubtedly on Milton's mind as well. Milton's conception does not quite reach the radical form of this understanding to be advocated in this book. Both randomness and chance, and the birth and disappearance of "particles" in chaos, and thus unstable, fleeting nature of any order that might emerge in and from it (unless some power manages to be stabilized and built on this order), are all part of this book's view of nature at the ultimate (quantum) level of its constitution. The second aspect just mentioned is specifically found in high-energy regimes and reflects or is reflected in the concept of virtual particle formation in quantum field theory, according to which the unstable, fleeting forms of order emerge from and disappear back into the foaming bubbling of chaos. This is what J. A. Wheeler refers to as "quantum foam" (Wheeler and Ford 2000, pp. 245–263). However, according to this study's view, the ultimate character of this constitution, of Milton's "embryon atoms," which we now refer to as "elementary particles" (still an unsettled concept in fundamental physics, as is its companion concept, that of quantum field), is "dark" beyond the reach of our understanding or possibly even any conception we can form. This view is closer to, but still ultimately transcends, the ancient Greek sense of chaos as *areton* or *alogon*, as that which is beyond all comprehension, than to Milton's conception of chaos here. On the other hand, Milton does appear to imply that our ways of experiencing the world and conceptions we could form of it, such as space, time, and causality (Kant's three great a priori givens of our thought), are "lost," that is, no longer applicable to chaos. Milton was certainly aware of the ancient Greek's idea of chaos as *areton* or *alogon*. So perhaps he was closer to the argument of this book on this point, except for the ultimately theological nature of his thinking. This book is concerned with the unrepresentable and possibly unthinkable "dark materials" of nature as they appear in quantum physics, placed outside or even assumed to be incompatible with theology. I prefer to leave theology to Milton. If anything, this study's understanding of the physical world, also because our interaction with it is governed by probabilistic thinking, is closer to the world of Shakespeare's plays (often invoked by Wheeler [e.g., Wheeler 1983, p. 204]), which tend to put the theological aside. They leave it to us "to take arms against a sea of troubles," a *sea*, a place governed by chance and probability (Shakespeare 2005, p. 700, *Hamlet*, III.2.55-87). The sea is often invoked by Shakespeare as such a place, and Wheeler's reference just mentioned is *The Tempest* (Shakespeare 2005, p. 1238, Act IV.1, 148–158). As Nestor says in *Troilus and Cressida*:

> … In the reproof of chance
> Lies the true proof of men. The sea being smooth,
> How many shallow bauble-boats dare sail
> Upon her patient breast, making their way
> With those of nobler bulk!
> But let the ruffian Boreas once enrage
> The gentle Thetis, and anon behold
> The strong-ribbed bark through liquid mountains cut,
> Bounding between the two most elements
> Like Perseus's horse. Where's then the saucy boat
> Whose weak untimbered sides but even now
> Co-rivalled greatness? Either to harbor fled,
> Or made a toast for Neptune. Even so
> Doth valor's show and valor's worth divide
> In storms of fortune. For in her ray and brightness
> The herd has more annoyance by the breeze
> Than by the tiger; but when the splitting wind
> Makes flexible the knees of knotted oaks,
> And flies fled under shade, why, then the thing of courage,
> As rous'd with rage, with rage does sympathize,
> And with an accent tun'd in selfsame key
> Retorts to chiding fortune.
> (Shakespeare 2005, p. 749, Act I.iii.33–54).

Shakespeare's music is the music of the sea, the music of chance and its complex harmonies, mixing chaos and order—chaosmic harmonies, as they were called by James Joyce, from whose *Finnegans Wake* M. Gell-Mann famously borrowed the term "quark" (Joyce 2012, p. 118). These chaosmic harmonies are opposed to the music of the spheres, that of Pythagoras or that of Kepler, another contemporary of Shakespeare. As my epigraph suggests, however, Joyce's masterpiece was in turn influenced by quantum theory, not inconceivably by the discovery of antimatter, which was widely discussed at the time, just as the Higgs boson or black holes are now, and was known to Joyce (Joyce 2012, pp. 383, 149). In Joyce's novel words transform into each other just as particles do in high-energy quantum physics. The appearance of Thetis in Shakespeare's passage is not by chance, and she is mentioned, again, in the play: Thetis is the mother of Achilles, the greatest of heroes. It is the rage of Achilles and his concern for the lack of virtue where *The Iliad* of Homer begins. While the chance to kill Achilles is small, it is bound to happen at some point with probability 100 %, but it is difficult to predict when, although the Trojan War increased this probability. Achilles is an important character in Shakespeare's play, where, great hero as he is, he is portrayed far less than heroically. He kills Hector by violating all possible rules of fair play, and by taking advantage of Hector's following these rules and sparing Achilles's life a bit earlier. Hector took a chance on this, but Achilles was not about to take any chances with Hector, by giving Hector a chance. Games of chance and probability are quite complex in Shakespeare's plays.

Dark beyond all thought as nature's "dark materials" are, they allow nature to create new and stable, including highly stable, forms of organization, which may be dynamic. They also allow us, by experimenting with nature, to create new *configurations* of experimental technology and even of nature itself, and through them, enable us to develop new understandings of nature and of our interactions with it. It is true that these interactions are ultimately nature as well, but they are specific to us. Of course, only nature, at least thus far, could create new worlds on the ultimate scale, new Universes, or even on smaller scales, like new stars and planets, apart from science fiction, fond of giving humans such capacity in a distant future. It is prudent to leave God aside or, again, to leave God to Milton. It is certainly more than merely prudent not to assume a god-like role in our scientific experimentation with nature in physics or elsewhere, in biology, for example. This is one of many lessons of twentieth-century physics, or of all modern science throughout its history, from Galileo on, which reminds us that the philosophy of physics is sometimes also a moral philosophy. Our experimentation, however, need not depend on and be measured by assuming such a role, given that the creation in question from the dark materials of nature is ongoing on local scales as well, including the minutest scales in question in quantum physics. The commitment itself to creative experimentation may well be imperative, or, in the language of (Kant's) moral philosophy, be the *categorical imperative* of all good science. That is, our aim in our pursuit of mathematics and science, no less than of philosophy and art, should be that of creative experimentation, in the service of the discovery of new features and principles in the workings of nature in our interaction with it, and of thought itself, from which, in particular mathematical thought, such principles cannot be separated. This is a point on which all forms of physics (classical, relativistic, or quantum) converge: creative experimentation, physical, mathematical, and sometimes philosophical, is the categorical imperative and the primary force of the *causality* of both, whatever the nature of this causality may be, a difficult problem in its own right, as it is in physics. However, quantum physics, this study argues, introduces new fundamental features and principles of this experimentation, as against classical physics and relativity, although relativity has already done so vis-à-vis classical physics, even if it did not depart from classical physics as radically as quantum mechanics did.

This book's title, "the principles of quantum theory," alludes to those of Werner Heisenberg's *The Physical Principles of the Quantum Theory* (Heisenberg 1930), based on his 1929 lectures given at the University of Chicago, and Paul Dirac's *The Principles of Quantum Mechanics* (Dirac 1930), both published in the same year. It also alludes, more obliquely and by way of a *partial* contrast between "principles" and "foundations," to the title of John von Neumann's *Mathematical Foundations of*

Quantum Mechanics, originally published in 1932 (Von Neumann 1932), in part as a response to Dirac's book. I shall explain the nature of this contrast below, indicating for now that it is partial, because the mathematical aspects or the specific mathematical character of quantum mechanics or quantum field theory is crucial to all three books and to this study. Von Neumann's response to Dirac (to which von Neumann's book was of course not limited) was motivated primarily by von Neumann's aim of establishing quantum-mechanical formalism as fully legitimate mathematically, vis-à-vis that of Dirac's version. While recognized for its great lucidity and formal generality, Dirac's formalism was not considered mathematically rigorous at the time. This was because of Dirac's reliance on his famous delta function, which was not a mathematically legitimate object then. Although this was to change, because the delta function was given a mathematical legitimacy by means of the so-called distribution theory later on, von Neumann's version of the formalism became standard and has remained standard ever since. Along with H. Weyl's 1928 *Theory of Groups and Quantum Mechanics*, translated into English in 1931 (Weyl 1931), and N. Bohr's (more philosophical) 1931 *Atomic Theory and the Description of Nature* (Bohr 1987, v. 1), Heisenberg's and Dirac's were the most important early books on quantum mechanics. The significance of these books has been momentous. They have had and continue to have a strong impact on our thinking concerning quantum theory. The impact of Bohr's essays assembled in *Atomic Theory and the Description of Nature* and his subsequent communications on the subject has been more indirect and more often than not defined by resistance to his ideas. These circumstances, however, have not diminished this impact itself, amplified by that of Bohr's confrontation with Einstein, which has overshadowed the history of the debate concerning quantum mechanics.

Von Neumann's *mathematical foundations* are not the same as Heisenberg's or Dirac's *physical principles*, and not only because of the difference between the mathematical nature of the former and the physical nature of the latter, important as this difference, on which I shall comment presently, may be. Von Neumann's foundations have physical dimensions or, in any event, are essentially related to physics, and Heisenberg's and even more so Dirac's principle thinking is fundamentally mathematical, albeit their work was not quite *mathematics* in its disciplinary sense in the way most (but not all) of von Neumann's book was, and this difference was reflected in their books. (Von Neumann was a mathematician.) Dirac's title, for one thing, says "principles," and the principles that ground his book are both, and often jointly, physical and mathematical. This is true for Heisenberg's book as well, as is testified to by his appendix (which was not part of the Chicago lectures, on which the book was based, but is nearly half of the book) "The Mathematical Apparatus of the Quantum Theory" (Heisenberg 1930, pp. 105–183). This title notwithstanding, the appendix is as much physical as it is mathematical, and there is plenty of mathematics in the main text as well. Still, von Neumann's primary aim was to give, to the maximal degree possible, a mathematical rigor and legitimacy to the formalism of quantum mechanics in its standard version, which was not a primary concern, or at least not an imperative, for Heisenberg or Dirac. According to Heisenberg, "the deduction of the fundamental equation of quantum mechanics" could not be seen as "a deduction in the mathematical sense of the word, since the equations to be obtained form themselves the postulates of the theory. Although made highly plausible [by mathematical considerations], their ultimate justification lies in the agreement of their predictions with the experiment" (Heisenberg 1930, p. 108). Bohr's *Atomic Theory and the Description of Nature* was a collection of previously published essays on complementarity, his central and most famous concept, grounding his interpretation, under the same name, of quantum phenomena and quantum mechanics, an interpretation considered, along with the concept of complementarity, in Chap. 3. As will be discussed there, even this early volume already presented more than one such interpretation, and Bohr's interpretation was to undergo yet further revisions subsequently. It is important that, in each case, it was an interpretation of both quantum mechanics and quantum phenomena, and thus also of quantum objects, which are rigorously distinguished from quantum phenomena in Bohr's interpretation. Thus, Bohr's interpretation would, at least in most of its aspects, hold for quantum phenomena, even if theories other than quantum mechanics were used to predict the data associated with quantum phenomena, although such a theory would of course have to allow for this interpretation. However, for the sake of convenience the term "interpretation," when applied to quantum mechanics or other forms of quantum theory (such as quantum field theory), as in Bohr's interpretation, will generally refer to interpretations of both quantum phenomena or, again, quantum objects, and the corresponding quantum theory, qualifying when necessary.

More generally, "foundations" and "principles" are, at least as they are defined in this study, different categories of thought. Both are important for our understanding of *fundamenta*l physics (which is yet another separate category) and for the argument of this book, even though its makes principles its main focus. This study deals with *foundational* thinking, most especially *principle* thinking (which is a form of foundational thinking), in *fundamental* physics. As other concepts considered here, these concepts may be understood differently, sometimes by reversing this pairing of foundational with thinking and fundamental with physics. Indeed, I shall, along with *foundational* concepts and theories, also speak of *fundamental* concepts and theories, because they belong to fundamental physics, although these concepts are also part of foundational thinking in fundamental physics and define this thinking. By "fundamental physics" I mean those areas of experimental and theoretical physics that are concerned with the ultimate constitution of nature, as we, as human beings, understand this constitution. I qualify because, in the view adopted by this study, this constitution could only be something conceived by the human mind or something assumed (by a human mind) to be beyond human conception. By "foundational" thinking or theories, I mean thinking or theories that concern fundamental physics, for example, the nature of space and time in relativity or the nature of elementary particles, as the ultimate material constituents of matter, in quantum mechanics. Thus, while "fundamental" refers, ontologically, to how *nature* is ultimately constituted, "foundational" refers, phenomenologically and epistemologically, to our *thinking* concerning the *fundamental* constitution of nature. It follows that our view of what is fundamental is unavoidably, even if often implicitly, defined by our foundational thinking. "Principle thinking" is foundational as well ("foundations" is a more general category). It is defined, in Einstein's terms (which I shall follow in this study), as grounded in "empirically discovered … general characteristics of natural processes, *principles* that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy" (Einstein 1919, p. 228; emphasis added).

To preliminarily illustrate the concept of principle and principle thinking, I would like to consider one of the earliest examples of the use of a principle in modern physics, Pierre de Fermat's "principle of least time," eventually developed into the principle of least action. A number of figures were involved in formulating the latter principle, beginning with G. Leibniz and P. L. Maupertuis (the priority has been disputed), and then L. Euler, J.-L. Lagrange, and Sir Rowan Hamilton, who gave the principle its rigorous mathematical form in classical mechanics, although the principle proved to be more general, extending to relativity and quantum theory. Fermat used the principle of least time to explain the so-called Snell law describing the refraction of light passing through a slab of glass. The principle was not necessary or even especially helpful at the time, because one could more easily use the Snell law in doing calculations in specific cases, while one would ultimately need the calculus of variations to do so from Fermat's principle and to give it a proper mathematical expression. However, Fermat's principle defined *how nature works*, and as such, it was profound and far reaching, especially once it was developed into the principle of least action. The subsequent history of physics has demonstrated the profundity and power of this principle on many occasions, including in relativity and quantum theory. It played key roles in D. Hilbert's derivation of Einstein's equations of general relativity (the Einstein-Hilbert action), E. Noether's proof of her celebrated theorems relating symmetry and conservation laws (equally at work in classical physics, relativity, and quantum theory), Schrödinger's derivation of his wave equation, and R. Feynman's path-integral formulation of quantum mechanics.

As will be seen, principles, in the sense of this study, are different from axioms and postulates, although a principle may involve postulates, as Fermat's principle of least time or the principle of least action does. Axioms and postulates (the latter concept is, as I shall explain, generally more suitable in physics) are also found in quantum foundations apart from principles, for example, "the quantum postulate" introduced by Bohr in 1927, as part of his interpretation of quantum phenomena and quantum mechanics in terms of complementarity (Bohr 1987, v. 1, p. 53). This postulate should not to be confused with Bohr's quantum postulates (plural) used in his 1913 atomic theory in connections with the discontinuous transitions (quantum jumps) of electrons from one energy level to another, ultimately subsumed, but also reinterpreted, by his 1927 quantum postulate. The latter refers to the fundamentally discrete and strictly individual nature of all observable quantum phenomena, defined strictly by what is observed in measuring instruments under the impact of quantum objects, and to be distinguished from quantum objects themselves. Planck's constant, h, *reflects* this discreteness and individuality, but does not necessarily represent them. In Bohr's view, it does not; instead h "symbolizes" them and the quantum postulate itself (Bohr 1987, v. 1, pp. 52–53). The quantum postulate, defined by Bohr as a postulate, rather than a principle (in contrast, for example, to his correspondence principle), may, however, be seen as a manifestation of a principle, the quantum discreteness principle, which, even though it was not expressly formulated as such, was central to both Heisenberg's work leading him to quantum mechanics and Bohr's interpretation of it. In effect, it functioned as a principle in Bohr's thinking all along, even, implicitly, in his 1913 theory of the hydrogen atom. Bohr's thinking was principle throughout his work on quantum theory, including his earlier work in the old quantum theory, beginning with his 1913 theory, which brought him to the center stage of quantum theory, where he remained a central figure throughout his life. This position was equally due to his, *more* physical, contribution to the old quantum theory and his, *more* philosophical, contribution to quantum mechanics. (The difference here is that of balance, as Bohr's contribution was both physical and philosophical in both cases.)

Principle thinking has a pronounced practical dimension, at least as this thinking is understood here. Principles guide one's work, including one's technical work, especially when it comes to the invention of new theories or changing the character of theoretical physics and its practice. This is what happened in Einstein's, Bohr's, Heisenberg's, and Dirac's work, my primary historical examples of principle thinking by the founding figures of quantum theory.

While, then, also addressing foundational thinking in quantum theory (unavoidably, given that principle thinking is foundational), this study will focus on principles and principle thinking. That also means thinking that leads to the invention of new such principles, which is, I would argue, one of the ultimate achievements of theoretical thinking in any field. Although not entirely absent, this focus has been less common in more recent discussions and debates concerning quantum foundations, with a few exceptions to be noted as this study proceeds. I would argue, however, that exploring the nature of quantum principles and principle thinking is exceptionally helpful in addressing the key issues at stake in quantum foundations and the debate concerning it. Principle thinking has been significant and led to major breakthroughs throughout the history of quantum theory, beginning with the old quantum theory and quantum mechanics, the first definitive quantum theory, which it remains within its proper (nonrelativistic) scope. It has, I shall argue, been equally important in quantum field theory, which has been the main frontier of quantum theory for quite a while now, and more recently in quantum information theory, where principle thinking was given new prominence.

The main questions at stake in the ongoing debate about quantum phenomena and quantum theory have been and remain the following two questions concerning *reality* and *realism* in quantum theory. Both concepts involve important qualifications and complexities, which I shall address in Chap. 1. For the moment, their preliminary definitions will suffice to convey the most essential points. By reality I refer to the nature of quantum objects and processes that are ultimately responsible for observed quantum phenomena, which cannot be identified with quantum objects in the way classical objects can, at least ideally and in principle, in classical physics. By realism, I refer to the possibility of *representing*, at least ideally and in principle, the architecture of quantum reality (which architecture may be temporal), in this case and in all modern, post-Galilean, physics, by means of a mathematical model.

Realism, in physics, could be and here will be defined more generally by this possibility of such a representation, or at the very least by the assumption that physical reality has a structure or architecture (usually conceived on the model of classical physics) even though this architecture cannot be captured, at least for now, by a mathematical model. It follows that, in this definition (others are possible), realism is not only a claim concerning the *existence* (reality) of something but also and primarily concerning the *character* of this existence.

Now, the first question is whether quantum mechanics, which provides excellent probabilistic or statistical predictions of the outcomes of quantum experiments (only such predictions appear to be possible, at least thus far, on experimental grounds), is also a realist theory *in this sense* of providing an idealized mathematical representation of quantum objects and processes. The difference (explained in Chap. 1) between the probabilistic and statistical predictions may define different interpretations of quantum mechanics, even if they are nonrealist, as will be discussed in Chap. 4. The second question, arising in view of the difficulties of developing realist interpretations of quantum mechanics, is whether such a realist theory of quantum objects and processes is possible at all, a question further complicated by the considerations of locality, on which I shall comment below.

These questions are hardly surprising, because they can be asked about any physical theory, or any fundamental theory in science or elsewhere, certainly in philosophy, or, with qualifications concerning the nature of mathematical reality, commonly assumed to be mental, in mathematics. They have been asked beginning with the pre-Socratics, for example, as concerns the Democritean materialist atomism or, conversely, its idealist counterparts, such as Parmenides's and then Plato's philosophy, which was inspired by Parmenides, but went further by assuming a mathematical, and specifically geometrical, nature of the ultimate reality. However, quantum phenomena and quantum mechanics (more so than the so-called old quantum theory that preceded quantum mechanics) presented major difficulties in answering these questions in the way they could be answered in classical physics or even relativity, although the latter already presented difficulties in this regard, especially given the behavior of photons there. (Ultimately, photons are quantum objects.) Accordingly, different answers and even a different way of asking these questions may be necessary, although many, beginning with Einstein, would not necessarily agree with this assessment. As Bohr said in 1949 (20 years into his debate with Einstein), most did not think that one needed to go that far in "renouncing customary demands as regards the explanation of natural phenomena," that is, as far as Bohr thought it was necessary to go (Bohr 1987, v. 2, p. 63). This renunciation was far reaching and ultimately led Bohr to the concept of a *reality* of quantum objects that precludes one from considering quantum mechanics as a realist theory or even assuming the existence of any *realist* theory of this reality, *at least as things stand now* as regards the experimental evidence available. This is a crucial qualification assumed by Bohr in all of his writings and to be assumed throughout this study (the essential nature of this evidence has not changed). This conception of reality may thus be defined as that of "reality without realism," and the principle corresponding to this concept and assumed by Bohr and in this study, the reality-without-realism (RWR) principle.

This concept leads to the first "divorce" between quantum physics understood in the spirit of Copenhagen and the preceding fundamental physics—the divorce of reality from realism, previously joined together and seemingly requiring each other.

The questions concerning the nature of reality behind quantum phenomena and the possibility, or impossibility, of realism in quantum theory came into the foreground early in the history of quantum mechanics, especially in the Bohr-Einstein confrontation. This confrontation has shaped the subsequent debate concerning quantum theory and its interpretations, and it continues to do so, as this debate itself continues with undiminished intensity, with no apparent end in sight. Einstein argued that quantum mechanics is incomplete because it is not able to describe, at least not completely, elementary individual quantum objects and processes, *analogously* to the way classical mechanics or electromagnetism, or relativity does, at least ideally and in principle, for the objects and processes each considers. In other words, it is not a realist theory or does not provide a realist mathematical model representing the elementary individual processes responsible for quantum phenomena. I will term this concept of completeness "Einstein-completeness." I stress "analogously" because it became clear early on that a new kind of theory would be necessary to provide such a classical description, as Einstein was careful to qualify. Indeed, it was Einstein who was the first to make this apparent. In addition to being the creator of relativity (a theory different from classical mechanics and classical electromagnetism as well), he was the first to show the incompatibility between quantum theory and the assumptions of classical statistical physics (Einstein 1906). This incompatibility suggested that probability and causality might have needed to be separated, *divorced*, from each other. This is because classical statistical physics is fundamentally linked to classical mechanics, which is assumed to apply to the individual behavior of the elementary constituents of the systems considered in classical statistical physics. This assumption is no longer sustainable in quantum theory, which, in the first place, is probabilistic or statistical even as concerns its account of elementary individual quantum processes, such as those associated with elementary particles (photons, electrons, neutrinos and so forth).

According to Einstein's concept of a realist and (they are, thus, related) complete theory, then, the observables representing the state of any individual system considered could be assigned definite values, defining the physical state of the system at any moment of time and its evolution in a classically causal way, and thus allowing one to predict its behavior (ideally) *exactly*. In principle, one could assume, as some do in interpreting quantum mechanics, that a physical theory could causally represent the behavior of the individual systems considered, while also assuming that any predictions concerning this behavior are probabilistic or statistical. This is, however, not Einstein's view of completeness.

Bohr counterargued that, while Einstein's claim concerning the Einstein-incompleteness of quantum mechanics may be true, and while, in view of the RWR principle, it is strictly true in Bohr's and other interpretations based on this principle, quantum mechanics may be seen as complete in a different sense. It is as complete as nature allows a theory of quantum phenomena to be within the proper scope of quantum mechanics, again, as things stand now. Quantum mechanics is complete insofar as it correctly predicts the outcomes of all quantum experiments performed thus far, even though it does not describe the behavior of quantum objects themselves in the way classical mechanics or relativity does, which (and thus, the Einstein-completeness of the theory) *may not* be possible. I shall term this concept of completeness “Bohr-completeness.” Because this study follows Bohr’s argument on this point and is primarily concerned with Bohr-complete interpretations of quantum phenomena, I shall henceforth mean by the completeness of quantum mechanics its Bohr-completeness, qualifying when at stake is the Einstein-completeness or the contrast between them.

The concept of Bohr-completeness and, in this case, correlatively the lack of realism are in the spirit of Copenhagen, initiated and shaped by Bohr’s thinking. The designation “the spirit of Copenhagen” is preferable to “the Copenhagen interpretation,” because there is no single such interpretation. Even within the spirit of Copenhagen, there are different views, which, moreover, have often undergone historical evolutions even as concerns the views of the single figures involved. As indicated above and as will be discussed in detail in Chap. 3, this is notably true even in Bohr’s own case. In part under the impact of his exchanges with Einstein, Bohr changed his views a few times, sometimes significantly, on his way to fully grounding his interpretation in the RWR principle in the late 1930s. Some interpretations designated “Copenhagen interpretations” only partially conform to the spirit of Copenhagen as understood in this study. Accordingly, one should be careful in specifying which interpretation defines a *particular* Copenhagen interpretation that one refers to, and I shall try do so throughout this study, which offers its own interpretation, in the spirit of Copenhagen, designated as “the statistical Copenhagen interpretation,” proposed in Chap. 4. For convenience and economy, I shall henceforth refer to RWR-principle-based interpretations as “nonrealist interpretations,” qualifying when the term nonrealist is used otherwise, as it is sometimes, for example, in referring to interpretations or theories that would be realist in the definition adopted in this study.

I might add that the so-called dominance of “the Copenhagen interpretation” is largely a myth. Indeed, given that there has never been a single such interpretation, the very existence of “*the* Copenhagen interpretation” is a myth, propagated by its advocates and opponents alike, because it can help both sides. As concerns more specifically the alleged dominance of Bohr’s views, Bohr’s own statement, made in 1949, after two decades of this presumed dominance, may well be the best evidence against it. He said: “I am afraid that I had in this respect only little success in convincing my listeners, for whom the dissent among the physicists themselves was naturally a cause of skepticism about the necessity of going so far in renouncing customary demands as regards the explanation of natural phenomena” (Bohr 1987, v. 2, p. 63). Einstein was undoubtedly foremost on his mind, especially on this occasion. The 1949 article just cited, “Discussion with Einstein on Epistemological Problems in Atomic Physics,” was his contribution to the so-called Schilpp volume, *Albert Einstein: The Philosopher Scientist*, edited by P. A. Schilpp (Schilpp 1949). Einstein spearheaded this dissent and the resistance to the spirit of Copenhagen, a resistance still as widespread as ever. Nonrealism in quantum theory, or pretty much anywhere, has always been and remains a minority view, even though most of its opponents, beginning, again, with Einstein, admitted that this view is "logically possible without contradiction" (Einstein 1936, p. 349). It is not a matter of logic but of irreconcilable philosophical positions concerning fundamental physics.

Admittedly, the denomination "the spirit of Copenhagen" is not fully definitive either, and it is not aimed to be, as is suggested by the word "spirit." Whether this spirit had "directed the entire development of modern atomic theory" even by that point (1930) may be questioned, given, for example, Einstein's role in its development prior to quantum mechanics (1905–1924), or Schrödinger's discovery of his wave mechanics in 1926, which were not in this spirit or, as in the case of Schrödinger's program, aimed against it. The spirit of Copenhagen was, nevertheless, a major force, however resisted, shaping and driving this development. Clearly, too, as indicated above, in choosing this impression, Heisenberg has in mind the confrontation between nature and the human spirit or mind (as I noted, German *Geist* has both meaning), given the philosophical implications of German *Geist*, the word that G. W. F. Hegel made central to German and European philosophy with his *Phenomenology of Spirit* (Hegel 1977). This confrontation, I argue, took a new form with quantum theory, both in general, because it posed a new task for physics and philosophy, and specifically as that between the quantum reality of nature and the spirit of Copenhagen. The spirit of Copenhagen is, again, defined by its questioning of the possibility of realism in considering the ultimate (quantum) constitution of nature, ultimately by adopting the RWR principle, which, in its strongest form, disallows not only a representation but also a conception of this constitution.

As will be seen, the absence of causality, as classically understood, is automatic under the RWR principle, thus joining the divorce of reality from realism and (again, as a consequence) the divorce of probability from causality, as classically conceived. If understood classically, causality implies, ontologically, that the state of a given system, at least as idealized by a given theory or model, is determined at all moments of time by their state at a particular moment of time, indeed at any given moment of time. Determinism, by contrast, implies, epistemologically, that we can make ideally exact predictions concerning the behavior of causal systems, which is not always possible. Until the emergence of quantum physics, probability or statistics was merely a practical means of dealing with causal system of great mechanical complexity, rather than a fundamental aspect of physics, necessary even in considering elementary individual processes and the events they lead to. Classical causality in physics implies that the behavior of the *individual* systems considered in classical physics (classical mechanics, classical electrodynamics, or classical statistical physics) or relativity could be predicted exactly, at least ideally and in principle, rather than only probabilistically, as in the case of such processes in quantum physics. The difficulties of not being able to do so in classical statistical physics or chaos theory are merely practical, defined by the mechanical complexity of the system considered in these theories, and in certain circumstances these difficulties could be circumvented. One could, for example, sufficiently isolate a molecule of a gas and make deterministic predictions concerning its behavior. As discussed in

Chap. 5, it is possible to define the concept of causality as compatible with the probabilistic or statistical nature of quantum predictions. Deterministic predictions are, again, precluded on experimental grounds even when they concern elementary individual quantum processes, such as those associated with elementary particles.

That quantum mechanics provides only probabilistic or statistical predictions even in these cases is, thus, a fundamental, rather than merely practical, matter. These predictions, however, are strictly in accord with what is actually observed, because identically prepared quantum experiments, in general, lead to different outcomes. There are no kinds of quantum processes and events, no matter how elementary, concerning which even ideally exact predictions are possible, as things stand now. This fact gives rise to a principle, the quantum probability principle, the QP principle, one of the starting points, even *the* starting point, of Heisenberg's thinking leading him to the discovery of quantum mechanics. As will be discussed in Chap. 4, these predictions may be seen, or interpreted, still assuming the RWR principle, as statistical rather than only probabilistic, insofar as only the statistics of multiple repeated experiments dealing with individual quantum processes could be estimated, rather than the probability of each such event, say, on Bayesian lines. In other words, it is a matter of interpretation whether one could assign probabilities to the outcomes of individual quantum experiments or could only deal with the statistics of (multiple) repeated experiments. In the case of statistical interpretations, the QP principle becomes the quantum statistics, QS, principle. When either interpretation is assumed possible, I shall, for the sake of economy, refer to the QP/QS principle.

The RWR principle and, with it, the suspension of causality are *interpretive* inferences from the QP/QS principle. It is, in principle, possible to have a realist and causal—and hence Einstein-complete—theory of elementary individual quantum processes, but as things stand now, any such theory must give these predictions, that is, predictions that coincide with those of standard quantum mechanics. One such theory, arguably the best-known one, is Bohmian mechanics (in all of its versions), which is, however, nonlocal. Given the appeal of the realist imperative of the type Einstein insisted on, there is no shortage of proposals, some of which will be mentioned later in this study. For the moment, I only note that (the standard form of) quantum field theory, underlying the so-called standard model of elementary particles, which accounts for all known forces of nature apart from gravity, is a probabilistic or statistical theory of the same type as quantum mechanics, at least when given a nonrealist interpretation. The question, then, becomes whether nature will at some point allow us to have an Einstein-complete theory of quantum phenomena. Einstein, taking Einstein-completeness as a fundamental principle, thought it should. Bohr thought that it *might not*, which is not the same as saying that it never will (e.g., Bohr 1949, in Bohr 1987, v. 2, p. 57).

Einstein remained unconvinced and never retreated from his position. His uncompromising refusal to accept that the ultimate theory of individual quantum processes *might* ultimately have to be probabilistic or statistical may appear surprising, given his deep understanding of statistical physics and the relationships between it and quantum theory, and his extraordinary contributions to both by exploring these relationships in the old quantum theory. Einstein's revolutionary 1906 argument, mentioned above, that Planck's law is incompatible with classical statistical physics, was the most powerful early indication that quantum phenomena may not allow for an analysis of individual quantum processes on classical lines, if at all (Einstein 1906). This is, again, because classical statistical physics presupposes that the elementary constitutive individual components of the systems in question behave in accordance with classical mechanics, which would, given Einstein's argument, suggest that quantum mechanics might be an irreducibly probabilistic or statistical theory even in dealing with elementary individual quantum processes. Most of Einstein's work on the old quantum theory may be viewed from this perspective and interpreted in this direction, ultimately pursued by Bohr in his 1913 atomic theory and then in his interpretation of quantum mechanics, but clearly rejected by Einstein himself as unacceptable, or at least incompatible with his understanding of fundamental physics and its principles. I shall explain some of the reasons for Einstein's attitude later in this study, merely noting at the moment that, in this work too, he appears to have always believed that a classical-like realist, Einstein-complete, and also causal theory of elementary quantum objects and processes should one day be found. All of his work on statistical physics and quantum theory, and their relationships, has retained this belief and was guided by it, a pursuit that, at least if viewed from the present perspective, was against the grain of his own analysis of quantum phenomena for two decades. Quantum mechanics ran contrary to these expectations, although Schrödinger's wave mechanics (much favored by Einstein, as against Heisenberg's matrix version) initially offered some hopes to meet them. As Einstein was quick to realize, these hopes were unwarranted. Einstein accepted that quantum mechanics was a viable statistical theory. But because it was not a theory describing or representing, ideally exactly, individual, especially elementary individual, objects and processes, it was incomplete. As far as he was concerned, quantum mechanics was epistemologically even worse than the old quantum theory (things would get even worse and move even further away from Einstein's ideal of a fundamental theory with quantum field theory). As such, the theory was a great disappointment to him, and not to him alone. Schrödinger was hardly less discouraged, after his hope for his initial program for wave mechanics had dissipated, although these hopes were partially revived later. The story of Einstein's engagement with and disappointment in quantum theory nearly repeated itself with Schrödinger, whose work prior to his discovery of his wave mechanics and his famous equation was on statistical physics and the relationships between it and atomic theory, established by Einstein. Schrödinger followed Einstein's work on both statistical physics and the old quantum theory, and Einstein's view of what the fundamental quantum theory should be. Schrödinger's work that immediately preceded and in part led to his discovery of wave mechanics was inspired by Einstein's work, via de Broglie's ideas (also crucial for Schrödinger's wave mechanics), on the Bose-Einstein theory of the ideal quantum gas.

At a certain stage of the Bohr-Einstein debate, in the 1930s, the question of locality was injected into this debate and our understanding of quantum mechanics, primarily owing to thought experiments invented by Einstein, especially those of the EPR (Einstein-Podolsky-Rosen) type, first proposed in the famous paper of

A. Einstein, B. Podolsky, and N. Rosen (Einstein, Podolsky, and Rosen 1935). (Einstein pursued this line of thought even earlier.) Einstein subsequently proposed somewhat, but not essentially, different versions of the experiment. By using these experiments, Einstein argued that quantum mechanics could be considered as offering a complete in his sense (Einstein-complete) description of individual quantum processes only if quantum mechanics or nature itself violates the principle of locality. The principle dictates that physical systems could only be physically influenced by their immediate environment or, in this case, specifically, that the instantaneous transmission of physical influences between spatially separated physical systems is forbidden. Einstein famously spoke of "a spooky action at a distance," found, or so he believed, in the EPR-type experiments, if quantum mechanics was complete, even Bohr-complete (A Letter to Born, 3 March, 1947 [Born 2005, p. 155]). Bohr counterargued that, even given these experiments, quantum mechanics could be shown to be both complete, by his criterion (Bohr-complete), and local, even if short of assuming the RWR principle and nearly automatically under this assumption, or at least that EPR or Einstein in his subsequent communications did not prove otherwise (Bohr 1935). I shall consider Bohr's argument in Chap. 3. As did Einstein, Bohr ruled out nonlocality, again, at least on the basis of the evidence available thus far, which is one of the reasons why neither Einstein nor Bohr saw Bohmian mechanics (introduced in 1952), which is nonlocal, as a viable alternative. Bohr's argument, thus, left open the question whether a more complete and specifically an Einstein-complete local theory of quantum phenomena is possible, a question that remains open.

Einstein later qualified that, if quantum mechanics is considered as only a statistical theory of quantum phenomena, a theory providing only a statistical estimate of the outcomes of multiple repeated individual experiments (including those of the EPR type), then it could be considered local (e.g., Einstein 1936; Born 2005, pp. 166–170, 204–205, 210–211). Thus, Einstein de facto accepted an argumentation of the type offered by Bohr, although he misread Bohr's 1935 reply itself to EPR's paper differently, by assuming that Bohr in fact allowed for nonlocality, which is in manifest conflict with Bohr's argument there (Einstein 1949b, pp. 681–682, Plotnitsky 2009, pp. 244–246). Bohr expressly speaks of the compatibility with "all exigencies of relativity theory," and locality is one of them (Bohr 1935, p. 700). In any event, this view would still leave quantum mechanics Einstein-incomplete, and hence, again, would also leave open the question whether nature allows us to have an Einstein-complete theory of these phenomena, or only a Bohr-complete theory of them. In Bohr's view, quantum mechanics is a probabilistic or statistical theory of quantum phenomena, even those resulting from elementary individual quantum processes, insofar as it only provides probabilistic or statistical estimates concerning the data observed in measuring instruments, which data defines quantum phenomena, again, in contradistinction to quantum objects. As noted above, however, this is fully in accord with the experimental evidence available thus far, because identically prepared (as concerns the physical state of measuring instruments) quantum experiments, in general, lead to different outcomes, even when they concern elementary individual quantum processes. Einstein wanted local *realism*, which relativity satisfied and in which the concept in part originated (although locality is a more general concept). Bohr, with quantum mechanics as the best available theory of quantum phenomena in hand, argued for local *reality* that precludes realism, in accordance with the RWR principle, and as such, given that it automatically precludes causality as well, entails the irreducibly probabilistic or statistical nature of our predictions concerning quantum phenomena. By the same token, quantum mechanics' ability to offers such predictions made it Bohr-complete. Eventually, especially in the wake of the Bell and the Kochen-Specker theorems, and related findings, the question of locality, rather than that of completeness, came to dominate the debate concerning quantum phenomena and quantum theory. However, because realism has remained a major concern, in particular given the lack of realism as a possible alternative to nonlocality, the question of completeness has remained germane to this debate.

The concept and principle of locality is commonly associated with relativity, especially special relativity, although general relativity conforms to the principle as well. However, while thus implied by relativity, locality is not equivalent to compatibility with relativity and is independent of other key concepts with which it is linked in relativity, such as the Lorentz invariance. Indeed, the latter is only locally or infinitesimally valid in general relativity. On the other hand, general relativity is, again, a local theory. Also, technically relativity prohibits the propagation of physical influences faster than the speed of light in a vacuum, c, which is finite, rather than instantaneously. Accordingly, this requirement could, in principle, be violated, while still allowing for locality. Einstein, in invoking, in the context of the EPR-type experiments, a "spooky action at a distance," clearly had in mind the principle of locality, rather than only the compatibility between quantum mechanics and relativity. Indeed, standard quantum mechanics is not relativistic and hence, technically, not compatible with special relativity (unlike quantum electrodynamics or quantum field theory, which deal with high-energy quantum phenomena), but it is or may be interpreted as local, or in any event may be required to be local. Nor does locality require one to maintain the concept of realism, which is less of a problem for relativity than for quantum physics, and is, quite possibly, a reflection of this difference between locality and relativity. Locality is fully consistent with the concept of reality without realism and the RWR principle. The principle allows for a local quantum reality, demanded by both Einstein and Bohr, but precludes local *realism*, demanded by Einstein. This makes quantum mechanics, while Einstein-incomplete, Bohr-complete, as complete a *local* theory of quantum phenomena as nature allows us, at least as things stand now. It is not insignificant either that relativity is a classically causal and in fact deterministic theory, while quantum mechanics or quantum field theory is neither deterministic nor, at least in nonrealist interpretations, (classically) causal, and thus is a local probabilistic or statistical theory. The locality principle may, thus, reflect deeper aspects of the ultimate reality of nature than those captured by relativity theory, general relativity included. I am not saying that the locality principle is quantum in nature, although it is conceivable that it might be, especially if the ultimate nature of gravity is quantum. This separation of locality from relativity is the third major divorce argued for in this study, along with that of reality from realism and that of probability from causality.

The project of this book is a philosophical account of the fundamental principles of quantum physics and their significance. As such, this project belongs to the philosophy of physics. It represents, however, a different form of philosophy of physics, vis-à-vis most other forms of the institutional philosophy of physics and specifically the philosophy of quantum theory, apart from some more historically oriented studies, where some aspects of the present approach could occasionally be found. Even more than in my emphasis on principles (which is, as I said, uncommon as well), this difference is reflected in my emphasis on *thinking* concerning quantum physics. By this I mean both thinking by the key figures considered here and our own thinking, that of this book's readers included, not the least *principle thinking* itself. Einstein, from whom I borrow the concept of principle theory, also expressly spoke about principle *thinking* and principle *thinkers*, among whom he counted both Bohr and himself. This was, it is true, before quantum mechanics and their debate concerning it. This fact, however, does not change this assessment. It was a debate between two principle thinkers about what the fundamental principles of quantum theory should be, even though Einstein's thinking by this point was as much what he called "constructive" as it was principle. Constructive thinking is effectively defined by the imperative of sufficiently closely, even if not fully, representing, in mathematical terms, the ultimate constitution of nature, or more accurately by *constructing* such a representation. While Einstein's own definition of a constructive theory (to be discussed in Chap. 1) is worded somewhat differently, it is conceptually equivalent to the one just given. Bohr thought that this type of representation might not be possible in quantum theory by virtue of the fundamental principles of quantum physics, as he saw them. This view implies that quantum mechanics is strictly a principle and not constructive theory, unless it is seen as constructing the ultimate reality behind quantum phenomena as beyond the possibility of a representational construction. This, however, would run against Einstein's definition of a constructive theory, while remaining compatible with his definition of a principle theory. Einstein's argument clearly concerned as much the character of thinking about fundamental physics as the physical and mathematical architecture of the theories resulting from this thinking, from classical physics to relativity to quantum theory. It may of course happen that the same theory results from different way of thinking, as was in fact the case in Heisenberg's principle (and nonrealist) thinking and Schrödinger's constructive (and realist) thinking that, nevertheless, led each of them to quantum mechanics.

Although the term thinking is commonly used without further explanation, generally referring to mental states or processes as effects of the neurological processes in the brain, which would probably suffice here as well, I would like to say a few words about the type of thinking, essentially creative scientific thinking, most especially at stake in this study. This thinking is a way in which our brains confront chaos in our interactions with the world. G. Deleuze and F. Guattari, whom I follow here, speak in this connection of "thought" [*la pensée*] rather than "thinking."

Chaos, too, is given a particular concept by Deleuze and Guattari, as a certain "virtual[ity]" leading to the birth and disappearance of "particles" with infinite speed, referring to the speed of thought, as this speed appears to us (Deleuze and Guattari 1994, p. 118). This concept does not appear to have been previously used in philosophy. It is borrowed by them, at least in part from quantum field theory and its concept of virtual particle formation to be discussed in Chap. 6, as is suggested by their use of the terms "virtual" and "particle." The connection between quantum field theory and this concept of chaos is obviously a transfer of a concept from one domain of theoretical thinking to another. While one might see this transfer as a metaphor, it is the functioning of this concept as such in Deleuze and Guattari's understanding of thought that is crucial. The quantum-theoretical concept in question deals with matter and is approached by way of exact, mathematical science, quantum field theory; Deleuze and Guattari's concept deals with thought and is approached by way of philosophy, which is not mathematical. Milton's description of chaos discussed earlier would work here just as well as that of Deleuze and Guattari. First, this description is consistent with Deleuze and Guattari's conception of chaos as the birth and disappearance of "particles" from chaos, which, as I noted, is invoked by Milton. Second, Milton's is a richer conception because it adds chaos as randomness or chance and, by implication, chaos as the unrepresentable or the unthinkable to chaos as the virtual. Of course, one can also add both to Deleuze and Guattari's concept of chaos. My point here is that this extended concept of chaos is necessary for understanding the nature of creative thinking as a confrontation with chaos. I might add that Milton's Satan never engages in any confrontation with chaos, because his thinking is never truly creative. Creative thinking must certainly confront randomness and chance, and *take chances*, *bets*, often with uncertain probabilities to succeed.

The view of creative thinking as a confrontation of chaos (now in all three senses just described) is hardly surprising: most thinking may be seen as giving order to our perceptions, images, ideas, words, and so forth, and thus as involving a confrontation with chaos. Thought (in Deleuze and Guattari's sense) is, however, a special form of this confrontation, because it maintains an affinity with and works together with chaos, rather than merely protecting itself from chaos, as would, for example and in particular, the dogmatism of opinion (*doxa*), including scientific opinion, if dogmatically accepted. The character of thought as a *cooperative* confrontation with chaos, making thought and chaos work together for the benefits of thought, makes thought creative, and, according to Deleuze and Guattari, art, science, and philosophy are, each in their own way, among the primary means, or even *the* primary means, of creative thought. This is why they see chaos not only as the greatest enemy but also as the greatest friend of thought, and its best ally in its yet greater struggle, that against opinion, always an enemy only, "like a sort of 'umbrella' that protects us from chaos." On the other hand, thought's "struggle with chaos is … the instrument in a more profound struggle against opinion, for the misfortune of people comes from opinion" (Deleuze and Guattari 1994, pp. 202, 206). This is equally true in physics or science, or mathematics. All major advances in physics were born in or required profound struggles against prevailing opinion. These struggles defined the thought of all key thinkers considered in this study, sometimes, as, famously, in Planck's case, without them quite realizing the degree to which they were waging this struggle. In the cases of Einstein, Bohr, Heisenberg, and Dirac this struggle was manifest and pursued with an unwavering determination and courage. Physics, especially fundamental physics, could not be advanced otherwise.

Physics is a product of human thinking, of creative human thinking or thought in the sense just described, under complex material, technological, psychological, historical, and sociological conditions. Accordingly, one can pursue, as I shall do in this study, a philosophy of physics that attempts to understand how physicists think under these conditions, especially at the time of and in the process of making new discoveries, for example, by means of inventing new concepts and principles, and sometimes changing the very nature of physics in the process. Galileo accomplished this by giving modern physics its mathematical or mathematical-experimental character, in this case, a descriptive or representational one. Newton accomplished this by making calculus the main mathematical technology of theoretical physics. Einstein accomplished this by rethinking the concepts of space, time, and motion, and discovering the principle of relativity defining these concepts in his special relativity theory, and by bringing together the mathematical principles of Riemann's geometry (which radically changed the principles of geometry and physics alike) and the principle of equivalence between gravitational and inertial mass in his general relativity. Heisenberg accomplished this by divorcing the mathematical formalism of quantum theory from the task of representing quantum objects and their behavior, and hence from the principles of realism, and making probability and the QP/QS principle and, by implication, the RWR principle his primary principles. Dirac accomplished this by bringing together the principles of special relativity and quantum mechanics, which also led him to the discovery of new principles, those that came to define quantum field theory. Other examples will be discussed in this study as well, such as those found in quantum information theory, the most recent incarnation of foundational and specifically principle quantum-theoretical thinking.

This study's approach is, thus, different from that of dealing primarily with the logical-axiomatic structure of quantum theory or that of addressing, in more general terms, broader epistemological or ontological questions, such as reality and causality, as is more common, especially, again, in the institutional (analytic) philosophy of physics. More recently the question of quantum information came to prominence as well, although, arguably, more so among foundationally inclined quantum physicists than among the philosophers of physics, some of whom, however, have addressed quantum information theory. Such questions are important to our thinking, too, including when it comes to the key principles of quantum theory, and these questions will be considered in this study. But they will be considered as part of human thinking, which is not inconsistent with giving them the same rigor that the analytic philosophy of physics requires and may help to do so.

Physics is thinking about nature by particular persons and communities, which share certain aspects and trends of thought. It is thinking about what is true or probable about nature or those aspects of nature that physics considers, and not infrequently, especially in dealing with foundations, it is also, more philosophically, thinking about the *nature* of our thinking about nature, again, the main philosophical concern of this study. In modern (post-Galilean) physics, classical, relativistic, or quantum, this truth or probability is determined by means of mathematical models. Such theories may do so either by using a mathematical representation of the processes responsible for these data and predicting them on the basis of this representation, as in classical physics or relativity, or just by using a mathematical formalism to predict these data in the absence of such a representation, as in quantum mechanics. How close we come to the ultimate constitution of nature in this way may depend on a given theory or on nature itself or rather our interactions with nature, on how far nature could allow our mathematical theories and experimental technologies to reach. These interactions are, again, ultimately part of nature, too, but a very particular part of it, specific to us.

Given the aims and scope of this study, I will not be concerned with psychological and sociological aspects of quantum-theoretical thinking. On the other hand, history will play a significant role in it. History is unavoidable in theoretical (or experimental) thinking in physics, which always builds on preceding thinking in physics, even at the time of new discoveries, however revolutionary or unexpected such discoveries may be. Every physical (or of course philosophical) idea, no matter how original or new, has a history, some trajectories of which may be short and others long, sometimes extending to ancient thought. Conversely, the history of physics or, again, philosophy is the history of concepts, although, in the case of physics, it is, in addition to the mathematical nature of these concepts, also the history of experimental technology, a combination that makes modern physics an experimental-mathematical science of nature. We create our ideas by engaging with this history, which helps us to understand earlier ideas and to create our own, especially when these concepts are created by the likes of Einstein, Bohr, Heisenberg, Schrödinger, Pauli, or Dirac. But they, too, created their ideas by engaging both with a more immediate history of these ideas, sometimes in each others' works (as in Heisenberg's engagement with Bohr's thought, or Dirac's with that of Heisenberg), and with a longer history of physics and philosophy, in some respects going back as far as Aristotle and Plato, or even to the pre-Socratics.

A qualification is in order. When I speak of the thinking of any particular figure, I do not claim to have a determinable access to this thinking. Such an access is limited even when the author is alive and could, in principle, provide one with as much information as possible concerning this thinking, or if the author had left an extensive record of this thinking, say, in letters and notes, that could supply this kind of information. Instead, I refer to thinking that one can follow and can engage with in one's own thinking on the basis of certain works of a given author, and even then in a particular reading or interpretation of these works, which can be interpreted differently, and, thus, related to different ways of thinking. A proper name, such as Einstein, Bohr, Heisenberg, Schrödinger, Pauli, or Dirac, is the signature underneath a given work or set of works, a signature that attests to one's role as a creator of these works, which serve as a guide for thinking that we can pursue as a result of reading them. In the process, one can of course also gain insights into how a given author might actually have thought. However, claims to that effect are hard to make with certainty, although some among such claims may be probable and even highly probable.

Be it as it may on this score, any theory or interpretation only becomes effective or, again, operative, to begin with, when it becomes part of our thinking, and a theory or interpretation advances physics, or the philosophy of physics, when it moves this thinking beyond itself. This does not mean that, helpful as they might be, the thinking and works of any particular author, no matter how great or important, or the understanding of this thinking and these works is the only path towards a better understanding of a given physical theory and, especially, advancing this understanding or the theory itself. When it comes to advancing thought and knowledge, one's loyalty to anyone's thinking becomes a secondary matter and is only valuable if it helps this advancement. My discussion of the figures considered in this study aims to be faithful to their thinking and writings as much as possible, as a matter of maintaining proper scholarly standards, and this study is motivated by my belief in the helpfulness of their thinking for understanding and advancing quantum theory. The project of this study is, however, not driven by loyalty to their particular ideas, but by a dedication to understanding, through these ideas, how thinking works in quantum physics, and it often works by moving along different trajectories. In part for that reason, this study also presents different and even conflicting ways of thinking in quantum theory, such as that of Bohr, Heisenberg, and Dirac, as principle thinking, vs. that of Schrödinger, as primarily constructive rather than principle thinking (although it had a principle dimension as well). At a certain point, our thinking concerning quantum physics will inevitably have to move beyond the authors discussed here (both founding figures just mentioned and others), and there is no special reason to assume that it will do so following the paths established by any of them. An entirely different trajectory, either already in place but unknown to us (or to some of us) or yet to be discovered, may be necessary for this task.

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Contents

Chapter 1
Concepts and Principles in Fundamental Physics

Abstract The aim of this introductory chapter is to outline the main concepts of this study, and to consider the nature of principle thinking in theoretical physics, most especially in quantum physics. After a brief introduction given in Sect. 1.1, Sect. 1.2 first defines the *concept* of concept, the main vehicle of thinking in theoretical physics. Then, it considers other key concepts of fundamental physics: theory, model, reality and realism, causality, randomness, probability and statistics, and locality. The deeper aspects of these concepts will be addressed throughout this study. The outline offered in this chapter is designed for introductory purposes and for avoiding misunderstandings concerning these concepts as defined by this study (they may be defined otherwise). Sec. 1.3 is devoted to a discussion of the concept of principle and the nature of principle thinking in theoretical physics, taking as its point of departure Einstein's distinction between principle and constructive theories.

1.1 Introduction

The nature of key quantum-theoretical concepts has been as central to quantum theory and debates concerning it as the nature of the corresponding key concepts to earlier physical theories, such as classical physics, thermodynamics, electromagnetism, and relativity, or subsequent theories, such as string or brane theory, and debates concerning them. One could hardly think of any major physical theory that would not have been intensely debated following its introduction and for quite a while afterwards, although new theories and debates concerning them make us forget these earlier debates. The centrality of physical concepts themselves is unavoidable because theoretical or, for that matter, experimental physics is impossible without them. Physicists may not always expressly consider or even define these concepts, especially the concept of concept itself, almost never defined in physics and rarely even in the philosophy of physics. But they cannot do without them.

By contrast, as noted in the Preface, while not entirely absent, the focus on fundamental principles (prominent earlier, especially in the wake of the introduction of quantum mechanics) has been less common in more recent discussions concerning quantum foundations, apart from quantum information theory, which, as will be

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seen in Chap. 7, revived this focus.[1] This is not surprising because the development and, to begin with, the discovery of a new theory, especially when it is revolutionary in character, often needs new principles. Classical physics, thermodynamics, electromagnetism, relativity, quantum mechanics, and quantum electrodynamics and (extending quantum electrodynamics) quantum field theory could all serve as examples of the importance of fundamental principles in the rise of new theories. I would argue, however, that exploring the nature of fundamental principles and principle thinking is exceptionally helpful in understanding the key issues at stake in quantum foundations and the debate concerning them for conceptual rather than only historical reasons. But then, part of my argument in this study is that conceptual and historical considerations are interconnected, and it helps to explore them together.

1.2 Concepts

1.2.1 Concepts, Theories, and Models

If, as F. Wilczek, a leading elementary particle theorist and a Nobel Prize laureate, argues, "the primary goal of fundamental physics is to discover profound concepts that illuminate our understanding of nature," then foundational thinking in fundamental physics is defined by concepts and is advanced by the discovery or invention of new concepts (Wilczek 2005, p. 239). I shall, for the moment, put aside whether concepts are discovered (as something pre-existing somewhere) or invented, constructed, and the debates surrounding this subject, giving a preference to the invention of concepts for reasons that will become apparent presently. The question that I want to ask is: What is a physical concept? Wilczek does not explain it, taking it for granted or assuming some general sense of it presumably shared by his readers (those of *Nature* or other physics journals). This is not uncommon. The term concept is often used, under similar assumptions, without further explanation in physical or even philosophical literature. Does Wilczek mean only a physical concept, for

[1]Among exceptions elsewhere are A. Zeilinger's article (Zeilinger 1999) and J. Bub's article on quantum mechanics as a principle theory in Einstein's definition (Bub 2000) (both of these articles, however, are linked to quantum information theory), A. Bohr, B. R. Mottelson, and O. Ulfbeck's critique of, among others, Niels Bohr (on which I comment in Chap. 3), (Bohr et al. 2004), and an earlier approach to Heisenberg's discovery of quantum mechanics by the present author (Plotnitsky 2009, 77–114). The approach developed in this study extends and revises (Plotnitsky 2015). An instructive recent example of the principle approach to fundamental physics beyond quantum theory is R. M. Ungar and L. Smolin's book, which builds on Smolin's earlier work (Ungar and Smolin 2014). The principles grounding Smolin's argument (presented separately in the book) are different from the principles of quantum theory, as considered in this study. Indeed, most of the key principles that ground Smolin's argument, beginning with Leibniz's principle of sufficient reason, are in conflict with most of the main principles of quantum theory advocated here, or for that matter, with most principles of relativity, both special and general. There is some overlap. The gauge-invariance principle, extensively used by Smolin, is consistent with the principles advocated here and is central to quantum field theory. Also, Smolin's overall view of mathematics and its role in physics is in accord with the equally non-Platonist philosophical stance adopted in this study.

example, or does he also allow that such a concept may have a philosophical component, and, thus, imply the discovery or invention of philosophical concepts as well? One might safely assume, given the specific concepts that Wilczek invokes, that the concepts in question have mathematical components, the presence of which has defined all modern, post-Galilean, theoretical physics. In illustrating his claim, Wilczek, too, associates the currently most significant physical concepts with mathematical entities, in particular, the concept of elementary particle with symmetry groups, to be discussed in Chap. 6. I shall also define a physical theory as an organized assemblage of concepts (in the present sense, explained below), associated with certain physical objects or phenomena, usually as defined by physical experiments or their equivalent in nature. Objects and phenomena are not the same even in classical physics, as Kant realized, although they could be treated as the same for all practical purposes. This is no longer possible in the case of quantum objects and quantum phenomena (defined by the effects of the interactions between quantum objects and measuring instruments), at least in interpretations of quantum phenomena in the spirit of Copenhagen, beginning with that of Bohr.

The present understanding or concept of concept follows G. W. G. Hegel and G. Deleuze and F. Guattari, who, however, apply this understanding to philosophical concepts, in a partial juxtaposition to scientific or mathematical concepts (Deleuze and Guattari 1994, p. 24–25). This study extends this concept of concept to physical and scientific, as well as mathematical concepts. It is not only that, in view of the fact that in physics, beginning with Aristotle (Aristotle 1984), a physical concept often contains philosophical components, to which the definition of concept about to be offered would apply, in accordance with Deleuze and Guattari's view. Rather a given physical or mathematical concept has the same type of architecture even in the absence of philosophical components being part of it. When present, either in physics (where concepts often have physical, mathematical, and philosophical components) or mathematics (where concepts are, more commonly, purely mathematical), a philosophical component of a physical or mathematical concept is only part of its architecture, although this component, too, is usually a concept and, thus, has the same type of conceptual architecture.

The definition of concept adopted by this study is as follows. A concept is not only a generalization from particulars (which is commonly assumed to define concepts) or merely a general or abstract idea, although a concept may contain such generalizations and abstract, specifically mathematical, ideas. A concept is a multicomponent entity, defined by the *organization* of its components, which may be general or particular, and some of these components are concepts in turn. It is the relational organization of these components that defines a concept. Consider the concept of "tree," even as it is used in our daily life. On the one hand, it is a single generalization of all (or most) particular trees. On the other hand, what makes this concept that of "tree" is the implied presence of further elements, components, or subconcepts, such as "branch," "root," "leaf," and so forth, and the relationships among them. The concept of tree acquires further features and components, indeed becomes a different concept, in botany. Botanically there are "trees" that could hardly fit the concept as it is commonly understood in daily life. This is characteristic of scientific use of concepts or just terms derived from the concepts of daily life, arguably especially in quantum theory. Thus, as Bohr emphasized, in referring to his use of such terms in his interpretation of quantum phenomena and quantum mechanics, "words like 'phenomena' and 'observations,' just as 'attributes' and 'measurements,' are used [here] in a way hardly compatible with common language and practical definition," although they may have been originally derived from this language and definition (Bohr 1949, 1987, v. 2, pp. 63–64).[2] The situation is even more pronounced in the case of complementarity, a word that does not appear to have been previously used, as *a noun* (as opposed to the adjective "complementary"), and was introduced by Bohr, as a new term, to designate a new concept. As he said: "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, p. 87). The statement tells us that complementarity is a new physical concept, "which does not belong to our daily concepts," and which must, accordingly, be understood in the specific sense Bohr gives it.

Simple, single-component, concepts are rare, if possible at all in rigorous terms, as opposed to appearing as such because their multicomponent structure is provisionally (or sometimes uncritically) cut off. In practice, there is always a cut off in delineating a concept, which results from assuming some of the components of this concept to be primitive entities whose structure is not specified. These primitive concepts could, however, be specified by an alternative delineation, which would lead to a new overall concept, containing a new set of primitive (unspecified) components. The history of a given concept, and every concept has a history, is a history of such successive specifications and changes in previous specifications.

The same type of process defines the history of a given theory, which may be seen as an organized assemblage of concepts, modified in the course of its history, for example, from Galileo to Newton and then to Lagrange and Hamilton in classical mechanics, or from Heisenberg and Schrödinger to Dirac and then to von Neumann in quantum mechanics. The history of a given theory is also defined by the history of its interpretations. The history of quantum mechanics has been that of a seemingly uncontainable proliferation of its interpretations, multiplying even within each type.[3] "An organized assemblage of concepts" will serve here as the definition of a theory, provided that the term concept is understood in the sense just defined. A theory relates to certain manifolds of phenomena or (they are, again, generally not the same) objects, which form the "reality" considered by this theory.

All modern, post-Galilean, physical theories establish such relations by means of the mathematical models they contain. I define a mathematical model, which is the

[2] Throughout this study, Bohr's key essays will be cited here, with their original publication date, from *The Philosophical Writings of Niels Bohr* (Bohr 1987), accompanied by the volume number, e.g., (Bohr 1949, 1987, v. 2, p. 40). These works are also listed separately in "References."

[3] It is not possible to survey these interpretations here. Just as does the Copenhagen interpretation, each rubric, on by now a long list (e.g., the many-worlds, consistent-histories, modal, relational, transcendental-pragmatist, and so forth) contains numerous versions. The literature dealing with the subject is immense. Standard reference sources, such as *Wikipedia* ("Interpretations of Quantum Mechanics"), would list the most prominent such rubrics.

only kind that this study considers, as a mathematical structure that enables a theory to establish such relations. These relations may be descriptive or representational, and derive their predictive capacity from their representational nature, as in the case of models used in classical mechanics, or they may be strictly predictive, without being representational, as in quantum mechanics, at least, again, if interpreted in the spirit of Copenhagen. A theory always involves an interpretation of the model or models it uses by virtue of giving a physical meaning to them, for example, again, by establishing the way in which its models relate to the observed phenomena or objects considered. A model has to be a model of something, even in mathematics or mathematical logic, where the concept of a model, while generally in accord with the present understanding, has additional specificity, technical and philosophical. This is because mathematical reality is phenomenal or, given that some of it may not be available to our phenomenal experience, mental. At least it is usually assumed to be (there are exceptions). I can, however, only mention the subject in passing here. "Realist" and "nonrealist," too, are interpretive conceptions, which may be adopted by different theories using the same model. For simplicity, I shall also speak of the corresponding interpretation of the theory containing a given model, interpreted by the theory, such as quantum mechanics, although, rigorously, a different interpretation defines a different theory.[4]

[4] As other major concepts discussed here, the concept of a mathematical model has a long history and is the subject of diverse and often diverging definitions, and interpretations of such definition, and literature on the subject is, again, extensive. It is not my aim to discuss the subject as such or, accordingly, engage with this literature, which would not be possible within my scope. It is also not necessary. The present concept of a model, while relatively open, is internally consistent and is sufficient to accommodate those models that I shall consider or the concepts of a model used, expressly or implicitly, by the key figures I shall discuss. We often gain from their work a deeper understanding of concepts, such as principle or model, even when they are not expressly defined. I would argue that this is especially so in the case of the nonrealist models considered by Bohr and Heisenberg, even though neither speaks of models, as defined by a rigorous concept (on can find casual uses of the term in their works). But that does not mean that such a concept, or thinking that in effect uses such as concept, is not found in their work. Some of the works generally addressing the question of reality and representation and cited below (note 5) address the subject. See also (Frigg and Hartmann 2012) and further references there, mostly on lines of analytic philosophy. In his recent works (e.g., Frigg 2010), Frigg considers the promising subject of the relationships between scientific models and literary fiction. Unfortunately, his analysis of literary fiction is too narrow and, beginning with his choice of literary works (David Lodge's *Changing Places*, a very conventional realist novel, hardly offers a real complexity here), bypasses the opportunity that literature can offer in exploring deeper complexities of the subject, sometimes indeed approaching those of quantum theory. I am thinking of such figures here as F. Kafka, J. Joyce, R. Musil, S. Beckett, or more recently T. Pynchon. In fairness, Frigg does not address quantum theory, apart from a brief (and in my view, problematic or, in any event, insufficiently qualified) remark that while "on the current view, the classical and the quantum model are not identical, the worlds of these two models—the set of all propositions that are fictional in the two models—are identical" (Frigg 2010, p. 262, n. 20). His analysis is limited to realist models and realist fiction, again, fiction that is far from the most sophisticated variety even in that category and, as such, in my view, not really suited for exploring mathematical modeling in quantum theory. I would argue, however, that quantum-theoretical, or, beyond physics, quantum-theoretical-like, thinking, is a juncture where the real depth and complexity of literature, philosophy, and physics meet. The subject is, however,

By a "quantum theory" I refer to any theory accounting for quantum phenomena, among them the standard quantum mechanics (introduced by W. Heisenberg and E. Schrödinger in 1925–1926), with which and the model comprised by its mathematical formalism I shall be primarily concerned here, and which is henceforth designated as "quantum mechanics," as against, for example, "Bohmian mechanics." The latter (in any of its versions) is a theory defined by a mathematically different model of reality, rather than a different interpretation of the standard quantum-mechanical formalism. I shall also discuss high-energy quantum theories, such as quantum electrodynamics and quantum field theory (in their currently standard forms) and finite-dimensional quantum theories (corresponding to discrete quantum variables, such as spin), primarily used in quantum information theory. By quantum phenomena, I refer to those physical phenomena in considering which Planck's constant h must be taken into account, and by quantum objects those entities in nature that, through their interactions with measuring instruments (or what function as such), are responsible for the emergence of quantum phenomena. By "quantum physics" I refer to the totality of quantum phenomena and experiments concerning them (experimental quantum physics) and quantum theories (theoretical quantum physics). The terms "classical phenomena" and "classical objects" (the difference between them, while still present, could be neglected in classical physics, unlike in quantum physics), "classical mechanics," "classical theory," and "classical physics," will be used in parallel.

I would like now to discuss, in a preliminary fashion (leaving a more rigorous treatment to the subsequent chapters), some examples of physical concepts. My first example is the concept of a moving body in classical mechanics. It has multiple components (physical, mathematical, and philosophical), beginning with the concept of motion, defined by such component concepts as position and velocity or momentum, mathematized by means of differential functions of real variables. This concept has its history, extending even to the pre-Socratics, but particularly to Aristotle's concept of motion, some elements of which are found in classical physics and relativity, although not in quantum mechanics (apart from the classical description of measuring instruments). Admittedly, Aristotle's concept lacks the mathematical architecture found in the concept of motion as defined by modern classical physics from Descartes, Galileo, and Newton on, which architecture defined a new concept of motion. However, as was noted by Bohr and Heisenberg, both of these physical concepts retained their connections to the daily-life concept of motion, which they refined, in the second case, also by giving it a mathematical architecture (Bohr 1954b, 1987, v. 2, p. 72; Heisenberg 1930, p. 11). As will be seen presently, these connections are important for defining and understanding a specific class of realist models—the visualizable models of classical mechanics, such as those of Descartes, Galileo, and Newton, which are essentially geometrical, although they have algebraic components. Classical mechanics, however, also has more

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beyond my scope here. I have considered it (by way of a preliminary approach) in (Plotnitsky 2012b). On the subject of quantum-like modeling beyond physics, see (Haven and Khrennikov 2013) and (Plotnitsky 2014).

strictly algebraic and nonvisualizable models, such as those of analytical (Lagrangian or Hamiltonian) mechanics.

Einstein's concept of relativistic kinematics in special relativity was a new, revolutionary physical concept, which implied a radical departure from the classical physics concept of motion, because of a new law for the addition of velocities,which becomes relevant when these velocities are close to c, $s = \frac{v+u}{1+\left(vu/c^2\right)}$. The physical motion defined by this law, again, when the velocity is close to c or is c, as in the case of photons (the behavior of which is particularly strange), has no counterpart in our phenomenal intuition. As such, it also reflects a radical change in our physical, as well as philosophical, understanding of space and time, and leads to a fundamentally different physics. This concept of motion is no longer a (mathematical) refinement of a daily concept of motion in the way the classical concept of motion is. The nature of motion defined by this law, and especially the behavior of photons, is not visualizable. In the present view, photons, as ultimately quantum objects, are beyond representation or even conception in any event. However, even within the limits of special relativity, the behavior of photons is remarkable, and I shall further comment on it presently. Analytical mechanics, Lagrangian and Hamiltonian, already moved beyond this refinement by virtue of its abstract, essentially algebraic, character (although it has an abstract geometry of symplectic manifolds, with which phase spaces are associated). Both versions, however, presupposed that the motion of *individual* classical objects is physically classical and is visualizable (the concept defined below). Accordingly, unlike relativity, analytical mechanics does not imply any change of the classical physical concept of motion, but only in the mathematization of this concept.

Einstein's concept of gravity in general relativity, mathematically represented by Riemannian manifolds of (a crucial point!) *variable curvature*, defined by the presence of matter (including that of fields), was another major new concept introduced by Einstein. The architecture of this concept is complex in its physical, mathematical, and philosophical aspects, and its history extends to both Galileo, as concerns the equivalence principle, and the Leibniz-Newton debate concerning the nature of space and time, and to the history of non-Euclidean geometry, which B. Riemann gave its proper mathematical foundations. Riemann's aim was a rethinking of the foundations of geometry in general, which made non-Euclidean geometry, or indeed Euclidean geometry, merely a particular case of a new general *concept* of geometry (in the present sense of concept). Riemann also rethought the relationships between physics and spatiality and geometry, along lines that were not that different from Einstein's thinking, as was noted by Einstein himself. Riemann cannot be said to have come anywhere close to Einstein's general relativity. Nevertheless, Riemann's thinking concerning these subjects was momentous. The formalism of general relativity, its mathematical model, could be seen as yet another form of Lagrangian formalism, first given to general relativity by Hilbert's derivation of Einstein's equations of general relativity. The theory could also be given a Hamiltonian formulation.

One would not want to bypass Einstein's arguably greatest contribution to quantum theory (although only one of his several momentous contributions to it), the concept of the photon as a particle of light, previously believe to be a continuous entity. The motion of the photon was, as already noted, also a new concept, because in special relativity, it is impossible to associate classical spatial or temporal concepts with a moving photon (there are no other photons). 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, again, no other photons), as does the concept of frame of reference, otherwise crucial to special relativity, when applied to moving systems other than photons. Indeed, this concept is one of the defining concepts of physics and all modern science. Putting the quantum nature of the photon aside for the moment, the photon's motion in special relativity cannot, thus, be captured by a visualizable realist model but only by an algebraic representational realist model. One wonders what concept of motion still applies in this case. A photon does get from one point to another (with a speed equal to c in a vacuum), but it is difficult to conceive how it does so. As will be seen in Chap. 6, quantum field theory gives us a more consistent way of thinking concerning this situation.

Heisenberg's concept of quantum variables, as infinite unbounded matrices with complex elements [in effect, operators in a Hilbert space over complex numbers, in a more rigorous formalism established by von Neumann a bit later (von Neumann 1932)], is fundamentally different from the representational concepts of classical physics or relativity. It was the first physical concept of this kind, and it was expressly intended to be developed as such. It was defined by making each such variable a mathematical entity enabling only the probabilistic predictions concerning the relationships between *quantum phenomena*, observed in measuring instruments, without providing a mathematically idealized description or representation of the behavior of *quantum objects* responsible for the appearance of these phenomena. This is in accord with Bohr's concept of quantum phenomena, defined as what is observed in measuring instruments and thus as irreducibly different from quantum objects, which are never observable as such. Nobody has ever seen, at least thus far, a moving electron or photon. It is only possible to observe traces of this "movement" (assuming even this concept applies) left in measuring instruments, traces that do not allow us to reconstitute this movement in the way it is possible in classical physics or relativity, an impossibility reflected in Heisenberg's uncertainty relations.

Mathematically, an especially novel feature of Heisenberg's variables was that, in general, they did not commute, that is, the product of PQ was, in general, not equal to QP: $PQ - QP \neq 0$. This feature eventually came to represent Heisenberg's uncertainty relations constraining certain simultaneous measurements, most notable, those of the momentum (P) and the coordinate (Q), associated with a given quantum object in the mathematical formalism of quantum mechanics and (correlatively) the complementary nature of such measurements in Bohr's sense. The physical interpretation of both is a complex matter that I shall discuss further in the next chapter, merely noting here that it is fundamentally linked, by the QP/QS principle, to the probabilistic and statistical nature of our predictions concerning quantum phenomena, including those associated with elementary individual quantum objects.

For the sake of economy and in accordance with the currently accepted view, I shall henceforth refer to such objects just as "elementary particles." I shall explain this concept in more detail and, *to the degree possible*, "define" it later in this study, most especially in Chaps. 3 and 6. I qualify because, if there is a generally accepted use of the term, there is no generally accepted concept thus designated. A relatively common, although not a universally shared, view is that we still do not have such a concept. If one follows a nonrealist interpretation, objects that are designated as elementary particles cannot be given a representation and hence a specific concept, any more than other quantum objects, in accordance with quantum theory in its current form, or may even be beyond the capacity of our thought altogether, which is the view adopted in this study. This inconceivability need not mean that elementary particles are not different from other quantum objects. In particular, their elementary character is defined by the fact that there is no experiment that allows one to associate the corresponding effects of their interactions with measuring instruments with other elementary quantum objects or more elementary individual quantum objects, of which a given elementary particle would be a composite. The status of a given particle may change from an elementary to a composite one (as it happened in the case of protons or neutrons, which revealed themselves as composites of quarks and gluons), once such an experiment is performed. The main point is, again, that elementary particles, their reality, could not be given realist representations by means of the available mathematical models of quantum theory. An elementary particle could only be defined in experimental-technological terms of the effects of its interactions with measuring instruments, effects that we can predict (probabilistically) by using the formalism of quantum mechanics or quantum field theory, including specifically such mathematical technology as symmetry groups. The same applies to the concept of a quantum field. As will be seen in Chap. 6, this concept is necessary in order to understand how quantum reality works (in terms of its effects on measuring technology) in high-energy quantum regimes, where one can no longer maintain the identity not only of a given particle but also of the *type* of a given particle even in a single experiment. Elementary particles can disappear and new ones can be born.

For the moment, Heisenberg referred to his new variables and their predictive functioning thus defined as "new kinematics," which was not the best term to use under the circumstances. As its etymology suggests, kinematics conventionally refers to variables representing the motion of physical objects, as it does in both classical physics and relativity, even though, as just explained, the kinematics of relativity is fundamentally different from that of classical physics. Thus, while as in classical physics and relativity, the architecture of Heisenberg's concept of quantum variables was jointly physical, mathematical, and philosophical, the relationships between these components were different in Heisenberg's scheme of quantum mechanics from those of classical mechanics and relativity. In these theories their mathematical formalisms represent, in general causally, the behavior of the corresponding objects in space and time, albeit by way of idealized models. In Heisenberg's scheme the mathematical concepts comprising the formalism or model of quantum mechanics only related, in terms of probabilistic predictions, to what is observed, under the impact of quantum objects, in measuring instruments.

The latter or, more accurately, their observable parts are described by means of classical physical concepts, although these concepts, it follows, cannot represent, any more than any other concepts, how such observed effects come about, nor predict these effects. As Heisenberg commented shortly before his paper introducing quantum mechanics was published: "What I really like in this scheme is that one can really reduce *all interactions* between atoms and the external world … to transition probabilities" (W. Heisenberg, Letter to R. Kronig, 5 June 1925; cited in Mehra and Rechenberg 2001, v. 2, p. 242; emphasis added). These are the probabilities of transitions from one event observable in a measuring arrangement to a possible future event observed in another measuring arrangement, or possibly between two different events observed in two different parts of the same arrangement.

This epistemology, including the difference, thus irreducible, between quantum objects and quantum phenomena, became the core of Bohr's interpretation of quantum phenomena and quantum mechanics as complementarity, an interpretation that in large part stemmed from Heisenberg's thinking just described, but that was developed under the impact of Bohr's subsequent exchanges with Einstein. Bohr's concept of quantum objects defines them, via the RWR principle, as unavailable to any representation or possibly even conception, although it is, again, not clear whether Bohr would have been willing to go that far. This fact gives this concept a special status insofar as this concept has no conceptual architecture that could be determinately ascertained, or in the second eventuality possibly in principle be applicable to it in any way. Phenomena, by contrast, are defined by what is actually observed, as effects of the interactions between quantum objects and measuring instruments, in measuring instruments and are thus subject to a classical description, under certain constraints, such as those defined by the uncertainty relations. Considered in their own right, the observable parts of measuring instruments are described by classical physics and its concepts. (They also have quantum and, hence, unobservable parts, through which they interact with quantum objects.) In classical physics, specifically in classical mechanics, this difference between objects and phenomena, while still, technically, valid, as defined, for example, on Kantian lines, can be disregarded, and although ultimately we only deal with phenomena, objects and phenomena can be treated as the same. Correlatively, the behavior of objects could be treated as independent of observation, because the interference of measuring devices, beginning with our bodies, into the process of observation or measurement can be neglected or compensated for (Bohr 1954a, 1987, v. 2, p. 72).

As in Heisenberg's original scheme, in Bohr's interpretation, the mathematical concepts comprising the formalism of quantum mechanics only relate, in terms of probabilistic or statistical predictions, to what is observed in measuring instruments, even, in contrast to classical mechanics, in dealing with elementary individual quantum processes and events (those concerning individual elementary particles). This situation is correlative to the epistemology just described, as defined by the fact that, unlike in classical mechanics, one cannot obtain any information (including that defined by probabilistic predictions) about quantum objects without interfering appreciably in their behavior. This makes all such information, including our predictions (which may be seen as probabilistic or statistical information) defined by the effects of the interaction between quantum objects and measuring instruments.

In this regard, as Bohr emphasized, the recourse to probability or statistics in quantum theory is essentially different from the use of probability and statistics in classical statistical physics, or in dealing with such events as a coin toss. There this use is necessitated by the mechanical complexities of the systems considered, the behavior of which, while it cannot be tracked in practice, emerges from the behavior of the individual constituents that is causal and, in the first place, amenable to a realist representation by means of classical mechanics. In quantum physics, in nonrealist interpretations, the behavior of the ultimate constituents of matter is not amenable to a realist representation (the RWR principle) and, as a consequence, is never causal, which makes the recourse to probability unavoidable in these cases (the QP/QS principle). This is in accord with the experimental evidence as it currently stands, but this evidence as such does not necessarily imply nonrealism, and in principle allows for realist or causal theories, including the corresponding interpretation of quantum mechanics.

As my final example, I would like to mention, by way of a contrast with Heisenberg and Bohr, Schrödinger's concept of the wave function, initially developed by Schrödinger as a representational concept, in a deliberate juxtaposition to Heisenberg's scheme just sketched. While the key mathematical aspects of this concept have survived and become indispensable in quantum mechanics, its representational nature and Schrödinger's wave mechanics as a whole, as a representational theory, have proven difficult to sustain. Schrödinger himself abandoned his initial project of wave mechanics shortly thereafter, although he eventually tried to return to it in the 1950s (Schrödinger 1995). The concept was converted by others, thinking in the spirit of Copenhagen, into a predictive concept of the Heisenbergian type. In the process, the mathematical model developed by Schrödinger received a nonrealist interpretation as well. This was not surprising because Heisenberg's and Schrödinger's formalisms were quickly shown to be mathematically equivalent as concerns their predictions. Schrödinger was among the first to do so. The transformation theory of Dirac and Jordan, developed shortly thereafter, brought them more rigorously together in mathematical terms.

1.2.2 Reality and Realism

I shall now define the concepts of reality and realism, in the case of realism, two sets of concepts. Admittedly, these concepts could be, and have been, defined in a great variety of ways, and they have been discussed and debated from the pre-Socratics on, and with a new vigor and from a new, revisionary set of perspectives during recent decades.[5] The definitions given here will not be able to capture all conceivable

[5] I am referring to the work of T. Kuhn, I. Lakatos, P. Feyerabend, and their followers in the so-called constructivist studies of science. The literature on these subjects (both more traditional, such as that in the analytic philosophy of physics, and more "revisionist," such as representing the work just mentioned) is massive. To give a few representative references, for analytic-philosophical approaches, see (Pincock 2012) and (van Frassen 2008); for more restrained post-Kuhnian

concepts thus designated, which is, again, impossible in any event, and I can only address the discussions and debates surrounding them in a limited fashion. Nevertheless, the concepts of reality and realism defined here are, I believe, sufficiently general to encompass a large spectrum of both concepts currently used in physics and the philosophy of physics. I will indicate some among more specific versions of these concepts as I proceed. In addition, this introductory discussion will, hopefully, help to avoid misunderstandings concerning this study's use of these concepts. Many of these definitions are sufficiently general to apply beyond physics. I shall, however, not be concerned with such extensions, except insofar as physics requires them or insofar as they shed light on physics.

By "reality" I refer, very generally, to that which actually exists or is assumed to exist, without any claims concerning the character of this existence. I understand existence as a capacity to have effects on the world with which we interact, the world that has such effects upon itself. In the case of physics, it is nature or matter that is generally, but not always (although exceptions are rare), assumed to exist independently of our interaction with it, and to have existed when we did not exist and to continue to exist when we will no longer exist. This assumption also holds in nonrealist interpretations of quantum mechanics, in these cases in the absence of a representation or even conception of the nature of this existence. The existence or *reality* of quantum objects, thus placed beyond representation or even conception, is inferred from effects they have on our world, specifically on experimental technology.

I define realism as a specific set of claims concerning what exists and, especially, *how* it exists, provided by the corresponding theory, by which I, again, mean an organized assemblage of concepts, accounting for the phenomena considered. In this definition, any form of realism is more than only a claim concerning the existence, *reality*, of something, such as physical objects, which we can represent, at least ideally, in classical physics, or quantum objects, about which nothing else could be said or even thought, if one's interpretation of quantum phenomena is nonrealist. Instead, realism is defined most essentially by claims concerning the *character* of this existence. Realist theories are sometimes also called ontological theories. The term "ontological" may carry additional philosophical connotations, with which I shall not be concerned here. Accordingly, the terms "realism" and "ontology" will, unless qualified otherwise, be used interchangeably. The extent of such claims or their degree of specificity, for example, concerning what is possible "in principle" or only "in practice," may vary. Nevertheless, as they are understood here (which understanding, I would contend, subsumes a large spectrum of realist theories), realist theories may be grouped in the following two types. *The first type* is *representational*: it assumes that the corresponding realist theories offer (usually idealized) *representations* of reality, comprised of the objects and processes these

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approaches, see (Cartwright 1983) and (Hacking 1983); in the context of the relationships between classical and quantum physics (Cartwright 1999, pp. 177–233); and for more radically constructivist treatments, see (Galison 1997; Latour 1999; Hacking 2000). These works also contain further references. Standard reference sources, such as the *Stanford Encyclopedia of Philosophy*, contain helpful articles (with extensive bibliographies) on these concepts as well.

theories consider. *The second type of realism* refers to theories that would presuppose an independent architecture (which may be temporal) of reality governing the behavior of the ultimate objects they consider, while allowing that this architecture cannot be represented, even ideally, either at a given moment in history or perhaps ever, but if so, only due to practical limitations.

One could, in principle, see a claim concerning the existence or reality of something to which a theory relates in any way, even without representing it, as a form of realism or ontology. This use of either term is sometimes found in advocating interpretations of quantum mechanics that are nonrealist in the present sense, and hence do not claim that quantum-mechanical formalism represents, even ideally, the behavior of quantum objects and processes, but only serves to predict the outcome of quantum experiments. However, placing reality outside—and thus, again, divorcing reality from—realism is in accord with a more common use of the term realism or ontology, and is advantageous in considering nonrealist interpretations, in the present RWR-principle-based sense, of quantum phenomena and quantum mechanics. Conversely, certain interpretations of quantum mechanics that are realist in the present sense, specifically those of the second type, are sometimes called "nonrealist," which, it follows, presupposes a narrower conception of realism than the one I adopt, essentially equivalent to the first, representational, type of realism, as defined here.

According to the first type of realism, then, a realist theory would offer a representation, in modern physics typically a mathematically idealized representation, by means of a corresponding model of the objects considered and their behavior, or sometimes, as in the so-called structural realism, of the *structures* defining such objects and their behavior, rather than these objects and their behavior themselves. An idealized representation of this type retains some of the features of the actual objects and processes considered and disregards others, in particular those that cannot be mathematized. "The unreasonable effectiveness of mathematics in physics," of which E. Wigner famously spoke, is at least in part due to the fact that modern physics only considers what is amenable to a mathematical treatment (Wigner 1960). This makes this effectiveness hardly unreasonable, especially in classical physics. On the other hand, this effectiveness is enigmatic in quantum theory insofar as we do not know why quantum mechanics or quantum field theory has this extraordinary predictive capacity (Plotnitsky 2011b). The properties or, in Einstein's language, "elements of reality" ideally represented, *idealized*, by a given realist model or theory (Einstein et al. [hereafter EPR] 1935, p. 138), and the relationships among them are usually assumed, by realist models of this first type, to exist independently of our interaction with the objects in question. This assumption, central to Einstein, defined his criticism of quantum mechanics and his argument in his debate with Bohr.

The mathematical formalism of a realist theory of the first type comprises a *representational* mathematical "model" of reality. (Realist theories of the second type do not, by definition, contain such models.) Such a model is sometimes identified with this theory, although in this study a theory is, again, understood as having a broader conceptual architecture, especially as concerns interpretive and epistemological elements found in it, than does a model, here (and typically elsewhere) limited to the mathematical aspects of the theory. Quantum mechanics, or classical physics or relativity, is hardly limited to its mathematical formalism, for one thing, by virtue of an interpretation it requires. All modern, post-Galilean, physical theories, again, proceed by way of idealized mathematical models. Realist mathematical models are representational models—idealized mathematical representations of physical objects and processes considered, again, as part of a given theory, as an assemblage of concepts, which also gives an interpretation to the model it uses. The predictive nonrealist models of quantum mechanics considered here do not offer and even preclude such a representation, while their interpretations assume the reality of quantum objects and processes, as "reality without realism," in accordance with the RWR principle. As I said, for simplicity, I will also speak of an interpretation of the mathematical model provided by a theory as an interpretation of this theory.

All realism in physics (or elsewhere) is *conceptual* realism, as was acutely realized by Einstein, who, in a Hegelian vein, saw the mediation of mathematical concepts as essential, and the practice of theoretical physics as that of the invention of new concepts through which one can approach reality.[6] He argued that a viable realist representation of physical reality could only be achieved by means of conceptual construction, "the free choice of [mathematical] concepts," rather than by means of observable facts themselves. Einstein's argument is important because it leads to a questioning of the uncritical use of the idea of observation (Einstein 1949a, p. 47). I shall return to this argument in Sect. 1.3. I might add here that a theory always adds dimensions that are other than mathematical to its mathematical concepts or models, and concepts that are other than mathematical to the theory itself.

The first type of realism allows for different degrees to which our models "match" reality. As noted above, the question concerning this degree could be and, beginning at least with Kant, has been posed even in considering classical physics, where our idealizations are more in accord with our phenomenal experience. Thus, Schrödinger's account, arguably following H. Hertz, of the power of models in physics addressed the limitations of our capacity to represent, to have a picture, *Bild*, of the ultimate reality even if the latter is assumed to be classical-like in character, which Schrödinger preferred it to be (Schrödinger 1935a, pp. 152–153).[7] While Schrödinger was thus close to Einstein in preferring the "classical ideal" to what transpired in quantum mechanics, he was, especially in view of quantum mechanics, more skeptical than Einstein as concerns the future of this ideal in fundamental physics. The question has also been posed concerning the degree to which the mathematical architecture of relativity (more removed from this experience) corresponds to the architecture of nature, as opposed to serving as a mathematical model for correct predictions concerning relativistic phenomena (Butterfield and

[6] Mathematical realism, sometimes also known as mathematical Platonism, is something else, in part because it deals with mental rather than physical reality, and it will not be addressed here.

[7] Schrödinger clearly realized that the kinetic theory of gases, electrodynamics, and relativity already complicate the application of the classical ideal, as noted already by Maxwell (Maxwell 1879) and then (Einstein 1921). Both works, especially the second one, are likely to have been familiar to Schrödinger. See also (Gray 2008, pp. 306–328), for a useful discussion. These theories, however, allow one to retain realism and classical causality, and thus the classical ideal, unlike quantum mechanics, which made one, at the very least, doubt the possibility of doing so.

Isham 2001). In this case, these predictions are ideally exact, deterministic, as opposed to the probabilistic or statistical predictions of quantum theory, again, even in dealing with elementary individual quantum processes—a fundamental difference, on which I shall further comment below.

There is a particular type of representational models that I shall single out—*visualizable* models. The concept of visualization has various and sometimes divergent meanings. Feynman's diagrams are often seen as visualizations of quantum processes of high energy governed by quantum field theory. Helpful as they are, however, Feynman's diagrams are merely *diagrams*, heuristic devices: they do not represent the quantum processes to which they refer, even if one holds that these processes are *visualizable* or otherwise representable. The role of Feynman's diagrams may be said to relate one to the *formalism* of quantum electrodynamics or quantum field theory, and thus to help one to work with this formalism in order to make probabilistic or statistical predictions concerning the outcomes of the experiments these diagrams are connected to. This, however, is quite different from representing or visualizing quantum processes themselves. Bohr's concept of complementarity, too, is sometimes seen as "regaining" the "lost visualization" in quantum mechanics (Miller 1978; Gieser 2005, pp. 68–69). I would argue that this is a misconception, certainly as concerns the behavior of quantum objects; and Bohr himself never spoke of and never saw complementarity in this way. Complementarity relates to visualizable phenomena observed in measuring instruments, but it does not provide any visualization or other representation of quantum objects and processes. There are other heuristic visualization tools used in quantum theory, on some of which I shall comment later in this study (e.g., Heisenberg 1930, p. 105; Coecke 2009). This is, however, not the kind of visualization that I have in mind.

By visualization, I mean the possibility of a (phenomenally) visually configurable geometrical representation, a picture, of the behavior of an individual physical object in classical mechanics, paradigmatically represented by the motion of a material body, idealized as a dimensionless massive point, obviously something that does not exist in nature, as far as we know. Bohr sometimes also speaks of a pictorial representation or pictorial visualization, with the German *Anschaulichkeit* (intuition) in mind. This idealization is a mathematical refinement of our daily perception and our visualizing representation of motion, which is also more amenable to being translated into a verbal description, although visualizable models are still conceptual models. It is true that there are classical systems the behavior of which and hence the corresponding models cannot be thus visualized, for example, multicomponent systems of great mechanical complexity, such as those considered in classical statistical physics. The corresponding mathematical models are statistically predictive, rather than representational. On the other hand, models of analytical mechanics or of chaos and complexity theories are representational, but they are not, in general, visualizable, although there are computer-generated partial visualizations of such models. Chaos theory is famous for its computer images and was advanced immeasurably through digital technology. In all of these cases, the individual behavior of the ultimate constitutive objects from which the behavior of these systems (all of them are ultimately composite) emerges is represented by the model of classical mechanics and, as such, is locally or infinitesimally visualizable, even though this visualization may be approximate.

In classical mechanics, this visualization is linked to the possibility of ideally representing the motion of classical objects by means of differential calculus. Indeed differential calculus may be seen as reflecting the possibilty of a geometrical visualization of the continuous or, more accurately, differential motion of an idealized material point, along a straight or curved trajectory. In fact, this motion, again, at the level of this idealized model, can be considered as infinitesimally linear, which circumstance led Newton to the invention of calculus, although he was compelled to recast his argument, initially developed via calculus, in terms of Euclidean geometry in *Principia* (Newton 1999 [1687]). One can, literally, draw a picture of such motion (as a small curve with a tangent vector as a given point on it) on a blackboard, actual or the one we envision in our mind, which also allows for a verbal description of this motion, making such models more descriptive. Accordingly, when speaking of descriptive models or theories, I shall primarily refer to visualizable models or theories, such as and in particular, classical physics. In this view, visualization is in effect equivalent to the applicability of the concept of classical motion as infinitesimally represented by differential functions of time and coordinates (as real variables).

This representation implies that position and velocity (or momentum) are both defined and can ideally be known at any moment of time, which, at least as concerns the knowledge of both, is impossible in the case of quantum objects, due to the uncertainty relations. If one adopts a nonrealist interpretation, no representation or even conception of quantum objects and their properties or behavior, either as continuous (or differential) or discrete, is possible, even in the case of elementary individual quantum objects and processes. Discreteness only occurs, by the QD principle necessarily, at the level of quantum phenomena observed in measuring instruments. Nor is there a physical continuity as concerns the behavior of quantum objects, comparable to that defining particle or wave motion in classical physics and represented there by differential functions of real variables. Quantum-mechanical mathematical formalism does involve differential calculus in Hilbert spaces over complex numbers, without, in nonrealist interpretations, assuming that this formalism and thus this calculus represent any physical reality. This formalism only relates (via Born's or analogous rules) to the probabilities or statistics of the outcomes of possible future quantum experiments on the basis of the data obtained in previously performed quantum experiments. Nonrealism, defined by the RWR principle, implies an exclusion of any representational models, defining realism of the first type, as well as realism of the second type about to be defined (which does not contain representations models), rather than only an exclusion of visualizable models, which is automatic once representational models are excluded. I shall, however, also refer to the impossibility of visualization in quantum theory in the context of Bohr's argumentation, because Bohr uses this language, again, with the German *Anschaulichkeit* (intuition) in mind. Bohr did not consider, at least not expressly, nonvisualizable realist models, perhaps because such models, such as those of classical statistical mechanics, are underlain by visualizable models, representing the behavior of the ultimate constituents of the systems considered by the corresponding theories. However, his interpretation itself of quantum phenomena and quantum mechanics clearly places quantum objects and processes, including, again, elementary ones, beyond representation, if not (Bohr, again, might not have been willing to go as far) beyond conception, rather than merely beyond visualization.

I now turn to *the second type of realism*. This type of realism would presuppose an independent architecture (which may be temporal) of reality governing the behavior of the ultimate objects considered by the corresponding theory, even if this architecture cannot be represented, however ideally, either at a given moment in history or perhaps ever, but if so, only due to practical limitations. In the first of these two eventualities, a theory that is merely predictive may be accepted for lack of a realist alternative, but under the assumption or with the hope that a future theory will do better, in particular by virtue of being a realist theory of the first type, just considered. Einstein adopted this attitude toward quantum mechanics, which he expected to be eventually replaced by such a realist theory, ideally, a field theory of a classical-like type, on the model of Maxwell's electrodynamics. Einstein's general relativity followed this program, as did his subsequent work on the unified field theory, and he saw the development of such a model as imperative in dealing with the ultimate constitution of nature, as against Heisenberg's "purely algebraic method" in quantum mechanics (Einstein 1936, p. 378). Even in the second eventuality, the ultimate constitution of nature is customarily deemed to be conceivable following realist models of classical physics, possibly adjusted to accommodate new phenomena, such as electromagnetism, and new concepts, such as field, classical, or possibly even quantum, or more recently automata. Relativity follows classical physics as concerns realism and causality, although, as explained earlier, not (or at least not entirely) visualizability. In general, however, the second type of realism implies that a proper theory or model of reality is not available and may not be available. If it becomes available, the situation reverts to the first type of realism.

What, then, unites both conceptions of realism and thus defines realism most generally is the assumption that an *architecture* of reality, rather than only reality itself, exists independently of our interactions with it, or at least that the concept of architecture or structure would apply to reality. The latter presupposition, again, defines structural realism, although structural realism, generally, makes stronger claims concerning this structure. In other words, realism is defined by the assumption that the ultimate constitution of nature possesses attributes, "elements of reality," and the structured relationships among them that may be either (a) known in one degree or another and, hence, represented, at least ideally, by a theory or model, or (b) unknown or even unknowable. The difference between (a) and (b) is that between the two types of realism here defined. Both, however, allow for, and the second implies, the difference between "objects," defined as what ultimately exists in nature, especially in its ultimate constitution, and "phenomena," defined as what appears to our mind.

This difference, as the difference between noumena (things-in-themselves) and phenomena (appearances or representations constructed by our mind), grounds Kant's philosophy. An analogous type of difference also grounds Bohr's distinction between "quantum phenomena" (defined as what is observed in measuring instruments) and "quantum objects," which are unobservable and, by the RWR principle, unknowable and unrepresentable, and possibly inconceivable. This distinction, thus, follows Kant, but it takes a more radical form in Bohr's epistemology, even if, unlike this study, Bohr did not go as far as placing quantum objects and their behavior beyond thought. Kant's position may be seen as realist or allowing for the possibility of realism *in the present definition*, especially realism of the second kind, but at certain junctures also of the first kind. Admittedly, Kant's argument involves further epistemological complexities that may make its realist or nonrealist character a matter of interpretation or choice of terminology. However, most standard interpretations of Kant's argument that I have encountered appear to be realist in the present definition. I shall consider Kant's argument in more detail below.

As I indicated above, sometimes theories (or models they contain) defined here as realist theories of the second type, are seen as "nonrealist." Such theories are, however, different from theories that are nonrealist in the present sense, theories based on or interpreted by using the RWR principle, because they at least allow that the reality they consider, while unknown or even unknowable, is still conceivable in certain organizational, architectural terms. As I also noted, however, sometimes nonrealist theories in the present sense are, conversely, seen as realist, because they assume the existence of reality, however much beyond representation or even conception this reality might be.[8] One could, following G. Berkeley, also understand nonrealism as the denial of the existence of external reality or matter altogether. This conception, used by Berkeley against Newton, is not without relevance to quantum theory.[9] It is, however, rarely adopted and will be put aside. As defined here, realism at the very least assumes that the *concepts* of independent properties and organization can in principle apply to reality, no matter how much off the mark at a given point or even ever our conceptions of this constitution may be. However, the hope of most proponents of realism is that our theories will eventually capture something of this architecture, along the lines of the first type of realism, as defined here. In addition, more commonly than not, this architecture is conceived on "the classical ideal," which entails that the behavior of the ultimate constituents of matter could be ideally represented, moreover causally, by means of mathematical models, particle or field based, following, respectively, classical mechanics or electromagnetism.

Interpretations of quantum phenomena and quantum mechanics in the spirit of Copenhagen, beginning with that of Bohr (in its ultimate version, developed in the late 1930s), not only do not make any of the realist assumptions just considered, but also, in Bohr's language, "*in principle* [exclude]" them (Bohr 1949, 1987, v. 2,

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[8] I. Hacking's concept of "entity-realism," defined vs. "theory realism," and some of its avatars, developed during the last decades in the philosophy of science and debates concerning the question of reality there (as in the works cited in note 5), may be considered from this perspective (Hacking 1983). I would argue that these concepts still appear to ultimately conform to Kant's epistemology of noumena, which are, while unknowable, still in principle thinkable.

[9] For example, if the reality of quantum objects or processes is entirely beyond conception, in what sense could it be seen as "exterior" to the human mind, given that to "be exterior to something" is a conception?

p. 62). While upholding the reality itself of quantum objects and processes (as these interpretations do), this exclusion divorces reality from realism, and defines this reality as a reality without realism, in accordance with the RWR principle. Several important qualifications are in order, however.

The RWR principle can assume three forms, implicit in the preceding discussion but not expressly stated there. The first, the weakest, form of the principle, disallows a *representation* or *analysis* of the ultimate nature of physical reality (assumed to be quantum) *by means of quantum mechanics* in any interpretation. The second, stronger, form of the principle disallows such a *representation by any means*. The third, the strongest, form of the principle disallows even a *conception* of this reality, thus placing it beyond thought altogether. The last two forms of the principle are independent of the theory *predicting* (such a theory can do no more, if the principle applies) what is observed in quantum experiments. Thus, the principle pertains to the corresponding *interpretations of quantum phenomena* themselves, rather than of quantum mechanics, even though these interpretations may emerge, historically, in conjunction with certain interpretations of quantum mechanics, interpretations that and thus the RWR principle (in either of these three forms) are far from generally accepted. As I noted from the outset of this study, contrary to the claims of the dominance of the Copenhagen interpretation, which, again, does not exist as a single interpretation in the first place, the spirit of Copenhagen has always, even during Bohr's lifetime, reflected a minority view, regardless of which form of the RWR principle one assumed. Bohr clearly adopted at least the second form of the RWR principle, in part because he assumed that quantum mechanics is as complete a theory of quantum phenomena as nature allows us to have, which makes it a Bohr-complete theory, as things stand now. It follows that, as things stand now, if quantum mechanics cannot represent quantum objects and processes, no theory could represent them. At least, Bohr adopted this form of the principle in the ultimate version of his interpretation (earlier versions allowed for a partial realism). It is, again, not clear whether he subscribed to the strongest form of the RWR principle, as does this study, specifically in the statistical Copenhagen interpretation offered in Chap. 4. Bohr's position on this point is a matter of interpretation, given that there does not appear to be a statement to this effect in his oeuvre.

However, while these two forms of the principle are different *philosophically*, they are equivalent for all practical purposes. In order to see why this is the case, it may be helpful to revisit Kant's epistemology, arguably the weakest form of realism available, bordering on nonrealism. Kant places the ultimate (noumenal) reality beyond our knowledge or understanding based on this knowledge, both of which are associated strictly with phenomena, defined as appearances to our mind. While, however, *beyond knowledge*, Kant's noumena are *not beyond conception or even, in principle, representation*, insofar as this thinking is logical (Kant 1997, p. 115). Although a rigorous scientific theory in physics or elsewhere, even a nonrealist one, must of course be logical as well, in Kant's epistemology, as in all realism, this requirement gives the possible architecture of noumena and thus nature a logical character, a conception that is a product of human thought. It is true that we are capable of thinking in a logically consistent way beyond what we know or what is phenomenally available to our understanding. Kant associates this type of thinking with what he calls Reason [*Vernunft*], a higher faculty than Understanding [*Verstand*], which, again, concerns phenomena only. (I capitalize Reason and Understanding when using them in Kant's sense.) By contrast, Reason could in principle reach the ultimate nature of things. (There are further complexities to Kant's concept of Reason and its difference from Understanding, but they are not germane at the moment.) Kant recognizes that our thinking concerning the character of noumena or things-in-themselves may be erroneous, even if it works in practice. However, his view of the situation still logically implies that this thinking might also be correct, even though it may not be possible to definitively verify that it is. Indeed, Kant suggests that some claims of Reason are in fact determinately true. I shall, however, put this part of his argument aside, because it represents a form realism of the first type considered above and is of less interest at the moment than the more skeptical dimension of Kant's thought, which only allows that such claims may be true but are never certain to be true. As Kant also argues, under this latter assumption of only possible rather than determinate truth of our conception of the ultimate reality, that this conception need not be justified in theoretical terms: a practical justification, the workability of such a conception, assumed if not actual realism, may suffice (Kant 1997, p. 115). However, because such a conception might still be true, this view implies a *possibility* of realism, albeit never fully guaranteed or definitively verifiable.

On the other hand, it is also possible that, in the case of certain phenomena, no conceptions of the corresponding noumenal reality that are practically justified could be found, while, conversely, it is the *absence* of such a conception that is practically justified. Although the latter possibility is not considered by Kant, this is what happens in the case of quantum phenomena, according to Bohr's and other nonrealist, RWR-principle-based, interpretations. It is worth spelling out the logic leading to this conclusion in more detail. First, retracting a step, if one applies Kant's reasoning to quantum objects and processes, a conception that we may form of them (and nothing a priori prevents us from doing so) may be incorrect or it may not be possible to be certain whether it is correct or not. However, if this conception allowed us to predict correctly the outcomes of quantum experiments, it would be practically justified. Indeed, any realism, certainly in modern physics, even the most direct or naïve one, is only meaningful if it is justified in practice, specifically if the representation provided by a given theory or model enables effective predictions of either probabilistic or determinate character concerning future phenomena. Quantum objects qua quantum objects, beginning with elementary particles, and their independent behavior are experimentally unobservable, and are only inferred from the traces of their interactions with measuring instruments, traces found in these instruments. Hence any conception we form of them could only be verified indirectly. Stated in these terms, Bohr's argument is that, given the character of quantum *phenomena* and data observed in them, in particular the statistical or statistically correlated data, no representational conception concerning quantum objects or processes appears to be justified either theoretically or practically, at least as things stand now and if, in addition, one assumes locality. The latter qualification became crucial from the time of the EPR thought experiment (1935) and especially in view of more recent developments, such as Bell's and the Kochen-Specker theorems, and related findings. On the other hand, the absence of such a conception and, of course, the use of the mathematical formalism, the mathematical model, of quantum mechanics in predicting, again, probabilistically or statistically, the outcomes of quantum experiments is at least practically justified.

If, however, no representation of quantum objects and processes by means of quantum mechanics or in general is possible (as I explained, the first implies the second, if quantum mechanics is a correct and Bohr-complete theory), then it is also possible that quantum objects and processes are beyond conception, beyond thought altogether. Certain quantum-level "properties," that is, that quantum-level efficacies of certain quantum effects, such as and in particular spin are good indications of this possibility. This is the strongest form of the RWR principle, assumed by this study, but perhaps not by Bohr, who, as I said, at least does not expressly state this possibility. Either of these versions of the principle would, in principle, be consistent with Bohr's, arguably, strongest statement on the subject that an "*analysis*" of quantum phenomena beyond a certain point and, hence, of the ultimate nature of quantum objects and their behavior, is "*in principle* excluded"—an *analysis*, but not necessarily a conception (Bohr 1987, v. 2, p. 62). Bohr might have ultimately assumed the strongest form of the RWR principle, because of his emphasis on the lack of causality in quantum processes, if one agrees, and Bohr might, with Wittgenstein's contention in the *Tractatus* that we cannot conceive of processes that are not causal (Wittgenstein 1922, p. 179). On the other hand, this contention does prevent the possibility a mathematical scheme would represent quantum processes, perhaps noncausally. Naturally, these statements require a proper definition of the concept of causality, which I shall offer below. A summary definition, given in the Preface, will suffice for the moment: as classically understood (as it is here), causality implies, ontologically, that the state of a given system, as idealized by a given theory or model, is determined at all moments of time by its state at a particular moment of time. Determinism implies, epistemologically, that we can make ideally exact predictions concerning the behavior of causal systems, which is not always possible.

I am not saying that the strongest form of the RWR principle is logically or physically necessary, but only that it is *interpretively possible*. There does not appear to be any experimental data compelling one to prefer any of these three forms, or for the matter to definitively claim for any one of them beyond its interpretative consistency or effectiveness. This is why for all practical purposes all three are pretty much equivalent as far as physics is concerned, and are merely different interpretative inferences, all in the spirit of Copenhagen. They and the resulting interpretations are, again, different philosophically, and this is significant. Nevertheless, in the remainder of this study, I shall refer to the RWR principle as a single principle, unless a qualification concerning which form is used is necessary.

It merits additional emphasis that, in all of these cases, one still deals with interpretations of quantum phenomena and quantum mechanics, rather than with "the truth of nature." These truths, to the degree they are possible, are only represented by the data observed in quantum phenomena and the probabilistically or statistically correct predictions provided by quantum mechanics itself, or locality, again, as things stand now. Any of these interpretations is, however, logically consistent and complete (Bohr-complete, while being Einstein-incomplete, by definition) and is in accord with these "truths," which justifies, at least practically, the RWR principle in any of its versions. Any one of them could still be challenged even on the basis of the currently available data (how successfully is another matter, on which I shall comment later in this study), or, again, could eventually be disproven in view of new data.

That said, the reasoning based in the RWR principle (in either of its forms) is not "an arbitrary renunciation" of an analysis or the possibility of a realist representation of the ultimate constitution of nature underlying quantum phenomena (Bohr 1987, v. 2, p. 62). It is an *argument* that, on the basis of a rigorous analysis of quantum phenomena and quantum mechanics, one is compelled to conclude that this constitution *might be* beyond the reach of representation by means of quantum mechanics or otherwise, or even thought itself, as things stand now. This argument also allows one to offer a corresponding logically consistent and complete (Bohr-complete) interpretation of quantum phenomena and quantum objects, as different from quantum phenomena, and of quantum mechanics. In such an interpretation, accordingly, the reality of this constitution and hence of quantum objects is not something that is merely unknown or as yet unthought and that hence could in principle become known or thought one day, which view would, in the present definition, qualify as realism. The reality defined by the RWR principle, reality without realism, is ineluctably beyond representation and possibly thought itself: quantum objects are real and have effects on the world we observe, and yet they prevent us from representing them, either by the formalism of quantum mechanics or possibly otherwise, or even from forming any conception of them. Moreover, each encounter with this reality, say, in a given quantum experiment, is a singular, unique encounter, without assuming that there is some single, all encompassing reality which we always confront. While each time unknowable or unthinkable, this reality is each time different.

At stake, then, is a confrontation, dramatically enacted in the Bohr-Einstein debate, between two ultimately incompatible ways of thinking. The first defined by the principle of realism, and the second, defined by the reality-without-realism (RWR) principle, which leads to the conception of reality without realism, possibly a form of reality to the character of which no conception could be assigned. Most physicists and philosophers have always assumed that it is unreasonable to go that far in renouncing realism, which has been and remains so successful in physics apart from quantum mechanics, a resistance, again, well realized by Bohr (1987, v. 2, p. 63). The question is whether nature will ultimately allow for the first way of thinking to succeed, as Einstein hoped and even believed it will, or, as Bohr thought, that it might not, which is, again, not the same as to say that it will not. These two positions are, thus, not symmetrical because Bohr allowed, or at least so it appears (it may be a matter of interpretation), that a realist theory of quantum phenomena may be developed in the future, while Einstein believed that nonrealism (and, as a consequence, the irreducibly probabilistic nature of quantum theory) will eventually give way to realism. Most, again, side with Einstein in this hope. This question might remain open for a long time to come and possible interminably. The reason for this possible interminability is that for this majority the situation will only be resolved if a realist theory of quantum phenomena, possibly (although this is not a common view) as an interpretation of the standard quantum mechanics or quantum field theory, were found.

Finally, there is still a form of realism associated with nonrealist, RWR-principle-based, interpretations, beginning with that of Bohr. It is defined by the interpretation of the physics of measuring instruments in which the outcomes of quantum experiments are registered as the effects of the interaction between quantum objects and these instruments. These instruments, or rather *their observable parts*, are assumed to be described by classical physics, which, however, cannot predict these effects. On the other hand, the interaction between quantum objects and measuring instruments is quantum, and hence, it is not amenable to a realist treatment. In each case, however, this interaction leaves, as its effect, a trace, a mark, or a set of marks in a measuring instrument, both of which could be very complex as they are in the case of the photographic traces and experimental technology of the Higgs boson. The numerical data associated with such marks can be predicted in probabilistic or statistical terms by quantum mechanics or quantum field theory. The origin of this "trace" is beyond the reach of experiment or theory and possibly thought itself, and in this sense the very concept of trace may be inapplicable, insofar as it cannot refer to anything that could in principle be traced. Such marks, however, or the data associated with them can be treated by means of a representational account and made part of a permanent record, which can be unambiguously defined, discussed, communicated, and so forth, and in this sense may be seen as realist or objective. These are the objective and objectively communicable truths mentioned above. At the same time, the architecture of these effects *compels* the introduction of the RWR principle and nonrealist interpretations based in it, interpretations that place quantum objects and processes beyond representation or even conception, which is to say *idealize* the ultimate reality accordingly. For, even this conception, the conception of the inconceivable, is still a product of human thought and, hence, is an idealization of reality.

1.2.3 Causality

The lack of causality, *as it is classically understood* (there are other conceptions of causality, some of which will be discussed later in this study), is an automatic consequence of the RWR principle, especially if one places the reality of quantum objects and processes beyond thought altogether, because causality would imply an at least partial conception of this reality. Conversely, as noted above, any conception and hence representation of reality may have to be causal, because, as Wittgenstein contended, we cannot conceive of processes that are not causal (Wittgenstein 1922, p. 179). However, even if one adopts a weaker form of the RWR principle, which only precludes a representation of this reality, causality is difficult to maintain in quantum theory. To do so would require a representation *analogous* to the one that obtains in classical physics, which might indeed be seen as a physical embodiment of and was in part born from the classical idea and ideal of causality. "Analogous" need not mean identical, as is shown by relativity, which is a classically causal theory. Schrödinger expressed this difficulty very well, albeit by way of a very different assessment of the spirit of Copenhagen, which he saw as "a doctrine born of distress." He said: "if a classical state [defined by the ideally definite position and the definite momentum of an object at any moment of time] does not exist at any moment, it can hardly change causally" (Schrödinger 1935a, p. 154).

But what is causality? It is a complex question, even in classical physics, but especially given that one can and even must consider nonclassical forms of causality, such as relativistic and quantum-theoretical (which is probabilistic), as I shall do in Chap. 5. Here, I only offer basic definitions and essential features of the key concepts of causality to be used in this study. Given that most of my uses of the term, apart from Chaps. 5–7, refer to classical causality, which quantum theory challenges and, in nonrealist interpretations, by "causality" I shall mean classical causality, unless qualified otherwise. I shall briefly comment on the key alternative conceptions of causality in the end of this subsection.

If understood classically, causality is an ontological category, reflecting the architecture of reality, or more accurately a particular claim concerning the architecture. It relates to the behavior of physical systems whose evolution is defined by the fact that the state of a given system, as idealized by a given theory or model, is determined at all moments of time by their state at a particular moment of time, indeed at any given moment of time. This concept is in accord with Kant's principle of causality, which states (this type of formulation of the principle has been commonly used since), that if an event takes place, it has, at least in principle, a determinable cause of which this event is an effect (Kant 1997, pp. 305, 308). It is commonly (although not universally) assumed that the cause must be prior to, or at least simultaneous with, the effect, an assumption known as the antecedence postulate (e.g., Born 1949, p. 9). Quantum phenomena expressly violate the principle, because no determinable event or process could ever be established as the cause of a given event, and only statistical correlations between certain events could be ascertained. As Heisenberg noted: "[quantum] physics ought to describe only the correlation of observations" (Heisenberg 1927, p. 83).

In contrast to causality as an ontological category, I define "determinism" as an epistemological category, associated with our knowledge of reality. Determinism denotes 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. This category, too, may be called classical determinism, because one could in principle define determinism otherwise, including in a more quantum-mechanical way. Schrödinger's equation is sometimes seen, in my view, misleadingly, as deterministic and on occasion also as causal (a view to be considered further in Chap. 3). Once again, however, unless qualified otherwise, I shall by determinism refer to classical determinism as just defined. Determinism is sometimes used in the same sense as causality, and in the case of classical mechanics, which handles single objects or a sufficiently small number of objects, causality and determinism essentially coincide, which is one of the reasons why classical causality is sometimes called "deterministic causality" (e.g., Dowe 2007, p. 18). Once a system is sufficiently large, one needs a superhuman power to predict its behavior exactly, as was famously explained by P.S. Laplace, who invented the figure of Laplace's demon as an image of this power. To this degree, classical causality is separated from determinism. However, this separation is practical rather than fundamental, because one can, in principle, or by isolating such constituents even in practice, handle the behavior of the individual constituents of such systems deterministically, in contrast to what obtains in the case of elementary quantum processes, which only allow for probabilistic or statistical predictions concerning the outcome of the experiments involving them. The real separation, or *divorce*, of causality and determinism, found in quantum mechanics or quantum field theory in nonrealist interpretations, is correlative to that of probability from classical causality. This divorce, it follows, entails a redefinition of causality in probabilistic or statistical terms, a redefinition to be offered in Chap. 5.

While, however, it follows automatically that noncausal behavior, *considered at the level of a given model*, cannot be handled deterministically, the reverse is not true. The underlined qualification is necessary because we can have (within their proper scopes) causal and even deterministic models of physical processes that may not be ultimately causal. The fact that the causal models of classical physics, including the deterministic models of classical mechanics, apply within their proper limits does not mean that the ultimate character of the actual processes that are responsible for classical phenomena is causal. They may not be, for example, by virtue of their ultimately quantum nature. Nor, conversely, does the noncausal character of a model, for example, that of quantum mechanics in a nonrealist interpretation, guarantee that quantum behavior is noncausal. It may ultimately prove to be causal and, to begin with, amenable to a realist treatment, as many, again, hope it will be one day or even already is.

As all models, noncausal or, in the first place, nonrealist quantum-theoretical models, are interpretive idealizations, formed by theories that contain these models. In particular, it is only determinism that quantum phenomena preclude, because identically prepared quantum experiments in general lead to different outcomes. This circumstance makes individual experiments unrepeatable as concerns their outcomes, as against the statistics of multiply repeated experiments, which are repeatable. It would be difficult, if not impossible, to pursue science without being able to repeat at least the statistical data our experiments provide. The lack of classical causality and, to begin with, realism, leading to the RWR principle, are, again, *interpretive inferences* from this situation, reflected in the QP/QS principle, which thus logically precedes the RWR principle.[10]

I shall now briefly comment on alternative conceptions of causality, which will be considered in detail in Chaps. 5–7. In particular, the term "causality" is some-

[10] This fact does not of course exclude the possibility of classically causal or, in the first place, realist interpretations of quantum mechanics, or alternative causal or realist quantum theories, such as Bohmian mechanics (which is nonlocal), or theories defined by an assumption of a deeper underlying causal dynamics, which makes quantum mechanics an "emergent," surface-level theory. Among recent proposals along these lines is A. Khrennikov's "pre-quantum classical statistical field theory," which is also instructive as a statistical interpretation of quantum mechanics (e.g., Khrennikov 2012; Plotnitsky and Khrennikov 2015). As does Bohmian mechanics, this theory both reproduces the statistical predictions of quantum mechanics and retains the dependence of the experimental outcomes on the context of measurement, which fact is correlative to complementarity.

times used in accordance with the requirements of (special) relativity, which 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. In other words, no physical causes can propagate faster that the speed of light in a vacuum, *c*, which requirement also implies temporal locality, and may be seen as a stronger form of the antecedence postulate stated above. Technically, this requirement only *restricts* classical causality, by a relativistic antecedence postulate, rather than precludes it, and relativity theory itself, special or general, is (locally) a classically causal and indeed deterministic theory. Quantum mechanics, as a probabilistic or statistical theory of quantum phenomena, lacks classical causality, at least in nonrealist interpretations. However, quantum mechanics (as well as finite-dimensional quantum theories and quantum field theory) respects a form of local "causality" as concerns its probabilistic or statistical predictions, which are assumed to conform to both temporal and spatial locality, and hence the relativistic antecedence, just described. Thus, compatibility with relativistic requirements would be maintained as concerns the way in which a given experiment, which already performed, determines, *probabilistically or statistically*, the possible outcomes of future experiments, a determination that is not classically causal or deterministic. More generally, whatever *actually happens* is only defined by spatially and temporally local factors, although the probabilistic or statistical predictions (which are not the same as what actually happens) need not be local. This situation allows one to define several corresponding concepts and principles of (probabilistic or statistical) causality in quantum physics, to be considered in Chaps. 5–7, in particular, the concept of "quantum causality," introduced in Chap. 5.

1.2.4 Randomness and Probability

By "randomness" or "chance" I refer to a manifestation of the unpredictable. Randomness and chance are not always the same, but the difference between them is not germane for my argument. I shall primarily speak of randomness, which is a more fitting concept in the present context. It may or may not be possible to estimate whether a random event would occur, or even to anticipate it as an event. By "event," I mean what is *experienced* as having *actually occurred*, in physics, as a result of a given experiment or because of the occurrence of a phenomenon that can be treated as the outcome of an experiment. I stress that, thus defined, events only refer to what was experienced as having actually happened, although events could be anticipated or predicted. Of course, in some cases, what was experienced as an event may prove not to have actually happened, either at all or in the form it was experienced, even in science, where the outcome of an experiment could be misinterpreted or misunderstood, or mistakenly assumed to have happened. Finally events are always discrete, although they could be experienced as part of a continuous flow that is either actually experienced or registered or is claimed to exist, as, for example, in causal interpretations of quantum mechanics or causal quantum theories, where events are assumed to be connected by a continuous process. In such interpretations or theories the sequences of discrete quantum events that are not connectable by a continuous and causal process in nonrealist interpretations are assumed to be so connected, similarly to the way they are assumed to be in classical physics.

In general, however, a random event may or may not result from some underlying (classically) causal dynamics unavailable to us. The first eventuality defines what may be called classical randomness, essentially an appearance of randomness underlain by a hidden causal architecture of reality. This view has been and remains a dominant form of ontological or realist thinking throughout the history of Western thought, from the pre-Socratics on, a view that, beginning with the rise of modernity, was reinforced by classical mechanics, especially following Newton's work. Thus, Kant and even a more skeptical Hume appear to have seen the ultimate character of the world as causal (although there is some debate concerning Hume's view on this point). What they denied was that the human mind could have access to this causality and, as a result, establish definitive *causal* connections between events, rather than surmise *probable* connections between them. Thus, as explained earlier, in classical statistical physics, randomness and the resulting recourse to probability are due to insufficient information concerning systems that are at bottom causal but whose mechanical complexity prevents us from accessing their causal behavior and making deterministic predictions concerning this behavior. Indeed, as also explained earlier, we can sometimes observe the behavior of the individual constituents of such systems, by isolating these constituents, and handle this behavior in causal and deterministic terms. Possible quantum effects, say, those of atomic behavior of some among such systems, are disregarded in classical statistical physics.

Once these effects come into play, as in the case of the thermodynamics of black-body radiation, this assumption could no longer be made, as Einstein was, again, the first to realize in the case of Planck's law. Einstein showed Planck's law to be incompatible with this assumption, and thus initiated the "divorce proceedings" between (quantum) probability and (classical) causality (Einstein 1906). Planck himself derived his laws under this assumption, even though he assumed (almost as a mathematical convenience) the quantum character of radiation emitted inside a black-body cavity. This, technically, made his derivation incorrect, while the law itself was correct, and thus more guessed than derived. Later on Einstein said that Planck's error was most fortunate for physics (Einstein 1949a, p. 37–43). Einstein correctly re-derived the law on several occasions (e.g., Einstein 1917). It was, then, re-derived yet differently and more quantum-theoretically by S. Bose, in the process also establishing the Bose-Einstein statistics governing the behavior of bosons, so named in Bose's honor (Bose 1924). (Fermions obey the Fermi-Dirac statistics.) Einstein made a major contribution to this subject as well. This was yet another testimony to his enormous overall contribution to quantum theory and to his determination, even obstinacy, in ultimately rejecting the implications of his ideas in quantum mechanics, a determination shaped by his realist convictions and way of thinking (Einstein 1925a, b). The situation found in quantum physics is thus different from that of classical physics, given the difficulties of sustaining arguments for the (classical) causality of the independent behavior of quantum objects or systems of quantum objects, even of elementary individual quantum objects, elementary particles. If an interpretation is nonrealist, the absence of causality is, again, automatic, and the recourse to probability or statistics is unavoidable in principle, again, even in the case of elementary quantum processes and the events they lead to.

Probability and statistics deal with providing estimates of the occurrences of certain individual or collective events, which defy deterministic handling (whether there is or not a hidden underlying causality determining these events), in physics or science in general usually in accordance with mathematical probability theories. The terms "probabilistic" and "statistical" are used differently. "Probabilistic" refers to our estimates of the probabilities of either individual or collective events, such as that of a coin toss or of finding a quantum object in a given region of space. "Statistical" refers to our estimates concerning the outcomes of identical or similar experiments, such as that of multiple coin-tosses or repeated identically prepared experiments with quantum objects, or to the average behavior of certain objects or systems.[11] A given definition of probability may already reflect this difference, as in the case of the Bayesian vs. the frequentist understanding of probability. The Bayesian understanding defines probability as a degree of belief concerning a possible occurrence of an individual event on the basis of the relevant information we possess. This makes the probabilistic estimates involved, generally, subjective, although there may be agreement (possible among a large number of individuals) concerning such an estimate. The frequentist understanding, sometimes also referred to, indicatively, as "frequentist *statistics*," defines probability in terms of sample data by the emphasis on the frequency or proportion of these data, which is often seen as more objective, although this can be debated. The Bayesian approach allows one to make estimates concerning individual and even unique events, say, betting on the outcome of a basketball game or, as in Pascal's wager, on the existence of God and the salvation of the soul, rather than on frequently repeated events, such as repeated coin tosses. In the latter case, our estimations are defined by previous experience of the same or closely similar events. It is true that, technically, no two coin tosses are ever quite the same even in initiating the movement of the coin, the point used by Bayesian theorists (there are differences between their views) against frequentist approaches to probability, which, again, reflect a more objectivist view (e.g., Jaynes 2003, pp. 317–320). In the frequentist view, however, they are sufficiently similar to be treated as statistically identical.

In quantum physics, where, as explained above, exact predictions appear to be, in general, impossible in principle even in dealing with elementary individual processes and events, one considers identical quantum objects such as electrons or photons (not the identical preparation of each, which cannot be assured), and the identically prepared measuring instruments as the initial condition of repeated experiments. The identical preparation of the instruments can be controlled because their observable parts can be described classically, while that of quantum objects themselves, again, cannot be. This is why the outcomes of quantum experiments,

[11] The standard use of the term "quantum statistics" refers to the behavior of large multiplicities of identical quantum objects, such as electrons and photons, which behave differently, in accordance with, respectively, the Fermi-Dirac and the Bose-Einstein statistics.

even if identically prepared in terms of the states of the measuring instruments involved, will in general be different. This difference is irreducible because, unlike in classical physics, it cannot be diminished beyond a certain limit (defined by Planck's constant, *h*) by improving the conditions of measurement, a fact also reflected in the uncertainty relations, which, as will be seen, are equivalent or at least correlative to the statistical nature of quantum predictions. This circumstance leads to quantum probability or statistics and the QP/QS, principle, which, again, logically precedes the RWR principle, and, thus, is not limited to nonrealist interpretations. According to Pauli: "The probabilities occurring in the new laws have then to be considered to be primary, which means not deducible from deterministic [causal] laws" (Pauli 1994, p. 32). Pauli gives an example: "As an example of these primary probabilities I mention here the fact that the time at which an individual atom will undergo a certain reaction stays undetermined even under conditions where the rate of occurrence of this reaction for a large collection of atoms is practically certain" (Pauli 1994, p. 32). The RWR principle is an interpretive inference, made by Pauli as well, from the QP/QS principle, which would, again, apply even in the case of causal theories, such as Bohmian mechanics, of quantum phenomena. Thus, while in classical physics, when the recourse to probability is involved, we proceed from causality to probability, in quantum theory it is a primitive concept and principle, and causality or, in nonrealist interpretations, the lack thereof, is inferred from this concept and principle. However, as will be discussed in Chap. 4, this situation could be interpreted on either frequentist or Bayesian lines, even if one takes a nonrealist view.

As noted above, the QP/QS principle must be equally maintained, as must, correlatively, be the uncertainty relations, in realist interpretations of quantum mechanics or by alternative theories of quantum phenomena, such as Bohmian theories. In Bohmian theories, a given quantum object is assumed to possess both position and momentum, defined exactly at any moment of time, which allows for realism and causality. However, these theories both, and, again correlatively, reproduce the statistical predictions of quantum mechanics and retain the uncertainty relations, because a given measurement always disturbs, *actually disturbs*, the object and displaces the value of one of the two conjugate quantities that enter the uncertainty relations. This type of disturbance of independent causal quantum behavior is in conflict with Bohr's interpretation or other RWR-principle-based interpretations (Bohr 1949, 1987, v. 2, p. 64).

Most representational realist models or theories are predictive as well. Furthermore, some mathematical models used in classical physics are strictly predictive, without offering representations of the objects and processes considered by the corresponding theories and deriving their predictions from these representations, as would be the case in classical mechanics, classical electromagnetism, and relativity. The models used in classical statistical physics are of that strictly predictive type: they offer statistical predictions concerning (large) classical systems. However, as discussed earlier, these models are still ultimately grounded in the representational, even visualizable, model of classical mechanics, which is assumed to causally describe the behavior of individual constituents of the systems considered in classical statistical physics. The situation is somewhat more complex but is not essentially different in chaos and complexity theories, which are only probabilistically predictive as well, due to the mechanical complexity of the systems considered and their sensitivity to the initial conditions. It is an intriguing question whether one can have strictly predictive *nonrepresentational* or *nonrealist* (rather than only nonvisualizable realist) models that are not probabilistic or statistical: we do not appear to have examples of such models thus far. I mean here *predictive models* that would not allow for or would resist a realist interpretation. Otherwise, classical or relativistic realist models, which make ideally exact predictions, could be interpreted, on Kantian lines, as only predictive rather than realist.[12]

The preceding discussion sidesteps some of the deeper aspects of probability, but it suffices for introductory purposes. I shall address some of these aspects throughout this study.[13] I conclude here by reiterating the following two key points. First, probability has a special temporal structure by virtue of its, correlatively, irreducibly futural and irreducibly discrete character, because one can only verifiably estimate future discrete events. (While one can, in principle, make probablistic estimates, "predictions," concerning past events, as historians often do, any possible "verification" of such a prediction, in general, by way of ascertaining its greater probability, can only occur in the future relative to this prediction.) While true in general, this is also strictly in accord with the ultimate character of all quantum events, which preclude us from causally connecting them, and about which, by the same token, only probabilistic or statistical predictions are possible, even, again, in dealing with primitive individual events. This qualification is, again, crucial, for otherwise, this situation pertains to all probabilistic or statistical situations, such as those of classical statistical physics. When it comes to quantum physics understood in the spirit of Copenhagen, probability is, in Jaynes's title phrase, truly "*the* logic of science," insofar as quantum theory is only about estimating outcomes of discrete future events, defined by experiments (or their equivalents in nature) with nothing to say about what happens between these events. There is no story to be told and possibly no concept to be formed as to how these outcomes come about, but luckily and even miraculously we have theories, such as quantum mechanics and quantum field theory, that give us, with great exactitude, the probabilities or statistics of the outcomes observed in quantum experiments.

The second aspect of probability that I would like to note here is as follows. Randomness introduces an element of chaos into order, *unless they are seen as defin-*

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[12] Could one, then, suggest that any physical theory, such as classical mechanics or relativity, that makes exact predictions, even ideally (there are, again, no other exact predictions) is ultimately wrong? This would make probability fundamental to all ultimate theories (say, those on Planck's scale), even if such theories are not quantum in the sense current quantum theories are. We know that classical mechanics and relativity are ultimately wrong, but so is, ultimately, quantum theory as now constituted. One might anticipate that quantum gravity will be probabilistic, even though many and even most, following Einstein, hope that (classical) causality or even determinism will be restored, that is, a model of this type will be developed.

[13] See (Gillies 2000; Hájek 2014; Khrennikov 2009) and references there. On the Bayesian philosophy of probability, in two different versions of it, see (De Finetti 2008) and (Jaynes 2003).

ing the world as a manifold of random events, perhaps underlying a perceived order, and reveals that the world confronts us with this element, *even if the ultimate constitution of nature is assumed to be classically causal*. The two emphasized qualifications describe two opposing forms of realism of the representational type (the first type of realism defined above). The assumption defined by the second qualification is much more common. But the first one has been entertained as well, beginning with the pre-Socratics, and may be called "the Jocasta ontology," because it was assumed and was dramatically expressed by Jocasta, Oedipus's mother and wife, in Sophocles's *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" (Sophocles [fifth century BC] 1984, 11, p. 146, ll. 1068–1072). No appeal to probability is possible under these conditions: next to nothing can be estimated with any degree of belief. This view is proven illusory in the play, because the lives of the characters are ultimately ruled by fate and, thus, by classical causal ontology. At least so the play is commonly interpreted, because there are dissenting readings, along more probabilistic, even if not quite quantum-mechanical, lines. It is clear, however, that the ancient Greeks contemplated a reversal of the causal classical ontology, as an ontology defined by the rule or misrule of chance, which makes all causal order an appearance or illusion.

Yet another ontology contemplated by the ancient Greeks was that of the interplay of chance and necessity, which was introduced, as the atomist ontology of nature, by Democritus and developed by Epicurus and Lucretius, whose *De Rerum Natura* is based in it (Lucretius 2009). This ontology inherits the problem of Jocasta's ontology insofar as the dynamics leading to random events, such as Lucretius's famous clinamen (the random swerve of an atom from a causal trajectory) is not given an explanation. Lucretius presents these swerves as random events "at quite uncertain times/And uncertain places," without an assumption, at least a stated assumption, of causality behind it (Lucretius 2009, Book Two, ll. 218–219, 42). On the other hand, Lucretius's account of this random swerving is not accompanied by a nonrealist account of its efficacy either, which, insofar as this ontology is assumed to be the ultimate ontology, makes this ontology representational and hence classical, even though not strictly causal. Lucretius's atomistic view of the world has periodically enjoyed a certain appeal in modern science, philosophy, and literature, after his great poem was rediscovered in the early fifteenth century. For example, along with atomism itself, which was revived at the time, in the work of John Dalton, a pioneer of modern atomism, the poem attracted much attention of literary authors in the late-eighteenth and early-nineteenth century. However, classical causal ontology has remained dominant, and it still is. Most of the influence of Democritean atomism, or Lucretius, was on the account of their materialism rather than their potential as concerns a possible questioning of causality. Although it may be possible to argue for some precursors, the concept of randomness that suspends the possibility of underlying causality and yet, which is crucial, allows for probability or statistics, thus, again, divorced from causality, appears to have emerged with quantum mechanics, beginning with Heisenberg's work.

Probability, then, introduces an element of order into situations defined by the role of randomness in them, and enables us to handle such situations better.

Probability or statistics is about the interplay of randomness and order. This interplay takes on a special, even unique, significance in quantum physics because of the presence of statistically ordered correlations (not found in classical physics) between certain data, such as those of the EPR-type experiments. Indeed, one of the greatest mysteries, if not the greatest mystery, of quantum physics is that, under certain, but not all, circumstances, random individual events conspire to be statistically correlated and thus statistically ordered multiplicities. At least in some interpretations, such as the statistical Copenhagen interpretation proposed in Chap. 4, all individual quantum events are random in the sense that they cannot be meaningfully assigned probability, and thus only statistical regularities can be ascertained. These correlations are correctly predicted by the formalism of quantum mechanics and rules, such as Born's rule, which are added to, rather than are inherent in, the formalism. This, again, does not mean that it is the only formalism that can predict these correlations. Bohmian mechanics, the predictions of which coincide with those of standard quantum mechanics, predicts them as well, but at the expense of nonlocality, which standard quantum mechanics appears to avoid.

1.2.5 Locality

As indicated in the Preface, the principle of locality states that no instantaneous transmission of physical influences between spatially separated physical systems ("action at a distance") is allowed or, which is a more current formulation, that physical systems can only be physically influenced by their immediate environment. Although not strictly equivalent, these two formulations are equivalent in most contexts to be considered in this study. Indeed, this study will assume a somewhat broader conception and principle of locality. This broader principle states that any possible definitive determinations concerning any given system—its physical state; the application of technology (such as that of *measurement*, which, rather than a *prediction*, definitively determines a physical state of a quantum system); the falsifiability of claims concerning quantum systems, and so forth—is local.[14] Assuming that any of these determinations (or most of them) is, *at a given moment of time*, possible from a spatially separated location would imply that some physical influence would have to be propagated instantaneously. Under certain circumstances, such as those of the EPR-type experiments, quantum mechanics can make *predictions* concerning the state of spatially separated systems. One's capacity to make or verify these predictions (make them definitive) and quantum mechanics itself are *local*.

Nonlocal theories, such as Bohmian mechanics (in all of its versions), allow for and even entail such instantaneous connections, even though it may not be possible

[14] The idea of this principle in this extended form is in part indebted to G. M. D'Ariano, who advocated this type of view, especially as concerns the locality of falsifiability, in several works cited in this study and in private communications with the present author. Related conceptions of locality are also found in the works of several other quantum information theorists, such as L. Hardy. I shall discuss the work D'Ariano and coworkers, and the work of Hardy in Chap. 7.

to actually trace or enact these connections by human means.[15] Nonlocality in this sense is usually, albeit not always, seen as undesirable. Standard quantum mechanics, again, appears to avoid it. However, the question of the locality of quantum mechanics or quantum phenomena is a matter of great subtlety and much controversy, especially in the wake of the Bell and Kochen-Specker theorems and related findings. As noted in the Preface, these developments even led to the dominance of the question of locality in recent debates concerning quantum theory, although because realism has remained a major concern, in particular given the lack of realism as a possible alternative to nonlocality, the questions of realism and completeness have continued to remain germane to these debates.[16] These developments also led to alternative conceptions of locality, sometimes linked to other conceptions, such as "separability" (which has to do with entangled systems). These alternative conceptions require a separate discussion, which I shall not undertake here, and it is not necessary for my main argument, in part because many (although not all) of these conceptions are consistent with the principle of locality adopted here, which remains central to the current discussions and debates in any event.

These findings are sometimes interpreted as implying that quantum mechanics or quantum phenomena (nature) is nonlocal. This view was already suggested, on the basis the EPR-type (thought) experiments, by Einstein, who, however, saw locality as imperative, and hence claimed that quantum mechanics must be incomplete, even Bohr-incomplete, that is, not as complete as possible given the available experimental data. This argumentation was challenged by Bohr. As I said, quantum phenomena allows, specifically in EPR-type situations, for *predictions*, for example and in particular by means of quantum mechanics, concerning a quantum system, say, S_1 at an, in principle, arbitrary distance from another quantum system, S_2, on the basis of a measurement performed on S_2. However, as Bohr argued in his reply to EPR (Bohr 1935), it is possible to interpret quantum mechanics so as to preserve locality, and thus to avoid what Einstein famously called "spooky action at a distance" [A Letter to Born, 3 March, 1947 (Born 2005, p. 155)]. As will be discussed in Chap. 3, in this view, one could speak of spooky *predictions* at a distance, *spooky* because there is no physical explanation of the nature of the quantum-level reality responsible for these predictions: there is, again, no story to be told of how quantum correlations come about. It does not follow, however, that there is a spooky *action* at a distance, a physical action that instantaneously connects the phenomena thus correlated. Bohr's argument essentially implied that, by virtue of being Einstein-incomplete

[15] See (Bohm and Hiley 1993) for an exposition of the last version of the theory developed by Bohm, in collaboration with B. Hiley.

[16] The literature dealing with these subjects is nearly as immense as that on interpretations of quantum mechanics, and it had been approaching mammoth proportions even before the fiftieth anniversary of Bell's theorem in 2014, which added hundreds of new works. Among the standard treatments are (Bell 2004; Cushing and McMullin 1989; Ellis and Amati 2000). See, also (Shimony 2013) and (Held 2014) for comprehensive introductions and references, and (Brunner et al. 2014) for the papers celebrating the fiftieth anniversary of Bell's theorem. It is worth keeping in mind that these theorems and most of these findings pertain to quantum data as such, and do not depend on quantum mechanics. Virtually all of these findings concern discrete variables.

(the RWR principle), quantum mechanics is Bohr-complete, as complete as nature allows it to be. Einstein ultimately accepted this view (as correlative to the statistical nature of quantum mechanics) as "logically possible without contradiction," but found it "contrary to his scientific instinct," because it lacked Einstein-completeness, the capacity to represent the behavior of individual quantum systems, in a causal and even, in principle, deterministic way (Einstein 1936, p. 375). The latter qualification, as noted from the outset, is part of Einstein-completeness, which requires that probability or statistics are reduced to causality at the ultimate level, just as they are in classical statistical physics, which need not mean that such a theory is classical mechanics. One could, in principle, see quantum mechanics as causally representing the independent behavior of the individual systems considered, while assuming that any predictions concerning this behavior are probabilistic or statistical, which, however, is unacceptable for a complete fundamental theory, according to Einstein.

The concept and principle of locality are commonly associated with relativity. As indicated in the Preface, however, while relativity conforms to locality and gave rise to the concept and the principle, locality is quite independent of relativity, and may and even need to be *divorced* from it in quantum physics or fundamental physics more generally. Reprising the considerations mentioned in the preface, first of all the concept of locality is independent of the concepts with which it is associated in relativity, in particular the Lorentz invariance of special relativity. For one thing, the Lorentz invariance is violated in general relativity, where it is only locally or infinitesimally valid, while locality is strictly maintained there. Secondly, technically, relativity prohibits a propagation of physical influences not only instantaneously but faster than the (finite) speed of light in a vacuum, a requirement that could, in principle, be violated (as a different speed limit on physical action may be discovered), while still allowing for locality. Thus, in inflationary models, the pre-Big-Bang universe expands faster, much faster, than the speed of light (accordingly, relativity does not apply), but it could still be local. Einstein's "spooky action at a distance," too, refers to an instantaneous action, which is in conflict with relativity as well, but is not defined by it. Standard quantum mechanics is not relativistic, but it is, or may be interpreted, as local, or in any event may be required to be local (corresponding to what is actually observed in low-energy quantum regimes). In addition, as noted earlier as well, relativity is a classically causal and in fact deterministic theory, and quantum mechanics or quantum field theory is, while local, neither deterministic nor, at least in nonrealist interpretations, (classically) causal, and thus is a local probabilistic or statistical theory. The locality of quantum mechanics implies that it is also in compliance with the key requirements of relativity, but it may be a deeper fact, as the Bell and the Kochen-Specker theorems and related findings mentioned above indicate. Moreover, as will be seen in Chap. 6, while relativistic, in the sense of conforming to the principles of (special) relativity, specifically that of the Lorentz invariance, the formalism of quantum field theory is not necessarily a consequence of these principles. At least Dirac's equation for the relativistic electron could be derived solely on the basis of certain principles of quantum information theory, without using the principles of relativity, which were used by Dirac in order to derive the equation (D'Ariano and Perinotti 2014). On the other hand, a form of the principle of locality is part of this alternative derivation.

This, again, need not mean that the principle is necessarily quantum in origin, although there are reasons to think that this may be the case, especially in view of fundamental physics at the Planck scale, if this physics is still quantum in the sense we give this term now. In any event, it does appear that the principle of locality reflects deeper aspects of nature than those captured by relativity theory (which is local as well), and that it may, in combination with the principles of quantum theory, define the ultimate constitution of nature, thus, again, divorcing locality from relativity. Relativity, then, at least special relativity but, not inconceivably, also general relativity may be surface effects of this deeper constitution defined by the principle of locality and the principles of quantum theory, a conjuncture that would reverse the view of the ultimate character of fundamental physics advocated by Einstein and quite a few of his followers. While most of them would accept this conjecture as concerns the role of the locality principle (fully consistent with and required by relativity), they would rather see the fundamental principles of relativity, especially general relativity, as a classical-like field theory, as more fundamental, and the key features of quantum physics as surface effects of an underlying field theory. Such a theory would need to reach beyond general relativity, for one thing, because it would have to contain electromagnetism (even a classical one), a unification program that Einstein pursued, unsuccessfully, all his life.

But then, the view that the ultimate theory, which is to say, the next theory (the only ultimate theory there can be), should be more in accord with the fundamental principles of quantum theory need not imply that such a theory will be any form of quantum theory currently known, such as quantum mechanics or quantum field theory. Extraordinarily successful as these theories are, in particular in grounding the standard model of elementary particle physics, they are far from complete or free of deficiencies even within their proper scope. Some new and perhaps now unimaginable theories and very likely new principles may be required to approach the ultimate constitution of nature. Will these theories retain the locality principle? My Bayesian bet would be that this will be more likely than not to be case, but one cannot be certain.

1.3 Principles

As noted in the preface, although principle thinking led to many major breakthroughs throughout the history of quantum theory (from the old quantum theory to quantum mechanics to quantum field theory to quantum information theory), the focus on fundamental principles has been uncommon in recent discussions of quantum foundations, dominated by mathematical or logical aspects of quantum theory. My argument in this study, however, is that exploring fundamental principles and the nature of principle thinking remains exceptionally helpful for understanding quantum theory and the debate concerning it, a debate that, again, has accompanied it throughout its history and is unlikely to end any time soon.

Principle thinking is a form of foundational thinking, and, as such, it is defined by fundamental concepts, such as those considered in the preceding section.

However, principle thinking is distinctive in its approach, in particular, because it is essentially connected to nonrealist thinking, specifically in quantum mechanics, the first nonrealist fundamental theory, at least the first theory that has received and may require a nonrealist interpretation. (By the latter I, again, mean interpretations based in the RWR principle.) Principle thinking may also be realist, as it is in classical physics or relativity. However, while realist thinking could be either principle or constructive (in Einstein's sense, explained below), or both, nonrealist thinking is strictly principle. The fundamental principles of quantum mechanics emerge, beginning with Heisenberg's work, in tandem with the emergence, again, for the first time in physics, of nonrealist thinking. Heisenberg himself did not initially assume that quantum phenomena and quantum mechanics *preclude* realism, in other words, he did not adopt the RWR principle as such, but instead something like a proto-RWR principle. He assumed (and showed) that one could establish quantum mechanics as a probabilistically or statistically predictive theory (in accordance with the QP/QS principle, which he did assume), based in a new type of mathematical model, a model that did not offer a representation of quantum objects and processes. The view that such a representation or model is "*in principle* excluded," in accord with the RWR principle, was advanced later by Bohr's interpretation and, moreover, only in the ultimate version of this interpretation developed in the late 1930s. Bohr's earlier versions of his interpretation retained realist elements, which were removed step by step under the impact of his exchanges with Einstein. Their exchanges concerning the EPR experiment, arguably, provided the main impetus for Bohr's ultimate, RWR-principle based, interpretation.

As will be seen in Chap. 2, the joint emergence of quantum mechanics and nonrealism was prepared by the preceding development of quantum theory, as the so-called old quantum theory, pioneered by Planck's discovery of his law of black-body radiation, the first quantum law, defined by what he aptly named the quantum of action, h. The theory was then developed by Einstein, Bohr, Sommerfeld, and others, including Heisenberg and Pauli. While primarily associated with quantum mechanics where they made their greatest discoveries, Heisenberg and Pauli also made important contributions to the old quantum theory, Pauli one of the greatest ones with his exclusion principle (Pauli 1925). That it was a principle is worth registering, as is the fact that its functioning as a principle, especially in *guiding* quantum-theoretical thinking (which function is part of the present concept of principle), played a major role in the subsequent development of quantum mechanics and quantum field theory. The old quantum theory has its history as well, including as a principle theory, specifically in the preceding developments of thermodynamics, the kinetic theory of gases, and electrodynamics, developments that led Planck to his discovery, made at the intersection of all three fields. This earlier history, which extends even to the pre-Socratics, in particular to Democritean atomism, will be put aside here, although the Democritean atomism or the atomism (conceptually still Democritean) of classical atomic physics will be invoked throughout by way of a contrast with Bohr's concept of atomicity. Both Bohr's and Einstein's contributions to the old quantum theory were particularly important as examples of principle thinking, and the very concept of principle theory is, again, due to Einstein. Einstein made momentous contributions (which, as I said, rivaled and even surpassed those of Planck) to the founding of quantum theory and its development leading to quantum mechanics, and thus to the development of some among the fundamental principles of quantum theory, such as the QP/QS principle, which Einstein never accepted as fundamental. Einstein had always been, and had seen himself as a principle thinker in relativity and quantum theory alike, or in statistical physics, albeit a principle thinker of a realist persuasion, which compelled him to combine principle and constructive thinking in his sense. This combination helped his work in all three fields, but ultimately brought him to his confrontational stance against quantum mechanics, his previous contributions to the developments of some its principles notwithstanding. As stressed from the outset, the rethinking of fundamental principles and the discovery of new such principles is crucial to fundamental physics. Pauli made this point in his assessment of Einstein's essentially principle work on quantum theory. As he said:

> If new features of the phenomena of nature are discovered that are incompatible with the system of theories assumed at that time, the question arises, which of the known principles used in the description of nature are general enough to comprehend the new situation and which have to be modified or abandoned. The attitude of different physicists [toward] problems of this kind, which make strong demand on the intuition and tact of a scientist, depends to a large extent on the personal temperament of the investigator. In the case of *Planck*'s discovery of 1900 of the quantum of action [*h*] during the course of his famous investigations of the law of the black-body radiation, it was clear that the law of conservation of energy and momentum and *Boltzmann*'s principle connecting entropy and probability were two pillars sufficiently strong to stand unshaken by the development resulting from the new discovery. It was indeed *the faithfulness to these principles* which enabled *Planck* to introduce the new constant *h*, the quantum of action, into his statistical theory of the thermodynamic equilibrium of radiation.
>
> The original investigation of *Planck*, however, had treated with a certain discretion the question whether the new "quantum-hypothesis" implies the necessity of changing the laws of microscopic phenomena themselves independent of statistical applications, or whether one had to use only an improvement of the statistical methods to enumerate equally probable states. In any case, the tendency towards a compromise between the older ideas of physics, now called the "classical" ones, and the quantum theory was always favored by *Planck*, both in his earlier and later work on the subject, although affirmation of such a possibility was to diminish considerably the significance of his own discovery.
>
> Such considerations formed the background of *Einstein*'s first paper on quantum theory …, which was preceded by his papers on the fundamentals of statistical mechanics and accompanied, in the same year 1905, by his fundamental papers on the theory of the Brownian movement and the theory of relativity. (Pauli 1994, p. 86; emphasis added)

This paper was the first step in the development of Einstein's argument for the incompatibility between the new quantum theory and the fundamental assumptions or, again, principles of classical physics, specifically, again, as manifested in the divorce of probability from (classical) causality in quantum theory. Einstein discovered a similar type of conflict in his special relativity, in both cases, however, without giving up the principles of realism or even seriously entertaining such a move then or later, although he, again, acknowledged that it was logically possible in quantum theory (Einstein 1936, p. 375). Pauli does not explain what he means by principles and does not distinguish between laws and principles, which is not uncommon. Heisenberg and Dirac do not explain this either in their books invoked at the outset of this study, their titles, *The Physical Principle of the Quantum Theory* and *The Principles of Quantum Mechanics*, notwithstanding (Heisenberg 1930; Dirac 1930, 1967). It was, again, Einstein, who reflected, in explaining the (principle) nature of relativity theory, on the concept of (fundamental) principles and on "principle [physical] theories," in juxtaposition to "constructive theories." Pauli, however, is right to argue that "it was indeed *the faithfulness to these principles* [that of energy conservation and that of fundamentally linking entropy to probability] which enabled Planck to introduce the new constant *h*, the quantum of action, into his statistical theory of the thermodynamic equilibrium of radiation" (emphasis added). The discoveries of Bohr, Heisenberg, and Dirac, and their followers, who ultimately brought quantum theory (Dirac, the discoverer of quantum electrodynamics, was the founder of this particular trajectory) to the Higgs boson and beyond, were equally enabled by their faithfulness to the principles of quantum theory and by their introduction of new such principles. Ultimately, it is, again, the invention of new principles that is most crucial, a point not lost on Einstein either.

Before I proceed to my discussion, via Einstein, of the concepts of principle and principle theory, I would like to define "axioms" and "postulates," again, as these terms will be used here. They are often used, in physics (mathematicians tend to be more careful), somewhat indiscriminately and interchangeably with each other, or of either with "principles," as a result often obscuring substantive points at stake. It is difficult to entirely avoid overlapping between the concepts designated by these terms, and sometimes those designated as "laws," especially because physical principles are often accompanied by or derive from (or conversely give rise to) postulates. It may also be a matter of the functioning of these concepts. Thus, the concept of symmetry (as invariance under transformation), which became central to all fundamental physics, could lead to principles, laws, and postulates, beginning with a very general form of the symmetry principle, saying that physical laws have symmetries that one must find. Conservation laws (which are closely connected to symmetry, via E. Noether's celebrated theorems) are sometimes seen as conservation principles. It is possible, however, to sufficiently, even if not entirely, analytically separate these concepts.

Euclid and, it appears, the ancient Greeks in general, distinguished between "axioms" and "postulates." Axioms were thought to be something manifestly self-evident, such as the first axiom of Euclid ("things equal to the same thing are also equal to each other"). A postulate, by contrast, is *postulated*, in the sense of "let us assume that *A* hold," thus indicating primarily that one aims to proceed under assumption *A* and see what follows from it according to established logical rules (this is similar to proceeding from axioms), rather than claiming *A* to be a self-evident truth. Euclid's postulates may be thought of as those assumptions that were necessary and sufficient to derive the truths of geometry, of some of which one might already be intuitively persuaded (e.g., "the first postulate: to draw a straight line from any point to any point"). The famous fifth postulate is a case in point. It defines Euclidean geometry alone, which in part explains millennia of attempts to derive it as a theorem. In Kant's understanding of geometry, inspired by Euclid, axioms are analytic and postulates synthetic propositions, the type of difference adopted by Einstein in his understanding of principle (analytic) vs. constructive (synthetic) theories, although, in the definition just given, both types of theories are derived by postulates rather than axioms. Keeping in mind further complexities potentially involved in defining and using these concepts in geometry and beyond, I shall adopt this understanding of axioms and postulates. Given that my subject is physics, I shall primarily refer to postulates, assumed on the basis of experimental evidence (as it stands now and hence is potentially refutable) and often, but not always, grounding principles. It is not easy to speak of axioms in the sense just defined in modern physics, to some degree in contrast to Aristotle's physics (Aristotle 1984). Next to nothing has the self-evidence of axioms even in classical mechanics (in part, as against, its mathematical models, for example, those using Euclidean geometry), and most of the uses of the term "axiom" are in effect closer to that of "postulate," as just defined. Bohr, more careful and etymologically attuned than most in using his terms, prefers both postulates, such as the quantum postulate, central to his Como lecture of 1927, his first article on quantum mechanics and complementarity (Bohr 1927, 1987, v. 1, pp. 52–53), and principles, such as his correspondence principle, to be discussed in the next chapter. As will be seen there as well, in Heisenberg's hands, the correspondence principle in effect gave rise to a mathematically expressed postulate. Complementarity functions as both a concept and a principle in Bohr. The term is also used by Bohr to designate his overall interpretation of quantum phenomena and quantum mechanics, largely (although not exclusively) based in this concept.

Einstein's distinction between "constructive" and "principle" theories is that between two contrasting, although in practice often intermixed, ways of thinking in theoretical physics. According to Einstein, "constructive theories," based on synthetic thinking in Kant's terms (adopted by Einstein), aimed at "build[ing] up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out" (Einstein 1919, p. 228). "A relatively simple formal scheme," essentially a mathematical model, is, thus, referring to more or the most elementary, such as molecular or atomic, constituents of physical reality, with a customary implication that this scheme represents, at least ideally, or, again, is a mathematical model of, the ultimate underlying reality responsible for these phenomena. Einstein's example of a constructive theory in classical physics is the kinetic theory of gases, which "seeks to reduce mechanical, thermal, and diffusional processes to movements of molecules—i.e., to build them up out of the hypothesis of molecular motion," described by the laws of classical mechanics (Einstein 1919, p. 228). As indicated earlier, the assumption that this motion obeys the laws of classical mechanics and conforms to its mathematical model was *in effect* let go by Planck's black-body radiation theory, which led to the rise of quantum physics, although it was, again, Einstein who was the first to realize the incompatibility between Planck's quantum hypothesis and this assumption (Einstein 1906). One could, however, build quantum theory independently of this assumption, that is, independently of (synthetically) *constructing* "the materials of a relatively simple [underlying] formal scheme," or model representing an underlying physical reality. Instead, as Heisenberg did, one can build quantum theory as a principle and nonrealist theory, or at least a theory that allows for a nonrealist interpretation.

In contrast to constructive theories, principle theories, according to Einstein, now expressly using Kant's terms, "employ the analytic, not the synthetic, method. The elements which form their basis and starting point are not hypothetically [synthetically] constructed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy" (Einstein 1919, p. 228). These theoretical representations are synthetic, constructed, as they would have to be already by virtue of the role mathematics plays in them. However, this synthesis or construction is different from the construction of "the materials of a relatively simple formal scheme" or the model defining a constructive theory, according to Einstein, because these representations only have to satisfy the mathematically formulated criteria or postulates established by principles, rather than represent the ultimate underlying physical nature of the phenomena considered. Einstein's language, that of representation or even processes (rather than phenomena), is colored by realist thinking, even in his description of principle theories. One can, however, think instead in terms of a nonrealist mathematical model that satisfies the criteria emerging from principles in the way the mathematical formalism of quantum mechanics satisfies the mathematically formulated criteria supplied by the founding principles, such as the QD and QP/QS principles, and thus without necessarily assuming realism. Thermodynamics, Einstein's example of a classical principle theory (parallel to the kinetic theory of gases as a constructive theory), is a principle theory because it "seeks by analytical means to deduce necessary conditions, which separate events have to satisfy, from the universally experienced fact that perpetual motion is impossible" (Einstein 1919, p. 228). This impossibility thus becomes a principle.

That such (physical) "principles give … rise to *mathematically formulated criteria*" and thus lead to a corresponding mathematical model with the resulting principle theory is crucial to principle thinking in all modern physics, as a mathematical-experimental science of nature, from Galileo to quantum theory, and beyond. This aspect of principle theories played a particularly important role in the history of principle thinking in quantum theory, beginning with Heisenberg's discovery of quantum mechanics. I shall also speak of the mathematical expression of such principles themselves, rather than only the mathematically formulated criteria the principles may give rise to, which extends Einstein's conception but is consistent with it. Heisenberg's thinking leading him to this discovery was principle and, as such, was influenced by Einstein's special relativity, which was a principle theory. Neither thermodynamic nor statistical mechanics were fundamental theories, unlike classical mechanics, which Einstein argued could not be derived from them. Later on, he expressed a parallel view that a proper, *constructive*, theory of quantum processes could not be derived from quantum mechanics, in part because he wanted such a theory to be a theory of continuous classical-*like* fields, on the *model* of Maxwell's electrodynamics. He said: "I do not believe that quantum mechanics will be the starting point in the search for [a proper theoretical basis for the right theory of the ultimate constitution of nature], just as one cannot arrive at the foundations of mechanics from thermodynamics or statistical mechanics" (Einstein 1936, p. 361).

Following and generalizing Einstein's concept along the lines just suggested, I define *principles* as *empirically discovered, general characteristics of physical phenomena that give rise to mathematically formulated criteria or postulates or the mathematical expression of such principles themselves that the theory and the mathematical models defined by it have to satisfy.* Following Einstein, too, I give a central role to the requirement that principles give rise to mathematically formulated criteria or postulates, as mathematical expressions of these principles. Thus, Heisenberg gave Bohr's correspondence principle a mathematical form, and by doing so also made it into a postulate, by requiring that the equations of quantum mechanics convert into those of classical physics in the classical limit. I shall also add the following qualification, which would probably have been accepted by Einstein. Principles themselves are not so much empirically discovered as formulated and thus, again, constructed *on the basis* of empirically discovered or established features of physical phenomena. As indicated earlier, it is far from always the case that such physical principles are formulated first independently of their mathematical expression and are then endowed with this expression. Physics and mathematics tend to be reciprocal in this construction, and it is not always possible to establish which of them comes first at any juncture of the process, or to entirely separate them.

This "construction" of principles is, again, different from the synthetic construction of an underlying theoretical scheme and the corresponding mathematical model, on the basis of which the phenomena considered would be explained and represented in a constructive theory. For one thing, a principle theory need not, by definition, contain such a scheme, although the latter might emerge from the principles adopted by the theory. Quantum mechanics, in nonrealist interpretations, does not involve or, by the RWR principle, precludes such a *scheme*. On the other hand, from Heisenberg on, quantum mechanics, in nonrealist interpretations, involves an inference of the existence and, hence, a construction of quantum objects from quantum phenomena, observed in measuring instruments. Quantum mechanics, in nonrealist interpretations, constructs quantum objects as unrepresentable and thus unconstructible. It is true that the existence or reality of quantum objects, specifically electrons and photons, was assumed by Heisenberg (on the basis of experimental evidence) in his work leading him to quantum mechanics. However, even though electrons and photons were considered classically or, in the case of the old quantum theory, semi-classically before quantum mechanics, their existence was still only established inferentially on the basis of certain marks observed in measuring instruments, such as silver-bromide photographic plates or cloud chambers. These marks were assumed to be the effects of the interactions between measuring instruments and quantum objects, which quantum theory was initially supposed to represent. Heisenberg renounced representing quantum objects and processes (which posed major difficulties even in the old quantum theory) by means of his mathematical model, which reduced "*all interactions* between atoms and the external world … to transition probabilities" (W. Heisenberg, Letter to R. Kronig, 5 June

1925; cited in Mehra and Rechenberg 2001, v. 2, p. 242; emphasis added). Heisenberg, again, stopped short of interpreting his scheme as entirely, in principle, excluding a realist model of quantum processes, as Bohr was to do later on. However, Heisenberg's scheme and, following it, quantum mechanics became strictly principle theories of quantum objects and processes in the absence of the kind of construction (that of the underlying reality represented by a mathematical model) that defines constructive theories. It only has a constructive dimension as concerns the description of measuring instruments by means of classical physics.

By contrast, Schrödinger was aiming for a constructive scheme and a model that would ideally represent quantum objects and processes, (re)*conceptualized* as waves and wave propagation, although this scheme was more complex than simple. This construction used, overtly and implicitly, certain principles as well, some of which in effect led to major difficulties in maintaining the constructive, *representational*, nature of his model in the way he initially thought possible.

As indicated earlier, Einstein persistently argued that it would be difficult to see "general characteristics of natural processes" as ever purely empirically given, if, which is Einstein's point as well, one could ever speak of anything as given purely empirically. He saw this view as 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* [*begrrifliche Konstruction*]." "Such a misconception," he added, "is [only] 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 1949a, p. 47; emphasis added). His use of "construction" merits a notice here. Such a choice of concepts is never entirely free either. This conceptual practice may perhaps be better seen in terms of experimenting with concepts and models, a point with which Einstein might have agreed, even if probably while still defending one's freedom in this process. It would be hard to dispute the importance of this freedom, limited as it might be, although some would, for example, advocates of the so-called superdeterminism (e.g., 't Hooft 2003). Einstein saw the mediation of mathematical concepts as essential to and irreducible in physics. He saw the practice of theoretical physics as that of the invention of new concepts through which one can approach reality, sometimes even to the point of overriding the experimental evidence (van Dongen 2010, pp. 89–95). This may ultimately be problematic, but is not entirely without justification, because experimental evidence can change and cannot always be relied on. It is also worth keeping in mind that, as Einstein was well aware (although the point was more important for Bohr), a theory always contains dimensions that are other than mathematical. However, as noted earlier, it follows from Einstein's view under discussion at the moment that all realism is conceptual, in modern physics, again, primarily, if not entirely, defined by mathematical concepts and models.

This is not to say that there is no difference between what is *experimentally* given and what is developed as a *theory* that relates to what is experimentally given. It is only that the experimentally given is never *purely given*, that is, it cannot be considered apart from some theoretical (in the broad sense) or phenomenological construction or organization. This fact compelled Einstein to advise Heisenberg, in the context of the latter's claim concerning dealing with "quantities which in principle are observable" in his paper introducing quantum mechanics (Heisenberg 1925, p. 261), that "theory decides what is observed" (Heisenberg 1971, p. 63; also Heisenberg 1989, p. 29). The exchange merits citation at a greater length:

> Heisenberg: "We cannot observe electron orbits inside the atom…. Now, since a good theory must be based on directly observable magnitudes, I thought it more fitting to restrict myself to these, treating them, as it were, as representatives of the electron orbits."
>
> "But you don't seriously believe," Einstein protested, "that none but observable magnitudes must go into a physical theory?"
>
> "Isn't that precisely what you have done with relativity?" I asked in some surprise…
>
> "Possibly I did use this kind of reasoning," Einstein admitted, "but it is nonsense all the same…. In reality the very opposite happens. It is the theory which decides what we can observe." (Heisenberg 1971, p. 63 also Heisenberg 1962, pp. 45–46)

The faithfulness of Heisenberg's recollection aside, Einstein was right, and modern, mathematical-experimental, physics would not be possible otherwise, as Heidegger argues in his important discussion of the birth of modern physics in Galileo and Descartes: "Upon the basis of the mathematical, the *experientia* becomes the modern experiment. Modern science is experimental because of its mathematical project. The experimenting urge to the fact is a necessary consequence of the preceding mathematical overriding [*Überspringen*] of all facts. But where this overriding ceases or becomes weak, mere facts are collected, and positivism [or naïve empiricism, rather than the scientific view of experiment] arises" (Heidegger 1967, p. 93; translation modified). Einstein's position, too, is derived from his argument, against Mach (whose views are implicitly questioned by Einstein in the passage cited above), that no scientific knowledge is possible apart from conceptual construction. In fact, "the principle of observable quantities," as it may be called, was not used in its pure form either by Einstein in relativity or by Heisenberg in his discovery of quantum mechanics. It was in both cases at least accompanied by what may be called "the principle of the conceptual determination of the observable," thus suggested by Einstein, a principle found, at least implicitly, as early as Plato or even the pre-Socratics, but first expressly established philosophically by Hegel in his *Phenomenology of Spirit* (Hegel 1977). In physics this determination is also mathematical, as both Einstein and Heidegger say. Heisenberg's argument in his first paper, again, did not really exhibit the empiricist or positivist misconception or prejudice Einstein questioned, in part because at stake, as announced even in his title, were primarily the *relations*, kinematical and mechanical, and thus also mathematical, between "quantities which in principle are observable," rather than these quantities themselves. In any event, Heisenberg took Einstein's remark and the principle of the theoretical determination of the observable to heart and used it in his work that led to the uncertainty relations.

Heisenberg (at least according to his later recollections) even went so far as to ask "Why not simply say that only those things occur in nature which fit our mathematical scheme" (Heisenberg 1963, Interview with T. Kuhn, 5 July 1963, *Archive for the History of Quantum Physics* [hereafter] *AHQP*). Coming from an oral interview, the statement should be treated with caution, as Heisenberg indeed warned, even doubting the accuracy of the transcript, as well worrying that he was not sufficiently careful in making this statement (Heelan 1975, p. 25). However, a certain primacy of creating a mathematical scheme or model in his thinking is hardly in doubt, thinking at this point indebted to Einstein, who by that time (the mid 1920s) strongly adhered to this type of view, albeit along realist lines. The question is of course also: "Where does this scheme come from?" I shall return to this question below. Heisenberg's later thinking clearly continued to exhibit this emphasis on the primacy of mathematics, even, as against his earlier work on quantum mechanics, of giving his thinking a realist bent (more along Platonist lines or those of structural realism), in this respect in contrast to Bohr. While well aware of the role of concepts in shaping and even defining observation, Bohr never shared this (near) reversal of the relationships between physics and mathematics. As will be seen in Chap. 2, some aspects of this thinking, thinking giving primacy to mathematics (the invention of a suitable mathematical scheme), transpire in Heisenberg's initial work on quantum mechanics in 1925, at this stage along more strictly nonrealist lines. In general, Heisenberg kept his affinity with Bohr and the spirit of Copenhagen throughout his life. Heisenberg's thinking also influenced Dirac, in whose work, as discussed in Chap. 6, this primacy of mathematical thinking is even more pronounced.

Einstein's general point just considered and his principle of the theoretical determination of the observable do not mean that there is no difference between theoretical and experimental construction. Instead, it directs our attention to the complexity of the relationships between them. In thermodynamics, "the impossibility of perpetual motion" could hardly be seen as empirically given; it was instead formulated, as a principle, on the basis of empirically established evidence. Principles, thus, need not have the self-evidence of axioms or, at least initially, the assumptive character of postulates, although, once introduced, they may function as postulates from which a given theory is built by means of logical rules and deductions. This study amplifies this understanding of principles, by seeing them as a foundation and guidance for inventing and building new theories, a view in effect assumed by Einstein.

Einstein, who by the time of quantum mechanics had developed a strong preference for constructive theories, hoped that quantum mechanics, as a principle theory, would be replaced by a constructive theory, in the spirit of Schrödinger's initial wave approach, which was constructive and, as such, much preferred by Einstein, as against Heisenberg's matrix version, which was principle. Schrödinger's wave mechanics was more in accord with Einstein's program, fully in place by then, for a unified field theory, which aimed to derive quantum discreteness from a continuous classical-like field theory (which Einstein was never able to do, in his several attempts at such a theory, any more than Schrödinger was in quantum mechanics). Schrödinger's work on his wave mechanics was nearly immediately preceded by a paper on the Bose-Einstein theory, a paper that, as did Einstein's own work on the subject, took advantage of Louis de Broglie's 1923 theory of matter waves, which was a constructive theory and contained a strong anticipation of Schrödinger's wave approach (Schrödinger 1926a). By contrast, for Heisenberg, quantum discreteness was one of the primary principles and postulates, although the concept required a complex interpretation, not given by Heisenberg in his paper and ultimately developed by Bohr, as different from Democritean discreteness or atomicity of quantum objects themselves. Quantum discreteness proved to be a major problem for Schrödinger's program, and one of the reasons that compelled him to abandon it, although he attempted to return to it later in his life in the 1950s (Schrödinger 1995).

For a while Einstein saw his own theoretical thinking, along with that of Bohr, as primarily principle, although he entertained hopes for a more constructive quantum theory in his initial work on the old quantum theory, beginning with his 1905 paper on the "heuristic" quantum theory of light quanta (Einstein 1905a).[17] This paper had manifested constructive inspiration, and was revolutionary in proposing the physically quantum nature of radiation, a proposal with which Planck never came to terms. Even at the time of his introduction of this distinction in 1919, Einstein still maintained a nearly equal value of each type of theory. However, his preference for constructive theories, defined by "free conceptual construction" and by inventing mathematics that embodies this construction in a realist manner, grew, ultimately to the point of a nearly unconditional insistence on them, which approach uncompromisingly defined Einstein's thinking by the time quantum mechanics entered the scene.[18]

Constructive theories are representational realist theories of the first type considered above, because they aim at developing mathematical models, assumed to idealize how nature works at the deeper level constructed by a theory, following the paradigm or the idea of classical physics, or "the classical ideal," as Schrödinger, again, called it (Schrödinger 1935a, p. 154). It was initially expected that this would have been the case in the quantum theory. The constitutive elements of chemical atoms (which were by then discovered to be no longer indivisible) and other particles, such as, at the time, electrons, protons, and photons (new atoms of nature, soon dubbed elementary particles), would behave according to the laws of classical mechanics or classical electrodynamics, or both.[19] These expectations, while they still guided Planck, were defeated by quantum theory, which Planck created, defeated to a large degree due to Einstein's and Bohr's work in the old quantum theory. At that stage Einstein and Bohr were allies, at least as far as physics, if not philosophy, was concerned, and Einstein was, on occasion, more daring and revolutionary. Bohr, for example, was famously reluctant to accept the idea of a photon. Einstein was also the first realize the joint significance of wave and particle aspects of radiation, or wave-particle duality, which, however, is not the same as *complementarity*, with which Einstein never quite came to terms

[17] Einstein, at this stage, juxtaposed the principle thinking and approach, such as his own, or that of Bohr and P. Ehrenfest to the work of "mathematical virtuosi," such as M. Born, P. Debye, and A. Sommerfeld (van Dongen 2010, pp. 157–158).

[18] See (van Dongen 2010) for an extended discussion of the transition of Einstein's thinking toward giving a strong primacy to the mathematical and conceptual structure of fundamental theories. This change also subordinated the role of principles to this agenda or defined principles in accordance with this role, rather than seeing principles as arising from "empirically discovered … general characteristics of natural processes" as in 1919 (Einstein 1919, p. 228).

[19] The term "atom" came to designate the smallest, but no longer physically indivisible, constituent unit of matter that manifests the identity of a chemical element.

(Einstein 1909a, b). Einstein brilliantly used Bohr's (1913) atomic theory in his great papers of 1916 (Einstein 1916a, b). His subsequent papers on quantum theory were equally groundbreaking (Einstein 1917, 1925a, b).[20] However, in turn spearheaded by Einstein, the hope for a classical-like—realist and causal—constructive theory of the ultimate (quantum-level) constitution of nature has continued to persist and even to remain dominant, and still is.

There were, thus, two "Einsteins:" "the realist Einstein" and "the quantum Einstein," often against his own grain. However, "the realist Einstein," would always prevail. Made even more confident by relativity and then his work in the unified field theory, "the realist Einstein" ultimately exiled "the quantum Einstein" from his philosophical and scientific thought.[21] "The realist Einstein" was also ultimately to take charge of the philosophy or ideology of foundational, including principle, thinking in fundamental physics.

As I said, nonrealism, as defined by the RWR principle, has always remained a minority view, a minoritarian philosophy, notwithstanding the so-called dominance of the Copenhagen philosophy. This dominance, as I said, is largely a myth, and had been even before the spirit of Copenhagen became more overtly marginal during the last four decades or so. Nevertheless, quantum mechanics made it possible, for the first time, to separate fundamental physics from realism, and it has accomplished that as a strictly principle theory, again, the first such theory, without having and even precluding a constructive-theoretical version of it. It related to quantum phenomena and the ultimate reality underlying them by excluding realism from this relation, in accordance with the RWR principle.

As I explained, in Heisenberg's initial thinking leading him to the discovery of quantum mechanics, the representation of quantum objects and their behavior was merely not considered, rather than "*in principle* excluded" as it was eventually by Bohr (1987, v. 2, p. 62). However, Heisenberg, who adopted the QP/QS principle, discovered that a good, indeed as good as possible predictive theory, by necessity probabilistic or statistical (the QP/QS principle), of the phenomena considered was possible in the absence of such a representation. This was the first step on the road leading to the RWR principle, which emerged, sometime in the mid 1930s in the Bohr-Einstein debate, especially in the wake of their exchange concerning the EPR experiment, which also revealed the essential role of the question of locality in quantum theory (EPR 1935; Bohr 1935). This exchange, which occurred at the height of Einstein's work on a (classical-like) unified field theory, reflected and was

[20] For an instructive discussion of Einstein's work in quantum theory, see (Stone 2015).

[21] For these two "Einsteins," see respectively (Stone 2015) and (van Dongen 2010). Van Dongen's study, while offering a helpful account of the realist Einstein, appears to under-appreciate somewhat the quantum Einstein, who is vividly brought out in Stone's book. In fairness, however, van Dongen addresses primarily Einstein's later work, in which the quantum Einstein recedes into the background. In combination, these two *complementary* studies (perhaps also in Bohr's sense) give a comprehensive, complete, Bohr-complete, picture of Einstein. An Einstein-complete picture of Einstein may not be possible: the epistemology of his thinking is too "quantum-like." One ontological claim that appears to hold is that Einstein never gave up on realism and, accordingly, had always remained antagonistic to the spirit of Copenhagen.

defined by the contrasting commitments to fundamental theories in physics—a *realist* and constructive by Einstein, and the *nonrealist* and principle by Bohr. The first involves principles but the second, asymmetrically, does not involve construction at the ultimate level considered by the theory. This exchange was crucial in helping Bohr to establish more firmly his concept of complementarity and realize its fuller significance, and as a result also to reach his ultimate interpretation of quantum phenomena and quantum mechanics, largely grounded in this concept.

It is worth reiterating that constructive theories may and often do involve principles, such as and in particular the principle of (classical) causality, found throughout modern physics from Galileo on until quantum mechanics put it into question. This principle is operative in most constructive theories at two levels. First, the construction itself is the application of the principle of causality, insofar as "building up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out," presupposes a cause-effect relationship between this scheme and these more complex phenomena. Secondly, the formal scheme in question usually represents, by means of the corresponding (realist) mathematical model, a causal architecture of reality.

Conversely, principle theories may involve constructive (given the latter term a somewhat broader meaning, but not essentially different from that of Einstein) and, correlatively, realist dimensions, as exemplified by relativity theory, explaining which occasioned Einstein's reflection on the subject under discussion. Einstein saw special relativity theory as primarily a principle theory, based in two apparently, but only apparently, irreconcilable principles. The first is the principle of relativity, which says, in Einstein's initial formulation, that "the same laws of electrodynamics and optics will be valid for all frames of reference for which the equation of mechanics hold good" (Einstein 1905b, p. 37). The second is the principle of the constancy of the velocity of light regardless of the motion of the source. These two principles are only apparently irreconcilable: special relativity theory reconciles them (in effect also establishing the Lorentz-invariance principle), a reconciliation that was Einstein's great achievement. The theory, however, also had a constructive dimension, emerging from its principles, defined by a new physical scheme of the relativistic structure of space and time, eventually linked to the concept of spacetime (due to H. Minkowski), and the kinematics of relativistic motion. The construction of this scheme may not have been Einstein's starting point, but it is part of special relativity. It is also true that this scheme may not have been exactly simple, and that it transforms rather than underlies the classical, Newtonian, scheme of space, time, and motion, and in this sense does not exactly conform to Einstein's definition of a constructive theory. But one could, I think, expand Einstein's definition of a constructive theory to this or similar cases, consistently with and in the spirit of Einstein's concept.

Einstein's general relativity is similarly both principle and constructive. It is principle because it is based most essentially on the equivalence principle (postulating the equivalence of inertial and gravitational mass). However, it also has a constructive and realist facet and even essence because it represents, *constructs*, the physical nature of gravity as the curvature of space or spacetime (curved by the presence of physical bodies or fields) and describes the behavior of its objects accordingly, in a realist and causal manner. Accordingly, the mathematical embodiment of the principle of general relativity, as well as a (realist) model of the theory, is Riemannian geometry of, in general, variable curvature. In his 1919 article under discussion and elsewhere in his earlier commentaries on the theory Einstein emphasized its principle aspects. Eventually, however, its constructive aspects and specifically its mathematical architecture (gravity made geometry) became dominant in Einstein's assessment of the theory and in his thinking, by then devoted primarily to his attempts to *construct* (in either sense) a unified field theory, as extensively discussed in (van Dongen 2010).

It follows that a principle theory could be either realist or not, in the first case unavoidably bringing with it a constructive dimension, unless the phenomena or objects in question are already given, rather than being constructed as the simpler constituents of more complex phenomena, which is to say, have already been constructed. Constructive theories are, as I explained, nearly by definition realist, unless one uses a given construction as a kind of heuristic device within a predictive (principle) theory. It is also true that a given theoretical construction may be revealed or, as noted earlier, be argued, on Kantian lines, merely to provide a predictive mechanism for a given theory, and, as indicated earlier, there are arguments to that effect concerning the status of spacetimes of general relativity (Butterfield and Isham 2001).[22] That, however, is not the same as developing a given theory as a principle one.

Thus, the distinction between constructive and principle theories is not unconditional, as was clearly realized by Einstein. This realization led him to ever more complex schemes of theoretical practice in fundamental physics, while ultimately preferring a constructive approach. Einstein, as I noted, even became enamored with the idea (the principle?) that this construction and the corresponding physical reality should emerge from a free invention of the mathematical scheme with only minimal, if any, connections to the experiment.[23] There is, however, an asymmetry between these two types of theories: a constructive theory always involves principles, at least philosophical principles, while a principle theory need not involve constructive strata related to the ultimate level considered by the theory, as quantum mechanics came to show, arguably for the first time. Quantum mechanics and quantum field theory are fundamentally principle theories, at least in nonrealist interpretations of them. Quantum mechanics is, again, the first theory that, in such interpretations, is strictly principle insofar as it precludes the claim for the constructive theorization of quantum objects and processes. The old quantum theory had constructive dimension to it, and was partially realist. Some phenomena considered by the theory, such as "quantum jumps," as they were called then, defining electrons'

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[22] In special relativity, too, the physical status of the concept of spacetime poses difficulties, which also tempt one to see it as part of the mathematical models of theory rather than a representation of physical reality. This concept is not found in Einstein's original formulation and was introduced by Minkowski (a mathematician) in 1908. Einstein, initially skeptical about the concept, ultimately adopted it and also used it in general relativity.

[23] For details, see (van Dongen 2010, pp. 89–95).

transitions between energy levels, defied mechanical models, abandoning which in considering quantum jumps as no longer subject to any such model led Bohr to his 1913 theory. This was a key precursor of Heisenberg's approach, which abandoned any mathematical representation of the behavior (perhaps no longer even motion!) of electrons in atoms altogether. While quantum mechanics has rarely been expressly described as a principle theory in accordance with Einstein's definition just given (one significant exception, albeit quite different from the present approach, being [Bub 2000]), the principle nature of quantum mechanics, at least Heisenberg's matrix mechanics was manifested from the outset, ultimately leading to nonrealist, RWR-principle based, interpretations of the theory. If interpreted in this way, quantum mechanics is a principle theory by definition, because it is not possible to "construct" the ultimate entities, quantum objects, responsible for the appearance of quantum phenomena. That is, such is the case unless, as I said, one sees quantum objects as *constructed* as *unconstructible*, possibly to the point of placing their character beyond thought altogether, which is the strongest form of the RWR principle.

I would like to close this section and this chapter by stressing that an appeal to fundamental principles need not imply that there is some unchanging, Platonist-like, essence to such principles. Principles change as our experimental findings and our theories change, and we cannot anticipate all of these changes, which are effected by forces that our existing theory and principles cannot control that can and even are bound to overtake these theories and principles by new ones. While having confidence in a given set of principles can help and drive one's creative work, an uncritical, dogmatic acceptance of any such set can inhibit and even prevent an advancement of thought and knowledge, which may require new principles, the invention of which is, I argue, one of the greatest forms of creativity in physics and beyond.

The principles of quantum mechanics replaced, within a new scope, some among the main principles of classical physics, which continue to remain operative within the proper scope of classical physics. Some of the principles of classical physics, however, extend to quantum mechanics, for example, as applicable to the classical physics of measuring instruments. There could be such changes within the same physical scope, as happened in the case of general relativity theory vs. Newton's theory of gravity, which was proven to be only an approximation even within its proper scope, because accounting for some phenomena, such as the motion of Mercury, required general relativity. (As explained earlier, Einstein's main motivation for general relativity was defined by certain general principles he assumed as valid or necessary and not by any specific experimental facts, which entered the picture later, confirming the theory and thus his principle intuition.) Some of the principles of quantum theory have changed as well, although in this case the development of the theory after quantum mechanics (which was a change from the old quantum theory within the same scope) was only defined by expanding the scope of quantum theory. The QP/QS principle has remained in place throughout the history of quantum mechanics, from Heisenberg on, although the mathematical expression of the principle was refined a few times, which fact tells us that there is no Platonist-like essence to the connections between mathematics and physics either, or even to mathematics itself. On the other hand, the correspondence principle has changed in its definition and functioning, by becoming the "mathematical correspondence principle," in Heisenberg's initial work on quantum mechanics, and it was given an even more general mathematical form in Dirac's work. While it was intimated by Heisenberg in his paper introducing quantum mechanics and while it could have been inferred from the complementarity principle, introduced in 1927, the RWR principle emerged a decade later in Bohr's work, as part of his, by then changed in turn, interpretation of quantum phenomena and quantum mechanics. In the 1990s, the principles of quantum theory were developed into those of quantum information theory, to be discussed in Chap. 7. Combining the principles of relativity and quantum mechanics by, ultimately, giving precedence to the principles of quantum mechanics led Dirac to the discovery of his famous equation for the relativistic electron. The discovery of antimatter, which was an unexpected bonus, eventually led to yet new principles, grounding quantum field theory and the standard model of elementary particle physics, thus extending the trajectory of quantum theory from Planck's quantum of action to the Higgs boson.

This is hardly the end, however, rather a continuous beginning. Where we go from here is a big question. But then again, it is the question that quantum theory and fundamental physics in general never stops asking, because it never really arrives at final answers, although, on occasion, as before Planck's discovery of the quantum action, it may be under an illusion that it was near such an answer. We have no such illusions after the Higgs boson. This question "Where do we go from here?" defines the trajectory, a complex web of trajectories, of foundational thinking in fundamental physics, and of its search for new fundamental principles and, in the absence of any Platonic essence to such principles, new *kinds* of fundamental principles, without necessarily abandoning, not completely in any event, the old ones. There is no end to fundamental physics, and it never had a real beginning either. Even Thales, Democritus, and other pre-Socratics, and Plato (there is physics in Plato, too), and certainly Aristotle, or in modern time, Galileo and Newton, were extending preceding physics, moving it on. Physics has always been and is likely to continue to be *moving on* to ever-new territories, to never ending new frontiers, which sometimes emerge within already established territories, expanding them from within.

Chapter 2
Bohr, Heisenberg, Schrödinger, and the Principles of Quantum Mechanics

Abstract The conceptual core of this chapter is Heisenberg's discovery of quantum mechanics, considered as arising from certain fundamental principles of quantum physics and as established by giving these principles a mathematical expression. The chapter also considers Bohr's 1913 atomic theory, a crucial development in the history of quantum theory ultimately leading to Heisenberg's discovery, and Schrödinger's discovery of wave mechanics, initially from very different physical principles. At the same time, Schrödinger had implicitly used some of the same principles that were expressly used by Heisenberg, thus meeting Heisenberg's program, against Schrödinger's own grain. After a general introduction given in Sect. 2.1, Sect. 2.2 considers some of the key aspects of Einstein's and Bohr's work in the old quantum theory, especially significant for the invention of quantum mechanics by Heisenberg and Schrödinger, discussed in Sects. 2.3 and 2.4, respectively. Sect. 2.5, by way of a conclusion, reflects on the new relationships between mathematics and physics established by quantum mechanics in nonrealist, RWR-principle-based, interpretations.

2.1 Introduction

Although it also gives major attention to Bohr's and Schrödinger's work, the conceptual core of this chapter is Heisenberg's discovery of quantum mechanics, considered as arising from certain fundamental principles of quantum physics and established by giving these principles a mathematical expression. That need not mean that such physical principles are formulated first, independently of their mathematical expression, and are then given this expression. The relationships between mathematics and physics are reciprocal in an actual process leading to a scientific discovery, and it is not always possible to establish which of them comes first, or to entirely separate them within this process. As discussed in Chap. 1, it is difficult, in modern, mathematical-experimental, physics, to ever have purely physical, empirical principles, entirely free of conceptual and specifically mathematical expression. Heisenberg's discovery exemplifies this situation as well, although overall physics tends to lead the way, to some degree in contrast to Dirac's work, to be discussed in Chap. 6, where mathematics takes the lead (and philosophy takes a back seat and

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indeed is left to others). It is true that some of the fundamental principles in question were more implicit than expressly formulated physically and that their mathematical expression was sometimes *relatively* provisional in Heisenberg's work leading to this discovery, as manifested in his first paper on quantum mechanics arising from and embodying this work, on which I shall focus here. Essentially, however, this mathematical expression was correct (hence, my emphasis on relatively). Heisenberg's scheme was developed into a full-fledged matrix quantum mechanics by Born, Jordan, and Heisenberg himself in three subsequent papers, and along different lines (using *q*-numbers) by Dirac (Born and Jordan 1925; Born et al. 1926; Dirac 1925). Born and Jordan deserved much credit for their work, not always fully given to them. Their (to return to Einstein's view of Born's work at the time) "virtuoso" mathematical work has been recognized most, although much of it quickly lost its significance. The reason was that Schrodinger's formalism, introduced shortly thereafter, became the primary mathematical tool of quantum mechanics, soon to be combined with the more formal approaches of Dirac and von Neumann, which, especially that of von Neumann, eventually became dominant. Matrix mechanics was cumbersome to work with, and one nearly needed to be a mathematical virtuoso to use it, as opposed to these alternative schemes. Born and Jordan's physics, however, was superb as well, especially their rigorous proof that conservation laws remain valid in matrix mechanics, and it deserves more recognition than it has received, even though most key ingredients of the theory were in place in Heisenberg's original paper. His contribution, both physical and mathematical, to the development of the full-fledged version of matrix mechanics was significant as well, especially as concerns the role of the key principles involved, in particular the correspondence principle (Born et al. 1926, p. 322; Plotnitsky 2009, pp. 105–107).

Heisenberg's discovery of quantum mechanics was among the most momentous discoveries in the history of physics, nearly comparable to Newton's discovery of classical mechanics (nothing perhaps could ever match it), Maxwell's discovery of his equations for electromagnetism, and Einstein's discoveries of special and then general relativity. However, as already explained, the relationships between the mathematical formalism invented by Heisenberg and the physical phenomena considered were entirely different—based in very different fundamental principles—in Heisenberg's scheme than in other theories just mentioned or indeed all physical theories prior to quantum mechanics, in part by virtue of new principles he adopted and put to work. Principles, again, are also defined by their guiding role in one's thinking. Heisenberg's new "calculus" (the term he sometimes used, undoubtedly with Newton in mind) did not represent the behavior of quantum objects. This calculus only related quantum phenomena, defined by what is manifested in measuring instruments impacted by quantum objects, in terms of probabilistic or statistical predictions concerning certain possible phenomena defined by possible future measurements on the basis of phenomena defined by the measurements already performed. As explained in Chap. 1, while essentially probabilistic or statistical in character, the situation is different from the one that obtains in classical statistical physics or other classical situations in which the recourse to probability or statistics becomes necessary, including, it may be noted, in the pre-quantum electron theory of H. Lorentz and others. In these cases, the individual constituents of the systems considered are, in contrast to elementary individual quantum systems, assumed to behave causally and to be treated by a representational and even visualizable mathematical model of classical mechanics, and, thus, as ideally or in principle predictable exactly, deterministically, by this model. However, because of the mechanical complexity of these systems, which (or correlatively, the behavior of their individual constituents) cannot be tracked in practice, the recourse to probability or statistics becomes necessary and requires a different type of the overall mathematical model. This model, however, is defined by the assumption, just stated, that the behavior of the individual constituents of these systems obeys the laws of classical mechanics and is described by its representational model. As noted in Chap. 1, this assumption, as Einstein was the first to show, was incompatible with the statistical laws of quantum theory, beginning with Planck's black body radiation law. By so doing, Einstein initiated the divorce between probability and (classical) causality, the union that made the use of probability in physics a practical matter, rather than a fundamental or a principle one, as it became in quantum theory, at least in certain, specifically nonrealist, interpretations of it. As also explained in Chap. 1, in the case of quantum phenomena, predictions concerning the outcomes of quantum experiments would have to be probabilistic or statistical, regardless of the theory that makes them, even in the case of elementary individual processes and events, such as those involving individual electrons or photons. This is because it is a well-established fact that identically prepared quantum experiments (as concerns the state of the measuring instruments involved) in general lead to different outcomes even in these cases. This situation represents the physical content of the quantum probability or statistics (QP/QS) principle, assumed by Heisenberg. Thus, unlike in classical physics, in quantum physics, there is no difference between predicting the behavior of individual or composite quantum systems: all such predictions are equally probabilistic or statistical.

Heisenberg's approach, influenced by Bohr's 1913 atomic theory and based on Bohr's correspondence principle (which was given a more rigorous mathematical form by Heisenberg), became the foundation for Bohr's interpretation of quantum phenomena and quantum mechanics in terms of complementarity, discussed in Chap. 3. The present chapter addresses some of Bohr's ideas as well, mainly focusing, however, on Bohr's work prior to his introduction of complementarity in 1927.

The mathematical formalism of quantum mechanics was developed from a different starting point and on more classical (realist and causal) lines, from more classical-like principles, by the theory's cofounder, Schrödinger. This tells us that there may be more than one way of thinking that leads to a correct theory. Nor, it also tells us, can one be certain what theory, or what *kind* of theory, will be more effective in helping us to solve the new (or sometimes old) problems nature confronts us with. I shall devote part of this chapter to Schrödinger's thinking in his work on his wave mechanics. This type of thinking continues to serve as inspiration for alternative approaches to quantum mechanics or to quantum phenomena themselves, resulting in theories that are different from quantum mechanics, on some of which I shall comment here and later in this study. Equally significant for my argument is that, along with classical-like principles, Schrödinger *had* in effect used some of the same principles that were expressly used by Heisenberg. In this way, Schrödinger's program met that of Heisenberg, against Schrödinger's own grain, given that his program was expressly developed as an undulatory (wave) alternative to matrix mechanics. While reflected in the mathematical equivalence of both models, this meeting place was defined by deep physical aspects of nature or our interactions with nature, features embodied in the fundamental principles of quantum theory, to which the formalism of quantum mechanics gave, with Schrödinger's help, a proper mathematical expression.

2.2 Following and Moving Beyond Einstein: Bohr's 1913 Atomic Theory

Bohr wrote his paper, "On the Constitution of Atoms and Molecules," which introduced a new model of the hydrogen atom, in 1913. It became the first part of his 1913 "trilogy," eventually published in his book 1924 book *The Theory of Spectra and Atomic Constitution* (Bohr 1924). While Bohr's work built on previous discoveries of Planck and, especially, Einstein, it also departed from them by making more radical assumptions concerning the behavior of both light and electrons. As is well known, the idea of a photon, as a particle of light, was rejected by Bohr, who until the early 1920s, still believed, more in accord with Planck, in the different, rather than common, character of particles, such as electrons, and radiation, such as light. This commonality was established more firmly by L. de Broglie's discovery of matter-waves, around the same time as the particle character of photons was established by Compton's experiment, which findings, finally, convinced Bohr. The corresponding elementary particles or fields were to be distinguished differently, for example and in particular, in terms of spin and statistics, fermions vs. bosons. Bohr had his reasons for his resistance to the idea of photons as particles. These reasons or this resistance itself (in fairness, far from uncommon at the time) is, however, secondary here. This is not only because he ultimately accepted the idea, but also and primarily because the most important and radical aspects of Bohr's 1913 thinking did not depend on whether one treated photons as particles or as merely quanta of energy. All of Bohr's key points would have equally applied and even amplified if he had accepted the photon hypothesis at the time. As Einstein was among the first to realize, Bohr's paper transformed our understanding of the ultimate nature of both radiation, such as light, and matter, such as electrons. Bohr's ideas also prepared the way for an even more radical revolution, physical, mathematical, and philosophical, brought about by quantum mechanics, a type of theory that never became acceptable to Einstein as an adequate account of the ultimate constitution of nature. As he famously said: "There is no doubt that quantum mechanics has seized hold of a beautiful element of truth and that it will be a touchstone for a future theoretical basis in that it must be deducible as a limiting case from that basis, just as electrostatics is deducible from the Maxwell equations of the electromagnetic field or as thermodynamics is deducible from statistical mechanics. I do not believe that quantum mechanics will be the starting point in the search for this basis, just as one cannot arrive at the foundations of mechanics from thermodynamics or statistical mechanics" (Einstein 1936, p. 361).

I shall only summarize the essential features of Bohr's theory, because I am primarily concerned with the main implications of Bohr's thinking and argumentation for the development of quantum mechanics, rather than with giving a proper historical account of Bohr's 1913 atomic theory and its development before quantum mechanics entered the stage.[1] Bohr's theory ambitiously aimed to remedy the difficulties of his former mentor Ernst Rutherford's earlier "planetary model" of the atom, with electrons orbiting atomic nuclei. Although a revolutionary conception in turn, this model was inconsistent with classical electrodynamics, which would dictate that the electrons in an atom would nearly instantly spiral down into the nucleus, and hence that atoms would not be stable, while they are manifestly stable. Bohr's theory avoided these difficulties by postulating an (only) partial inapplicability of classical electrodynamics, as well as classical mechanics. Similarly to Einstein's thinking in special relativity before him and Heisenberg's thinking in quantum mechanics after (and following) him, Bohr's thinking reversed the preceding thinking, in part even that of Einstein concerning quantum phenomena. Bohr saw as a solution where the preceding theorists, even Einstein (who used a similar strategy in his quantum-theoretical and his relativistic thinking), saw a problem. As quantum revolutionary as he was in turn, Einstein was reluctant to make the type of move made by Bohr, arguably because of his realist views.

Bohr's theory, unlike that of Rutherford, was based on Planck's and Einstein's *quantum* theories, which postulated the possibility of the discontinuous emission of light in the form of light quanta (or energy), $h\nu$, ultimately understood as photons, courtesy of Einstein. Making his own revolutionary and audacious move, Bohr postulated both the so-called stationary states of electrons in the atom, at which they could remain in orbital motion, and discontinuous "quantum jumps" between stationary states, resulting in the emission of Planck's quanta of radiation, without electrons radiating continuously while remaining in orbit, thus, in conflict with classical electrodynamics. In addition, again, in contradiction to the laws of classical electrodynamics, Bohr postulated that there would exist a lowest energy level at which electrons would not radiate, but would only absorb, energy.[2] Bohr abandoned, as apparently hopeless, an attempt to offer a mechanical explanation for such transitions, as opposed to the stationary states themselves. The latter, he said, "can be

[1] Among helpful accounts are (Kragh 2012), which offers a comprehensive treatment of Bohr's atomic theory in its historical development, and (Folse 2014), a brief, more philosophically oriented, account.

[2] Bohr's 1913 postulates should not be confused with Bohr's more general concept of "the quantum postulate," introduced, along with the concept of complementarity, in 1927, following quantum mechanics, although the quantum postulate, too, concerned quantum phenomena themselves and did not depend on quantum mechanics (Bohr 1927, 1987, v. 1, pp. 52–53).

discussed by help of the ordinary mechanics, while the passing of the system between different stationary states cannot be treated on that basis" (Bohr 1913, p. 7). A decade later, Heisenberg will abandon attempts to mechanically treat stationary states as well, doing which was becoming increasingly difficult and ultimately impossible in the interim (Heisenberg 1925).[3] Bohr's postulates were, thus, in manifest conflict with both classical mechanics, because they implied that there is no mechanical explanation for "quantum jumps" between orbits or stationary states, and with classical electrodynamics, because of the way in which electrons would (or, in the case of the lowest energy states, would not) radiate energy.

On this point, Bohr's thinking also moved beyond Einstein's thinking, revolutionary as the latter was in turn, concerning the quantum nature of radiation, for the following reasons. In Bohr's theory, the electron would absorb or emit energy only by changing its orbital state from energy E_1 to energy E_2. The frequency of the absorbed or emitted energy was defined in accordance with Planck's and Einstein's rule as $h\nu = E_1 - E_2$. In order, however, to get his theory to correspond with the experimental data (spectral lines) in question, Bohr combined this postulate with another quantization rule or postulate, which allowed that energies for orbiting electron were whole number multiple of h multiplied by half of the final orbital frequency, $E = \frac{1}{2} nh\nu$. It was thus half of the energy, $E = nh\nu$, that Planck, in deriving his black body radiation law, assumed for his oscillators. These two assumptions, combined with classical formulas that related the frequency of an orbit to its energy, gave Bohr the Rydberg frequency rules for hydrogen spectral lines, well established by then. Thus, in Bohr's scheme, only certain frequencies of light could be emitted or absorbed by a hydrogen atom, strictly in correspondence with Rydberg rules. Another point is worth noting here, courtesy of L. Freidel (2016). The classical electron theory of H. Lorentz and his followers considered the probability of finding an electron in a given state, under the underlying realist assumptions, in particular that of (causally) representing the motion of electrons in terms of oscillators. Bohr's theory was instead concerned with the probabilities of *transitions* between stationary states, thus essentially defining quantum discreteness and the QD principle, without assuming the possibility of representing these transitions and, as a result, abandoning causality as well. This change of attention toward transition probabilities was central to Einstein's remarkable treatment, using Bohr's theory, of spontaneous and induced emission and absorption of radiation (Einstein 1916a, b), and then to Heisenberg's discovery of quantum mechanics, which abandoned any attempt at a mechanical (orbital) representation of even stationary states, as well as of transitions between them (Heisenberg 1925). Finally, this shift was also central

[3] Both Bohr's theory and quantum mechanics predicted the probabilities or statistics of transitions between them, but unlike Bohr's theory, which treated stationary states classically and hence also by representing them (as orbits), matrix mechanics did not treat the behavior of electrons in stationary states at all. Dirac's q-number scheme and then Schrödinger's equation were able to do so, but now also in probabilistically or statistically predictive terms, rather than representational terms (against Schrödinger's initial hopes). As I said, by that time the concept of electron orbit was no longer possible to sustain even for stationary states.

to Dirac's first paper on quantum electrodynamics and thus to the birth of the latter (Dirac 1927b, Schweber 1994, pp. 24–32). Note that one no longer thinks so much in terms of discrete quantum *objects*, such as electrons, but rather, in virtually Heisenberg's terms, of discrete *states* of these objects and probabilities of predicting these states. It follows that there is no longer either any underlying continuity or any underlying causality of quantum processes, but only probabilities of transitions between allowed stationary states. Pauli's 1925 exclusion principle put further restrictions on such allowed states in atoms with two or more electrons (Pauli 1925). However, while thus building on Einstein's ideas, Bohr's theory was also a more radical departure from classical electrodynamics than Einstein's work dared to be (prior to 1913, as opposed to Einstein's subsequent papers on quantum theory, written with Bohr's theory in hand). According to A. D. Stone:

> Bohr did something so radical that even Einstein, the Swabian rebel, had found it inconceivable: Bohr *dissociated* the frequency of the light emitted by the atom from the frequency at which the electron orbited the atom. In the Bohr formula, $\left[h\nu = E_1 - E_2\right]$, there are two electron frequencies, that of the electron in its initial orbit and that of the electron in its final orbit; *neither* of these frequencies coincides with the frequency, ν, of the emitted radiation! This was a pretty crazy notion to a classical physicist, for whom light was *created* by the acceleration of charges and must necessarily mirror the frequency of the charge motion. Bohr admitted as much: "How much the above interpretation differs from an interpretation based on the ordinary electrodynamics is perhaps most clearly shown by the fact that we have been forced to assume that a system of electrons will absorb radiation of a frequency different from the frequency of vibration of electrons calculated in the ordinary way" (Bohr 1913, p. 149). However, he noted, using his new rule, "obviously, we get in this way the same expression for the kinetic energy of an electron ejected from an atom by photo-electron effect as that deduced by Einstein" (Bohr 1913, p. 150). So, as his final justification, he relied on exactly the experimental evidence that motivated Einstein's light-quantum hypothesis and inaugurated the search for a new atomic mechanics. (Stone 2015, pp. 177–178)

Stone is right to speak only of the same "experimental evidence," because Einstein's hypothesis already used the concept of photon, which Bohr, again, rejected at this point. Bohr's theory proved to be correct, as Einstein, who realized its revolutionary nature specifically on this point, came to recognize soon thereafter and to use it with great effectiveness in his subsequent work (Stone 2015, pp. 177–178). Apart from explaining quite a few previously known and puzzling data, the theory also quickly proved its predictive power. In essence, Bohr's postulates have remained part of quantum theory, thus further suggesting, as did Einstein's previous work, that a classical mechanical theory and laws do not apply to the quantum constitution of matter. The postulates were given a proper mathematical expression only with quantum mechanics. They were given a more rigorous meaning by Bohr's interpretation of quantum mechanics and quantum phenomena, which, beginning with Bohr's 1927 concept of the quantum postulate (which is different from his 1913 postulates, stated above), reconceived quantum discreteness, the QD principle, in terms of quantum phenomena, rather than the Democritean atomicity of quantum objects themselves. In retrospect, Bohr's 1913 postulates almost cried out: "give up the idea of orbits!" Heisenberg did just that, which, however, took another decade. But a retrospective view, while not without its benefits, is rarely a reliable guide to how discoveries occur. While the view of quantum theory as a (probabilistic or statistical) theory predicting the transitions between states was there to stay and still governs quantum theory, the idea of orbits for stationary states soon ran into major difficulties (such as some of these orbits falling into the nucleus) for this cry to be heard, albeit not by everyone. In writing to Pauli (who did question the idea of orbits previously but still appears to have failed to completely renounce it) soon after his discovery of quantum mechanics, Heisenberg says:

> 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 (*hv*-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 Mehra and Rechenberg 2001, v. 2, p. 284; emphasis added)

This was yet to come, however. In the meantime, Einstein, convinced by major experimental confirmations (e.g., the Pickering-Fowler spectrum), accepted Bohr's theory, which he admiringly saw as a "miracle" (Einstein 1949a, pp. 42–43) and used in his great 1916 papers (in which he re-derived Planck's law yet, again) (Einstein 1916a, b). Conceptually, however, Bohr's theory could not satisfy Einstein's realist hope, anymore than later on quantum mechanics could, and both could only be seen by Einstein as at most correct but not as complete, Einstein-complete. His predilection for a classical-like field theory must have played a role as well, and it is worth keeping in mind that at the time he was working and publishing articles on his general relativity (completed in 1915), which was a theory of continuous fields, an ideal of a fundamental theory never relinquished by Einstein (Einstein 1949a, pp. 83–85). According to G. Hevesy, Einstein himself had "similar ideas [to those of Bohr], but did not dare to publish them" (Stone 2015, p. 178). His assessment of the theory as a miracle may not have been without a certain ambivalence either: it was more a miracle than a theory. As Stone notes: "Bohr's atomic theory was hardly the new mechanics for which Einstein had been searching. There was still no underlying principle to replace classical mechanics, just another ad hoc restriction on classical orbits, a variant of Planck's desperate hypothesis" (Stone 2015, p. 178).

That was never to change. In a way, the subsequent developments of quantum theory leading to quantum mechanics, which offered such underlying principles, and beyond made things worse, as far as Einstein was concerned. These new principles continued to define quantum mechanics and then quantum electrodynamics and quantum field theory, without giving way to any underlying classical-like principle or set of principles that Einstein wanted. Einstein in turn never abandoned his "search for a more complete conception," ideally an Einstein-complete realist field theory, which would, again, avoid probability and statistics at the ultimate level (Einstein 1936, p. 375; also Einstein 1949a, pp. 83–85). Without ever accepting the nonrealist implications of his findings, Einstein continued to make major contributions to the probabilistic and statistical understanding of quantum theory, which at the same time were reaffirming his concept of the photon. His 1916 papers cited above, made a remarkable use of Bohr's theory, which Einstein by then fully accepted, again, as correct, but not complete, which was an assessment that he later extended to quantum mechanics (Einstein 1916a, b). Einstein's epistemological reservations are understandable: Bohr's 1913 theory was a decisive step, arguably the decisive first step, on the *nonrealist* trajectory of quantum theory.

Bohr, by 1913 back in Denmark after a few years in England, sent the manuscript of his paper to E. Rutherford, the editor of *Philosophical Magazine*, a leading *physics* journal, founded a century earlier, where Bohr wanted to publish the paper and its sequels already in preparation. Upon reading the paper, Rutherford, in a letter to Bohr, in addition to making a crucial remark concerning Bohr's argumentation (on which I shall comment below), offered a "criticism of minor character" concerning "the arrangement of the paper":

> I think in your endeavour to be clear you have a tendency to make your papers much too long, and a tendency to repeat your statements in different parts of the paper. I think that your paper really ought to be cut down, and I think this could be done without sacrificing anything to clearness. I do not know if you appreciate the fact that long papers have a way of frightening readers, who feel that they have not time to dip into them. … I will go over your paper very carefully and let you know what I think about the details. I shall be quite pleased to send it to *Phil. Mag.* but I would be happier if its volume could be cut down to a fair amount. In any case I will make any corrections in English that are necessary. … I shall be very pleased to see your later papers, but please take to heart my advice, and try to make them as brief as possible consistent with clearness. … P.S. I suppose you have no objection to my using my judgment to cut out any matter I may consider unnecessary in your paper? Please reply. (A Letter to Bohr, March 20, 1913, reproduced in "The Rutherford Memorial Lecture," Bohr 1987, v. 3, p. 41)

In commenting on this criticism of Rutherford in "The Rutherford Memorial Lecture" in 1958, Bohr said: "[This] point raised with such emphasis in Rutherford's letter brought me into a quite embarrassing situation. In fact, a few days before receiving his [letter] I had sent Rutherford a considerably extended version of the earlier manuscript. …" (Bohr 1987, v. 3, p. 42). Rutherford, in meantime, tried to reason with Bohr again, now in responding to an expanded version of the paper, a few days later: "The additions are excellent and reasonable, but the paper is too long. Some of the discussions should be abbreviated. As you know it is the custom in England to put things very shortly and tersely, in contrast to the German method, where it appears to be a virtue to be as long-winded as possible" (A Letter to Bohr, March 250, 1913, cited in Rosenfeld 1963, p. xiv).

Bohr "replied" by taking a ship from Copenhagen to Manchester. According to his recollections in his Rutherford Memorial Lecture, which notes the "embarrassing" nature of the situation, after sending to Rutherford an even longer version:

> I therefore felt the only way to strengthen matters was to get at once to Manchester and talk it all over with Rutherford himself. Although Rutherford was as busy as ever, he showed an almost angelic patience with me, and after discussions through several long evenings, during which he declared he had never thought I should prove so obstinate, he consented to

> leave all the old and new points in the final paper. Surely, both style and language were essentially improved by Rutherford's help and advice, and I have often had occasion to think how right he was in objecting to the rather complicated presentation and especially to the many repetitions caused by references to previous literature. (Bohr 1987, v. 3, p. 42)

Well, perhaps! But then something reflecting the character of Bohr's thinking would be lost as well. Besides, we do not know the details of these negotiations and what Rutherford aimed to cut or change. Be that as it may, Bohr's determination and Rutherford's patience both deserve credit for bringing Bohr's paper to publication. Eventually Bohr received his Nobel Prize for the work presented in this and related articles. More importantly they changed the course of atomic physics as only few works have done.

Rutherford, in his letter, also made a substantive comment, reaching to the core of Bohr's argument. He said: "There appears to me one grave difficulty in your hypothesis, which I have no doubt you fully realise, namely, how does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you would have to assume that the electron knows beforehand where it is going to stop" (A Letter to Bohr, March 20, 1913, reproduced in "The Rutherford Memorial Lecture," Bohr 1987, v. 3, p. 41). In 1917, Einstein, continuing his own exploration of the nature of the quantum, now with Bohr's work in hand, added a related question: "How does an individual light-quantum, emitted in an atomic transition, know in which direction to move?" (Einstein 1917, p. 121, cited in Pais 1991, p. 153). Pais, who cites both Rutherford's and Einstein's remarks in his biography of Bohr, comments as follows: "In typical Rutherford style he had gone right to the heart of the matter by raising the issue of cause and effect, of causality: Bohr's theory leaves unanswered not only the question why there are discrete states but also why an individual electron in a higher [energy] state chooses one particular lower state to jump into" (Pais 1991, p. 153). Leaving the language of "choice" on the part of electrons and photons aside for the moment (I shall return to this subject below), Rutherford's and Einstein's statements represent the classical—realist and causal—way of thinking, which neither ever gave up and with which Bohr was already willing to part even then. As I said, contrary to Rutherford's view of Bohr's hypothesis as "a grave difficulty," Bohr saw the situation and, hence, his hypothesis as a solution rather than a problem, thus anticipating and inspiring Heisenberg's attitude in his discovery of quantum mechanics. Quantum mechanics "answered" these questions more fully, albeit not to Rutherford's or Einstein's satisfaction. Pais concludes by saying: "These questions [of Rutherford and Einstein] were to remain unresolved until … quantum mechanics gave the surprising answer: they are meaningless" (Pais 1991, p. 153). That may be, but not to Rutherford and Einstein, or to many others following them. Accepting this answer requires a very different philosophical attitude, which remains a minority view; and, if the situation is accepted as unavoidable, as it was at the time or now, it is often seen as unfortunate and, hopefully, temporary. We have, however, continued to confront and debate quantum mechanics for over a century now.

While Rutherford was primarily an experimental physicist, who also made important theoretical contributions, Bohr was primarily a theoretical physicist, who had, however, done important experimental physics earlier in his career. Bohr's first published paper was on the experiments he had performed himself dealing with the surface tension of liquids, admittedly his only experimental paper, but a significant contribution to experimental physics, nevertheless. His second published paper dealt with the theoretical part of the same problem and was purely theoretical. These papers stemmed from Bohr's entry into a 1905 prize competition concerning this problem, a competition that Bohr won (Pais 1991, pp. 101–102). Bohr also worked in Rutherford's lab, where he began to develop his ideas concerning the atomic constitution of matter, eventually leading to his 1913 atomic theory. Bohr valued experimental physics greatly and championed its significance throughout his life. It is, accordingly, not surprising that his interpretation of quantum phenomena and quantum mechanics, and the key principles in which this interpretation was based (the QD principle, the QP/QS principle, the complementarity principle, and the RWR principle) were all grounded in experiment.

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). One might question the view that Bohr was primarily a philosopher, rather than a physicist. He was clearly both, and even the mathematical aspects of physics, granted, more significant for Heisenberg's thinking, played a greater role in Bohr's thinking than it might appear (Bohr 1956). I would further argue that he was at his best as a philosopher when thinking philosophically about physics, rather than in extending, usually in a preliminary and tentative way, his ideas, such as his concept of complementarity, beyond physics. Bohr's thinking concerning quantum phenomena and the principles of quantum theory may be said to be both *fundamentally* physical and *fundamentally* philosophical. It is fundamentally physical, and not only theoretical but also experimental, because, as Heisenberg said, "its every detail can be subjected to the inexorable test of experiment." However, as explained in Chap. 1, no such test is possible, apart from our conceptual, philosophical determination of both physical experiments and what they tell us about how *nature* makes the outcomes of these experiments possible. I underline nature, because, while we can control the set-ups of such tests, we can never fully control their outcomes, including in quantum physics. This fact, as Bohr stressed, further ensures the objectively verifiable nature of our experiments and thus the disciplinary character of quantum physics and its continuity with classical physics and relativity. On the other hand, in quantum physics, what kind of experiment we perform, what kind of questions we ask, determines the course of reality (in the absence of realism), rather than follows what would have happened independently of our interaction with quantum reality. This is a crucial point to which I shall return later in this study.

Bohr follows, *up to a point*, Einstein, his great philosophical enemy, or rather his greatest philosophical enemy and his greatest *philosophical* friend. I stress philosophical, because personally they were always friends. I would argue, however, that they were philosophical friends in part by being philosophical enemies, because their confrontation helped each to deepen and develop his philosophical views. As noted in Chap. 1, Einstein assumed, against Mach, the principle of the conceptual determination of the observable. The principle implies that one can only develop, as far as it is humanly possible, a true understanding of the nature of physical reality through a *free* conceptual construction, and not merely, if at all, on the basis of experience, and hence experimental evidence. Bohr follows this principle insofar as he sees such a conceptual construction (which is, as I said, never quite free) as decisive as well: there could be no quantum mechanics or his interpretation of it, or of quantum phenomena, otherwise. Bohr's concept of a quantum jump in his 1913 theory is, admittedly, somewhat more empirical, albeit it not completely so. But his concepts of the quantum postulate in the Como lecture of 1927, also complementarity introduced there but modified in his later works, and phenomena or atomicity in the 1930s, are all examples of this conceptual construction, also using the term concept in the sense of this study, as a multi-component entity defined by the organization, architecture, of these components. However, Bohr departs from Einstein insofar as this conceptual construction is, in his interpretation, no longer in the service of representing the ultimate nature of quantum *reality*, quantum objects and processes, including the primitive individual ones. This was Einstein's main realist imperative for a physical theory, a principle in effect. This imperative was never abandoned in Einstein's work on quantum theory either, before or after Bohr's atomic theory, which Einstein used with great effectiveness in his work. For Bohr, by contrast, his conceptual construction was in the service of our predictions concerning the outcomes of quantum experiments, or quantum phenomena, predictions enabled by quantum theory, and phenomena made possible by unconstructible quantum objects, and by our experimental, technological construction enabling our interactions with quantum objects.

Einstein, again, found this way of thinking about physics "logically possible without contradiction," but "very contrary to his scientific instincts," which were also, and even in the first place, his philosophical instincts, based in his realist imperatives (Einstein 1936, p. 375). And yet these were at least some elements of this type of thinking (admittedly we are as yet quite far from the RWR principle or even a proto-RWR principle used by Heisenberg in 1925) that Einstein acknowledged in invoking "insecure and contradictory foundations" which, nevertheless, led Bohr to his 1913 theory. According to Einstein's comment made 30 years later: "That this insecure and contradictory foundation [of the old quantum theory] was sufficient to enable a man of Bohr's unique instinct and sensitivity to discover the principal laws of the spectral lines and of the electron shells of the atoms, together with their significance for chemistry, appeared to me as a miracle—and appears to me a miracle even today. This is the highest musicality in the sphere of thought" (Einstein 1949a, pp. 42–43; translation modified). Although beautiful and reflecting the magnitude of Bohr's achievement in a way undoubtedly gratifying to Bohr, the comment, as I said, still appears to suggest Einstein's unease concerning the "foundations" on which Bohr built his theory, which, it should not be forgotten, was semiclassical (a common name for the old quantum theory) and thus partially realist. However, it was ultimately its other, "nonrealist," half that took over and, beginning with Heisenberg's discovery, was fully developed, in the spirit of Copenhagen, by Bohr. These foundations never became secure for, or were accepted as secure or even as foundations by, Einstein (he, again, admitted that quantum mechanics was a consistent theory revealing a partial truth about nature), as his overall reflections on the same occasion and throughout his philosophical writings make clear. On the other hand, one might argue that this highest musicality in the sphere of thought had never left Bohr's thinking about quantum physics. If anything its harmonies had became ever more complex, without losing any of their musicality.

Bohr realized not only that "natural philosophy in our day and age carries weight only if its every detail can be subjected to the inexorable test of experiment," as Heisenberg said, but also that, reciprocally, the experimental evidence has a conceptual and hence philosophical dimension to it, and that this fact acquires new complexities in quantum physics, including experimental physics. Although these complexities and their role in Bohr's thinking became more apparent and developed after the discovery of quantum mechanics, Bohr's 1913 paper was a harbinger of these complexities and his later thinking. Some of the "German" long-windedness of Bohr's paper, resisted by Rutherford, might have reflected Bohr's struggle with these complexities and his emerging sense that they might be unavoidable in quantum theory and could not be handled by means of thinking and principles that defined classical physics or even relativity, which already questioned the adequacy of classical thinking and principles. By contrast, although sensing these complexities, Rutherford was not ready to give up on classical thinking in physics, with which Bohr's most radical moves in the paper were in conflict. Rutherford was then, and for the remainder of his life, thinking about these complexities in classical-like terms, just as did Einstein at the time and even (with a new sense of them) after quantum mechanics, which, by contrast, brought Bohr to his ultimate understanding of this "entirely new situation" in physics (Bohr 1935, p. 700).

Such complexities are often bypassed in the disciplinary practice of physics. Quantum theory is no exception because theoretical physicists can productively work on the mathematics of quantum theory and relate this mathematics to experimental data, without engaging with or worrying about these complexities, whatever is their philosophical positions or inclinations. In this regard, they are in effect practitioners of a new type of theoretical physics, introduced by Heisenberg (which I shall discuss in closing this chapter), however suspicious or resistant they may be concerning this approach, to the point of denying that this is what they are doing. However, these complexities become manifest and cannot be avoided if one asks deeper foundational questions, such as those that were in effect at stake in Bohr's paper, especially, again, when such questions are precipitated by a crisis. Understanding the quantum constitution of matter could not, in Bohr's view, bypass philosophical issues, in particular those at stake in quantum theory. The debate concerning it was new then, and in fact was not quite a debate yet, because the epistemological problems of the old quantum theory were expected by most to be solved and to be solved on classical lines. This debate took its modern shape with the creation of quantum mechanics in 1925, and Bohr's confrontation with Einstein, which ensued in its wake.

Bohr's exchange with Rutherford was also a continuation of another old debate, which extends from the pre-Socratics and, in the modern age, from Descartes on. It concerns the role of philosophical thinking in physics, experimental and theoretical. By referring to "the German method, where it appears to be a virtue to be as long-winded as possible," Rutherford also appears to have referred to a more philosophical approach to physics. If so (it is difficult to be entirely certain), Bohr, who had a strong philosophical background and interests, was, even at the time, likely to have had a different assessment of this "method" and of its "long-windedness," and of the pertinence of philosophy in physics. The question is to what degree a more philosophical way of thinking could or should be brought into physics, or conversely "exiled" from it, as Rutherford would perhaps have preferred it to be, the title, *Philosophical Magazine*, and the history of the journal he edited notwithstanding.

Rutherford saw the significance of Bohr's theoretical argument, however troubled he might have been by the most radical ideas of this argument. Rutherford remained cautious as to how definite Bohr's argument was for quite some time, expecting a more classical solution to the problem of atomic constitution, a hope that became even more frustrated with the subsequent developments of quantum theory and became even less likely to be realized by quantum mechanics. It is not clear whether Rutherford ever reconciled himself to the kind of thinking in physics that Bohr and then Heisenberg and others, such as Born, Pauli, Jordan, and Dirac, adopted. As Rutherford's letter makes apparent, he also realized Bohr's desire for and even obsession with clarity and precision, a hallmark of all of Bohr's writings. Rutherford may not have perceived the relevance of Bohr's more philosophical thinking trickling into his paper (at this stage it was no more than that) to his physical argument. For Rutherford, this philosophical thinking was not relevant to or in any event not sufficiently significant for physics—it was more a manifestation of the long-windedness of the German method. For Bohr, philosophical thinking was essential to physics, especially quantum physics, even, again, experimental physics, and his 1913 papers already began to reflect his more philosophical style of thinking. Rutherford did, with considerable caution, accept Bohr's most radical epistemological and, hence, philosophical move: the impossibility of offering an ultimate explanation of *some* physical processes in the atoms. This move was a product of a fusion of physics and philosophy in Bohr's thinking. It inspired Heisenberg, who took this idea to a still more radical limit by extending it to placing *all* physical processes inside atoms beyond the reach of explanation and limiting himself to predicting their outcomes observed in measuring instruments.

Bohr's elaboration reflecting the situation under discussion merits additional attention in the context of Heisenberg's discovery. Bohr says: "While, there obviously can be no question of a mechanical foundation of the calculation given in this paper, it is, however[,] possible to give a very simple interpretation of the result of the calculation on p. 5 [concerning stationary states] by help of *symbols* taken from the mechanics" (Bohr 1913, p. 15; emphasis added). The sentence is best known for its first part: "there obviously can be no question of a mechanical foundation of the calculation given in this paper." The statement, as the preceding discussion here makes clear as well, poses, quite dramatically, the question of causality and the principle of causality, and in effect the principle of realism, as applicable to quantum jumps. Heisenberg echoes this statement in his paper introducing quantum mechanics: "a geometrical interpretation of such quantum-theoretical phase relations in analogy with those of classical theory seems at present scarcely possible" (Heisenberg 1925, p. 265).

Heisenberg's paper is, however, also a response to Bohr's sentence as whole, and Heisenberg's approach is a full-scale (rather than limited, as in Bohr's theory) enactment of the program *implicit* in this sentence, even if Bohr himself might not have fully realized these implications or their scale at the time. We no longer really *read* Bohr's paper (and few have ever done so) by thinking through each sentence of it, and Bohr always invested a major effort in each of his sentences. I have no doubt, however, that Heisenberg had read his paper very carefully, even though his many discussions with Bohr before and during his work on quantum mechanics would have been sufficient for Heisenberg to know Bohr's thinking concerning the subject and to inspire him. As Bohr immediately grasped as well, Heisenberg's own approach to quantum mechanics amounts to taking "*symbols* … from the ordinary mechanics," where they represent classical physical variables (such as position and momentum) and equations connecting these symbols, and giving both a totally different mathematical form and a new physical meaning. In Heisenberg's theory, these symbols became (unbounded) infinite matrices with complex elements (instead of the regular functions of coordinates and time, as in classical physics) and are given proper rules of algebraically manipulating them. It was a combination of these symbols, still those of formally classical equations, and new variables, demanded by the new principles of quantum theory (the QD and QP/QS principles) that defined the architecture of quantum mechanics. Physically, these new variables were linked to the probabilities or statistics of the occurrences of certain observable phenomena, manifested in atomic spectra, instead of describing the motion of quantum objects on the model of classical mechanics. Heisenberg's "new kinematics," as he called it, was nothing else. In this sense, Heisenberg's mechanics was *symbolic* mechanics, as Bohr had often referred to it (or to Schrödinger's wave mechanics), thus echoing his earlier thinking concerning his 1913 atomic theory, and extending it to his interpretation of quantum mechanics, and to his philosophical thinking, which quantum theory made imperative for him.

Bohr offers a helpful elaboration in his 1929 article, which is also echoed in his remark cited in Chap. 1 to the effect that "an artificial word like 'complementarity' which does not belong to our daily concepts serves only … to remind us of the epistemological situation …, which at least in physics is of an entirely novel character" (Bohr 1937, p. 87). He says:

> Moreover, the purpose of such a technical term [complementarity] 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 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, 1987, v. 1, pp. 19–20)

Bohr's "example" is not accidental and has its history beginning with the question "How does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other?" asked by Rutherford. Rutherford and others, in particular Dirac (who even spoke of an electron as having a "free will"), used such expressions without any further explanation, even though they might have been aware of the pitfalls of doing so. By contrast, Bohr's use of "a free choice on the part of nature" and similar locutions must (at least after his 1927 discussion with Einstein at the Solvay Conference in Brussels) be considered with this passage in mind. Heisenberg offers a penetrating comment in response to Dirac's appeal to "choice on the part of nature," which Heisenberg questioned on experimental grounds, in the course of a discussion that took place at the same conference. Dirac's comment in effect implied the causal nature of independent quantum behavior undisturbed by observation in accord with his transformation-theory paper (Dirac 1927a) and Bohr's Como argument, which, as will be seen in Chap. 3, was influenced by that paper. By contrast, while referring to his uncertainty-relations paper (Heisenberg 1927), which used the *mathematics* of Dirac's transformation theory, Heisenberg expressed a view that was closer to Bohr's post-Como thinking, which no longer assumes that independent (undisturbed) quantum processes are causal. This view guides Bohr's subsequent analyses of the double-slit experiment, which figured in the Bohr-Einstein exchange and in the general discussion at the 1927 Solvay Conference. Heisenberg says (according to the available transcript):

> I do not agree with Dirac when says that in the [scattering] experiment described nature makes a choice. Even if you place yourself very far from your scattering material and if you measure after a very long time, you can obtain interference by taking two mirrors. If nature had made a choice, it would be difficult to imagine how the interferences are produced. Obviously we say that nature's choice can never be known until the decisive experiment has been done; for this reason we cannot make any real objection to this choice because the expression "nature makes a choice" does not have any physical consequence. I would rather say, as I have done in my latest paper [on the uncertainty relations], that the *observer himself* makes the choice because it is not until the moment when the observation is made that the "choice" becomes a physical reality. (Bohr 1972–1999, v. 6, pp. 105–106)

The technical details of the experiment are not important at the moment, apart from noting the significance of scattering experiments in the development of quantum mechanics; the experiment itself is essentially equivalent to the double-slit experiment. The crucial point is that one in effect deals with the complementary character of certain quantum experiments and with our choice of which of the two mutually exclusive or complementary experiments we want to perform, rather than with a choice of nature. Without realizing it, Heisenberg describes the so-called delayed choice experiment of Wheeler (1983, pp. 190–192; Plotnitsky 2009, pp. 65–69). Moreover, Heisenberg suggests that, in considering quantum phenomena, only what has already occurred as the outcome of a measurement could be assigned the status of reality at the level of observation, which view came to define Bohr's concept of phenomenon, to be discussed in Chap. 3. By contrast, the reality of quantum objects is beyond observation or representation.

Bohr's comment cited above was written in 1929, following the Solvay Conference and further exchanges, and it refers specifically to his article, "The

Quantum of Action and the Description of Nature" (Bohr 1929a). At stake here is not merely stressing the metaphorical or "picturesque," rather than physical, use of expressions like "a choice on the part of nature," but, as Heisenberg's remark makes clear, also a deeper epistemological point. These deeper epistemological considerations eventually made Bohr either avoid speaking in these terms or to qualify their metaphorical or picturesque use. The situation implies, as Bohr will say in his reply to EPR, "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," and ultimately a radical renunciation of the classical ideal of reality as well (Bohr 1935, p. 697). The difference, reflected in Bohr's respective phrasings, is that, while causality is renounced, the existence, *reality*, of quantum objects is assumed, without our being able to conceive of this existence, even by use of such words as to "be" or to "know," which makes this reality a reality without realism, in accordance with the RWR principle. These words, invoked by Bohr in his 1929 elaboration cited above, are not accidental either. They are common in our everyday language but, as Bohr clearly implies, they are complex philosophically, referring to the philosophical problems of ontology and epistemology, which have been the subject of profound philosophical discussions from the pre-Socratics to Heidegger and beyond, and they acquire radically new dimensions with quantum theory. Bohr is known to have replied, after the rise of quantum physics but before quantum mechanics, 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 and Ford 1998, p. 131). Both of these questions are still unanswered and, in Bohr's ultimate view, based in the RWR principle, are unanswerable, or even unaskable. This principle allows quantum objects to exist, to be real, but it does not allow for realism that would enable us to represent their nature and behavior. One cannot say anything about them apart from the effects of their interactions with the classically observed macro world (it is ultimately quantum as well), where such questions as "Where can something be said to be?" can be meaningfully asked.

Under this assumption, what is "sometimes picturesquely described as a 'choice of nature' [between different possible outcomes of a quantum experiment]" will be given by Bohr a different meaning as well. As he says in 1954, "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 1954b, 1987, v. 2, p. 73). In other words, it points to the impossibility of a causal or any representation, if not even conception (on which Bohr's view is, again, unclear) of quantum processes. This more radical understanding, was yet a few years away even in 1929, by which time, however, Bohr managed to overcome the problem of causality that plagued the Como argument of 1927. Bohr, by this point, was influenced by both Schrödinger's wave mechanics and, more significantly, by Dirac's 1927 transformation-theory paper, which, while a major contribution otherwise, also argues for the causal nature of independent quantum behavior undisturbed by observation (Dirac 1927a). Bohr would be better off staying with Heisenberg's initial argument introducing quantum mechanics and his own initial understanding of the quantum-mechanical situation based on Heisenberg's and Born and Jordan's papers (Heisenberg 1925; Born and Jordan 1925), which developed Heisenberg's argument into a full-fledged matrix mechanics. Although the road ahead was to be long and difficult, Bohr's 1913 atomic theory set the trajectory toward Heisenberg's discovery of quantum mechanics, to which I shall now turn.

2.3 From Bohr to Heisenberg, and from Heisenberg to Bohr: The Founding Principles of Quantum Mechanics

Both Heisenberg's initial approach to quantum mechanics in 1925 and Bohr's initial interpretation of the theory, offered in 1927, were guided by the following four main principles: (1) the QD principle, (2) the QP/QS principles, (3) the correspondence principle, and in Bohr's case, (4) the principle of complementarity, added to the first three principles. I begin by defining the first three principles, central for Heisenberg's work:

1. the QD principle, or the principle of quantum discreteness, states that all quantum phenomena, defined as what is observed in measuring instruments, are individual and discrete, which is not the same as the (Democretian) atomic discreteness of quantum objects themselves;
2. the QP/QS principle, or the principle of the probabilistic or statistical nature of quantum predictions, states that all quantum predictions are of this nature, even in the case of elementary individual quantum processes and events, such as those associated with elementary particles; and
3. the correspondence principle, as initially used by Bohr and others, stated that the predictions of quantum theory must coincide with those of classical mechanics in the classical limit, but was given by Heisenberg a mathematical form, which required that both the equation and variables used convert into those of classical mechanics in the classical limit.[4]

In Heisenberg's hands, each of these principles also gave rise to a mathematically expressed postulate. This is crucial if one wants, on the basis of a given set of principles, to establish a mathematical model (that of quantum mechanics, quantum field theory, or quantum finite-dimensional theory, considered in quantum information theory) predicting the outcome of quantum experiments, which is all one needs in this case by the QP/QS principle.

The QD principle originated in Bohr's 1913 theory of the hydrogen atom, discussed in Sect. 2.1, as based on "quantum postulates," pertaining to the discrete behavior ("quantum jumps") of electrons in atoms. According to Heisenberg in his paper introducing quantum mechanics: "In order to characterize this radiation we

[4] Bohr's ad hoc but ingenious use of the correspondence principle in the old quantum theory is less germane to my argument in this study and will be put aside.

first need the frequencies which appear as functions of two variables. In quantum theory these functions are in the form:

$$\nu(n, n-\alpha) = 1/h\{W(n) - W(n-\alpha)\}\text{''} \text{ (Heisenberg 1925, p. 263)}$$

Bohr's quantum postulates should not, again, be confused with what Bohr calls "the quantum postulate" of the Como lecture, which was developed in the wake of quantum mechanics and, as will be discussed in Chap. 3, expressly defined quantum discreteness as that of quantum phenomena observed in measuring instruments (Bohr 1927, 1987, v. 1, pp. 52–53). The above formula remains valid and mathematically expresses Bohr's 1927 quantum postulate, which is nearly inherent in Heisenberg's scheme, given that this scheme and this formula refer only to what is observed in measuring instruments. This concept of quantum discreteness was eventually recast by Bohr in terms of his concept of phenomenon, introduced in the 1930s, in the wake of his exchanges with Einstein concerning the EPR-type experiment, and central to Bohr's ultimate, RWR-principle-based, interpretation (Bohr 1938, 1949, 1987, v. 2, p. 64).

The postulate that mathematically expressed the QP/QS principle was the formula for the probability amplitudes cum Born's rule, which is a postulate as well, as reflected in related conceptions, such as von Neumann's projection postulate or Lüder's postulate. Heisenberg only formulated this postulate in the particular case of quantum jumps and the hydrogen spectra, rather than, as Born did, as universally applicable in quantum mechanics and indeed as one of its primary postulates. Born's rule is not inherent in the formalism but is added to it: it is *postulated*.

The correspondence principle played an essential role in the development of matrix mechanics by Heisenberg, who gave it a rigorous mathematical form, made it the mathematical correspondence principle. In this form, the principle required that both the equations of quantum mechanics (which were formally those of classical mechanics) and variables used (which were different) convert into those of classical mechanics at the classical limit. The processes themselves, however, are still assumed to be quantum. Heisenberg reiterated the significance of the correspondence principle in his joint paper with Born and Jordan in the passage in the paper, apparently written by him, after some discussion with Born and Jordan, who, Heisenberg thought, did not sufficiently realize the role of the principle in matrix mechanics (Born et al. 1926, p. 322; Plotnitsky 2009, pp. 105–107). On the other hand, Heisenberg's mathematical correspondence principle was different from Bohr's correspondence principle, for one thing, because Heisenberg's redefinition also related the principle to a mathematically expressed postulate.

As will be seen, the principle acquires its own form in quantum electrodynamics and quantum field theory, insofar as the equations of the theory must convert into those of quantum mechanics in the corresponding limit. Thus, the quantum-mechanical limit of Dirac's equation is Schrödinger's equation, technically, a far limit, after neglecting spin, via Pauli's spin theory (which used multi-component wave functions), which is the immediate quantum-mechanical limit of Dirac's theory. The mathematical correspondence principle motivated Heisenberg's decision to retain the equations of classical mechanics, while using different variables, thus in contrast to the approach of the old quantum theory. The equations were formally the same, and the trick of conversion was that new quantum variables could be substituted for any classical variables at the classical limit. (In Dirac's case both the equation and the variables were different and the conversion at the quantum-mechanical limit concerned both, and was nontrivial at the time.) The old quantum theory was defined by the reverse strategy of retaining the variables of classical mechanics but adjusting the equations of classical mechanics to make better predictions of the quantum data. (The correspondence principle, too, was used, ad hoc, as part of this strategy.) The approach was not without its successes, even major successes, beginning with Bohr's 1913 atomic theory, but ultimately failed, even in describing stationary states, conceived as "orbits," rather than as energy levels as in quantum mechanics.

Bohr's interpretation, first proposed in 1927, added a new principle, the complementarity principle. The principle stemmed from the concept of complementarity, defined by: (a) a mutual exclusivity of certain phenomena, entities, or conceptions; and yet (b) the possibility of considering each one of them separately at any given moment of time and (c) the necessity of using all of them at different moments for a comprehensive account of the totality of phenomena that one must consider in quantum physics.[5]

The QD and QP/QS principle are correlative, although this was understood only in retrospect, following the development of Bohr's interpretation, eventually leading him to the RWR principle. The RWR principle could be inferred from the complementarity principle, because the latter prevents us from ascertaining the complete composition of the "whole" from "parts," to the degree these concepts apply. Such complementary parts never add to a whole in the way they do in classical physics or relativity, given that at any moment of time only one of these parts could be ascertained, and hence is the only "whole" at this moment of time. Complementarity is not immediately or self-evidently related to a mathematically expressed postulate or a set of such postulates, and Bohr has never expressly done so. However, the noncommutativity of the multiplication of the corresponding variables, as the position and the momentum operators, in the formalism of quantum mechanics could be seen as the mathematical expression of the complementarity principle. I shall consider this subject in more detail in Chap. 3.

The uncertainty relations, which are correlative to the complementary relationship between the variables involved in them, are sometimes seen as manifesting a principle as well, and Bohr and Heisenberg do on occasion speak of the uncertainty principle (e.g., Bohr 1935, p. 697; Heisenberg 1989, p. 29). The uncertainty rela-

[5] The concept and principle of complementarity, as formulated here, are closer to the way they are presented in Bohr's later works, from 1929 on, impacted by his debate with Einstein. In these works, the concept is exemplified by position and the momentum measurements. Such measurements are always mutually exclusive, and as such correlative to the uncertainty relations, but both possible to be performed on a given quantum object at different points of time and both necessary for a complete (Bohr-complete) account of the behavior of quantum objects, in Bohr's ultimate, RWR-principle-based, interpretation, in terms of effects quantum objects can have on measuring instruments.

tions may, however, be better seen as expressing a postulate (or a set of postulates) and a law of nature, a view that was central to Bohr, especially in his exchanges with Einstein.

While Heisenberg did not expressly refer to either the QD or the QP/QS principle, both were at work in his derivation of quantum mechanics, via the postulates described above. As indicated in Chap. 1, these principles could be considered as primary to the RWR principle or, arguably, to any other quantum-theoretical principle. The RWR principle is an interpretive inference from these principles, which would, again, apply even in the case of causal theories of quantum phenomena, such as Bohmian mechanics. The latter, a constructive theory (in all of its versions and in all of its current interpretations), does not conform to the RWR principle and entails both realism and causality at the quantum level. This is the case even though it retains, correlatively, both the uncertainty relations and the probabilistic or statistical character of quantum predictions, which strictly coincide with those of standard quantum mechanics. As explained in Chap. 1, in classical physics, when the recourse to probability or statistics is involved, we proceed from causality to probability because of our inability to track this causality. This, while leaving room for probability, gives primacy to causality, which makes probability reducible, at least ideally and in principle, specifically in considering elementary individual classical processes or events. In quantum mechanics, beginning with Heisenberg's discovery of it, probability, thus divorced from causality, becomes a primary concept and the QP/QS principle a primary principle, as is, correlatively, the QD principle, while the RWR principle is interpretively inferred from them. As explained in Chap. 1, the absence of classical causality is an automatic consequence of the RWR principle. On the other hand, quantum mechanics, while not a relativistic theory, is consistent with relativistic causality (essentially locality) or other concepts of causality, which, as will be discussed in detail in Chap. 5, are probabilistic or statistical, as they must be, if the QD/QS principle is assumed.

I shall now consider how the principles and postulates just described worked and were given a mathematical expression in Heisenberg's discovery of matrix quantum mechanics, as presented in his original paper (Heisenberg 1925). This mathematical expression was only partially worked out and sometimes more intuited than properly developed, which took place a bit later in the work of Born, Jordan, and Heisenberg, and in some of its aspects even later with von Neumann's recasting of quantum mechanics into his rigorous form of Hilbert-space formalism. Nevertheless, Heisenberg's creativity and inventiveness were remarkable.[6] Bohr's initial comment on Heisenberg's discovery, in 1925 (before Schrödinger's version was introduced), shows a clear grasp of what was at stake: "In contrast to ordinary mechanics, the

[6] This does not of course mean that Heisenberg's invention of quantum mechanics was independent of or was not helped by preceding contributions, even beyond the key pertinent works in the old quantum theory by Einstein, Bohr, Sommerfeld, and others, discussed in Sect. 2.1. H. Kramers's work on dispersion and his collaboration with Heisenberg on the subject were especially important for Heisenberg's work (Kramers 1924; Kramers and Heisenberg 1925). See (Mehra and Rechenberg 2001, v. 2) for an account of this history.

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 1925, 1987, v. 1, p. 48).[7] Later, in the Como lecture, Bohr used even stronger, almost "political" terms, of "emancipat[ion]" from "the classical concept of motion:" "The new development was commenced in a fundamental paper by Heisenberg, where he succeeded in *emancipating himself completely* from the classical concept of motion by replacing from the very start the ordinary kinematical and mechanical quantities by symbols which refer directly to the individual processes demanded by the quantum postulate" (Bohr 1927, 1987, v. 1, pp. 70–71). By this Bohr, again, means that these symbols refer to the outcomes of individual quantum processes, in terms of the probabilities of transitions between stationary states.

This approach may be considered in quantum-informational terms. The experimental situation is defined by (a) certain *already obtained* information, concerning the energy of an electron, derived from spectral lines (associated with a hydrogen atom) observed in measuring instruments; and (b) certain possible future information, concerning the energy of this electron, *to be obtainable* from spectral lines, predictable, in probabilistic or statistical terms, again, associated with events to be observed in measuring instruments. Heisenberg's strategy was to abandon the task of developing a mathematical scheme representing how these data or information are connected by a spatio-temporal process, and derive his predictions from this scheme. He knew from the old quantum theory that this would be difficult and perhaps impossible to do, especially given that even an elementary individual event, such an emission of a single photon could not be predicted exactly, even ideally. Instead he decided to try to find a mathematical scheme that would just enable these predictions, using the principles stated above (except for complementarity, which came later). Unlike in Bohr's atomic theory, in Heisenberg's scheme, the stationary states were no longer represented in terms of orbital motion, but only in terms of energy values, which would change discontinuously and acausally, changes statistically predicted by Heisenberg's scheme. Part of the formal mathematical architecture of the scheme was provided by the equations of classical mechanics by virtue of the mathematical correspondence principle. Classical variables, however, would

[7] Bohr was not unprepared for this eventuality, as is clear from his letter to Heisenberg (Letter to Heisenberg, April 18, 1925, Bohr 1972–1996, vol. 5, pp. 79–80). The letter was written in the wake of the collapse of the so-called Bohr–Kramers–Slater (BKS) proposal, which, among other things, implied that the energy conservation law only applied statistically (Bohr et al. 1924), and shortly before Heisenberg's discovery of quantum mechanics. Bohr's article was in preparation as a survey of the state of atomic theory before Heisenberg's discovery of quantum mechanics, but it was modified in view of this discovery and Born and Jordan's work on casting Heisenberg's mechanics into its proper matrix form. Bohr added a section from which I cite here. Bohr's views expressed in this section are crucial, and I shall return to his argument there in closing this chapter.

not give correct predictions. So, they had to be replaced by new variables, which were complex-valued variables of a type never used in physics previously.

Two qualifications are in order. First, I am not saying that Heisenberg's matrix mechanics was, or that quantum mechanics or quantum field theory *is*, (only) quantum information theory. Heisenberg was concerned with how fundamental quantum objects and processes work, even though these workings defy being represented, which fact is an effect of these workings. The information at stake was still about them, rather than part of information processing or communication by using quantum technology, experimental and mathematical. But then, as will be seen in Chap. 7, quantum information theory, too, may serve the purposes of fundamental physics, rather than only aim at theorizing quantum information processing between devices. My point is that, in Heisenberg's approach, quantum mechanics contains a constitutive quantum-informational dimension.

My second qualification follows Heisenberg's own, offered in his 1930 *The Physical Principles of the Quantum Theory*: "It should be distinctly understood, however, that this [the deduction of the fundamental equation of quantum mechanics] cannot be a deduction in the mathematical sense of the word, since the equations to be obtained form themselves the *postulates* of the theory. Although made highly plausible by the following considerations [essentially the same that led him to his discovery of quantum mechanics], their ultimate justification lies in the agreement of their predictions with the experiment" (Heisenberg 1930, p. 108). This is an important point, especially in the context of projects deriving quantum theory (of whatever kind: quantum mechanics, quantum field theory, or finite dimensional quantum theory) from fundamental principles. It opens the question as to which postulates are considered "natural" or "reasonable," or what constitutes a proper derivation. One might argue that it is not sufficiently first-principle-like to see the equation of quantum mechanics as postulates and, in general, prefer a less mixed derivation of quantum theory than that of quantum mechanics by Heisenberg or Schrödinger. So far, most attempts at such derivations, such as those, along the lines of quantum information theory, discussed in Chap. 7, have concerned finite-dimensional quantum theory, aiming, however, at extending the principles involved beyond these limits, indeed all the way to quantum gravity.

In reflecting on Heisenberg's conception of these quantities, one might observe first that, in order to invent a new concept of any kind, one has to construct a phenomenological entity or a set of entities and relations among them. In physics, one must also give this construction a mathematical architecture, defining the corresponding mathematical model, with which one might indeed start, as Heisenberg in effect did, and which enables the theory to *relate* to observable phenomena and measurable quantities associated with them. Heisenberg's approach, as that of Dirac later on (influenced by Heisenberg), may even be best seen as defined by an attempt to find, first, *under the guidance of the physical principles assumed*, an independent (abstract) mathematical scheme that would then be related to the data obtained in the quantum experiments in question. As noted in Chap. 1, influenced by Einstein, Heisenberg, later on, even spoke of his thinking as defined by the question: “Why not simply say that only those things occur in nature which fit our mathematical scheme!” (Heisenberg 1963, Interview with T. Kuhn, 5 July 1963, *Archive for the History of Quantum Physics*; Heisenberg 1962, pp. 45–46). Although one might, again, be cautious, as Heisenberg was himself, concerning such a retrospective statement, this primacy of the mathematical scheme or model in his or Dirac’s thinking is central. Two points are of the main interest here. The first is that this scheme emerges from a combination of the formal architecture of classical equations this scheme inherits, on the one hand, and new variables that Heisenberg invents, on the other; the second is that these variables are a consequence Heisenberg’s mathematization (cum an equivalent of Born’s rule) of the QP/QS principle.

This approach, defined by giving primacy to creating first a mathematical scheme or model is general, rather than specifically quantum-mechanical, in character, and is close to that of Einstein’s mathematical thinking, guided by the equivalence principle, in the case of general relativity, in partial contrast to special relativity, where mathematics was less dominant. (As noted in Chap. 1, this primacy of first developing a suitable mathematical scheme becomes even stronger in Einstein’s subsequent work on the unified field theory.) However, as against Einstein’s realist requirement, a mathematical model need not represent, in the way it does or at least may be assumed to do in classical physics or relativity, observable physical phenomena or objects responsible for these phenomena. The mathematical models of quantum mechanics or quantum field theory only relate to the observed quantum phenomena probabilistically or statistically, without, at least in nonrealist interpretations, providing a representation of quantum objects and processes. This nevertheless allows the theory to remain a mathematical-experimental science of nature, in this respect in accord with the project of modern physics from Galileo on. However, the relationships between the mathematics of the model and experiment are fundamentally different. Indeed, as just noted, this mathematical architecture arises from this new, QP/QS-principle-defined, way of relating one’s model to the observed phenomena.

Heisenberg’s invention of his matrices was made possible by his idea of arranging algebraic elements corresponding to numerical quantities (transition probabilities) into infinite square tables. It is true that, once one deals with *transitions between two stationary states*, rather than with *a representation of such states*, matrices appear naturally, with rows and columns linked to each possible state respectively. This naturalness, however, became apparent or, one might say, *became natural*, only in retrospect. This arrangement was a phenomenological construction, which amounted to that of a mathematical object, a matrix, an element of general noncommutative mathematical structure, part of (infinite-dimensional) linear algebra, in a Hilbert space over complex numbers, in which Heisenberg’s matrices (“observables”) form an operator algebra. One can also see this scheme as a representation of an abstract algebra, keeping in mind that Heisenberg’s infinite matrices were unbounded, which fact, as became apparent shortly thereafter, is necessary to have the uncertainty relations for the corresponding continuous variables. There are further technical details: for example, as unbounded self-adjoint operators, defined on infinite dimensional Hilbert spaces, these matrices do not form an algebra with respect to the composition as a noncommutative product, although some of them satisfy the canonical commutation relation. These details are, however, secondary at the moment.

Heisenberg begins his derivation with an observation along the lines of a proto-RWR principle: "[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, p. 263; emphasis added). My emphasis reflects the fact that, in principle, a measurement could associate an electron with a point in space, but not as a function of time representing its motion, as in classical mechanics.[8] If one adopts a strictly RWR-principle-based interpretation, one cannot assign any properties to quantum objects themselves (not even single such properties, such as a position, rather than only certain joint ones, which is precluded by the uncertainty relations) but only to the measuring instruments involved. This, again, amounts to establishing a mathematical scheme that enables the processing of information (which is, qua information, classical) between measuring devices. Heisenberg describes his next task as follows: "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:

$$v(n, n-\alpha) = 1/h\{W(n) - W(n-\alpha)\} \tag{2.1}$$

and in classical theory in the form

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

This difference, which reflects the QD principle, leads to a difference between classical and quantum theories as concerns the combination relations for frequencies, which correspond to the Rydberg-Ritz combination rules. However, "in order to complete the description of radiation [in correspondence, by the mathematical correspondence principle, with the classical Fourier representation] it is necessary to have not only the frequencies but also the amplitudes" (Heisenberg 1925, p. 263). On the one hand, then, by the correspondence principle, the new, quantum-mechanical equations must formally contain amplitudes. On the other hand, these amplitudes could no longer serve their classical physical function (as part of a continuous representation of motion) and are instead related to the discrete transitions between stationary states. In Heisenberg's theory and in quantum mechanics since then, these "amplitudes" become no longer amplitudes of physical motions, which makes the name "amplitude" itself an artificial, *symbolic* term. They are instead linked to the probabilities of transitions between stationary states: they are essentially what we

[8] As noted earlier (note 3), matrix mechanics did not offer a treatment of stationary states, in which and only in which one could in principle speak of a position of an electron in an atom.

now call probability amplitudes.[9] The corresponding probabilities are derived, from Heisenberg's matrices, by a form of Born's rule for this limited case (Born's rule is more general). One takes square moduli of the eigenvalues of these matrices (or equivalently, multiply these eigenvalues by their complex conjugates), which gives one real numbers, corresponding, once suitably normalized, to the probabilities of observed events. (Technically, one also needs the probability density functions, but this does not affect the essential point in question.) The standard rule for adding the probabilities of alternative outcomes is changed to adding the corresponding amplitudes and deriving the final probability by squaring the modulus of the sum.[10] This quantum-theoretical reconceptualization of "amplitude" is an extension of the conceptual shift from finding the probability of finding an electron in a given state to the probability of the electron's discrete transitions ("quantum jumps") from one state to another, found already in Bohr's theory and manifested in Bohr's frequency rule, as discussed in Sect. 2.1. This is not surprising: Bohr's frequency rule, which embodies the QD principle, is Heisenberg's starting point. The mathematical structure thus emerging is in effect that of vectors and (in general, noncommuting) Hermitian operators in complex Hilbert spaces, which are infinite-dimensional in the case of continuous variables. Heisenberg explains the situation in these, more rigorous, terms in his 1930 book (Heisenberg 1930, pp. 111–122). In his original paper, he argues as follows:

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[9] It is true that quantum data may present itself in terms of interferometry, which is seen in the graphical representation of counting rates (proportional to the probabilities in question) that are typically oscillatory. In referring to this data, one could speak more intuitively, albeit still metaphorically, of "amplitudes" of these oscillations, just as one speaks of "interference" in referring to the (discrete) interference pattern observed in the double-slit experiment in the corresponding set-up (with both slits opens and no devices installed allowing one to establish through which slit each quantum object passes). I am indebted to G. Jaeger for pointing out this aspect of the quantum-mechanical situation. However, these amplitudes (which are related to real measurable quantities) are not the same as the "symbolic" amplitudes in question. The latter amplitudes are complex quantities enabling us to predict the probabilities relating to the oscillations in question. This is why these amplitudes are seen as "symbolic" by Bohr and Heisenberg, that is, as symbols borrowed from classical physics without having the physical meaning they have there. To cite Bohr: "The symbolic character … of the artifices [of the quantum-mechanical formalism] also becomes apparent in that an exhaustive description of the electromagnetic wave fields leave no room for light quanta and in that, in using the conception of matter waves, there is never any question of a complete description similar to that of the classical theories. Indeed, … the absolute value of the so-called phase of the waves never comes into consideration when interpreting the experimental results. In this connection, it should also be emphasized that 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 [as concerns what is observed]" (Bohr 1929b, 1987, v. 1, p. 17).

[10] For these reasons, quantum probabilities are sometimes referred to as non-additive. For a classic account of quantum probability amplitudes, see (Feynman et al. 1977, v. 3, pp. 1–11). Feynman has an excellent earlier article on the subject (Feynman 1951). See also (Gillies 2000; Hájek 2014; Khrennikov 2009).

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:

$$\begin{gathered} \textit{Quantum theoretical:} \\ \mathrm{Re}\left\{A(n,n-\alpha)e^{i\omega(n,n-\alpha)t}\right\} \\ \textit{Classical:} \\ \mathrm{Re}\left\{A_\alpha(n)e^{i\omega(n)\alpha t}\right\}. \end{gathered}$$

(Heisenberg 1925, pp. 263–264)

The problem—a difficult and, "at first sight," even insurmountable problem—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, p. 264). As we have seen, this incommensurablity, which is in an irreconcilable conflict with classical electrodynamics, was one of the most radical features of Bohr's 1913 atomic theory, on which Heisenberg builds here. This strategy is, again, linked to the shift from calculating the probability of finding an electron in a given state to calculating the probability of an electron's transition from one state to another.

Just as Bohr did in inventing his atomic theory, Heisenberg proceeds to inventing a new theory around this problem, in effect, by making it into a solution, as if saying: "This is not a problem, the classical way of thinking is." His new theory offers the possibility of predicting, in general probabilistically, the outcomes of quantum experiments, at the cost of abandoning the physical description or representation, however idealized, of the ultimate objects and processes considered. This cost was unacceptable to some, even to most, beginning with Einstein, but it was a new principle for Heisenberg and Bohr, ultimately leading to the RWR principle. Heisenberg says: "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, p. 264; emphasis added). "Analogous" could only mean here that, rather than being analogous physically, the way the phase enters mathematically is analogous to the way the classical phase enters mathematically in classical theory, in accordance with the *mathematical* form of the correspondence principle, insofar as quantum-mechanical equations are formally the same as those of classical physics. Heisenberg only considered a toy model of an anharmonic quantum oscillator, and thus needed only a Newtonian equation for it, rather than Hamiltonian equations required for a full-fledged theory, developed by Born and Jordan (Born and Jordan 1925; Born et al. 1926).

In this way, Heisenberg gave the correspondence principle a mathematical expression, or, again, changed it into the mathematical correspondence principle. The variables to which these equations apply could not, however, be the same, because, if they were, the equations would not make correct predictions for low quantum numbers.

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

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

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

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

or

$$x(n,t)=\int_{-\infty}^{+\infty} A_\alpha(n)e^{i\omega(n)\alpha t}d\alpha$$ " (Heisenberg 1925, p. 264).

Heisenberg next makes his most decisive and most extraordinary move. He 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, p. 264). "However," he says, "one might readily regard the ensemble of quantities $A(n,n-\alpha)e^{i\omega(n,n-\alpha)t}$ [an infinite square matrix] as a representation of the quantity $x(t)$" (Heisenberg 1925, p. 264).

The arrangement of the data into square tables is a brilliant and, as I said, in retrospect, but, again, only in retrospect, natural way to connect the relationships (transitions) between two stationary states. 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* (which, again, abandons motion and other spatio-temporal concepts of classical physics at the quantum level) of the classical equation of motion that he considered, as applied to these new variables, Heisenberg needs to be able to construct the powers of such quantities, beginning with $x(t)^2$, which is all that he needs for his equation.[11] The answer in classical theory is 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 … by

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

[11] A quantum-theoretical interpretation refers here to the change of classical variables to quantum variables, rather than a physical interpretation of the resulting mathematical model (matrix mechanics), although this change implies certain physical features, specifically a predictive rather than representational nature of the model.

or
$$= \int_{-\infty}^{+\infty} A(n, n-\alpha) A(n-\alpha, n-\beta) e^{i\omega(n, n-\beta)t} d\alpha$$ " (Heisenberg 1925, 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" (Heisenberg 1925, p. 265). This combination of the particular arrangement of the data and the construction of an algebra of multiplying his new variables is Heisenberg's great invention. (As I noted, although some of them satisfy the canonical commutation relation, technically, these matrices do not form an algebra with respect to the composition as a noncommutative product.) The "naturalness" of this assumption should not hide the radical and innovative nature of this assumption or indeed discovery, one of the greatest in twentieth-century physics.

Although it is commutative in the case of squaring a given variable, x^2, this multiplication is in general noncommutative, expressly for position and momentum variables, and Heisenberg, without quite realizing it, used this noncommutativity in solving his equation, as Dirac was the first to notice. Taking his inspiration from Einstein's "new kinematics" of special relativity, Heisenberg spoke of his new algebra of matrices as the "new kinematics." As noted in Chap. 1, this was not the best choice of term because his new variables no longer described or were even related to motion as the term kinematic would suggest, one of many, historically understandable, but potentially confusing terms. Planck's constant, h, which is a dimensional, dynamic entity, has played no role thus far. Technically, the theory, as Einstein never stopped reminding us, wasn't even a mechanics, insofar as it did not offer a representation of individual quantum processes, or for that matter of anything else. "Observables," for the corresponding operators, and "states," for Hilbert-space vectors, are other such terms: we never observe these "observables" or "states," but only use them to predict, probabilistically, what will be observed in measuring instruments. To make these predictions, one will need Planck's constant, h, which thus enters as part of this new relation between the data in question and the mathematics of the theory.

Heisenberg's overall scheme essentially amounts to the Hilbert-space formalism (with Heisenberg's matrices as operators), introduced by von Neumann shortly thereafter, thus giving firmer and more rigorous mathematical foundations to Heisenberg's scheme, by then developed more properly by Heisenberg himself, Born and Jordan, and, differently (in terms of q-numbers), by Dirac. My main point here is that Heisenberg's matrices were (re)invented by him from the physical principles coupled to a mathematical construction leading to an actual algebra, which Heisenberg had to define, beginning with the noncommutative multiplication rule. Dirac, who followed Heisenberg's principle way of thinking in his work on both quantum mechanics and quantum electrodynamics, was also the first to fully realize that noncommutativity was the most essential feature of Heisenberg's scheme. Remarkably, Heisenberg himself, as well as Pauli, far from seeing it as essential, thought that ultimately the theory should be freed from it, and Pauli initially thought that the theory should not be probabilistic either. He changed his mind on both counts only after Schrödinger's equation was introduced (Plotnitsky 2009, pp. 89–90).

The physical principle behind quantum noncommutativity is a more complex matter. In fact, as indicated above, it is Bohr's complementarity principle, or in any event the complementarity principle appears to be the best candidate. Conversely, quantum noncommutativity can be seen as the mathematical expression of the complementarity principle, even though noncommutativity was discovered first, in part as a response to the QP/QS principle. As noted above, the QP/QS principle itself was given its mathematical expression, via the complex Hilbert-space structure cum conjugation, inherent in this structure, and Born's rule. This expression is in turn coupled to complementarity, a coupling manifested in the uncertainty relations. Finally, insofar as the complementarity principle implies it, the RWR principle, too, is mathematically expressed in noncommutativity, if one interprets the situation accordingly, along nonrealist lines.

The nature of the mathematics used, that of the infinite-dimensional Hilbert spaces over complex numbers, already makes it difficult to establish realist representations of physical processes, given that the representation at each point should correspond to what could be observed if a measurement were performed, and all such measurements would have to be real (technically, rational) numbers. Bohr noted this point on several occasions, also, as will be seen, in connection with Schrödinger's formalism, for which Schrödinger initially had realist inspirations, only temporarily relinquished, under the pressure of such problems (Bohr 1987, v. 1, pp. 76–77). In the case of quantum-mechanical formalism, the relationship between a given variable, which is a complex mathematical variable, and measurement is defined by the probability of the outcome of this measurement, always a *future* measurement, the probability predicted by means of this formalism, on the basis the data obtained in some previously performed measurement. The probability is a real number, given by Born's rule. In some interpretations, such as the statistical Copengagen interpretation proposed in Chap. 4, the quantum-mechanical formalism is seen as predicting the statistics of possible outcomes, in general different, of many repeated experiments, which are identically prepared (as concerns the physical states of the measuring instruments involved).

In Heisenberg's original approach, these relationships between variables and measurement appear as a result of a protracted and complicated effort, helped by his ingenious use of the mathematical correspondence principle. This part of Heisenberg's paper, the part that also deals with dynamics, is somewhat cumbersome, and it would be difficult to follow it properly without giving it more space than my limits allow. The essential points could, however, be explained more easily by using Heisenberg's Chicago Lectures of 1929, *The Physical Principles of the Quantum Theory* (Heisenberg 1930), which refined his argumentation by taking advantage of the fully developed quantum formalism in the work, in addition to Heisenberg himself, of Born and Jordan, Schrödinger, and Dirac. These points, however, are essentially in place in Heisenberg's original paper as well.

I shall consider, first, the role of complex numbers, an essential and thus far unavoidable feature of quantum-mechanical formalism in all currently available versions. The significance of this role is apparent but is not commented on in Heisenberg's original paper. In his Chicago lectures, Heisenberg observes, as he did in his original paper, that "a representation [of quantum variables infinite unbounded complex-valued matrix variables] is … meaningless both mathematically and physically until properties and rules of operations for the matrices have been defined. The correspondence principle must be our guide here" (Heisenberg 1930, p. 110). In other words, whatever is defined for the quantum-theoretical form of the equations (with quantum matrix variables) must be in correspondence with, convert into, classical equations with classical variables (functions of real variables) at the classical limit, say, again, in the region of high quantum numbers. We keep in mind that the *behavior* of electrons in this region is still quantum. Heisenberg then says:

> In the first place, the classical expression for the coordinate [in terms of Fourier series] must have a real value; since the terms are complex in [the classical Fourier representation], this can be the case only if for each term there occurs [its] conjugate imaginary. This will also be true of the elements of the matrix [q_k, representing the coordinates] if we assume
>
> $$q_k(mn) = q_k^*(nm),$$
>
> since [by the Rydberg-Ritz combination frequencies rule] $\nu(mn) = -\nu(nm)$. The asterisk denoted the conjugate imaginary. Matrices with this type of symmetry are called Hermitian and in the quantum theory all co-ordinate matrices are assumed to be of this kind. (Heisenberg 1930, pp. 110–111)

In other words, "coordinates" are now Hermitian operators in a Hilbert space. The same correspondence argument, leading to the Hermitian operator representation, applies to the time derivative of any coordinate, and then to the momenta, thus allowing one to establish the rules for addition and multiplication of such variables, which is the standard rule of matrix algebra, in which multiplication is in general noncommutative. The fact that it is noncommutative specifically in the case of position and momentum variables is, again, "due to the fact that quantum frequencies obey the Rydberg-Ritz combination principle" (Heisenberg 1930, p. 112).

The appearance of complex quantities in Heisenberg's scheme and in (standard) quantum-mechanical formalism in general is thus due to the following two key factors. The first, corresponding to the first key experimental conditions in question, is that the quantum frequencies rule is de facto equivalent to the irreducible discreteness of quantum phenomena. The second is defined by the limit of considerations relating quantum and classical theory, embodied in the mathematical correspondence principle (the use of formally the same equations). Ironically, it is the fact that the complex quantities found in the Fourier representations of classical motion disappear in the final solutions of the corresponding equations (solutions that are real variables and quantities) that requires the use of the conjugate complex matrices in quantum-mechanical equations where the solutions are in general complex and not real variables. The link between these variables and the corresponding quantities observed in experiments is in terms of probabilities, calculated by means of Born's rule, which, again, amounts the use of complex conjugation. As will be seen in the next section, the same factors shaped Schrödinger's derivation of his equation, in which complex numbers were, accordingly, irreducible as well. While only implicit in Schrödinger's derivation of his equation, the correspondence considerations of the type used by Heisenberg were important to Dirac's derivation of his relativistic equation for the electron, now understood with Schrödinger's equation (cum Pauli's spin theory) as the nonrelativistic quantum-theoretical limit of Dirac's theory. These considerations may not rise to the status of key experimental factors, such as quantum discreteness and the role of probability in quantum predictions, in defining the nature of quantum formalism. They do, however, have an experimental dimension insofar as they reflect the fact that the observable parts of measuring instruments may need to be described by classical physics. (As noted earlier, measuring instruments also contain quantum strata through which they interact with quantum objects.) That is, once we are in the region where we can apply, say, to an electron, classical physics (and hence disregard the quantum aspects of the electron's behavior), the situation is classical insofar as the "behavior" of electron is both described by classical equations and can be treated as independent of the role of measuring instruments. (I add quotation marks because, as stressed throughout, the actual behavior of electrons in these regions is still quantum and can have quantum effects.) These considerations also pose the question of the borderline, also known as the "cut," between the quantum and the classical domain. I shall consider the cut in the next chapter, merely noting, in accordance with the comments just made, that, as Bohr explains, "in each experimental arrangement and measuring procedure we have only a free choice of th[e] place [where the discrimination between the object and the measuring instrument can be made] within a region where the quantum-mechanical description of the process concerned is effectively equivalent with the classical description" (Bohr 1935, p. 701).

It follows, then, that, as regards the role of complex-valued variables, the quantum situation is very different from that which obtains in classical physics, when complex numbers are used, for example and in particular, when one deals with the Fourier representation of classical motion. In classical physics, real coordinates, represented, in terms of *both frequencies and amplitudes*, by the Fourier series or, when the motion is aperiodic, integrals (with complex variables, canceling each other in the solution), correspond to actual physical quantities, as real-value functions of time, which, in classical formalism, describe, in an idealized manner, the physical processes in question. Corresponding to this situation, complex numbers disappear in the formal solutions of the equations of classical physics. By contrast, in quantum mechanics, the mathematical formalism, especially, again, the amplitudes found in the corresponding Fourier series, no longer corresponds to the observable quantities derived from continuous functions of time, describing the (quantum) processes in question in the theory. The formal solution of the equation of quantum mechanics would still contain complex variables, complex vectors in a Hilbert space, which can be related to the probabilities of quantum events via the Born or analogous rules (e.g., von-Neumann's projection postulate or Lüders's postulate). These probabilities and the measured quantities themselves are real numbers. (Technically, measured quantities are rational numbers.) Indeed, as noted above, it is, ironically, the circumstance that the complex quantities found in the Fourier representations of classical motion disappear in the final solutions of the classical equations, that, by the mathematical correspondence principle, requires the use of the conjugate complex matrices in quantum-mechanical equations, making their solutions in general complex variables. The irreducible role of complex variables in the formalism is a distinctive feature of quantum mechanics in all currently available versions of it. That the mathematical architecture within which complex numbers appear and acquire this irreducible role was originally derived in part, but *only in part*, from the correspondence argument is secondary, although of course not coincidental. I stress "in part" because the most crucial part of this derivation, the introduction of new matrix variables by Heisenberg or the wave function by Schrödinger, was independent from or in any event did not significantly depend on the correspondence argument. Nor did Born's discovery of his rule for relating complex variables involved to probability via conjugation depend on the correspondence argument.

The key features of the mathematical formalism that Heisenberg arrived at in his discovery of quantum mechanics were, then, brought by him into accord with the three main experimentally established principles stated at the outset of this section, in part by giving them their proper mathematical expressions. The first is the discreteness of quantum phenomena considered, corresponding the QD principle; the second is the unavoidable recourse to probability, corresponding to the QP/QS principle (with probabilities involved calculated through complex-valued probability amplitudes and Born's rule); and the third is the mathematical correspondence principle. As will be seen, these features also enter Schrödinger's derivation of his formalism, with the difference that his starting principles were in fact classical and specifically realist. This derivation had, however, to be negotiated with quantum principles, against Schrödinger's own grain, but enabling him to arrive at the right mathematical model. Heisenberg, by contrast, proceeded from these principles by abandoning the project of a realist representation of quantum objects and processes, even if without interpretively assuming such a representation to be impossible altogether (the RWR principle). Rather than attempting to use this scheme to represent the physical objects processes considered (a task abandoned as hopeless from the start), he found a way of coupling his scheme to probabilistic or statistical predictions, via a Born-type rule applied to spectra. It is true that the relationships between the complementarity principle and the formalism emerged later and that Bohr was less concerned with giving a mathematical expression to his principles, although he did realize the relationships between them and the formalism of quantum theory. In this respect, this study brings some Heisenberg into Bohr's argumentation and some Bohr into that of Heisenberg.[12] As noted from the outset, in Heisenberg's initial work on quantum mechanics, these physical principles were not quite given a fully rigorous mathematical expression, which emerged in the subsequent work of Heisenberg himself, Born and Jordan, Dirac, and finally von Neumann.

The implications, this study argues, are radical both for the philosophy of physics or for philosophical thinking in general, and for our understanding of the nature

[12] In general, as noted in Chap. 1, their views were always somewhat different and, especially, diverged more from the 1930s on, without, however, ever losing some affinities. These affinities position both views within the spirit of Copenhagen.

and the practice of theoretical and experimental physics alike. The situation takes an even more radical form in quantum field theory and is given yet new dimensions in quantum information theory, arguably the two most significant frontier developments of quantum theory and its foundations. The first has been in place for nearly as a long as quantum mechanics but, unlike the latter, still far from completed in its proper (high-energy) scope, with important connections to string and brane theory. The second is more recent, about four decades old. I shall discuss some implications of Heisenberg's revolution in the practice of theoretical and experimental physics for these theories in the final section of this chapter; and I shall address quantum field theory and quantum information theory, and *emerging* connections between them from the principle point of view in Chaps. 6 and 7. The next section considers Schrödinger's wave mechanics and his trajectory leading to his discovery, which was quite different from that taken by Heisenberg.

2.4 Reality and Realism in Schrödinger's Wave Mechanics

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 of quantum phenomena and of the behavior of quantum objects that would be realist and causal. It was expected to be able, just as classical mechanics did, both to *represent* the physical processes at a subatomic level (as undulatory or wave processes) *and*, on the basis of this representation, to *predic*t, ideally exactly, the outcomes of quantum experiments. Schrödinger was aware of Heisenberg's and Born and Jordan's work on matrix mechanics, and of the successes of the theory. Apart, however, from his discontent with both the mathematical and epistemological difficulties of matrix mechanics, which his wave mechanics would, he hoped, be able to avoid, his path, first, to his wave equation for the electron and then to his more ambitious program for a wave quantum mechanics was different. He proceeded from de Broglie's ideas concerning matter waves and related work by Einstein, which used de Broglie's theory, much admired by Einstein (1925a, b). Schrödinger was not fond of the probabilistic character of matrix mechanics either or of Born's probabilistic interpretation of the wave function, offered shortly after Schrödinger discovered his equation. Bohr, who adopted Born's interpretation, spoke of quantum waves as *symbolic*, as part of the quantum-mechanical machinery for predicting the probabilities or statistics of quantum experiments, which is more precise than speaking of "probability waves," as Born did (e.g., Bohr 1935, p. 697). This type of concept in effect introduced or at least anticipated in Bohr's earlier collaboration with H. Kramers and J. Slater in the BKS paper (Bohr et al. 1924). Schrödinger saw this interpretation as an extension of Bohr's atomic theory and then Heisenberg's approach, and hence in essential conflict with his own, or Einstein's, desiderata for quantum theory. He defined his vision in his letter 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* *processes*. It is extremely probable that this is possible" (Schrödinger to Bohr, 23 October 1926, cited in [Mehra and Rechenberg 2001, v. 5, p. 828]; emphasis added).

Given the preceding history of quantum theory, including matrix mechanics, one could have been skeptical concerning the suitability of Schrödinger's program. Arguably most significantly, the problem of discreteness of quantum phenomena had never found an adequate resolution within this program, which indeed aimed to dispense with this concept. For example, 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 wave approach was in conflict with Planck's radiation law, with which Heisenberg's approach was fully consistent. These difficulties ultimately compelled Schrödinger to abandon his project of wave mechanics, although, as noted earlier, in the late 1940s he returned to the view that the project could be viable (Schrödinger 1995). Be it as it may, Schrödinger's thinking of the ultimate nature of the physical world in terms of waves is significant, including as an instructive attempt, successful or not, to relate continuity and discontinuity in terms of underlying continuity. While, especially in retrospect, Schrödinger's program appears to have been difficult to complete, it reflected several deep aspects of modern physics, classical and quantum, and of the relationships among physics, mathematics, and philosophy, which have been and remain crucial to quantum mechanics and debates concerning it.[13]

Schrödinger's philosophy compelled him to postulate the underlying causal dynamics of fundamental constituents and processes in nature. As noted earlier, his position, just as that of Einstein, was not a naively realist one: he was well aware of the approximate or idealized character of all our physical theories and models, and was sometimes more skeptical than Einstein as concerns the future of the "classical ideal" both championed (Schrödinger 1935a, p. 152). This (classically) causal underlying dynamics may, under certain circumstances, lead to probabilistic or statistical outcomes in predicting actual situations, analogously to the way it happens when we use probability in classical physics. This ideal was, clearly, in a sharp and, in Schrödinger, deliberate contrast with the spirit of Copenhagen and the RWR principle, which, again, automatically preclude causality, as Schrödinger, as we have seen, never failed to realize (Schrödinger 1935a, p. 154). We keep in mind that this RWR-principle-based view is an interpretive inference, from the QP/QS principle, and as such does not, in principle, preclude a realist and causal interpretations of quantum mechanics or alternative realist and causal theories of quantum phenomena. Schrödinger clearly realized this fact. This realization additionally motivated his vision and research, similarly to the way it did for Einstein. Just as Einstein, Schrödinger never gave up on this ideal. It was the *classical ideal*, as he, again, saw it in the cat-paradox paper in order to juxtapose it to the "doctrine" defining quantum

[13] It is not my aim to offer a comprehensive account of Schrödinger's work on wave mechanics, which has received several extended treatments. Mehra and Rechenberg give Schrödinger more space than to any other founding figure, and my analysis here is indebted to their historical discussion (Mehra and Rechenberg 2001, v. 5). I am less in accord with their philosophical argumentation, and indeed part with it nearly altogether. Another major study is (Bitbol 1996). Schrödinger's collected papers on wave mechanics are assembled in (Schrödinger 1928). For his other important papers on quantum mechanics, see (Schrödinger 1995).

mechanics in the spirit of Copenhagen, "the doctrine … born of distress" (Schrödinger 1935a, p. 152). By then, in 1935, the mathematics of the doctrine would include his equation, no longer seen by him as offering a hope for a viable alternative to the "doctrine," again in accordance with Einstein's views. As noted earlier, Einstein, too, initially had hopes for Schrödinger's program, but quickly abandoned them. Schrödinger's hopes were, however, revitalized by EPR's paper, to which his cat-paradox and related papers responded, also by introducing the concept of quantum entanglement in both German [*Verschränkung*] and English. Schrödinger gradually regained faith in his initial thinking (Schrödinger 1995). I shall return to the EPR dimension of the cat-paradox and related papers by Schrödinger later in this study.

Be that as it may, in 1926, following de Broglie's ideas and, thus, taking a different path from that taken by Heisenberg, Born and Jordan, or Dirac, Schrödinger thought that it was possible to develop a wave mechanics that would account in representational, realist terms for the behavior of quantum objects responsible for quantum phenomena. While these phenomena were, as he was well aware, discrete, they would in Schrödinger's scheme arise from a continuous wave-like vibrational process. Elementary particles, mathematically considered by then as dimensionless point-like entities, would be replaced with particle-like *effects* of wave-like vibrations on this underlying wave-like vibration. As he said even before he developed his wave mechanics, but announcing the vision that led to it: "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). *The wave radiation forming the basis of the universe*—no less! The phrase is as remarkable for its philosophical ambition as for its physical one. At bottom, then, everything would be continuous, field-like. Accordingly, his mechanics was undulatory, *wave*, mechanics, rather than *quantum* mechanics, and it was analogous to classical wave physics, but, as he stressed, not identical to the latter, for one thing, given the difference between his mathematical formalism and that of classical physics. Schrödinger's "wave radiation forming the basis of the universe" was unobservable, possibly in principle unobservable. That, however, need not mean that this radiation could not exist. For one thing, quantum objects are not observable either, and yet they are assumed to exist, to be real, even in nonrealist, RWR-principle-based, interpretations.

Importantly, rather than using de Broglie's propagating waves, Schrödinger's 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. This mathematical efficiency is also found in his mathematical program in general, which 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 and then Bohm's theory a wave accompanies the particle in question, such as an electron, in the manner of a pilot wave. In Schrödinger's wave mechanics, at the level of the ultimate constitution of nature there were only waves with "particles" seen as certain singularity-like surface effects. Schrödinger, whose thinking showed some hesitancy and oscillations throughout, generally stopped short of claiming that these waves strictly *represented*, the ultimate reality of nature, as opposed to providing an intuitively accessible (*anschaulich*) model sufficiently *approximating*, still ideally, this reality, which is a form of realism in the definition adopted in this study. The theory would also possess a powerful predictive capacity, even if, in terms of underlying reality, a more limited representational capacity—more limited, but far from entirely absent in the way it was in Bohr's or Heisenberg's approach to 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. In Heisenberg's theory there would be no waves, but at the cost of renouncing any representation of physical processes concerning electrons in space-time, eventually leading Bohr to the RWR principle and his ultimate interpretation. 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, comprised by multiple discrete individual phenomena in certain circumstances, such as the appearance of the interference pattern in the double-slit experiment.

There are well-known and *relatively* straightforward paths to Schrödinger's equation. Perhaps the most natural is to derive Schrödinger's equation 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 ways it is done it in textbooks on quantum mechanics. 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 (Schrödinger appears to be the first to have written it), does not work for a relativistic electron because it has, at most, only a limited predictive capacity. One needs Dirac's equation to make correct relativistic predictions, a subject discussed in Chap. 6. 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 in the nonrelativistic case.

In terms of theoretical justification, the situation was 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. One could defer this problem to a future theory, and in fact Dirac's theory, the correct theory of the (free) relativistic electron, was not that far off. Perhaps most significant is the following problem. One derives the *right* nonrelativistic equation from a *wrong* relativistic one, the Klein-Gordon equation, if one could even speak of a "derivation," because, rigorously, Schrödinger's equation is the nonrelativistic limit (via Pauli's spin theory) of Dirac's equation. Accordingly, Schrödinger's (nonrelativistic) equation was a guess, albeit a correct guess, which would have needed to be justified otherwise. This is what Schrödinger attempted to do and, in some measure (but not completely!), accomplished in his first published paper of wave mechanics, which derives his equation differently.

Schrödinger began to lay down his more ambitious program for wave mechanics in his second paper on the subject, which also offered a new derivation of his equation. Schrödinger's new approach was based on 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. The problem of quantization was now understood as an eigenvalue problem to be treated 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, by transforming this variable into $H(x)$, it may be possible to find a vector X (eigenvector) such that $H\left(X\right) = EX$, where E is a number (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. 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 for the matter the time-independent Schrödinger's equation, mathematically *represents*. (It represents nothing in RWR-principle-based interpretations of it.) We do know, however, what it predicts.

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 semi-classical 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 that is found in his first paper. He did so by replacing, without a real theoretical justification from the first principles (which type of justification was never achieved), the mechanical Hamilton-Jacoby equation

$$H\left(q, \partial S / \partial q\right) = E$$

with a wave equation by substituting $S = K \ln \psi$ (K is a constant that has a dimension of action). This was a radical step, which led him, via a mechanical-optical analogy, to the equation,

$$\Delta\psi + \left(2m / K^2\right)\left(E - V\right)\psi = 0,$$

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

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

Here $K^2 = h^2 / 4\pi^2$, h is the Planck constant and m_e the electron mass. The Hamilton-Jacoby equation considered by Schrödinger was used in the old quantum theory, specifically by Sommerfeld and P. 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 was made via the mechanical-optical analogy and the connections between the principle of least action in mechanics and Fermat's principle in optics, and it was to give to S the wave form by $S = K \ln\psi$.[14] In the case of the hydrogen atom, one thus also replaces the deterministic mechanics of the particle motion with amplitudes and then probabilities. Schrödinger did not realize this at the time. He aimed at a (wave-like) deterministic picture, but ultimately arrived elsewhere. That would take a while to realize, although intimations of the situations were emerging nearly at every step of Schrödinger's realization of his program.

Thus, while at bottom everything in his scheme would be continuous and vibrational (wave-like), Schrödinger still had to account for discrete, quantum, observational phenomena, such as the discrete character of radiation energy, defined by Planck's constant, and Bohr's frequency conditions and other experimentally established features of the behavior of the hydrogen atom. For Heisenberg these factors were the starting point, ultimately leading him to the introduction of his new (matrix or operator) variables, while keeping the formal structure of the classical equations themselves, a combination that defined the architecture of the mathematical model of matrix mechanics. Schrödinger needed to modify both classical equations, such as, in the derivation given in his first published paper, the Hamilton-Jacobi equation, and variables to which his equation would apply. In this case, the main variable in question was his famous wave or ψ-function, essentially a complex *vector* in a Hilbert space, rather than an *operator* as the coordinate-variable or the momentum-variables were in Heisenberg's scheme.

In order to accommodate, as he had to do, quantum conditions within his scheme, Schrödinger was forced to adjust, in an ad hoc way or just about, both the wave equation with which he started and the wave function *S*, which, as noted, took the form $S = K \log \psi$, with $K = h/2$. This (neglecting the relativistic variation of mass) allowed him to replace "the quantum conditions" with a "variation problem" for ψ (Schrödinger 1926b, p. 361, Schrödinger 1928, p. 2). In other words, the wave equation that Schrödinger wrote down for his new ψ-function was the ordinary wave equation, involving two time derivatives of ψ and the Laplacian. The phase velocity of the waves related to the energy and mechanical velocity of the point particle according to the Hamilton-Jacobi theory. The group velocity in the dispersive medium is thus identical with the mechanical velocity of the point particle. However, just as Heisenberg (in whose scheme this was nearly automatic), Schrödinger had to

[14] It is worth keeping in mind that by the time of writing this paper Schrödinger already knew his equation, which, as indicated above, he discovered differently, by directly using de Broglie's formulas, rather than in his first published paper. Accordingly, his introduction of this variable is not as unmotivated or sudden as it might appear.

obtain classical mechanics as a limit of his wave theory. Accordingly, a wave packet made out of a superposition of waves with many frequencies had to have the group velocity implied by the dispersion relation in the Hamilton-Jacobi theory. That is, essentially by the correspondence argument (although Schrödinger would not have seen this in these terms), in the limiting case of large quantum numbers, the wave packet must move with the particle's (electron's) mechanical velocity. Again, analogously to the way Heisenberg's variables had to be given their complex matrix form, the dependence of frequency on energy due to the Bohr frequency condition then forced the time-dependence of the ψ-function to have a very special form. In particular, it separates into the product of a spatially dependent term with a factor of the form $e^{i2\pi Et/h}$, or its complex conjugate $e^{-i2\pi Et/h}$. This made complex numbers irreducible in the *solutions* of the equation, just as in the case of Heisenberg's matrix version, and, as in Heisenberg's case, the correspondence-type coincidence of classical and quantum predictions at the classical limit played a role in initially establishing the form of the wave function. Again, ironically, the fact that complex numbers (found in the Fourier representation of classical motion) disappear in the final solutions of the classical equations was responsible for the fact that they do not in the solution of the quantum equation. This fact requires a different type of relationship between the formalism and the quantities obtained in experiments, which are real, indeed, as measurable quantities, technically rational. These relationships, provided by Born's rule (defined by the complex conjugation), are probabilistic or statistical, which is, again, in accord with what is actually observed, insofar as identically prepared (as concerns the state of the measuring instruments used) quantum experiments in general lead to different outcomes. Born introduced his probabilistic interpretation of the wave function and his rule by taking advantage of the particular character in the wave function, as just described. One can see that the task of associating this mathematical model with a realist representation of quantum processes could not have been easy, if possible at all, as Bohr realized and often noted. As he said:

> 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, 1987, v.1, pp. 76–77; emphasis added)

By stressing the symbolic nature of both the wave and the matrix theory, Bohr affirms their physically analogous nature, their mathematical equivalence having already been established by then. This argumentation is not that far away from his ultimate, RWR-principle-based, interpretation, although Bohr is a decade away from this interpretation itself at the time.

While the predictive power and effectiveness of Schrödinger's equation was immediately apparent (also vis-à-vis the cumbersome procedures of matrix mechanics), Schrödinger's program for wave mechanics as a descriptive theory of quantum processes was never fulfilled, or in any event, it would be difficult to see it as fulfilled.[15] Bohr and others reinterpreted Schrödinger's equation along the lines of thinking used in deriving matrix mechanics, as considered above. It is clear, however, that, in Schrödinger's case, too, these were the same quantum conditions that led him to a correct equation.

Although Schrödinger's program was enthusiastically received by some, most notably Einstein, skepticism toward the program quickly took over, and dissent from the Copenhagen-Göttingen side emerged just as quickly, in his case, in part in view of Heisenberg's matrix theory and its epistemological implications, but only in part because, as already noted, the program had physical difficulties regardless of them. As noted in Chap. 1, Einstein's enthusiasm quickly faded away as well (Stone 2015, pp. 274–275). Schrödinger's equation itself was of course welcomed by nearly everybody of the "devil" (Copenhagen-Göttingen) party—Bohr, Born, Dirac, Pauli, and ultimately by Heisenberg, who was initially skeptical even concerning the equation, rather than only Schrödinger's overall wave program.

Some of the immediate reactions, comparing Heisenberg's and Schrödinger's *programs* merit a brief detour here. Sommerfeld, in particular, commented as follows: "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 me to be suspicious for the time being" (Sommerfeld to Schrödinger, 3 February 1926, cited in Mehra and Rechenberg 2001, v. 5, p. 502).

It would be more accurate to say that Heisenberg starts from the epistemological postulate not to put into the theory more data than can be observed. But this is early in the history of quantum mechanics, and Sommerfeld's slippage is understandable. In any event Schrödinger defended his approach, but interestingly, by seeing Sommerfeld's objection in terms of "possibly unnecessary assumptions," rather than "unobservable ballast." This is an important difference, arising in part from Schrödinger's view of the situation in terms of trajectories of motions, "the electrons orbits with their loops," as represented by the "the fundamental equations of mechanics" (Schrödinger to Sommerfeld, 20 February 1926, cited in Mehra and Rechenberg 2001, v. 5, p. 502). Orbits and all classical mechanical concepts of motion were abandoned by Heisenberg in view of the just about insurmountable problems they posed. Schrödinger was aware that his own scheme by no means resolved these problems, either at the time or later when his time-dependent equation was introduced, although he did hope to avoid these problems by reconceiving all these processes in terms of wave motion. The passage from his paper, cited above, clearly suggests both his awareness of these problems and his hopes of resolving them. These hopes were grounded in his belief in the existence of a "*vibration*

[15] Cf., however, Schrödinger's argument in (Schrödinger 1995), mentioned above.

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 Mehra and Rechenberg 2001, v. 5, p. 533). His theory or model does not appear to have ever been able to fulfill these hopes. Nor was Sommerfeld's criticism ever really countered by Schrödinger's subsequent papers, in which he promised to provide "more general foundations of the theory" (Schrödinger to Sommerfeld, 20 February 1926, cited in Mehra and Rechenberg 2001, v, 5. p. 502).

Schrödinger pressed on with his program in his second paper, which, as Mehra and Rechenberg note, "establishes the foundations and the definite outlines of what was later [in his next communication] called 'wave mechanics'" (Mehra and Rechenberg 2001, v. 5, p. 533). The program was, as I noted, to be enacted through the mechanical-optical analogy, accompanying, since Hamilton's work, the Hamilton-Jacoby framework for classical mechanics, the connections to which Bohr, too, refers on a number of occasions. Schrödinger wanted to "throw more *light* on the *general* correspondence which exists between the Hamilton-Jacoby differential equation of a mechanical problem and the 'allied' *wave equation*," that is, Schrödinger's equation (Schrödinger 1926b, p. 13; cited Mehra and Rechenberg 2001, v. 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. 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 a realist and causal and character of his theory. It is worth citing Schrödinger's notebook comments written in preparation for his second paper:

> The somewhat dark connections between the Hamiltonian differential equation $\left[H\left(q, \partial S / \partial q\right) = E\right]$ and the wave equation $\left[\Delta\psi + \left(2m / K^2\right)\left(E - V\right)\psi = 0\right]$ 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 $\left[\Delta\psi + \left(2m / K^2\right)\left(E - V\right)\psi = 0\right]$ 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. (*Archive for the History of Quantum Physics*, Microfilm No. 40, section 6; cited as Notebook II, p. 1 in Mehra and Rechenberg 2001, v. 5, p. 543).

The situation is more complicated than Schrödinger makes it sound, especially if one takes into account subtler aspects of these connections when they apply to the quantum-mechanical, as opposed the classical, case. If anything, these connections became even darker as quantum mechanics developed, in spite of its successes as a physical theory. As Schrödinger wrote 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, 20 February 1926, cited Mehra and Rechenberg 2001, v. 5, p. 542)

Schrödinger does, however, close his second paper with the following 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 become 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, p. 527, cited in Mehra and Rechenberg 2001, v. 5. p. 559)

The physical analogy with optics, thus, should be carefully distinguished from the Hamiltonian one. The latter analogy only gives one the wave function in a q-space (the configuration 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 fundamental equations of mechanics, in order to form a picture of the manifold of possible processes" (Schrödinger 1926c, p. 506, cited in Mehra and Rechenberg 2001, v. 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 the configuration space and not by the motion of *image points* in that space. This argumentation reflects the possibilities (and hopes) for wave mechanics and the potential difficulties involved, as well as Schrödinger's awareness of these difficulties. The business of waves in the configuration space, which was one of the immediately recognized difficulties of the program, was one of the things that Einstein found difficult to stomach as well (Stone 2015, p. 275). Schrödinger offered a number of observations concerning the nature of quantum processes as reflected in his equation and came close to the probabilistic interpretation of it, but never quite got there. Perhaps he could not have done so, given his overall philosophy and agenda. For it follows from his equation 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. He was reluctant, however, to accept the kind of suspension, let alone prohibition on lines of the RWR principle (this view, again, came later) of any representation of the underlying quantum behavior, reality, which ideal of classical physics he was not about to surrender. He was readier to give up his equation, and, as he reportedly said to Bohr at some point, he was sorry to have discovered it, and for about two decades he did give up *on* it as reflecting the ultimate workings of nature. But, as I said, in the later 1940s, he appears to have changed his mind yet again and returned to his original views (Schrödinger 1995). 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 equation of the old quantum theory of atomic structure (working with the Hamilton-Jacoby partial differential equation) had been rather artificially forced. This was soon felt by Schrödinger himself. … Did the new formulation of undulatory mechanics lead in a less arbitrary and artificial way to wave equations that described atomic systems and processes, notably the successful nonrelativistic hydrogen equation? … Was the wonderful analogy, between Hamiltonian mechanics and higher-dimensional non-Euclidean spaces and the undulatory optics, just highbrow idealistic decoration and useless for the practical purposes of atomic theory? (Mehra and Rechenberg 2001, v. 5, pp. 571–573)

Mehra and Rechenberg are right to reject "so pessimistic a view of [Schrödinger's] achievement" (Mehra and Rechenberg 2001, v. 5, p. 573). They do not, however, explore the deeper complexities of the situation or the optical-mechanical analogy of Schrödinger as reflecting these complexities. I would like to briefly comment on the subject in order to give a clearer picture of the situation. It is worth stating the main point of this analogy in classical mechanics in F. Klein's terms, familiar to Schrödinger: *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 is powerful and effective in developing the mathematical formalism of classical mechanics. It may, however, 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 three-dimensional space. It is, one might say, a *metaphor*, which, it may be added, is part of a new mathematical model of classical physics, developed by analytic mechanics. As noted in Chap. 1, while representational and thus realist, this model is ultimately nonvisualizable when the system considered is large, although it is underlain by a visualizable model that describes each individual constituent of the system. 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 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, 1987, v. 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 length" (Schrödinger to Wien, 22 February 1926, cited in Mehra and Rechenberg 2001, v. 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 Mehra and Rechenberg 2001, v. 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, again, in a certain general sense, that is, insofar as he seemed to have thought on the model of general relativity, that (classical-like) fields should be seen as primary and perhaps ultimate physical entities anyhow. As is clear from the last section of his cat-paradox paper, Schrödinger was as suspicious of quantum electrodynamics and quantum field theory as of quantum mechanics (Schrödinger 1935a, p. 167). Accordingly, Schrödinger appears to have been thinking along these more cautious lines of the relationships between these "waves" and actual physical vibrations corresponding to them (perhaps indirectly) 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.

Giving a physical content to the concept of waves applied to the behavior of quantum objects posed major and perhaps ultimately insurmountable difficulties. In nonrealist interpretations, the idea that quantum behavior is wave-like is abandoned or, when one adopts the strongest form of the RWR principle, is rigorously precluded. We recall, however, that in these interpretations, the same is also true as concerns the concept of a particle. As Heisenberg noted, even if one assumes—as, again, up to a point, both Bohr and Heisenberg did—that one could apply either (classical) concept at the quantum level, one could only do so "with certain limitations" and never in its entirety (Heisenberg 1930, p. 47). It is true that a particle is also an idealization in classical mechanics, but of a different kind: this idealization represents it as a dimensionless point endowed with mass, which, ideally, conforms to the classical concepts and laws of motion. In quantum mechanics, the concept cannot apply to quantum objects in full measure in view of the uncertainty relations or, in accordance with the RWR principle, at all, any more than can the concept of wave or possibly any other concept. On the other hand, apart from the fact that there are the uncertainty relations for quantum waves as well (Heisenberg 1930, pp. 48–52), physically, all observable individual quantum phenomena are individual and discrete (discontinuous from each other), in accordance with the QD principle. Any quantum phenomenon that is wave-like in appearance is 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.[16] This circumstance is one of the reasons why wave–particle complementarity is not something that appealed to Bohr, a point on which I shall further comment in the next chapter.

It is, thus, difficult, if possible at all, to develop a representational and especially causal model of quantum processes, to which the mathematical "optical" machinery in the configuration space can relate in the classical-like representational way, in quantum physics. One may, accordingly, have to 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 or statistical, 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 representational models of the kind that one can construct in classical physics. In truth, the type of infinite-dimensional space over complex numbers used in quantum mechanics (for continuous variables) or even finite-dimensional complex Hilbert spaces used in dealing with discrete variables could be called space only by analogy and metaphorically. Indeed this use is metaphorical even in the case of classical physical models, nonvisualizable models of analytic mechanics, representing large mechanical system by phase spaces of high dimensions. Of course, the term space is well established mathematically even in these cases—now. It was not the case for most the nineteenth century, when space meant physical space.

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. According to Born's famous formulation: "[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 1926, p. 804; also Born 1949, p. 103). This formulation, especially the statement that "the motion of particles follows the probability law," is somewhat vague, which is forgivable given that it was the first attempt to understand the wave function in these terms. What is this law? Does Born mean that one could 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 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, while stated metaphorically (one cannot speak of a propagation, causal or not, of probability otherwise than metaphorically), the second part of Born's formulation is in accord with the spirit of Copenhagen. Schrödinger himself makes it clear in his cat-paradox paper and other papers on quantum entanglements, by speaking of the wave function as an "expectation-catalog," even as he disparages the spirit of

[16] Such traces are dot-like only at a low resolution, which "disguises" a very complex physical object, composed of millions of atoms, and is particle-like only in the sense that a classical object idealized as a particle would leave a similar trace.

Copenhagen itself (Schrödinger 1935a, p. 158). The wave function gives us only probability amplitudes and one needs to use Born's or some related rule to form such catalogues. (One also needs to use probability densities.)

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, future events, on the basis of certain previously performed experiments, already established events. These probabilities are determined and, again, determined exactly by Born's rule, applied to the wave function or alternative procedures that achieve the same results. There is no propagation of probabilities either, but only certain catalogs and patterns of probabilities, reset after each new measurement, which makes the preceding "history" of measurement irrelevant as concerns one's prediction from this point on, before the next actual event will reset our probability catalogs yet again. The physical manifestations of these patterns 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.

What becomes apparent, then, is that whether they are related to continuous or discrete variables and quantum mechanics, quantum phenomena make us confront the essentially probabilistic or statistical 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 a similar rule. These rules, or again, postulates, have no rigorous justification from within the formalism or model itself, even though the shift, defining Born's rule, from a complex quantity to its modulus, which is a real quantity (necessary for expressing the probability of a given event), is natural mathematically. The justification for Born's rule is experimental: it works! To return to Heisenberg's terms, these rules become postulates of the theory, if not of the model the theory uses, to which these rules are exterior (Heisenberg 1930, p. 108). This justification—it works!—is not discountable, of course, and classical physics, from Galileo and Newton on, is ultimately justified experimentally, too, with that crucial difference that there our predictions are defined and, in this sense, justified by the representational 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 equations (we can, accordingly, no longer speak of equations of motion) become, in Heisenberg's terms, a new kinematics and a new form of relationship 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 who aimed to do precisely this and thus to circumvent the probabilistic character of matrix mechanics as well. Heisenberg's program, grounded in the QP/QS principle was a founding move of quantum nonrealism, ultimately leading to the RWR principle.

While they retain Schrödinger's mathematics, nonrealist, RWR-principle-based interpretations reverse the vision that grounded and guided Schrödinger's physical program, defined by the idea of "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, probabilistic or statistical predictions enabled by Schrödinger's equation and Born's or related rules are physically linked to a set of discrete individual phenomena, corresponding to certain wave-*like* correlational patterns. A certain Hamiltonian optical-mechanical 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 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 accompanied by representational and causal models. By contrast, in quantum physics, in nonrealist interpretations, such models are abandoned or even precluded, thus, making the "optics" in question strictly predictive and moreover probabilistically or statistically predictive. The quantum-mechanical Hamiltonian equations map no motion in space and time and only predict probabilities or statistics of the outcomes of experiments, staged as physical situations defined by classical observable phenomena, manifested in measuring instruments.

Beyond explicating an important part of quantum mechanics, the preceding remarks and my overall discussion of Schrödinger's work allows one to reassess his contribution to quantum theory and his thinking itself. By offering this reassessment, this discussion also becomes a tribute to Schrödinger's work on quantum mechanics, even though some of the most significant aspects of this work, as assessed here, emerge against the grain of his thought and his conception of what a fundamental theory ought to be. As I have stressed throughout, however, there are also alternative perspectives on foundational physics, including quantum theory, and thus on what a successful fundamental theory of quantum phenomena should be, perspectives that are closer to Schrödinger's, or Einstein's, thinking, rather than to that of Heisenberg and Bohr. I hope that my discussion of Schrödinger's thinking may also be helpful to those who hold such alternative views. I do not mean that my aim is to convince them to abandon these views. Apart from the fact that there is little hope that one can succeed in doing so, this aim is not necessarily the most beneficial one for the aim this study hopes to accomplish, which is to contribute, however modestly, to deepening our understanding of quantum foundations. This aim is, I believe, best achieved by allowing this confrontation, these many confrontations, initiated by founding figures, to continue. Certainly these figures learned a great deal from each other by responding and, sometimes, not responding to each other's thinking and arguments. Our best bet might well be to continue to learn from all of them and from each other.

2.5 “A Rational Quantum Mechanics” and “A New Era of Mutual Stimulation of Mechanics and Mathematics”

I began this chapter with Bohr’s comments on matrix mechanics and in the last section, “The Development of a Rational Quantum Mechanics,” of “Atomic Theory and Mechanics” (Bohr 1925, 1987, v. 1, p. 48). These comments were written in the wake of Heisenberg’s paper and Born and Jordan’s first paper, which developed Heisenberg’s argument, into a full-fledged, matrix mechanics, but before Schrödinger introduced his wave mechanics (Heisenberg 1925; Born and Jordan 1925). Now, in closing, I would like to cite Bohr’s concluding remarks there. These remarks concern the role of mathematics in quantum mechanics, which, Bohr suggests, is as significant as it has ever been in modern physics. On the other hand, quantum mechanics also establishes a new type of relationship between mathematics and physics that, in accord with the argument of this study (which follows Bohr on this point), makes quantum mechanics, especially in nonrealist interpretations, depart from all preceding physics in its use of mathematics. Bohr’s comments might be unexpected, given the subsequent trajectory of his thought, especially his insistence on the defining role of measuring instruments, rather than on the central significance of mathematics in quantum mechanics, more crucial for Heisenberg. The measuring instruments came to replace “the mathematical instruments,” invoked in Bohr’s passage, in playing “an essential part,” even the most essential part, in his interpretation of quantum phenomena and quantum mechanics from the 1927 Como lecture on. However, in 1925, in the wake of Heisenberg’s discovery and Born and Jordan’s work of matrix mechanics, Bohr wrote:

> 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, 1987, v. 1, p. 51)

Bohr’s appeal to “the *rational* formulation of the new quantum mechanics” is worth registering, especially in conjunction with his several invocations of the “irrationality” inherent in the quantum postulate. I shall return to this point in Chap. 3, merely noting for the moment that the “irrationality” invoked in his earlier writings is not any “irrationality” of quantum mechanics, which Bohr, again, always sees as a “rational” theory (Bohr 1925, 1987, v. 1, p. 48). It is a rational theory of something that may, in a certain sense, be irrational—that is, inaccessible to a rational representation or perhaps to thinking itself (the view adopted here). This fact requires the replacement of a rational representational theory, such as classical physics, with a *rational* probabilistically or statistically predictive theory, which replacement is the new rational quantum mechanics, introduced by Heisenberg. Finally, this appeal also, and I would argue, deliberately, echoes Newton's characterization of his mechanics as "rational" in the *Principia* (Newton 1999) and, by doing so, implies that, while, unlike Newton's mechanics, the new quantum mechanics does not represent quantum processes in space and time, it is equally rational.

The subsequent history has proven that Bohr was too optimistic as concerns the physicists' attitude. There has been "thankfulness that mathematics in this field, too, presents us with the tools to prepare the way for further progress." On the other hand, discontent with "the limitation" in question has never subsided and is still with us. Einstein, again, led the way. He did not find satisfactory or even acceptable this state of affairs as concerns physics or this type of use of mathematics in physics, and this limitation, which, I argue here, ultimately extends to any representation rather than only visualization. Schrödinger was quick to join, with many, even a substantial majority of, physicists and philosophers to follow.

Be it as it may on this score, Bohr is right to stress the essential role of the mathematics in question for quantum mechanics, especially for 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.) It is also significant that Bohr speaks of "a new era of mutual stimulation of *mechanics* and mathematics," rather than more generally physics and mathematics, although Heisenberg's discovery redefined the relationships between them as well. Most fundamentally at stake, however, are elementary *individual* quantum processes and events. The mathematical science, which is both representational and predictive, of these processes in classical physics is mechanics. It is, correspondingly, classical mechanics that is now replaced by quantum mechanics, but as a nonrealist theory that only predicts, moreover in probabilistic or statistical terms, such individual events as effects of quantum processes upon measuring instruments without representing these processes.

Perhaps ironically, Heisenberg's approach creates new and greater possibilities for the use of mathematics and physics, thus, as I said, revealing, what Wigner called, "the unreasonable effectiveness of mathematics in physics," as against classical physics, where this effectiveness is, as I also explained, not unnatural (Wigner 1960). This is because, on the one hand, we search for a natural mathematical representation of the processes that we (visually) observe in daily life, and on the other, disregard what we cannot mathematize. This effectiveness in quantum theory is enigmatic, as well as fortunate, because in the absence of a mathematical representation of elementary individual quantum processes and events of the type found in classical mechanics or (with further reservations explained earlier) in relativity, it is unclear why quantum mechanics makes predictions strictly in accord with what is observed. Indeed, it follows that one's choice of a mathematical scheme under these conditions becomes relatively arbitrary insofar as one need not provide any representational physical justification for it, but only to justify this scheme by its capacity to make proper predictions of the data in question. It is true that the actual developments of the mathematical formalism of quantum mechanics extended (via the correspondence principle) from the representationally justified formalism of classical mechanics. One can, however, also start directly with the Hilbert-space or C^*-algebra formalism, or sheaf and category theory, or possibly arrive at still some other formalism by experimenting, as it were, with mathematics or mathematical technology, the "mathematical instruments" invoked by Bohr. The role of complex numbers and certain other (shared) mathematical aspects of the formalism (all versions of the formalism have been mathematically equivalent thus far) that appear in this formalism have been ubiquitous thus far and appear to be unavoidable. It is difficult, however, to be entirely certain that this will remain the case in the future.

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 the 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." A similar argument was 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).

Heisenberg, modestly, does not mention his own pioneering role in the invention of this "mathematical scheme," which was, however, hardly a secret to his readers. Once again, not everyone at the time, beginning with Einstein, or since then, saw this scheme as "entirely adequate" for the treatment of atomic processes. In nonrealist interpretations, beginning with that of Bohr, this scheme did not represent atomic (quantum) processes at all. That Heisenberg *found* a mathematical scheme that could predict the data in question was as fortunate as that mathematics is free of this limitation, for this freedom is also found in classical physics and in relativity, beginning at least with Lagrange's and Hamilton's analytical mechanics. It is true that matrix algebra was introduced in mathematics before Heisenberg, who was, again, unaware of it and had to reinvent it, although the unbounded infinite matrices that he used were not previously studied in mathematics and were given a proper mathematical treatment by Born and Jordan later. But, even if Heisenberg had been familiar with it, his scheme would still have needed to be invented as a mathematical model dealing with quantum phenomena. This, Heisenberg realized, was possible to do if one limits oneself to probabilistic or statistical predictions in the absence of any representation of quantum objects and their behavior. Indeed, mathematics now becomes in a certain sense primary, even though, quantum mechanics cannot be reduced to mathematics and, as against classical physics, contains an irreducible nonmathematical remainder, because no mathematics can apply to quantum objects and processes. But then, nothing else, physics or philosophy, for example, can apply either. The key physical intuition was that there could be no physical intuition that could possibly apply to quantum objects and processes, while one could use mathematics to predict the outcomes of experiments. In other words, this situation required the kind of conjoined physical and mathematical intuition displayed by

Heisenberg. This intuition depends fundamentally on the role of mathematics, even as it redefines the relationships between mathematics and physics.

Bohr's elaboration under discussion 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 that Bohr's views of quantum mechanics, from "Atomic Theory and Mechanics" (Bohr 1925) on, are defined by the essential roles of both measuring and mathematical instruments, of experimental and mathematical technologies, 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 1954b, 1987, v. 2, p. 68). It suggests that, even if considered apart from physics, mathematics is a form of technology, a form of technology of thought, rather than something absolute or ideal, along Platonist lines, a technological perspective to be considered in more detail in Chap. 7. For the moment this view of mathematics allows one to see Heisenberg's discovery of quantum mechanics from this technological perspective as well, following both Bohr and Heisenberg in his earlier work, in contrast to somewhat stronger Platonist tendencies in Heisenberg's later thinking. I qualify this assessment, because 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 one all the more aware of this complexity. In particular, one could see Heisenberg's return to Plato, as against Aristotle, in continuity with Heisenberg's approach to quantum mechanics. As is well known, this discovery had some links to Plato, specifically, Plato's mathematical "atomism" in *Timaeus*, which Heisenberg was reading at the time and to which he referred on several occasions in his later writings (e.g., Heisenberg 1962, pp. 39, 43). (According to *Timaeus*, elementary atoms are mathematical forms, rather than physical entities.) More crucial here are conceptual connections defined by 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 realist mathematical models of motion, which have grounded classical physics from Galileo on. The mathematical grounding of modern physics goes beyond Aristotle, but, as discussed earlier, is still shadowed by Aristotle's physics and its concept of motion in this regard. Quantum mechanics and then higher level quantum theories are defined by fundamentally different physical principles and, as a result are radically different, nonrealist, mathematical models of nature, models that offer no mathematical or other representation of motion or anything else at the quantum level and thus in effect abandon the classical or possibly any conceivable concept of motion. At least, again, they allow for this type of interpretation and make realist alternatives difficult. Quantum mechanics, however, does not replace the concept and mathematics of motion with a Platonist mathematical model of nature based on an immutable reality, a reality without motion or change. The concept of motion and possibly even change may not apply to quantum objects or their behavior. But neither could the concept of the immutable, "standing still," whether mathematical or not. We certainly register the effects of change between measurements, and even 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 at the quantum level, especially if one assumes the theory and nature to be local.[17] Nevertheless no concept of change may still be applicable at the quantum level, certainly at the level of quantum effects manifested in measuring instruments.

The role of these effects of change might have been one of the reasons why, in his later thinking Heisenberg was compelled to invoke certain dynamic properties of matter itself at the quantum level and even the possibility of representing these properties mathematically (Heisenberg 1989, p. 79). Heisenberg, again, sees this view as Platonist, in opposition to Democritean atomism, which he wants to abandon, just as Bohr did beginning with the Como lecture of 1927. What each offers or aspires to achieve instead appears to be quite different, however. Building on and radicalizing Heisenberg's original approach to quantum mechanics and the fundamental principles of quantum theory, Bohr, as will be seen in the next chapter, replaces the Democritean doctrine with his new "atomism" of the individual phenomena observed in measuring instruments (e.g., Bohr 1949, 1987, v. 2, pp. 32–33). By contrast, Heisenberg in his later thinking, especially in quantum field theory, appears to want to give a certain mathematical or, ideally, mathematizable and hence representational or at least realist on Kantian lines (the second type of realism defined in Chap. 1) non-Democritean ontology to the ultimate constituents 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, ultimate quantum or sub-quantum dynamics, he does not invoke motion and thus, on this point, bypasses Aristotle along with Democritus.

This is not surprising, because from the 1930s on, Heisenberg's primary model becomes that of quantum field theory and virtual particle formation, to be discussed in Chap. 6. This model makes it difficult to speak of physical motion on the model of classical physics. In particular, in contrast to the low-energy regimes governed by quantum mechanics, this kind of "motion" deprives us of the possibility of maintaining the identity of a given elementary particle, say, an electron even with a single experiment. Instead the particle is transformed by the process into another particle

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[17] Cf. J. 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 derive from the idea that it does not appear possible by means of quantum theory to describe or represent the motion of the ultimate constituents of nature. From the present 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.

or a set of particles in the process of its motion (e.g., a positron, a photon, an electron–positron pair, etc.). In nonrealist interpretations, these transformations, just as elementary particles themselves, manifest their existence, reality, only in the corresponding observable and measurable effects. These effects are coupled to a particular mathematical formalism, and thus certain configurations of experimental technology are coupled to those of mathematical technology. The allowable, or forbidden, transitions and the probabilities of such transitions (the theory is probabilistic or statistical, the QP/QS-principle-based, just as quantum mechanics is) are rigorously specified by quantum field 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 the changes in quantum phenomena we observe and to the very emergence and constitution of these phenomena. In this view, however, this "happening" or this "something" is beyond any representation of possibly any conception we can form, including those of happening or something-ness, or particle or field, or, when it comes to probabilities, of propensity. Indeed, given the randomness of individual quantum events and the fact that all patterns found in quantum events are correlational and thus collective, it is quite difficult to speak of propensity. Heisenberg continues to maintain 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, because, as explained above, mathematics need not depend on such concepts. Heisenberg even speaks, via Aristotle of matter itself not as reality but only as a possibility, and of the existence of matter as only a form, thus closer to structural realism (Heisenberg 1962, p. 119).

Still, it is difficult to bring this mathematical ontology in accord with the RWR principle, which precludes any form of ultimate ontology or realism, even of the structural-realist type. Nor, in the RWR-principle-based view, are there propensities to the behavior of quantum systems themselves. There only degrees of expectation and corresponding probabilities or statistics defined by the overall experimental setup of performed actual measurements and possible measurements with predicted outcomes, usually dealing with large sets of repeated measurements. The mathematics of quantum theory enables us to make these probabilistic or statistical expectations as accurate as appears possible thus far, and no other predictions, again, appear possible thus far. But this is also all that this mathematics does for us, at least if interpreted in accordance with the RWR principle, and no other mathematics *appears* to be able to do more, at least, again, insofar as one maintains the locality of the theory or nature itself, although (hence my emphasis) the debate concerning this last point continues.

It is, again, remarkable and even miraculous, as well as mysterious (which is not the same as mystical), that the mathematics of quantum mechanics enables us to make such estimates 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. It is, accordingly, not surprising either, especially given the subsequent developments of quantum field theory, to which he made important contributions, that Heisenberg eventually came to see mathematics along more ontological lines.

Once again, however, there is ambivalence in his attitude, because, as noted in Chap. 1, he always continued to maintain certain affinities with Bohr’s view.

I am aware that this assessment is itself ambivalent or ambiguous, but I also do not think that these complexities in Heisenberg’s later views could be definitively resolved, or even (although one cannot be certain on this point either) were even resolved in his own mind. Be that as it may, while we may be lucky that nature allows our mathematics to do so and to have this mathematics, itself a gift of nature via the human mind (also 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.

Nor is this all that he had accomplished. For, Heisenberg’s revolutionary thinking also revolutionized the very practice of theoretical physics, and, as a consequence, it redefined experimental physics as well, or perhaps made experimental physics realize what its practice had in fact already become by that point. The practice of experimental physics no longer consists, as in classical experiments, in tracking the independent behavior of the systems considered, but in *unavoidably* creating configurations of experimental technology that reflect the fact that what happens is *unavoidably* defined by what kinds of experiments we perform, by how we affect quantum objects, rather than only by their independent behavior. My emphasis on “unavoidably” reflects the fact that, while the behavior of classical physical objects is sometimes affected by experimental technology, in general we can observe classical physical objects, such as planets moving around the sun, without appreciably affecting their behavior. This does not appear to be possible in quantum experiments. That identically prepared quantum experiments lead to different outcomes, thus making our predictions unavoidably probabilistic or statistical, appears to be correlative to the irreducible role of measuring instruments in quantum experiments. Bohr came to realize this early in his work on complementarity, although perhaps not until his first exchanges with Einstein on the subject in 1927–1928. As he said: “Since, in the observation of [quantum] phenomena, we cannot neglect the interaction between the object and the instruments of observation, the question of the possibilities of observations again comes to the foreground. … 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” (Bohr 1987, v.1, p. 93).

The practice of theoretical physics, then, no longer consists, as in classical physics or relativity, in offering an idealized mathematical representation of quantum objects and their behavior, but in developing mathematical machinery that is able to predict, in general (again, in accordance with what obtains in experiments) probabilistically or statistically, the outcomes of quantum events and of correlations between some of these events.

As will be seen in Chaps. 6 and 7, the situation acquires a more complex and more radical form in quantum electrodynamics and then quantum field theory, and experimental physics in the corresponding (high) energy regimes. While, at least in the present view, conforming to the situation just outlined, quantum electrodynamics and quantum field theory are characterized by, correlatively:

1. more complex configurations of phenomena observed and hence measuring apparatuses, experimental technology, involved, and thus more complex configurations of effects of the interactions between quantum objects and measuring instruments;
2. a more complex nature of the mathematical formalism or models of the theory, its mathematical technology, in part reflected in the necessity of renormalization;
3. a more complex character of the quantum-field-theoretical predictions and, hence, of the relationships between the mathematical formalism and the measuring instruments involved, between the mathematical and the experimental technologies of high-energy quantum physics.

In this view, all quantum events, from those associated with Planck's quanta to those associated with the Higgs boson, are observed in rigorously specified configurations of experimental technology. This fact establishes the connections between the mathematical and the experimental technology of quantum physics, and makes technology in its broader sense a kind of foundation of quantum physics. In order to understand why such is the case, however, we need to traverse the landscape of Bohr's thinking following the introduction of quantum mechanics.

Chapter 3
Complementarity: "This New Feature of Natural Philosophy"

Abstract This chapter considers Bohr's argument concerning, in his phrase, "the epistemological lesson of quantum mechanics," an argument advanced under the general heading of complementarity, the term that initially referred to a concept and a principle but that eventually came to designate Bohr's interpretation of quantum phenomena and quantum mechanics. While centered on Bohr's ultimate, RWR-principle-based, interpretation, the chapter will address the development of Bohr's thinking, in part under the impact of his debate with Einstein, leading Bohr to this interpretation. Sect. 3.1 offers a general introduction. Sects. 3.2 and 3.3 consider the concept and principle of complementarity. Sect. 3.4 discusses the EPR experiment and what I shall call "the EPR complementarity," which grounded Bohr's analysis of the experiment in his reply to EPR's paper. Sect. 3.5 discusses Bohr's ultimate, RWR-principle-based, interpretation. Sect. 3.6 considers Bohr's view of quantum probability and statistics.

3.1 Introduction

This chapter offers an analysis of Bohr's argument, developed over three decades under the general heading of complementarity, concerning quantum phenomena and quantum mechanics, and the main physical and philosophical implications of this argument, especially those due to the RWR principle, which ultimately came to define his interpretation. In Bohr's work, the term complementarity designated a concept, a principle, and an interpretation of both quantum phenomena and quantum mechanics, largely grounded in this concept and guided by this principle. While Bohr rarely expressly spoke of complementarity as a principle, its use and, especially, its guiding role as a principle are clearly apparent in his writings. Bohr sometimes also spoke of complementarity as a "viewpoint," in effect, however, referring to his interpretation (Bohr 1935, p. 696). It is important that it is an interpretation of

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both quantum phenomena and quantum mechanics.[1] As throughout this study, however, I shall mostly speak of Bohr's interpretation, qualifying only when necessary. Bohr's ultimate, RWR-principle-based, interpretation was essentially in place by the late 1930s. A few revisions were added later on, but they were, I would argue, minor. On the other hand, several earlier revisions, in part under the impact of Bohr's debate with Einstein, were significant, especially because they were characterized by progressively more radical departures from realism, eventually leading Bohr to the RWR principle and his ultimate interpretation.

The reception of Bohr's argument for complementarity has been and continues to remain mixed. Bohr is customarily careful in his choice of terms, definitions, and arguments. However, his plural use of the term, the lack of a single definitive formulation of the concept at any stage of his work, and the absence of reflections on Bohr's part on changes in his interpretation could be confusing and have discouraged some of his readers. Another factor that hindered physicists and most philosophers of physics was the verbal character of Bohr's writing, rather than a reliance on the quantum-mechanical mathematical formalism and its *efficiency*, as is more common in quantum theory. (The reasons for my emphasis will become clear momentarily.) Von Neumann's formulation of quantum mechanics, or still more abstract versions, such as those in terms of C^*-algebra or, more recently, category theory, have nearly uniformly defined not only theoretical physics but also the institutional (primarily analytic) philosophy of quantum physics. Bohr's argumentation was also inhibited by, in his own word, "the *inefficiency* of expression," of which he was "deeply aware," possibly also having an unfavorable contrast with the *efficiency* of mathematics on his mind (Bohr 1949, 1987, v. 2, p. 61, emphasis added). This inefficiency, however, was also and even primarily due to epistemological complexities of his argument and the radical character of his views, which Bohr was determined to bring out and which mathematics sometimes hides (Bohr 1949, 1987, v. 2, p. 61). These complexities and the radical character of his views were, arguably, most responsible for the resistance to Bohr's argument, as Bohr, again, realized (Bohr 1949, 1987, v. 2, pp. 61, 63).

In my view, the difficulties of Bohr's writing are exaggerated, in part because of this resistance. On the other hand, these difficulties cannot be denied or explained away by this resistance either. Thus, Einstein confessed, after decades of his exchanges with Bohr, that he was "unable to attain … the sharp formulation … [of] Bohr's principle of complementarity" (Einstein 1949b, p. 674). That Einstein saw complementarity as a principle is noteworthy and unsurprising, given Einstein's views of Bohr as a principle thinker, as discussed in Chap. 1. It is true that

[1] As indicated earlier, in some of its aspects, this interpretation still holds for quantum phenomena even if a theory other than quantum mechanics is used to predict the data in question in quantum experiments, as exemplified by Bohmian mechanics and other recent proposals, some of which were mentioned earlier. At the same time, Bohmian mechanics and most of these proposals entail the realist character of the mathematical models they use at least as concerns quantum objects and their behavior (the situation is more complex as concerns quantum phenomena, still defined by what is observed in measuring instruments). The QD and QP/QS principles, and the uncertainty relations hold in these theories as well, but again, under the assumption that the ultimate behavior of quantum objects is causal.

complementarity did not, at least expressly, "give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy," as required by Einstein's definition of principle thinking (Einstein 1919, p. 228). As noted earlier, however, it could do so, specifically by way of the criteria associated with the noncommutativity of the quantum-mechanical formalism. As I shall argue, as a *quantum-theoretical* concept (it could be used elsewhere) complementarity includes the mathematics of quantum mechanics, along with the probabilistic or statistical nature of quantum predictions, as part of its architecture. These aspects of complementarity became especially crucial to Bohr's analysis of the EPR argument and his reply to EPR's paper (Bohr 1935). It is of some interest that, as indicated earlier, Einstein misread Bohr's argument there, by attributing to Bohr the assumption of nonlocality of quantum phenomena. Indeed the statement just cited occurs as part of Einstein's comment on Bohr's reply (Einstein 1949b; Bohr 1987, v. 2, pp. 681–682). I would argue that this misreading was in part due to insufficient attention on Einstein's part to the architecture of complementarity as a concept. On the other hand, Bohr's reply is among the more difficult of Bohr's works and the most antithetical to Einstein's way of thinking and, arguably, writing. Bohr's phrase "the inefficiency of expression" refers to his reply in commenting on it in his 1949 "Discussion with Einstein."

This chapter is designed to clarify Bohr's argument concerning complementarity in all three senses of the term, including in Bohr's reply to EPR, to which Sect. 3.4 of this chapter is devoted. My main aim, however, is to explore deeper physical, philosophical, and mathematical aspects and implications of this argument and of complementarity *as a quantum-theoretical concept*, as the concept of concept is defined in Chap. 1, in particular its probabilistic or statistical character or, correlatively, its mathematical aspects. Although rarely considered in commentaries on Bohr, these aspects of the concept and of Bohr's interpretation are crucial, including to Bohr's exchanges with Einstein, for whom probabilistic or statistical considerations were central as well. Einstein realized the radical nature of Bohr's argument, his difficulties with or even misconception concerning Bohr's reply to EPR notwithstanding. He saw this argument "as logically possible without contradiction, but … so contrary to [his] scientific instinct that [he could not] forego the search for a more complete conception," that is, for an Einstein-complete and hence realist theory, which would avoid probability or statistics in considering elementary individual processes and events (Einstein 1936, p. 349). Bohr recognized, with some chagrin, that this rejection was based on Einstein's philosophical stance or "attitude." He said: "Even if such an attitude might seem well balanced in itself, it nevertheless implies a rejection of the whole argumentation exposed in the preceding [essentially explaining Bohr's ultimate interpretation], aiming to show that in quantum mechanics, we 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 [beyond a certain point]" (Bohr 1949, 1987, v. 2, p. 62). This (we are in 1949) is Bohr's most definitive expression of the RWR principle, although Bohr, again, might not have subscribed to the strongest form of the principle, which implies that the nature of quantum objects and processes is not only beyond analysis and representation but also beyond conception. As will be seen later in this chapter and in Chap. 4, this particular exchange contains further subtleties, which are related to the probabilistic or statistical nature of quantum mechanics, which makes it Einstein-incomplete, while allowing it to be Bohr-complete, as complete a theory of quantum phenomena as possible, as things stand now.

Bohr’s statement just cited reflects the essence of his interpretation, as, by this point, grounded in the RWR principle and his concept of phenomenon (discussed in Sect. 3.5), and his conception of completeness of a physical theory, Bohr-completeness, vis-à-vis Einstein-completeness. Bohr saw this concept of completeness as sufficient for, in the words of his reply to EPR, quantum mechanics to be in accord “with the basic principles of science,” an accord, Bohr argues, ensured by the role of the concept of complementarity in it (Bohr 1935, p. 700). Einstein’s criticism, Bohr contended, “could not be directed” to disproving this argument. “In my opinion,” he said, “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* [*not descriptions or representations*!] did not exhaust the possibilities of observation, and Einstein’s argumentation could be directed to neither of these ends” (Bohr 1949, 1987, v. 2, p. 57; emphasis added). Einstein would not have necessarily objected to this last assessment as such. However, given that the formalism, while logically consistent, was manifestly short of representing individual quantum processes, this was not a kind of completeness Einstein deemed necessary in a fundamental theory and believed possible in a proper theory of quantum phenomena as well.

As that of Bohr, the argument of this chapter has a strong philosophical flavor. However, my main concern in this chapter is physics, just as it was Bohr’s main concern in his writings on complementarity, and even his essays that aimed at extending the concept and, in his famous phrase, “the epistemology lesson of quantum mechanics” beyond physics, would always give at least equal attention to physics (Bohr 1987, v. 3, p. 12). Accordingly, complementarity will be primarily considered here as a *physical* and specifically quantum-theoretical concept or principle, or, again, as an interpretation of quantum phenomena and quantum mechanics. This demarcation, again, gives complementarity, as a concept and an interpretation, additional features, in particular, probabilistic or statistical and mathematical features. These features are rarely emphasized or even considered in commentaries on Bohr, but they are essential to complementarity as a *quantum-theoretical* concept and to Bohr’s interpretation.

This chapter will be most essentially concerned with Bohr’s ultimate interpretation, based in the RWR principle. However, in part in order to better understand Bohr’s thinking that shaped this interpretation, I shall address the development of his thinking, which evolved throughout Bohr’s scientific life, beginning with his earlier work on the old quantum theory, discussed in the preceding chapter, or his still earlier thinking. As I said, on several occasions this evolution was marked by departures, even major departures, from Bohr’s previously established views. In particular, while Bohr’s concept of complementarity was introduced in the Como lecture in 1927 (Bohr 1927), it was modified by Bohr, along with, and correlatively to, a change in his interpretation, in 1928–1929 under the impact of his initial exchanges with Einstein. The Como lecture is the only article on complementarity that was not impacted by these exchanges and that did not respond to Einstein's criticisms of quantum mechanics.[2] These exchanges grew into the famous confrontation between them, which has shaped and continues to shape the debate concerning quantum mechanics and its interpretation.[3] Bohr's interpretation was progressively characterized by more radical departures from realism, eventually leading him to the RWR principle and to abandoning realism altogether.

In the mid-late 1930s, two new (correlative or even equivalent) concepts, "phenomenon" and "atomicity," were introduced by Bohr, in conjunction with the RWR principle, and became increasingly prominent in his writings. Neither these concepts nor the RWR principle were in place in Bohr's reply to EPR's paper. However, most of the key ingredients of these concepts and the key rationales for the RWR principle are found there, given Bohr's concept of "the finite [quantum] and uncontrollable interactions between quantum objects and measuring instruments" (Bohr 1935, p. 700), to be discussed in Sect. 3.4. Bohr introduced the concepts of phenomenon and atomicity, which were also essentially connected to the RWR principle, in his 1937 "Complementarity and Causality" (Bohr 1937) and in his 1938 Warsaw lecture, "The Causality Problem in Atomic Physics" (Bohr 1938). The concept and the principle of complementarity retain their significance throughout Bohr's work. However, his evolving views also led him to rethink the nature of specific complementarities, such as that of position and momentum measurements, and to the introduction of new complementarities. I shall also argue that his rethinking of the EPR experiment in terms of complementarity gave rise to a new specific complementarity, which I shall call "the EPR complementarity." Although Bohr did not use this term, the EPR complementarity may give Bohr's concept its arguably deepest meaning and significance as a quantum-theoretical concept.

It is also sometime in the 1930s that the term "complementarity" came to be used by Bohr to designate his overall interpretation of quantum phenomena and quantum mechanics. I reiterate that, according to the present view, Bohr's interpretation is *an* interpretation, one of several possible interpretations, again, of both quantum phenomena and quantum mechanics, and not an expression of the ultimate truth of nature. Bohr would, I think, have agreed with this view, although some of his statements may suggest a stronger claim in this regard. However, as Bohr often stressed, this interpretation is logically consistent and is in conformity with all the relevant experimental evidence available thus far. As Bohr said in his reply to EPR, from the vantage point of complementarity (in all three senses): "quantum mechanics within its scope would appear as a completely rational description of physical phenomena, such as we meet in atomic physics" (Bohr 1935, p. 696). His emphasis on the com-

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[2] Articles on complementarity written by Bohr after Einstein's death were still shaped by the ideas that emerged in these exchanges.

[3] Bohr gave an extensive account of this confrontation, while also presenting, as part of this account, his ultimate interpretation, in "Discussion with Einstein on Epistemological Problems in Atomic Physics" (Bohr 1949), his contribution to the so-called "Schilpp volume," *Albert Einstein: Philosopher-Scientist*, edited by P. A. Schilpp (Schilpp 1949).

pletely rational nature of this description is, again, customary in his writing for the reasons indicated in Chap. 2. Quantum mechanics is as complete as nature allows our theories of quantum phenomena to be, as things stand now, or in present terms, is Bohr-complete, even though it is not Einstein-complete, insofar as it is not a realist and causal (and hence, at this level, ideally deterministic) theory of individual quantum processes and events. Or at least, quantum mechanics could be interpreted as Bohr-complete. Bohr's statement just cited is cautious and is hardly a claim concerning much beyond the consistency and completeness (Bohr-completeness) of Bohr's interpretation, which is why I said above that Bohr would have been likely to see his interpretation as an interpretation, rather than the truth of nature.

As throughout this study, while associating it with the spirit of Copenhagen, to which it gave rise, I distinguish Bohr's interpretation from the Copenhagen interpretation, a rubric that can be best seen as referring to a set of loosely related interpretations linked to but, as a rule, different from that of Bohr. There is more than one interpretation of quantum phenomena and quantum mechanics even in Bohr's case. Bohr himself, again, did not reflect on and barely, if at all, acknowledged these changes. On the other hand, one often encounters inflections of his earlier view by his later ones in his commentaries, as in his later discussions of the Como lecture (Bohr 1929b, 1987, v. 1, pp. 9–15; Bohr 1949, 1987, v. 2, pp. 31–32). That he did not comment on these changes is an intriguing point, given that some of them were significant and even major. One could only conjecture his reasons, and I shall refrain from any definitive claims concerning these reasons. However, these changes themselves are important for my argument, because they reveal the deeper aspects of complementarity as a concept, a principle, and an interpretation of quantum phenomena and quantum mechanics.

3.2 The Concept of Complementarity: Parts Without a Whole

Although the concept or the principle of complementarity was never quite given by Bohr an exact, "sharp," definition in a single formulation, such as the one offered in Chap. 2, the type of definition stated there may be surmised from several of Bohr's statements. I would argue that by the time of his reply to the EPR argument and even by 1929 (Bohr 1929a, b), Bohr gave the concept or principle of complementarity this type of meaning, even if, again, not in a single sharp definition. Bohr comes closest to such a formulation in "Discussion with Einstein," although the article apparently failed to satisfy Einstein on this point, given that his remark concerning his inability to attain "the sharp formulation … [of] Bohr's principle of complementarity" was made following and responding to this article in "the Schilpp volume" (Einstein 1949b, p. 674). I shall focus on the concept of complementarity, although much of the present discussion would apply to the principle of complementarity as well. According to Bohr: "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, 1987, v. 2, p. 40). This formulation clearly implies the difference between quantum objects and quantum phenomena (the concept established by Bohr at this point) and, as a result, the fact that any possible information concerning quantum objects is only that about their effects on measuring instruments, effects that define phenomena. I shall return to this epistemology in Sect. 3.5. It is worth restating the definition of complementarity given in Chap. 2. Complementarity is defined by:

(a) *a mutual exclusivity of certain phenomena, entities, or conceptions; and yet*
(b) *the possibility of considering each one of them separately at any given moment of time; and*
(c) *the necessity of using all of them at different moments of time for a comprehensive account of the totality of phenomena that one must consider in quantum theory.*

This definition is very general and allows for different instantiations of the concept in the case of quantum phenomena and for the application of the concept beyond physics. It is worth keeping in mind that such instantiations are concepts in their own right. Part *(b)* is not stated in the above formulation from "Discussion with Einstein." It can, however, be easily established on the basis of Bohr's other elaborations there, or elsewhere, especially, again, in his reply to EPR, where, as will be seen below, Bohr's argument crucially depends on this possibility. That we have a free choice as concerns what kind of experiment we want to perform is in accordance with the very idea of experiment in science, including in classical physics (Bohr 1935, p. 699). However, contrary to the case of classical physics or relativity, implementing our decision concerning what we want to do will allow us to make only certain types of predictions and will exclude the possibility of certain other, *complementary*, types of predictions. We actively shape what will happen, define the course of reality, if a nonrealist interpretation of quantum phenomena and quantum mechanics is adopted. In this sense, complementarity may be seen, as it was by Bohr, as a generalization of causality and as representing what may be called "quantum causality" (discussed in Chap. 5), in the absence of classical causality and realism. Parts *(b)* and *(c)* of the definition are just as important as part *(a)*, and by missing them, as is often done, one would miss much of the import of Bohr's concept, or would (deliberately or not) depart from it.[4]

[4] An instructive case of departure from Bohr's concept is L. Susskind's concept of the black-hole complementarity, as the mutual exclusivity of the physical situations inside and outside a black hole (Susskind 2006, pp. 334–336). This concept is different from that of Bohr, say, as applied to the position and momentum measurements performed on a quantum object, which entails the possibility, available at any moment of time, of performing either a position or a momentum measurement associated with this object, according to *(b)* in applying Bohr's concept. In the case of Susskind's black-hole complementarity no such alternative is available. An object considered could only, at any point, be either inside or outside a black hole, which makes it impossible to define a complementary situation at this moment of time. There is no choice of the type required by Bohr's concept. One merely deals with two mutually exclusive situations.

Bohr's complementarity is a *concept*, and it should be treated as such by respecting its specificity. As noted in Chap. 1, according to Bohr, the term complementarity, as an "artificial word" designating a quantum-theoretical concept, "does not belong to our daily concepts" and "serves to remind us of the epistemological situation here encountered, which at least in physics is of an entirely novel character" (Bohr 1937, p. 87). Indeed, as explained in Chap. 2, the concept of complementarity is *correlative* to this entirely novel epistemological situation, ultimately defined by the RWR principle. In this understanding, complementarity prevents us from ascertaining the "whole" composed from the complementary "parts," thus, in conflict with the conventional understanding of parts *complementing* each other within a whole. At any moment of time only one of these parts and not the other could be ascertained, as an effect of quantum reality manifested in a measuring device. Hence, this ascertainable part is the only "whole" at this moment of time. Bohr developed this understanding, which, again, amounts to adopting the RWR principle (in conjunction with the QD and QP/QS principles), and revised the concept of complementarity accordingly in view of the EPR-type experiments. As a quantum-theoretical concept, complementarity also acquires predictive, probabilistically or statistically predictive, aspects and, it follows, mathematical aspects, because one needs mathematics, such as that of quantum mechanics, to predict probabilities or statistics of quantum experiments. When complementarity applies to *predictions* concerning (complementary) experimental arrangements, in which the corresponding measurements and application of physical concepts occur, this application is probabilistic or statistical, in accordance with the QP/QS principle. These predictions correspond to, in Schrödinger's terms, "expectation-catalogs," provided by the formalism and Born's or related rules.

The definition of complementarity given above is more general, rather than only defining it as a quantum-theoretical concept. This definition allows for applications of the concept elsewhere in physics and beyond physics. Bohr and a few others inspired by him, such as W. Pauli, K. G. Jung, and M. Delbrück, proposed using the concept in philosophy, biology, and psychology, where the concept became a subject of renewed attention recently, as part of the general surge of interest in the use of quantum-mechanical-like modeling.[5] Some among extensions of complementarity beyond physics are manifestly different from Bohr's concept because they limit complementarity to *(a)* and *(c)*. This may be permissible in the corresponding domains, but this is not how the concept is conceived by Bohr or used by him as a quantum-theoretical concept, because part *(b)* of the concept—*the possibility of applying each complementary component separately at any given moment of time*—is essential to complementarity as a quantum-theoretical concept. I shall not consider extensions of the concept beyond physics. This is not to deny the significance of the connections between physics and other fields manifested in the use of com-

[5] See (Pauli 1994, pp. 149–164), which addresses connections between quantum theory and psychology. On complementarity in biology, see, Bohr's "Light and Life" and "Light and Life—Revisited" (Bohr 1954a, 1987, v. 2, pp. 3–12, 1962a, 1987, v. 3, pp. 2–29). See also (Plotnitsky 2014; Haven and Khrennikov 2013; Wang and Busemeyer 2015).

plementarity or Bohr's thinking in general beyond physics and made possible by them. This would be against the spirit of Bohr's thinking, as reflected in his numerous remarks made throughout his writings, for example, that in the second volume of his philosophical essay, entitled, *Essays 1933—1957 on Atomic Physics and Human Knowledge*, a title defined by these connections. He said: "We are not dealing here with more or less vague analogies, but with an investigation of the conditions for the proper use of our conceptual means of expression [shared by different fields]. Such considerations not only aim at making us familiar with the novel situation in physical science, but might … be helpful in clarifying the conditions for objective description in wider fields" (Bohr 1987, v. 2, p. 2). An earlier statement, reprised by Bohr here, nuances his point: "We are not dealing here with more or less vague analogies, but with clear examples of logical relations which, in different contexts, are met with in wider fields" (Bohr 1958, 1987, v. 3, p. 7).

While it is true that Bohr himself only made suggestions concerning such investigations rather than pursued them, they have been undertaken by others, as in the works cited above, as well as on previous occasions by the present author (e.g., Plotnitsky 1994, 2002). An investigation of this type is, again, beyond the scope of this study. It might, however, be helpful to venture beyond physics in commenting on the genealogy of the concept of complementarity in Bohr's thinking, which genealogy does extend beyond physics, including to psychology, Bohr's main scientific interest before physics (Bohr 1962b, session 5; Folse 1985, p. 175; Plotnitsky 2012a, pp. 172–179).[6] First, this commentary may help us to understand Bohr's thinking leading him to complementarity and some aspects of his use the concept in quantum theory. Secondly, because, as I argue, quantum physics was the main shaping force in this thinking, asymmetrically positioning the genealogy of complementarity between physics and other fields, these comments will explain some of the reasons for this asymmetry.

William James's use of "complementary" (as an adjective) in referring to a split of "the total possible consciousness" into parts in his seminal (principle) 1890 work, *The Principles of Psychology,* is often cited in this connection. James said: "in certain persons, at least, the total possible consciousness may be split into parts which coexist but mutually ignore each other, and share the objects of knowledge between them. More remarkably still, they are complementary" (James 1890, p. 204). Whether Bohr was influenced by James in this regard (or even knew of James's use of the term) or more generally has been debated. The evidence is sparse. My own sense is that possible influences of James's thinking on that of Bohr, or independent parallel traits in their thinking, would have been related less to complementarity than to some of the key epistemological aspects of James's pragmatism.[7] These influences, then, would be analogous to and conjoined with possible influences of Hume, Kant, and Nietzsche, specifically their critiques of causality and, in

[6] Among the works addressing Bohr's philosophical background, see (Faye 1991; Favrhold 1992; Folse 1985; Honner 1987; Murdoch 1987; Plotnitsky 1994).

[7] James also influenced, along similar lines, the so-called quantum Bayesianism or Qbism, discussed in Chap. 4.

Nietzsche’s case, realism, to which, as the idea that the world, as it independently exists could be represented or even conceived, as a “fable” (Nietzsche 1977, pp. 485–486).[8] As will be seen below, there are also junctures of Bohr’s thinking where one could suggest connections to and possible influences of Bergson, Husserl, Heidegger, and Whitehead (all Bohr’s contemporaries, prominent at the time), although these figures are rarely invoked by commentators on Bohr. A friend of Bohr’s father, Georg Brandes, whom Bohr admired, was an early champion of Nietzsche and taught the first ever course on Nietzsche at the University of Copenhagen. In addition, even in his early thinking, along the lines of complementarity, in psychological epistemology, Riemann’s ideas concerning the multivalued functions of complex variables might have been more significant than those of James (Bohr 1972–1999, v. 1, p. 513, 1962b, session 5; Plotnitsky 2012a, pp. 172–179).

In any event, Bohr’s concept of complementarity is very different from that of James. For one thing, it is not clear whether James ever developed this insight concerning “complementary” parts of consciousness into a real concept. In fact it is not entirely clear what “complementary” means in James beyond the fact that “the total possible consciousness may be split into parts which coexist but mutually ignore each other, and share the objects of knowledge between them.” It appears that James had something additional in mind, given that he also said that “more remarkably still, they [these parts] are complementary.” It is, however, never made clear what he specifically refers to. That Bohr makes complementarity into a noun (James only uses “complementary” as an adjective) is significant, because doing so helped Bohr to define it as a concept and even make it into a concept in the sense of this study. Most crucial, however, is the specificity of the architecture of Bohr’s concept, beginning, again, with the fact that James’s formulation does not entail parts *(b)* and *(c)* of Bohr’s concept, both of which arise from physical considerations. Equally important, once one moves to complementarity as a quantum-theoretical concept, are the probabilistic or statistical, or correlatively mathematical, aspects of it, which give the concept its quantum-theoretical specificity, also found in Bohr’s concepts of phenomena and atomicity, through which all complementary configurations are defined in Bohr’s later thinking. It was quantum physics that made these concepts, again, in the specificity of their architecture, necessary for Bohr, although Bohr’s thinking concerning quantum physics also helped him to define more general philosophical aspects of these concepts, such as parts *(a), (b),* and *(c)* of the concept of complementarity, and gave these concepts their radical philosophical potential.

In considering this potential, one deals with *extending* them, as already established concepts, beyond physics. In tracing their genealogy in Bohr’s thinking, the traffic proceeds in the other direction. Such an investigation, however, might also want to avoid vague analogies and explore a proper use of conceptual means shared by different fields, as urged by Bohr in the case of extending complementarity

[8] Ultimately, James *appears* (a claim that could, I admit, be challenged) to be a realist in the present definition, while Nietzsche is not. One might also note James’s reliance on an unexamined concept of “use,” which is open to a critique along Nietzschean lines.

beyond physics. Consider, for example, the case of cubism, which, I would suggest, presents both risks of the first and possibilities for the second. Cubism has been invoked as an influence on Bohr, who owned a 1911 cubist painting, *La Femme au Cheval* [A Woman on a Horse], by Jean Metzinger, which he bought at an auction in 1932 (as will be seen, this date may be significant) and apparently liked to explain to his visitors (Anderson 1967; Pais 1991, p. 335). According to *Wikipedia*, which appears to draw on A. I. Miller (2005): "The artist has broken down the picture plane into facets, presenting multiple aspects of the subject simultaneously. This concept first pronounced by Metzinger in 1910—since considered a founding principle of Cubism—would soon find its way, via complementarity, into the foundations of the Copenhagen interpretation of quantum mechanics; the fact that a complete description of one and the same subject may require diverse points of view which defy a unique description" ("*La Femme au Cheval*," Wikipedia). The article then cites Miller: "Cubism directly helped Niels Bohr to discover the principle of complementarity in quantum theory, which says that something can be a particle and a wave at the same time, but it will always be measured to be either one or the other. In analytic cubism, artists tried to represent a scene from all possible viewpoints on one canvas.… An observer picks out one particular viewpoint. How you view the painting, that's the way it is. Bohr read the book by Jean Metzinger and Albert Gleizes on cubist theory, *Du 'Cubisme'* [1912]. It inspired him to postulate that the totality of an electron is both a particle and a wave, but when you observe it you pick out one particular viewpoint" (Miller 2005, p. 44).

This is not a promising starting point, conceptually or historically, for exploring possible connections between complementarity and cubism. Conceptually, these statements are in conflict with Bohr's concept and his thinking, even granting that Miller's remarks occur in a popular article. Bohr would have never "postulate[d] that the totality of an electron is both a particle and a wave, but when you observe it you pick out one particular viewpoint." As should be apparent from the preceding discussion, this statement is manifestly in conflict with Bohr's concept of complementarity.[9] In Bohr's definition, complementary parts never reflect a totality, or a whole, so that one could, arbitrarily, at will, pick a particular point to observe one part or the other. Bohr expressly says as much: "we are not dealing with an incomplete description characterized by the arbitrary picking up of different elements of physical reality at the cost of sacrificing other such elements" (Bohr 1935, p. 699). Also, a single electron is never both a particle and a wave, for one thing, because there would not be complementarity then. Ultimately, for Bohr it is neither. To the degree that one could see an electron as a particle, one could speak of a probability wave associated not with this electron but with what we can predict concerning the outcomes of possible experiments performed on this electron.

Historically, while there are affinities between cubism in Bohr's thinking and complementarity, the claim that "cubism *directly* helped Bohr to discover the principle of complementarity in quantum theory" is, I would argue, a stretch, even if one

[9] As noted in Chap. 1, Miller also argued, unconvincingly, that Bohr's complementarity restored to quantum mechanics visualization lost in the preceding development of the theory (Miller 1978).

speaks only of “helping.” Apart from the fact that Bohr had other sources and inspirations, complementarity has a specific set of features that would be difficult to find in and hence to derive from cubism. Miller misses most of these features, which leads to a major, even if not uncommon, misrepresentation of the concept. The ultimate reason for Bohr’s invention of complementarity is, I argue, a set of interpretative problems found in quantum physics, and the overall architecture of the concept responds to and thus arises from these problems, especially its quantum-theoretical architecture, but also its philosophical architecture, which could be extended beyond quantum theory.

That said, however, unlike, it is worth noting, Einstein (Pais 1991, p. 335), Bohr had interest in and intellectual affinities with cubism or modernist art in general. Metzinger’s painting is too tempting not to consider in this connection, even though it would be difficult to make definitive claims concerning its impact, or that of cubism in general, on Bohr’s thinking and complementarity. It is not clear, for example, at what point Bohr read Metzinger and Gleizes’s book. My surmise from the record (although it is, again, difficult to be certain) is that Bohr developed his interest in cubism after quantum mechanics and the introduction of complementarity (as noted above, he bought the painting in 1932). He might have and is likely to have seen some cubist paintings earlier and they might well have stayed in his memory and impacted some of his thinking from the unconscious. It is also likely that he saw cubist paintings, such as *La Femme au Cheval*, in his own, quantum-mechanical, way and found things there that the painters themselves were unlikely to contemplate. But then, it is the essence of great art (or of everything great, Bohr’s thought included) to allow others later on to move beyond how one thought in creating it.

Could one say, then, that in considering the relationships between cubism and complementarity “we are not dealing …with more or less vague analogies, but with an investigation of the conditions for the proper use of our conceptual means of expression,” shared by different fields (Bohr 1987, v. 2, p. 2)? Could they be connected in a way helpful to explore “logical relations which, in different contexts, are met with in wider fields” (Bohr 1987, v. 3, p. 7)? They, I shall now suggest, might be, particularly in considering the problem of fragmentation, which, central to cubism, extends across art, philosophy, and mathematics and science, and beyond, in a new way from the early twentieth-century on, and which is at stake in the concept of complementarity. Complementarity is a concept of fragmentation, a fragmentation without a whole that this fragmentation fragments.

To begin with, once one deals with fragments one could only make a probabilistic (if not necessarily numerical) assumption as concerns the whole of which these fragments are parts or a lack of such a whole, and then try to justify why our estimate is correct or is more or less likely. In fact, that these fragments are the fragments of a whole is already an assumption that may or may not be true. This fact is crucial to complementarity, as underlain by the RWR principle, in which case, this assumption is not true. Cubist paintings still appear to rely on our Euclidean intuition of objects and relations between them in space. Or do they? Perhaps they are instead aiming to tell us that the whole or whatever it is that is behind the parts is different than our Euclidean thinking tells us it may be. We know, of course, that our Euclidean intuition and images of objects and their relations are constructions of our brain from much more fragmented pictures that our eyes "see," to the degree one could speak of seeing apart from this construction. Given that this was discovered before cubism, which is contemporary with the rise of modern neuroscience, cubists must have been aware of this as well. Bohr certainly knew this when he undertook his early investigations in psychological epistemology. Or perhaps, cubist paintings tell us, closer to "this seeing without seeing," that these fragments give us the best connection to the real, without representing it. This is what a woman on a horse is! We should not trust our Euclidean intuition, Euclidean *realism*, which tells us what is the nature of *reality* behind this image, thus no longer, a representation of this reality.

Did cubists take the next step, taken by Bohr with complementarity? This is doubtful in my view. Cubism might have ultimately remained closer to its other contemporary and precursor, relativity, which fragments reality, too, by its representation, but still, by being a representational, realist theory, retains representation and a certain (admittedly, not Euclidean) wholeness behind parts. Cubism, or relativity, might have helped Bohr to take this step. I surmise, however, that Bohr would have taken this step in any event, as necessitated by the nature of quantum phenomena, in order to bring quantum mechanics in accord "with the basic principles of science," which was the main task the concept of complementarity aimed to accomplish (Bohr 1935, p. 700). In the case complementary quantum phenomena in Bohr's sense, "fragments" or "parts" do not add up to a whole at all. They are not, or are unlikely to be, "fragments" or "parts" of a "whole," in the way it happens in classical physics or even relativity, in which the *breakdown* of the concept of wholeness (in either sense) is the essence of complementarity. This, again, makes the use of such terms of as "whole" and "parts" provisional and ultimately inapplicable. Each complementary part is the only wholeness there is at any given point. Nor, if one uses complementarity in Bohr's sense, could one speak of complementary parts as adding to a whole.

J. S. Bell faults Bohr on his idiosyncratic use of the term complementarity as relating to "elements which *contradict* one another, which do not add up, or derive from, a whole." Bell even ventures a psychological "explanation" (my quotation marks): "Perhaps [Bohr] took a subtle satisfaction in the use of a familiar word with the reverse of its familiar meaning" (Bell 2004, p. 190). One can understand Bell's discontent with Bohr (in part, for the reasons stated from the outset of this chapter), and it is not my intention to reproach Bell *for this discontent* itself. Expressly siding with Einstein, Bell does not favor Bohr's interpretation or for that matter quantum mechanics itself, which attitude is legitimate and, again, far from uncommon. On the other hand, even leaving aside Bell's unwarranted psychological surmise, Bell's comments themselves are unfortunate and misleading. First of all, Bell is not accurate. While "complementary" as an adjective is a familiar word, "complementarity" as a noun is not. Philosophical, let alone physical ("charm"? "color"?), concepts often change the meaning of familiar words, or have no linguistic meaning at all, but designate only mathematically defined entities. As noted earlier, according to Bohr, "an artificial word like 'complementarity' which does not belong to our daily concepts serves only … to remind us of the epistemological situation here encountered, which at least in physics is of an entirely novel character" (Bohr 1937, p. 87). Complementarity is a new physical and philosophical concept, which must, accordingly, be understood in the specific sense Bohr gives it. There is no point in attempting to relate it to a common meaning it is given in our daily life (where in fact it is not used as a noun) or meanings we might prefer to give it for whatever reasons, as Bell does, by defining complementary parts as adding up to a whole. This is precisely what Bohr wants to avoid, because he needs a new concept to account for a "new feature of natural philosophy," and not, as Bell suggests, for the reason of some "subtle satisfaction in the use of a familiar word with the reverse of its familiar meaning." There is no evidence that Bohr ever had such a satisfaction. On the other hand, there is plenty of evidence for his physical reasons for defining complementarity in the way he did: the uncertainty relations, the double-slit and other iconic quantum experiments, the EPR experiment, and so forth.

Reaching the limits of this breakdown of the idea of parts as adding to a whole, in Bohr's ultimate interpretation, based in the RWR principle, the complementary parts are manifested, as *effects*, in measuring instruments under the impact of quantum objects. Accordingly, even a partial representation of quantum reality is no longer available. We only deal with parts of the observed classical world, which, however, still never sum up into a whole. Each such configuration—an effect cum the specific and specifically defined experimental arrangement in which this effect is observed—defines, technologically, a phenomenon or an instance of atomicity in Bohr's sense (to be more properly explained below). This constitution of quantum phenomena is general, and not all of such instances need have a possible complementary counterpart. When, however, one deals with complementary phenomena one never deals with complementary properties of quantum objects or their independent behavior, because, by the RWR principle, no attribution of such properties, single or joint, to quantum objects is possible in the first place. Indeed, no complementary arrangements or phenomena can ever be associated with a single quantum object. One always needs two quantum objects in order to enact, in two separate experiments, two complementary arrangements, say, those associated, respectively, with the position or the momentum measurement, with the measured quantity itself physically pertaining strictly to the measuring instrument involved. The uncertainty relations, too, now apply to the corresponding variables physically pertaining to measuring instruments and not to quantum objects. Furthermore, in Bohr's interpretation, the uncertainty relations mean that one cannot even define, rather than only cannot measure or represent, both variables simultaneously. This view was in place even before Bohr developed his ultimate, RWR-principle based, interpretation. The recourse to probability or statistics, to which the uncertainty relations are correlative and which is, again, part of the architecture of complementarity as a quantum-theoretical concept, is the cost of our active role in *defining* physical events in quantum physics, rather than merely tracking them, as in classical physics. I shall return to these aspects of Bohr's concept below. I mention them here, in order to indicate the complexity and, again, *specificity* of the architecture of complementarity, especially as a quantum-theoretical concept, keeping in mind the definition of concept adopted by this study and explained in Chap. 1. In addition, the presence of these features in the architecture of complementarity confirms that physics appears to always override all other considerations in Bohr's thinking, perhaps because quantum physics confronted him, as it still confronts us, with questions that only physics can answer. But it can rarely do so completely on its own either, and sometimes it needs to take advantage of philosophy and art in its search for these answers or the right way of asking these questions.

3.3 Complementarity as a Quantum-Theoretical Concept: Measurement, the Uncertainty Relations, and Expectation-Catalogs

In turning to complementarity as a quantum-theoretical concept, I note first that wave-particle complementarity, with which the concept of complementarity is associated most commonly, did not play a significant role in Bohr's thinking, at least following the Como lecture of 1927 and even there. Bohr certainly thought, even before quantum mechanics, about the problem of wave-particle *duality*, as it is sometimes known. De Broglie's formulas for matter waves (in which wave-properties could be understood symbolically rather than physically) and Schrödinger's wave mechanics, rather than only his equation (which, too, could be interpreted symbolically and, via Born's interpretation of the wave function, in probabilistic or statistical terms) had a certain appeal to Bohr in his initial thinking concerning complementarity. Bohr used de Broglie's formulas in his elegant elementary derivation of the uncertainty relations in the Como lecture (Bohr 1927, 1987, v. 2, pp. 57–60). This derivation was helpful in establishing them as the law of nature, rather than an artifact of quantum mechanics, which, by virtue of containing them, could be seen as a theory that conforms to this law, as, and correlatively, it conforms to the law of the probabilistic or statistical nature of quantum predictions. Considered as symbolic and as associated via Born's interpretation with probabilities, quantum waves were instrumental in establishing this conformity. However, beginning at least with the introduction of wave mechanics by Schrödinger (if not De Broglie's matter waves), Bohr was aware of the difficulties of applying the concept of physical waves to quantum objects, even by way of complementarity. Bohr does not appear to have extensively used wave-particle *complementarity* as a concept, although some of his earlier statements do suggest such a concept, for example, when speaking of wave and particle ideas as complementary in the Como lecture, again, following the qualifications just given (Bohr 1927, 1987, v. 1. p. 75). 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. Instead either "picture" is seen by Bohr as an *effect* or set of *effects*, particle-*like* (which may be individual or collective) or wave-*like* (which are always collective), of the interactions between quantum objects and measuring instruments, in which these effects are manifested. Bohr continued to use the idea of symbolic waves as related to probability or statistics of quantum predictions, in accordance with Born's interpretation, as a kind of "probability wave," with the qualifications given in Chap. 2 (Born 1926, p. 804, 1949, p. 103). Probability waves are "expectation-catalogs" concerning discrete events. In the case of statistical interpretations one would associate these catalogs with discrete events manifested in repeated identically prepared experiments. As I noted, a similar idea was entertained by Bohr before Born and even before quantum mechanics in the BKS paper (Bohr et al. 1924), which helped Heisenberg's thinking that led him to the discovery of quantum mechanics.

Bohr's first specific instance of complementarity was introduced, along with the concept itself, in the Como lecture, "The Quantum Postulate and the Recent Development of Atomic Theory" of 1927 (published in 1928). It was the complementarity of space–time coordination and the claim of causality. This complementarity proved to be problematic, and the concept of complementarity was not adequately defined in the lecture either. Bohr was never satisfied by the lecture and famously delayed its publication. By the time it was finally published Bohr already changed his views under the impact of the intervening discussion with Einstein that took place in Brussels just one month later (Bohr 1949, 1987, v. 2, pp. 41–47).[10] Bohr's overall argument in the Como lecture was grounded in Planck's quantum postulate, as Bohr called it. The *concept* of the quantum postulate presented there was, however, Bohr's own and contained several features that were used in other instantiations of complementarity and in Bohr's concepts of phenomenon and atomicity later on. According to Bohr, the "essence [of quantum theory] may be expressed in the so-called quantum postulate, which attributes to any atomic processes an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's quantum of action [*h*]" (Bohr 1927, 1987, v.1, p. 53). Bohr then said:

> 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,

[10] The present discussion follows the published version. Although one cannot be certain how definitively it represents Bohr's views expressed in Como, it appears to be as definitive as any available drafts of the lecture. It was so treated as such by Bohr himself, for example, in his "Introductory Survey" to *Atomic Theory and the Description of Nature*, published in 1931 (Bohr 1987, v. 1, pp. 9–15). This collection gives the article's date as 1927, perhaps because by the time the lecture was published, in 1928, Bohr had changed his view. For the history and drafts of the article, beginning with the draft of the lecture (it is not clear to what degree Bohr's presentation followed it), and helpful commentaries by J. Kalckar, see volume 5 of Bohr's collected works (Bohr 1972–1996, vol. 5).

> 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, 1987, v. 1, pp. 53–54)

As indicated earlier, the "irrationality" invoked here and elsewhere in Bohr's writings is not an "irrationality" of quantum mechanics itself, which Bohr, again, sees as a *rational* theory, a rational theory of something that may be "irrational" in the sense of being inaccessible to a rational representation. This fact requires the replacement of a rational representational theory, such as classical mechanics (which, as I noted, Newton characterized as "rational" in his *Principia*), with a *rational* probabilistically or statistically predictive theory, which is a "rational quantum mechanics" (Bohr 1925, 1987 v. 1, p. 48). Bohr's point (which is often misunderstood) is the difference between the rationality of a theory and the irrationality of what this theory rationally deals with. 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. Quantum mechanics and complementarity are rational forms of understanding this situation. The point is made expressly in Bohr's 1929 "Introductory Survey" to his *Atomic Theory and the Description of Nature*. 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, 1987, v. 1, p. 7; emphasis added). Bohr, as we have seen, also says there: "Moreover, the purpose of such a technical term [as complementarity] 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" (Bohr 1929b, 1987, v. 1, pp. 19–20). By that time Bohr's main instantiations of the concept of complementarity and even the concept itself, as a quantum-theoretical concept, had changed. His view of the rational nature of quantum mechanics is, however, the same as in the Como lecture. Bohr introduced complementarity there as follows:

> On one hand, the definition of the state of a physical system [here a system consisting of a quantum object or set of quantum objects], as ordinarily understood [i.e., in classical mechanics], 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 could 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.… Indeed, in the description of atomic phenomena, the quantum postulate presented us with the task of developing a "complementarity" theory the consistency of which can be judged only by weighing the possibility of definition and observation.* (Bohr 1927, 1987, v. 1, pp. 54–55; emphasis added)

These formulations come short of rigorously defining complementarity as a concept and conveying its proper architecture as it was eventually worked out by Bohr. In particular, it is not clear what complementary means here beyond "exclusive," corresponding, in terms of the definition of Bohr's concept in its ultimate form given above, to: *(a) a mutual exclusivity of certain phenomena, entities, or conceptions*. Parts *(b)* (*the possibility of considering each one of them separately at any given moment of time*) and *(c)* (*the necessity of using all of them at different moments of time for a comprehensive account of the totality of phenomena that one must consider in quantum theory*) of the concept that came to define it later are not stated in the lecture. While *(c)* appears to be implied and to be the reason why Bohr sees space–time coordination and the claim of causality not only as "mutually exclusive" but also as "complementary," *(b)*, which became central for defining the concept later on, does not figure in the lecture. The reason may be that, as explained above, the complementarity of space–time coordination and the claim of causality makes it difficult to rigorously define this feature. In any event, the complementarity of space–time coordination and the claim of causality proved to be short-lived. It disappeared from Bohr's writings following his exchanges with Einstein in 1927, mentioned above, exchanges that were to continue until Einstein's death in 1955. I cannot consider the reasons for Bohr's abandonment of this complementarity in detail, and shall restrict myself to a few essential points.[11] The main reason appears to have been that, after or even in the course of his discussion with Einstein in Brussels, Bohr realized that one could not claim that the independent, "undisturbed," behavior of quantum objects is causal.

An emphasis on, and the very notion of, "disturbance" contribute to the problems of Bohr's argument, which, again, never satisfied Bohr, although he republished the lecture without changes later (Bohr 1927, 1987, v. 1, pp. 52–91). Bohr, as I said, never expressly commented on the changes in his view, perhaps because they had changed already before the Como lecture was published in 1928. This may also be the reason why the 1931 republication of the lecture in (Bohr 1987, v. 1, pp. 52–91), dates the lecture 1927, which is when it was given, and not 1928, which is when it was first published. Bohr's appeal to "disturbance" is understandable insofar as, unlike in classical mechanics, the independent, "undisturbed," behavior of quantum objects cannot, in principle, be observed. However, the concept is problematic because it may imply the possibility of a specific spatiotemporal and possibly causal

[11] I have considered Bohr's Como argument in (Plotnitsky 2012a, pp. 59–70).

concept of this behavior, which appears to have been entertained by Bohr around the time of the Como lecture and shaped his argument there. The concept of "disturbance" of quantum objects by measurements was abandoned by Bohr, as was the concept of the "creation" of the attributes of quantum objects by measurements (Bohr 1938, p. 104, 1949, 1987, v. 2, p. 64, 1954b, 1987, v. 2, p. 73). Bohr's preferred term became "interference:" measurements *interfere* with quantum objects. The latter do, again, exist and behave independently, but there is nothing that we can say or even imagine concerning this existence or behavior.

The renunciation of the ideal of causality or, and to begin with, of realism in considering independent quantum processes became central to Bohr's thinking concerning quantum phenomena following his initial exchanges with Einstein, and ultimately led Bohr to the RWR principle and his interpretation based in this principle. His next publication on the subject abandons the view that the independent behavior of quantum objects could be considered causal and the complementarity of the space–time coordination and the claim of causality (Bohr 1929a). By the time of his reply to EPR, he speaks of "a final renunciation of the classical ideal of causality" (Bohr 1935, p. 697). Eventually Bohr came to see complementarity as "a rational generalization of the . . . ideal of causality," which is very different from maintaining (classical) causality, even if only as complementary to something else (Bohr 1949, 1987, v. 2, p. 41). The Como argument merits, however, a brief further discussion.

Heisenberg's uncertainty relations were arguably the most decisive impetus for Bohr's invention of the concept of complementarity and the development of his first interpretation of quantum phenomena and quantum mechanics, presented in the Como lecture. On the other hand, the complementarities corresponding to the uncertainty relations, such as that of position and momentum measurements, or that of the time and the energy measurements, were somewhat secondary to that of space–time coordination and the claim of causality. The Como argument was *influenced* by Schrödinger's approach, developed, as discussed in Chap. 2, under the assumption that quantum processes could be causally represented in spatiotemporal terms by a quantum-theoretical (wave) model. This is not to say that Bohr accepted the latter assumption itself (hence, my emphasis on "influenced"). The idea that the lack of determinism in predicting quantum phenomena is due to the disturbance introduced by observation into a causal independent evolution of a quantum system, appears to originate with Dirac. Dirac developed this view, under the impact of Schrödinger's wave mechanics, sometime in 1926, while he was at Bohr's Institute in Copenhagen and was working on his transformation theory there (Dirac 1927a). By bringing causality into the picture, Bohr in part revised his earlier view, considered in Chap. 3, shaped by Heisenberg's argument that led him to the discovery quantum mechanics (Bohr 1925, 1987, v. 1, pp. 48–49). Heisenberg himself in his uncertainty-relations paper had a more critical view of both Schrödinger's program and of the application of the idea of causality to quantum processes. His views, however, were in turn to change under the impact of Bohr's Como argument, although Heisenberg remained ambivalent concerning attributing causality to the independent quantum processes. In fact, Bohr, too, appears to have been ambivalent on this point, which accounts for internal tensions and even ambiguities of several of his statements concerning this point in the Como lecture.

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* [not identically to!] *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 [$\Delta q \Delta p \cong h$] (generalizable to any canonically conjugate quantities whatsoever)" (Heisenberg 1927, p. 68; Heisenberg's emphasis). The disturbance introduced by measurement leads to the impossibility, reflected in the uncertainty relations, of establishing both quantities simultaneously. As a result, it becomes equally impossible to maintain the causal connections between (observed) quantum events in the way this is done in classical physics, where causality is possible because we can, ideally, define both variables at any point. This is why Bohr argued that, while the union of "the space-time co-ordination and the claim of causality" characterizes the classical theories, this union is no longer possible in quantum theory, where these two idealizations become complementary to and thus mutually exclusive with each other.

However, Bohr's claim concerning their complementary (specifically their mutually exclusive nature) makes his position different from that of Heisenberg in the uncertainty-relations paper. This is because, unlike Heisenberg, Bohr, at this stage, attributes causality to the undisturbed quantum behavior, at least to the quantum-mechanical models representing this behavior. Heisenberg does not speak of causality in referring to this behavior at this point of his argument, and speaks strongly against causality in quantum theory later in the paper. It is true that Heisenberg's formulation just cited technically allows one to assume causality at the quantum level, because he 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." However, even though he speaks of "disturbance," he does not say that such concepts can be defined simultaneously for an undisturbed system at any given point of time, as would be required in order to maintain causality. In closing his article, Heisenberg argues that "the presumption that behind the perceived statistical world [of quantum observations] there still hides a 'real' world in which causality holds" is "fruitless and senseless . . . speculation" (Heisenberg 1927, p. 83). Heisenberg rejects, just as Bohr does, causality at the level of observation in view of the uncertainty relations, which establish "the final failure of causality" (Heisenberg 1927, p. 83). This strong assessment, coupled to the statement, just cited, against the existence of a hidden causal *reality* behind the observed statistical world, suggests that Heisenberg rejects the view that causality applies at the quantum level. Indeed, he adds that "[quantum] physics ought to describe only the correlation of observations," rather than quantum processes themselves (Heisenberg 1927, p. 83).

One might argue that Heisenberg's claim concerning "the *final* failure of causality," echoed in Bohr's later appeal to "a final renunciation of the classical ideal of causality" (Bohr 1935, p. 697), is too strong. First, however, Heisenberg, as does Bohr later on, maintains only that this failure refers strictly to quantum processes and that causality still applies in the classical domain. Secondly, his claim is made under the assumption that quantum mechanics is correct. Heisenberg does say earlier in his paper, referring to Dirac's paper on the transformation theory (Dirac 1927a), that "one can say, if one will, with Dirac, that the statistics are brought in by our experiments" (Heisenberg 1927, p. 66). Heisenberg's meaning, however, appears to be different from that of Dirac. Heisenberg refers to the necessity of statistical considerations even in classical experiments, rather than, as Dirac does, to the causal nature of the independent quantum processes. Heisenberg's elaborations may be read as suggesting, along Kantian lines, that the ultimate constitution of nature may be beyond the representational capacity of human thought, and hence we cannot even know whether it is causal or not (Heisenberg 1930, p. 11). Dirac also maintained that the independent quantum processes could be considered causal in *The Principles of Quantum Mechanics* through all of its editions, including the last, the fourth, in 1958, revised in 1967 (Dirac 1967). Part of the reason for Heisenberg's position was his strong advocacy of matrix mechanics against Schrödinger's approach, which he disparaged more than Bohr and Dirac at the time. Heisenberg, Dirac, and Bohr equally saw the role of observation as irreducible in the constitution of quantum phenomena, as against Schrödinger's view. Nevertheless, Schrödinger's theory appears to be significantly responsible for bringing causality and, to begin with, the spatiotemporal representation of quantum processes back into quantum theory after the exile both suffered following Heisenberg's discovery of matrix mechanics. In his subsequent writings, influenced by Bohr's Como argument, Heisenberg, too, appears to assume that the undisturbed quantum behavior is causal, but still more ambiguously and, it appears, only in a certain mathematical sense, to be explained presently, rather than in any physical sense.

Indeed, the idealizations of both observation and definition invoked by Bohr are mathematical as well. The idealization of observation is that of (idealized) space or time measurements and their coordination. The idealization of definition is that of the mathematical formalism of quantum mechanics, as a realist and causal (idealized) representation of quantum processes in the absence of observation. If, however, the independent behavior of quantum objects is, as Bohr maintains, mutually exclusive with observation, it follows tautologically that this behavior is unobservable. Bohr says as much in the passage cited above: "On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense" (Bohr 1927, 1987, v. 1, p. 54). In what sense, then, could one speak of "the claim of causality" concerning this behavior, at least in physical terms? He also says that, "an independent reality in the ordinary physical sense can neither be ascribed to the phenomena [object?] nor to the agencies of observation" (Bohr 1927, 1987, v. 2, p. 54). If, however, an independent reality cannot be ascribed to the phenomena or at least objects, which, I think, would be more accurate (because the phenomena cannot, by definition, be independent of the agencies of observation, if one includes, as one must, our bodies among these agencies), one could hardly argue that causality could be ascribed to the objects' behavior.

One might speak of "mathematical causality," or "determination," whereby the equations of quantum mechanics determine a mathematical object, such as a wave function and the corresponding equation, Schrödinger's equation, at any time once it is known at a given time. As discussed in Chap. 2, however, unlike in classical mechanics, where the same type of mathematical determination holds as well, in quantum theory 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. What the mathematical determination defined by the mathematical model of quantum mechanics determines is, after each measurement, a statistical expectation-catalog for possible future measurements at subsequent points (Schrödinger 1935a, p. 154). Bohr, however, appears to imply a physical causality in his claim, without supplying the necessary mechanism for this kind of translation. For he says that, while 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 offer realist, representational and causal models of the physical processes they consider, ideally independent of observation. Hence, it appears that Bohr implies an analogously idealized physical causality of (undisturbed) quantum processes, even though they are now, unlike in classical physics, mutually exclusive with space–time coordination, defined by observation.

In sum, Bohr's argument for the possibility of the claim of causality concerning the independent (undisturbed) behavior of quantum objects, as complementary to space–time coordination, does not appear to be sustainable.[12] One might instead argue as follows, in accord with Bohr's subsequent view of the situation, and thus using "the post-Como Bohr" against "the Como Bohr." In classical mechanics, the role of measuring instruments could, in principle, be neglected or compensated for, as Bohr states at the outset of the Como lecture (Bohr 1927, 1987, v. 1, p. 53). Hence, in classical physics this behavior can be considered independently and happens to be causal, and the formalism of classical mechanics represents this behavior—at least in the case of idealized models. (We keep in mind qualifications made earlier concerning more complex classical systems, such as those considered in chaos and complexity theories, which qualifications are, however, practical rather than fundamental.) Bohr is thus right to speak of "the union . . . of the space–time coordination and the claim of causality . . . [that] characterizes the classical theories." In quantum physics, the independent behavior of quantum objects and our observations are mutually exclusive, in view of the irreducible "disturbance" affecting

[12] As noted earlier, the causal behavior of quantum objects is a rigorously established feature of some alternative interpretations of quantum mechanics, such as the many-worlds interpretation, or alternative theories of quantum phenomena, such as Bohmian mechanics. 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 or realist, as well as causal—again, at the cost of nonlocality. In the many-worlds interpretation the observational disturbance plays no role, and locality is preserved.

the behavior of quantum systems in any act of observation or measurement. Even the slightest possible observational "interference" (again, a better term than "disturbance," and Bohr came to use it as well), for example, by a single photon, would be sufficient, as Heisenberg explains in his uncertainty-relations paper (Heisenberg 1927, p. 65). It is this situation that compels Bohr to speak of the irreducible role of measuring instruments in the constitution of all observable quantum phenomena, which will ultimately define his concept of phenomenon. (As noted above, the Como lecture is not always precise as concerns the difference between objects and phenomena in quantum physics.) The consequence is that the independent behavior of quantum objects is unobservable. But then, again, on what grounds could one speak of this independent behavior as causal? And, in what sense could one claim that the formalism of quantum mechanics describes this independent behavior, especially given that Bohr insists on the symbolic character of this formalism, including in Schrödinger's version? A viable alternative claim, defining Bohr's later, RWR-principle-based, view, is that no claim of any kind could be made concerning the independent behavior or, it follows, independent properties of quantum objects. Bohr began to think along these lines after his discussion with Einstein in Brussels soon after he gave the Como lecture and before it was published, although it took another 10 years and more help from Einstein for Bohr to arrive at this interpretation.

The view that the independent behavior of quantum objects is causal is persistent and sometimes is advanced under the rubric of the Copenhagen interpretation, in part because of Bohr's Como argument and its influence. The Como lecture, however, is not the only or, I would argue, even the main reason for this persistence. As I said, this view itself appears to originate with Dirac, who advanced it in his important and influential 1927 paper "The Physical Interpretation of the Quantum Dynamics" (Dirac 1927a), which introduced the transformation theory, completed while in Bohr's institute in Copenhagen in 1926.[13] The paper, which proved to be instrumental for Dirac's work on his relativistic equation for the electron, had a major impact on Heisenberg's thinking and his paper introducing the uncertainty relations, where, however, Heisenberg, again, strongly argued against quantum-level causality. As I noted, both papers influenced Bohr's thinking at the time, as did Schrödinger's wave mechanics, keeping in mind that, while Schrödinger's program aimed at a causal theory of quantum processes, it was different. The concept of disturbance of quantum processes by observation played no role in his arguments, and he hoped that the behavior of quantum systems would, at least in principle, be accessible and even handled, ideally, deterministically. The *juxtaposition* of causality and observation, as disturbance, was only implied, although difficult to miss, in Dirac's paper, and appears to have been first expressly stated, in terms of complementarity, by Bohr in the Como lecture. Dirac adopts Bohr's Como language, while not mentioning complementarity (a concept that he tends to shun in general) in *The Principles of Quantum Mechanics*. He says: "[W]e must revise our ideas of causality. Causality applies only to a [quantum] system which is left undisturbed. If a

[13] The transformation theory was independently discovered by Jordan (Jordan 1926a, b).

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 systems and the equations 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 [formal] 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 observable results, the theory enabling us to calculate in general only the probability of our obtaining a particular result when we make an observation" (Dirac 1967, p. 4).[14] This is close to Bohr's Como argument and to the complementarity of space–time coordination and the claim of causality.

Heisenberg (in part as against his position in his uncertainty-relations paper, discussed above) in his Chicago lectures (Heisenberg 1930, pp. 64–65) and then von Neumann in his *Mathematical Foundations of Quantum Mechanics* (von Neumann 1932, pp. 417–418; Plotnitsky 2009, pp. 205–214) also adopted this view, probably following Bohr rather than Dirac. Given their prominence and impact, these works, especially von Neumann's book, might well have been primarily responsible for the persistence of the view under discussion here, more so than Bohr's Como lecture, rarely read, apart from Bohr scholars and historians, or Dirac's 1927 article, mostly known for the transformation theory.

As I said, Heisenberg's position on the subject retained a certain ambivalence concerning the situation. His argumentation also helps one to realize the difficulties of Bohr's Como argument, as just discussed. Heisenberg speaks of "causal relationships expressed by mathematical laws," rather than physical causality, which is difficult to assume because, as he also says, a "physical description of phenomena in space–time is impossible"—any physical description, let alone a causal one (Heisenberg 1930, pp. 64–65). This contention is not surprising, given that, as noted above, Heisenberg strongly argued against introducing physical causality into the quantum mechanical situation in his uncertainty-relations paper. As he also says: "There exists a body of exact mathematical laws, but these cannot be interpreted as expressing simple relationships between objects existing in space and time" (Heisenberg 1930, p. 64). Heisenberg's later statement 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 aware of this. One is, thus, again, compelled to ask: "What is, then, the meaning of causality under these conditions?

[14] It is not a matter of the "smallness" of a quantum system, which could be large, a macrosystem, but only of the "smallness" of its ultimate constituents, as Dirac was of course aware. Large quantum systems cannot be observed as quantum systems without using classically described measuring instruments and hence without them being "disturbed." I shall further comment on this point below.

Which relationships—and between which elements—are causal and are, as such, expressed by mathematical laws of quantum theory?" Heisenberg does not answer these questions any more than do Bohr (in the Como lecture), Dirac, or von Neumann. However, by posing this question—by, on the one hand, invoking "causal relations expressed by mathematical law" and, on the other, stating that these laws "cannot be interpreted as expressing simple relationships between objects existing in space and time"—Heisenberg invites the type of analysis undertaken here (Heisenberg 1930, pp. 64–65).

Bohr himself, helped by Einstein's criticism, came to realize the difficulties of sustaining this view and abandoned it, along with the complementarity of space–time coordination and the claim of causality, again, possibly even before, the Como lecture, never quite revised to Bohr's satisfaction, was published in 1928. He returns to his more Heisenbergian view of 1925, discussed in Chap. 2 (Bohr 1925, 1987, v. 2, pp. 48–51). This view was supported by instantiations of the concept of complementarity that replaced the complementarity of space–time coordination and the claim of causality. Arguably, the most significant among them, which also rigorously conform to the definition of complementarity given here, are those of space–time coordination and the application of momentum or energy conservation laws. There are two complementarities here: the first is that of the position and momentum measurements and the second is that of the time and the energy measurements. These complementarities are correlative to Heisenberg's uncertainty relations and establish Bohr's interpretation of the latter, and were implied or intimated in the Como lecture as well, but not expressly formulated there. They became central to Bohr's thinking, following his exchanges with Einstein in 1927–1928, which, as I said, were a decisive factor in changing Bohr's Como argument and in initiating the key aspects of Bohr's thinking eventually leading him to his ultimate views, crystallized by the late 1930s, under the impact of further exchanges with Einstein.

Bohr's new argument was first presented in print in his 1929 article "The Quantum of Action and the Description of Nature" (Bohr 1929a), Bohr's first published work on complementarity after his exchanges with Einstein and shaped by them, as Bohr noted later (Bohr 1949, 1987, v. 2, p. 52). The article represents a major step in the development of Bohr's thinking. Beginning with this article, Bohr's argumentation is firmly based on the complementarities of "the space-time description and the laws of conservation of energy and momentum," and correlatively, the uncertainty relations and the probabilistic or statistical character of quantum predictions, in turn correlative to each other (Bohr 1929a, 1987, v. 1, p. 94). These complementarities are also those between different experimental arrangements, in which the corresponding measurements and applications of physical concepts occur. While the corresponding complementary measurements are determinate, each complementarity is probabilistic or statistical, as concerns *predicting* the corresponding complementary phenomena. It is the complementarity of two alternative, mutually exclusive, expectation-catalogs, each defined by one of the two possible alternative complementary measurements already performed, while, at the same time, irrevocably precluding one from compiling the alternative expectation-catalog. Thus, if one measures the position of a given quantum object, one can form an expectation-catalog concerning future position measurements on this objects, while, by the same token, making it impossible to combine such a catalog concerning future momentum measurements concerning this object, and vice versa.

Bohr's new argument also enabled him to give a rigorous interpretation of Heisenberg's uncertainty relations. Technically, the uncertainty relations, $\Delta q \Delta p \cong h$ (where q is the coordinate, p is the momentum in the corresponding direction), only prohibit the simultaneous *exact* measurement of both variables, which is always possible, at least ideally and in principle, in classical physics, and allows one to maintain causality there. Even this statement needs further explication, however, and the physical meaning of the uncertainty relations is much deeper in Bohr's interpretation and a complex subject in its own right with a long history, which cannot be considered here, beyond what is pertinent to Bohr's thinking.[15] First of all, the uncertainty relations are not a manifestation of the limited accuracy of measuring instruments, because they would be valid even if we had perfect measuring instruments. In classical and quantum physics alike, one can only measure or predict each variable *within* the capacity of our measuring instruments, and in classical physics one can, in principle, measure both variables simultaneously within the capacity, and improve the accuracy of this measurement by improving this capacity, in principle indefinitely. The uncertainty relations preclude us from doing so for *both* variables regardless of this capacity in dealing with quantum objects. In Bohr's interpretation, the uncertainty relations make each type of measurement complementary to the other, in conformity with the definition of complementarity given above. In addition, one not only cannot measure both variables simultaneously but also cannot define them simultaneously, and moreover, one cannot ever define them alternatively for the same quantum object. We always need at least two objects to define both variables. That is, if, after determining, say, the position of the electron, emitted from a source, at time t_m after the emission, we want to determine the conjugate momentum, we need to repeat the same, identically prepared, emission (for example, from the same source) of another electron, and then measure its momentum after the same time t_m after the emission. The uncertainty relations apply to these two measurements on two different quantum objects, still with further qualifications explained below. As will be seen, this circumstance plays a key role in Bohr's argument in his reply to EPR (Bohr 1935).

Probabilistic or statistical considerations are unavoidable in considering both the uncertainty relations, which is commonly recognized, and complementarity, which is usually overlooked. This aspect of complementarity was not generally emphasized by Bohr either, although the probabilistic or statistical nature of quantum predictions is central to his interpretation, especially, again, in considering those concerning elementary individual quantum objects and processes. In classical physics, the behavior of the individual objects or small compound systems could be predicated ideally exactly and that of large compound systems probabilistically or

[15] See (Hilgevoord and Uffink 2014) for a useful, although somewhat one-sided survey. The uncertainty relations remain a subject of ongoing foundational discussions. Among the more illuminating technical contributions are (Ozawa 2003) and (Busch and Shilladay 2006).

statistically. By contrast, in quantum physics, our predictions are always probabilistic or statistical regardless which quantum objects these predictions concern. Consider the following interpretation of the uncertainty relations, given by A. Peres, but consistent with Bohr's view:

> 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 are identically prepared [in terms of the physical state of the measuring instruments]) and therefore these measurements cannot disturb each other in any way. The uncertainty relation [$\Delta x \Delta p \cong h$] only reflects the intrinsic randomness of the outcomes of [individual?] quantum tests. (Peres 1993, p. 93)

Randomness here may refer to the fact that we may not even be able to assign a probability to the outcome of an individual trial, but only to ascertain the statistics of many repeated trials, a point to which I shall return in the next chapter. Not everyone would accept Peres's claim that this is "the *only* correct interpretation" of the uncertainty relations or accept this interpretation itself. It suffices here, however, that this interpretation is consistent with the experimental evidence and with Bohr's views. As Peres also observes, "an uncertainty relation [such as $\Delta q \Delta p \cong h$] is not a statement about the accuracy of our measuring instruments. On the contrary, its derivation assumes the existence of *perfect* instruments" (Peres 1993, p. 93). Bohr corroborates this observation: "the statistical character of the uncertainty relations in no way originates from any failure of measurement to discriminate within a certain latitude between classically describable states of the object, but rather expresses an essential limitation of applicability of classical ideas to the analysis of quantum phenomena" (Bohr 1938, p. 100). Elsewhere Bohr offers a striking sentence, stressing that in question in the uncertainty relations is not the accuracy of measurement but complementarity, fundamentally linked to the irreducible difference between measuring instruments (or phenomena in Bohr's sense) and quantum objects in quantum physics. He says: "we are of course not concerned with a restriction as to the accuracy of measurement, 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, 1987, v. 3, p. 5; also Bohr 1937, p. 86, 1954b, 1987, v. 2, p. 73). The predictions involved will still be probabilistic or statistical, reflecting the probabilistic or statistical nature of the uncertainty relations and complementarity. A given measurement associated with either complementary variable will be (ideally) exact.

At this stage of Bohr's thinking (we are in 1929), he still assumes that it is possible to assign either one or the other conjugate quantity (never both because of the uncertainty relations) to a given quantum object, albeit only *at the time of measurement* and not independently. This assumption is no longer allowed in Bohr's ultimate interpretation, because quantum objects and their behavior are placed beyond any possible representation, even if not conception, by the RWR principle (Bohr 1949, 1987, v. 2, pp. 63–64). Even in 1929, however, causality no longer applies to our understanding of quantum processes (and, hence, cannot be part of any complementarity, as it was in the Como argument). On the other hand, as indicated in Chap. 2, the conservation laws can be maintained in quantum physics apart from causality, to which they are linked in classical physics. Bohr changed the term “complementarity” to “reciprocity” in his 1929 article under discussion, in part, it appears, because the response to the concept in the physics community (apart from a few of those who were close to Bohr) was not enthusiastic. Bohr, who came to see the change as a “blunder,” returned to the language of complementarity in his 1931 Bristol lecture, “Space-Time Continuity and Atomic Physics” (Bohr 1972–1996, v. 6, pp. 361–370). The lecture also uses the double-slit experiment, a staple of Bohr’s argumentation from this point on.

The language of *effects*, in the absence of causality, introduced in these two works, became ubiquitous in Bohr’s work. If “the wonderful development of the art of experimentation has enabled us to study the effects of individual atoms,” it has, Bohr argued, also limited us to the observation or descriptive study of these effects and only to them (Bohr 1929a, 1987, v. 1, p. 92). As quantum objects, atoms themselves, while they exist or are real, cannot, in principle be represented, any more than can be any quantum object, which could be macroscopic in scale, although its quantum nature would be defined by its ultimate microscopic constitution. This nature may be manifested in certain effects, as in the case of Josephson junctions, or may not be so manifested, as in the case of other macro-objects, from everyday objects to planets to stars to galaxies to the Universe itself. The quantum nature of the Universe was more manifested at its early stages, at the time of or before the Big Bang, the quantum effects of which we can observe by means of our measuring instruments from within the current state of the Universe. All these entities exist, are *real*. However, they require quantum experiments and the corresponding instruments to establish their quantum character. They can only be *observed* as classical objects, while, as quantum objects, they remain unobservable. Their quantum nature only manifests itself in certain effects that they produce in the appropriate measuring instruments by interacting with them, which interactions also allow us to distinguish such objects, even though each one of them cannot be represented in terms of their independent properties.[16]

The description of these effects is classical and can be treated as realist or objective, while no representation of quantum objects and their behavior, and hence of the efficacity of these effects, is, again, possible. As Bohr notes: “It lies in the nature of physical observation … that all experience must ultimately be expressed in terms of classical concepts, neglecting the quantum of action [h]. 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, 1987, v. 1, pp. 94–95). Bohr’s appeal to classical concepts in

[16] Carbon 60 fullerene molecules were observed as classical and as quantum objects (Arndt et al. 1999).

interpreting quantum phenomena is often misunderstood. While the subject requires a separate discussion, its key points may be easily stated here. First, this formulation makes clear the following point, 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. Elsewhere, Bohr speaks of "the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or 'individuality,' characterizing the elementary [quantum] processes" (Bohr 1949, 1987, v. 2, p. 34). Secondly, the *interaction* between quantum objects and measuring instruments is quantum (otherwise no measurement could take place); hence they are never subject to description in terms of classical physical or any other concepts, anymore than any quantum process is. In other words, the preparation of measuring instruments is classical and thus controllable, which enables us to repeat it, and this preparation itself need not involve h. The outcomes of measurements, under the impact of quantum objects, while observed by classical means, will involve h. By contrast, measurements related to classical objects do not involve h. "The Quantum of Action and the Description of Nature" and even Bohr's reply to EPR still allow 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 attribution can only apply to measuring instruments, onto which the physical application of the uncertainty relations is transferred, or, again, more accurately to classically describable parts of the instruments, which also have quantum strata through which they interact with quantum objects. This interaction is "irreversibly amplified" to the macroscopic, classical level, say, a spot left on a silver screen, a process that is akin to decoherence (e.g., Bohr 1954b, 1987, v. 2, p. 73; Bohr 1935, p. 701).

Another crucial contribution of "The Quantum of Action and the Description of Nature" is Bohr's analysis of the probabilistic or statistical character of quantum mechanics. This analysis is also significant because it establishes more firmly the architecture of complementary as a quantum-theoretical concept, defined jointly by its mathematical and its probabilistic or statistical dimensions. As stressed throughout this study, the irreducibly probabilistic or statistical nature of our predictions concerning quantum phenomena is an experimentally fact because the same experimental arrangements, which we can control in the manner of classical physics, in general yields different outcomes, which thus cannot be controlled (Bohr 1954b, 1987, v. 2, p. 73). Unlike in classical physics, we cannot, because of the uncertainty relations, asymptotically reduce this difference in the outcome of the same experiments by improving the precision of our measuring instruments, given that, as noted above, the uncertainty relations would apply even if we had perfect instruments. In Bohr's interpretation, this situation is a consequence of the irreducible role of measuring instruments in the formation of quantum phenomena. In "The Quantum of Action and the Description of Nature" Bohr argues that "in the observation of [quantum] phenomena, we cannot neglect the interaction between the objects and the instruments of observation. 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" (Bohr 1929a, 1987, v. 1, p. 93). However, we can reproduce and hence objectively ascertain the statistics of repeated experiments that are identically prepared (as concerns the state of the apparatus). His use of “statistical,” rather than “probabilistic,” is, again, noteworthy and implies that at stake are the statistics of repeated identically prepared trials. Elsewhere Bohr writes: “In this situation, there could be no question of attempting a causal analysis of radiative phenomena, but only, by a combined use of the *contrasting* [complementary] pictures, to estimate probabilities for the occurrence of the individual radiation processes” (Bohr 1949, 1987, v. 2, p. 34; emphasis added). This is one of the rare occasions on which Bohr refers to probabilities of the occurrence of individual quantum processes, rather than statistics of multiple experiments, perhaps because he refers to Einstein’s original analysis of the emission or absorption of photons (Einstein 1905a). Bohr reverts to “statistical considerations” (referring, admittedly, to “systems of great structural complexity,” considered in classical statistical physics) in the elaboration that immediately follows to state “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” (Bohr 1949, 1987, v. 2, p. 34; emphasis added). I shall consider this difference, central to Bohr’s argument, in Sect. 3.6, after the discussion of the EPR experiment to which I turn next and Bohr’s ultimate interpretation in Sect. 3.5.

3.4 The EPR Experiment: Complementarity, Correlations, and Locality

For half a century, beginning with Bell’s celebrated theorem, proven around 1964 (shortly after Bohr’s death in 1962), the debate concerning quantum foundations has shifted toward the questions concerning quantum correlations between spatially separated events, typically of the type observed in Bohm’s version of the EPR experiment (considered in Bell’s theorem), and quantum entanglement. These correlations can be ascertained experimentally and thus apart from quantum mechanics. Unlike the original thought-experiment proposed by EPR, which deals with continuous variables and cannot be performed in a laboratory, Bohm’s version of the EPR experiment, which deals with discrete variables, could and has been performed, confirming the existence of quantum correlations. On the other hand, quantum entanglement refers to “entangled states,” which are part of the mathematical formalism of quantum mechanics, enabling one to predict these correlations. The subject was initiated by the famous article of Einstein, Podolsky, and Rosen (EPR), which argued for the incompleteness or else the nonlocality of quantum mechanics (or of quantum phenomena), and by Bohr’s reply to it, both published under the same title, “Can Quantum-Mechanical Description of Physical Reality be Considered Complete?,” in 1935 (Einstein et al. 1935; Bohr 1935). Neither article used the terms “correlations” or “entanglement,” although EPR’s argument was based on an entangled state of two EPR particles, and Bohr in effect used the idea of quantum correlations and their statistical nature as part of his argument. The concept of entanglement was introduced, in German (*Verschränkung*) and in English, and given its prominence by Schrödinger in three papers, including his famous cat-paradox paper, all inspired by EPR's paper (Schrödinger 1935a, b, 1936). The language of correlations came into prominence later, more or less in the wake of Bell's theorem. Indeed, the subject was relatively dormant until Bell's theorem rekindled it, and it has remained at the center of the debate concerning quantum foundations ever since.

The center stage of this debate was gradually taken by the question of the locality of quantum phenomena or quantum mechanics, rather than of its completeness, which was primarily at stake in the Bohr-EPR exchange, although locality was important to this exchange as well. Obviously, the nonlocality of quantum phenomena implies that of quantum mechanics, insofar as it is a correct theory, but not the other way around, which, if quantum phenomena or nature are local, would imply that quantum mechanics, if nonlocal, is incorrect. The question of locality became even more central to Einstein's subsequent communications on the subject, compelling Einstein to use his famous expression "spooky action at a distance" [*spukhafte Fernwirkung*] (e.g., Born 2005, p. 155). However, because realism has remained a major concern, in particular given the lack of realism as a possible alternative to nonlocality, the question of completeness and of suitable criteria of completeness have remained germane to this debate. Bell's and the Kochen-Specker theorems, and related findings, along with sharpening the concept of locality, gave more rigorous sense to the concept of the completeness of quantum mechanics, and the difference between Einstein-completeness and Bohr-completeness. This difference was already at stake in the Bohr-EPR exchange, and missing it sometimes led to misreadings of Bohr's reply, including by Einstein, although Einstein's main misunderstanding consisted in interpreting Bohr's argument as allowing for nonlocality (Einstein 1949b, pp. 681–682). Some see the EPR correlations as implying a violation of locality by either quantum mechanics or by quantum phenomena themselves. EPR's argument and related arguments by Einstein just mentioned, again, contended that quantum mechanics is either incomplete or nonlocal, which Einstein saw as unacceptable, because he assumed that nature is local, in part because locality was a consequence of relativity, although, as I explained, a concept or principle locality is independent of relativity. Bohr contested Einstein's argumentation by offering an interpretation of quantum phenomena and quantum mechanics, centered on the joint role of measuring instruments in the constitution of quantum phenomena, including those of the EPR type, and the concept of complementarity. It was Bohr's analysis of this role, which, he argued, was underappreciated by EPR, that allowed Bohr to conclude that "quantum-mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands for completeness," again, Bohr-completeness (Bohr 1935, p. 696, 700n). It would, he further argued, do so without sacrificing locality, at least by virtue of the compatibility of Bohr's argument and, thus, his interpretation of quantum phenomena, including, crucially, those of the EPR type, and quantum mechanics with "all exigencies of relativity theory," which implies locality (Bohr 1935, 700n). Contrary to Einstein's reading of Bohr's reply (a reading that is not uncommon), Bohr thought nonlocality to be as unacceptable as did Einstein.

Bohr's interpretation in his reply is closer to that in "The Quantum Action and the Description of Nature," discussed in the preceding section, than to his ultimate interpretation, which was developed following his exchange with EPR and Einstein's subsequent communication on the subject. However, the essential logic of his reply, especially its argument for the irreducibly role of measuring instruments and complementarity in considering quantum phenomena, could be presented in Bohr's later terms as well. Bohr argued that, because of this joint role, quantum phenomena, including of the EPR type, disallow EPR's conception of physical reality and the corresponding criterion of physical reality they introduce, or at least, the unqualified *way* in which the criterion is used by EPR. Hence, rather than demonstrating a deficiency of quantum mechanics with respect to either completeness or locality, EPR's *argument* itself appears as insufficient in view of a more rigorous analysis (than that given by EPR) of the nature of quantum phenomena, including those considered in the EPR *experiment*. It is true that Bohr only argued for (along with locality) the Bohr-completeness of quantum mechanics, rather than for its Einstein-completeness, which requirement quantum mechanics expressly does not meet in Bohr's interpretation. However, this is all he needed to do in his reply. EPR did not argue that quantum mechanics was not Einstein-complete, but rather that, to return to Bohr's locution, "its predictions [did] not exhaust the possibilities of observation," which is to say, that it was not even Bohr-complete (Bohr 1949, 1987, v. 2, p. 57). In his later communications on the subject, Einstein acknowledged that the statistical predictions of quantum mechanics might be seen as exhausting the possibilities of observation, making quantum mechanics Bohr-complete. Einstein still found this to be unsatisfactory because he saw the Einstein-completeness, which also implies classical causality at the ultimate level, as a requirement for a fundamental theory.

Ironically, any classical-like Einstein-complete theory that would predict the EPR-type correlations appears to be nonlocal in view of Bell's theorem, the Kochen-Specker theorem, and related findings, which thus far deal with discrete variables. Among the most famous of them (indeed they are by now classic) are those of D. M. Greenberger, M. Horne, and A. Zeilinger and L. Hardy, and, from the experimental side, A. Aspect's experiments and related experimental work, such as that by A. Zeilinger and his group (Greenberger et al. 1989, 1990; Hardy 1993; Aspect et al. 1982).[17] The advent of quantum information theory during recent decades, in part stimulated by these findings, gave this problematic further prominence and significance. These findings, beginning, again, with Bell's and the Kochen-Specker theorems, or indeed the EPR experiment and Einstein's and Bohr's arguments con-

[17] I only cite some of the key earlier experiments. There have been numerous experiments performed since, some of them in order to question or to find loopholes in these and other experiments, seen as confirming Bell's theorem, a subject that requires a separate treatment. One ought to mention, however, recently announced loophole-free tests of Bell's theorem (Giustina et al. 2015; Hensen et al. 2015; Shalm et al. 2015), culminating a long-standing series of efforts.

cerning it, and their implications continue to be intensely debated. I shall bypass these debates here, given that my main concern is the role of the concept of complementarity, rarely invoked, as against the related but different concept of contextuality, and even more rarely appreciated in these debates, and often marginalized even in commentaries on Bohr's reply. I would argue, however, that what is at stake in these theorems are situations that, in Bohr's words in his reply to EPR, "the notion of *complementarity* is aimed at characterizing" (Bohr 1935, p. 700). The EPR-type correlations reflect or are given a physical meaning by a form of complementarity, which I call "the EPR complementarity." I shall now sketch EPR's argument and Bohr's reply, focusing on the role of complementarity in Bohr's reply.[18]

EPR base their argument on two 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*." This criterion as such does not necessarily imply the Einstein-completeness of the theory, although EPR appears to have had it in mind. The Bohr-completeness suffices, and quantum mechanics, Bohr argues against EPR's contention to the contrary, satisfies this criterion, in either the interpretation Bohr uses in his reply, which allows one to attribute a single physical property to a quantum object itself at the time of measurement, or in Bohr's ultimate interpretation, which disallows even this attribution. The question turns on what are actually available or even possible elements of reality, and whether quantum phenomena change what could in principle be considered as such elements and as reality. One answer is proposed by EPR by means of their second criterion as a *sufficient* criterion of reality: "*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*" (Einstein et al. 1935, p. 138). (Here a "system" refers to a physical object, classical or quantum.)[19] EPR's argument for the incompleteness of quantum mechanics in accordance with the first criterion depends on the assumption, which EPR consider to be self-evident, that the second criterion applies in quantum physics as it does in classical physics, that is, it applies to independent properties ("elements of reality") pertaining to quantum objects. Bohr counterargues that, while EPR's criterion of reality does unambiguously apply in classical physics, it acquires "an essential ambiguity" when it is applied without further qualifications (not offered by EPR) to quantum phenomena, which are, again, different from quantum objects, a difference in turn underappreciated by EPR (Bohr 1935, p. 697). Bohr, in his reply, shares with EPR the assumption that one of the two con-

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[18] I have considered the exchange on several previous occasions, most extensively in (Plotnitsky 2009, pp. 237–312). I revisit the subject here, first, because this discussion will allow the reader to see the key points of Bohr's argument without consulting that lengthy analysis. Secondly, the present analysis is different in several important respects, in particular in stressing the decisive role of complementarity in Bohr's argument. For the connections to contextuality, see (Plotnitsky 2016).

[19] In Bohr, the term "quantum system" sometimes, including at certain point of in his reply, refers not to a system consisting of one or several quantum objects but to a system consisting of a quantum object, such as an electron, and the measuring instrument involved, in their interaction with each other. I shall clarify Bohr's reference whenever necessary as I proceed.

jugate physical quantities could be assigned to a quantum object itself *by and at the time of measurement*, or even, at least ideally, by prediction, thus in accord with EPR's criteria of completeness. The "counterpart-ness" in question in this criterion becomes complicated, which eventually compelled Bohr to abandon the second assumption. Doing so does not affect his logic in his reply because he argues EPR's criterion of reality contains "an essential ambiguity" even under this assumption. In any event, Bohr does not reject this assumption but rather argues that it needs to be qualified, given the experimentally established nature of quantum phenomena, as defined by the role of the measuring instruments. That includes the EPR case, where it might appear to be possible to circumvent this role, but, Bohr argues, it actually is not possible to do so. It is EPR's failure to provide these qualifications that leads to an essential ambiguity in their criterion of physical reality. Once this ambiguity is removed by these qualifications, it can be shown that every element of physical reality that could, at least ideally or in principle (the EPR experiment is, again, an idealized thought-experiment), be ascertained for a well-defined quantum phenomenon does have a counterpart in quantum mechanics, *insofar as it could be predicted by quantum mechanics*. The probability of such predictions is not always unity, although sometimes, as in the EPR experiment, it is, again, ideally. In other words, quantum mechanics predicts as much as nature allows us to predict, at least as things stand now, and is thus as complete as possible, Bohr-complete, although not Einstein-complete. But then, as noted above, whatever their ultimate desiderata are in this regard, EPR argue that quantum mechanics is not even Bohr-complete, insofar as it fails to predict certain elements of reality that, they argue, could be definitively ascertained an EPR-type experiment. As also noted above, their criterion of completeness does not imply Einstein-completeness either. In other words, EPR's argument does not show or even argue that the conditions for Einstein-completeness of a possible alternative theory are in place by virtue of their argument, as Einstein, again, subsequently acknowledged, at least de facto, in view of the statistical considerations involved. EPR's original argument, however, to which Bohr responds in his reply, claims that quantum mechanics fails to predict what is in principle possible to predict and that is not even Bohr-complete. This claim is more troubling from where Bohr stands, because Bohr only requires that a physical theory needs to be Bohr-complete to conform to "the basic principles of science," and this is the case he aims to argue in his reply (Bohr 1935, p. 700).

The crux of the EPR argument is that the EPR (idealized) experiment allows for predictions with certainty concerning quantum objects without physically interfering with them by means of measurement, or, in EPR's language, "without in any way disturbing the system." This possibility would seem to circumvent the irreducible role of measuring instruments in the constitution of all quantum phenomena, specifically in making predictions concerning them. EPR clearly realized that, if a prediction concerning the future behavior of a given quantum system requires a measurement performed on this system, quantum mechanics would then predict all that is possible to predict in view of the uncertainty relations. But they did not realize that the uncertainty relations, which, they thought, their argument allows them de facto (although not in practice) to circumvent, would still affect the situation in the way that impairs their argument, even though the type of predictions they consider are possible. An EPR prediction concerning a given quantum object (using this term to designate any quantum object or system), say, S_2, of the EPR pair (S_1, S_2), is enabled by performing a measurement on another quantum object, S_1, with which, S_2, has previously been in interaction, but from which it is spatially separated at the time of the measurement on S_1. Specifically, once S_1 and S_2, forming an EPR pair (S_1, S_2), are separated, quantum mechanics allows one to simultaneously assign both the *distance between* the two objects and *the sum of their momenta*, because the corresponding Hilbert-space operators *commute*. With these quantities in hand, by *measuring* either the position or, conversely, the momentum of S_1, one can (ideally) *predict exactly* either the position or the momentum for S_2 without physically interfering with, "disturbing," S_2, which would, EPR assume, imply that one can simultaneously assign to S_2 both quantities as elements of reality pertaining to S_2. "The authors," Bohr summarizes in his reply, "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 [object], to attach definite values to both of two canonically conjugate variables, [EPR] consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed" (Bohr 1935, p. 696). It again follows, then, that the formalism would, according to EPR, not even be Bohr-complete, insofar as quantum mechanics does not predict all that it is possible to predict. The only alternative, as EPR saw it, would be the nonlocal nature of quantum phenomena, which quantum mechanics then describe as a complete (now even as Einstein-complete) theory (Einstein et al. 1935, p. 141). They disallow this possibility, as does Bohr, although this alternative has, as I said, been entertained, especially in the wake of Bohmian mechanics (which is nonlocal) and then the Bell and the Kochen-Specker theorems, and related findings mentioned above. These theorems and findings, again, concern discrete variables, for which, however, one could consider EPR's argument and Bohr's counterargument.

Bohr argued that the situation does not allow one to dispense with the role of measuring instruments, because this role entails limitations on the *types* of measuring arrangements used in determining the quantities in question, even if one does so in terms of prediction, including in the EPR case, rather than measurement. These limitations result from "*an influence on the very conditions which define the possible types of predictions, regarding the future behavior of the system* [S_2]" (Bohr 1935, p. 700). It is disregarding this influence, as EPR do, that gives EPR's criterion of reality its "essential ambiguity" when it applied to quantum phenomena, which ambiguity disables their argument. By contrast, by exposing the irreducible significance of this influence and by taking it into account, Bohr was able to argue that neither EPR's argument for the incompleteness of quantum mechanics nor their alternative reasoning that entails nonlocality could be sustained. Bohr's thinking concerning the situation eventually led him to his ultimate interpretation, in which *only what has already occurred* determines any physical quantity considered, and *not what has been predicted* (even with certainty) and is yet to be confirmed by a measurement. As I noted, however, unlike Bohr's later arguments, that of his reply assesses EPR's argument on their own terms, whereby it is possible to assign certain properties to quantum objects themselves under the constraints of the uncertainty relations, rather than in terms of Bohr's ultimate epistemology, where such an assignment is no longer possible even for a single physical quantity. Bohr's reply also assumes this assignment to be possible on the basis of *predictions* rather than only *measurements*, albeit only predictions that are *in principle verifiable*. This is, as will be seen, a crucial qualification, which, Bohr argues, is necessary in a rigorous analysis all quantum phenomena, those of the EPR-type included, but not considered by EPR.

It may appear that EPR's criterion of physical reality applies, *without any further qualification*, in quantum physics, just as it does in classical physics. For, it is only a joint *simultaneous* measurement or *simultaneous* prediction of two conjugate quantities considered by quantum mechanics that is impossible in view of the uncertainty relations. The value of a single variable can always be measured with any degree of precision, and hence considered to be ideally exact and be ideally exactly predicted in *certain circumstances*, even, as in the case of the EPR experiment, without any measurement being performed on the object in question. (In general, such predictions are not exact.) Bohr, as I said, provisionally accepts EPR's criterion of reality in his reply and faults EPR on the lack of certain qualifications necessary when one applies this criterion to quantum, as opposed to classical, phenomena. If one applies, as EPR do, the criterion without these qualifications, the incompleteness of quantum mechanics, or else nonlocality, would follow. For, given that after the prediction in question, say, concerning the position variable for S_2 is made by virtue of the measurement performed on S_1, one can perform an alternative measurement, that of the momentum variable on S_2. This would allow one to establish the value of *both* variables exactly at the time of this measurement, thus de facto circumventing the uncertainty relations. In fact, as Schrödinger appears to have been first to note, one could even simultaneously make alternative (complementary) measurements on S_1 , say, the position measurement, which determines its position, and S_2, the momentum measurement, which determines its momentum, and thus simultaneously predict (ideally exactly) the alternative second variable for each system, the momentum for S_1 and the position for S_2, thus, again, avoiding the uncertainty relations. This joint determination, however, is *not simultaneous* in the same location, because one of the two *predictions* in one location would need to be communicated with a finite speed (the fastest would be c) to the location of the other system. This lack of local simultaneity, which would also be required by the principle of locality, is important here as well. For, as will be seen, EPR admit that their argument would not work 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*," which *restricts* their criterion of reality (Einstein et al. 1935, p. 141). In EPR's view, this qualification, which makes their criterion more restrictive, is unacceptable, because it entails nonlocality. Once the criterion is assumed to apply in its original form in quantum mechanics, the latter may be assumed to be local but is incomplete. According to this logic, then, quantum mechanics is either incomplete or nonlocal. Bohr contends that neither EPR's argument for the incompleteness of quantum mechanics nor their alternative reasoning leading them to nonlocality is applicable. I shall, first, explain Bohr's reasoning in his critique of EPR's argument for the incompleteness of quantum mechanics and then consider the question of locality.

Both EPR and Bohr assume that the EPR experiment for (S_1, S_2) can be set in two alternative ways so as to predict (using quantum mechanics), with a probability equal to unity, either one or the other of the measurable quantities associated with these variables for S_2 on the basis of measuring the corresponding quantities for S_1. Let us call this assumption "*assumption A*."

On the basis of this assumption, EPR infer that *both* of these quantities can be assigned to the quantum object under investigation, S_2 , even though it is impossible to do so simultaneously (in view of the uncertainty relations for either one or the other measurement on S_1). This makes quantum mechanics incomplete because it has no mechanism for this assignment, unless, again, one allows for nonlocality, which is unacceptable (Einstein et al. 1935, p. 141). Let us call this inference "*inference E*" (for Einstein).

Bohr argues that, while *assumption A* is legitimate, *inference E* is unsustainable because a realization of the two situations necessary for the respective assignment of these quantities would involve two incompatible, in fact complementary, experimental arrangements and, thus, in effect *two different quantum objects* of the same type (e.g., electrons or photons). There is no physical situation in which this joint assignment is ever possible for *the same object*, either simultaneously (in view of the uncertainty relations) or even separately. If one makes the EPR prediction, with a probability equal to unity, for the second object, S_{12} , of a given EPR pair, (S_{11}, S_{12}), one would always need a different EPR pair (S_{21}, S_{22}) to get to "the last critical stage of the measuring procedure," performed on S_{21}, in order to make an alternative EPR prediction concerning S_{22} (Bohr 1935, p. 700). I designate this inference as "*inference B*" (for Bohr).

Nor is an identical assignment of the single quantity ever possible, or in any event, ever guaranteed, for two "identically" prepared *objects* in the way it can be in classical physics. This is because quantum experiments cannot be controlled so as to identically prepare quantum objects but only so as to identically prepare the measuring instruments involved, because this behavior is classical. The quantum strata of measuring instruments throughout which they interact with quantum objects do not affect these preparations but only the outcome of an actual measurement. On the other hand, this interaction itself is uncontrollable, which fact is central to Bohr's argument in his reply, which invokes this "finite and incontrollable interaction" at two key junctures of his argument (Bohr 1935, pp. 697, 700). It follows that the outcomes of repeated, identically prepared, experiments, including those of the EPR type, cannot be controlled either, even ideally or in principle (in the way it is in classical physics), and these outcomes will, in general, be different. This circumstance makes statistical considerations unavoidable in the EPR experiment, even though each actual predictions involved can be made with a probability equal to unity. This aspect of the situation does not appear to have been realized by EPR, whose *inference E* and their argumentation based in it in effect depends on the possibility of the identical repetition of the EPR experiment, which is precluded by *inference B*.

One can diagrammatically represent the situation as follows. 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 objects; and p is the probability of prediction, via the wave function, Ψ. Then:

The EPR experiment (EPR and Einstein’s view):

S_1	S_2
X_1	Ψ_1 (with $p=1$) $\rightarrow X_2$
Y_1	Ψ_2 (with $p=1$) $\rightarrow Y_2$

The EPR experiment (Bohr’s view):

S_{11}	S_{12}
X_{11}	Ψ_1 (with $p=1$) $\rightarrow X_{12}$
S_{21}	S_{22}
Y_{21}	Ψ_2 (with $p=1$) $\rightarrow Y_{22}$

This diagram is that of a complementarity, and it may well represent the instance of the concept that reflects its deepest aspects as a quantum-theoretical concept. As Bohr came to realize a bit later, standard quantum measurements are EPR-like insofar as the quantum objects involved in them are entangled with the apparatus (e.g., Bohr 1938, pp. 101–103, 1949, 1987, v. 2, p. 60; Plotnitsky 2009, pp. 263–265). As was indicated earlier and as will be explained in more detail presently, the probabilistic or statistical character of these predictions still plays a role in the EPR experiments, even though the EPR predictions are, ideally, predictions with certainty. One can especially appreciate the profundity of these connections because, while purely abstract or symbolic insofar as it does not describe quantum objects or for that matter anything at all, the quantum-mechanical formalism is nevertheless able to predict something as intricate as the EPR correlations. I shall call the complementarity depicted by the above diagram the EPR complementarity. The EPR complementarity expresses the very essence of complementarity, as Bohr came to realize, insofar as he saw the EPR experiment as an expression of the essence of complementarity (Bohr 1935, p. 700). The EPR complementarity can be described as a follows, thus also linking it to contextuality (Plotnitsky 2016).

Once one type of measurement (say, that of variable X) is performed on S_{11}, enabling the corresponding prediction on S_{12}, we irrevocably cut ourselves off from any future possibility of making the alternative, complementary, measurement (that of Y) on S_{11} and, thus, equally irrevocably, from the possibility of ever predicting the second variable for S_{12} (Bohr 1935, p. 700). There is simply no way to define that variable for S_{12}, except of course by a measurement, which, however, defeats the very purpose of EPR's argument. This could only be done on S_{22}, which is to say by preparing another EPR pair and performing a complementary measurement of Y on S_{21}, which will irrevocable prevent us from establishing X for S_{22}. This, again, may be the deepest meaning of complementarity, and the analogous complementary situation obtains in the case of the standard measurement, as Bohr's reply shows (Bohr 1935, pp. 697–699). As Bohr stressed in his reply, stemming from "our freedom of handling the measuring instruments, characteristic of the very idea of experiment" in all physics, classical or quantum (or of course relativity), our "free choice" concerning what kind of experiment we want to perform is essential to complementarity (part *(b)* of the main definition given earlier) (Bohr 1935, p. 699). The expression "free choice" or its equivalents, such as "freedom of choice," are repeated throughout in his reply (Bohr 1935, pp. 699–701). However, as against classical physics or relativity, implementing our decision concerning what we want to do will allow us to make only certain types of predictions and will exclude the possibility of certain other, *complementary*, types of predictions. In this sense, complementarity is, again, a generalization of causality, in the absence of classical causality and, in the first place, realism, because it defines what, which reality or at least which type of reality, can and cannot happen as a result of our decision concerning which experiment we perform. In the EPR case, according to Bohr:

> [By measuring the position of the first particle of a given EPR pair] we have by [the very procedure necessary to do so] 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 … any possibility of deducing from the behavior of this particle [its] position … relative to the rest of the apparatus, and have thus no basis whatever for predictions regarding the location of the other particle. (Bohr 1935, p. 700)[20]

Accordingly, it is only possible to establish both quantities for two EPR pairs, (S_{11}, S_{12}) and (S_{21}, S_{22}), never for one, and it is not possible to maintain that if we had predicted the second quantity, instead of the first one, for S_{12}, it would be the same, even ideally, as it is for S_{22}.

Bohr himself does not explain the situation in terms of two different objects and EPR pairs necessary in order to make the second EPR prediction, which might have helped to make his argument clearer. This, however, is at least an implication of his argument, given his insistence in his reply, as in the passage just cited, 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, 1987, v. 2, pp. 57, 60; also Bohr 1935, p. 699). In view of this mutual exclusivity, which is, again, due to the irreducible role of the measuring instruments, the second quantity in question cannot in principle be assigned to the *same quantum object*, *once one such quantity is assigned*. More accurately, one

[20] For a discussion of Bohr's "staging" of the EPR experiment, as a (double-slit) thought experiment, see (Plotnitsky 2009, pp. 294–301).

should speak of complementarity rather than mutual exclusivity, because we always have a freedom of choice in making another arrangement and thus measuring or predicting the other conjugate variable in question, which is, as I have stressed here, a defining aspect of complementarity as a concept or principle. This assignment is not possible even if one accepts EPR's criterion of reality, whereby such an assignment is made on the basis of a prediction. It is not possible once an experiment enabling one to make the first prediction is performed, because the first object S_1 (using the notation corresponding to EPR's view of the experiment) or S_{11} (using the notation corresponding to Bohr's view of the experiment) is no longer available. The simultaneous assignment of both is, again, precluded by the uncertainty relations, which is not only recognized by EPR but is also germane to their argument. They aim to show that the uncertainty relations could be circumvented by arguing that both variables could in fact *be assigned* to a given quantum object at any moment of time, even though only one of them could be measured or predicted. This is what leads them to reason that quantum mechanics is incomplete, or else nonlocal. Bohr, by contrast, sees the uncertainty relations as an insuperable *law of nature*, *reflected in quantum mechanics*, a law that disallows one to ever, under any circumstances, assign both quantities simultaneously to the same quantum object.

As the preceding discussion explains, it is never possible, on experimental grounds, to coordinate two experiments concerning quantities that are subject to the uncertainty relations on two different objects so as to make it possible to consider (in the way it can be done in classical mechanics) both as identically prepared *objects*. We can only control the identically preparation of the physical state of the observable parts of the measuring apparatus involved because this preparation and (physically) this state are classical, but not the state of the objects during the measurement and hence the outcome of the prediction we make on the basis of this measurement. In the EPR case, we can predict with the probability 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_{21}, of, unavoidably, another, "*identically prepared*," EPR pair (S_{21}, S_{22}). We cannot, however, coordinate these predictions in such a way that they could be considered as pertaining to two identically prepared objects in the way it 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 the outcome would, in general, not be the same as in the case of the first pair (S_{11}, S_{12}).[21] We can only coordinate such measurements and predictions statistically, and thus establish the EPR correlations (for continuous or discrete variables), as Bell realized. This does not help EPR, since their argument de facto presupposes exact rather than statistical

[21] That the ideal experiment proposed by EPR cannot be performed does not diminish this point, which, however, will be reflected in the actual experiments statistically approximating the EPR experiment. See note 22.

coordination of such variables as belonging to the same, or an *identically prepared*, object of the same, or an identically prepared, EPR pair. Masked as it may be by the predictions with a probability equal to unity involved in the EPR experiment, the probabilistic or statistical nature of quantum predictions and of complementarity is still essential.

The considerations just offered could, as I said, be transferred, with a few easy adjustments, to Bohm's version of the EPR experiment and spin variables, which can, again, be actually performed in a laboratory, unlike the experiment originally proposed by EPR.[22] In this case, too, there is the EPR complementarity (which corresponds to generalized uncertainty relations for spin-measurements) insofar as 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 consistent with the Bell–EPR correlations (as they are often called in this context), which are statistical (Mermin 1990, pp. 107–108).[23] The argument concerning locality, to which I now turn, could be similarly transferred to the case of discrete variables as well.

EPR acknowledged a possible loophole in their argument by admitting that they did not demonstrate that one could ever *simultaneously* ascertain both quantities in question for the same quantum object, such as S_2 in the EPR experiment, in the same location, either that of S_1 or S_2.[24] They, however, see this requirement as implying nonlocality in the EPR situation and hence as unreasonable. According to EPR:

> 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

[22] As indicated earlier, the actual experiment proposed by EPR, dealing with continuous variables, cannot be physically realized, because the EPR-entangled quantum state is not normalizable. This fact 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) that statistically approximate the idealized entangled state constructed by EPR for continuous variables. I cannot consider these experiments here, but they appear to be fully consistent with the present argument.

[23] See also the remainder of Mermin's excellent discussion, including his elegant proof of Bell's theorem (1990, pp. 110–176). For connections to contextuality, see, again, (Plotnitsky 2016).

[24] As noted earlier, courtesy of Schrödinger, but the point merits being repeated here, one could simultaneously make alternative (complementary) measurements on S_1, say, the position measurement, which determines its position, and S_2, the momentum measurement, which determines its momentum, and thus simultaneously predict (ideally exactly) the alternative second variable for each system, the momentum for S_1 and the position for S_2. This joint determination, however, is *not simultaneous* in the same location, and, thus, is in accord within EPR's initial criterion of reality, without the restriction in question. It is, again, also in accord with the locality principle.

> 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 the reality of P or Q depend upon the process of measurement carried out on the first system [the first object of the EPR pair considered, S_1], which does not disturb the second system in any way. No reasonable definition of reality could be expected to permit this. (Einstein et al. 1935, p. 141)

Nonlocality would indeed follow, if one assumed, as EPR do, that the measurement, say, of P, on S_1 *fixes the physical state itself* of S_2 by "a spooky *action* at a distance," rather than allows for what one might call "a spooky *prediction* at a distance," by fixing *the possible conditions* of such a prediction, as explained above. It follows, under the first assumption, that an alternative measurement of Q on S_2 would discontinuously change this fixed state, although EPR do not examine this last eventuality. Or, as Einstein argued on later occasions, 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 object or system, S_2, by a different "spooky action at a distance," defined by a different measurement performed on S_1 (Born 2005, p. 205).[25] This is why EPR contend that, if quantum mechanics *is* complete by their criterion, then the physical state of a system (object), here S_2, could be *determined* by a measurement on a spatially separate system (object), S_1, in violation of locality, while their criterion of *reality* no longer applies in its original form. If it is local, their main argument, based on their criterion of reality, shows (they believe) that it is incomplete. Einstein, as I noted, thought that Bohr accepted the alternative of locality vs. completeness, and retained completeness by allowing for nonlocality (Einstein 1949b, pp. 681–682). Einstein, however, misread Bohr's argument, which only allows for a spooky *prediction*, and *not action*, at a distance. According to Bohr, "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] argument [of his reply] and all exigencies of relativity theory" (Bohr 1935, p. 701n). This compatibility, which implies locality (although the latter is, as noted, is an independent concept), enables Bohr's critique of EPR's argument for the incompleteness of quantum mechanics, without sacrificing locality.

There is a difference between determining (fixing) the state of a physical object *by* a prediction and *possibly* doing so *on the basis* of a prediction. In Bohr's view, physical states of, at least, quantum objects cannot be seen as finally determined (even when we have predicted them exactly) unless either the actual measurement is made or unless the possibility of *verifying* the prediction is assured insofar as such a measurement could, in principle, still be performed so as to yield the predicted value. This last requirement in turn becomes a necessary qualification of EPR's criterion of reality in the case of quantum phenomena. This is because, if one assumes the validity of EPR's criterion in its original (unrestricted) form, the

[25] Einstein does note on the same occasion that the paradox is eliminated if quantum mechanics is only a statistical theory of ensembles and not of individual events, because, in this case, no single measurement of a given variable on S_1 or, more accurately, S_{1n} determines exactly the value of the corresponding variable on S_{2n} (e.g., Born 2005, p. 205).

measurement of the alternative quantity, Q, on S_2 would automatically disable any possible verification of the original prediction. It is crucial that it is always possible to perform this alternative measurement. This is one of the reasons why, as stated from the outset of this study, the assumption of the *independent existence or reality* of quantum objects or something in nature to which one can relate by using this term becomes especially important for Bohr's analysis of the EPR experiment and of the question of locality in it. This independent existence opens the possibility of this measurement. Once this alternative measurement is performed, the original prediction becomes meaningless as in principle unverifiable. This, again, implies that both quantities in question could never be experimentally ascertained, either simultaneously or separately, for the same quantum object and hence that quantum mechanics could not be shown to be nonlocal by EPR's logic here, anymore than it can be shown to be incomplete by their logic.

This is why Bohr argues that it is not a question of physical, "mechanical," influence of the measurement on S_1 upon the physical state of affairs concerning S_2, which is another indication that locality in general and the locality of quantum mechanics in particular are on his mind throughout. As he says: "of course there is in [the EPR] case no question of a mechanical disturbance of the system [object] under investigation during the last critical stage of the measuring procedure" (Bohr 1935, p. 700). Nevertheless, there is "an influence," missed by EPR, which implies that EPR's "criterion of physical reality … contains [if applied to the EPR experiment or quantum phenomena in general] an essential ambiguity as concerns [their] expression 'without in any way disturbing the system'" (Bohr 1935, p. 700). Contrary, again, to frequent misreadings (including, again, by Einstein) of Bohr's argument, *in which everything is physically local*, this is not 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, "spooky predictions at a distance," concerning the corresponding future measurement on S_2. Bohr makes this point very clearly: Even though there is no physical or mechanical disturbance, "even at this stage [when one makes a measurement on S_1 in order to make a prediction concerning S_2] 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* [S_2]" (Bohr 1935, p. 700). The influence in question is, thus, defined, *locally*, by *fixing* the conditions concerning the type of possible predictions concerning S_2 by making the corresponding measurement on S_1, which *in principle excludes* an alternative EPR measurement S_1 and thus the conditions necessary for making an alternative EPR prediction for S_2. This influence, thus, concerns the *local* conditions of the measurement on S_1 and, correspondingly, the prediction, the only possible prediction by virtue of this measurement, concerning S_2. ("Local" merits an excessive emphasis here.) It concerns the determination of one possible experimental set-up, as opposed to the other possible set-up, and it is never possible to combine both set-ups, which defines what I call the EPR complementarity. Once one of the two possible set-ups 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 *locally* (in the location of S_1) *influences* what kind of predictions concerning S_2 are possible, even though we do not interfere with it. Bohr, thus, clearly takes the locality requirement as axiomatic, just as Einstein does.[26]

EPR's argument is, Bohr argues, disabled by the nature of quantum phenomena, as defined by the irreducible role of measuring instruments in the constitution of these phenomena, and hence by the impossibility of considering 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 qualifications of this criterion required by these conditions, which is to say, the lack of taking into account the complementarity involved—the EPR complementarity. Hence, Bohr concludes in the elaboration, which in effect defines the EPR complementarity:

> From our point of view we now see that the wording of the above mentioned criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression "without in any way disturbing a system." Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete. On the contrary this description, as appears from the preceding discussion, may be characterized as a rational utilization of all possibilities, of unambiguous interpretation of measurements, compatible with the finite [quantum] and uncontrollable interaction between the object and the measuring instruments in the field of quantum theory. In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic principles of science. It is just this entirely new situation as regards the

[26] Einstein's subsequent arguments of the EPR type are sometimes viewed as offering a stronger or even different case by focusing more sharply on the question of locality (e.g., Einstein 1936; Born 2005, pp. 166–170, 204–205, 210–211; Einstein 1949a, pp. 77–85; and, in commenting on Bohr's reply, Einstein 1949b, pp. 681–682). I would argue, however, that, while these arguments may streamline EPR's argument, they still make their case in terms of the alternative between locality and completeness along the lines of EPR's argument. In other words, these arguments do not really make a different case. Indeed, Einstein never claimed otherwise. Instead he thought that these arguments bring into a sharper focus the essential features of EPR's argument. This may be so. The point remains, however, that these arguments 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. Some of these arguments add analyses of quantum mechanics as a theory of ensembles, but, as noted throughout, such a theory would not satisfy Einstein as a fundamental theory, which he expected to provide an (ideal) representation of elementary objects and processes. In other words, he requires such a theory to be Einstein-complete, rather than only Bohr-complete.

> description of physical phenomena that the notion of *complementarity* aims at characterizing. (Bohr 1935, p. 700; Bohr's emphasis)[27]

Bohr's claims 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" and this elaboration, often cited, including by Bohr himself (Bohr 1949, 1987, v. 2, p. 61), have posed difficulties for Bohr's readers and frequently caused confusion on their part. As noted at the outset of this chapter, Bohr acknowledged these difficulties and the main reason for them on the same occasion: "I am deeply aware if the inefficiency of expression which must have made it very difficult to bring out the essential ambiguity involved in a reference to physical attributes of [quantum] 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, 1987, v. 2, p. 61).[28] However, this elaboration and Bohr's meaning in this particular clause should pose no special difficulties given the preceding analysis. 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}. Only one of these quantities could be established for S_{12} without disturbing it, but once it is established, never the other quantity without disturbing it. 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 measurement of a complementary type on S_{21}. 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. The coordination of such events can only be statistical. We cannot establish both quantities for the same system *without in any way disturbing it*. The only way to establish the second quantity for this system would be to perform a measurement on it, which, however, would erase the determination of the first quantity, if one assumes, as the EPR do, that it could be made on the basis of a prediction on the first object of the corresponding EPR pair. This point, as we have seen, is also crucial for maintaining the locality of quantum mechanics. Thus, the ambiguity in question indeed relates to the clause "without in any way disturbing the system," which, if one wants to apply this clause rigorously in the EPR situation, requires qualifications explained in the present analysis but not provided by EPR. As Bohr notes earlier in his reply:

> 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 arbitrari-

[27] In this passage the term "system" is interchangeable with the term "quantum object," which is, as I indicated throughout my discussion, indeed the case pretty much throughout the exchange.

[28] For a close reading of this elaboration see (Plotnitsky 2009, pp. 301–306).

ness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. (Bohr 1935, p. 699; emphasis added)

Any attempt to apply both is ambiguous, and complementarity provides a necessary disambiguation, in correspondence with the uncertainty relations. As already explained, the physical meaning of the uncertainty relations and, thus, the use of Heisenberg’s formula itself, $\Delta q \Delta p \cong h$ (where q is the coordinate, p is the momentum in the corresponding direction), are adjusted by Bohr in accordance with this view. As he argued “Complementarity and Causality” (1937), echoing EPR’s argument and specifically both elaborations just considered, also “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, p. 86)

This elaboration nearly amounts to a counterargument to EPR on its own accord, although it is unlikely to have been possible without Bohr’s thinking through the situation in his reply to EPR.

Nor, as I explained, would EPR’s argumentation justify their conclusion that if quantum mechanics is complete it is nonlocal. Instead the EPR complementarity entails “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,” while allowing for the locality of quantum phenomena and quantum mechanics (Bohr 1935, pp. 697, 702). At least, such is the case as things stand now, and nothing has changed in this regard since 1935. The necessity of undertaking this revision leads Bohr to his ultimate interpretation, which, as I shall explain in the next section, defines quantum-level reality as a reality without the possibility of realism, in conformity with the RWR principle. Thus, as Bohr says, complementarity and the corresponding interpretation of quantum phenomena and quantum mechanics provide “room for new physical laws [those of quantum mechanics], the coexistence of which [with the basic principles of science might at first sight appear irreconcilable]” (Bohr 1935, p. 700). In other words, complementarity, in all of its three senses, allows one to see quantum mechanics as a complete (Bohr-complete) theory of quantum phenomena. As such it is fully in accord with “the basic principles of science,” including, importantly, the locality principle, unless one sees, as Einstein did, an (ideal) representation of elementary quantum process and thus the Einstein-completeness of a given fundamental theory as an equally basic principle. What complementarity failed to reconcile, then, was Bohr's and Einstein's views of what the basic principles of science are, by virtue of excluding, on Bohr's part, the necessity for a physical theory to be Einstein-complete.

The EPR complementarity expresses the very essence of complementarity as a quantum-theoretical concept, as Bohr came to realize in considering the EPR experiment, even if by way of a "reversal," insofar as he, as just discussed, saw the EPR experiment as an expression of the essence of complementarity. The EPR experiment and the EPR complementarity give us another, arguably deeper, illustration that quantum phenomena prevents us from ascertaining the complete composition of the "whole" from "parts" in considering quantum phenomena. These two cases, that of a standard quantum experiment and of the EPR experiment, are subtly connected in this regard as well. As I noted earlier, Bohr came to realize that any quantum measurement can be seen in terms of the EPR experiment by considering the quantum object under investigation and the quantum part of the measuring instrument, interacting with this object, as entangled, with the quantum part of the instrument serving as the second object of the EPR pair thus formed (Bohr 1938, pp. 101–104, 1949, 1987, v. 2, pp. 59–60). I have considered this aspect of quantum measurement elsewhere (Plotnitsky 2009, pp. 264–268). Here, I would like to bring in Schrödinger's famous discussion of the EPR experiment in his own response to EPR's paper.

Bohr does not fail to point out this aspect of the EPR experiment, as manifested in the formalism of quantum mechanics, in a note at the outset of his reply, the only explicit use of the quantum-mechanical formalism there via "transformation theorems" (Bohr 1935, pp. 697–698, n.). Schrödinger, however, makes the case more sharply, via the concept entanglement, which, as I said, he introduced, in both German and English, in the wake of and in response to EPR's paper in three papers he devoted to it (Schrödinger 1935a, b, Schrödinger 1936). As will be seen in Chap. 7, this feature of quantum phenomena is also crucial to deriving the finite-dimensional quantum theory from the principles of quantum information. According to Schrödinger:

> When two systems, of which we know the states by their respective representatives, enter into temporary physical interaction due to known forces between them, and when after a time of mutual influence the systems separate again, then they can no longer be described in the same way as before, viz. by endowing each of them with a representative of its own. I would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought. By the interaction the two representatives [the quantum states] have become entangled. Another way of expressing the peculiar situation is: the best possible knowledge of a *whole* does not necessarily include the best possible knowledge of all its *parts,* even though they may be entirely separate and therefore virtually capable of being 'best possibly known,' i.e., of possessing, each of them, a representative of its own. The lack of knowledge is by no means due to the interaction being insufficiently known—at least not in the way that it could possibly be known more completely—it is due to the interaction itself. (Schrödinger 1935b, p. 555)

This "peculiar" situation "haunts" (Schrödinger's own word) all three of his papers on entanglement. Schrödinger describes this situation in great detail, without ever missing its statistical dimensions (“expectation-catalogs”), even though he sees quantum mechanics, at least if interpreted in the spirit of Copenhagen, as “a doctrine born of distress” (Schrödinger 1935a, p. 154). His analysis helps us to better understand Bohr’s argument in his reply (although Schrödinger himself had difficulties with it) and the nature of the EPR complementarity, and thus, again, complementarity itself as a quantum-theoretical concept. Schrödinger’s point to the effect that “the lack of knowledge is by no means due to the interaction being insufficiently known—at least not in the way that it could possibly be known more completely—it is due to the interaction itself” is strictly in accord with Bohr’s view. Bohr gives this point a nonrealist interpretation (which Schrödinger resists) as concerns the ultimate character of this interaction, which is “finite [quantum] and uncontrollable” (Bohr 1935, pp. 697, 700). One must keep in mind that, as related to “questions about the future,” the “trait” in question is probabilistic or statistical (Schrödinger 1935a, p. 160). This is the only knowledge the wave function provides, by way of an expectation-catalog, and, when it comes to predictions, any complementarity, too, is a complementarity of expectation-catalogs. Complementary measurements give us definitive knowledge, but, again, never concerning the same quantum objects. In any event, the maximal expectation-catalog for the combined system does not contain the maximal expectation-catalog for each part. Speaking of the situation in this way is preferable to speaking of a whole and parts, because it is not clear in what sense, then, the combined system is the whole of its two parts. When we have a complete expectation-catalog (the maximal probabilistic or statistical knowledge concerning the outcomes of possible future experiments) for each of the two completely separate systems, then we also have such a catalog for both systems together. But the converse—if we have a complete catalogue for a combined system, then we have a complete catalogue for each separate system—is not necessarily true. Schrödinger explains this in terms of expectation-catalogs in his cat-paradox paper:

> This is the point. Whenever one has a complete expectation-catalog—a maximal total knowledge—of a ψ-function for two completely separate bodies, or, in better terms, for each of them singly, then one obviously has it also for the two bodies together, i.e., if one imagines that neither of them singly but rather the two bodies together make up the object of interest, of our questions about the future.
>
> But the converse is not true. *Maximal knowledge of a total system does not necessarily include total knowledge of all its parts, not even when these are fully separated from each other and at the moment are not influencing each other at all.* Thus it may be that some part of what one knows may pertain to relations or stipulations between the two subsystems (we shall limit ourselves to two), as follows: if a particular measurement on the first system yields *this* result, then a particular measurement on the second the valid expectation statistics are such and such, but if the measurement in question on the first system should have *that* result, then some other expectation holds for that on the second; should a third result occur for the first, then still another expectation applies to the second; and so on, in the manner of a complete disjunction of all possible measurement results which the one specifically contemplated measurement on the first system can yield. In this way, any measurement process at all or, what amounts to the same, any variable at all of the second system can be tied to the not yet known value of any variable at all on the first, and of course *vice versa* also. If that is the case, if such conditional statements occur in the combined catalog, *then it cannot possibly be maximal in regards to the individual systems*. For the content of two maximal individual catalogs would by itself suffice for a maximal combined catalog; the conditional statement could not be added on. (Schrödinger 1935a, p. 160)

I interrupt the quotation to note that the situation is clearly due to the complementary nature of the possible measurements and predictions involved, which prevents us from speaking of the whole of two complementary parts of these measurements or of expectation catalogs it provides, or, again, of a "whole" and "parts" altogether. Schrödinger continues:

> These conditional predictions, moreover, are not something that has suddenly fallen in here from the blue. They are in every expectation-catalog. If one knows the ψ-function and makes a particular measurement and this makes a particular result, then one again knows the ψ-function, *voilà tout*. It's just that for the case under discussion, because the combined system is supposed to consist of two fully separated parts, the matter stands out as a bit strange. For thus, it becomes meaningful to distinguish between measurements on the one and measurements of the other subsystem. This provides to each full title to a private maximal catalog; on the other hand it remains possible that a portion of the attainable combined knowledge is, so to say, squandered on conditional statements, that operate between the subsystems, so that the private expectancies are left unfulfilled—even though the combined catalog is maximal, that is even though the ψ-function of the combined system is known.
>
> Let us pause for a moment. This result in its abstractness actually says it all: Best possible knowledge of a whole does not necessarily include the same for its parts. … The whole is in a definite state [ψ-function], the parts taken individually are not. (Schrödinger 1935a, p. 161)

The statement that "the maximal knowledge of a total system does not necessarily include the total knowledge of its parts" is repeated, with minor variations, several times in the cat-paradox paper and its companion articles. The idea itself, which is "[not] *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought," again, literally "haunts" them: "best possible knowledge of the whole *does not* include best possible knowledge of its parts—and that is what keep coming back to *haunt* us" (Schrödinger 1935b, p. 555; also Schrödinger 1935a, p.166; emphasis added). This is the ghost of entanglement, the ghost of complementarity, the ghost of the quantum, the ghost of reality without realism.

3.5 Bohr's Ultimate Interpretation: Phenomena, Atomicity, and the RWR Principle

Bohr's ultimate interpretation was developed sometime in the late 1930s after his exchanges with Einstein concerning the EPR-type experiments (Einstein, again, offered several versions of his argument concerning them). This interpretation stabilized in the 1940s, with only minor modifications added later on.[29] It was, I argue, grounded in the concept of "reality without realism" and the RWR principle. To briefly recapitulate this concept, quantum objects exist, are *real*, and yet their nature and independent behavior cannot be represented, even ideally and in principle, by

[29] I have offered a full-fledged treatment of Bohr's interpretation in (Plotnitsky 2012a). On his ultimate interpretation, see pp. 137–157.

means of quantum mechanics, or possibly any theory, and might even be beyond conception. It is, accordingly, the reality of quantum objects that is responsible for the impossibility of realism in considering their independent nature and behavior. One can express this point in parallel or even correlatively to Schrödinger's assessment of the EPR situation cited above. According to Schrödinger, "the lack of knowledge [concerning each system of an EPR pair] is by no means due to the interaction [between them] being insufficiently known—at least not in the way that it could possibly be known more completely—it is due to the interaction itself" (Schrödinger 1935a, p. 155). One might equally say that the impossibility of a realist representation of quantum objects and behavior (the impossibility manifested in "the lack of knowledge" invoked by Schrödinger) is not due to this behavior being insufficiently known—at least not in the way it could possibly be known more completely—it is due to the nature of the reality of quantum objects and their behavior. Or, in Bohr's words, expressing the RWR principle, a more complete knowledge of this behavior is "*in principle* excluded" (Bohr 1949, 1987, v. 2, p. 62).

That this representation is not provided or is "*in principle* excluded," even and in particular in the case of elementary quantum processes, makes quantum mechanics Einstein-incomplete, as Einstein maintained. It is, however, Bohr-complete, which is all that Bohr claims for it, and, Bohr argues, it may also be interpreted as local. As I indicated earlier, Einstein ultimately accepted that, as a statistical theory of ensembles, quantum mechanics could be considered local, but he still found this unsatisfactory, given his insistence on Einstein completeness, which requires an ideally exact rather than probabilistic or statistical theory of elementary processes and events (e.g., Einstein 1936; Born 2005, pp. 166–170, 204–205, 210–211). The situation, as it related to Einstein's view and in general, requires further qualification as concerns the probabilistic or statistical nature of quantum mechanics as a theory of elementary processes and events, which I shall offer in Chap. 4. For the moment, the question is, again, whether nature would allow us or not to do more. Einstein thought it should. Bohr's view was that it *might not*, which, again, is not the same as to say that *it never will*. As Bohr noted, however, specifically having in mind Dirac's work and subsequent developments of quantum field theory, "such argumentation does of course not imply that, in atomic 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" (Bohr 1958, 1987, v. 3, p. 6).

The reason for this potential limitation concerning our knowledge or even conception of the ultimate architecture of nature is the irreducible role of measuring instruments in the constitution of quantum phenomena, including those associated with elementary quantum processes, as opposed to classical physics, where this role could in principle be disregarded or compensated for. As we have seen, this role was important for Bohr's thinking concerning the quantum-mechanical situation beginning with Heisenberg's discovery of quantum mechanics, but became especially significant in Bohr's reply to EPR and his subsequent arguments. This role, he argued, was irreducible even in predictions concerning a (distant) quantum object, one object of an EPR pair of objects, without previously performing a measurement on it, but by performing a measurement on another quantum object, the other object of the EPR pair. (The initial preparation of both objects would involve a measuring device.) Bohr's view of this role eventually led him to his concept of phenomenon, a concept introduced in the last 1930s (Bohr 1937, 1938), under the impact of his exchanges with Einstein concerning the EPR experiment, and Bohr based his ultimate interpretation in this concept. According to Bohr:

> 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 [describing the relevant observable parts measuring instruments]. (Bohr 1949, 1987, v. 2, p. 64; emphasis added)

As explained earlier, quantum-mechanical formalism is "symbolic" because while the theory uses mathematical symbols *analogous* to those used in classical mechanics, it does not, in Bohr's interpretation, refer these symbols to and, hence, does not represent, however ideally, the behavior of, quantum objects. Referring, phenomenologically, to "observations" or to *observed* physical situations, rather than, ontologically, to these situations themselves, explains Bohr's choice of the term "phenomenon." This choice is in accord with Kant's philosophy or, historically closer to Bohr, Husserl's phenomenology, specifically the phenomenology of consciousness, which, in Husserl, also involves a rigorous specification (overt or implicit) of the circumstances in which phenomena occurs. Of course, in Husserl, these circumstances are themselves phenomenological and do not involve the specifications of measuring instruments, which relate, ontologically, to their physical conditions, described by classical physics. This is an important difference, because phenomenally identical entities (such as a spot on a silver-bromide screen) must be considered as different depending on the set-up of the experiment (such as the double-slit experiment) that defines them. Consciousness is crucial, because our conscious experience is necessary to have phenomena in Bohr's sense as registered phenomena (it is our consciousness that register them with the help of measuring instruments) and to guarantee that we can unambiguously communicate our knowledge concerning them. Otherwise, no quantum physics or any physics, or science, would be possible. At the same time, quantum physics places insurmountable limits on how far our consciousness or thinking in general (and hence, also our unconscious) can reach. Because the observed parts of the measuring arrangement are, as objects, described classically, phenomena and, physically, measuring instruments, as objects (not quantum objects!), could be considered as identical to each other, as they are in classical physics. By contrast, the *emergence* of these phenomena, which is due to the interaction between measuring instruments and *quantum objects*, and quantum objects and processes themselves are no longer available to a representation by means of quantum mechanics or otherwise, and are thus different from phenomena.

This difference is, again, beyond Kant's difference between phenomena (representations) and things-in-themselves (objects), which allows for realism. It is also beyond Husserl's phenomenology, or even Heidegger's ontology, which emphasized the difference between (phenomenally experienced) entities, "things," and processes (Being-becoming) that bring them about, to which the concept of the emergence of quantum phenomena from the interactions between quantum objects and measuring instruments is closer, but is more radical. In Heidegger, this process is still available to thought, as things-in-themselves are in Kant. The difference between Heidegger and Kant is Heidegger's emphasis, which is closer to Hegel than to Kant, on the process of becoming and emergence. In this respect (there are major differences in other respects) Heidegger is closer to the conjunction of "process and reality" found in the work of his contemporary Whitehead (Whitehead 1929). In Bohr, the *reality* of quantum objects and *processes*, including those that lead to the *emergence* of physical situations manifest in quantum experiments and observed as phenomena in his sense, are absolutely disconnected from and are beyond phenomenal representation, even if not conception. In this sense, terms like "object" and "process" are provisional and ultimately inapplicable any more than any other terms—things-in-themselves (Kant), Being (Heidegger), Geist (Hegel), *noesis* (Husserl), or process (Whitehead). This makes Bohr's conception of reality and his epistemology more radical than those of these thinkers, and arguably the thinking of Western philosophy in general, except for Nietzsche and some of his twentieth-century followers, such as J. Derrida (Plotnitsky 1994). In particular, none of these conceptions involve the probabilistic or statistical, or mathematical aspects, found in Bohr's scheme.

Part of Bohr's concept of phenomenon is that this concept, in conformity to the RWR principle, rigorously precludes any representation of quantum objects and their behavior, which implies the irreducible difference, again, beyond Kant, between quantum phenomena and quantum objects. Bohr explains the situation already in his reply to EPR, which contains nearly all of the necessary ingredients for his concept of phenomenon:

> 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*. It is true that the place within each measuring procedure where this discrimination is made is in both cases largely a matter of convenience. While, however, 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, its fundamental importance in quantum theory, as we have seen, has its root in the indispensable use of classical concepts in the interpretation of all proper measurements, even though the classical theories do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics. In accordance with this situation there can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow us to predict the results to be obtained by a given experimental arrangement described in a totally classical way, and which have found their general

> expression in the transformations theorems, already referred to [pp. 697–697n.] (Bohr 1935, p. 701)

The situation is also connected to 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 reason, also known as the Heisenberg or the Heisenberg-von Neumann cut. Bohr's statement may suggest that, while observable parts of measuring instruments are described by means of classical physics, the independent behavior of quantum objects is described or represented by means of quantum-mechanical formalism. As discussed earlier, while this type of view was adopted by Bohr in the Como lecture, and then by others, such as Dirac and von Neumann, or more ambivalently, Heisenberg, it was not Bohr's view after he revised the Como argument. Bohr does say here that observable parts of measuring instruments are described by means of classical physics, again, with a crucial qualification that this description only concerns these observable parts (a qualification usually missed by commentators). However, he does not say, and does not mean (there is no evidence to conclude otherwise) that the independent behavior of quantum objects is described by quantum-mechanical formalism, the "symbols" of which are assumed here, as elsewhere in Bohr, to have only a probabilistically or statistically predictive role, by the QP/QS principle.

While "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" (emphasis added), it is true only largely, but not completely. This is why Bohr brings into consideration the transformation theorems, which, as he said at the outset of his reply, "perhaps more than any other feature of the formalism contribute to secure its mathematical completeness and its correspondence [in the sense of the correspondence principle] with classical mechanics" (Bohr 1935, p. 696n.). At the juncture of his reply under discussion at the moment, Bohr 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 1935, p. 701)

In other words, quantum objects are always on the other side of the "cut" and may even be defined accordingly—as that which is always, in any possible experiment, on the other side of the cut. 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 observed in quantum experiments 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. By contrast, at the other end, quantum objects and processes can never be isolated, either materially (from the measurement process and measuring instruments) or phenomenally (in the usual sense) insofar as we cannot, even in principle, represent what actually happens at that level or how, in accordance with the RWR principle. They can only be seen as cut off from our thought, in the epistemological sense of being inaccessible to a representation or possibly conception. This is why Bohr speaks of “the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or ‘individuality,’ characterizing the elementary processes” (Bohr 1949, 1987, v. 2, p. 34). Complementarity (now in the sense of Bohr’s interpretation) allows one to do so and, thus, to deal with “regularities beyond the grasp of [classical] description,” but at the cost of abandoning all realism in considering quantum objects and their behavior, which is to say, by adopting the RWR principle (Bohr 1949, 1987, v. 2, p. 41). Bohr’s use of “regularities” is worth noting here because it connotes the possibility of establishing laws (probabilistic or statistical as they might or must be in the case of quantum phenomena) governing these regularities.

This argumentation is 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 (Heisenberg 1935), which was written before Bohr’s reply and arguably influenced the latter, especially as concerns the features just considered, features that ultimately led Bohr to his concept of phenomenon.

Defined by “the observations [already] *obtained* under specified circumstances,” phenomena in Bohr sense refer only to already *registered* phenomena, phenomena that already happened, rather than could be *predicted*. For one thing, such predictions are, *in general*, probabilistic and, hence, what will happen can never be assured, unlike, ideally, in classical mechanics. In some experiments, such those of the EPR type, such predictions could be seen as ideally “determinate,” as Bohr noted in defining the concept of phenomenon in the passage above, but still only ideally. Ultimately, all quantum predictions, by whatever means, are, in general, probabilistic or statistical, in accordance with the QP/QS principle. The fact itself that a phenomenon in Bohr’s sense, defined by what is observed in measuring instruments, only refers to what has already occurred rather than to what is predicted is linked to his analysis to the EPR experiment, as considered in Sect. 3.3. Indeed, the concept itself appears to have emerged as a result of this analysis. Thus, the phenomena associated with the measurement, say, at the final stage of the sequence of experimental procedures involving an EPR pair (S_1, S_2), on S_1 say, that of the position of S_1, is the phenomena defined by the outcome of this measurement and only by this outcome. It does not included anything associated with S_2, even though it predicts, ideally with the probability one, the outcome of the determinate measurement on S_2 and thus is completely defined locally in the location of S_1. Any phenomenon associable with S_2 must involve one or another measurement on S_2, even if one assumes, as Bohr did in his reply, that one could define the measurable quantity in question, as associated with S_2, by a verifiable (by means of a measurement, which defines a phenomenon) prediction with the probability one. Otherwise, nonlocality becomes indeed possible, as EPR argue. This assumption is no longer held by Bohr in his ultimate interpretation, where any measurable quantity can only be defined by the corresponding phenomenon and hence only by an actual measurement.

Bohr's concept of phenomenon includes a rigorous specification of each arrangement, determined by the type of measurement or prediction we want to make, which specification implies that each phenomenon corresponds to what is observed in a single experiment, and only in a single experiment. This is necessary for the following reason. As explained earlier, that we always have a free choice as concerns what kind of experiment we want to perform defines the very idea of experiment in all physics. The situation is different in quantum physics because, implementing our decision concerning what we want to do will allow us to make only certain types of prediction (for example, that concerning future position measurements) and will unavoidably exclude the possibility of certain other, *complementary*, types of prediction (in this case, those concerning future momentum measurements). A rigorous specification of each experimental arrangement defines each phenomenon and the complementary nature of some of these phenomena.

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 as the same entity. It would appear as the same regardless of the difference in the physical conditions and, hence, the outcome or rather the outcome, "interference" or "no interference," of the experiment, or more accurately the *outcomes* (plural) because these two patterns are defined collectively by the two complementary setups of the experiment. The first is that with both slits open and no counters, which would allow us to know through which slit each quantum object passed, and the second is that when such a knowledge is possible in one way or another (for example, by using counters), even in principle rather than only actually. According to Bohr's understanding, however, each mark is (contextualized as) 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 all circumstances. While, thus, a given single event does not allow one to establish in which setting it had occurred, the statistical distribution of the traces on the screen will always be different in these two setups. That the difference between two sets of outcomes is manifested only collectively over a large number of trials is important in interpreting the nature of probability assignments or, as in the statistical Copenhagen interpretation presented in Chap. 4, the impossibility thereof of predicting the outcome of each individual experiment.

All quantum phenomena are, then, unique and unrepeatable, and are always discrete relative to each other, thus manifesting the QD principle in terms of Bohr's concept of phenomenon.[30] Not all individual phenomena may be as essential as those involved in complementary situations, say, those, correlative to the uncertainty relations, which gives such situations a special role. The irreducible individuality of each quantum phenomenon is, however, essential in all situations, and as such, along with the irreducibly probabilistic nature of our predictions, is *prior* to complementarity. That some of these phenomena are complementary or, correlatively, subject to the uncertainty relations remains crucial, again, in particular in the EPR-type situations.

The concept of phenomenon, again, entails the concept of quantum objects or of the *reality* of quantum objects, as beyond a representation by means of quantum mechanics or possibly any other theory, even if not any possible conception (the most radical form of the RWR principle, which is assumed in this study). In other words, the concept of phenomenon is correlative to the RWR principle, in a rigorous conjunction with both the QD principle and the QP/QS principle. Physical quantities obtained in quantum measurements and defining the physical behavior of certain (classically described) parts of measuring instruments, are *effects* of the interactions between quantum objects and measuring instruments. But, in this interpretation, these properties are no longer assumed to correspond, even in principle, to any properties, "elements of reality," pertaining to quantum objects, even any single such property, rather than only certain joint 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 less radical view, implies that the physical state of an object cannot be defined on the model of classical physics, which requires an unambiguous determination of both conjugate quantities for a given object at any moment of time and independently of measurement, which is no longer possible in quantum physics because the uncertainty relations. This fact is crucial to all of Bohr's arguments concerning quantum mechanics and complementarity, which reflect and arise from this impossibility, even under his earlier assumption that certain properties could be assigned to a quantum object at the time of measurement. However, in Bohr's ultimate interpretation, as based in the RWR principle, an attribution *even of a single property* to any quantum object as such is *never possible—before, during, or after measurement*. The conditions that experimentally obtain in quantum experiments only allow one to rigorously specify measurable quantities that can, in principle, physically pertain to measuring instruments and only to them. Even when we do not want to know the momentum or energy of a given quantum object and thus need not worry about the uncertainty relations,

[30] It is perhaps this situation that, at least in part, compels (Ungar and Smolin 2014) to argue that each event is subject to a different law, which means that laws of physics change, as subject to a multiple historical evolution of the universe. Ungar and Smolin, however, also assume classical causality of each such law. From the present perspective individual quantum events are only subject to probabilistic estimates under general laws of quantum mechanics or quantum field theory, laws equally applied to all quantum events. In the statistical Copenhagen interpretation, discussed in Chap. 4, individual quantum events are beyond even the probabilistic laws of quantum mechanics, which are strictly statistical and thus only concern multiple quantum events.

neither the exact *position* of this object itself nor the actual time at which this "position" is established is ever available and hence in any way verifiable. These properties, even assuming they could be defined (as they can be in some interpretations of quantum mechanics or in Bohmian theories), are lost in "the finite [quantum] and uncontrollable interaction" between quantum objects and measuring instruments (Bohr 1935, p. 697). However, this interaction leaves a mark in measuring instruments, which can be treated as a part of a permanent, objective record, which can be discussed, communicated, and so forth. The uncertainty relations, which are, again, statistical, remain valid, of course, and pertain to measurements and predictions alike. But they now apply to the corresponding (classical) variables of suitably prepared measuring instruments, impacted by quantum objects. We can either prepare our instruments so as to measure a change of momentum of certain parts of those instruments or so as to locate a spot impacted by a quantum object, but never do both together. This makes the uncertainty relations correlative to the complementary nature of these arrangements, almost replaces them with complementarity.

As indicated in Chap. 2, this view is fully consistent with the concept of elementary particles, some of the properties of which might appear to be ascertainable as independent of or invariant relative to measurement, by virtue of their apparently permanent nature. First of all, the fact, crucial to all quantum theory, that elementary particles cannot be distinguished from each other within each of their types (electrons, photons, quarks, and so forth), while these types themselves are rigorously distinguishable from each other, is consistent with the view that the nature of elementary particles and their behavior are beyond representation or even conception. Both features are consistently defined by the corresponding sets of effects manifested in measuring instruments, effects from which we infer the existences of elementary particles, in the first place. Some of these effects, such as those of mass or charge, are classical-like and others have no classical counterpart, such as spin, although such effects, too, are only inferred from certain particular classical effects manifested in measuring instruments, and are not representable as such. As noted in Chap. 1, spin is a good example of this impossibility of representing the quantum-level efficacy of such effects and, in this case, even conceiving of this efficacy. An elementary particle of a given type, say, an electron, is defined by a given set (potentially very large, but specific to each type) of possible phenomena or events observable, as effects, in measuring instruments associated with it. This set is the same for all electrons, although the correlation between any such phenomenon and any given electron can never be strictly (rather than statistically) assured, which reflects the fact that electrons are indistinguishable from one another. To cite H. Weyl, "the possibility that one of the identical twins Mike and Ike is in the quantum state E1 and the other in the quantum state E2 does not include two differentiable cases which are permuted on permuting Mike and Ike; it is impossible for either of these individuals to retain his identity so that one of them will always be able to say 'I'm Mike' and the other 'I'm Ike.' Even in principle one cannot demand an alibi of an electron!" (Weyl 1931, p. 241).[31] One cannot be certain that one encounters the same electron, say, in an experiment designed to detect it after it was emitted by a source, even in quantum-mechanical regimes, although the probability that it would be a different electron is low. In high-energy regimes, governed by quantum electrodynamics or quantum field theory, speaking of the "same" electron detected in the course of a given experiment nearly loses its meaning. Two electrons could be distinguished by a changeable property associated with them (but, in the present view, manifested only in measuring instruments), such as their positions in space or time, momentums, energy, or the directions of spins. Such variables may be subject to the uncertainty relations and complementarity. It is possible, for example, to simultaneously locate two different electrons in separate regions in space. It is not possible, however, to distinguish electrons from each other on the basis of their (rest) mass, charge, or the value of their spin. These quantities are not subject to the uncertainty relations or complementarity, although the *direction* of spin is. However, in the present interpretation, such properties, too, could only be associated with electrons or other particles by the corresponding set of phenomena observed in measuring instruments but not rigorously attributable to these objects.[32]

It follows, then, that nothing about quantum objects themselves could ever be extracted from phenomena. This situation defines what Bohr calls the wholeness or indivisibility of phenomena, which makes them "closed": the records of the actual quantum behavior that led to their emergence are sealed with them and cannot be unsealed. As he says:

> 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. … 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. … the quantum-mechanical formalism permits a well-defined application referring only to such closed phenomena. (Bohr 1954b, 1987, v. 2, pp. 72–73; also Bohr 1949, 1987, v. 2, p. 51)

This situation also led Bohr to his associated concept of "atomicity," in the original Greek sense of an entity that is not divisible any further, which, however, now

[31] The statement is cited in (French 2014). See also (Jaeger 2013), for a comprehensive realist treatment of the subject from a realist perspective.

[32] It might be added that Bohr's views concerning this subject has been often misunderstood, as they were, for example, in (Ulfbeck and [Aage] Bohr 2001; [Aage] Bohr et al. 2004), which target the use of the idea of particles, including, in my view mistakenly, by (Niels) Bohr. In particular, they disregard Bohr's argument that the concept of particle 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. Particles qua particles disappear in Bohr's and other RWR-principle-based interpretations—only Bohr's "technological atoms" as phenomena in his sense remain. The term "particle" was sometimes used by Bohr for the sake of convenience and economy of discourse, or in dealing with arguments, such as that by EPR, that appeal to the concept.

applies at the level of phenomena, rather than referring, along Democritean lines, to indivisible physical entities, "atoms," of nature. Bohr's rethinking of the quantum-mechanical situation in terms of phenomena in his sense 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. In part recasting his concept of the quantum postulate in the Como lecture, both concepts, phenomenon and atomicity, emerged in Bohr's work at about the same time (1937–1938) 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," although Planck's quanta, too, were originally understood on these classical lines as quanta of energy (Bohr 1938, p. 38; also Bohr 1949, 1987, v. 2, p. 33, Bohr 1958, 1987, v. 3, p. 2). As is Bohr's concept of phenomenon, the concept of "atomicity" is defined in terms of *individual* effects of quantum objects on the classical world, as opposed to Democritean atoms of matter itself. "Atomicity" in Bohr's sense refers to physically complex and hence *physically* subdivisible entities, and no longer to single physical entities, whether quantum objects themselves or even point-like traces of physical events. In other words, these "atoms" are individual phenomena in Bohr's sense, rather than indivisible atomic quantum objects, to which one can no longer ascribe atomic physical properties any more than any other properties.[33]

Any attempt to "open" or "cut through" a phenomenon (this would require a different experiment, and hence one is never really cutting through the same phenomenon, which confirms the uniqueness of each) can only produce yet another closed individual phenomenon, a different "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 at 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, 1987, v. 2, p. 40). "Consequently," he adds in the statement cited earlier, also redefining complementarity in terms of phenomena, "the evidence obtained under different experimental conditions cannot be comprehended within a

[33] Bohr's conception of atomicity has rarely been examined. Among very few exceptions are Folse (Folse 1985, 1987) and Stapp (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 an argument that quantum mechanics is nonlocal, which is, as I argue here, difficult to sustain. Folse's more recent commentaries no longer invoke nonlocality, however (Folse 2002, p. 93). The philosophical genealogy of this concept is not easy to trace, although one can find a few parallels, in particular Whitehead's conception of atomicity ("drops of experience") in *Process and Reality* (Whitehead 1929), a parallel discussed in (Stapp 2007). It is, however, unclear whether Whitehead's ideas were familiar to Bohr or had impact on his thought, although they might have been. See also (Epperson 2012) on the connections between Whitehead's philosophy and quantum theory, although Epperson's book, I would argue, misrepresents Bohr's views.

single picture, but must be regarded as *complementary* in the sense that only the totality of the phenomena [in his sense] exhaust the possible information about the objects" (Bohr 1949, 1987, v. 2, p. 40). As a result, again, only probabilistic or statistical estimates, concerning the outcomes of experiments can be given: it is, in principle, impossible to reach the level of objects themselves, where one could encounter anything causal. That is why the situation simultaneously entails "the impossibility of subdividing quantum phenomena and reveal[s] the ambiguity of ascribing customary physical attributes to atomic objects" (Bohr 1949, 1987, v. 2, p. 51).

Each phenomenon is *individual*, each—every (knowable) effect conjoined with every (unknowable and even unthinkable) process of its emergence—unique and unrepeatable. Some phenomena 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, this view allows one to define and identify such entities, including, as explained above, elementary particles. Thus, along with quantum atomicity as *indivisibility,* 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 in the sense of being 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, there is yet another form of quantum *discontinuity*, that defined as the inaccessibility of quantum objects themselves, the impossibility of applying either of these concepts (continuity or discontinuity) or any conceivable concept to them or their relation to the manifested effects of their interaction with measuring instruments.

Bohr's concepts of phenomenon and atomicity have important implications for the instantiations of the concept of complementarity. As explained, one never deals with complementary properties of quantum objects themselves or their independent behavior, given that, by the RWR principle, no attribution of any such properties, single or joint, is ever possible. Indeed, as discussed in Sect. 3.4, the EPR experiment tells us that no such complementary arrangements or phenomena can even be associated with a single quantum object. One would always need two quantum objects in order to enact, in two separate experiments performed in two complementary arrangements, say, those associated with the position and the momentum measurement, respectively, with, in each case, the measurement itself physically pertaining strictly to the measuring instruments. It, again, follows that the uncertainty relations, too, apply to variables physically pertaining to the measuring instruments.

Importantly, our freedom of choosing the experimental setup only allows us to select and control the initial setting up of a given experiment but not its outcome, which, again, can only be probabilistically estimated. This fact reflects the "objectivity" of the situation, defined by the verifiability and, thus, the possibility of the unambiguous communication of the data involved in our experiments and, hence, the objective character of quantum mechanics in this interpretation. Bohr summarizes this point in his 1955 "Unity of Knowledge":

> 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 [individual or collective quantum objects] 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 … forced us … to pay proper attention to the conditions of observation. (Bohr 1954b, 1987, v. 2, p. 74; emphasis added)

Thus, the observers in quantum physics are as detached from *measuring instruments* as the observers in classical physics are from *classical objects*. This detachment ensures the objectivity of Bohr's scheme: the data in question or our predictions based on this data are the same for and hence independent of any particular observer. On the other hand, the measuring instruments used in quantum measurement can, in an act of observation or measurement, never be "detached" from quantum objects because the latter cannot be "extracted from" the *closed* observed phenomena containing them (Bohr 1954b, 1987, v. 2, p. 73). Phenomena *cannot be unsealed* so as to reach quantum objects by disregarding the role of measuring instruments in the way it is possible in classical physics or relativity. Although quantum objects do exist independently of us and of our measuring instruments, they can never be observed independently, and thus are in conflict with Einstein's ideal of objectivity or completeness, which require a theory to describe the properties of the objects considered by this theory independently of measurement. Quantum objects have reality but, in this interpretation, prevent realism in our theories concerning them, by the RWR principle. As I noted from the outset of this study, nobody has ever seen, at least not thus far, a moving electron or photon as such, but only traces of this "movement" (assuming even this concept applies), traces that do not allow us to reconstitute this movement itself in the way it is possible in classical physics or relativity. Any representation, even if not conception, of quantum objects themselves and their behavior, is *"in principle* excluded" (Bohr 1949, 1987, v. 2, p. 62).[34]

[34] Bohr's appeal to a "detached" observer could be related to Einstein's and Pauli's contrasting views of the situation (Plotnitsky 2013). Bohr might have had both views in mind, although Pauli was unhappy with Bohr's use of the terms and possibly the conception itself (Gieser 2005, pp. 131–133). For Einstein, the quantum-mechanical observer is not sufficiently detached and, hence, not sufficiently objective. In Bohr's view of the situation, as just explained, 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 can never be "detached" from quantum objects, because these objects cannot be extracted from the closed phenomena, containing them. Phenomena cannot be opened so as to reach quantum objects themselves by disregarding the role of measuring instruments in the way it is possible in classical physics or relativity, and thus in conflict with Einstein's desiderata for fundamental physics. For Pauli, the quantum-mechanical observer is still too detached, at least for a successful approach to quantum field theory, in view of the fact that the theory does not take into account the atomic structure of measuring instruments (Pauli 1994, p. 132).

It is sometimes argued that Bohr’s interpretation arises from his view that quantum mechanics or even physics in general should not be concerned with the ultimate character of nature, or reality, but only with what can be *known* about nature. Bohr did say on one occasion that in “our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience” (Bohr 1929b, 1987, v. 1, p. 18). It is worth keeping in mind that this statement, actually uncommon, if not unique, in Bohr, reflects Bohr’s earlier view, predating his post-EPR view, let alone his ultimate view. However, even if one follows this formulation, it is, one might argue, not so much that Bohr wants to arbitrarily, merely as a matter of his philosophical position, renounce “disclosing the real essence of the phenomena,” which could, after all, be part of our experience. Instead, one might argue that Bohr came to think, especially following his exchange with EPR, that, insofar as quantum mechanics and higher-level quantum theories are correct, they suggest that a representational model of quantum-level reality *may be* “*in principle* excluded,” excluded, again, beyond Kant’s epistemology. This, again, need not mean that such a model is bound to remain excluded (hence, my emphasis on “may be”). As far as it is excluded, however, Bohr’s position reflects a much stronger view than that urging one merely to renounce “disclosing the real essence of the phenomena,” because, if such is the case, even if one wanted to have such a model, one might not be able to develop it. By the same token, it might no longer be possible “to disclose the real essence of the [quantum] phenomena,” although it is possible “to track down, so far as it is possible, relations between the manifold aspects of our experience,” and such a tracking would not be possible otherwise. Bohr was concerned with reality as much as Einstein was. The difference is that, unlike Einstein, Bohr came to accept the possibility of “reality without realism” and, thus, the RWR principle as a fundamental principle.

In sum, in Bohr’s ultimate interpretation, quantum phenomena, defined strictly by what is observed in measuring instruments, tell us that a realist representation, even if not a conception, of quantum objects and processes in their independent existence or reality is not possible, at least, again, as things stand now. Quantum objects and processes are assumed to exist independently of us, to be independently real, and as such to be responsible for emergence of quantum phenomena. Their existence or reality is inferred from the character of quantum phenomena and the data they contain, including and in particular, quantum randomness and quantum correlations. These phenomena are objects insofar as the data observed in them can be unambiguously established, transmitted, communicated and so forth. Hence, quantum mechanics or higher-level quantum theories are objective insofar as it predicts objectively confirmable or verifiable probabilities or statistics of quantum experiments, that is of these data. Indeed, objectivity itself may define by this verification, in the case of a *single performed* measurement, determinately, and otherwise statistically, because, while we can repeat a given experiment, we cannot in general repeat a given measurement with the same outcome. The existence of quantum field theory, to be discussed in Chap. 6, is additionally important because we can have a theory beyond quantum mechanics, accounting by mathematically different means for more complex phenomena of epistemologically the same character, insofar as the ultimate constitution of nature manifested in the corresponding phenomena is beyond realism. High-energy quantum phenomena and quantum field theory can be interpreted in this way, while fully conforming to what is actually established experimentally and consistently with the logical and mathematical structure of the theory. The question, again, remains whether nature will ultimately allow for a more realist, Einstein-complete, theory, as Einstein and, following him, many others some believed that it will one day, and some even claim that this already the case. In Bohr's view, while is this is possible, it is also possible that our fundamental theories will remain nonrealist, and correlatively, irreducibly probabilistic or statistical, even as concerns the elementary individual processes they consider.

3.6 Complementarity and Probability

Bohr's interpretation, especially in its ultimate, RWR-principle-based version, entails the understanding of quantum randomness and quantum correlations, and of probability or statistics in quantum theory that is different from those found in classical physics, including classical statistical physics. This understanding, which is defined by divorcing probability from causality, was, again, found already in Heisenberg's paper introducing quantum mechanics and was made further apparent by Born's interpretation of the wave function. However, it was, arguably, expressly developed as such for the first time by Bohr, beginning, as discussed in Sect. 3.3, with his 1929 "The Quantum of Action and the Description of Nature" (Bohr 1929a), although his definitive statements to that effect had to wait nearly another decade. This understanding is defined by the absence of realism, ultimately as defined by the RWR principle. This absence of realism implies the inapplicability of the idea and ideal of (classical) causality to *elementary individual quantum processes and events associated with them*, such as those concerning elementary particles, and to the corresponding phenomena (in either sense), where such events are observed as effects of the interaction between quantum objects and measuring instruments. All such events are, again, discrete, "atomic" (in Bohr's sense), which is Bohr's ultimate expression of the QD principle. This makes any predictions concerning them unavoidably probabilistic or statistical, and quantum mechanics in Bohr's interpretation or any interpretations based in the RWR principle deal only with such predictions, in accordance with the QP/QS principle.

This understanding, thus, fundamentally departs from the view adopted in classical statistical physics or other areas of classical physics in which probability or statistics is used. In these cases, the behavior of individual entities, either considered as such, as in classical mechanics, or comprising the multiplicities considered statistically in classical statistical physics, is assumed to be ideally causal. However, the mechanical complexity of these systems makes the recourse to probability or statistics unavoidable in predicting their behavior. A coin toss is, arguably, the most common example of this situation, unless the quantum aspects of the constitution of the coin are considered a factor (which is, generally, not the case). As discussed from the outset, this assumption is incompatible with the statistical laws of quantum theory, beginning with Planck's law, as was, again, established by Einstein (Einstein 1906). Unlike in the case of classical individual events, such as a coin toss, in the case of quantum phenomena it does not appear possible—and in Bohr's or other RWR-principle based interpretations, it is in principle impossible—to subdivide these phenomena into phenomena, concerning which our predictions could be exact, even ideally or in principle. Any attempt to do so will require the use of an experimental setup that leads to a phenomenon or set of phenomena of the epistemologically same type (they could be different physically), concerning which we could again only make probabilistic or statistical predictions. Interfering with quantum objects, as we must in order to make any quantum phenomena possible, rather than being detached from them in the way we are from classical objects, makes probability or statistics unavoidable not only in practice but also in principle. As explained earlier, the probabilities themselves are obtained from the formalism by means of the procedure, which was first used by Heisenberg (in a limited case of hydrogen spectra) and developed more generally by Born (Born's rule), and which had not been encountered in physics or anywhere else before quantum mechanics.

Bohr presents his view of this situation, announcing the divorce of probablity and causality in quantum theory, in terms of the concepts of phenomenon and atomicity in the Warsaw lecture, "The Causality Problem in Atomic Physics," which invokes both concepts in its opening paragraph. He says:

> 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 proper to atomic phenomena, and above all to offer an entirely sufficient basis for 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, implies a shortcoming of the causality ideal itself. (Bohr 1938, pp. 94–95)

Later in the lecture Bohr argues that "the *statistical* character of the uncertainty relations," considered earlier in this chapter, "expresses an essential limitation of applicability of classical ideas to the analysis of quantum phenomena" (Bohr 1938, p. 100). In his 1949 "Discussion with Einstein," he states: "[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, 1987, v. 2, p. 34). One should perhaps refer to the indivisibility *and* individuality of phenomena, restricting us to probabilistic or statistical 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. The main point, however, is that the lack of causality in the case of quantum phenomena is due to "the inability of the classical frame of concepts to comprise the peculiar feature of indivisibility, or 'individuality,' characterizing the elementary processes"—the unrepresentable and possibly unthinkable quantum processes that result in the indivisibility and individuality of quantum phenomena. As I have argued all along, this inability is an interpretive inference from the primary experimental situation, defined by the QP/QS principle, which limits us to probabilistic or statistical estimates of the outcomes of all quantum experiments. It is a logically consistent interpretation of quantum formalism, the mathematical model of quantum mechanics, initially established by Heisenberg, on the basis of the QD and QP/QS principles, and a form of proto-RWR principle, as discussed in Chap. 2.

It is worth noting that Bohr refers to the inability of the classical frame of concepts to comprise the ultimate nature of quantum objects and processes, even though this frame remains indispensable in comprising their effects manifested in measuring instruments. In each case, the wave function provides, to return to Schrödinger's way of putting it, probabilistic or statistical expectation-catalogs of the outcomes of quantum experiments. Any such catalog is reset with each new measurement, which renders previous history of measurement on the same object irrelevant as concerns our predictions from this point on (Plotnitsky 2009, pp. 73–76). As will be seen in the next chapter, the meaning of these catalogs may be subjected to a further interpretation, either along probabilistic, such as Bayesian, or statistical lines, even if one adopts nonrealism or the RWR principle.

The fact that probabilistic or statistical predictions of quantum mechanics (or higher-level quantum theories) are correct remains enigmatic in such interpretations insofar as it does not appear to have, and these interpretations do not and cannot have, an underlying physical justification of the type found in classical statistical physics or chaos and complexity theories. As I said, the enigma of quantum correlations—that, while each individual quantum event is *always* irreducibly lawless, in certain circumstances, such as those of the EPR type experiments or the double-slit experiment (when the interference pattern appears), multiple quantum events exhibit statistical correlational orders—may be the greatest of all quantum enigmas. It is true that order generally suggests that there is a law that should capture how it emerges, and that quantum mechanics does not provide such a law, in the way classical physics and relativity do, but only provides a mathematical machinery, technology, for predicting the probabilities or statistics of the outcomes of quantum experiments. In this regard, Einstein was correct to argue that the quantum riddle remains unsolved, unless one argues, with Bohr, that quantum phenomena themselves tell us that this is as far as nature *may* allow us to go, in other words, that the lack of solution may be the only solution, at least as things stand now. Quantum correlations are consistent with both the irreducibly random character of the individual quantum events (these correlations are statistical!) and the unrepresentable or even unthinkable nature of quantum objects and processes, which is, again, responsible for this character, but which leaves the ultimate nature of quantum phenomena enigmatic. Indeed, it was the existence of these correlations, inherent in the EPR experiment, that provided the ultimate impetus for Bohr's ultimate, fully RWR-principle-based, interpretation and, as a result, a deeper meaning of the concept of complementarity as a quantum-theoretical concept, especially as manifested in the EPR complementarity. It follows that one cannot really says that correlations are the greatest enigma of quantum phenomena because there is neither quantum phenomena apart from correlations nor quantum mechanics apart from entanglement that enables it to predict correlations. Correlations are, as Schrödinger argued, "[not] *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought" (Schrödinger 1935b, p. 555). "Correlations and only correlations," David N. Mermin once said, in effect translating this statement, without citing it, but undoubtedly aware of it (Mermin 1998).

I am reluctant to argue that correlations necessitate this type of interpretation, although Bohr sometimes comes close to making this type of claim. I adopt a more cautious view that nature *may not*, rather than that it *will not*, allow us to go further. As things stand now, the enigmatic combination of individual lawlessness of individual quantum events and the statistically correlated nature of certain aggregates of such events leaves space for statistical predictions, a space that quantum mechanics is able to use, perhaps maximally. But, it leaves little, if any, space for the description or even conception of the processes responsible for this combination, and as a consequence, for any causality behind it. Realism gives space to reality without realism, and as a consequence, (classical) causality gives space to probability without (classical) causality.

Complementary quantum phenomena are consequences and manifestations of this reality, manifestations that, as I have argued here, are productive of new thought and knowledge—thought and knowledge that would not be possible otherwise even though and because they make the unthinkable their essential part. We are far from having exhausted the potential offered by this thought and knowledge or, to return to Bohr's language in his reply to EPR, by "this entirely new situation as regards the description of physical phenomena," which "the notion of *complementarity* aims at characterizing." In closing his reply Bohr reiterates that complementarity as a "new feature of natural philosophy means a radical revision of our attitude as regards physical reality" (Bohr 1935, p. 702). This revision invites us to explore new possibilities of thought and knowledge, possibilities that, I would argue, extend and must be explored beyond Bohr. This is the greatest reason to try to understand what he was trying to tell us about quantum phenomena and quantum theory, or about how nature and mind work in physics and beyond.

Chapter 4
The Statistical Copenhagen Interpretation

Abstract The main goal of this chapter is to present "the statistical Copenhagen interpretation," developed by the present author. This interpretation will be compared with the Bayesian interpretation of quantum mechanics, or a set of such interpretations, specifically nonrealist ones. The chapter also considers Bohr's interpretation from this perspective and argues that Bohr appears to have ultimately inclined to a statistical view close to that of the statistical Copenhagen interpretation, which, however, expressly adopts a stronger form of the RWR principle, by placing quantum objects and processes not only beyond representation but also beyond conception, beyond thought. Following a brief introduction given in Sect. 4.1, Sect. 4.2 is devoted to the statistical Copenhagen interpretation, and Sect. 4.3 to the discussion of Bohr's position concerning the statistical vs. probabilistic (Bayesian) view of quantum mechanics.

4.1 Introduction

The nonrealist understanding of quantum probability, established, following Bohr, in the last section of the preceding chapter, allows for different interpretations. These interpretations are essentially of two types: the first type is statistical and the second is probabilistic. The main interpretation of the first type to be considered here is "the statistical Copenhagen interpretation" (hereafter SCI), introduced in the previous work of the present author (Plotnitsky 2016; Plotnitsky and Khrennikov 2015). The second type is represented by Bayesian interpretations, here specifically those that are nonrealist, RWR-principle-based. It is possible to have a realist Bayesian interpretation of quantum mechanics, or of course realist and ultimately causal (as concerns the elementary quantum processes) statistical interpretations of it, or again, other realist or causal theories of quantum phenomena, such as Bohmian ones, which could be interpreted along either Bayesian or statistical lines.[1] I am,

[1] In addition to Khrennikov's interpretation cited in Chap. 1 (Khrennikov 2012; Plotnitsky and Khrennikov 2015), I might note a compelling statistical interpretation proposed in (Allahverdyan et al., 2013). I mention these two interpretations primarily because, although ultimately realist, they exhibit instructive affinities with SCI, as discussed in (Plotnitsky and Khrennikov 2015). There are other statistical interpretations of quantum mechanics, which cannot be considered within my scope here.

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however, only concerned with nonrealist, RWR-principle-based, interpretations, again, following Bohr's understanding of probability established in his ultimate interpretation. Whether Bohr's interpretation is in accord with SCI or with some form of a Bayesian interpretation is a matter of interpretation, an interpretation of Bohr's interpretation. I shall suggest that, while it may, in principle, be interpreted either way, Bohr appears to have ultimately inclined to a statistical view, which brings his interpretation closer to SCI, except that the latter adopts a stronger form of the RWR principle, by placing quantum objects and processes not only beyond representation but also beyond conception. (As noted from the outset, there is no direct evidence that Bohr subscribed to this view.) Pauli may be argued to have expressly adopted a statistical view, in accord with SCI, although there is, again, no evidence that he subscribed to the form of the RWR principle assumed here in defining SCI, which thus epistemologically distinguishes it. Indeed, my starting point is Pauli's view of the quantum-mechanical situation is this regard.

4.2 Quantum Probability and the Statistical Copenhagen Interpretation

Pauli's arguably most definitive assessment of the nature of quantum probability follows Bohr and even uses Bohr's customary locutions ("rational generalization" and "the finiteness of the quantum of action"), although, while in the spirit Copenhagen, Pauli's overall interpretation of quantum phenomena and quantum mechanics is different from that of Bohr. The discussion of Pauli's interpretation is beyond my scope. Briefly, as noted in Chap. 3, his interpretation makes the consciousness of the observer and the measuring apparatus used inseparable, while Bohr and SCI (which follows Bohr on this point) "detach" the consciousness of the observer from the apparatus, too much so for Pauli, from whom I borrow the term "detach," as did Bohr (Plotnitsky 2013). On the other hand, Pauli clearly adopts a statistical, rather than Bayesian, view of quantum predictions, at least when they are made by quantum mechanics. According to Pauli:

> As this indeterminacy [that is reflected in the uncertainty relations] is an unavoidable element of every initial state of a system [a quantum object] that is at all possible according to the new [quantum-mechanical] laws of nature, the development of the system can never be determined as was the case in classical mechanics. The theory predicts only the *statistics* of the results of an experiment, when it is repeated under a given condition. Like the ultimate fact without any cause, the *individual* outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case, if quantum or wave mechanics is interpreted as a rational generalization of classical physics [mechanics], which take into account the finiteness of the quantum of action [Planck's constant, h]. The probabilities occurring in the new laws have then to be considered to be primary, which means not deducible from deterministic [causal] laws. As an example of these primary probabilities I mention here the fact that the time at which an individual atom will undergo a certain reaction

> stays undetermined even under conditions where the rate of occurrence of this reaction for a large collection of atoms is practically certain. (Pauli 1994, p. 32)[2]

Pauli speaks of determinism rather than causality, but this does not affect the main point in question at the moment because his concept of determinism is in effect the same as the present concept of causality. Besides, as explained in Chap. 1, in the case of classical mechanics, which is the main classical reference here, both notions coincide. Most important for the present discussion is Pauli's main claim: "The theory predicts only the *statistics* of the results of an experiment, when it is repeated under a given condition. Like the ultimate fact without any cause, the *individual* outcome of a measurement is, however, in general not comprehended by laws." Given that Pauli does not specify otherwise, this appears to include probabilistic or, in this view, *statistical* laws of quantum mechanics, which, by definition, only apply to statistical multiplicities of repeated quantum experiments. Pauli corroborates this reading elsewhere in the context of complementarity. He says: "In the general case of the quantum-mechanical state of a material particle, neither the position nor the momentum is predictable with certainty; in consequence the state can be described only by *statistical* statements about the distribution of values of the results of possible measurements of position or momentum of this state. Formally these statements are embraced symbolically in a wave function, consisting of a real and an imaginary part" (Pauli 1994, p. 99; Pauli's emphasis).

This assessment of the quantum-mechanical situation has a major implication, which is not stated by Pauli in this form but which leads to SCI. In this interpretation, elementary quantum processes and events associated with them are not only beyond representation by means of the quantum-mechanical formalism, or possibly, and in the present, if not Bohr's, view, even conception, but are also beyond even probabilistic predictions. Exact predictions, even ideal ones (there are, again, no other exact predictions), are excluded on experimental grounds in view of the uncertainty relations. The latter are independent of the theory or interpretation one uses, by the QP/QS principle, which in the case of SCI becomes strictly the QS principle. Thus, realist and causal interpretations of quantum mechanics or Bohmian theories exclude exact predictions as well. In nonrealist, RWR-principle-based, interpretations, which preclude causality, such predictions are, again, excluded automatically. In other words, in SCI, the outcome of a future individual quantum experiment, a future individual quantum event, cannot, *in general*, be assigned probability: it is random. Indeed, randomness may even be (epistemologically) defined by the impossibility of such an assignment. Only the statistics of multiply repeated (identically prepared) experiments can be predicted, which gives the corresponding statistical meaning to the expectation-catalogs provided by the formalism. I shall explain the emphasis on "in general" presently, merely noting for the moment that

[2] Among other major figures who adopted this type of position were Schwinger (2001, pp. 14–15) and, earlier, again, with a negative attitude, Schrödinger (1935a, p. 154). As noted in Chap. 2, Schwinger's interpretation appears to assume that the independent behavior of quantum systems is causal.

in some experiments, it is possible to assign a probability to individual quantum events for all practical purposes, but I would argue, not in full rigor.

It might appear that, rather than in terms of frequencies or statistics, one could speak rigorously (rather than only for practical purposes) of the probability of a future individual quantum event, similarly to the way one does in the case of a single coin toss, and of assigning this probability by using quantum-mechanical formalism, say, again, the wave function. This possibility is implied by the standard Bayesian views of probability, defined as a degree of belief and found, for example, in (Jaynes 2003) or (De Finetti 2008).[3] It is not my aim to deny these claims, or argue against Bayesian approaches to quantum theory, which need not be limited to these claims alone. I argue instead that these particular claims pose difficulties given the data observed in quantum experiments, at least thus far, and that in any event the statistical view of quantum phenomena and quantum mechanics, defined by SCI is consistent with these data.

Claims of this Bayesian type are also seen as possible and appear to be made by the currently most prominent Bayesian approach to quantum theory, the so-called quantum Bayseanism or QBism (e.g., Fuchs et al., 2014; Mermin 2016, p. 219). QBism also adopts, *in the present terms*, a nonrealist, RWR-principle-based, interpretation of quantum objects and processes, and *in this respect*, is in agreement with Bohr's interpretation or SCI. QBism is both Bayesian and nonrealist, but not all Bayesian or, conversely, all nonrealist positions are QBist. The nonrealist position defining SCI is not. Some among QBist, such as C. A. Fuchs, would prefer to see their view as "realist" because they do assume, as Bohr and the present study do, quantum *reality*, the reality of quantum objects. As indicated earlier, some commentators claim Bohr to be a realist on the same grounds. This appears to me to be a matter of terminological preference, which is understandable, given a general resistance among physicists and philosophers to anything that is not "realist." I would argue, however, that *in this respect* QBists, Bohr, and the present author hold similar, in the present language, nonrealist, views insofar as they do not see quantum mechanics as providing a representation of reality, but only giving probabilistic or statistical predictions concerning the outcome of quantum experiments. This claim is, I think, easily supported by QBists's communications, such as those cited here.

[3] Such views may, and Jaynes's and De Finetti's do, deviate from each other in other respects. Bayes's theorem itself (Bayes himself proved a version of it), 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, is general and valid in the frequentist approach as well. Bayes's formula or theorem relates the conditional and marginal probabilities of events *A* and *B*, where *B* has a nonzero probability of occurrence:

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

Here $P(A)$ is the prior probability or marginal probability of *A* (also known as the "prior"), which does not take into account any information concerning *B*; $P(A|B)$ is the conditional probability of *A*, given *B*, called the posterior probability, which depends on the value of *B*; $P(B|A)$ is the conditional probability of *B* given *A*; and $P(B)$ 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*.

This is not to say, that these views are not different in other respects, even apart from how they see the nature of quantum probability. Thus, QBists aim to depart from Bohr, in particular, by giving a greater role to subjective dimensions of probability or knowledge in general in quantum theory and beyond, while the present view is closer to Bohr in this regard. On the other hand, there is no evidence that QBists subscribe to the strong form of the RWR principle, adopted by this author, which places the nature or reality of quantum objects and processes beyond thought altogether.

It is not my aim to dispute or even significantly engage with QBism, which, again, involves other features pertaining to the philosophy of quantum mechanics, the philosophy of physics or science, the philosophy of probability, and philosophy in general. If anything, I am inclined to a positive view of QBism, because it has contributed to the nonrealist understanding of quantum phenomena and quantum mechanics, and thus to advancing the spirit of Copenhagen, although QBists themselves might prefer to stress more the difference from the spirit of Copenhagen, and from Bohr's views.[4] I am only concerned with the possibility or impossibility of assigning probabilities to individual quantum events, which is a more general Bayesian assumption that should not be simply identified with QBism, which, however, appears to make it. Could it be Bayesianism otherwise? This question does bear on the questions of reality, with or without realism, and the epistemology in quantum theory, and differences in this regard between SCI and QBism. These issues will, however, only be mentioned in passing here.

SCI may be seen (a matter of perspective, however) as more radical epistemologically than any Bayesian view, insofar as not even the probabilistic "knowledge" (to the degree that our probabilistic estimates amount to knowledge) concerning, or even the application of the concept of probability to, individual quantum events is possible. First of all, at least in most quantum situations, any verification of such individual estimates would still involve multiple events, and these individual estimates will rely on that data, reflecting these repeatable statistics. Admittedly, a Bayesian might contest this point in the case of quantum phenomena similarly to the way it might be and has been contested by Bayesians in the case of our estimates of the probability of a

[4] See (Mermin 2016, pp. 232–248) on the difference between QBism and Bohr, even if not in "the spirit Copenhagen," which is not the language or concept used by Mermin, who may or may not see QBism in this spirit. Mermin does suggest some affinities between QBist and Bohr's views, albeit without exploring Bohr's views in any depth, in particular, as concern the nature of Bohr's concept of reality as "reality without realism," and thus the RWR principle. This is a juncture of Bohr's thought at which Bohr is on some points closer to QBism and on others departs more radically from it than Mermin suggests. Mermin primarily (correctly) stresses the difference between QBism's emphasis on subjectivity vs. Bohr's view of measurement as objective, as considered above, albeit, again, without really examining Bohr's concept of objectivity, for example, in relation to the disciplinary practice of physics. Mermin does see QBism in terms of the statistical vs. Bayesian view of quantum probability. I might add that, while QBists are quite right to critique the (naïve) conceptions and requirement of objectivity or naïve realism, still common in the scientific community, it appears to me that the concept of subjectivity (and certain concepts associated with it, such as and in particular "belief") are used by them uncritically, rather than analytically examined. Perhaps, it will come in time.

coin toss (Jaynes 2003, pp. 317–320). More crucial is the point that an individual quantum event may be beyond (rigorously) assigning it a probability at all.

Consider the double-slit experiment. It is true that in the case of the interference setup (both slits open and there are no counters allowing us to detect through which slit each particle in question passes), we observe the interference pattern (in the absence of actual waves, "correlational pattern" might be a better term), which is defined by the zones of permitted or "forbidden" impact. This pattern is strictly statistical in nature, and it manifests itself only in a very large number of trials, around 70,000. The statistical nature of this pattern also reflects the fact that rigorously there is no zone of "forbidden" impact for any individual trial. Any given trial can leave its mark anywhere on the screen, as is clearly shown by any number of the double-slit experiments performed with photons or by the famous data of A. Tonomura's single electron build-up experiments (Tonomura et al., 1989). Some trials (a statistically small number) can produce no impact at all.

The presence of the interference pattern, which amounts to quantum correlations, in the interference setup, of the double-slit experiment, or the existence of other quantum correlations, such as those of the EPR type, is crucial for and a major motivation for SCI. This is because, while in the alternative, no-interference, setup, we in principle map the screen by area and assign an equal probability for each area (just as in the classical case), in the interference setup, when we have the correlational pattern, any given hit could still be anywhere, while statistically the pattern will inevitably emerge. So, the corresponding effects are exhibited only statistically, but they are still a defining, even the most defining feature of quantum phenomena. This returns us to, Mermin's maxim, cited earlier: "correlations and only correlations" are the essence of the quantum (Mermin 1998). There would be no need for SCI if there were no quantum correlations.

In view of these considerations, it appears difficult to speak *rigorously* of assigning a probability to an individual trial. It is true that given the setups in certain experiments, such as the Stern-Gerlach experiment or some interferometry experiments it may be possible to speak, nearly with certainty, of an object having a 50 % probability of taking one path or another in the corresponding arrangement. First, however, if so, this is true only in some experiments, which is why I use "in general" in speaking of the impossibility of such assignments. Secondly, even in these cases, there will be some trials, however small in number, in which no outcome will be registered, and unlike in classical cases, where similar statistical deviations may occur, it is not a matter of outside interferences, but something inherent in quantum experiments.

It would, then, also appear (even if, again, without offering a definitive assessment) that these circumstances complicate, even if not exclude, the application, in quantum physics, of the Bayesian view, insofar as it refers to estimates, bets on, the outcome of single events on the basis of the information one has.[5] A given quantum phenomenon or event would, in Bohr's definition, be seen in relation to the conditions defining this multiplicity, such as one or the other setups of the double-slit

[5] Of course, in the Bayesian scheme of things, there may be events to which one cannot assign probabilities, but not in general, which is the opposite of what takes place in SCI, where such assignments are only possible sometimes but are impossible in general.

experiment, that gives rise to the interference pattern or that in which this pattern will not appear. According to Pauli: "The mathematical inclusion, in quantum mechanics, of the *possibilities* of natural events has turned out to be a sufficiently wide framework to embrace the irrational *actuality* [i.e., that beyond even the laws of quantum theory] of the single event as well" (Pauli 1994, p. 47). In any event, SCI appears to be consistent with the experimental data in question.

This situation, for example, again, as manifested, especially graphically, in the double-slit experiment, reveals an important dimension of complementarity, also as a statistical concept. Complementarity means that the behavior of the same quantum objects, of quantum objects of the same type, say, electrons, cannot be governed by the same physical law, especially a representational physical law, in two complementary contexts, because their collective behavior is different in each of these contexts, and their individual behavior is random, even if, in some contexts, collectively correlated. Hence, as Pauli says in the statement cited above, "the individual outcome of a measurement is … in general not comprehended by laws." Quantum mechanics, in RWR-type interpretations, provides no such physical laws, but only a set of mathematical laws (algorithms) for correctly predicting the probabilities or statistics of the outcomes of quantum experiments in all contexts considered. Or rather, quantum phenomena and quantum mechanics compel us to rethink the very concept of physical law. In SCI, the individual outcome of a quantum experiment cannot even be assigned a probability, and hence cannot be comprehended even by the mathematical laws of quantum mechanics.

I am not saying that the Bayesian approach does not work in general in physics and beyond: there are many situations where it does, for example, when we need to estimate the probabilities of certain human events, as in betting on the outcome of a basketball game. Even in quantum experiments one could, in principle, make any predictions one likes or must in view of one's Bayesian prior, including along the lines of the Jocasta ontology defined in Chap. 1, according to which everything would be random and one could assign no probability at all to any given future event or statistics to any set of future events in any circumstances.[6] One could also estimate (subjectively), *statistical* expectations concerning the multiple repetitions of a given experiment, say, in a double-slit experiment with both slits open, and such estimates, too, could be better or worse, or possibly outright wrong. In this sense, one could even speak, without being paradoxical, of "statistical Bayesianism." However, the question is that of the effectiveness (and, hence, unavoidably verification) of our predictions in physics, of who will do, *bet*, better in physics, in predicting the outcomes of physical experiments, in the case of quantum phenomena probabilistically or statistically, because no other predictions are possible thus far. Consider, again,

[6] This type of Bayesian prior is found in T. Stoppard's play *Rosencrantz and Guildenstern are Dead*, in a deliberate contrast to or a parody of these characters' prior in *Hamlet*, where they, naively, bet on the future with certainty, as opposed to Hamlet himself. Hamlet weighs his Bayesian bets with great deliberation and changes them with new information, which he assiduously seeks, to gain more certainty on his bets throughout the play. And yet he often bets wrong as well. Games of probability, especially of the Bayesian variety, are quite complex in Shakespeare, and one can find all types of priors in them.

the double-slit experiment. One will "win" with quantum mechanics in hand against those who do not know it or whose theory is not as good, but one would win or, speaking more rigorously, will not consistently lose, only in many trials, which fact implicitly contains statistics.[7] This is far from insignificant, and is a powerful reason to use quantum mechanics for predicting the outcomes of quantum experiments. My point is only that quantum mechanics, in general, offers one no help as concerns predicting and hence betting on the outcome of a single experiment, because one cannot rigorously predict what happens, although, as noted earlier, this is in practice workable in some experiments—in practice, but not in full rigor. Accordingly, still speaking with more caution than definitiveness and by way of an interpretive choice, the statistical approach to quantum mechanics may be more rigorous than a Bayesian one. Pauli appears to have thought so, as well. More significant, as noted earlier, is that, while the repetition of the same experiment with the same outcome (which was deemed previously essential to science, was a "basic principle of science") is no longer possible, it is possible to repeat the statistics of the same repeated experiment. This repetition, at least thus far, appears to be necessary in the practice of physics as a mathematical science of nature, and, to return to Bohr's formulation, enables us to see quantum mechanics as fully in accord "with the basic principles of science" (Bohr 1935, p. 700). Admittedly, in this view, the strict repetition of the outcome of single experiment is no longer seen as such a principle. This should not be surprising, because, as stated from the outset, fundamental principles can change and some could be abandoned and new principles could be introduced, as it happened, for example, in relativity or indeed even in the history of classical physics. The irreducibly probabilistic or statistical nature of all quantum predictions and the repeated statistics of multiple experiments are new principles, the QP or the QS principle. Both assume this repetition, at least in most interpretations, even if one interprets quantum probability on Bayesian lines. As will be seen in the next chapter these principles allow one to define a concept and principle of quantum causality.

4.3 Probability and Statistics in Bohr

Bohr's position concerning this alternative between assigning probabilities to individual quantum events on Bayesian lines and the statistical interpretation of quantum predictions, without such an assignment, does not appear to have been expressly stated in his works. Hence, it could be interpreted either way; and it was interpreted along more Bayesian lines previously by the present author, an interpretation to be questioned here (Plotnitsky 2009). On other hand, there does not appear to be a statement in Bohr suggesting a Bayesian view either. Indeed, Bohr, who is careful in selecting his terms, clearly preferred "statistical" to "probabilistic" in referring to quantum predictions throughout his oeuvre, although, as noted earlier, there are a few statements referring to estimating probabilities rather than statistics, possibly

[7] I am indebted to J-Å. Larsson for suggesting this last qualification.

for contextual historical reasons (e.g., Bohr 1949, *PWNB* 2, p. 34). This and, more importantly, the logic of Bohr's argument, make the present author inclined to see his view along statistical, rather than Bayesian, lines, thus revising, on this point, the position taken in (Plotnitsky 2009). Consider Bohr's comment (in 1949) on Einstein's 1936 criticism of quantum mechanics, a comment already cited earlier, as expressing the RWR principle, as defining Bohr's ultimate interpretation. The passage, which I now cite at length, and Einstein's argument to which it responds also relates, however, to crucial statistical questions at stake in quantum mechanics and its interpretation. According to Bohr:

> Einstein … argue[d] 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, and his attitude to the belief that it should offer an exhaustive description of the individual phenomena is expressed in the following words: "To believe this is logically possible without contradiction, but is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception [that of the description of individual quantum processes, as an ideally exact description]." … Even if such an attitude might seem well balanced in itself, it nevertheless implies a rejection of the whole argument exposed in the preceding [essentially an argument explaining Bohr's interpretation], aiming to show that, in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis [reaching quantum objects and their behavior] of atomic phenomena but with a recognition that such an analysis is *in principle* excluded. (Bohr 1949, 1987, v. 2, pp. 61–62; Einstein 1936, p. 349)[8]

It might appear that Bohr implies here that quantum mechanics does, contrary to Einstein's assessment, provide an exhaustive "description" of the *individual* quantum phenomena, rather than is a means of accounting for the average behavior of a large number of atomic systems. Or rather, more accurately (given Bohr's interpretation in its ultimate version, as considered in Chap. 3), Bohr might appear to suggest that quantum mechanics provides an exhaustive *predictive account* of individual quantum phenomena, in the sense of enabling one to assign probabilities to our predictions concerning these phenomena. However, he does not say that these pre-

[8] There is a curious nuance to Einstein's assessment in question. It is true that this is, as Bohr says, an assessment to the effect 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 based in Einstein's belief that one should be able derive the inner workings of individual quantum systems even *on experimental grounds*. It replies to the following rhetorical question: "Is there really any physicist who believes that we shall never 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 impossible to ever get an inside view, also literally in the sense of intuition [*Anschaulichkeit*], concerning the inner workings of individual quantum systems, and establish an Einsten-complete theory of them, even if not derive this theory from quantum mechanics as a statistical theory of ensembles. As discussed in Chap. 1, Einstein came to doubt this possibility, a doubt expressed on the same occasion as well (Einstein 1936, p, 361). As also discussed in Chap. 1, one is likely to need a theory different from quantum mechanics in order to achieve this, if quantum phenomena will ever allow us to do so. Einstein's "dream" was, again, an ontologically classical-like field theory (Einstein 1936, p. 378, 1949a, pp. 83–85).

dictions, which, it follows, are unavoidably probabilistic, concern individual phenomena, rather than the statistics obtained in repeated identically prepared experiments, as, say, in the double-slit experiment. It is true that, as explained earlier, Bohr defines each phenomenon as individual. But, for the reasons explained earlier as well, he also expressly defines a phenomenon only as an *already observed*, *registered* phenomenon, and not as anything predicted. This allows one to interpret quantum-mechanical predictions as, *in general*, statistical (rather than as probabilistic, Bayesian, predictions pertaining to individual quantum experiments) consistently with Bohr's definition of phenomenon, "in general," again, because in some cases individual predictions are possible for all practical purposes, but, again, never completely.[9]

Accordingly, what Bohr says here is at the very least compatible with SCI. His elaboration, then, could be read as follows. It is true that, at least in his interpretation, as based in the RWR principle, quantum mechanics does not provide a representation of individual quantum objects and processes, and, if this is the criterion for the completeness of quantum theory, it is incomplete, or in terms of this study, is Einstein-incomplete. The question, as noted from the outset of this study, is whether nature or our interactions with nature allow us to do better. Bohr says that it might not, at least as things stand now, which is, again, not the same as saying that it never will. This is the meaning of his statement, expressing the RWR principle, that "in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis [reaching quantum objects and their behavior] of atomic phenomena but with a recognition that such an analysis is *in principle* excluded." This reading is fully consistent with the fact that quantum mechanics is "a means of accounting for the average behavior of a large number of atomic systems" in repeated experiments, a means of predictive accounts, providing the statistics of these repeated experiments. In other words, it is not only that individual, or any other, quantum objects and processes *are beyond description or even conception*, but also that the outcome of each individual quantum experiment is *beyond prediction* as well. It is rigorously random, while, however, allowing us to have the statis-

[9] In certain situations, such as those of the EPR type, as considered in Chap. 3, we can, for all practical purposes, predict certain quantities exactly (at least in the case of discrete variables), but this is never true in full rigor either. There is always a nonzero probability that the object in question will not be found where it is expected to be found at the moment of time for which the prediction is made. As noted earlier, unlike the Bell-Bohm version of the EPR experiment for spin (at stake in Bell's theorem), the actual experiment proposed by EPR, dealing with continuous variables, cannot be physically realized, because the EPR-entangled quantum state is not normalizable. As indicated in Chap. 3, there are experiments (e.g., those involving photon pairs produced in parametric down conversion) that statistically approximate the idealized entangled state constructed by EPR for continuous variables. These experiments are consistent with the present argument. They also reflect the fact that the EPR thought experiment is a manifestation of correlated events for identically prepared experiments with EPR pairs, which can in this regard be understood on the model of the Bell-Bohm version of the EPR experiment. In any event, there are quantum experiments, such as, paradigmatically, the double-slit experiment, in which the assignment of probabilities to the outcomes of individual events is difficult or even all but impossible to assume.

tics of the multiple repeated experiments. Any analysis beyond that of phenomena in Bohr's sense (which are individual) and the statistics of repeated experiments (which are always multiple) is "*in principle* excluded," again, at least as things stand now. In other words, that one cannot even assign (along Bayesian lines) probabilities to individual quantum events is as exhaustive account of such events as is available, as things stand now.

This may have been unacceptable to Einstein, but may, again, be as much as possible, which makes quantum mechanics Bohr-complete, as complete as possible, as things stand now. Einstein might not have had in mind all the nuances just spelled out, but this is secondary, vis-à-vis the interpretation itself thus suggested. Einstein did note that, if quantum mechanics is a statistical theory of ensembles, then the paradox of nonlocality arising from his analysis of the EPR-type experiment would disappear, and quantum mechanics could be seen as local. As he said on several occasions, 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, 1949a, p. 81; Born 2005, pp. 205, 211). This is because in this case a given single prediction on S_2 enabled by performing the corresponding measurement on S_1 of the EPR pair (S_1, S_2) need not have a definitive outcome.

This statistical alternative, however, still leaves quantum mechanics Einstein-incomplete, because of its inability to provide a properly exhaustive physical representation of the behavior of individual quantum systems (of the kind classical mechanics does for classical objects), including for the individual constituents of the systems considered in classical statistical physics. Accordingly, this alternative would still be insufficient for him to accept quantum mechanics as the way of describing nature in its ultimate constitution. 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" (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). In other words, in this view, quantum mechanics provides no account of individual quantum systems at all, is Einstein-incomplete, which (although, as will be seen momentarily, not Einstein's overall ensemble view of quantum mechanics) is in accord with Bohr's interpretation or to SCI. In either of these interpretations, however, quantum mechanics is, again, complete insofar as it does all that nature or, again, our interactions with nature allow us to do. It is Bohr-complete. So, with the analysis of the EPR experiment given in the preceding chapter in mind, both the completeness (again, admittedly, only the Bohr-completeness) and locality of quantum mechanics would be preserved, in either Bohr's interpretation, whether one interprets it on Bayesian or statistical lines, or in SCI.

As indicated above, Einstein's overall "ensemble" view of quantum mechanics (it does not quite amount to an interpretation, because Einstein never sufficiently elaborated it) is different from either SCI or Bohr's interpretation (whether it is understood probabilistically or statistically). Einstein thought of quantum mechanics as a theory of ensembles along the lines of classical statistical mechanics. He assumes that the observables representing the state of each individual system under consideration, say, in a multiple set of trials in the same setting, could be assigned definite values, defining the physical state of the system at any moment of time and its evolution in a classically causal way, and thus allowing one to predict its behavior (ideally) exactly. This is indeed how he understood the completeness of a physical description (Einstein-completeness). One needed, however, to find the equations that would describe this behavior, which were lacking in quantum mechanics (e.g., Einstein 1936, p. 349). Accordingly, this view is in conflict with quantum mechanics as currently constituted, which does not provide any means for such a representation. Even if one assumed, along the lines considered in Chap. 3, that the formalism of quantum mechanics represented the independent behavior of individual quantum system causally, any predictions by means of this formalism are still probabilistic or statistical, as they are in SCI and Bohr's interpretation, which, again, do not make this assumption, any more than those of Einstein. Hence, Einstein's ensemble view is equally in conflict with Bohr's interpretation and SCI. In other words, this difference is, again, defined by Einstein's assumption that an Einstein-complete theory of the ultimate constitution of nature was possible and that only the corresponding equations were lacking.[10] They still are. Bohr's interpretation (if given a statistical interpretation) or SCI is statistical in the absence of this assumption. Either assumes, on the contrary, that such as theory is "*in principle* excluded," as things stand now. An Einstein-complete theory of elemental individual quantum processes, if it becomes possible, would require a radical revamping of the whole quantum-mechanical or quantum-field-theoretical framework, as Einstein indeed argued, thinking of such a possible theory along the lines of conceptually classical-like field theory, just as general relativity was.

4.4 Cosmology and Probability

SCI suggests the following intriguing cosmological observation, assuming, in accordance with a currently standard view, that the very early, pre-Big-Bang, history of the universe is quantum, and that that its birth in the Big Bang is a quantum and hence noncausal event. If such is the case, in any interpretation based on the RWR principle (applied in high-energy regimes), our mathematical machinery, that of quantum field theory or its extensions that might be necessary in this case, will not be able to provide us with a representation of this early history, say, in accordance with some form of inflation theory. This does not mean that there is nothing that we can say about these early stages of the Universe. Our classical observations of the traces of these early processes may be established as configurations of

[10] For a discussion of Einstein's ensemble view of quantum mechanics and complexities it involves, see (van Dongen 2010, 174–181), which, however, only considers the situation in terms of Einstein-completeness (in the present terminology).

quantum phenomena, like the traces on the screen found in the double-slit experiments. Note that we can frame this early history from the "outside" in the current state of the Universe and use it as a measuring instrument in which certain effects of this early quantum state of the Universe could in principle be observed. It is true that thus far all assumptions concerning this state are extrapolated from post-Big-Bang classical or relativistic events and their effects (such as cosmic radiation), and that most of the early, say, again, inflationary, quantum history of the Universe appears to be erased without a trace and, if so, is altogether beyond reach. However, the existence of some traces of the early Universe quantum origin in the classically observed present-day Universe is conceivable and is even expected. These traces would give us information, again, physically classical but quantum in its origin, insofar as these traces are considered as effects of the early history of the Universe as a quantum object. The situation is essentially similar to those of effects we observe in other quantum experiments, say, an interference pattern in the double-slit experiment, which could be conducted on a cosmological scale (e.g., Wheeler 1983, pp. 190–194). The available traces might also enable us in principle to estimate, probabilistically, different successive (classically manifest) stages of the Universe as quantum phenomena, say, by writing something like a Schrödinger-like equation for the state defined by these traces, once again, however, without enabling us to say anything about the quantum aspects of the process itself (e.g., Hawking 1984). That is, one can do so, unless one adopts the SCI view, vs. a Bayesian view. A Bayesian interpretation would in principle make such estimates possible, although even in this case, our priors could not be based on really analogous events (an origin of a Universe) but on other quantum events. By contrast, SCI would make such estimates impossible. For, if one assumes SCI, one cannot *rigorously* count on any such prediction, because the outcome of this future history of the Universe would be assumed to be random. There is no way to witness repeated identically prepared experiments, assuming even that the birth of our Universe could ever in principle be repeated, given a singular quantum nature of this event, whatever it was that gave the birth our Universe.[11]

It would follow then that our predictions concerning the future of the universe could only be reliably based on the classical or relativistic aspects of its current or past states, or by extrapolating these aspects into possible earlier quantum states, as far as possible, possibly by some Bayesian estimates based on this information. Naturally, this type of argumentation, apart from being hypothetical in any event, still depends on our fundamental theories being quantum-field-theoretical-like, in RWR-principle-based interpretations, or their cousins, such as string or brane theories, assuming that the latter allow for this type of interpretation as well. A theory of an altogether different type may be necessary, or even in fundamental physics currently handled by quantum mechanics and quantum field theory, for example, because one still needs to take gravity into account, and possibly even apart from gravity. For now, however, as will be discussed in Chap. 6, apart from gravity, quantum field theory works exceptionally well, albeit not entirely flawlessly and not to everyone's

[11] The so-called cosmic landscapes theories could be considered from this perspective.

satisfaction. But then, many, beginning, again, with Einstein, have not been satisfied with quantum mechanics either, and many are still not satisfied with it, even though it has worked equally well for near a century.

Chapter 5
Could the Quantum-Mechanical Description of Physical Reality Be Considered Causal?

Abstract This chapter introduces the concept and principle of "quantum causality," and makes suggestions toward rethinking the nature of causality in view of the possibility of such a concept and principle, in juxtaposition to the classical concept and principle of causality, as defined in Chap. 1. Sec. 5.1 serves as a general introduction. Sec. 5.2 offers a discussion of the classical philosophical understanding of causality. Sec. 5.3 considers classical physics and relativity as classically causal theories, with a particular emphasis on qualifications that the use of classical causality in these theories requires. Sec. 5.4 considers quantum mechanics, in nonrealist, RWR-principle-based, interpretations, which preclude classical causality, and proposes the concept and the principle of "quantum causality."

5.1 Introduction

The short answer to the question posed by my title, which paraphrases that shared by EPR's paper and Bohr's reply ("Could the Quantum-Mechanical Description of Physical Reality be Considered Complete?"), is yes. This answer is not in conflict with the RWR principle and nonrealist interpretations of quantum mechanics based in it, which preclude a description of quantum objects and processes, because they do not preclude and in fact require a physical description, classical in nature, of quantum phenomena. It is to our prediction concerning these phenomena and the data associated with them that the concept of causality in question applies. My positive answer may, nevertheless, appear surprising, given the preceding discussion in this study. It might, however, not be entirely unanticipated either, if one accepts Bohr's view of complementarity as a generalization of causality, under the experimentally established conditions of the ultimately probabilistic and statistical character of our predictions concerning quantum phenomena, regardless of the theory we use to makes these predictions. This experimental circumstance, obviously, complicates my positive answer to this question. Indeed, it requires one to redefine the concept and the principle of causality, by making them quantum, by, as it were, "quantizing" them, and thus to rethink the nature of causality in general in view of quantum theory. This chapter will offer such a redefinition and will make

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suggestions toward this rethinking. It would be difficult to do more given the immensity or even inexhaustibility of the subject, matched by the immensity of literature, from Plato and Aristotle on, dealing with causality, and I could only cite a limited number of works especially pertinent to my argument.

As explained in the preceding chapters, classical physics, in particular classical mechanics, and its (idealized) mathematical models are customarily seen as causal, while quantum mechanics and its mathematical model are primarily, including in this study, seen as noncausal, although this view is far less uniform than that of classical mechanics as a causal theory. The situation, however, is more complex. First of all, while classical mechanics may be meaningfully considered as causal in the (more or less) standard classical sense, defined in Chap. 1, the application of this concept of causality in classical mechanics is subject to qualifications and limitations, already by virtue of qualifications and limitations of the application of the mathematical model of classical mechanics. Secondly and more importantly, as I shall argue here, quantum mechanics may be considered "causal" in turn, *even if one follows the spirit of Copenhagen, as defined by the QD, QP/QS, and RWR principles*. This is of course a crucial qualification, given that there are classically causal, or to begin with realist, interpretations of quantum mechanics, such as the many-worlds interpretation of quantum mechanics, or causal and realist alternative theories of quantum phenomena, such as various versions of Bohmian mechanics. Whether the ultimate constitution of nature at or beyond the reach of quantum mechanics or quantum field theory, say, at the Planck scale, 1.22×10^{19} GeV, where the effects of gravity become strong, is (classically) causal or not, or more accurately could be handled by a (classically) causal or realist theory or not, is an open question. If, however, one wants to speak of causality in the case of classically noncausal or, to begin with, nonrealist, RWR-principle-based, interpretations of quantum mechanics or quantum field theory, one needs an alternative concept and principle of causality. I shall propose such a concept or principle, that of "quantum causality," in this chapter. As will be discussed in Chap. 7, quantum information theory adds new possibilities for such alternative concepts, which are close to the concept of quantum causality to be proposed here, in part because they are probabilistic or statistical as well.

The possibility of such an alternative concept or principle has significant implications for the philosophy of causality. In particular, contrary to a long-standing philosophical aspiration, this possibility suggests that there might not be a single concept of causality, however general, under which all available and workable concepts of causality may be subsumed as special cases. Instead we may need to develop different concepts of causality for different purposes in physics and other sciences, or in philosophy. These concepts may share certain features, which would justify the application of the term "causality" to them. But they may also contain features that would irreducibly distinguish them from each other. In some cases, we may need more than one such a concept in a single field, for example, in modern cosmology, which equally involves classical and relativistic theory, which are classically causal, and quantum theory, which may not be and is not, in nonrealist interpretations. But then, is this field really single?

5.2 Classical Causality: Philosophy

I begin with an astute and far-reaching remark of Heisenberg's, made in his discussion of Kant's argument for the idea and the law of causality, as given a priori, rather than derived from experience. This argument, Heisenberg contends, no longer applies in "atomic [quantum] physics" (Heisenberg 1962, pp. 89–90). Heisenberg, however, makes a broader point:

> 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)

Helped by insights gained from quantum physics, Heisenberg's criticism of Kant has a point. Indeed, Kant, if he could have benefited from these insights, might have agreed with Heisenberg. Apart from the belief that it will be possible to arrive at some absolute truth by pure reason [*reine Vernunft*], a claim on which Kant has been challenged from the moment he made it, Kant is not that far from Heisenberg. First of all, although not embedded in "a mathematical scheme," Kant's analysis is also characterized by a search for a sharper definition and the limits of applicability of his concepts, in part through establishing the connections between them. Also, to briefly reprise the discussion of Kant given in Chap. 1, Kant argued that concepts, either those given a priori, such as space, time, and causality, or those established from experience, are operative primarily in the *phenomenal* domain (that of what appears to our thought), governed by the faculty of Understanding [*Verstand*], where these concepts are generally more effective. Their capacity to reach, *understand*, the *noumenal*, the character of things-in-themselves, as they actually exist in nature or mind (including, as will be seen in Chap. 7, when it comes to mathematics), is limited, and the truth of their claims concerning noumena is never guaranteed. Kant, again, allows a greater capacity for reaching the noumenal and thus for realism at the ultimate level, to the faculty of Reason [*Vernunft*]. (As earlier, I capitalize "Understanding" and "Reason" when using them in Kant's sense of each as a philosophical rather than a common daily concept.) It is this capacity that Heisenberg questions, again, helped, by quantum mechanics and the spirit of Copenhagen, which teaches us that human thought cannot represent and possibly even conceive of the ultimate nature of things. Kant, again, gives Reason and, as concerns (the representation of) nature, even Understanding more power than they arguably warrant.

He believed both Euclidean geometry and Newtonian mechanics to be true, but contrary to a common view of his argumentation, not without qualifications, even if against his own grain. One might argue that Kant rather thought that geometry and physics are unlikely to be, rather than in principle cannot be, different. Kant's views concerning both are complex and are, I surmise, unlikely to be definitively established, and it is not my aim to do so here. My point is only that there are complexities to his philosophy that deserve more credit than they are customarily given, sometimes even in philosophical commentaries, outside the scholarship on Kant and sometimes even in this scholarship. As discussed in Chap. 1, even though Kant does give Reason a greater chance and even certainty in reaching the ultimate reality of things, he recognized that our thinking, even by means of Reason, let alone Understanding, concerning noumena might be wrong, even if it works in practice (Kant 1997, p. 115). As we now know, this is what happens in classical physics. On the one hand, the distinction between phenomenal and noumenal entities and processes, although technically valid, could be disregarded and is disregarded by the idealized models of classical physics, most immediately classical mechanics, which also allows one to consider these processes as causal within these models and mathematize them accordingly. On the other hand, the ultimate, or at least underlying, nature of these processes is, in the current view, quantum and, hence, classical causality does not or, at least, may not apply at this level. This knowledge, however, had to wait until quantum theory. Nevertheless, while Kant, as Hume before him, believed that the ultimate nature of the world is causal, Kant, even given the power he assigned to Reason, was ambivalent concerning whether the human mind could have an access to this causality and establish definitive *causal* connections between real events, rather than surmise *probable* connections between them. Hume, who did not have a concept analogous to Kant's Reason, denied the human mind the capacity to reach the true nature of these connections altogether. It is worth noting that, although Kant understood the significance of probability in practical matters, he argued against the use of the idea of probability in critical philosophy, especially metaphysics, because foundations of a philosophical theory should be certain, he believed, using geometry as an example (Kant 1997, pp. 661–662). This is not surprising, given that his main mathematical and physical models were Euclidean geometry and Newtonian mechanics, respectively, and these models leave little space to probability. On the other hand, Hume was more open to probability. In classical physics, at least or more expressly classical mechanics, causality, again, works because we can, ideally, disregard the difference between the noumenal and the phenomenal, as well as the influence of our instruments, beginning with our bodies, upon what is observed. As discussed earlier, classical statistical physics and chaos or complexity theories (naturally, not familiar to Kant), introduced further complexities, which are, however, of practical rather than fundamental nature, because at bottom these theories are causal. In sum, as we have seen throughout this study, in classical physics and its models realism and causality work together. By contrast, in quantum mechanics, at least in nonrealist, RWR-principle-based, interpretations, what is practically justified is that no representation or even conception of the quantum-level reality and thus the ultimate reality of nature as it is currently understood is possible. The absence of classical causality is, again, automatic.

Kant, nevertheless, deserves credit for questioning the limits of the idea of causality, even, according to Nietzsche, who had little else positive to say about Kant, and who came to question these limits far beyond Kant, even reaching, arguably for the first time, the RWR principle. Nietzsche emphatically stated: "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 might not have been giving enough credit to Hume for similarly exploring these limits, but he is right about Kant and about the fact that "to this day we are not done with this fixing of limits." Nietzsche was undoubtedly also thinking of thermodynamics and (classical) statistical mechanics, which had a profound impact on his philosophy. Quantum theory confirmed this assessment, and it does not appear that we are likely to fix these limits any time soon. As we have seen throughout this study, understanding these limits in theoretical physics involves complex negotiations of the experimental data, mathematical formalism, and philosophical thinking. It is only through these negotiations that the nature of the concept and principle of causality and their limits in physics can be established more firmly, even if never completely, as Heisenberg rightly argues.

I shall now consider the concept of classical causality in more detail, starting from the following dictionary definition of causality: "*Causality* is the relationship between an event (the *cause*) and a second event (the *effect*), where the second event is a direct consequence of the first" (*The Random House Webster's Unabridged Dictionary* 2005). This is a good definition, essentially adopted, as that of *classical causality* as an ontological category (connecting events), by this study as well, and it is not so easy to significantly improve on it. While most definitions in philosophical literature, from Hume and Kant, or even Plato and Aristotle, on, do refine it and probe the limits within which it applies, they retain the key elements of this definition, which is, of course, in turn indebted to the philosophy of causality. According to Plato, in the *Phaedo*, for example, our "inquiry into nature" is a search for "the causes of each thing; why each thing comes into existence, why it goes out of existence, why it exists" (Plato 2005, *Phaedo*, A 6–10; cited in Falcon 2015). One can make this definition more general by extending the application of terms "cause" and "effect" to entities (individual or, since there may be more than one cause to a given effect, collective), A and B, other than events, and without requiring that A and B should be entities of the same kind. This generalization is useful in physics. For example, the gravity of the Sun and other bodies in the solar system, which is cumbersome, but possible, to define as a spatiotemporal event or manifold of events (also in the mathematical sense of "manifold," say, that of a phase space) can be seen as the cause of the motion of planets, and hence of *events* in this motion. The physical nature of gravitation is not completely known, and it was not known at all at the time of Newton, who reluctantly assumed it to propagate instantaneously in space and time and hence as not local (one needs a concept of gravitation field, such as that adopted by general relativity to make it local). Such considerations show the complexity of the relationships between causality and succession, as well, and especially, the difficulties of using the idea of the ultimate cause in physics and elsewhere: the ultimate origins of things are not known, or, again, even conceivable. The very concept of origin, any such concept we can form, may not apply to these ultimate "origins."

M. Born offers the following definition: "Causality postulates that there are laws by which the occurrence of an entity B of a certain class depends on the occurrence of an entity A of another class, where the word entity means any physical object, phenomenon, situation, or event. A is called the cause, B the effect" (Born 1949, p. 9). Although one may need a set of sequential or parallel causes for a given event, and hence the concept of the complete set of causes to properly define entity A here, Born's and most other standard definitions of causality can be easily adjusted to accommodate this qualification. Born adds two other (again, common) postulates, that of antecedence and that of contiguity: "Antecedence postulates that the cause must be prior to, or at least simultaneous with, the effect" and "Contiguity postulates that cause and effect must be in spatial contact or connected by a chain of intermediate things in contact" (Born 1949, p. 9). Both of these postulates would follow from the locality principle, which would enable one to express them more rigorously. For example, one might ask with Heidegger: "What is a thing?" in this formulation, and what does it mean for things to be in contact? (Heidegger 1967). As Heidegger explains in this book, beginning with Galileo, classical physics, as a mathematical-experimental science of nature, changed the way this question is asked in science (Heidegger 1967, p. 93). Indeed, he argues that the pre-Socratics asked this question very differently from Plato, who is closer to modern science in asking it by virtue of his appeal to mathematics as a paradigmatic model for a philosophical inquiry into the nature, or "thing-ness," of things. By starting with "effect," Born de facto formulates the *principle* of causality, which states that if an event takes place, it has a cause of which it is an effect. This principle is crucial for both the application of and the *critical* analysis of causality, from Hume and Kant on.

Thus, while Kant defines, similarly to the dictionary definition cited above, the relation of causality as that of, first, the cause and, secondly, the effect, the principle of causality proceeds from the effect to the cause. Kant says: "the concept of the *relation of cause and effect*, the former of which determines the latter in time, as its consequence [*Folge*], as something that could merely precede it in the imagination (or not even be perceived at all)" (Kant 1997, p. 305). By contrast, as suggested already by Plato's passage from the *Phaedo* cited above, the *principle* of causality proceeds from effects to causes: "If, therefore, we experience that something happens, then we always presuppose that something else precedes it, which it *follows* in accordance with a rule" (Kant 1997, p. 308). Kant is careful to qualify that "the logical clarity of this representation of a rule determining the series of occurrences, as that of the [particular] concept of cause, is possible if we have made use of it in experience." "But," he concludes, "a consideration of [causal representation], as the condition of the synthetic unity of the appearances in time, was nevertheless the ground of experience itself, and therefore preceded it a priori" (Kant 1997, p. 309).

This conclusion has been challenged, from the time of the publication of Kant's first *Critique* on, but this is a secondary matter at the moment, as opposed to the implication, against Kant's own grain, that causality may not apply to the ultimate workings of nature.

In Kant, the principle also implies that under the same conditions identical events will take place and, thus, that, in science, identical experiments will lead to identical outcomes, a contention fundamentally challenged by quantum physics, where only the statistics of sets of identically prepared experiments are repeatable. Hume, too, uses this aspect of causality in his empirical ("regularity") theory of causality in nature, in which case he allows for a meaningful application of the concept (e.g., Dowe 2007, pp. 18–21). While Hume doubted the validity of the concept of causality at the human level more than Kant, he, as noted above, appears to have a similar view concerning nature as ultimately causal. There is, again, some debate on this point among scholars as concerns Hume's view, which might be argued only to allow for "the relation of contiguity and succession … [as] independent of, and antecedent to the operations of the [human] understanding," rather than causality (Hume 1978, pp. 168–196). However, Hume's empirical regularity theory of causality does appear to suggest the ultimate causality in nature as "independent of our thought and reasoning" along with the antecedent nature of causality, implied by the successive nature of this relation (Hume 1978, pp. 168–169).[1] In sum, Hume's and Kant's critiques of causality apply primarily to our claims concerning causality and not to the architecture of the world itself assumed to be causal, even though the human mind can perceive this architecture at most only partially, via the regularity of certain causal conjunctions of events, which lead to this critique. Both Hume and Kant also maintain the antecedent and, hence, asymmetric relationships between cause and effect, which is, again, customary, but as will be seen below, not uniform.

Kant's philosophy of causality has been and remains the paradigmatic approach to *classical causality*. As indicated in Chap. 1, there are several reasons to adopt this terminology, beginning with the fact that the historical period of the Enlightenment (roughly the eighteenth century), is sometimes referred to as the Classical Age, and also as the Age of Reason. Kant was a major figure of this period and was largely responsible for the term "Enlightenment," apparently introduced in his famous essay "Answering the Question: What is the Enlightenment?" (Kant 1991). The terminology also correlates with the term "classical physics," the models of which are defined, at their ultimate levels (ideally represented by classical mechanics), by classical causality, although the term classical physics was introduced in the beginning of the twentieth century, in juxtaposition to relativity and quantum physics. Newtonian mechanics was a key development of the Classical Age, and, along with Euclidean geometry, Kant's main scientific inspiration. Philosophically, the classical and classically causal view of the world may be illustrated by a famous excerpt from A. Pope's *An Essay on Man* and his, equally famous, proposed epitaph for

[1] Cf. Dowe, who cites the passage (Dowe 2007, pp. 18–20).

Newton. The first passage is a variation on Leibniz's theme of pre-established harmony, an important conception in the history of classical causality. Pope says:

> All Nature is but art, unknown to thee;
> All chance, direction, which thou canst not see;
> All discord, harmony not understood;
> All partial evil, universal good:
> And, spite of pride, in erring reason's spite,
> One truth is clear: Whatever IS, is RIGHT.
> (Pope 1985, p. 129, *An Essay on Man*, Epistle 1, ll. 289–294)
> Nature and Nature's laws lay hid in night;
> God said: "Let Newton be!" and all was light.
> (Pope 1985, p. 92, "Proposed Epitaph for Isaac Newton, who died in 1727")

The epitaph is of more interest, first, because Newton's physics was a paradigmatic example of, and a justification for, the classical view in science, as captured by a mathematical model representing the physical world, and in philosophy, which, beginning with John Locke, followed it and the search for conceptually, if not mathematically, the same type of model in philosophy. There were some prominent exceptions, to be sure, Bishop Berkeley, for one, or for that matter, Leibniz, as concerns the model in its Newtonian specificity, but not as concerns causality or realism. As noted from the outset of this study, this situation (in which a given model might be challenged but the assumption of realism and causality would not be) became paradigmatic in physics, or science, and philosophy alike, pretty much until quantum theory. Secondly, there is a subtle shift of perspective in the epitaph vis-à-vis the first passage, although this perspective is found in *An Essay on Man* as a whole. The epitaph reflects the Enlightenment belief, culminating in Kant's concept of Reason, in the human capacity to perceive the ultimate nature of things with a great degree of approximation, even if never completely. The human genius, for which the genius of Newton stands, is sufficient for understanding the laws of material and human nature alike, as ultimately set by God, who is, however, not dead yet. Pope was no Nietzsche.

As discussed earlier, while we do use measuring instruments, beginning with our bodies, in classical physics, and while, correlatively, the idealization of classical mechanics rigorously applies only to phenomena, in accordance with Kant's view, we can, again, correlatively, neglect or circumvent the influence of measuring instruments and consider the behavior of objects as independent, and thus treat phenomena as objects. This situation defines the realist idealization and (idealized) model of classical mechanics, which is also assumed to be causal, an assumption well confirmed within the limits of this idealization or model.[2] Thus, to return to the example of the Solar system, *within the limits of this idealization*, one can maintain both:

[2] It is worth reiterating here that, while classical statistical physics, chaos and complexity theory, electrodynamics, and relativity may complicate this view, these theories retain classical causality and, in the first place, realism at the ultimate level of their models.

(a) that the configuration of the Solar system, defined by its gravity, at a given time causes any subsequent event in the motion of a given body, say, a planet, such as Mars, in this system (barring outside interferences); and
(b) that, if the Sun and other bodies in the solar system were not there, the motion of a given planet would not be observed in the way it is.

In other words, both the definition of cause given above and the principle of causality are permissible and are assumed within the limits of the idealization of classical mechanics. The latter, moreover, usually presupposes both antecedence ("the cause must be prior or simultaneous with the effect") and contiguity ("cause and effect must be in spatial contact or connected by a chain of intermediate things in contact"), which is why Born invokes them. These requirements were strengthened by relativity, which restricted the propagation of physical influences by the speed of light in a vacuum. Relativity 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. As noted in Chap. 1, sometimes the term "causality" is used in physics to designate compliance with this requirement. The relativity requirements, or antecedence and contiguity, do not depend on the *principle* of classical causality. All three requirements are satisfied in quantum mechanics, including in nonrealist, RWR-principle-based, interpretations and thus in the absence of classical causality. In this sense, just as quantum reality is without realism, quantum causality (properly defined below) is without classical causality, and as such, again, divorces probability from causality or is one of the outcomes of this divorce.

As indicated above, following Kant, it had been a generally accepted requirement for scientific practice or even, in Heisenberg's words, "the basis of all scientific work," the basis in turn justified and even established by classical mechanics, that identically prepared experiments lead to identical outcomes, the basis in turn established in classical mechanics (Heisenberg 1962, pp. 89–90). This repetition is also possible there because the statistical errors in repeated experiments can in principle be neglected, which fact is crucial to establishing the idealization and model of classical mechanics, beginning even with Galileo. The situation is different in the case of quantum phenomena, say, resulting in an emission of a photon by an electron or what we so interpret from observing a spot on a photographic plate. Although we do know, with reasonable certainty, that such events occur, claims *(a)* or *(b)* above could not be made with certainty, as in classical mechanics, but only with a certain probability, predicted by quantum mechanics. Technically, as noted earlier, one cannot guarantee even that the emission occurred in any given case, which cases are, however, statistically negligible. In other words, while one can, in a certain sense, always speak of quantum "effects," their causes are similar to the way *ultimate* causes function in philosophy or in classical physics. Even if such ultimate causes existed, their connections to effects cannot be meaningfully tracked down physically, even if one adopts a causal theory of quantum phenomena, either by assuming a causal interpretation of quantum mechanics or by using an alternative theory, such as Bohmian mechanics. In classical physics, this difficulty of tracking ultimate causes is usually handled by establishing spatial and temporal frames that limit a given case to those causes that can be meaningfully tracked down, at least, again, in principle. In quantum theory, however, this type of framing does not appear possible, even for individual events, which, as discussed, are not comprehended by laws. At least they are not comprehended by a causal law, but, if one adopted a statistical interpretation of quantum phenomena and quantum mechanics, like the statistical Copenhagen interpretation, SCI, even by any probabilistic law. In this case, as discussed in Chap. 4, individual events are random and cannot be assigned probabilities; there are only statistical regularities, including correlations of a character not found in classical physics (certain classical events may of course be correlated, usually causally). These regularities allow for the statistics of sets of identically prepared experiments to be repeatable and in this sense objective. Something has to be repeatable for us to be able to do science.

Historically, Kant's or Hume's views concerning causality, which accord with the preceding history of philosophy, are of course not surprising. Although probability theory was sufficiently advanced by then and had its impact in physics and beyond, the ultimate nature of the world was uniformly interpreted, just as it had been before, in terms of the underlying but unknown and perhaps unknowable classically causal architecture, as it was for example by Laplace or, it appears, by T. Bayes. The latter is an interesting case, which I shall put aside here, beyond noting that, while his view of probability appears to exhibit certain affinities with the Bayesian philosophy of probability, especially on account of his arguably subjective understanding of probability, they should not be identified with the latter, which is a twentieth-century development (Stigler 2000). As noted in Chap. 1, the possibility of questioning causality and even (more rarely and usually partially) some actual questioning of it, has been entertained as early as the pre-Socratics and had resurged now and then during modernity, in part under the influence of Lucretius's *De Rerum Natura*, the most sustained exposition of ancient atomism (Lucretius 2009). Ultimately, however, classical causal ontology has remained dominant, even with the rise of modern atomism, and then the development of kinetic theories of gases and thermodynamics, and even the existence of atoms have continued to be debated into the early twentieth century. Einstein's early work in statistical physics was still also a response to those, Mach among them, who questioned this existence. Until the twentieth century and quantum physics, very few, such as Nietzsche (late in the nineteenth century, though), doubted that some form of classical causality would be at work in the ultimate constitution of the world. Bohr, as we have seen, did not fail to note this fact: "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" (Bohr 1938, p. 94). Einstein's immense contributions, mentioned earlier, to statistical physics and the statistical aspects of the old quantum theory notwithstanding, Einstein never gave up on the classical ideal of causality, and following Einstein, many and even most still hope that this will ultimately prove to be the case. As noted from the outset of this study, the spirit of Copenhagen has always been and remains a minority view. According to S. J. Gould:

> I confess that, after 30 years of teaching at a major university [Harvard], I remain surprised by the unquestioned acceptance of this [causal] view of science—which, by the way, I strongly reject …—both among students headed for a life in this profession, and among intellectually inclined people in general. If, as a teacher, I suggest to students that they might wish to construe probability and contingency as ontological properties of nature, they often become confused, and even angry, and almost invariably respond with some version of the old Laplacean claim. In short, they insist that our use of probabilistic inference can only, and in principle, be an epistemological consequence of our mental limitations, and simply cannot represent an irreducible property of nature, which must, if science works at all, be truly deterministic [causal, in the present definition]. (Gould 2002, p. 1333)[3]

One should not be too surprised, given the dominance of the classical—realist and causal—view throughout Western intellectual history, and especially, the hope, inspired by Einstein, who never wavered in his views, that the classical ideal of causality will be restored to fundamental physics sooner or later. The dominance of classical thinking should not be unexpected either, even apart from its extraordinary effectiveness in philosophy and science throughout Western history before the rise of quantum theory and, in most areas of physics, even after it. It may even be argued, as it was, as noted earlier, by Bohr and Heisenberg, that classical thinking reflects the essential workings of our neurological machinery born in our evolutionary emergence as human animals and possibly helping our survival. In other words, our thinking in general, as the product of this machinery, appears to be realist and causal, which may explain why Kant was compelled to assign to causality, along with space and time, an a priori nature.[4] This, as noted throughout, is true even if quantum mechanics is interpreted in accordance with the RWR principle. One is compelled to infer the existence of quantum objects as unrepresentable and even unthinkable from certain configurations of their effects on something (manifested in measuring instruments or their equivalents) that we can think and know, unavoidably through classical thinking. This inference is, thus, *a product of theoretical thinking*, reached via classical thinking, because the nonclassical, RWR-principle-based, underpinnings of a given situation make their existence apparent only in certain classical features as effects of these underpinnings. The particular character of these effects, including their probabilistic or statistical and, sometimes, statistically correlational nature, appears to defy the possibility of a realist and causal understanding of their emergence and compels at least those who follow the spirit of Copenhagen to think of this emergence via the RWR principle. Given the evolu-

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[3] It must be qualified, that contingency (a concept that I haven't used thus far) is not the same as randomness, which would prevent the use of probability in the case of single random events. Contingency is instead the interplay of randomness and causality, or more accurately and more broadly regularity (which is not always causal). It is the interplay of that which is governed by a law and that which inflects this law, as does indeed any particular case in which the law applies, or inhibits or prevents its application, or makes it inapplicable, thus, requiring a new law, to remain contingent rather than random. The question is what is the nature of the law and the regularities it reflects, in particular, whether this law is causal or not, and that of evolution may not be, just as the laws of quantum mechanics, in nonrealist interpretations, are not.

[4] Recent neurological research appears to support this view as well (e.g., Berthoz 2000, 2003).

tionary origin of classical thinking, it is hardly surprising that it was so pervasive in Western thought and culture, and that it has ultimately become a form of ideology.

Although they followed and promoted this ideology and, especially Kant, played a shaping role in making it dominant, Hume and Kant, again, deserve credit for their realization of the complexities and limitations of classical causality, and for their critical analyses of these complexities, aimed at the lack of sensitivity to these limitations and unwarranted extrapolations of classical causality. It is not a question of abandoning classical causality, which would, again, be impossible in any event, if, along with realism, it reflects our biological and neurological nature. Besides, both realism and causality, retain their positive and even indispensable role in nonrealist thinking. Instead, as Nietzsche said in his comment on Kant cited above and as has always been maintained by the best nonrealist thinkers, such as Bohr, the question is one of demarcating the limits of realism and classical causality, and, beyond these limits, of possibly establishing other concepts of causality, as I shall attempt to do in Sect. 5.4. First, however, I would like to consider further the role and limits of classical causality in classical physics and relativity.

5.3 Classical Causality: Physics

The main reason that classical causality works in classical mechanics is that, within the idealization of the theory, the state of the object at a given moment in time may be shown to define the states of this object at all future moments in time, *within the range of the system's history as defined for a given case*. In other words, the situation is in accord with the definition of classical causality, at least, ideally, which, however, is, again, all that is required in modern physics. The state of the system is defined by its position and momentum, both of which can be, ideally, measured and predicted, and therefore considered as properly definable and determinable at the same time at any given point in the evolution of the system. Indeed, the present state is equally *defined by* the past states, and it would allow us to make definitive conclusions concerning past states and predict all future states, within the frame of a given experiment. I am reluctant to say that the present state of the system also *defines* all its past states. For, although the equations of classical mechanics allow us to know these past states, physically the relation of determination between states proceeds from the past or present to the future in most applications of the model of classical mechanics.

As I shall discuss presently, while it is easier to speak of physical causes, defined by the laws of classical mechanics, in relation to which all states of the system are effects, viewing the relationships between such states themselves as those between causes and effects requires further qualifications. These qualifications will also explain why the assumption that the cause precedes or is, at most, simultaneous with the effect is appropriate to the idealization of classical mechanics, or in relativity, where this assumption is amplified by the finite limit on the propagation of physical influences, as explained above. Mathematically, the equations of classical mechanics or relativity are time reversible, as are the equations of quantum mechanics, although this claim requires additional qualifications in quantum mechanics.[5] This reversibility may suggest (and it does to some) that time reversal and backward-in-time causality are possible. In the present view, it seems more reasonable to exclude both from the idealization of classical mechanics or that of relativity, or that of quantum mechanics (where both ideas are sometimes entertained as well), because there is, thus far, neither experimental evidence nor, at least to the present author, other compelling reasons to consider them. I shall return to the subject below.

Some speak of classical causality as "deterministic causality," which, as explained earlier, is not out of place (given that all classically causal systems are defined by individual constituents described by classical mechanics or electromagnetism, which are deterministic when applied to individual systems), or again, beginning with Laplace, "determinism" (e.g., Dowe 2007, p. 18). I prefer, in accordance with the definition given in Chap. 1, to understand determinism as having to do with our capacity to make predictions concerning the behavior of a given system. The causal character of classical mechanics, at least in dealing with individual classical objects or, in certain but not all circumstances, a small number of objects, allows for exact, deterministic predictions concerning individual systems in many practical cases, when we can neglect unavoidable practical deviations (in view of the limited capacity of our measuring instruments) from strictly exact predictions. Under these conditions causality and determinism, as defined here, again, coincide. As discussed earlier, however, this is no longer the case when we are dealing with more complex systems, such as those considered in classical statistical physics or chaos theory (that of three bodies under gravitational attraction suffices to require chaos theory). While such theories or the corresponding models are ultimately causal or are underlain by causal models (that of classical mechanics in the case of the behavior of the individual constituents of the systems considered in classical statistical physics), we have no capacity to make exact predictions concerning their behavior. With due adjustments, required by the postulate that the speed of light in a vacuum, c, is constant and independent from the speed of the motion of the source, classical causality also applies in special relativity, or in general relativity. Both are classically causal locally. This postulate 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. In other words, no physical causes can propagate faster than c. This requirement may be called relativistic causality, and it is satisfied in in low-energy quantum regimes governed by quantum mechanics (which, as such, is not a relativistic theory) or quantum field theory (which is relativistic), without requiring classical

[5] For one thing, one would need to replace the variables involved with their complex conjugate. I shall bypass these qualifications, because in nonrealist, RWR-principle-based, interpretations, these equations have only a predictive role and do not describe any physical processes in space and time, and as such always only concern the future and tell us nothing about the past. Hence, their mathematically time-reversible character has no physical significance.

causality there. Unlike relativity itself, special or general, these theories are not classically causal locally. In fact, relativistic causality thus defined is closely related to locality. It implies locality as concerns physical influences between spatially separated systems.

The question of cause and effect in classical mechanics has, however, further complexities. One could still say that, for each given measurement and the corresponding prediction, the state of the object in question established by this measurement at a given moment in time is a cause of, and thus determines, its future state, as an effect, at any given future moment. This statement, however, is only true insofar as this causal determination is enabled and defined by the laws of motion used, say, Newton's laws, and the corresponding equations, which are assumed to reflect certain physical forces in nature itself. This *determination* mathematically idealizes the corresponding physical configuration in nature. One might say that the *real* physical cause for any determination, including that of the initial state that defines a given situation, is the gravitational field defined by the Sun and other corporeal objects or fields in the Solar system. From this viewpoint, a given state of any single object can only be seen as a *physical* cause of its future states insofar as the whole configuration of bodies and forces involved, which determines the law of motion and hence of causality, is considered as part of this state. On the other hand, one might see these factors as built into the state of an object as defined by its position and momentum, at each point. Then one might say that each given state is a cause of all subsequent states, as effects, with the *law* of causality defined by the laws of motion for this system.

In addition, the history of a classical system, thus considered, only goes so far in a given representation, and thus, as indicated above, involves the suspension of the ultimate cause or even many more remote causes. Newton bracketed the physical nature of and hence the causes of gravity and was (wisely) content to merely take its force into account in his law of gravity. This bracketing allows one to apply this law, including as a law of causality, as part of the overall "legislature," as it were, of causality for a given classical system, defined by Newton's law of gravity and other laws of Newton's mechanics. While this application is only possible within those limits where we need not be concerned with the physical nature of gravity itself, these limits are very broad and allow us to consider a large number of physical systems. In most applications of classical physics, the earlier history of a given system, say, that of the emergence of the Solar system, is bracketed as well, although some cases assume large spatial and temporal frames, all the way to the scale the Universe, at least up to a point. Once one gets closer to the Big Bang, the practical use of the model becomes difficult, even if one remains within the classical scheme. But, at least once galaxies are formed, one can have good, albeit limited, approximations and assessments using Newton's theory of gravity.

Einstein's general relativity has, to some degree, resolved the problem of the nature of gravity, but only to a degree, given that the ultimate nature of gravity may be quantum. In this case, our theory of gravity may and, on the view adopted here, would require the suspension of classical causality at the ultimate level. Just as in classical physics, however, for many practical cases in which we use relativistic gravity, the *ultimate* nature of gravity is not crucial. On the other hand, the difference between general-relativistic and Newtonian laws of gravity *is crucial*, even in explaining the behavior of the motion of planets, such as, famously, Mercury, in the Solar system. Classical causality, however, applies, with certain qualifications, in general relativity as well, as does relativistic causality, again, with certain adjustments. Thus, the speed of light in a vacuum, *c*, while still a limit on all physical processes, is now only constant locally and the Lorentz invariance only applies locally as well. At the same time, as already noted, both special and general relativity are classically causal locally.

As I said, it is usually assumed that there is no backward-in-time physical influence, even though the equations of classical physics are mathematically symmetrical with respect to time reversal. In addition, these equations or those of relativity do not provide for the concept of now, defined only from the outside by the clocks we use and our consciousness, a circumstance much pondered by Einstein throughout his life. This assumption, along with the historical framing just defined, makes classical causality related to, but not the same as, the temporal division of past and future. Causes always precede effects: bodies, such as planets, move in a gravitational field, such as that of the solar system, because of the earlier history of this field, even though these bodies contribute to this field. Relativistic considerations, again, impose further restrictions on causality.

It is argued sometimes that general relativity and quantum mechanics suggest and even imply the possibility of retroaction in time and backward-in-time causality (e.g., Dowe 2007, p. 188). I do not find these arguments sufficiently compelling in either case. First of all, there is, thus far, no experimental evidence for either retroaction in time or backward-in-time causality. It is true that there are legitimate hypothetical arguments for the possibility of retroaction in time and, by implication, for the corresponding concept of causality. Among them is the existence of closed time loops in K. Gödel's solutions of the equations of general relativity ("Gödel's metric"), K. Thorne's wormhole "time-machines," the hypothetical existence of tachions (particles that travel only faster than light in a vacuum, which is not technically forbidden by relativity), and a few others. It is a different question *how* compelling these arguments are and to whom. For, while most arguments against retroaction in time do not altogether rule it out, the problems of the assumption remain serious on well-known logical and physical grounds. In addition, there is, again, no experimental evidence supporting the idea. Its main *physical* appeal appears to be that it may "solve" certain, actual or presumed, problems of the current quantum theory, from quantum mechanics to quantum field theory to (as yet not developed) quantum gravity. Its main *philosophical* appeal is that *some* physicists entertain the idea on the grounds just stated. Given that retroaction in time cannot be completely ruled out by our current theories, one could of course explore the corresponding notions of causality. It is also true that both relativity and quantum theory taught us that we should not trust our general (everyday) or even philosophical intuition in fundamental physics. Accordingly, one might agree with Dowe's *general* contention that "it will not do for the philosopher to rule out a priori what the scientist is currently contemplating as a serious hypothesis" (Dowe 2007, p. 188). It does not appear to me, however, that there are sufficiently compelling physical reasons (such as those mentioned above) to pursue the possibility of retroaction in time, which Dowe has *specifically* in mind here, while there are more compelling reasons against this possibility.[6] In other words, *how seriously* this particular hypothesis is contemplated by scientists is not altogether clear, and I would argue that it is not *very* seriously entertained widely, although it is by some. Given that our current fundamental theories are incomplete, it is possible that retroaction in time or backward-in-time causality might one day be shown to be a feature of nature. For now, however, although the equations of classical physics or relativity are symmetrical with respect to time reversal, the assumption that there is no retroaction in time and no corresponding causal influence is reasonable and, within a wide range, effective in physics.

In sum, classical causality, applied at the level of idealized mathematical models (which, however, define all post-Galilean theoretical physics), is workable in classical mechanics and, with certain qualifications (not fundamental in nature), elsewhere in classical physics and relativity. However, the application of these models is limited by spatial, temporal, and other frames we impose, although one might also see these frames as parts or parameters of the model. In other words, in accordance with Heisenberg's argument with which I began here, although by connecting classical causality to other concepts of classical physics we can define this concept and its limits more sharply, we still do not know, at least not completely, how far classical causality ultimately extends in physics. It does appear, however, that classical causality is likely to have a limited domain of application in physics. In particular, it may not apply at all either on very small scales, in view of quantum physics, or on very large scales, for a complex set of reasons, which include the apparently quantum origins of the universe and the ultimately quantum character of gravity. In other words, a kind of causal picture that Laplace and others envisioned for the universe is unlikely to apply. The idea of an overall causal universe, including one based on the classical concept of causality, is by no means completely abandoned, however. It was, for example, advocated by G. 't Hooft and others, sometimes, under the rubric of superdeterminism, for the reasons motivated by the complexities of the EPR-Bell type experiments and Bell's theorem (e.g., 't Hooft 2003). The

[6] Dowe's argument for backward-in-time causality in quantum mechanics is primarily motivated by a philosophical discontent, which is, again, common, with interpretations in the spirit of Copenhagen (the RWR-principle-based ones, beginning with that of Bohr, would be among them) and certain attempts to address the problems posed by the EPR experiment and Bell's theorem (Dowe 2007, pp. 182–183). To the present author, these reasons, again, do not appear to be sufficient to resort to backward-in-time causality, given the difficulties of applying it in physics, as explained above. Dowe's gloss of "the Copenhagen interpretation" hardly does justice to the views of most followers of the spirit of Copenhagen, and specifically those of major figures, such as Bohr, whose view on the subject is nothing like it would appear from Dowe's gloss. Dowe's overall argument is shaped by a rather limited view of the history of the question of causality in physics and philosophy, a view, by and large, restricted to the Anglo-American analytic philosophical tradition, and a few earlier authors, such as Hume. Remarkably, Kant is not considered. Nor are Nietzsche or the American pragmatists, such as C.S. Peirce and W. James, who offered important critiques of the idea of causality. None of the founding figures of quantum theory is discussed either.

proponents of superdeterminism, certainly 't Hooft, do not think in terms of anything like classical mechanics in considering the behavior of individual quantum systems, even though 't Hooft does want to depart from standard quantum mechanics or quantum field theory. Similarly, while Bohmian and other causal quantum theories are different from classical mechanics, they are classically causal. There are, again, also causal interpretations of quantum mechanics.[7] It appears difficult to sustain such interpretations, especially if one wants to avoid certain, at least in the present view, undesirable consequences, such as nonlocality or backward-in-time causality. It is notable that most such extensions of the concept of causality, for example, again, that proposed by Dowe, which he, again, sees as enabling a better approach to explaining the EPR-Bell correlations than more standard alternatives, are essentially classical in their conceptual architecture. In Dowe's case, this extension is developed with the help of H. Reichenbach's fork mechanism (Dowe 2007, pp. 192–209). The only difference is that backward-in-time causation is now allowed. This difference is of course crucial physically. Once again, however, in my view, there do not appear to be sufficiently compelling reasons to adopt retroaction in time and backward-in-time causality, in contrast to the standard view of classical causality, which is compelling for many reasons and which is manifestly workable within large limits.

On the other hand, it appears to me that there are good reasons to ask the following question. Assuming a nonrealist, RWR-principle-based, interpretation, which precludes classical causality, and assuming both spatial locality and the absence of retroaction in time or backward-in-time causality (temporal locality), is it possible to introduce a different concept of causality? The answer, I would argue, is yes, and I shall now propose such a concept.[8]

5.4 Quantum Causality: Causality and Complementarity

As explained in Chap. 2, if one adopts a nonrealist, RWR-principle-based, interpretation of quantum phenomena and quantum mechanics, the nature of both experimental and theoretical physics changes. I shall discuss this change further in Chap. 7, restricting myself for the moment to the most essential point, most pertinent to quantum causality that I am about to introduce. Experimentally we no longer track, as we do in classical physics or relativity, the independent behavior of the systems considered. In other words, we no longer track what happens in any event, by however ingenious experiments. Instead we *define* what will happen in the

[7] I shall leave these interpretations aside here, because they, or Bohmian theories, tell us little new about causality, even if one adopts backward-in-time causality, which has a classical architecture, and thus leaves the philosophical basis of classical causality in place.

[8] I have proposed this type of concept previously in (Plotnitsky 2011a). The present concept, however, departs from this earlier concept along several lines, especially by linking this concept to both locality and complementarity.

experiments we perform, by how we *experiment* with nature by means of our experimental technology, even though and because we can only predict what will happen probabilistically or statistically. Thus, in the double-slit experiment, the two alternative setups of the experiment, whether we, respectively, can (by way of using one experimental device or another) or cannot know, even in principle, through which slit each particle, say, an electron, passes, we obtain two different outcomes of the statistical distributions of the traces on the screen (with which each particle collides). Or, thus also giving a rigorous meaning to the uncertainty relations, we can set up our apparatus so as to measure and correspondingly predict, again, probabilistically or statistically, either the position or the momentum of a given quantum object, but never both together. Either case requires a separate experiment, incompatible with the other, rather than merely representing an arbitrary selection of either type of measurement within the same physical situation, by tracking either one of its aspects or the other in the way we do in classical mechanics. There, this is possible because we can, at least in principle, measure and assign simultaneously both quantities within the same experimental arrangement. In quantum physics we cannot.

It is this determination, probabilistic or, if one adopts the statistical Copenhagen interpretation, statistical though it is, of what can or conversely cannot happen by virtue of our experimental decision at any given moment in time that defines what I call "quantum causality." Or rather, given that quantum events or phenomena may occur without our staging of quantum experiments, this determination is an instance of quantum causality in the case of human quantum experiments. This instance, however, provides a model of the more general definition, which may, also as a principle, be formulated as follows, and which, as discussed in Chap. 3, was in effect crucial to Bohr's argument in his reply to EPR and beyond, although Bohr did not speak in terms of quantum causality. *Whatever happens as a quantum event and is registered as such (thus providing us with the initial data) defines a possible set of, in general probabilistically or statistically, predictable outcomes of future events and irrevocably rules out the possibility of our predictions concerning certain other, such as and in particular complementary, events*.

At the same time, each such event completely erases any data obtained in any preceding events as meaningful for the purposes of our predictions concerning future events from this point on, and thus resets our expectation-catalog (Plotnitsky 2009, pp. 73–76). There is nothing that can help us to improve the probabilities of our predictions, neither the information previously obtained by measurements on the same object, nor a repetition of the same experiment with another quantum object of the same kind with the identical preparation, in the way it can be done in classical physics. On the one hand, no connection to any past event can ever be guaranteed in the case of individual quantum events (again, no mechanical cause of any quantum event can be found) and no exact repetition that establishes classical-like regularity of events is possible. This situation clearly excludes both classical causality and, automatically, backward-in-time causality. On the other hand, quantum events that have already occurred do define future quantum events in strong, even if probabilistic or statistical, terms, until, in each such case, the next experiment is performed, which will irrevocably change our expectation-catalog. Quantum causality thus manifestly divorces probability from classical causality, and causality from determinism. The nature of the second break, briefly mentioned in Chap. 1, becomes clearer now. In classical physics or relativity classical causality and determinism coincide in the case of individual physical processes, while in quantum mechanics, in nonrealist interpretations, our predictions concerning elemental quantum processes are probabilistic or statistical as well, just as they are in the case of all other quantum processes. Accordingly, whatever quantum causality there may be, it is probabilistic and statistical, and thus is free, divorced, from both classical causality and determinism.

Bohr's complementarity is obviously in accord with this concept of quantum causality, which also gives a fitting meaning to Bohr's view of complementarity as a generalization of causality as discussed earlier, especially in the context of the EPR experiment and the EPR complementarity. My subtitle in this section is Bohr's twice repeated (in part for this reason) conjunction of complementarity and causality (Bohr 1937, 1958). The first of these articles presents Bohr's ultimate, fully RWR-principle-based, interpretation for the first time, and the second is Bohr's last exposition of this interpretation and the last essay, at least the last finished and published essay, on complementarity.

The essence of complementarity, in particular as a principle, is also predictive, probabilistically or statistically predictive, as everything significant is in quantum physics. On the one hand, stemming from "our freedom of handling the measuring instruments, characteristic of the very idea of experiment" in all physics, classical or quantum (or of course relativity), our "free choice" concerning what kind of experiment we want to perform is essential to complementarity (Bohr 1935, p. 699). On the other hand, as against classical physics or relativity, implementing our decision concerning what we want to do will allow us to make only certain types of predictions and will exclude the possibility of certain other, *complementary*, types of predictions. It is in this sense that complementarity is a generalization of causality, in the absence of both classical causality and, in the first place, realism, because, as just explained, it defines what reality can and cannot be brought about as a result of our decision concerning which experiment we perform. The predictions defined by complementarity will still be probabilistic or statistical—expectation-catalogs—reflecting the probabilistic or statistical aspects of complementarity or the uncertainty relations, and indeed, as I argue in this study, making probability and statistics part of its conceptual architecture. A given measurement associated with any complementarity, a measurement of always only one and not the other complementary variable, will be (ideally) exact. Thus, complementarity is a manifestation of quantum causality, which is a more general concept because it would also apply to quantum predictions that are not related to any complementarity.

As discussed above, in order to effectively apply classical causality in classical physics and relativity, we impose artificial frames, most especially spatial and temporal ones, but also others, for example, by bracketing the atomic or quantum constitution of the physical objects considered. In some cases, our spatiotemporal frames may extend quite far, for example, in the history of the Solar system or even the known universe itself, nearly to its origin, some 14 billion years ago—nearly, but not quite, because the very early, pre-Big-Bang, history of the Universe appears to be quantum. If such is the case, however, *in the present view,* our mathematical machinery, technology, will not be able to provide us with the description or representation of this early, pre-Big-Bang, history. This, as noted in the preceding chapter, does not mean that there is nothing that we can say about these early stages of the Universe as quantum events by means of inference from the traces left by the Big Bang, in its classical aspects, if not the preceding quantum history. It is also possible, however, that, at the next stage of our interminable and interminably inconclusive encounter with it, nature will show itself to be either more classical or will confront us with something more mysterious than quantum physics or anything we can imagine now. Indeed, as will be seen in the next chapter, quantum field theory, beginning with quantum electrodynamics, already requires a more radical departure from classical thinking than does quantum mechanics.

Chapter 6
The Principles of Quantum Theory, Dirac's Equation, and the Architecture of Quantum Field Theory

Abstract This chapter considers the role of the fundamental principles of quantum theory and principle thinking in quantum field theory, beginning with quantum electrodynamics. The fundamental principles of relativity will be addressed as well, in view of their role in quantum electrodynamics and quantum field theory. Dirac's work, in particular his derivation of his relativistic equation of the electron by combining the principles of relativity and quantum theory, is the main focus of this chapter, in parallel with Heisenberg's work as the main focus in the discussion of quantum mechanics in Chap. 2. Heisenberg's work, especially his paper introducing quantum mechanics, which Dirac studied very carefully, was a major influence on Dirac's thinking throughout, I would argue, all of his work. This influence, however, does not diminish the originality and creativity of Dirac's thinking, which, ultimately, led him to the discovery of his equation for the relativistic electron and antimatter, one of the greatest discoveries of fundamental physics. After a general introduction given in Sect. 6.1, Sect. 6.2 addresses Dirac's discovery of his equation. It argues, along the lines of the argument developed in Chap. 2 in considering Heisenberg's work, that Dirac's discovery was that of a mathematical machinery, technology, responding to and, in some key respects, specifically as concerns the role of antimatter, anticipating the architecture of high-energy quantum phenomena, as manifested in the experimental technology that defines them. Although not a quantum field theory or even quite quantum electrodynamics, Dirac's theory of the electron, based in his equation, provided some of the key physical, mathematical, and epistemological ingredients of quantum electrodynamics or quantum field theory as a viable nonrealist theory, to be considered here in terms of a particular concept of quantum field. Dirac's equation, which expressly considered electrons as particles, was not a field equation, but given its essentially quantum-field-theoretical nature, it would also be difficult to see it merely in terms of relativistic quantum mechanics of particles, as some suggest. Sec. 6.3 addresses the architecture of quantum field theory, as grounded, in addition to the QD and QP/QS principles, which are, just as in quantum mechanics, primary, in the combination of the RWR principle and the particle-transformation, PT, principle. The PT principle emerged as a result of Dirac's discovery of antimatter, an unintended consequence of his

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equation. New concepts of both "elementary particle" and "quantum field" are, I argue, required by the combination of both principles, and I shall suggest such concepts here. These concepts allow one to pose and relate the questions "What is a quantum field?" and "What is an elementary particle?" in a new way, even if not answer them. Sec. 6.4 discusses the role of renormalization, and comments on the future of quantum theory and fundamental physics given the current state of quantum field theory.

6.1 Introduction

This chapter addresses the role of the fundamental principles and principle thinking in high-energy quantum physics and quantum field theory, and their implications for, to return to Milton's phrase, our understanding of the "dark materials" through which nature continuously creates new order in our world and new worlds, possibly even new Universes, either sequential or parallel.[1] Most (even if not all) of the currently entertained possibilities for new fundamental theories, in particular those, such as string and brane theories, that aim to incorporate gravity (currently treated by means of general relativity, which is a classical-like field theory), are grounded in quantum field theory or maintain relationships with it.[2] Dirac's work, most especially his 1928 discovery of his relativistic equation for the electron, is my main example of principle thinking in this chapter and my point of departure for considering the principles of quantum field theory. In the next chapter, I discuss the derivation of Dirac's equation from the principles of quantum information theory by G. M. D'Ariano and P. Perinotti (D'Ariano and Perinotti 2014). The principles of quantum field theory both extend the principles of quantum mechanics and introduce new principles, one of which, the particle-transformation principle, or the PT principle, is particularly important. For the sake of economy, throughout this chapter and the next chapter, both which discuss and compare all these theories, I shall refer to quantum mechanics as QM, quantum electrodynamics as QED, and quantum field

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[1] The subject of multiple parallel or (both terms are used) alternative Universes, which entered fundamental physics with the many-worlds interpretation of quantum mechanics, has acquired new prominence (on different grounds) in the wake of string and brane theories, leading, unsurprisingly, to a controversy and, equally unsurprisingly (given the appeal of the idea and its controversial nature) to numerous popular expositions. See, for example, (Susskind 2006) and (Vilenkin 2007), which consider two among current proposals, and (Greene 2011), which offers a more comprehensive survey of the subject. These books also contain further references, including to technical literature. See also "Multiverse" (*Wikipedia*). For an alternative, "sequential," view of Universes, each emerging from the preceding one, see (Penrose 2012). I shall, however, not be concerned with these subjects here.

[2] These relationships were given a new dimension by J. Maldacena's work on the so-called AdS/CFT (Anti-de-Sitter/Conformal-Field-Theory) correspondence. See "AdS/CFT Correspondence" (*Wikipedia*). For a technical introduction, see (Năstase 2015).

theory as QFT.[3] (Quantum theory, which refers to any theory dealing with quantum phenomena will not be abbreviated.)

Dirac's work, including that leading him to his discovery of his equation, was influenced by and, in some of its key respects, followed Heisenberg's thinking in his work on quantum mechanics, as discussed in Chap. 2. Just as Heisenberg's discovery, that of Dirac was enabled by his confidence in the principles of quantum theory and illustrated the power of these principles. The discovery also represents one of the most remarkable and intriguing episodes in the history of quantum theory, which I shall now recount by way of a prologue.

Dirac's equation contained a major difficulty (or what appeared as such when the equation was introduced) inherited from its short-lived predecessor, the Klein-Gordon equation, even though Dirac's equation was a major advance and was seen as such by Dirac and others. In particular, unlike the Klein-Gordon equation, Dirac's equation enabled him to answer, for the relativistic electron, the following question, the main question of quantum theory: "What is the probability of *any dynamical variable* at any specified time having a value laying between any specified limits, when the system is represented by a given wave function ψ_n?" Dirac clarified that the Klein-Gordon theory "can answer such questions if they refer to the position of the electron … but not if they refer to its momentum, or angular momentum, or any other dynamic variable" (Dirac 1928, pp. 611–612; emphasis added). There was, however, a major problem equally found in both theories. According to Dirac:

> [Either equation] refers equally well to an electron with charge e as to one with charge $-e$. If one considers for definitiveness the limiting case of large quantum numbers one would find that some of the solutions of the wave equation are wave packets moving in the way a particle of $-e$ would move on the classical theory, while others are wave packets moving in the way a particle with charge e would move classically. For this second class of solutions W has a negative value. One gets over the difficulty on the classical theory by arbitrarily excluding those solutions that have a negative W. One cannot do this on the quantum theory, since in general a perturbation will cause transitions from state with W positive to states with W negative. Such a transition would appear experimentally as the electron suddenly changes its charge from $-e$ to e, a phenomenon which has not been observed. The true relativi[stic] wave equation should thus be such that its solutions split up into two noncombining sets, referring respectively to the charge $-e$ and the charge e.
>
> In the present paper we shall only be concerned with the removal of the first of these difficulties [the Klein-Gordon equation's inability to predict the probability of all dynamical variables of the system represented by a given wave function]. The resulting theory

[3] I shall only be concerned with standard versions of QED and QFT, and not with alternative formalisms, for example, those along the lines of Bohmian mechanics. I will also not discuss algebraic quantum field theories (AQFT), although much of my argument would apply to them. For an introduction to the current state of QFT, including AQFT, see (Kuhlman 2015) and references there, which include most standard physical and philosophical treatments of the subject, such as, to give a representative physical and a representative philosophical example (Weinberg 1996; Teller 1995). An exceptionally lucid nontechnical account of QED is given by Feynman (Feynman 1985). For a clear and elegant nontechnical account of more advanced developments, such as quantum chromodynamics (QCD), and some future prospects, see (Wilczek 2009).

> [embodied in Dirac's equation] is therefore still only an approximation, but it appears to be good enough to account for all the duplexity phenomena without arbitrary assumptions. (Dirac 1928, p. 612)

Dirac's theory inherits this problem of the Klein-Gordon theory because mathematically Dirac's equation may be seen as a "square root" of the Klein-Gordon equation, which means that every solution of Dirac's equation is a solution of the Klein-Gordon equation. The opposite is, of course, not true, which reflects the fact that the Klein-Gordon equation ultimately does not work. The difficulty does not appear in the low-energy regime, or rather, it disappears at the low-energy limit, because Dirac's equation converts into Schrödinger's equation, where this problem does not arise. One might briefly register Dirac's appeal to the correspondence principle for large quantum numbers, where our observation could indeed be made with classical definitiveness, by disregarding the uncertainty relations and the role of measuring instruments, even though the processes corresponding to these quantum numbers are still quantum, which fact indeed leads to the effect in question. In commenting on this problem in his *The Physical Principles of the Quantum Theory*, Heisenberg reprised Dirac's assessment and even amplified it by saying that Dirac's "theory is *certainly* wrong" (emphasis added). Heisenberg added an intriguing twist. He said: "The classical theory could eliminate this difficulty by arbitrarily excluding the one sign, but this is not possible to do according to *the principles of quantum theory*. Here spontaneous transitions may occur to the states of negative value of energy E; as these have never been observed, *the theory is certainly wrong*. Under these conditions it is very remarkable that the positive energy-levels (at least in the case of one electron) coincide with those actually observed" (Heisenberg 1930, p. 102; emphasis added). "The theory is certainly wrong"—no less! He was soon to change his mind about it.

Dirac's theory proved to be better than it appeared at the time of its introduction even to its creator. Indeed, it has proven to be correct. It was the understanding of nature in high-energy quantum regimes assumed at the time that was deficient. That, in general, a perturbation will cause transitions from states with positive E to states with negative E, and that such a transition would appear experimentally as the electron suddenly changes its charge from –e to e, is what actually happens, and it will have been experimentally established in a year or so. This feature was inherent in all high-energy quantum processes, and was a reflection of the fact that particles are born and disappear, and transform into each other in these regimes, which gives rise to the particle transformation (PT) principle in QED and QFT. Antimatter was staring into Dirac's and other theoretical physicists' eyes. It took, however, a few years to realize that it was antimatter and that this type of transition (eventually understood in terms of the creation and annihilation of particles, and virtual particle formation) defines high-energy quantum physics.[4]

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[4] The key steps leading to this understanding, and some of them, such as Dirac's hole theory, were quite ingenious in their own right, are well known and have been discussed in literature (e.g., Schweber 1994, 56–69). W. Furry and R. Oppenheimer's 1934 paper (Furry and Oppenheimer 1934) was, arguably, the first to present the current view, stated here and discussed in Sect. 6.3,

This is one of the great stories of quantum theory. My main interest, however, is another facet of Heisenberg's thinking transpiring here, to which I referred above as "an intriguing twist" of his assessment. This facet was equally found in Dirac's thinking. What is intriguing but also crucial (more crucial than intriguing) is that, while Heisenberg and Dirac had their doubts concerning Dirac's theory itself, neither appears to have doubted "the *principles* of quantum theory." This confidence was reflected in the titles (neither accidental not lightly chosen) of Heisenberg's and Dirac's books (both published in 1930), invoked from the outset of this study and giving it its title. Dirac's theory might have been wrong, but the principles of quantum theory had to be right. It might have been a matter of coming up with a different theory, based on these principles, but not a matter of replacing these principles themselves, which had demonstrated their validity and efficacy in quantum mechanics. As it happened, Dirac's theory proved to be correct, although far from the end of the story of QED and QFT, which has continued to conform to the principles of quantum theory in question. They still do. Heisenberg and Dirac proved to have been right in this confidence.[5]

Dirac's adherence to the principles, physical, mathematical, and philosophical (however implicit the latter might have been in Dirac's thinking, not known for its philosophical inclinations), of both relativity and quantum mechanics defined his thinking in his work on quantum theory. His faithfulness to these principles became especially crucial in his work on QED and his equation for the relativistic electron. The introduction of this new mathematical formalism was a momentous event in the history of quantum physics. It was comparable to that of Heisenberg's introduction of his matrix variables, also based in the key physical, mathematical, and philosophical principles of quantum mechanics, some of which Heisenberg also established (others came courtesy of Bohr). Bohr saw "Dirac's ingenious quantum theory of the electron," as, on the one hand, "a most striking illustration of the power and fertility of the general quantum-mechanical way of description," begun with Heisenberg's matrix mechanics, and, on the other, as reflecting "new fundamental features of atomicity, which are ultimately connected with nonclassical aspects of quantum statistics expressed in the exclusion principle, and which demanded a still more radical [than in quantum mechanics] renunciation of explanation in terms of a pictorial representation" (Bohr 1987, v. 2, p. 63).[6] Heisenberg was even more

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although, as will also be discussed in Sect. 6.3, earlier work of Dirac, Jordan, and others on second quantization was important for developing this view as well.

[5] Later on, Dirac lost his confidence in QED. This was not because he lost his faith in the fundamental principles of quantum theory, but rather because of the theory's inability to give these principles a proper (which for Dirac also meant mathematically elegant) mathematical expression. However, inelegant and even messy, and, to some, mathematically questionable, if legitimate at all, as it might be, the theory initiated by Dirac proved to be a remarkable success, from antimatter to the Higgs boson. I shall return to this Janus-like nature of QED and QFT below.

[6] At this point (the statement dates from 1949), by "new fundamental features of atomicity" Bohr clearly refers to his concept of atomicity, perhaps influenced as much by the developments of QED, as by the development of quantum mechanics. QED and QFT played a major role in the development of Bohr's thinking concerning quantum theory and complementarity. Bohr made a

emphatic. He saw Dirac's theory as an even more radical revolution than quantum mechanics was. 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," an assessment often repeated in his later writings (Heisenberg 1989, pp. 31–33). The same volume contains Heisenberg's companion article, also written in the 1970s entitled "What is an Elementary Particle?," still an unanswered question, even if by now a better-asked one, not the least thanks to Dirac's pioneering work, on which the subsequent elementary particle physics has continued to build (Heisenberg 1989, pp. 71–88).[7]

This chapter argues for a nonrealist, RWR-principle-based, view of both QED and QFT, which extends this study's view of QM and (because of new features and principles introduced by these theories) makes this view even more radical. It also offers a conceptual and historical support for this argument, *from the principle perspective of this study*. This chapter does not aim to offer a comprehensive conceptual and historical analysis of QED or QFT, and will only cite a limited number of works especially pertinent to this argument, with the aim of shedding a new light on the key foundational aspects of both theories. Although still far outnumbered by the philosophical literature on QM, where the number of books and articles is nearly apocalyptic in number, the philosophical literature on QFT has significantly increased during the last two decades or so. This literature continues to reflect major difficulties in providing a realist or (this term is more commonly use in considering QFT) ontological interpretation of QFT or of the corresponding (high-energy) phenomena themselves, although much of this literature either aims to offer such an interpretation or argue for it to be desirable or even imperative. This aim is shared with and extends most philosophical and sometimes physical, approaches to QM. It is debatable that prospects for realism are better in QFT. One the other hand, for reasons to be explained in this chapter, one could see that QFT poses even greater challenges to realism than does QM. By the same token, these challenges encourage those who, like the present author, are inclined toward a nonrealist view of quantum theory, and who also see the QFT situation as an additional reason to take a nonrealist view of QM. Consider M. Kuhlman's conclusion, "Taking Stock: Where do we Stand?," of his helpful article (with manifestly realist preferences and offering an extensive survey of various forms of realism in QFT and debates concerning them) on QFT (Kuhlman 2015). The "we" in this question is of some interest, and may not be as general as it may appear, because it seems to refer only to those (admittedly, again, a majority) who aim at realist interpretations of QFT and, in some cases, even assume that otherwise one cannot meaningfully speak of an interpretation of a physical

major contribution to the field by a classic treatment of quantum-field measurement in his collaborations with Rosenfeld (Bohr and Rosenfeld 1933; Bohr and Rosenfeld 1950). The latter, an updated version of their 1933 article, takes into account the intervening developments, including the renormalization of QED, accomplished just then. I shall return to this work below. For a detailed treatment of Bohr's engagement with QED and QFT, see (Plotnitsky 2012a, pp. 89–106).

[7] This is clearly shown by Weinberg's 1996 "update" under the same title (Weinberg 1996), and many subsequent works, some of which I shall cite below.

theory. Kuhlman's assessment and his article overall are defined by the difference, justified historically, between the particle and the field interpretations of QFT. Kuhlman says:

> A *particle interpretation* of QFT answers most intuitively what happens in particle scattering experiments and why we seem to detect particle trajectories. Moreover, it would explain most naturally why particle talk appears almost unavoidable. However, the particle interpretation in particular is troubled by numerous serious problems. There are no-go theorems to the effect that, in a relativistic setting, quantum "particle" states cannot be localized in any finite region of space-time no matter how large it is. Besides localizability, another hard-core requirement for the particle concept that seems to be violated in QFT is countability. First, many take the Unruh effect to indicate that the particle number is observer or context dependent. And second, interacting quantum field theories cannot be interpreted in terms of particles because their representations are unitarily inequivalent to Fock space (Haag's theorem), which is the only known way to represent countable entities in systems with an infinite number of degrees of freedom.
>
> At first sight the *field interpretation* seems to be much better off, considering that a field is not a localized entity and that it may vary continuously—so no requirements for localizability and countability. Accordingly, the field interpretation is often taken to be implied by the failure of the particle interpretation. However, on closer scrutiny the field interpretation itself is not above reproach. To begin with, since "quantum fields" are operator valued it is not clear in which sense QFT should be describing physical fields, i.e., as ascribing physical properties to points in space. In order to get determinate physical properties, or even just probabilities, one needs a quantum state. However, since quantum states as such are not spatio-temporally defined, it is questionable whether field values calculated with their help can still be viewed as local properties. The second serious challenge is that the arguably strongest field interpretation—the wave functional version—may be hit by similar problems as the particle interpretation, since wave functional space is unitarily equivalent to Fock space. (Kuhlman 2015)

Before concluding, Kuhlman remarks on "the two remaining [realist] contestants" that "approach QFT in a way that breaks more radically with traditional ontologies than any of the proposed particle and field interpretations," which, however, have manifest difficulties of their own. The first is "Ontic Structural Realism": "Ontic Structural Realism (OSR) takes the paramount significance of symmetry groups to indicate that symmetry structures as such have an ontological primacy over objects. However, since most OSRists are decidedly against Platonism, it is not altogether clear how symmetry structures could be ontologically prior to objects if they only exist in concrete realizations, namely in those objects that exhibit these symmetries" (Kuhlman 2015). The second approach, advocated by Kuhlman himself (Kuhlman 2010), is Dispositional Trope Ontology (DTO). "[The latter] deprives both particles and fields of their fundamental status, and proposes an ontology whose basic elements are properties understood as particulars, called 'tropes.' One of the advantages of the DTO approach is its great generality concerning the nature of objects which it analyzes as bundles of (partly dispositional) properties/tropes: DTO is flexible enough to encompass both particle and field like features without being committed to either a particle or a field ontology" (Kuhlman 2015).

While Kuhlman does not discuss the difficulties of this approach, he nevertheless concludes, by indicating general difficulties of any realist or ontological interpretation of QFT, difficulties that, I would contend, arise from and extend those found in QM. He says: "In conclusion one has to recall that one reason why the ontological interpretation of QFT is so difficult is the fact that it is exceptionally unclear which parts of the formalism should be taken to represent anything physical in the first place. And it looks as if that problem will persist for quite some time" (Kuhlman 2015). This may be true if one wants or, as Kuhlman appears to do (and he is hardly alone), requires a realist or ontological interpretation. But would one necessarily want such an interpretation and why would one want or require it? The reasons (pretty much the same as in the case of QM) for wanting or requiring it have already been discussed at some length in this study, and there is no need to do so yet again here. A better question, albeit already posed by this study as well in the case of QM, may be: Is such an interpretation even possible? The answer to the question "Which parts of the formalism should be taken to represent anything physical in the first place?" may be "None at all!" Just as in the case of QM, the question of the *possibility* of realism or ontology in QFT precedes the question of which ontology is more suitable.

6.2 Dirac's Equation: Combining the Principles of Relativity and Quantum Theory

Dirac's thinking concerning the problem of the free relativistic electron was based in the fundamental principles of both relativity and quantum theory, those of quantum theory represented by QM. Dirac's starting point was that, in order to equally satisfy, and to mathematically express, the physical principles of both theories, the equation he needed to find had to have certain specific mathematical features. While, however, formally mathematical, these features reflected and emerged from fundamental physical principles of both theories, which mathematically expressed these principles. Dirac followed Heisenberg and the spirit of Copenhagen insofar as his aim was the invention of a consistent mathematical scheme by means of which one could predict, probabilistically or statistically, the outcomes of relevant experiments, without being concerned, at least overtly, with representing the behavior of the relativistic electron in space and time. Dirac's theory was a principle rather than constructive theory, which contained all of the fundamental principles of QM, interpreted in the spirit of Copenhagen: the QD principle, applied to the observed high-energy quantum phenomena, the QP or (depending on interpretation) QS principle. The status of the RWR principle in Dirac's thinking is a more complex matter. Dirac appears to have always retained the view, considered in Chap. 3, that he introduced in his paper on transformation theory: the probabilistic or statistical nature of quantum predictions is due to our interference into quantum processes by measurement, while the undisturbed behavior of quantum objects is causal and is represented as such by the formalism of quantum theory (QM, QED, or QFT) (Dirac 1927a). Dirac never addressed the difficulties this claim entails, for example, given the presence of complex quantities in the formalism, which are difficult to associate with what occurs in space and time by means other than moving from (complex) amplitudes to (real) probabilities, via Born's rule. Whatever Dirac's views concerning this situation might have been, his formalism is open to a nonrealist, RWR-principle-based, interpretation, which will be adopted here. It was the conformity of Dirac's theory to the QD principle, invoked by Dirac at the outset of his paper, that compelled Bohr to speak, in the passage cited above, of Dirac's theory as an "illustration" of "new fundamental features of atomicity," clearly in Bohr's sense of the term. Bohr's concept of atomicity or (they are, again, equivalent) phenomenon is, however, firmly linked to the RWR principle, which "*in principle* exclude[s]" realism (Bohr 1987, v. 2, p. 62).

The mathematical features in question were as follows. The first feature, required by the *principles of special relativity theory*, mathematically expresses in Einstein's relativistic kinematics, was that time and space must enter symmetrically and be interchangeable, which implies the locality principle but is specific to special relativity. This feature is not found in Schrödinger's nonrelativistic equation for the electron, because it contained the first derivative of time and the second derivatives of coordinates, although the equation satisfies locality, as does, at least in the present view, quantum mechanics. As will be discussed in Chap. 7, D'Ariano and Perinotti derive Dirac's equation by only using the locality principle (and other quantum-informational principles), without using the principles of special relativity (D'Ariano and Perinotti 2014). Dirac's equation automatically satisfy the locality principle, but commonly only as a consequence of the principles of relativity, expressed by the mathematical structure of his equation. By contrast, in D'Ariano and Perinotti's derivation, the principles of special relativity, specifically the Lorentz invariance, enter Dirac's equation as the consequences of the locality principle (applicable universally and thus on Planck's scale, where the Lorentz invariance may not apply) and appear, as they must in Dirac's equation, at its proper (Fermi scale) of QFT (D'Ariano and Perinotti 2014, p. 1).

The second feature, required by *the principles of quantum mechanics*, as embodied in the mathematical formalism of the theory, was that the equation Dirac needed must be *first-order* linear in time, just as Schrödinger's equation was. This feature is connected to several other key features of the formalism, again, mathematically expressing certain physical principles, which had important epistemological implications, such as the QD and QP/QS principles, and possibly the RWR principle.[8] Among these features were the noncommutativity of certain quantum variables, linear superposition, and the conservation of the probability current, which entails positive definite probability density (the QP/QS principle) and which, combined with the first order derivative in time, may be seen as unitarity, following (D'Ariano and Perinotti 2014). The QP/QS principle is given a mathematical expression in Dirac's theory *analogously* to the way it was by Heisenberg in quantum mechanics, but within a more complex mathematical formalism employed by Dirac.

[8] As discussed earlier, at the time, Heisenberg and others, including Bohr, only assumed a form of proto-RWR-principle, or even allowed for a certain residual realism, even while seeing the role of measurement as irreducible.

Another key affinity with Heisenberg's approach was Dirac's use of the mathematical correspondence principle, defined by the requirement that at the classical limit the equations of quantum mechanics convert into those of classical physics. Dirac used the mathematical correspondence principle in his earlier work on quantum mechanics, inspired by Heisenberg's paper. Indeed, while Heisenberg was the first to use it, it was Dirac who appears to have been the first to expressly formulate it as the mathematical correspondence principle. In his first paper on quantum mechanics, he said: "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, p. 315). Applying the principle in the case of his equation meant that at the nonrelativistic quantum limit this equation would convert into Schrödinger's equation. Proving this fact was mathematically more difficult than in Heisenberg's case, where the application of the principle was nearly automatic because Heisenberg used classical equations. (One still needed to show that matrix variables convert into classical variables.) Dirac's use of the principle also suggests the following extension of it. The mathematical formalism of a given higher-level quantum theory should at the lower limit convert into the mathematical formalism of the corresponding lower-level theory (usually already established). Thus, for example, in the case of string or brane theory, this means that its equations should, at the corresponding lower limit, covert into those of QFT.[9] As will be seen, D'Ariano and Perinotti's derivation of Dirac's equation might be understood as an enactment of this generalized mathematical correspondence principle, because Dirac's equation, which belongs to the usual (Fermi) scale of high-energy physics, "emerges from the large-scale [extending to Planck's scale] dynamics of the minimum-dimension QCA [quantum cellular automaton]" (D'Ariano and Perinotti 2014, p. 1). That said, however, unlike Heisenberg (who borrowed his equation from classical physics, while using new quantum variables, which he invented), Dirac did not directly use the mathematical correspondence principle, as opposed to other quantum principles just mentioned, to *derive* his equation. It was a new equation even formally, in this respect more akin to Schrödinger's equation.

Dirac's thinking leading his to his equation was further shaped by the following interrelated factors:

1. *The influence of Heisenberg's thinking leading to the discovery of quantum mechanics, as considered in* Chap. 2. As is well known, Dirac studied Heisenberg's paper introducing quantum mechanics very carefully, and it left, I would argue, a lasting impact on all of his thinking and work, including his founding papers on QED and his hole theory (e.g., Schweber 1994, pp. 24–32). As discussed in Chap. 2, Heisenberg's paper was in certain ways preliminary, and was developed into a full-fledged matrix mechanics a few months later by

[9] The situation, again, acquires a new dimension in view of the AdS/CFT correspondence (see note 2).

Heisenberg himself, M. Born, and P. Jordan. Dirac, unfamiliar with this subsequent work, arrived at his own, equally full-fledged, version, based in his *q*-number formalism, independently (Dirac 1925). However, as also explained in Chap. 2, Heisenberg's paper manifestly embodied the fundamental principles of quantum mechanics.

2. *Dirac's work on the transformation theory, his "darling," as he called it* (Dirac 1962b). It was introduced by Dirac in 1926, while at Bohr's Institute in Copenhagen, and independently discovered by Jordan at the same time (Dirac 1927a; Jordan 1926a, b). The transformation theory was especially important for Dirac's work on his equation, as concerned linearity in $\partial/\partial t$, and positive definite probability density, both equally central to the transformation theory, which, at the time, encoded the principles of quantum mechanics most generally, because it encompassed both Heisenberg's and Schrödinger's mechanics.
3. Dirac's 1926–1927 work on QED (short of its relativistic form), yet another of his major contributions (e.g., Dirac 1927b).

These factors made Dirac better prepared than others at the time for the discovery of his equation, which is, again, not to say that the originality of his thinking had not played a decisive role in this discovery.

Although Dirac's logic described above seems eminently reasonable in retrospect, it appears that only Dirac thought of the situation in this way at the time. His famous conversation with Bohr that occurred then is revealing:

> Bohr: What are you working on?
> Dirac: I am trying to get a relativistic theory of the electron.
> Bohr: But Klein already solved that problem. (Dirac 1962b)

Dirac disagreed, and, for the reasons just explained, it is clear why he did and why Bohr should have known better. The Klein-Gordon equation, to which Bohr referred, is relativistic and symmetrical in space and time, but it is not a first-order linear differential equation in either, because both variables enter via the second derivative, $\frac{\partial^2}{\partial t^2}, \frac{\partial^2}{\partial x_i^2}$. One can derive the continuity equation from it, but the probability density is not positive definite. By the same token, the Klein-Gordon equation does not give one Schrödinger's equation

$$i\hbar \frac{\partial}{\partial t}\psi(r, t) = \left[\frac{-\hbar^2}{2m}\nabla^2 + V(r, t)\right]\psi(r, t)$$

in the nonrelativistic limit, where m is the particle's mass, V is potential energy, ∇^2 is the Laplacian, and ψ is the wave function (the position-space wave function). Schrödinger, who appears to be the first to have written down the Klein-Gordon equation in the process of his discovery of his wave equation, abandoned the Klein-Gordon equation in view of the incorrect predictions it gave in the nonrelativistic limit. In other words, the Klein-Gordon equation does not convert into Schrödinger's equation in the nonrelativistic limit. On the other hand, Dirac's equation, a square root of the Klein-Gordon equation, converts into Schrödinger's equation in the nonrelativistic limit, which, again, was a major factor in Dirac's thinking. Thus, Dirac's equation conforms to the mathematical correspondence principle, applied in relativistic quantum-theoretical regimes. Technically, at its immediate nonrelativistic limit, Dirac's theory converts into Pauli's spin-matrix theory, while Schrödinger's equation, which does not contain spin, is the limit of Pauli's theory, if one neglects spin (Pauli 1927). Thus, the Klein-Gordon equation was not a right way of bringing the principles of relativity and quantum theory together. Dirac found the right way to do so by taking a square root of the Klein-Gordon equation, which may not be so difficult by current mathematical standards of theoretical physics, but was nontrivial at the time. As a result, as noted earlier, Dirac's equation enabled him to answer, for the relativistic electron, the question, "What is the probability of *any dynamical variable* at any specified time having a value laying between any specified limits, when the system is represented by a given wave function ψ_n?" which the Klein-Gordon theory could only answer for "the position of the electron … but not [for] its momentum, or angular momentum, or any other dynamic variable" (Dirac 1928, pp. 611–612; emphasis added). This is the main question of all quantum theory, defined by or defining the QP/QS principle, and, if one also adopts the RWR principle, it is the only question one can meaningfully ask concerning the behavior of quantum objects. For Einstein, who developed a keen interest in Dirac's mathematics (but not physics), the main question of any fundamental theory would be: How do the dynamical variables of the theory represent the corresponding "elements of reality" so these elements could be predicted ideally exactly, rather than probabilistically or statistically in the absence of such a representation?

Dirac's mathematical task was more difficult than that of Heisenberg, because the considerations just outlined required both new variables, as in Heisenberg's scheme, and, in contrast to Heisenberg's scheme (which used the equations of classical mechanics), a new equation. Hence, as I said, Dirac didn't use the mathematical correspondence principle to derive his equation, but only to show that it converts into Schrödinger's equation at the quantum-mechanical limit. Dirac did, however, use the Klein-Gordon equation, of which he, again, took a square root, to satisfy the necessary principles of quantum mechanics and to give them a proper (relativistic) mathematical expression. As those of Heisenberg's matrix mechanics, Dirac's new variables proved to be noncommuting matrix-type variables, but of a more complex character, involving the so-called spinors and the multicomponent wave functions, the concept discovered by Pauli in his nonrelativistic theory of spin (Pauli 1927). Just as Heisenberg's matrices, Dirac's spinors had never been used in physics previously, although they were introduced in mathematics by W. C. Clifford about 50 years earlier (following the work of H. Grassmann on exterior algebras). And just as Heisenberg in the case of his matrices, Dirac, too, was unaware of the existence of spinors and reinvented them.[10]

[10] Einstein, as I noted, developed a major interest in Dirac's equation, as a spinor equation, and he used it, in his collaborations with W. Mayer, as part of his program for the unified field theory,

In spite of the elegant simplicity of its famous compact form,

$$i\gamma \cdot \partial\psi = m\psi,$$

reproduced on the plate in Westminster Abbey commemorating Dirac, Dirac's equation encodes an extremely complex Hilbert-space machinery. The equation, as introduced by Dirac, was

$$\left(\beta mc^2 + \sum_{k=1}^{3}\alpha_k p_k c\right)\psi\left(x, t\right) = i\hbar\frac{\partial\psi\left(x, t\right)}{\partial t}$$

The new mathematical elements here are the 4×4 matrices α_k and β and the four-component wave function ψ. The Dirac matrices are all Hermitian,

$$\alpha_i^2 = \beta^2 = I_4$$

(I_4 is the identity matrix), and the mutually anticommute:

$$\alpha_i\beta + \beta\alpha_i = 0$$
$$\alpha_i\alpha_j + \alpha_j\alpha_i = 0$$

The above single symbolic equation unfolds into four coupled linear first-order partial differential equations for the four quantities that make up the wave function. The matrices form a basis of the corresponding Clifford algebra. One can think of Clifford algebras as quantizations of Grassmann's exterior algebras, in the same way that the Weyl algebra is a quantization of symmetric algebra. Here, $\boldsymbol{p}$ is the momentum operator in Schrödinger's sense, but in a more complicated Hilbert space than in standard quantum mechanics. The wave function $\psi\,(t, \boldsymbol{x}\,)$ takes value

conceived as a classical-like field theory, modeled on general relativity, and in opposition to quantum mechanics and, by then, quantum field theory. Accordingly, he only considered a classical-like spinor form of Dirac's equation, thus depriving it of (Einstein might have thought "freeing" it from) its quantum features, most fundamentally, discreteness (h did not figure in Einstein's form of Dirac's equation), and probability. Einstein hoped but failed to derive discreteness from the underlying field-continuity. As noted above, by this point Einstein abandoned the principle approach in favor of the constructive approach or rather, given that he still used fundamental principles, a mixed approach anchored in the constructive one. His use of Dirac's equation was part of this new way of thinking. He was primarily interested in the mathematics of spinors, which he generalized in what he called "semivectors" (van Dongen 2010, pp. 96–129). It is worth noting that, unlike Einstein, O. Klein (for example, in his version of the Kaluza-Klein theory) always took quantum principles, especially discreteness, as primary, rather than aiming, as did Einstein, or earlier Schrödinger, to derive quantum discreteness from an underlying continuity of a conceptually classical-like field theory. This is hardly surprising coming from a long-time assistant of Bohr. Klein's thinking, which led to several major contributions, was always quantum-oriented. It is just that the Klein-Gordon equation did not manage to bring quantum theory and relativity together successfully. The equation itself was later used in meson theory. Of course, Dirac's equation, too, was a unification of quantum mechanics and special relativity, but not of the kind Einstein wanted.

in a Hilbert space $X = C^4$ (Dirac's spinors are elements of X). For each t, $y\ (t, \boldsymbol{x}\ )$ is an element of $H = L^2\ (R^3; X) = L^2\ (R^3) \otimes X = L^2\ (R^3) \otimes C^4$. This mathematical architecture allows one to predict the probabilities of quantum-electro-dynamical (high-energy) events, which, as discussed below, have a greater complexity than quantum-mechanical (low-energy) events.

Beginning with Heisenberg, finding new matrix-type variables or, more generally, Hilbert-space operators became the defining mathematical element of quantum theory.[11] The current theories of weak forces, electroweak unifications, and strong forces (quantum chromodynamics, QCD) were all discovered by finding such variables. This is correlative to establishing the transformation (symmetry) group, a Lie group, of the theory and finding representations of this group in the corresponding Hilbert spaces. This is true for Heisenberg's matrix variables as well, as was discovered by H. Weyl and E. Wigner, who introduced the Heisenberg group. In modern elementary-particle theory, irreducible representations of such groups correspond to elementary particles, the idea that was one of Wigner's major contributions to quantum physics (Wigner 1939). This was, for example, how M. Gell-Mann discovered quarks, because at the time there were no particles corresponding to the irreducible representations (initially there were three of those, corresponding to three quarks) of the symmetry group of the theory, the so-called SO (3). It is the group of all rotations around the origin in three-dimensional space, R^3, rotations represented by all three-by-three orthogonal matrices with determinant 1. (This group is noncommutative.) The electroweak group that Gell-Mann helped to find as well is SU (2), the group of two by two matrices with the determinant 1. The genealogy of this group-theoretical thinking in QFT extends from Dirac's four by four matrices and, earlier, Pauli's two by two spin matrices.

Dirac begins his paper by commenting on previous relativistic treatments of the electron, specifically the Klein-Gordon equation and its insufficiencies. He says:

> [The Gordon-Klein theory] appears to be satisfactory so far as emission and absorption of radiation are concerned, but is not so general as the interpretation of the non-relativi[stic] quantum mechanics, which has been developed sufficiently to enable one to answer the question: What is the probability of any dynamical variable at any specified time having a value laying between any specified limits, when the system is represented by a given wave function ψ_n? The Gordon-Klein interpretation can answer such questions if they refer to the position of the electron … but not if they refer to its momentum, or angular momentum, or any other dynamic variable. We would expect the interpretation of the relativi[stic] theory to be just as general as that of the non-relativi[stic] theory. (Dirac 1928, pp. 611–612)

The term "interpretation" means here a mathematical model of the physical situation, rather than, as is common now, a physical interpretation of a given quantum-theoretical mathematical formalism or model cum the phenomena it relates to. Dirac's statement does not mean that a physical representation of quantum processes in space and time (at least as independent of measurement) is provided,

[11] It is worth keeping in mind that von Neumann's book, which appeared in 1932 (after Dirac's 1930 *Principles*), was not yet published at the time. Related articles by von Neumann were published, but they would have been unlikely to be familiar to Dirac.

as against only predictions, in general probabilistic, of the outcomes of quantum experiments. As discussed above and as is clear from this passage, he only assumed the capacity of a given theory to enable such predictions is sufficient if such predictions are possible for any dynamic variable.[12] The main deficiency of the Klein-Gordon scheme from this perspective was, again, its inability to provide "the probability of any dynamical variables at any specified time having a value laying between any specified limits, when the system is represented by a given wave function ψ_n." Dirac then argues that the first-order derivative in time, missing in the Klein-Gordon equation, is a proper starting point for the relativistic theory of the electron. He says: "The general interpretation of nonrelativi[stic] quantum mechanics is based on the transformation theory, and is made possible by the wave equation being of the form

$$\left(H - W\right)\psi = 0, \tag{6.1}$$

i.e., being linear in W or $\dfrac{\partial}{\partial t}$, so that the wave function at any time determines the wave function at any later time. The wave function of the relativi[stic] theory must also be linear in W if the general interpretation is to be possible" (Dirac 1928, p. 612).

Before proceeding to his derivation, Dirac comments, in the statement with which I began here, on the second difficulty of the Klein-Gordon equation, that of the transitions from states of positive energy to those of negative energy. Dirac's theory, again, inherits this difficulty because mathematically every solution of Dirac's equation is a solution of the Klein-Gordon equation, of which Dirac's equation is a square root. (The opposite is, again, not true.) As discussed above, luckily for the future of quantum theory, the difficulty proved to be not the weakness but the strength of Dirac's theory.

Dirac's *derivation* of his equation is enabled by his use of two key principles, mentioned above. The first is the invariance under Lorentz transformations (a relativity principle). The second is the first-order linearity in time cum the mathematical correspondence principle: the equivalence of whatever new equation one finds to Schrödinger's equation (Eq. (6.1) above) in the limit of large quantum numbers, which requires correspondence with Pauli's spin theory as an intermediate step (Dirac 1928, p. 613). Other key quantum principles stated above, are fulfilled and given their mathematical expression automatically once this correspondence, again, lacking in the Klein-Gordon theory, is in place.

In the absence of the external field, which Dirac considers first and to which I shall restrict myself here, since it is sufficient for my main argument, the Klein-Gordon equation "reduces to

$$\left(-p_0^2 + p^2 + m^2c^2\right)\psi = 0 \tag{6.2}$$

[12] As also discussed earlier, Dirac might have assumed that the mathematical model defined by his equation provides such a representation, but even if so, his article does not claim it does.

if one puts
$p_0 = \frac{W}{c} = i\frac{\hbar}{c}\frac{\partial}{\partial t}$" (Dirac 1928, p. 613).

Next Dirac uses the symmetry between time, p_0, and space, p_1, p_2, p_3, required by relativity, which implies that because the Hamiltonian one needs is linear in p_0, "it must also be linear in p_1, p_2, and p_3." He then says:

> [the necessary] wave equation is therefore in the form
>
> $$\left(p_0 + \alpha_1 p_1 + \alpha_2 p_2 + \alpha_3 p_3 + \beta\right)\psi = 0 \tag{6.3}$$
>
> where for the present all that is known about the dynamical variables or operators α_1, α_2, α_3, and β is that they are independent of p_0, p_1, p_2, p_3, i.e., that they commute with t, x_1, x_2, x_3. Since we are considering the case of a particle moving in empty space, so that all points in space are equivalent, we should expect the Hamiltonian not to involve t, x_1, x_2, x_3. This means that α_1, α_2, α_3, and β are independent of t, x_1, x_2, x_3, i.e., that they commute with p_0, p_1, p_2, p_3. We are therefore obliged to have other dynamical variables besides the coordinates and momenta of the electron, in order that α_1, α_2, α_3, β may be functions of them. The wave function ψ must then involve more variables than merely x_1, x_2, x_3, t.
>
> Equation (6.3) leads to
>
> $$\begin{aligned} 0 &= \left(-p_0 + \alpha_1 p_1 + \alpha_2 p_2 + \alpha_3 p_3 + \beta\right)\left(p_0 + \alpha_1 p_1 + \alpha_2 p_2 + \alpha_3 p_3 + \beta\right)\psi \\ &= \left[-p_0^2 + \sum \alpha_1^2 p_1^2 + \sum\left(\alpha_1\alpha_2 + \alpha_2\alpha_1\right) p_1 p_2 + \beta^2 + \sum\left(\alpha_1\beta + \beta\alpha_1\right)\right]\psi \end{aligned} \tag{6.4}$$
>
> where Σ refers to cyclic permutation of the suffixes 1, 2, 3. (Dirac 1928, p. 613)

I pause here to reflect on Dirac's way of thinking, manifested in this passage and throughout his derivation. Taking advantage of noncommutativity in (6.4) is worth a special notice not only because this is one of Dirac's fortes but also because, as Dirac was, as I said, among the first, or even the first, to realize, it represents the mathematical essence of quantum theory and is crucial to the mathematical expression of its fundamental principles. Equation (6.3), a square root of the Klein-Gordon equation, is already Dirac's equation in abstract algebraic terms, thus, expressing Dirac's approach to finding, first, an abstract mathematical scheme suitable for the expression of the principles of quantum theory and, hopefully, for predicting the data in question in his relativistic theory of the electron. A general, abstract mathematical scheme comes first.

One would now need to find α_n and β, to find the actual form of the equation. It was not entirely different in Heisenberg's approach, where it was the necessary mathematical scheme, that of classical equations, for which different variables needed then to be found. Nevertheless, inspired by that of Heisenberg as it was even in this regard (if we recall that Dirac's work on quantum mechanics stems from reading Heisenberg's 1925 paper), Dirac's approach goes further on this road. Mathematics and the invention of a cohesive formal mathematical scheme are, again, no less important for Heisenberg, and, as I argue here, it was Heisenberg who transformed the very practice of theoretical physics along these lines, and by implication that of experimental physics as well. However, one does not find in

Heisenberg, at least in his work on QM, the same kind of, to use Dirac's word, *play* with (still more) abstract mathematical structures that is characteristic of Dirac (1962a), perhaps because the equations used by Heisenberg were "ready-made" by classical physics. Some of Heisenberg's later work on QFT exhibits this Dirac-like approach more, but still not to the same degree, and Dirac, arguably, contributed to this transformation of the practice of theoretical physics as much as Heisenberg did. Dirac's earlier work on *q*-numbers quantum-mechanical formalism already displayed this power of abstract mathematical thinking, again, however, arising from and governed by the physical principles of quantum theory. It is true that antimatter was a consequence of the mathematical structure of Dirac's equation. This is not uncommon in theoretical physics, whether constructive or principle: new physical objects are discovered and new physical principles are often established as consequences of the mathematical formalism. This is what happened in the case of Dirac's equation as well. Nevertheless, Dirac's equation was what it was because of the fundamental physical principles on which it was based. It is, again, equally crucial that this equation mathematically expressed these principles. I might add that, notwithstanding Dirac's famous later comment on mathematical beauty as necessary to physical laws and despite the fact some of this beauty manifests in his derivation and his equation itself, it does not appear that the mathematical beauty was Dirac's primary aim or guidance here (Dirac 1939, p. 124). This may be argued even apart from the fact, far from unimportant, that the formal *simplicity* at least of these manipulations and of the symbolic form of his equation, especially in its maximally compact form above, $i\gamma\cdot\partial\psi = m\psi$, the later embodies a great calculational complexity, beginning with the Hilbert space structure it embodies ($H = L^2\ (R^3;\ X) = L^2\ (R^3) \otimes X = L^2\ (R^3) \otimes \mathrm{C}^4$). This fact would require one to reconsider the very concept of the mathematical beauty of physical laws or even of mathematical concepts, which commonly embody this complexity. Einstein's equation of general relativity is a good parallel to Dirac's equation in this regard, but many other examples could be cited, including in the subsequent developments of QFT (e.g., Wilczek 2009, pp. 61–63). The subject is, however, beyond my scope. Dirac now proceeds as follows:

> [Equation (6.4)] agrees with the Klein-Gordon equation, in the absence of the external field $\left(-p_0^2 + p^2 + m^2c^2\right)\psi$] if
>
> $$\alpha_r = 1, \alpha_r\alpha_s + \alpha_s\alpha_r = 0\left(r \neq s\right) r, s = 1,2,3.$$
>
> $$\beta^2 = m^2c^2, \alpha_r\beta + \beta\alpha_r = 0$$
>
> If we put $\beta = \alpha_4 mc$, these conditions become
>
> $$\alpha_\mu^2 = 1, \alpha_\mu\alpha_\nu + \alpha_\nu\alpha_\mu = 0\left(\mu \neq \upsilon\right)$$
>
> $$\mu, \upsilon = 1,2,3,4. \tag{6.5}$$
>
> (Dirac 1928, p. 613)

Dirac, again, takes advantage of a partial mathematical correspondence with the Klein-Gordon equation (that between the function of complex variables and its square root), which allows him to derive certain algebraic conditions that α_μ and β must satisfy. Dirac will now state that "we can suppose α_μ's to be expressed in some matrix scheme, the matrix elements of α_μ being, say, $\alpha_\mu\,(\zeta'\zeta'')$" (Dirac 1928, p. 613). This supposition is not surprising given both the formal mathematical considerations (such as the anticommuting relations between them) and the preceding history of matrix mechanics, including Dirac's own previous work. As noted above, by then it was a virtually established mathematical principle of quantum theory. We know or may safely assume from Dirac's account of his work on his equations that matrix manipulation, "playing with equations," as he, again, called it, was one of his starting points (Dirac 1962a). In addition, Pauli's theory, which is about to enter Dirac's argument, provided a ready example of a matrix scheme (Pauli 1927; Kragh 1990, pp. 55–56, 60). It was clear that matrix algebra of some sort would be a good candidate for α_μ. Dirac, yet again, gets extraordinary mileage from considering the formal properties of the variables involved, even before considering what these variables actually are and as a way of gauging what they should be. This is his next step, which reveals another remarkable consequence of the necessity of the particular matrix variables required by Dirac. For if we "suppose α_μ's to be expressed in some matrix scheme, the matrix elements of α_μ being, say, $\alpha_\mu\,(\zeta'\zeta'')$," then "the wave function ψ must be a function of ζ as well as x_1, x_2, x_3, t. The result of α_μ multiplied into ψ will be a function (α_μ, ψ) of x_1, x_2, x_3, t, ζ defined by

$$\sum\nolimits_{\zeta'} \alpha_\mu \left(\zeta\zeta'\right) \psi\left(x, t, \zeta'\right)\text{" (Dirac 1928, p. 614).}$$

Dirac is now prepared "for finding four matrices α_μ to satisfy the conditions (6.5)," those forming the Clifford algebra, and for finding the actual form of variables that satisfy formal Eqs. (6.3) or (6.4). Dirac considers first the three Pauli spin matrices, which satisfy the conditions (6.5), but not the Eqs. (6.3) or (6.4), which needs four by four matrices. He says:

We make use of the matrices

$$\sigma_1 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}; \sigma_2 = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}; \sigma_3 = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}$$

which Pauli introduced to describe the three components of the spin angular momentum. These matrices have just the properties

$$\sigma_r^2 = 1\sigma_r\sigma_s + \sigma_s\sigma_r = 0\left(r \neq s\right), \tag{6.6}$$

that we require for our α's. We cannot, however, just take the σ's to be thereof our α's, because then it would not be possible to find the fourth. We must extend the s's in a diagonal matter to bring in two more rows and columns, so that we can introduced three more matrices ρ_1, ρ_2, ρ_3 of the same form as σ_1, σ_2, σ_3, but referring to different rows and columns, thus:

$$\rho_1 = \begin{pmatrix} 0010 \\ 0001 \\ 1000 \\ 0100 \end{pmatrix}; \rho_2 = \begin{pmatrix} 00-i0 \\ 000-i \\ i000 \\ 0i00 \end{pmatrix}; \rho_3 = \begin{pmatrix} 1000 \\ 0100 \\ 00-10 \\ 000-1 \end{pmatrix};$$

$$\sigma_1 = \begin{pmatrix} 0100 \\ 1000 \\ 0001 \\ 0010 \end{pmatrix}; \sigma_2 = \begin{pmatrix} 0-i00 \\ i000 \\ 000-i \\ 00i0 \end{pmatrix}; \sigma_3 = \begin{pmatrix} 1000 \\ 0-100 \\ 0010 \\ 000-1 \end{pmatrix}$$

he ρ's are obtained from σ's by interchanging the second and the third row, and the second and the third columns. We now have, in addition to Eq. (6.6)

$\rho_r^2 = 1\, \rho_r\rho_s + \rho_s\rho_r = 0(r \neq s)$, and also

$$\rho_r\sigma_t = \sigma_t\rho_r. \tag{6.7}$$

(Dirac 1928, p. 615)

These matrices are Dirac's great invention, parallel to Heisenberg's invention of his matrix variables. Dirac's matrices form the basis of the corresponding Clifford algebra and define the mathematical architecture where the multicomponent relativistic wave function for the electron appears from the first principles, in contrast to Pauli's theory where the two-component nonrelativistic wave function, necessary to incorporate spin, appears phenomenologically. Spin is an automatic consequence of Dirac's theory, unintended (just as antimatter was), but fundamental. The entities Dirac's matrices transform are different from either vectors or tensors and are, again, called spinors, extensively studied, as mathematical objects, by E. Cartan (who did not initially use the term due to Ehrenfest) in 1913.

The rest of the derivation of Dirac's equation is a nearly routine exercise, with a few elegant but easy matrix manipulations. Dirac also needs to prove the relativistic invariance and the conservation of the probability current, and to consider the case of the external field, none of which is automatic, but is standard textbook material at this point. The most fundamental and profound aspects of Dirac's thinking as principle thinking are contained in the parts of his paper just discussed, and I can draw my main conclusions concerning the significance and implications of Dirac's theory of the relativistic electron, which his equation embodies, from them.

Dirac's theory is a remarkable example of principle thinking in theoretical physics, in which fundamental physical principles of relativity and quantum theory combine with mathematics so that physics guides the mathematics, which both gives these principles their precise mathematical expression and leads to new physics. Dirac's theory was not only a result of his use of already established principles, but also led to new principles, inherent or implied in his equation, although it took a while to realize the radical nature of these implications. The most crucial of them emerged from the concept of antimatter, the most revolutionary consequence of Dirac's theory (although the experimental discovery of the positron by C. D. Anderson, in 1932, was independent), in particular what may be called the particle-transformation principle, the PT principle. (Dirac himself initially proposed the so-called "hole theory," which postulates that the continuum of negative energy states that are solutions of Dirac's equation are filled with electrons, while "holes" in this continuum appear as positrons. The theory had some followers for quite a while and still resurfaces on occasion in QFT.) The PT principle extends and is a consequence of the antiparticle principle, which states that for every particle there is an antiparticle, although some particles, sometimes known as Majorana particles, such as photons, are their own antiparticles. The PT principle adds the loss of particle identity in quantum experiments to the nonrealism of the RWR principle found already in quantum mechanics. I shall now discuss the PT principle, which changed our concept of the elementary particle and of the ultimate constitution of matter. It is also correlative to symmetry and invariance principles, which have played such a momentous role in the development of QFT.

6.3 The Unrepresentable and the Multiple: Particles and Fields in QFT

As explained above, Dirac's equation encodes a complex mathematical architecture, which embodies the fundamental physical principles of QED and QFT. It is, again, true that Dirac's equation is not a field equation. However, given its mathematical architecture and the physics this architecture reflects, it would also be difficult to see it merely in terms of relativistic quantum mechanics, as some suggest (e.g., Kuhlman 2015). It might be more naturally seen in particle rather than field terms and it might have been initially seen in particle terms by Dirac, but the fundamental aspects of the architecture in question could be interpreted in terms of fields, and as noted from the outset of this chapter, these two interpretations continue to compete with each other. From the present, RWR-principle-based, perspective, the reality in question cannot be captured by either concept, or possibly by any representational concept. As I shall suggest, however, a *nonrepresentational* concept, that could be and here will be understood as that of quantum field, may be necessary in view the PT principle, in addition to the (equally nonrepresentational) concept of elementary particle, found already in QM, as considered earlier.

The Hilbert space associated with a relativistic electron in Dirac's theory is a tensor product of infinite dimensional Hilbert space (encoding the mathematics of continuous variables) and a finite-dimensional Hilbert space over complex numbers, which, in contrast to the two-dimensional Hilbert space of Pauli's theory, C^2, is four-dimensional in Dirac's theory, C^4. Spin is contained in the theory automatically, which was an unexpected bonus of the equation, albeit in this case, unlike that of antimatter, immediately realized by everyone. This fact helped the reception of the equation, the problem of negative energy values notwithstanding.

Dirac's wave function $\psi(t, \boldsymbol{x})$ takes value in a Hilbert space $X = C^4$ (Dirac's spinors are elements of X). For each t, $\psi(t, \boldsymbol{x})$ is an element of

$$H = L^2\left(R^3; X\right) = L^2\left(R^3\right) \otimes X = L^2\left(R^3\right) \otimes C^4.$$

The full-fledge QED and other forms of QFT give this type of architecture an even greater complexity. It is difficult to overestimate the significance of this architecture, which amounts to a very radical view of matter, first manifested in the existence of antimatter. This architecture mathematically responds to, and, in Dirac's work, led to a discovery of the following physical situation, keeping in mind that, in the present view, the theories involved only provide probabilities or statistics of the outcomes of quantum events, registered in measuring instruments.

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 as a thought experiment (keeping in mind that one needs a rather complex experimental technology to accelerate an electron in order for it to reach this energy). The probability of the outcome would be properly predicted by QED. But what will be the outcome? The answer, as we know, is not what a classical or even our quantum-mechanical intuition would expect, and this unexpected answer was a revolutionary discovery of QED, beginning with Dirac's equation. To appreciate the revolutionary nature of this discovery, let us consider, first, what happens if we deal with a classical object, analogous to an electron, and then a low-energy quantum electron in the same type of arrangement. I speak of a classical object because the "game of small marbles" for electrons was finished even before quantum mechanics was introduced. An electron, say a Lorentz electron, of a small finite radius, would be torn apart by the force of its negative electricity. This required theoretical physics to treat the electron mathematically as a dimensionless point, without really giving it a physical architecture, as Dirac does not fail to note in his paper, in conjunction with spin, which obviously complicated the situation (Dirac 1928, p. 610). One could still treat an electron classically, for example, as noted above, by the correspondence principle, when it is far away from the nucleus. This treatment is an idealization because this behavior is quantum, and hence could lead to quantum effects. On the other hand, within the idealization of classical physics, we may treat classical objects (Newton did so already) as dimensionless points endowed with mass.

We can take as an example of the classical situation a small ball that hits a metal plate, which situation could be used for either a position or a momentum measurement, or indeed a simultaneous measurement of both, and time t. As discussed earlier, although we still ultimately deal with phenomena (in Kant's or Husserl's sense of what appears to our consciousness, rather than in Bohr's sense of what is observed in measuring instruments), in classical mechanics, we can assume to be dealing with the objects themselves involved, without appreciably affecting anything. The place of the collision could, at least in an idealized representation of the situation, be predicted exactly by classical mechanics, and we can repeat the experiment with the same outcome on an identical or even the same object. Most importantly, regardless of where we place the plate, we always find the same object, at least in an experimental situation shielded from outside interferences, which could deflect the ball or even destroy it before it reaches the plate.

By contrast, if one considers an electron in the QM regime, beyond the fact that it is impossible, because of the uncertainty relations, to predict the place of collision exactly or with the degree (in principle unlimited) of approximation possible in classical physics, there is a nonzero probability that we will not observe such a collision at all. It is also not possible to distinguish two observed traces as belonging to two different objects of the same type, or to distinguish such objects in the first place, a circumstance that becomes even more crucial in high-energy regimes. Unlike in the classical case, in dealing with quantum objects, there is no way to improve the conditions of the experiment to avoid this situation. QM, however, gives us correct probabilities or statistics for such events, including correlations, such as those of the EPR type, without, in nonrealist (RWR-principle-based) interpretations, representing the quantum objects and processes responsible for these events. This is accomplished by defining the corresponding Hilbert space, with the position and other operators as observables, and using the formalism, say, Schrödinger's equation for the state vector $|\psi>$(in the case of a pure state), and Born's rule for obtaining the statistics of possible outcomes, once we repeat the experiment a large number of times. In a single experiment an electron could, in principle, be found anywhere, or, again, not found at all, and if one adopts the statistical Copenhagen interpretation, one cannot in general assign a probability to any individual event. It is random.

Once the process occurs at a high energy, governed by QED, the situation is still different, indeed radically different. One might find, in the corresponding region, not only an electron, as in classical physics, or an electron or nothing, as in the QM regime, but also other particles: a positron, a photon, an electron–positron pair. Just as does QM, QED, beginning with Dirac's equation, rigorously predicts which among such events can occur, and with what probability or statistics, in accordance with the observations just described, without, in RWR-principle-based interpretations, describing the corresponding quantum processes themselves. In order to enable these predictions, however, the corresponding Hilbert-space machinery becomes much more complex, essentially making the wave function ψ a four-component Hilbert-space vector, as opposed to a one-component Hilbert-space vector, as in quantum mechanics. Furry and Openheimer's 1934 paper cited earlier was arguably the first to speak in these terms. They noted that accounting for the interaction between an electron and a Coulomb field (i.e., predicting the corresponding effects) required a state vector with an infinite number of components, each component giving the probability amplitude for the corresponding event—finding an electron, a positron, an electron-positron pair, and so forth (Furry and Oppenheimer 1934; Schweber 1994, p. 5). (I borrow but slightly modify Schweber's description to bring it in accord with the present, nonrealist, view.) In the case of Dirac's equation, this Hilbert space is, as noted, $H=L^2\,(R^3;\,X)=L^2\,(R^3)\otimes X=L^2\,(R^3)\otimes \mathrm{C}^4$ and the operators are defined accordingly. This structure allows for a more complex structure of predictions (which are still probabilistic or statistical) corresponding to the situation just explained, usually considered in terms of virtual particle formation and Feynman's diagrams.

Once we move to still higher energies or different domains governed by QFT the panoply of possible outcomes becomes much greater. The Hilbert spaces and operator algebras involved would be given a yet more complex structure, in relation to the appropriate Lie groups and their representations, defining (when these representations are irreducible) different elementary particles, as indicated above (Wigner 1939). In the case of Dirac's equation we only have electron, positron, and photon, single or paired. It follows that in QFT 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. This qualification is, again, important because the identity of particles within each type is strictly maintained in QFT, just as it is in QM. In either theory one cannot distinguish different particles of the same type. In QFT, 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, theoretically governed by the concept of virtual particle formation. This description is still too crude and one needs to supplement it by the concept of quantum field (a concept that I shall introduce, as based on the RWR and PT principle below), but it is sufficient for making my main point at the moment. The operators used to predict the probability of such events are the creation and annihilation operators.

This picture clearly takes us beyond QM. For, while the latter precludes the applicability of classical concepts, such as objects (particles or waves) and motion, at the quantum level, it still preserves the identity of quantum objects and of the types of quantum objects within the same experiment. As explained earlier, it is still possible to speak of this identity, even though, by the RWR principle, these objects themselves are unrepresentable or even unthinkable and only manifest themselves and the type of their identity, say, electrons vs. photons, in their effects on measuring instruments. This is sufficient to properly define each elementary particle. However, it is no longer possible to preserve this identity within the same experiment in high-energy quantum regimes, because, as just explained, one may observe different types of particles in the course of the same experiment, which leads to the PT (particle-transformation) principle. It is a principle because this situation is found and, due to this principle qua principle, is to be expected in any regime requiring and governed by QFT. This principle was at work, in conjunction with or as correlative to various symmetry principles, in the QFT of nuclear forces (quantum chromodynamics or QCD), for example, and governed the practice of theoretical physics, not the least in many discoveries of new particles, such as quarks. Indeed, especially, again, as correlative to these symmetry principles, but as a *more expressly* physical one, the PT principle is one of the most important principles of high-energy theoretical physics. It should be added, in accord with the overall analysis given in this chapter, that while symmetry principles are mathematical, they reflect and express profound physical principles, as does, for example, the gauge symmetry principle. This principle is found already in Maxwell's electrodynamics, but it is especially important in general relativity and QFT. Thus, quantum electrodynamics is an abelian gauge theory with the symmetry group U(1) (this group is commutative), and it has one gauge field, with the photon being the gauge boson. The standard model is a non-abelian gauge theory with the symmetry group U(1) × SU(2) × SO(3) and broken symmetries, and it has a total of twelve gauge bosons: the photon, three weak bosons, and eight gluons.

One still needs to bring this situation in accord with Bohr's concepts of phenomena and atomicity, equally necessary under the conditions of the RWR principle, cum both the QD and QP/QS principles, in QM and QFT. These concepts need to be given new features under the PT principle, as Bohr suggested in the statement cited at the outset of this chapter, in speaking of "Dirac's ingenious quantum theory of the electron" as reflecting "*new* fundamental features of atomicity" (Bohr 1987, v. 2, p. 63; emphasis added). One needs to bring together the mathematical and the experimental technologies of QFT, under the QD and QP/QS principles, and ultimately the RWR principle. I shall now suggest one way in which this could be done, with an understanding that this is a *suggestion* rather than a definitive solution of the problem of particles and fields in QED. As noted from the outset, this problem is long standing and difficult, and it would be too presumptuous to claim a solution. On the other hand, as also noted from the outset, most of the difficulties associated with this problem arise from one or another realist imperatives (property realism, structural realism, and so forth). The present approach avoids these difficulties. Admittedly, this would not satisfy those, again, a majority, who see realism as imperative in QFT as elsewhere in fundamental physics. But then, this imperative could be questioned in turn, and there does not appear to be anything in physics itself that requires it.

As discussed in Chap. 1, low-energy QM regimes already allow for a concept of elementary particle corresponding to Bohr's concepts of phenomenon and atomicity, beginning with the fact that elementary particles within the same type cannot be distinguished from each other, while these types themselves are rigorously distinguishable from each other. Both features are consistently defined by the corresponding sets of effects manifested in measuring instruments, and thus are consistent with the RWR-principle-based view that the *character* of elementary particles and their behavior is beyond representation or possibly even conception, a view that precludes one from associating any properties with elementary particles themselves. An elementary particle of a given type, say, an electron, is defined by a given set of possible phenomena or events (the same for all electrons) observable in measuring instruments in the experiments associated with particles of this type, in this case, all electrons. Speaking of the "same" electron detected in the course of a given experiment, again, loses its meaning in high-energy regimes and, hence, in QED or QFT, because of the PT principle. The *elementary* character of an elementary particle is, as explained in Chap. 1, defined by the fact that there is no experiment, actual or conceivable, that allows one to associate the corresponding effects on their interactions with measuring instruments with more elementary individual quantum objects. Once such an experiment becomes conceivable or performed, the status of a given object as an elementary particle could be experimentally disproved or challenged, as it happened when hadrons were proved to be composed of quarks and gluons. With these qualifications in mind, one could speak of "elementary particles" as the ultimate elemental constituents of matter. These constituents are not "particles" in any specifiable sense that we can give this term. That is, they cannot be comprehended by any concept of particle, any more than, as such, by other concepts, such as wave or field, although, as will be seen presently, "quantum field" could be defined as a concept otherwise. Elementary particles, and their reality, could be seen not in term of their realist representations or even conception, but in technological terms of particular types of effects of their interactions with measuring instruments, effects that we can predict, probabilistically or statistically, by using quantum theory. The permanent or invariant characteristics associated with elementary particles, such as mass, charge, or spin, could be understood in these terms as well.

This conception of elementary particles (as being beyond conception as concerns their independent constitution) is retained in high-energy quantum regimes and QFT in nonrealist interpretations, based the QD, QP/QS, and RWR principles. Unlike that of low-energy quantum regimes and QM, however, in this case, this conception is not sufficient and needs to be supplemented by the concept of quantum field, which I shall now introduce. This concept is *physical* rather than mathematical, although it has its specific mathematical counterpart in QFT. The main reason for the necessity of this concept is the PT principle, with the multiplicities of elementary particles becoming progressively greater and structurally more complex once we move to yet higher-energy regimes. The concept of quantum field responds to this situation. A quantum field, including that associated with a given elementary particle, is still, physically, a quantum object, and hence, by the RWR principle, cannot be assigned any conceptual or any other architecture. It has, however, a particular architecture of effects, composed by sets of effects associated with different types of particles. It brings the irreducibly unrepresentable or possibly even unthinkable and the irreducible multiple together in QFT.

I would like to briefly revisit first, by way of a contrast, the classical concept of field. Essentially, field is, or is represented by, a differentiable manifold with a set of scalar (a scalar field), vector (a vector field), or tensor (a tensor field) variables associated with each point and the rules for transforming these variables (usually by means of differential functions) from point to point of this manifold. One can also think of it as a fiber bundle over a manifold with a connection. The concept of fiber bundle is used in QFT, where it is associated with local gauge symmetry, in the present view without representing, any more than any part of the mathematical formalism of QFT, any quantum physical process but only being part of the probabilistically or statistically predictive machinery of QFT. In classical physics or relativity, the variables in question become associated with or map measurable quantities associated with, the field, thus providing a field ontology associated with a given phenomena. The situation is not essentially different from the case of classical particle ontology, except that in this case we deal with continuously (indeed differentially) moving discrete objects, while in the case of fields we deal with a continuously (differentially) propagating matter. This field ontology also allows, ideally and in principle, for exact predictions concerning future events associated with this field via certain measurable (field) quantities.

In nonrealist, RWR-principle-based, interpretations of quantum phenomena, this type of ontology is impossible, as reflected in the fundamental principles of quantum theory, if understood along nonrealist lines, beginning with the QD and the QP/QS principles, and then the RWR principle (expressing nonrealism), and, in QED and QFT, the PT principle, again. By the QD principle we deal with a discrete manifold of phenomena and sets of quantities associated with each phenomenon, and hence, a discrete manifold of such quantities in both QM and QFT. Both theories relate, in terms of probabilistic or statistical predictions (the QP/QS principle) the continuous (technically, differential) mathematics to the discontinuous configurations of the observed data, without, by RWR principle, representing the objects and processes considered. In QFT, however, these quantities could no longer be associated with a single quantum object, for example and in particular, a given elementary particle. It is the corresponding quantum object or quantum stratum of matter that I call a "quantum field." This stratum is responsible for a multiple and transformational, Heraclitean, architecture that appears at the level of the effects of a quantum field, and is sometimes invoked in this connection, albeit, unlike here, usually within the realist paradigm (e.g., Redhead 1990, p. 22). In the present view, however, this field itself, which is the efficacy of the effects defining this architecture, cannot be assigned this architecture, any more than any other. It has neither a form of Being nor that of Becoming, neither that of the One nor that of the Many, and so forth. The architectures of these effects is, again, more multiple and richer than in QM-regimes. One could speak of such a field-like stratum there as well (we can also have nonrelativistic quantum fields), but in a reduced or degenerate form that preserves the particle identities—photons always remain photons (or disappear), electrons remain electrons (or disappear), and so forth. In QFT regimes particles transform into one another. All this is, again, only manifested at the level of effects in a given experiment, effects whose efficacy (without causality) is a quantum field. In itself a quantum field, again, cannot be given any conceptual, including mathematical, representational model: it is a principle and not a constructive concept in Einstein's sense, although it is *theoretically* constructed, constructed as unconstructible, as all quantum objects are by the RWR principle. Hence, a given quantum field is neither continuous nor discontinuous, any more than it is anything else that we can describe or even conceive of, but it has, it is, the efficacy of a particular architecture of effects, manifested in the architecture of the corresponding configuration of measuring instruments.

Although, as explained above, quantum objects could be understood or distinguished from other types of quantum objects only in terms of effects they may have on measuring instruments, this suffices to rigorously distinguish different quantum fields, to the degree possible, because elementary particles of the same type and hence the fields associated with them are not generally distinguishable. They are only distinguishable in terms of their variable quantities, such as, position, momentum, or energy, of a given particle, manifesting its corresponding field in a given experiment. In sum, a quantum field is *real*, although it can never be realized or, one might say, actualized as such, but is only actualized in terms of its effects upon the measuring instruments.[13] As I shall explain presently, one can see a quantum field as *virtual*, and connect this concept to such concepts as virtual particle formation and the corresponding structures in the formalism (the creation and annihilation operators, which are, again, part of the mathematical technology for making probabilistic or statistical predictions, without representing anything). From this perspective, both the virtual and the actual are real, but they are different types of the real, the first being a reality without realism, reality that precludes realism, and the second a reality that allows for realism. Both QM and QFT are forms of mathematical machinery, mathematical technology, that use their continuous (technically, differential) Hilbert-space formalism to rigorously predict the discrete effects in question, in probabilistic or statistical terms, and nothing else appears to be possible in quantum physics, at least, again, as things stand now.

Thus understood, quantum field is, again, *a physical concept*, rather than a *mathematical concept*, but it is associated with a mathematical concept, commonly also called "quantum field," defined in terms of a predictive Hilbert space formalism with a particular vector and operator structure, enabling the proper probabilistic predictions of the QFT phenomena concerned. While there is a large consensus, although not a uniform one (some think the concept of particle suffices), that a physical concept of quantum field is necessary, most of the proposals concerning it proceed along realist lines.[14] The present concept is, by contrast, nonrealist, even though it assumes the reality—reality without realism—of quantum fields, as an instance of Bohr's concept of a quantum object as something that is unavailable to a representation or, in the strongest form of the RWR principle, even conception. This view, again, makes the term "quantum objects" merely a linguistic designation not associated to any specifiable concept.

The operators enabling one to predict the probabilities for the "annihilation" of some particles and "creation" of other particles, that is, again, for the corresponding measurable quantities observed in measuring instruments, are called annihilation and creation operators. In RWR-principle-based interpretations, these operators do not describe anything either, but only give the probabilities or statistics established by quantum experiments, by the QP/QS principle, just as the wave functions do in QM. Both, to return to Schrödinger's language, provide expectation-catalogs for the outcomes of possible high-energy experiments. These catalogs, however, are essentially different from those of QM because in the case of QED or QFT they are those of probabilities or statistics of the appearance of quantities associated with other types of particles even in experiments initially performed on a particle of a given type.[15]

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[13] Although one might see the sets of effects in question here as a kind of bundle of properties, the present, nonrealist, view is essentially different from Kuhlman's realist Dispositional Trope Ontology (DTO) mentioned above, because the latter assumes such bundles or tropes as quantum level properties (Kuhlman 2010, 2015).

[14] See, again, (Kuhlman 2015), which clearly confirms this point.

[15] The wave function of QM formalism in low-energy regimes, say, for an electron in an atom, can be recast, quite elegantly, in terms of annihilation and creation operators as well, by the procedure

From this perspective the concept of virtual particle formation can be understood as follows. Let us, again, for simplicity, consider an experiment in the QED regime in which we begin with a single electron, registered at time t_0, the registered outcome of which is, say, a positron at time t_n, but, again, it could also be an electron-positron pair, a photon, etc. We call this positron a real particle, although for the reasons to be explained momentarily, it may also be called *actual*. The theory predicts the probabilities or statistics of these outcomes, extensively confirmed by experiments, which makes QED the best-confirmed physical theory ever. Now, consider a discrete sequence of times: t_1, t_2, t_{n-1}, between t_0 and t_n, and a discrete sequences of possible measurements (all such sequences are, again, discrete in quantum physics), at which, *if they were performed*, any of the outcomes just mentioned could have taken place, again, with the probability properly predicted by a theory. Once such a measurement is performed a new expectation-catalog is defined for future measurements. We speak of particles that could thus be in principle "observed" (insofar as the corresponding quantities associated with them would have been measured), *but are never actually observed,* as virtual particles. In other words, virtual particles cannot be associated with any quantum phenomenon, including in Bohr's sense. The qualification that virtual particles are never actually observed is crucial. Only those particles that are registered by observation are considered "real particles," while "virtual particles" are never registered by observation. "Actual" may, again, be a more accurate term than "real," because virtual particles are as real as actual particles. Both equally exist. Virtual particles are born and disappear very quickly. We do know, however, that, even though they are never observed, virtual particles come into existence because they have probabilistically or statistically ascertainable effects on actual particles and our measurements concerning them (e.g., Wilczek 2009, pp. 45–50). On the other hand, in the RWR-principle-based interpretations, ultimately, anything actual can only be an effect of a particle upon a measuring instrument. Hence, while they are real and while there is, in such interpretations, a difference between "virtual" and "real" particles, insofar as only the latter interact with measuring instruments, even real particles are never strictly actual, only their effects are.

In this sense, the quantum field itself of an experiment may be seen as the *virtual*. Any quantum field is real, although it can never be actualized as such, in itself, but

known as "second quantization," which was one the first steps toward QED. One sometimes speaks, appealingly but loosely, of the first quantization as making particles into waves, and the second quantization as making waves particles again, in a new sense. While the procedure was developed to deal with quantum many-body systems, and reflects the indistinguishability of elementary particles of the same type, it is applicable even to a single electron in an atom, normally described by a wave function, thus replaced by annihilation and creation operators. In high-energy regimes, governed by QED or QFT, it is meaningless to ever speak of a single electron even in the hydrogen atom. 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" (Pais 1986, p. 325). Besides, we still have quarks plus gluons inside this proton, in the same transformational existence, although we cannot (because of the "confinement" of quarks) register their effects apart from those of this proton.

only in terms of its effects on the measuring instruments, effects associated with particles that are actualized at the same time when these effects are observed. Both the virtual and the actual are, again, real, but they are different types of the real.

As indicated earlier, one could, in principle, think of the virtual of a quantum field as what J. A. Wheeler called "quantum foam," whereby the classical picture of matter as a motion of particles or waves is replaced by that of incessant birth and disappearance of quantum entities (Wheeler and Ford 1998, pp. 245–263). Tempting as it may be, however, Wheeler's picture potentially reinstates realism, and thus is different from the view just presented, although Wheeler's conception itself appears to be sufficiently open to allow for the present view as well. What is important is that the nature of the quantum field itself is as beyond this conception as it is beyond any other, for example, that of the indivisible Platonic or Parmenidean One, as proposed in (Barbour 1999), admittedly not a common conjecture. The QFT becoming, or birth and disappearance of anything, are only manifest at the level of effects of quantum fields manifested in measuring instruments, as manifolds of always discrete phenomena and sets of quantities associated with each phenomenon.

We can now see more clearly why Heisenberg saw Dirac's theory as an even more radical revolution than quantum mechanics as "perhaps the biggest change of all the big changes in physics of our century. It was a discovery of utmost importance because it changed our whole picture of matter. … It was one of the most spectacular consequences of Dirac's discovery that the old concept of the elementary particle collapsed completely" (Heisenberg 1989, pp. 31–33). As I noted, the volume just cited, originally written in the 1970s, contains a companion article entitled, "What is an Elementary Particle?," still an unanswered question, as is "What is a Quantum Field?," both of which the present argument tries to pose in a new way. It would, again, be presumptuous to claim that this argument answers these questions, and even a claim of posing them more effectively already, I admit, carries too much presumption. Heisenberg elaborated:

> So the final result at this point seems to be that Dirac's theory of the electron has changed the whole picture of atomic physics. After abandoning the old concept of the elementary particles, those objects which had been called "elementary particles" have now to be considered as complicated compound systems, and will have to be calculated some day from the underlying natural law, in the same way as the stationary states of complicated molecules will have to be calculated from quantum or wave mechanics. We have learned that energy becomes matter when it takes the form of elementary particles. The states called elementary particles are just as complicated as the states of atoms and molecules. Or, to formulate it paradoxically: every particle consists of all other particles. Therefore we cannot hope that elementary particle physics will ever be simpler than quantum chemistry. This is an important point, because even now many physicists hope that some day we might discover a very simple route to elementary particle physics, as the hydrogen spectrum was in the old days. This, I think, is not possible. (Heisenberg 1989, pp. 34–35)

The theory made a remarkable progress and has acquired a much richer content and architecture since its introduction or these remarks by Heisenberg, most famously in the electroweak unification (or at least "mixture") and the quark-gluon model of nuclear forces, both underlain by the Higgs field. Some of these developments, at their earlier stage, were noted in Heisenberg's articles here cited, which emphasized the role of symmetry and group theory, central to all of these developments. Many predictions of the theory, from quarks to electroweak bosons and the concept of confinement and 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 QFT that led to string and then brane theories. However, the essential epistemological points and principles here considered have remained in place, just as they have in the case of QM, and as stressed throughout this book, debates concerning them have never subsided or lost any of their intensity either. That is, these developments still equally allow for nonrealist, RWR-principle-based interpretations. (Other key principles of quantum theory, such as the QD and QP/QS principle are satisfied on experimental grounds.) In this respect, Heisenberg's assessment just cited or Bohr's 1958 assessment of QED and QFT cited earlier would only require relatively minor adjustments. A more recent statement by Pais, who was both a major practitioner of QFT and a prominent historian of quantum theory, confirms this. The statement is, it is true, no longer that recent either. It was made in 1986, but not much has changed in this respect since, as is clear, for example, from Wilczek's 2005 review of the present state of the theory (Wilczek 2005; also Wilczek 2009), cited earlier, Weinberg's "update" of Heisenberg (Weinberg 1996), Kuhlman's survey (Kuhlman 2015), and other recent assessments. Technical achievements of the theory, or relevant experimental physics, have remained momentous. Wilczek received his 2004 Nobel Prize (shared with David Gross and David Politzer) for his contribution. This work itself (on the asymptotic freedom of quarks) was done before Pais's comments and is discussed in Pais's book. According to Pais:

> Is there a theoretical framework for describing how particles are made and how they vanish? There is: quantum field theory. It is a language, a technique, for calculating the probabilities of creation, annihilation, scattering of all sorts of particles: photons, electrons, positrons, protons, mesons, others, by methods which to date invariably have the characters of successful approximations. No rigorous expression for the probability of any of the above-mentioned processes has ever been obtained. The same is true for the corrections, demanded by quantum field theory, for the positions of energy levels of bound-state systems [e.g., atoms]. There is still a [Schrödinger] equation for the hydrogen atom, but it is no longer exactly soluble in quantum field theory. In fact, in a sense to be described [i.e., the sense explained above in this chapter], the hydrogen atom can no longer be considered to consist of just one proton [or three quarks plus gluons in the nucleus] and one electron. Rather it contains infinitely many particles. (Pais 1986, p. 325)

Pais added another remark, still fully valid as well: "In quantum field theory the postulates of special relativity and of quantum mechanics are taken over unaltered, and brought to a synthesis which perhaps is not yet perfect but which indubitably constitutes a definitive step forward. It is also a theory which so far has not yielded to attempts at unifying the axioms of general relativity with those of quantum mechanics. Is quantum field theory the ultimate framework for understanding the structure of matter and the description of elementary processes? Perhaps, perhaps not" (Pais 1986, p. 325). It is true that string and, especially, brane theory, developed from QFT, were not quite part of the scene at the time, but, while giving new shape to this question and new hopes, these theories are far from answering it, and some of the optimism concerning them waned somewhat more recently or became less widespread. Some skepticism concerning them existed throughout their, by now no longer so short (40 years!), history.

Given Heisenberg's or the present view of the situation, Heisenberg's statement cited above (corroborated by Pais here) suggests that Dirac's discovery ultimately revealed the following philosophical aspect of fundamental physics, at least if it is considered quantum. The complex *precedes* the simple, meaning this logically, rather than ontologically, which makes all simplicity provisional. That is, we cannot hope to decompose the complex into essentially, fundamentally more simple constituents, but only into structurally equally or even more complex ones, which is not to say that such a decomposition is not necessary or important. We still speak of elementary particles and make good use of this concept, thus defined, by combining the RWR and the PT principle. It is also important that Heisenberg speaks of the situations in terms of concepts, in a sense analogous to those given to this term by this study, with each concept being an irreducible complex architectural entity, the components of which, parts or "particles," are no less simple than the whole. His further comments reflect on our fundamental physical concepts. Heisenberg, first, comments on the concept of an elementary particle, given the QFT situation as defined, in present terms, by the PT principle, not mentioned as such but clearly implied by his comments cited above. These comments are in accord with the QED situation as described here, although, as will be seen presently, his interpretation may be different. He says:

> You may say that [in view of the situation here considered] our belief in elementary particles was a prejudice. But again I think that would be too negative a statement, because all the language which we have used in atomic physics in the last 200 years is based, directly or indirectly, on the concept of the elementary particle. We have always asked the question: "Of what does this object consist, and what is the geometrical or dynamical configuration of the smaller particles in the bigger object?" Actually, we have always gone back to this philosophy of Democritus; but I think we have now learned from Dirac that this was the wrong question. Still, it is very difficult to avoid questions which are already part of our language. Therefore it is natural that even nowadays many experimental physicists, and perhaps even some theoreticians, still look for really elementary particles. They hope, for instance, that quarks, if they exist, may be able to play this role.
>
> I think that this is an error. It is an error because, even if the quarks were to exist, we could not say that the proton consists of three quarks. We would have to say that it may temporarily consist of four quarks and one antiquark, or five quarks and two antiquarks and so on. And all these configurations would be contained [as virtual configurations] in the proton, and again one quark may be composed of two quarks and one antiquark and so on. So we cannot avoid this fundamental situation; but since we still have questions from the old concepts, it is extremely difficult to stay away from them. Very many physicists have looked for quarks, and will probably do so in the future. There has been a very strong prejudice in favor of quarks during the last 10 years, so I think they ought to have been found, if they existed. But that is a matter to be decided by the experimental physicists (Heisenberg 1989, pp. 35–36).

Heisenberg was wise to make this last qualification at the time of writing, in 1976. We now not only have quarks, six of them, or gluons, but also $W^{\pm}$ and Z bosons, and the Higgs boson. Also, it would be more accurate to speak in terms of virtual particles rather than in terms of particles being thus "composed" of other particles. This may, however, be due to Heisenberg's desire not to be too technical (the concept of a virtual particle, as we have seen, requires much qualification) in these lectures, aimed at a wider audience. He is, however, right on his essential points, those concerning these implications of Dirac's discovery of antimatter. What he describes is essentially how we now understand quarks and gluons inside atomic nuclei or mesons, and we cannot observe either independently because of the so-called "asymptotic freedom." They are confined inside hadrons (barions and mesons). His conclusion, following another, now mathematical line of thought, that of symmetry groups, discussed here from Wigner to Wilczek, Wigner's concept of elementary particle as, for each of them, mathematically defined by (it would be more rigorous to say as being associated with) an irreducible representation of the corresponding symmetry group and related developments clearly on his mind. Heisenberg says:

> There remains the question: "What then has to replace the concept of a fundamental particle?" I think we have to replace this concept by the concept of a fundamental symmetry. The fundamental symmetries define the underlying law which determines the spectrum of elementary particles. … [W]hat we have to look for are not fundamental particles, but fundamental symmetries. And we have actually made this decisive change in the concepts, which came about by Dirac's discovery of antimatter, that I do not think we need any further breakthrough to understand the elementary—or rather nonelementary—particles. We must learn to work with this new and unfortunately rather abstract concept of the fundamental symmetries; but this may come in time. (Heisenberg 1989, p. 36)

Heisenberg's final conclusion is correct. We have certainly learned to work with this concept even more efficiently since. Indeed physics had been using this concept effectively for quite a while even by then. The rest of this statement might, however, be debated, as is suggested by the preceding analysis, which follows Bohr's concept of phenomena or atomicity, defined by our experimental technology, and thus, in some cases, as effects of *elementary particles* on this technology, and thus retaining the physical concept of elementary particle in this new definition. It is not clear to what degree, if any, Heisenberg, who was well familiar with Bohr's argument on this point, follows Bohr, given that he thinks of replacing elementary particles with fundamental symmetries, closer to structural realism or related realist approaches to QFT, and in accord with his more mathematically oriented ontological thinking noted above. Fundamental symmetries carry clear affinities with Plato's view of elemental entities, "atoms," as mathematical forms rather than physical entities, in *Timaeus*. As noted in Chap. 2, Heisenberg was fascinated with the dialogue since the time of his discovery of quantum mechanics (he read the dialogue at the time) and later refers to it in the present context as well (Heisenberg 1962, pp. 39, 43). Plato's atoms are Platonic solids, which are invariant under symmetry groups (admittedly, not Plato's concept, even though Plato could not have been aware of these symmetries themselves). As I also noted in Chap. 2 (and in Chap. 1), however, Heisenberg's position in this regard is not entirely clear. Heisenberg, who retained certain affinities with Bohr's thinking throughout his life, does give the concept of a fundamental symmetry a physical flavor (perhaps!) as replacing that of a no-longer elementary "elementary" particle. He does not say, however, that it is a realist concept. In the present view, this concept is, again, defined in terms of mathematical technology by means of which we relate, probabilistically and statistically, to the configurations of our experimental technology. Some of these configurations are, in the present view (although not Heisenberg's view as given here), associated with elementary particles as quantum objects, defined in general as beyond representation or even conception. Indeed, while some among the fundamental symmetries could be more directly associates with actual symmetries in space and time, others involve infinite-dimensional (Hilbert-space) group representation, where such relations are not possible, and the role of these symmetries is manifested in our measurements otherwise.

Although it was not answered by Dirac's theory the question of "What is an elementary particle?" or (these questions cannot be dissociated) "What is a quantum field?" was advanced immeasurably by Dirac's equation and then QED and QFT. Although it still remains unanswered, we reached much further on the trajectory established by Dirac, since the time of Heisenberg's articles, just discussed, in the 1970s, reaching the (QFT) standard model of particle physics. This advancement was guided by the same principles. Following this path, QFT, again, made remarkable progress since its introduction or, again, since Heisenberg's remark, a progress resulting in the electroweak unification and the quark model of nuclear forces, developments that commenced around the time of these remarks. Many predictions of the theory, from quarks to electroweak bosons and the concept of confinement and asymptotic freedom to the Higgs boson, to name just a few, were spectacular, and the field has garnered arguably the greatest number of Nobel Prizes in physics. It was also QFT that led and is still fundamentally linked to, mathematically and physically, string and then brane theories. These theories are of course far short of the physical successes of QFT, if one could indeed speak of any actual physical successes thus far in this case. On the other hand, these theories do have physical *implications*, which are important and motivate the work of string and brane theorists.

6.4 On Renormalization

QFT is not free of difficulties, even apart from its mathematically unwieldy and as yet incomplete character (vis-à-vis quantum mechanics within its proper limit), and it certainly requires further advancement, which may lead to developments beyond QFT itself, as a mathematical technology of handling the tasks posed by high-energy physical phenomena. The standard model is only partially unified thus far (there is no single symmetry group), although achieving such a complete unification is not always seen, including by this author, as imperative, insofar as the set of theories involved predict all the available data in their respective domains. The capacity of QFT to quantize gravity or bring together all forces of nature is a different matter, which led to the rise of string and brane theory, or alternative approaches. This subject is, however, beyond my scope, in part given how far we are from realizing these goals. Therefore it is, as I have noted earlier, far from clear what role, if any, the principles of quantum theory in question in this study will play in any of these approaches or some yet unknown approaches that might be necessary for these tasks. The theory's unwieldiness, although much bemoaned by many, including Dirac, is not necessarily a problem either, insofar as it works.

What are, then, its substantive problems? Those that led to the necessity of renormalization might be, and the subject deserves a brief reflection here, given its special and even unique role in mathematical modeling in physics. This is because, while QFTs (most require renormalization) are extremely accurate in their empirical predictions concerning the statistics of relevant experiments, these theories, in the case of interacting quantum fields, are mathematically unsatisfactory or even objectionable, if legitimate at all, which also poses new questions concerning the relationships between mathematics and physics. It was realized by the early 1930s that the computations provided by QED were reliable only as a first order of perturbation theory and led to the appearance of infinities or divergences in the theory when one attempted to use the formalism for calculations that would provide closer approximations matching the experimental data. These difficulties were eventually handled through the renormalization procedure, which became and has been ever since a crucial part of the machinery of QED and QFT. In the case of quantum electrodynamics, renormalization was performed in the latter 1940s by S-I. Tomonaga, J. Schwinger, and R. Feynman (which brought them a joint Nobel Prize in 1965), with important contributions by others, especially F. Dyson, and earlier H. Bethe and H. Kramers. The Yang-Mills theory, which grounds the standard model, was eventually shown to be renormalizable by M. Veltman and G.'t Hooft in the 1970s (bringing them their Nobel Prize). This allowed a proper development of the standard model of all forces of nature, except for gravity, which has not been given its quantum form thus far.

The renormalization procedure is difficult mathematically even in QED. The mathematics of the electroweak theory or of quantum chromodynamics, QCD, which handles the strong force, is nearly prohibitively difficult. While it is possible to see renormalization in more benign ways, via the so-called "renormalization group" and "effective quantum field theories," and while it has been very effective thus far, its mathematical legitimacy is not really established. Roughly speaking, the procedure might be seen as manipulating infinite integrals that are divergent and, hence, are mathematically illegitimate. At a certain stage of calculation, however, these integrals are replaced by finite integrals through artificial cut-offs that have no proper mathematical justification within the formalism 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. These calculations are, again, experimentally confirmed to a very high degree. As already noted, quantum electrodynamics is the best experimentally confirmed theory thus far. I will not address the technical details of renormalization further here, and will allow myself to refer to (Kuhlman 2015; Cao 1999; Teller 1995) and, for a historical account, to (Schweber 1994, pp. 595–605). The discussion of the relevant subsequent developments, more especially the renormalization group and effective quantum field theories, are also beyond my scope and main concerns here. According to Kuhlman's summary:

> EFTs [effective field theories] describe relevant phenomena only in a certain domain since the Lagrangian contains only those terms that describe particles which are relevant for the respective range of energy. EFTs are inherently [approximate] and change with the range of energy considered. EFTs are only applicable on a certain energy scale, i.e., they only describe phenomena in a certain range of energy. Influences from higher energy processes contribute to average values but they cannot be described in detail. This procedure has no severe consequences since the details of low-energy theories are largely decoupled from higher energy processes. Both domains are only connected by altered coupling constants and the renormalization group describes how the coupling constants depend on the energy. (Kuhlman 2015).

The approach, thus, deals with variations of physical quantities across contiguous scales, and relates distant scales through "effective" descriptions, while linking all scales in a systematic way. The actual physics found at each scale is extracted with the specific computational techniques suited to it. A change in scale is a scale transformation, and the renormalization group is closely related to scale invariance and conformal invariance, which are symmetries that make a system considered appear as the same at all scales, termed scale-similarity. Now, again, citing Kuhman, who cites H. Georgi, although J. Schwinger makes the same point apart from EFTs (Schweber 1994, p. 366):

> The basic idea of this new story about renormalization is that the influences of higher energy processes are localizable in a few structural properties which can be captured by an adjustment of parameters. "In this picture, the presence of infinities in quantum field theory is neither a disaster, nor an asset. It is simply a reminder of a practical limitation—we do not know what happens at distances much smaller than those we can look at directly" (Georgi 1989, p. 456). This new attitude supports the view that renormalization is the appropriate answer to the change of fundamental interactions when the QFT is applied to processes on different energy scales. The price one has to pay is that EFTs are only valid in a limited domain and should be considered as approximations to better theories on higher energy scales. This prompts the important question whether there is a last fundamental theory in this tower of EFTs which supersede each other with rising energies. Some people conjecture that this deeper theory could be a string theory, i.e., a theory which is not a field theory any more. Or should one ultimately expect from physics theories that they are only valid as approximations and in a limited domain? (Kuhlman 2015).

Although, as Kuhlman acknowledges, there is no consensus on this point among physicists or philosophers, this approach provides an arguably better perspective on renormalization and offers more grounds for hope and even prospects for resolving the conflict between mathematics and physics, but it does not and, one might argue, in itself cannot, resolve this conflict. Here, however, accepting renormalization as legitimate, at least sufficiently legitimate, I would like to ask a different question: "Why does QFT, say, QED, contain such infinities, in contrast to quantum mechanics?" The subject is still little explored philosophically, although some helpful insights are found in (Kuhlman 2015; Cao 1999; Teller 1995). Bohr's argumentation, arising from the analysis given in his famous paper with Rosenfeld (Bohr and Rosenfeld 1933), suggests one possible reason, which is of particular interest in the context of this study. Other reasons have been advanced, but I shall not address them here, although some of them may be connected to Bohr's argumentation. The mathematical formalism of QFT, Bohr and Rosenfeld argued, is essentially linked to the same idealization of measurement that is used in low-energy QM regimes, insofar as both idealizations disregard "the atomic structure of the field sources and the measuring instruments" (Bohr and Rosenfeld 1933, p. 520). As Bohr noted in 1937, clearly with his 1933 work with Rosenfeld in mind:

> 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, p. 88; also Bohr and Rosenfeld 1933, pp. 480–481, 520–521)

In classical physics, then, we can disregard measuring instruments altogether, at least ideally and in principle, which is, however, how mathematical models work in physics. In low-energy quantum physics, while we cannot disregard the measuring instruments, we can disregard the atomic constitution of measuring instruments without any mathematical problems in predicting the outcomes of experiments by QM. However, these predictions are now strictly probabilistic or statistical. In high-energy quantum physics, governed by QED and QFT, which are equally probabilistic or statistical, we can still disregard the atomic structure of the measuring instruments, using the same idealization that we use is quantum mechanics, but now at a mathematical cost, that of the necessity of renormalization.

Could it be this idealization, which poses no problems of this type in QM, that is responsible for the appearance of the illegitimate infinities in QFT? Dyson thought so, at least in the case of QED, under impact of Bohr and Rosenfeld's analysis and his discussions with R. Oppenheimer (Schweber 1994, p. 549). Dyson's argument was not without qualifications, and, as will be seen presently, he was not arguing, quite the contrary, that one needs to replace QED or for that matter even avoid renormalization. As he said:

> We interpret the contrast between the divergent Hamiltonian formalism 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 classical discussion of the measurability of field-quantities. The second picture is of collections 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)

Schweber's commentary on this passage clarifies the situation:

> A "real observer" can measure energy levels, and perform experiments involving the scattering of various elementary particles—the observables of S-matrix theory—but cannot measure field strengths in small regions of spacetime. The "ideal" observer, making use of

> the kind of "ideal" apparatus described by Bohr and Rosenfeld, can make measurements of this last kind, and the commutation relations of the fields can be interpreted in terms of such measurements. The Hamiltonian density will presumably always remain unobservable to the real observer whereas the ideal observer, "using nonatomic apparatus whose location in space and time is known with infinite precision" is presumed to be able to measure the interaction's Hamiltonian density. "In conformity with the Heisenberg uncertainty principle, it can perhaps be considered a physical consequence of the infinitely precise knowledge of location allowed to the ideal observer, that the value obtained by him when he measures Hamiltonian density is infinity." If this analysis is correct, Dyson speculated, the divergences of QED are directly attributable "to the fact that the Hamiltonian formalism is based upon an idealized conceptualization of measurability" (Schweber 1994, p. 548; citing Dyson 1949, p. 1755; emphasis added)

In other words, "if this analysis is correct" (it is conjectural), in our interactions, via measuring instruments, with quantum objects we are, thus far, "ideal observers," subject to the uncertainty relations.[16] The observable strata of these instruments are still described classically, just as they are in quantum mechanics, and specifically by disregarding their atomic constitution, which would compel us to take into account such constants as α and m. The necessity of doing so does not mean that we need to actually describe this atomic constitution and the quantum interaction between quantum objects and the measuring instruments. This may not be possible and is not possible in the present view, which precludes such a representation by the RWR principle. However, even if we continue to restrict our theory to probabilistic or statistical predictions, in the absence of any representation of quantum objects and processes (including, again, those occurring in the measuring instruments while interacting with quantum objects), taking into account, in this nonrepresentational way, the atomic structure of the measuring instruments would require a different type of theory. Would such a theory, assuming, again, it is possible, be able to avoid the infinities? Perhaps! The idealized conceptualization of measurability in question is only one of many idealizations found in QFT. Some of these idealizations are shared with QM, such as disregarding the atomic structure of measuring instruments. Although in the present, RWR-principle-based view, no representational claim is made concerning the character of elementary particles or any quantum objects in either QM or QFT, including QED, all these theories inherit their Hamiltonian or Lagrangian formalism, which depends on the idealization of classical objects as dimensionless points endowed with mass. Others are going further than those of QM. Renormalization may be due to the insufficiency of any of these idealizations, or any given combination of them.

But then, how compelling are the reasons for avoiding infinities or getting rid of renormalization? Dyson did not think that his considerations, just considered, implied that QED should be replaced by an essentially different theory. In fact he did not think that that there is, in view of his argument, anything especially wrong with the QED infinities or renormalization. Bohr and Rosenfeld's updated (1950)

[16] The latter were the main subject of Bohr and Rosenfeld's paper, written in response to L. Landau and R. Peierls's argument, contested by Bohr and Rosenfeld, concerning a possible inapplicability of the uncertainty relations in quantum field theory (Bohr and Rosenfeld 1933).

version of their analysis, which addressed renormalization, appears to confirm this view as well (Bohr and Rosenfeld 1950). As Schweber explains:

> If [Dyson's] notion of measurability is accepted, the correlation between expressions which are unobservable to a real observer and expressions which are infinite "is a physically intelligible and acceptable feature of the theory. The paradox is the fact that it is necessary … to start from the infinite expressions in order to deduce finite ones." What may be therefore looked for in "a finite theory" is not necessarily a modification of the present theory which will make all infinite quantities finite "but rather a turning around of the theory so that all the finite quantities shall become primary and the infinite quantities secondary" (Schweber, p. 548; Dyson 1949, p. 1755).

I shall comment on one way in which Dyson thought this "turning around" might be possible presently. Dyson himself concluded that "the purpose of the foregoing remarks is merely to point out that there is no longer [once renormalization is shown to work], as there seemed to be in the past, a compelling necessity for a future theory to abandon some essential features of the present electrodynamics. The present electrodynamics is certainly incomplete, but is [in view of its renormalizability] no longer certainly incorrect" (Dyson 1949, p. 1755; cited in Schweber 1994, p. 548). It might still prove to be incorrect, but it is no longer certainly incorrect!

Nevertheless, Dyson's conjecture that renormalization might arise because we are ideal observers and use (or because we use) idealized measuring instruments is worth pondering a bit further, for the following reason. The Hamiltonian character of the quantum formalism was initially brought into QM from classical mechanics, via Bohr's correspondence principle in its mathematical form, by directly transferring the equations of classical mechanics, while changing their variables to those of QM. In addition, this formalism was made purely predictive and, moreover, probabilistically or statistically predictive, even ideally, and lost its representational capacity, which, along with its ideally exact predictions, defined its use in classical mechanics. The formalism was then "transferred" by Dirac, again, as a probabilistically predictive formalism, into QED, via the adjusted version of the mathematical correspondence principle, adjusted because Dirac's equation must convert, via Pauli's spin theory, into Schrödinger's equation at the quantum-mechanical limit, and the same type of conversion is found elsewhere in QED and QFT. Accordingly, unlike in QM, the formalism was changed formally as well. Nevertheless, it was still a descendent of the Hamiltonian mathematical formalism of classical mechanics. Neither classical mechanics nor QM need to take into account the atomic structure of measuring instruments or sources of fields, classical mechanics naturally (and it can in fact disregard the role of measuring instruments altogether) and QM, it appears, without any real physical justification, but luckily still allowing us to make correct predictions concerning the outcomes of quantum experiments. With QED we run out of luck as concerns the finite nature of the formalism, although not out of luck as concerns physics, because renormalization works.

It follows that, while the correspondence principle, Bohr's great contribution to quantum theory, which was given a mathematical form by Heisenberg, was crucial for the development of QM and then QED, it may have also been primarily responsible for the divergent nature of the mathematical formalism of QED and

QFT. Indeed, the correspondence principle and a possible inadequacy of the Hamiltonian character of the QFT formalism may be responsible for the divergences found in QFT even if they are, physically, due to other factors than our neglect of the atomic structure of measuring instruments, insofar as these factors are reflected in the Hamiltonian formalism of the theory. The Hamiltonian framework may also be responsible for us not being able to turn it around the way Dyson suggested—by making primary the finite rather than the infinite quantities in the theory. Dyson (and a few others, for example, at one point Dirac) believed that future quantum-field theories "which would probably still be based on a modified Lagrangian approach would be able to address more local and detailed questions than scattering matrix elements did" (Schweber 1994, p. 549). That has not really proved to be the case thus far, but in any event, it would still be too close to classical mechanics, for better or worse. Thus far, however, it has been mostly for the better, both justifying classical physics itself beyond its limits and, through classical physics, giving all modern physics its continuity, amidst its several revolutions, radical as these revolutions might have been. We might need both, this continuity of physics and these radical transformations. At least, we continue to remain lucky with our physics, because renormalization works thus far, at least, again, insofar as our quantum theories can disregard gravity.

Our future fundamental theories might prove to be finite (some versions of string and brane theory appear to hold such a promise), thus proving that the necessity of renormalization is merely the result of the limited reach of our quantum theories at present. The emergence of other finite alternatives, proceeding along entirely different trajectories, may not be excluded either. While, however, a finite theory may be preferable, renormalization may not be a very big price to pay for the theory's extraordinary capacity to predict the increasingly complex manifold of quantum phenomena that physics has confronted throughout the history of QFT. And then, a finite theory may also not be possible, in which case renormalization will continue to be our main hope. For now, QED or rather QEDs continue to provide an extraordinarily effective mathematical technology to handle ever-new types of phenomena observed in ever-new configurations of high-energy measuring technology. LHC is the latest example of the machine (in either sense) for creating such configurations, one of which (a very complex one) enabled us to confirm the existence of the Higgs boson, an essential component of the standard model. Some of the earlier discoveries, such as those of the electroweak (W^+, W^-, and Z) bosons, the top quark, and the tau-lepton are hardly less significant, and the experimental technologies accompanying them are nearly equally spectacular, although the technology, mathematical and experimental, that enabled the discovery of the Higgs was helped and was even made possible by computer technology. The latter has been part of our QED technologies for quite a while, not the least in the history of the renormalization group, beginning with K. Wilson's pioneering use of it. However, its use reached a hitherto unprecedented degree in the discovery of the Higgs boson, which would not be possible without it, not the least in assessing the statistical data involved and thus digitalizing the QP/QS principle as well. The discovery of the Higgs boson may still require further confirmation or experimental finessing as concerns its physical nature (e.g., its exact mass), and the data involved may require advancing and perhaps adjusting the standard model itself, which is not entirely complete in any event. However, the discovery was deemed confirmed enough (that two independent detectors indicated a likely presence of the Higgs particle helped) to award the 2013 Nobel Prize to F. Englert and P. Higgs, who were among those involved in development of the mathematical theory of the Higgs field, along with (alphabetically) R. Brout, G. Guralnik, C. Hagen, and T. Kibble. Of course, as all technologies, the mathematical technology of QED and the experimental technology of the type found in LHC might, and even one day must, become obsolete and be replaced by other technologies. Indeed, LCH is already deemed obsolete and the search for funding for a new, more powerful accelerator is on. Thus far, however, both have worked marvelously, beginning with Dirac's introduction of quantum electrodynamics and his equation for the relativistic electron, which remains a spectacular and illuminating case, and led to the discovery of the Higgs boson. A pentaquark (predicted by QFT quite a while ago) is the latest confirmed discovery, but undoubtedly not the last. New discoveries are bound to follow and may confirm the power of QFT or defeat it. Some very recently observed events, may pose some tantalizing questions in this regard (Castelvecchi 2015). A defeat could be more exciting, because it will require a new theory, perhaps an entirely new kind of theory. Either way physics will win. Our fundamental theories are bound to be defeated sooner or later. Physics always wins, at least it has always done so since Galileo or even Aristotle, or the pre-Socratics, such as Democritus.

QFTs remain incomplete insofar as they do not cover the scales beyond the standard scale of high-energy physics, where in particular the effects of gravity would need to be taken into account. String or brane theory is still the most widely entertained proposal, again, born from and closely and fundamentally related to QFT. The theory is even more complex mathematically than QFT and remains highly hypothetical physically, with uncertain chances to be connected to experiments in any near future, although certain consequences of the theory could eventually be tested. These factors have made the theory controversial. While it has many prominent advocates and promoters, and a large (although no longer quite as large) cohort of practitioners and adherents, it also has quite a few detractors, sometimes to the point of dismissing it as useless and even "dangerous" metaphysics. But then, most proposed alternatives, such as, say, loop quantum gravity, are hardly less hypothetical or controversial. These controversies are beyond my scope here. Instead, I shall, in the next chapter, the closing chapter of this study, consider a possible alternative approach to high-energy quantum physics, an approach, based in quantum information theory. This approach has recently led to an alternative derivation of Dirac's equation from the principle of quantum information theory alone, without using special relativity, while, however, using the locality principle (D'Ariano and Perinotti 2014). This is, to be sure, only a first step toward QFT as quantum information theory, but an important one. Besides, quantum information theory is a major development in its own right, not the least as concerns the fundamental principles of quantum theory, and as such it cannot be bypassed by a study, like the present one, concerned with these principles.

Chapter 7
The Principles of Quantum Information Theory, Dirac's Equation, and Locality Beyond Relativity

Abstract This chapter considers the principles of quantum information theory in conjunction with the principles of QM and QFT, as discussed in earlier chapters. Several recent approaches to quantum information theory will be addressed, such as those by B. Coecke and, more extensively, L. Hardy. Particular attention will, however, be given to the recent work by G. M. D'Ariano and co-workers, G. Chiribella and P. Perinotti. The approach to quantum information theory that they developed allowed D'Ariano and Perinotti to derive Dirac's equation from the principles of quantum information alone, rather than, as Dirac did, by combining the principles of quantum theory and special relativity. The locality principle, however, plays a key role in this derivation, which, I suggest, may have important implications for fundamental physics. I will proceed as follows. After brief introductory remarks given in Sects. 7.1, 7.2 considers D'Ariano, Chiribella, and Perinotti's program of finite-dimensional quantum theory (QFDT), based on these principles, and related work, especially that of Hardy, in terms of the operational language of circuits. Sect. 7.3 discusses D'Ariano and Perinotti's derivation of Dirac's equation.

7.1 Introduction

While most current approaches to fundamental physics beyond QFT, such as string or brane theory, or several other proposals for quantum gravity, are primarily constructive theories (although they also use principles), M. D'Ariano, G. Chiribella, and A. Perinotti's approach to QFDT and to Dirac's equation is primarily a principle one. It has a constructive dimension as well, given the role of the concept of quantum cellular automata (QCA) there. This aspect of their approach will, however, only be mentioned in passing, primarily to the degree it can be interpreted in a non-realist and, hence, strictly principle way, in accordance with the overall philosophical position adopted by this study, even if not by D'Ariano, Chiribella, and Perinotti's. (I am not entirely certain from their writings thus far as regards D'Ariano, Chiribella, and Perinotti's's view concerning the ontological status of QCA.)

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That Dirac's equation can be derived strictly from "principles of information processing," without using relativity, suggests, at least to this writer, that the ultimate architecture of matter, including that related to the locality principle (used in this derivation) may be quantum, possibly something beyond quantum, but not relativistic-like, physically, mathematically, or epistemologically. If so, relativity, at least special relativity (but, not inconceivably, general relativity as well), is a surface-scale effect of this deeper quantum or beyond-quantum fundamental constitution of nature, although the overall set of principles of information processing in question extends beyond those defining this constitution. Admittedly, this approach to QED and QFT is at an early stage (it is much more advanced as concerns QFDT, which generally neglects relativistic effects), given that thus far it has only given a derivation of Dirac's equation. This is, however, a major step in extending the quantum-informational approach beyond QM. I cannot, within my scope, consider the technical aspects of this derivation and also have to bypass several key features of the authors' physical argument, including, again, the role of QCA in their approach, developed in several articles, to which and further references there I refer the reader (Chiribella et al. 2010, 2011; D'Ariano and Perinotti 2014).

7.2 The Principles of Quantum Information Theory and the Operational Language of Circuits

D'Ariano, Chiribella, and Perinotti's program is inspired by "a need for a *deeper understanding of quantum theory* itself from fundamental principles," which, they contend, has never been really achieved, and is motivated by the development of quantum information theory, and in part for that reason deals with discrete variables and the corresponding (finite-dimensional) Hilbert spaces.[1] Among the key predecessors is the work of C. A. Fuchs's (e.g., Fuchs 2003) and L. Hardy (e.g., Hardy 2001), equally motivated by the aim of deriving QFDT from a more natural set of principles, postulates, or axioms.[2] Hardy's paper just cited was, arguably, the first derivation of that type. Neither of these two approaches is constructive or realist, nor, again, is that of D'Ariano, Chiribella, and Perinotti, or in any event, they allow for nonrealist, RWR-principle-based interpretations. The different terms just mentioned, "principles," "postulates," and "axioms" (all of which are used by D'Ariano, Chiribella, and Perinotti as well), do not affect the essential aspects of these programs, which could, I would argue, all be viewed in terms of principle theories and principle thinking, as considered in this study. All these attempts are, again, thus far limited to QFTD (again, generally neglecting

[1] By "theory" they refer, more in accord with the term "model" as used in this study, to the mathematical structure of quantum theory, rather than to its physical aspects.

[2] See Chiribella et al. (2011) for further references. For introductions to the subject of quantum information theory and further references, see Jaeger (2007) and Mermin (2007). See also J. A. Wheeler's visionary manifesto (Wheeler 1990). Fuchs's work more recently "mutated" to a somewhat different, if related, program, that of quantum Bayesianism or QBism, discussed in Chap. 4 (e.g., Fuchs et al. 2014 and further references there).

relativistic effects). I should note that, while arguing for the role of fundamental principles in quantum theory, from QM to QFT, in its emergence and functioning, this study does not claim that a rigorous derivation of QM, let alone QFT, from such principles has been achieved. This remains an open question, even more so when dealing with continuous variables (to which most of my discussion has been restricted thus far), where the application of the principles of quantum information theory is more complex as well.[3] As will be seen, D'Ariano, Chiribella, and Perinotti's program, or Hardy's work, has important affinities with both Bohr's thinking, as concerns the role of measuring technology in quantum theory ("circuits"), and Heisenberg's thinking leading him to his discovery of quantum mechanics, as considered in Chap. 2. As discussed throughout, however, Bohr's and Heisenberg's thinking is fundamentally connected in turn, in part via complementarity, which, especially the EPR complementarity, is significant to D'Ariano, Chiribella, and Perinotti's argument, even though the concept is only implicit there.

According to Chiribella, D'Ariano, and Perinotti: "[T]he rise of quantum information science moved the emphasis from logic to information processing. The new field clearly showed that the mathematical principles of quantum theory imply an enormous amount of information-theoretic consequences. … The natural question is whether the implication can be reversed: [I]s it possible to retrieve quantum theory from a set of purely informational principles?" (Chiribella et al. 2011, p. 1).[4] The authors aim to offer "a complete derivation of finite-dimensional quantum theory based on purely operational [informational] principles:"

> In this paper we provide a complete derivation of finite dimensional quantum theory based on purely operational principles. Our principles do not refer to abstract properties of the mathematical structures that we use to represent states, transformations, or measurements, but only to the way in which states, transformations, and measurements combine with each other. More specifically, our principles are of *informational* nature: they assert basic properties of information processing, such as the possibility or impossibility to carry out certain tasks by manipulating physical systems. In this approach the rules by which information

[3] Cf. (De Raedt et al. 2014), which pursues a logical-inference-based approach to deriving standard (infinite-dimensional) quantum mechanics, specifically by considering Schrödinger's derivation of his equation as presented in his first paper (Schrödinger 1926b) considered earlier. Their argument, however, still relies on certain fundamental quantum principles, such as, expressly, "uncertainty about individual events," referring, I surmise, to elementary individual quantum processes, for otherwise this uncertainty is found in classical statistical physics or chaos theory (De Raedt et al. 2014, p. 47). They are quite right to stress the role of logical inference in scientific reasoning, following Pólya (1954) and Jaynes (2003). The question of "the first principles" (the initial assumptions, postulates, and so forth) remains open, however. Thus, both Heisenberg's and Schrödinger's derivation of quantum mechanics depended on classical mechanics and, in Heisenberg, the mathematical correspondence principle, which the quantum-informational programs in question here want to avoid, as not sufficiently first-principle-like. Besides, as discussed in Chap. 2, neither Heisenberg's nor Schrödinger's "derivation" was free from guesses that, while, to be sure, accompanied by plausible reasoning and logical inferences, would be difficult to fully explain by them. On the other hand, the principal role of classical physics in both cases is unquestionable. See also the discussion of the role of the Hamiltonian formalism in QM and QFT in Chap. 6, Sect. 6.4.

[4] References cited from Chiribella et al. (2010, 2011) and D'Ariano and Perinotti (2014) are adjusted in accordance with the bibliography of this study.

> can be processed determine the physical theory, in accordance with Wheeler's program "it from bit," for which he argued that "all things physical are information-theoretic in origin" (Wheeler 1990). Note [however, that] our axiomatization of quantum theory is relevant, as a rigorous result, also for those who do not share Wheeler's ideas on the informational origin of physics. In particular, in the process of deriving quantum theory we provide alternative proofs for many key features of the Hilbert space formalism, such as the spectral decomposition of self-adjoint operators or the existence of projections. The interesting feature of these proofs is that they are obtained by manipulation of the principles, without assuming Hilbert spaces from the start. (Chiribella et al. 2011, p. 2)[5]

Among the principles advanced by the authors, the purification principle plays a particularly, indeed uniquely, important role in their program, as an essentially quantum principle, which distinguished QFDT from classical information theory, the single principle necessary to do so:

> The main message of our work is simple: within a standard class of theories of information processing, quantum theory is uniquely identified by a single postulate: *purification*. The purification postulate, introduced in (Chiribella et al. 2010), expresses a distinctive feature of quantum theory, namely that the ignorance about a part is always compatible with the maximal knowledge of the whole. The key role of this feature was noticed already in 1935 by Schrödinger in his discussion about entanglement (Schrödinger 1935b), of which he famously wrote "I would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." In a sense, our work can be viewed as the concrete realization of Schrödinger's claim: the fact that every physical state can be viewed as the marginal of some pure state of a compound system is indeed the key to single out quantum theory within a standard set of possible theories. It is worth stressing, however, that the purification principle assumed in this paper includes a requirement that was not explicitly mentioned in Schrödinger's discussion: if two pure states of a composite system AB have the same marginal on system A, then they are connected by some reversible transformation on system B. In other words, we assume that all purifications of a given mixed state are equivalent under local reversible operations. (Chiribella et al. 2011, p. 2)

The authors also speak of "the purification postulate," and they refer to the remaining informational principles as "axioms," because "as opposed to the purification 'postulate,' … they are not at all specific [to] quantum theory" (Chiribella et al. 2011, p. 3). While these terminological distinctions, especially the second one, may be somewhat tenuous, they do not affect Chiribella, D'Ariano, and Perinotti's argument. These postulates and axioms do define principles in the present sense (and as I explained from the outset, principles generally involve postulates), and in particular, jointly, provide the *guidance* for deriving the finite-dimensional quantum theory. Besides, as will be seen presently, the authors state their strictly operational principles later in the article (Chiribella et al. 2011, p. 6).

The purification principle is a new principle. First, even beyond the fact that it has a richer content than does Schrödinger's statement, Schrödinger never saw his claim as a principle (rather a "trait"), perhaps also because of his critical view of quantum mechanics. The principle could be related, along the lines discussed earlier,

[5] It might be noted that, unlike von Neumann, Heisenberg and Dirac did not assume Hilbert spaces from the start either, but rather *arrived* at them with the help of, even if not quite *derived* them from, the fundamental principles they assumed.

to Bohr's concept or principle of complementarity, which, as discussed in Chap. 3, implies that "the ignorance about a part [one of the two complementary parts] is always compatible with the maximal knowledge of the whole," making the very language of "parts" and "whole" provisional. There is no whole of which these parts are parts. Chiribella, D'Ariano, and Perinotti's formulation to the effect that "every physical state can be viewed as the marginal of some pure state of a compound system" avoids this difficulty. As also discussed in Chap. 3, Bohr saw the EPR experiment (the background for Schrödinger's claim here cited and for his concept of entanglement) as a manifestation of complementarity, and specifically as expressing what I called the EPR complementarity, underlain by the RWR principle (Bohr 1935, 1987, v. 2, p. 59).[6] Thinking in terms of "circuits" (not found in Schrödinger) is also close to Bohr's view of the role of measuring instruments in the constitution of quantum phenomena.

It is worth briefly revisiting these connections to illustrate the profound nature of the purification postulate and its implications. The EPR complementarity, I argue, gives a new dimension to the concept and principle of complementarity, insofar it reveals a new way of the composition of the "whole" from complementary "parts," because the "complementary" parts do not add up to a whole in the way parts do in classical physics or relativity, or elsewhere. Each part is still the only whole available at any given moment of time. This situation, however, changes, even in quantum mechanics, the relationships between our knowledge concerning the "whole" and our knowledge concerning parts of this "whole." This change is manifested jointly in the complementary measuring arrangements or "the circuits of devices," and the formalism of quantum mechanics, and indeed by conjoining them, which is the essence of complementarity. Bohr, arguably, deserves more credit for this aspect of the situation, as well as for realizing this conjunction, and in this sense is, again, closer to Chiribella, D'Ariano, and Perinotti's argument than Schrödinger is. The purification principle is given a more rigorous mathematical expression (in the finite-dimensional case) by Chiribella, D'Ariano, and Perinotti than one finds in Bohr. Bohr was not especially concerned with finding mathematical expressions for his key principles, although, as we have seen, Bohr was well aware of how the EPR situation is expressed in the formalism, and realized the role of this expression (Bohr 1935, pp. 696–697, 697n.1). There is no invocation or, it appears, thinking in terms of circuits of devices in Schrödinger, who, however, deserves much credit for grasping the "trait" in question, in some respects, more deeply, than anyone else at the time. I have considered Schrödinger's argument in more detail earlier and shall only recapitulate the most essential point, invoked by Chiribella, D'Ariano, and Perinotti:

> When two systems, of which we know the states by their respective representatives, enter into temporary physical interaction due to known forces between them, and when after a time of mutual influence the systems separate again, then they can no longer be described in the same way as before, viz. by endowing each of them with a representative of its own.

[6] Bub's article, cited earlier, also considers QM as a principle theory in Einstein's sense in order to address the EPR-type experiments and quantum entanglement (Bub 2000).

> I would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought. By the interaction the two representatives [the quantum states] have become entangled. Another way of expressing the peculiar situation is: the best possible knowledge of a *whole* does not necessarily include the best possible knowledge of all its *parts*, even though they may be entirely separate and therefore virtually capable of being 'best possibly known,' i.e., of possessing, each of them, a representative of its own. The lack of knowledge is by no means due to the interaction being insufficiently known—at least not in the way that it could possibly be known more completely—it is due to the interaction itself" (Schrödinger 1935b, p. 155).

The last qualification is, again, crucial: QM and complementarity, inherent in this situation, provide as complete a knowledge concerning, at the very least, entangled systems as possible, a Bohr-complete knowledge. An Einstein-complete knowledge concerning such systems, which would demand that this interaction could, *in principle*, be known, is not possible, at least, again, as things stand now. This, Einstein hoped, would change one day when a better theory is developed, as he believed was bound to happen. He was, again, reluctant to see this situation as uncircumventable in view of the available experimental evidence, which thus far confirms this state of affairs and quantum mechanics, less unconditionally in the latter case. This is because one cannot exclude that an alternative formalism could predict the data in question, as Bohmian mechanics does, at the expense of nonlocality, the possibility or impossibility of which is, again, a crucial aspect of the situation. These considerations are, however, secondary at the moment, given that we deal with the derivation of the standard QFDT. Chiribella, D'Ariano, and Perinotti's purification principle offers a rigorous mathematical expression of this situation, perhaps the most rigorous available thus far.

While, however, having an essential and even unique role in their operational derivation of QFDT, the purification principle is not sufficient to do so. They need five additional axioms:

> In addition to the purification postulate, our derivation of quantum theory is based on five informational axioms. The reason why we call them "axioms," as opposed to the purification "postulate," is that they are not at all specific of [to?] quantum theory. These axioms represent standard features of information processing that everyone would, more or less implicitly, assume. They define a class of theories of information processing that includes, for example, classical information theory, quantum information theory, and quantum theory with superselection rules. The question whether there are other theories satisfying our five axioms and, in case of a positive answer, the full classification of these theories is currently an open problem.
>
> Here we informally illustrate the five axioms, leaving the detailed description to the remaining part of the paper:
>
> 1. *Causality*: the probability of a measurement outcome at a certain time does not depend on the choice of measurements that will be performed later.[7]
> 2. *Perfect distinguishability*: if a state is not completely mixed (i.e., if it cannot be obtained as a mixture from any other state), then there exists at least one state that can be perfectly distinguished from it.

[7] As explained earlier, this principle is different from that of classical causality (indeed already by virtue of the principle's appeal to probability), while being consistent with locality. See also Pawlowski et al. (2009) for a suggestive quantum-informational concept of causality.

3. *Ideal compression:* every source of information can be encoded in a suitable physical system in a lossless and maximally efficient fashion. Here *lossless* means that the information can be decoded without errors and *maximally efficient* means that every state of the encoding system represents a state in the information source.
4. *Local distinguishability*: if two states of a composite system are different, then we can distinguish between them from the statistics of local measurements on the component systems.
5. *Pure conditioning:* if a pure state of system AB undergoes an atomic measurement on system A, then each outcome of the measurement induces a pure state on system B. (Here *atomic measurement* means a measurement that cannot be obtained as a coarse graining of another measurement.) (Chiribella et al. 2011, p. 3)

"All these *axioms* are satisfied by classical information theory," and it is "the purification *postulate*" that singles out quantum information theory (Chiribella et al. 2011, p. 3; emphasis added). The authors also "make precise the usage of the expression 'operational principle' in the context of [their] paper," which point should not be missed if one wants to properly understand their argumentation, as grounded in fundamental physical (informational) principles:

> By [an] operational principle we mean here a principle that can be stated using only the operational-probabilistic language, i.e., using only
>
> 1. the notions of system, test, outcome, probability, state, effect, transformation;
> 2. their specifications: atomic, pure, mixed, completely mixed; and
> 3. more complex notions constructed from the above terms (e.g., the notion of "reversible transformation").
>
> The distinction between operational principles and principles referring to abstract mathematical properties, mentioned in the Introduction, should now be clear: for example, a statement like "the pure states of a system cannot be cloned" is a valid operational principle, because it can be analyzed in basic operational-probabilistic terms as "for every system A there exists no transformation C with input system A and output system AA such that C $|\varphi) = |\varphi)|\varphi)$ for every pure state φ of A." On the contrary [By contrast?], a statement like "the state space of a system with two perfectly distinguishable states is a three-dimensional sphere" is not a valid operational principle, because there is no way to express what it means for a state space to be a three-dimensional sphere in terms of basic operational notions. The fact that a state [space] is a sphere may be eventually derived from operational principles, but cannot be assumed as a starting point. (Chiribella et al. 2011, p. 6)

This distinction is essential, even though operational principles must be given a proper mathematical expression in the formalism of the theory, which in this case is accomplished through the operational language of circuits, or of the mathematical-experimental technology of circuits.

The necessity of these additional axioms is not surprising. As discussed above, Heisenberg's grounding principles—the QD principle, the QP/QS principle, and the *proto*-RWR principle (which suspended, rather than strictly precluded, the representation of quantum objects and processes)—were not sufficient to derive quantum mechanics either. To do so, he needed the correspondence principle, to which he gave a mathematical expression. Dirac, too, needed, a larger set of principles, postulates, and assumptions (even apart from those of relativity) for deriving his equation than those that define its specifically quantum character, including, again, a form of mathematical correspondence principle. What is remarkable, however, is that one needs only one "postulate" to distinguish classical and quantum information theory. A similar situation transpires in Hardy's article mentioned above, where indeed this difference turns not only on a single "axiom," but on the use of a single word, "continuity," technically a single feature of the situation, "the *continuity* of a reversible transformation between any two pure states" (Hardy 2001, p. 2; emphasis added).[8] Hardy's continuity axiom is different from Chiribella, D'Ariano, and Perinotti's purification postulate. The point is that in both cases one single axiom differentiates quantum from classical systems. On the one hand, there may not be a single system of axioms or postulates from which QFDT could be derived. On the other hand, all these characteristic traits, *the* characteristic traits, beginning with the role of h, may well reflect something, singular about quantum phenomena, which is also manifested in the key traits of the mathematical formalism of QFDT, such as noncommutativity and the role of complex numbers. This is more immediately apparent in the case of Schrödinger's "characteristic trait" or (correlatively) Chiribella, D'Ariano, and Perinotti's postulate, which concern entanglement. But this is also true in the case of Hardy's continuity axiom. According to M. Dickson: "The quantum state is 'continuous' (in Hardy's sense) because for any two pure states, there is another pure state that is 'between' them, and in fact this 'middle' state is a superposition of the two original states. In other words, continuity holds precisely because the [quantum-mechanical] superposition *principle* holds. Continuity fails in the classical theory because the superposition principle fails there. From this point of view, it is less surprising—though not necessarily less important—that continuity is what makes the difference, in Hardy's framework, between classical and quantum theory" (Dickson 2011, p. 321; emphasis added). The superposition principle, I might add, is fundamentally linked to both complex numbers and noncommutativity in the formalism, and, via Born's or related rules, to the probabilistic or statistical predictions, in accordance with the QP/QS principle, embedded in both Chiribella, D'Ariano, and Perinotti's and Hardy's axioms. Hardy expressly relates to statistics in his axioms (Axiom 1), in affinity with the statistical Copenhagen interpretation proposed in Chap. 4: "Axiom 1. *Probabilities*. Relative frequencies (measured by taking the proportion of times a particular outcome is observed) tend to the same value (which we call the probability) for any case where a given measurement is performed on a ensemble of n systems prepared by some given preparation in the limit as n becomes infinite" (Hardy 2001, pp. 1, 10). Hardy's axiom is in fact statistical, and could be interpreted in terms of the statistical Copenhagen interpretation. Chiribella, D'Ariano, and Perinotti also speak of "the *statistics* of local measurements" as well (Chiribella et al. 2011, p. 3; emphasis added).

There are instructive specific parallels between Chiribella, D'Ariano, and Perinotti's and Heisenberg's approaches. The QP/QS principle is present in both cases, given that Chiribella, D'Ariano, and Perinotti see QFDT (or by implication

[8] In accordance with the definition given in Chap. 1, "postulate" may be a better term, because one can hardly have the self-evidence of "axioms" in such cases, but this is a secondary matter, which, as I said, does not affect the essence of Hardy's argument itself.

QM and QFT) as an "operational-probabilistic theory" of a special type, defined by the purification postulate (Chiribella et al. 2011, p. 3). As they say: "The operational-probabilistic framework combines the operational language of circuits with the toolbox of probability theory: on the one hand experiments are described by circuits resulting from the connection of physical devices, on the other hand each device in the circuit can have classical outcomes and the theory provides the probability distribution of outcomes when the devices are connected to form closed circuits (that is, circuits that start with a preparation and end with a measurement)" (Chiribella et al. 2011, p. 3). This is similar to Heisenberg's thinking, as discussed above, although the concept of "circuit," not found in Heisenberg, and its role in Chiribella, D'Ariano, and Perinotti's approach is closer to Bohr's view of the role of measuring apparatus in his interpretation. Heisenberg found the formalism of QM by using the mathematical correspondence principle, which allowed him to adopt the equations of classical mechanics while changing the classical variables. The mathematical correspondence principle was not exactly the first principle, because it depended on the equations of classical mechanics at the classical limit where h could be neglected, which one might prefer to be a consequence of fundamental quantum principles. Heisenberg's variables were more of a guess, albeit a logical guess, given that at stake were the probabilities of transitions in either direction (toward higher or lower value) between the energy levels of the electron in the hydrogen atom. In fact, as noted above, using the second quantization is more of a derivation in this case. Heisenberg needed new variables because the classical variables did not give Bohr's frequencies rules for spectra, and the corresponding probabilities or statistics. Heisenberg discovered, again, more or less by a guess, that these rules are satisfied by, in general, noncommuting matrix variables with complex coefficients, related to amplitudes, from which one derives, by means of a Born-type rule, probabilities or statistics for transitions between stationary states, transitions manifested in spectra, observed in measuring devices.

In this way, Heisenberg's derivation was essentially related to the fact that the observable parts of measuring instruments are classically describable devices, which are akin to Chiribella, D'Ariano, and Perinotti's "operational circuits" and related quantum-informational schemes, except that these schemes, in order to derive QFDT, start with giving a mathematical structure to these circuits.

Chiribella, D'Ariano, and Perinotti, thus, aim to arrive at mathematical architecture of QFDT in a more first-principle-like way, in particular, independently of classical physics or of quantizing it. (The latter, to begin with, does not have discrete variables, such as spin, which are purely quantum, with which QFDT is associated.) This is, again, accomplished by using the rules governing the structure of operational devices, circuits. These rules are *more empirical*, but they are *not completely empirical*, because circuits *are* given a mathematical structure. But then, as discussed earlier, with Einstein's help, nothing is ever completely empirical even in observation. This move of Chiribella, D'Ariano, and Perinotti is *parallel* to Heisenberg's arrangement of the quantities he used into the square tables, matrices, which we now take for granted, but which, as explained in Chap. 2, was in itself a mathematical invention giving structure, architecture to the manifolds of physical quantities, ultimately linked to probabilities of transitions between stationary states. In fact, as just noted, this could even be seen in relation to circuits where these quantities are observed or (probabilistically) predicted as spectra. Heisenberg, again, needed other elements, just described, to establish this architecture fully, quite differently from how Chiribella, D'Ariano, and Perinotti aim to do this. Nevertheless, in this case, too, one is concerned with the architecture, that of circuits, from which the mathematical architecture of the theory emerges.[9] We keep in mind that in both approaches, one only deals with "mathematical representations" linked to and providing the probabilities or statistics of the outcomes of discrete quantum experiments, thus in accord with the QD and QP/QS principles, without providing any representation of quantum processes themselves, thus in accord with the RWR principle. As Chiribella, D'Ariano, and Perinotti say: "The rules summarized in this section define the operational language of circuits, which has been discussed in detail in a series of inspiring works by Coecke" (Chiribella et al. 2011, p. 4). Coecke and others working in this area primarily aimed at recasting the architecture of at least the finite-dimensional Hilbert-space formalism into a new language, rather than deriving QFTD. According to Coecke:

> The underlying mathematical foundation of this high-level diagrammatic formalism relies on so-called *monoidal categories*, a product of a fairly recent development in mathematics. Its logical underpinning is *linear logic*, an even more recent product of research in logic and computer science. These monoidal categories do not only provide a natural foundation for physical theories, but also for proof theory, logic, programming languages, biology, cooking. … So the challenge is to discover the necessary additional pieces of structure that allow us to predict genuine quantum phenomena. These additional pieces of structure represent the capabilities nature has provided us with to manipulate entities subject to the laws of quantum theory. (Coecke 2009, p. 1)

The concept of circuit, thus, represents the arrangements of measuring instruments that are capable (only certain instruments are) of quantum measurements and predictions, which are probabilistic or statistical, and sometimes, as with the EPR or the EPR-Bohm type of experiments, or in the double-slit experiment, are correlated, and as such have specific architecture. This is also a realist representation of these arrangements, which is possible, because they are described by classical physics, even though they interact with quantum objects, and thus have a quantum stratum, which enables this interaction, disregarded by this classical representation without, as explained earlier, any detriment to either measurements or predictions.[10] The specific architecture and the properties of circuits, "*the necessary additional pieces of structure*," help and ideally enable us to derive the formalism of QFDT, or one might say of the "arrangements" of elements (such as Hilbert-space operators) that define this formalism. This formalism, again, has probabilistically or statistically

[9] A remarkable precursor to this approach is Schwinger's framework of "the algebra [of the symbols] of quantum measurements," which in effect extends from Heisenberg's approach and Bohr's thinking, from which Schwinger borrows the terms "kinematic" and "symbolic," respectively (Schwinger 1988, 2001). See Jaeger (2016).

[10] The information thus obtained is physically classical, but its architecture and mode of transmission are quantum; that is, it cannot be classically generated.

predictive relations to what is observed, without, in nonrealist interpretations, representing quantum objects and processes.

One can gain further insights into the architecture of circuits from Hardy's approach, where circuits play an equally central role. He arrives at a different set of main assumptions necessary to derive QFDT than those of Chiribella, D'Ariano, and Perinotti, but the main strategy is the same: it is defined by establishing the architecture of circuits that, perhaps with additional axioms, would allow one to derive the mathematical formalism of QFDT. According to Hardy:

Circuits Have

- A setting, s(H), given by specifying the setting on each operation.
- An outcome set, o(H), given by specifying the outcome set at each operation (equals o(A) × o(B) × o(C) × o(D) × o(E) in this case). We say the fragment "happened" if the outcome is in the outcome set.
- A wiring, w (E), given by specifying which input/output pairs are wired together.

(Hardy 2013, p. 7)

With this definition in hand, I shall consider some of Hardy's fundamental assumptions in a different paper, which make my main point more transparent. According to Hardy:

We will make two assumptions to set up the framework in this paper. …

Assumption 1. The probability, Prob (A), for any circuit, A (this has no open inputs or outputs), is well conditioned. It is therefore determined by the operations and the wiring of the circuit alone and is independent of settings and outcomes elsewhere. (Hardy 2010, p. 11)

This is a physical postulate, essentially that of spatial and temporal locality (or the corresponding causality), combined with probability or statistics, along the lines of the QP/QS principle. The task now becomes how to derive a QFDT that could correctly predict these probabilities. One needs another assumption:

Assumption 2: **Operations are fully decomposable**. …. We assume that any operation $A_{a1b2\ldots c3}^{d4e5\ldots f6}$ can be written, … in a symbolic notation,

$$A_{a1b2\ldots c3}^{d4e5\ldots f6} \equiv^{d4e5\ldots f6} A_{a1b2\ldots c3} X_{a1}^{a1} X_{b2}^{b2} \ldots X_{c3}^{c3}\, {}_{d4}X^{\mathrm{d4}}\, {}_{e5}X^{\mathrm{e5}} \ldots {}_{f6}\, X^{\mathrm{f6}}$$

In words we will say that any operation is equivalent to a linear combination of operations each of which consists of an effect for each input and a preparation for each output. …

We allow the possibility that the entries in ${}_{d4e5\ldots f6}A_{a1b2\ldots c3}$ are negative (and this will, indeed, be the case in quantum theory). Hence, in general, this cannot be thought of as physical mixing. …

Assumption 2 introduces a *subtly* different attitude than the usual one concerning how we think about what an operation is. Usually we think of operations as effecting a transformation on systems as they pass through. Here we think of an operation as corresponding to a bunch of separate effects and preparations. We need not think of systems as things that preserve their identity as they pass through—we do not use the same labels for wires coming out as going in. This is certainly a more *natural* attitude when there can be different numbers of input and output systems and when they can be of different types. Both classical and quantum transformations satisfy this assumption. In spite of the different attitude just mentioned, we can implement arbitrary transformations, such as unitary transformations in quantum theory, by taking an appropriate sum over such effect and preparation operations. (Hardy 2010, pp. 19–20; emphasis added)

After a technical discussion of "duotensors" (which I put aside), Hardy suggests an intriguing principle:

> *Physics to mathematics correspondence principle*. For any physical theory, there [exist] a small number of simple hybrid statements that enable us to translate from the physical description to the corresponding mathematical calculation such that the mathematical calculation (in appropriate notation) looks the same as the physical description (in appropriate notation).
>
> Such a principle might be useful in obtaining new physical theories (such as a theory of quantum gravity). Related ideas to this have been considered by category theorists (Coecke and Paquette 2009). A category of physical processes can be defined corresponding to the physical description. A category corresponding to the mathematical calculation can also be given. The mapping from the first category to the second is given by a functor (this takes us from one category to another). (Hardy 2010, p. 39)

This "hybrid" construction is necessary to give structure or architecture to circuits so that it could then be translated into a proper formalism of QFDT, thus establishing QFDT as a proper quantum theory. In this case the translation, the "functor" in question, is "virtually direct." As I said, Coecke and other quantum category theorists establish this type of "functor" by recasting the Hilbert-space formalism of QFDT so as to connect the two categories in question—that of circuits and that of mathematical structures, enabling proper predictions, rather than deriving this functor from axioms or postulates, as Hardy or Chiribella, D'Ariano, and Perinotti aim to do.

Circuits, then, and their arrangements symbolically embody or represent the technological architecture of measuring instruments capable of detecting and measuring quantum events, and also enabling the probabilistic predictions of future events. Their arrangement and operations in the second respect are enabled by rules that should ideally be derived from certain sufficiently natural assumptions, independent, for example, of the correspondence principle, which make quantum theory depend on classical physics. On the other hand, circuits and their arrangements are still described classically, a fact possibly related to Bohr's correspondence principle, likely in Heisenberg's mathematical form, a connection that, however, requires a separate analysis.

An important and difficult question is the relationship between the architecture of the "circuits," that is, experimental arrangements used in the experiments, in standard (infinite-dimensional) QM, and the mathematical architecture of QM, or the same relationship in QFT. Consider the double-slit experiment, say, in the interference pattern setup (both slits are open and no counters installed allowing one to determine through which slit each particle passes). It is a circuit, which embodies (or could be represented by a scheme that embodies) preparations, measurements, and predictions, all manifested in the emergence of the interference pattern. However, the mathematical architecture enabling these predictions is a combination of the formal architecture of the equations of QM, equations taken from classical physics by the mathematical correspondence principle, and new types of variables, "guessed" by Heisenberg, rather than derived from the architecture of the circuits itself used in the experiments in question in QM.[11] I would not presume to rigorously describe the setup of the double-slit experiment as a circuit, but it is one nevertheless, a complex one, albeit child's play in comparison to the circuitry found in high-energy quantum physics, such as that of the LHC. Yet, the LHC is a circuit, too, with the language of which (the language of) the mathematics of QFT is fundamentally linked in terms of the statistics of experiments enabled by the LHC, such as those that confirmed the existence of the Higgs boson and thus our ideas, embodied in both QFT and the LHC, concerning the deeper constitution of nature. One can hardly doubt, however, that the Higgs boson is only a point on the arc initiated by Planck's quanta, an arc that may be far from its highest point, if we can even reach this point.

Such questions will, however, need to be addressed if one is to extend these types of derivations of QFDT to QM or to QED and QFT, or beyond as both Hardy and D'Ariano, Chiribella, and Perinotti aspire to do, along somewhat different gradients. For, after all, important as this task may be, it is hardly sufficient for these programs to merely derive certain already established theories. Their primary value ultimately lies in solving still outstanding problems, in what they can do for the future of fundamental physics. Hardy aims to rethink general relativity in operational terms, analogous to those of QFDT, and then to reach, in principle, quantum gravity by combining the operational frameworks of quantum theory (including quantum field theory) and general relativity (Hardy 2007). D'Ariano and co-workers, by contrast, appear first to move from QFDT to QFT, via the concept of quantum cellular automata, QCA (which makes their framework no longer strictly operational), again, with the ultimate goal of quantum gravity, but possibly by bypassing general relativity as an intermediate stage. Their derivation of Dirac's equation is a major step in this direction, and, unlike Dirac's initial derivations or other known derivations of it, it only uses, along with other quantum-informational principles, *the principle of locality*, rather than those of *special relativity* (D'Ariano and Perinotti 2014).

7.3 Dirac's Equation and High-Energy Quantum Physics Without Relativity

The principal way of thinking in quantum information theory considered in the preceding section is equally central to D'Ariano and Perinotti's derivation of Dirac's equation, in the framework of quantum cellular automata. As I said, I cannot address the concept of cellular quantum automaton, or the technical mathematical aspects of this derivation, which cannot be given justice here. The two main points that I want to emphasize are as follows.

[11] Cf. (Hardy 2001, p. 26)

The first point, in accordance with the program of this study, is the significance of the fundamental principles in quantum theory, now including the principles of quantum information, some of which recast and thus give a deeper meaning to previously established quantum principles. The task of understanding these principles is far from being completed, and in some respects, it has barely begun to be undertaken. The fact that D'Ariano and Perinotti are able to give their principles as principles of quantum information a proper mathematical expression is important, and doing so is a major accomplishment.

The second point is that Dirac's equation could be derived by using principles of information processing alone, without using the relativity principle, while, however, using the locality principle. Although this derivation is only a starting point of high-energy quantum theory, it has major implications for foundational thinking in fundamental physics, in particular the possibility that, while the locality principle retains its significance in large-scale physics, the principles of relativity may not. In D'Ariano and Perinotti's scheme:

> The Dirac equation in three space dimensions *emerges from the large-scale dynamics* of the minimum-dimension quantum cellular automaton [QCA], satisfying [linearity], unitarity, locality, homogeneity, and discrete isotropy of interactions [without appealing to special relativity]. The Dirac equation is recovered for small wave vector and inertial mass, whereas Lorentz covariance is generally distorted in the ultrarelativistic limit [of very large wave-vectors] (D'Ariano and Perinotti 2014, p. 1).

This scale is beyond "the usual Fermi scale of high-energy physics," because "the QCA as a microscopic mechanism for the emergent quantum field" is proposed "as a framework to unify a *hypothetical* discrete Planck scale with the usual Fermi scale of high-energy physics" (D'Ariano and Perinotti 2014, p. 1). The hypothetical nature of some of the assumptions made and the theories cited by D'Ariano and Perinotti is important and must be kept in mind. I shall return to this aspect of their argument below.

Dirac's equation might be seen as an enactment of the mathematical correspondence principle applied at a level beyond the Fermi scale.[12] This enactment is complex, which should not come as a surprise. Even establishing, via Pauli's spin theory, that Schrödinger's equation is the quantum-mechanical limit of Dirac's equation was nontrivial, especially at the time, and it was, again, crucial. In the present case, one deals with a deeper level of quantum *reality*, or possibly a reality beyond the quantum, although D'Ariano and Perinotti's framework suggests, at least to this author, that at stake is a certain general quantumness, reflected in the linearity and unitarity of their framework at the large scale. The key principles of quantum theory and their quantum-theoretical mathematical expressions and consequences are clearly at work throughout—the probabilistic or statistical nature of the theory (the QP/QS principle), locality, Hilbert spaces, operators, noncommutativity, the tensor structure, symmetry, group representation, and so forth (D'Ariano and Perinotti

[12] On the other hand, the article provides "an analytical description of the QCA for the narrow-band states of quantum field theory in terms of a dispersive Schrödinger equation holding at all scales" (D'Ariano and Perinotti 2014, pp. 1, 4).

2014, pp. 2–3). There is some important and implicative new mathematics as well, such as that of "quasi-isometric embedding," a profound recent development in geometry, primarily due to M. Gromov and his geometric-group theory (D'Ariano and Perinotti 2014, p. 3). The QCA is the *quantum* cellular automaton operative at the large scale. The derivation of Dirac's equation is achieved by finding Dirac's automata, which are "obtained by locally coupling Weyl automata" and give the Dirac equation at the relativistic limit from the informational principles proposed, *all of these principles*, rather than only linearity and unitarity, which are quantum in character (D'Ariano and Perinotti 2014, pp. 5, 6–9). This is a reflection and recasting of the fact that Dirac's spinors are composed of Weyl's spinors.

In this regard, the situation is parallel to deriving QFDT from a larger (than only quantum) set of principles. Locality, homogeneity, and isotropy are additional principles here, which allow one to dispense with the principle of relativistic (Lorenz) invariance in deriving the equation. Locality merits a special attention, both in view of its fundamental, irreducible role in this derivation and in general. The locality requirement itself and the corresponding principle are defined by the requirement that the cardinality of the set, S_g, of sites g', interacting with the site g (both from a denumerable set G of systems, involved in the cellular automaton and described by Fermionic field operators) is "uniformly bound over G, namely $\left|S_g\right| \leq k < \infty$ for every g" (D'Ariano and Perinotti 2014, p. 2). This concept or principle is more general and deeper than the principle of (special) relativity, defined the Lorentz invariance, although relativity, special or general, conforms to the principle of locality. The Lorentz invariance emerges from this locality principle at the corresponding (relativistic) limit and the standard (Fermi) scale of quantum field theory, and hence of Dirac's equation. The significance of the locality requirement is, however, more general and is critical to the informational QCA framework proposed by the authors. This requirement is consistent with the view of locality adopted from the outset of this study, in part following D'Ariano and Perinotti: all physical interactions and indeed everything that essentially defines phenomena, information, and so forth is local. The principle has important connections to local symmetries, specifically local gauge symmetries of QFT (those of the Yang-Mills theory), for example, those of the color group SO(3), which corresponds to the local symmetry whose gauging gives rise to quantum chromo dynamics (QCD). These connections, far from sufficiently explored in literature, cannot, however, be addressed here. In addition, as noted in Chap. 1, according to D'Ariano (private communication), while falsifiability is a crucial (Popperian-like) requirement to be satisfied by this framework, it is equally crucial that all falsifiability is *local*, which may be seen as a principle in its own right.

The QCA as such is, again, an enactment of large-scale quantum dynamics, the quantum character of which is manifest in and is defined by linearity and unitarity. There are significant potential implications and benefits of this broader conception beyond the fact the Dirac equation is the Fermi-scale limit of the theory:

> The additional bonus of the automaton framework is that it also represents the canonical solution to practically all issues in quantum field theory, such as all divergences and the problem of particle localizability, all due to the continuum, infinite volume, and the

> Hamiltonian description. ...Moreover the QCA is the ideal framework for a quantum theory of gravity, being quantum *ab initio* (the QCA is not derivable by quantizing a classical theory), and naturally incorporates the informational foundation for the holographic principle, a relevant feature of string theories (Zwiebach 2004; Becker et al. 2008) and the main ingredient of the microscopic theories of gravity of (Jacobson 1995) and (Verlinde 2011). Finally, a theory based on a QCA assumes no background, but only interacting quantum systems, with space-time and mechanics as emergent phenomena.
>
> The assumption of Planck-Scale discreteness has the consequence of breaking Lorentz covariance along with all continuous symmetries: These are recovered at the Fermi scale in the same way as in the doubly-special relativity of Amelino-Camelia (Amelino-Camelia 2002; Amelino-Camelia and Piran 2001), and in the deformed Lorentz symmetry of Magueijo and Smolin (2002, 2003). Such Lorentz deformations have phenomenological consequences, and possible experimental tests have been recently proposed by several authors (Moyer 2012; Hogan 2012; Pikovski et al. 2012; Amelino-Camelia et al. 2009). The deformed Lorentz group of the automaton has been preliminarily analyzed in (Bibeau-Delisle et al. 2015). (D'Ariano and Perinotti 2014, p. 1)

It is beyond my scope to address these aspects of the program and their implications. They are exciting and promising but would also require an extended careful analysis, including considering the works cited here. However, while the potential significance of the proposed program for large-scale physics is thus made apparent, the hypothetical nature of the theories just mentioned should be kept in mind as well. To cite D'Ariano and Perinotti's conclusion:

> In conclusion, we remark that [the] Lorentz covariance is obeyed only in the relativistic limit $|\mathbf{k}| \ll 1$, whereas the general covariance (corresponding to invariance of $\omega_k^{E^\pm}$) is a nonlinear deformation of the Lorentz group, with additional invariants in the form of energy and distance scales (Bibeau-Delisle et al. 2015), as in the doubly-special relativity (Amelino-Camelia 2002; Amelino-Camelia and Piran 2001) and in the deformed Lorentz symmetry (Magueijo and Smolin 2002; Magueijo and Smolin 2003), for which the automaton then represents a concrete microscopic theory. Correspondingly, also [the] CPT symmetry of Dirac's QCA is broken in the ultrarelativistic scale. (D'Ariano and Perinotti 2014, p. 9)

The theories invoked here are highly hypothetical. Amelino-Camelia and Piran, cited here, call a hypothetical suspension of the (Lorentz) relativistic invariance a "drastic assumption," albeit made to address certain puzzling data (Amelino-Camelia and Piran 2001, p. 1). It may be somewhat less drastic insofar as the locality principle is preserved. In any event, it may pay off to be cautious, even though and indeed because alternative proposals, some of which are based on alternative proposal for using cellular automata in quantum theory ('t Hooft 2014), are equally hypothetical. However, that Dirac's equation could be derived without using the relativity (Lorentz-invariance) principle, in addition to being a major achievement in its own right, again, manifests a promise and potential of the authors' informational program for exploring fundamental physics on scales beyond those of quantum field theory.

Profound foundational questions are at stake. How these questions will be pursued and what role the principles of quantum informational theory will play in it are difficult to predict. The present discussion does not aim to do so. It does suggest, however, that fundamental physical principles may play a more significant role than they have more recently in approaching these questions. Equally crucially and more radically, it also suggests, with the help of both Dirac's own and D'Ariano and

Perinotti's derivations of Dirac's equation, that the fundamental principles of *quantum physics* might prove to be more crucial in this pursuit than those of relativity, not inconceivably, general relativity included.[13] *Quantum principles* may also prove to be more crucial rather than those of general relativity in understanding gravity, although, as explained above, some form of locality principle may be equally crucial, and homogeneity and isotropy are likely to be required as well, as very general principles. In other words, while relativity may be abandoned on the large scale (extending to the Planck scale), quantum theory is likely to survive, reflecting the ultimately quantum character of nature. If so, and one cannot, once again, be certain, we may need yet new quantum principles, possibly conceived on lines of principles of information processing and QCA, even if developed, as these informational principles are, by building on some among the older principles.

But then, this may be our best way to arrive at new principles in, to cite Heisenberg's title, "physics and beyond" (Heisenberg 1962). Fundamental physics as understood here is always physics and beyond, not the least because it is also a physics of the beyond. It is the physics of that which is beyond the reach of the physics available at a given moment of time, and beyond physics itself, for example, if one adopts the RWR principle, which, in its strongest version, places the ultimate constitution of *nature* beyond thought itself, but not beyond existence or reality. This does not stop physics, quite the contrary, because, as I argued, in the *spirit* of Copenhagen, throughout this study, this "beyond the reach of physics," also makes possible new physics, physics that would not be possible otherwise. Nature and spirit, or rather nature, spirit, and technology, continue to work together.

[13] Cf. also (D'Ariano 2010, 2012).

Chapter 8
Conclusion: The Question Concerning Technology in Quantum Physics and Beyond

Abstract Chapter 7 suggests that the principles of quantum information theory, implicitly at work, as I have argued, already in Heisenberg's discovery of quantum mechanics, may play a major role in the future of fundamental physics and principle thinking there. Heisenberg's work and quantum information theory also share their "technological" nature in the broad sense of technology, as considered in Chap. 2 (Sect. 2.5). Indeed, which is the main theme of this conclusion, the question of quantum information and "the question concerning technology," in Heidegger's famous phrase, are deeply interconnected and may even be seen as one and the same question. For, the physics of quantum information is also the physics of the relationships between experimental and mathematical technology. All technology is essentially linked to information. Indeed, technology may be best defined as the means of obtaining information about systems, in the case of quantum systems, in the RWR-principle-based interpretations, as the means of generating information (classical in nature) through the interaction between quantum systems and the technology of measuring instruments. In this way, this conclusion brings together all of the main themes and fundamental principles discussed in this book.

The discussion undertaken in Chap. 7 suggests that the principles of quantum information theory are likely to play a major role in the future of fundamental physics. As I have argued in this study, they have implicitly done so beginning with Heisenberg's work, with which quantum information theory also shares its technological dimension in the broad sense of technology discussed in the end of Chap. 2 (Sect. 2.5) in conjunction with Heisenberg's work. For, if "on the one hand experiments are described by circuits resulting from the connection of physical devices, [and] on the other hand each device in the circuit can have classical outcomes and the theory provides the probability distribution of outcomes when the devices are connected to form closed circuits (that is, circuits that start with a preparation and end with a measurement)" (Chiribella et al. 2011, p. 3), then the *physics* of quantum information is also the physics of the relationships between experimental and mathematical technologies. Technology, experimental, mathematical, or other, is essentially linked to information. Indeed, it is arguably best defined as the means of obtaining information about systems. In the case of quantum systems, in nonrealist, RWR-principle-based, interpretations, technology (jointly experimental and

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mathematical) becomes the means of generating, *creating*, information (classical in nature) through the interaction between quantum systems and the technology of measuring instruments, thus bringing together the main principles of quantum theory, as considered in this book.

It is important to keep in mind that experimental technology is a broader concept than that of measuring instruments. It would, for example, involve devices that make it possible to use the measuring instruments, or still other devices. More generally, technology is a means of doing something and doing it more successfully than previously, to get "from here to there," as it were. In this sense, any technology is the invention of new wheels, which is, however, not the same as reinventing the wheel. Thus, the experimental technology of quantum physics enables us to understand how nature works at the ultimate level of its constitution, in this case, strictly in the sense of what kind of effects this constitution produced upon measuring devices, without being able to represent or even conceive of the character, architecture of this constitution. This, however, is sufficient to have QM, QFT, or QFDT, and to use their technologies for obtaining information through quantum phenomena, which quantum objects enable us to have, and to work with this information, as in quantum cryptography and computing. Quantum objects themselves are not technology; it is something technology helps us to discover, understand, work with, and so forth. However, they can become part of technology, beginning with the quantum parts of measuring instruments through which parts these instruments interact with quantum objects, concerning which we obtain information. Quantum objects can also become part of devices we use elsewhere, such as lasers, electronic equipment, and MRI machines (this list is very long by now). Thus, the discovery of the Higgs boson is the result of the joint workings of three technologies:

1. The experimental technology of the Large Hadron Collider (LHC);
2. The mathematical technology of QFT (sometimes coupled to the technology of philosophical thought);
3. Digital computer technology.

Any quantum event, I argue, is made possible through the joint workings of the first two technologies, with the third becoming increasingly more prominent. Although the role of digital technology is one of the defining aspects of contemporary physics, I can only mention it in passing. There is yet another technology (in the present, extended sense) involved: that of science as a cultural project, which is, however, a separate subject, which, too, can only be mentioned in passing here.[1]

As discussed at the end of Chap. 2, via Bohr's argument concerning the role of "mathematical instruments" in Heisenberg's discovery of quantum mechanics, in its use in physics, all, or in any event most, mathematics is technology in this broader sense. But one could think of the technological functioning of mathematics even in mathematics itself, insofar as certain mathematical tools, such as, say, homotopy or

[1] See Chap. 1, note 5, especially Galison (1997) and Latour (1999), mentioned there, and for a discussion of this subject by the present author (Plotnitsky 2002).

cohomology groups, are technologies akin to measuring instruments in physics. According to J-P. Marquis, who borrows the concept from physics and even specifically quantum physics "homotopy groups should be thought of as *measuring instruments* since they provide information about certain crucial aspects of spaces. … [T]hey are epistemologically radically different from … *transformation* [symmetry] groups of a space. ...Homotopy groups (and here we might as well mention homology and cohomology groups) do not act on anything. The purpose of these *geometric* devices is to classify spaces by their different *homotopy* [or cohomology] *types*." By contrast, fibrations (through which homotopy and cohomology groups are defined) are not "measuring instruments," but rather "devices that make it possible to apply measuring instruments [such as cohomology and homotopy groups] and other devices" (Marquis 2006, p. 259). In theoretical physics, specifically elementary particle physics, symmetry groups are a mathematical technology. It follows, however, that in parallel with experimental technology, the mathematical technology of physics or even of mathematics itself is not only or even primarily a way or representing reality (although it may be this, too) but is rather a way of experimenting with reality, and thus also creating new realities, physical and mathematical.

Taking advantage of and bringing together both main meanings of the word "experiment" (as a test and as an attempt at an innovative creation), one might argue that, while not without help from nature, the practice of quantum physics is the first practice of physics or science that is both, jointly, *fundamentally* experimental and *fundamentally* mathematical. That need not mean that this practice has no history; quite the contrary, beginning, again, with Galileo's and Newton's work. Both were also experimentalists, both in the conventional sense and in this sense under discussion at the moment, as were most other major figures of experimental physics or relativity, who advanced and made possible both theories—Boyle, Huygens, Young, Faraday, and Michelson are just a few major names. (The list of even major figures becomes long from the late nineteenth century on.) Lagrange's and Hamilton's analytical mechanics, Maxwell's electrodynamics, the thermodynamics of Maxwell, Boltzmann, Gibbs, and Planck, and Einstein's relativity are all major events of this history of experimenting with mathematical models in physics. As I said at the outset of this study, the commitment itself to creative experimentation may well be, in the language of (Kant's) moral philosophy, the categorical imperative of all good science. This is certainly a point on which classical and quantum physics converge: creative experimentation, physical, mathematical, or philosophical is the categorical imperative and the primary force of *causality* of both, whatever the nature of this causality (a difficult problem in its own right) may be. Nevertheless, this experimentation acquires a new form with quantum mechanics and then extends to higher level quantum theories, and a new understanding of the nature of quantum phenomena and thus experimental quantum physics, as discussed in Sect. 2.5 of Chap. 2.

The practice of quantum physics is fundamentally experimental because we no longer track, as we do in classical physics or relativity, the independent behavior of the systems considered, and thus track what happens in any event, by however ingenious experiments. Instead we *define* what will happen in the experiments we perform, by how we *experiment* with nature by means of our experimental technology, even though and because we can only predict what will happen probabilistically or statistically.[2] Thus, in the double-slit experiment, the two alternative setups of the experiment, whether we, respectively, can (by way of using one experimental device or another) or cannot know, even in principle, through which slit each particle, say, an electron, passes, we obtain two different outcomes of the statistical distributions of the traces on the screen, with which each particle collides. Or, thus also giving a rigorous physical and philosophical meaning to the uncertainty relations, we can set up our apparatus so as to measure and correspondingly predict, again, probabilistically or statistically, either the position or the momentum of a given quantum object, but never both together. Either case requires a separate experiment, incompatible with the other, rather than merely representing an arbitrary selection of either type of measurement within the same physical situation, by tracking either one of its aspects or the other in the way we do in classical mechanics. There, this is possible because we can, at least in principle, measure and assign simultaneously both quantities within the same experimental arrangement. In quantum physics we cannot. This difference, as we have seen, was crucial to Bohr's understanding of the situation, including in the EPR experiment:

> 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 1935, p. 699; emphasis added)

Quantum physics, however, changes what experiments do: they *define* what will or will not happen, in terms of probabilistic or statistical *quantum causality*, as defined in Chap. 5, rather than allowing it to follow what is bound to happen in any event in accordance with *classical causality*.

By the same token, quantum physics is also fundamentally mathematical, because the mathematical formalism of the theory is not in the service of tracking, by way of a mathematical representation, what would have happened anyhow, which would shape the formalism accordingly, but is in the service of predictions required by our experiments. The mathematical formalism of quantum theory is able to predict correctly the experimental data in question without offering a representation at all of the physical processes responsible for these data. Quantum mechanics, at least in nonrealist, RWR-principle-based, interpretations, is strictly a theory, a mathematical technology, of predictions concerning the outcomes of possible future experiments on the basis of previously performed experiments, both defined by our experimental technology. This is why quantum mechanics established radically new relationships between mathematics and physics, vis-à-vis those, essentially realist in character, found in classical physics and relativity. Indeed, it also follows that, as quantum physics experiments with nature by using

[2] I am indebted to G. M. D'Ariano on this point.

mathematical and experimental technology, quantum theory experiments with mathematics itself, more so and more fundamentally than in classical physics or relativity. This is because we invent, in the way Heisenberg or Dirac did, effective mathematical schemes of whatever kind and however far they may be from our general phenomenal intuition, rather than proceed by refining mathematically our phenomenal representations of nature, which process limits us in classical physics or even (to some degree) in relativity.

As discussed in Chap. 2, Heisenberg's thinking, thus, revolutionized the very practice of theoretical physics, and it redefined experimental physics as well, or reflected what the practice of experimental physics had in fact already become by that point. I briefly recapitulate the nature of this transformation here. The practice of experimental physics does not consist of tracking what happens or what would have happened independently of our experimental technology, but in creating, again, *unavoidably* creating, configurations of this technology. This situation reflects the fact that what happens is *unavoidably* defined by what kinds of experiments we perform, and how we affect quantum objects, rather than only by their independent behavior, although their independent behavior does of course contribute to what happens. The practice of theoretical physics no longer consists in offering an idealized mathematical representation of quantum objects and their behavior, which is impossible by the RWR principle. It consists in developing mathematical machinery that is able to predict, in general (in accordance with what obtains in experiments) probabilistically or statistically, the outcomes of (always discrete) quantum events and of correlations between such events, observed in the configurations of experimental technology, which brings together the QD and QP/QS principles. The situation, as the analysis of QFT offered in Chap. 6 shows, acquires a more complex form in quantum electrodynamics and quantum field theory, and experimental physics in the corresponding (high) energy regimes, in view of the PT principle, which is added to the RWR, QD, and QP/QS principles of quantum mechanics, or QFDT. This addition reflects the greater complexity of high-energy physics as concerns the configurations of experimental technology defining it; a more complex nature of the mathematical formalism as the mathematical technology of the theory; and a more complex character of the quantum-field-theoretical predictions; and, thus, of the relationships between the mathematical and the experimental technologies in high-energy quantum physics.

Indeed, as suggested above, we can adopt this experimental-technological viewpoint and this quantum-theoretical model, thus also the RWR principle, in mathematics itself. Consider R. Langlands's argument concerning the so-called Langlands program, one of the most extraordinary endeavors of twentieth-century mathematics. A. Wyles's proof of Fermat's theorem was, for example, connected to the program. Langlands's argument was influenced and even inspired by quantum mechanics and quantum field theory, and the role of infinite-dimensional group representations there. According to Langlands:

> The introduction of infinite-dimensional representations entailed an abrupt transition in the level of the discourse, from explicit examples to notions that were only described metaphorically, but at both levels we are dealing with a tissue of conjectures that cannot be attacked frontally.

> The aesthetic tension between the immediate appeal of concrete facts and problems on the one hand, and, on the other, their function as the vehicle to express and reveal not so much universal laws as an entity of a different kind, of which these laws are the very mode of being, is perhaps more widely acknowledged in physics, where it has long been accepted that the notions needed to understand perceived reality may bear little resemblance to it, than in mathematics, where oddly enough, especially among number theorists, conceptual novelty has frequently been deprecated as a reluctance to face the concrete and a flight from it. Developments of the last half-century have matured us, as an examination of Gerd Falting's proof of the Mordell conjecture makes clear, … but there is a further stage to reach.
>
> It may be that we are hampered by the absence of a central unresolved difficulty and by the extremely large number of currently inaccessible conjectures, at whose extent we have hardly hinted. Some are thoroughly tested; others are in doubt, but they form a coherent whole. What we do in the face of them, whether we search for specific or general theorems, will be determined by our temperament or mood. For those who thrive on the interplay of the abstract and the concrete, the principle of functoriality for the field of rational numbers and other fields of numbers has been particularly successful in suggesting problems that are difficult, that deepen our understanding, and yet before which we are not completely helpless. (Langlands 1990, p. 209)

One would need a proper discussion of the mathematical theories involved to make apparent the mathematical and philosophical profundity of what is at stake in this passage and in Langlands's program itself, and such a discussion is beyond my scope.[3] A more general philosophical argument is sufficient to make my main point, which is as follows. The program and the way of pursuing it harmonize with my argument in this book, applied to quantum theory, especially the point "that the notions needed to understand perceived reality may bear little resemblance to it." According to the nonrealist argumentation offered by this study, such notions, more radically, bear no resemblance to this reality at all, a reality that in the first place cannot be perceived, but is only assumed, inferred from realities that we can perceive. (Langlands would likely have agreed with this last qualification.) Besides, in accordance with the spirit of "fragmentation without wholeness" advocated in this study, I am not certain about the existence of "the whole" in this case, even though Langlands's program has reached a firmer level of definition and cohesion since this statement was made (in 1990). Rather we deal with a heterogeneous and yet interactive engagement of theories and concepts, including differently if interactively defined concepts, sometimes different while having the same name, such as space or number. It is an intriguing question, again, beyond my scope here, whether one can speak of complementarity in Bohr's sense in mathematics. However, one can speak of a fragmentation without a whole that this fragmentation "fragments."

The differences between the two types of "objects"—material in physics and mental in mathematics—remain important, of course. In particular, even when one assumes the RWR principle, there still exists something in *nature*, idealized as quantum objects, that is responsible for the observed quantum phenomena. The meaning of the existence of mathematical objects as such is an entirely different

[3] See (Bernstein and Gerbard 2003) and (Gelbart 1984) for a more accessible account.

question, which has been debated since Plato, whose ghost still overshadows this debate, but especially in modern mathematics, following Cantor's set theory (Gray 2008). This question cannot be addressed here, and it would be presumptuous to offer any general thoughts concerning this subject apart from engaging a great deal of philosophical and technical literature concerning it. One could nevertheless argue for analogues, such as those suggested by Langlands, of the quantum-mechanical situation in mathematics. Such analogues arise when one needs to rely on the properties of a more accessible mathematical object, or one type of such objects, in order to understand another mathematical object, or another type of object which is ultimately inaccessible or even ultimately an inconceivable mathematical object, which can nevertheless be indirectly consistently defined (again, leaving aside the question of its existence). I now use "understanding" in the sense of establishing rigorous connections (similar to those between mathematical formalism and quantum objects in quantum mechanics or quantum field theory), rather than merely in terms of more or less loose analogies or metaphors. These connections become possible if both types of objects can be linked in a particular way, as happens, for example, in the case of Langlands's program. Riemann's work with the functions of complex variables by means of certain properties of these functions (such as their singularities), rather than by means of defining them by explicit formulas, is a remarkable early example of this kind of situation. In fact, however, much modern mathematics operates under these conditions (whether the practitioners subscribe to this view or not), or at least, as in quantum mechanics or quantum field theory, this type of interpretation of modern mathematics or all mathematics as ultimately technological is possible. In any event, luckily for us, this technological approach works in mathematics, just as it works in quantum physics, where, again, both nature and experimental technology help us.

In this respect, while *modernist* mathematics may by defined, following Gray in his *Plato's Ghost* (Gray 2008), by its divorce from physics and the (nonmathematical) world, this technological mathematics, which is often the same *mathematics* technically, may be defined by its divorce from mathematical representation of mathematical reality, in this case, inaccessible by definition. This may be seen as the final break with Platonism. As such, it may in fact reconnect itself with the world, as it does in relativity and, more radically, in quantum theory, as Gray acknowledges, via Einstein in the case of relativity (Gray 2008, p. 324, n. 28). But Gray misses the RWR-principle-based thinking found in quantum theory, which connects physical reality without realism with mathematics without realism. He misses Heisenberg's and Bohr's *modernist* revolution in physics and its radical nature, a revolution that also took place in modernist mathematics.

This thinking in mathematics and physics does, however, retain something, perhaps the most important thing, from Plato—from the *spirit* of Plato—rather than the *ghost* of Plato (the difference between the two words is both infinitesimal and infinite). Plato's thought, too, may be less Platonist than it might appear and than it might have appeared even to Plato himself. This thinking retains the essential, shaping role of the movement of thought, a movement or a flight that drives this technology of mathematics and physics. Even though, unlike in Plato (perhaps!), such mathematical objects are beyond representation and even conception, we still work with them and give them the power of mathematical technology in both mathematics and, connecting the physical and the mathematical unthinkable, physics, and thus a dimension of truth, not absolute, to be sure, but perhaps all the more powerful and important for that. Heisenberg's thinking, closer to that of Plato, of fundamental symmetries in his later works is defined by the movement of thought just as was his thinking, close to that of Bohr, leading him to the discovery of quantum mechanics, thus in both instances more in the spirit of Plato than following the ideology of Platonism as an "ism."

One might also see philosophy or even thinking itself, or at the other end of this axis science as a cultural project, as technology, although not only as technology.[4] This subject, however, may lead one into "metaphysical depths," which would require an engagement with philosophical works that would be difficult to undertake here. In commenting on some of the most basic concepts we use, H. Weyl once said: "We cannot set out here in search of a definitive elucidation of what is to be a state of affairs, a judgment, an object, or a property. This task leads into metaphysical depths. And concerning it one must consult men, such as Fichte, whose names may not be mentioned among mathematicians without eliciting an indulgent smile" (Weyl 1928, p. 7). This is a cutoff that I must adopt here as well. Nevertheless, I would like to finish by citing M. Heidegger's conclusion (skipping a few sentences

[4] Cf. an intriguing recent approach to representing sensation-perception dynamics in terms of quantum-like mental instruments or, one might say with the discussion given in Chap. 7 in mind, "circuits" in Khrennikov (2015). A qualification might be in order. There have been recent arguments concerning the technological nature of thought itself in the sense of the *material* embodiment ("embodied cognition") or material extension ("extended cognition") of thought, for example, those following Clark and Chalmers (1998). Arguments in support of such claims do not appear to me to have been rigorously developed thus far. I am not denying that there are relationships between thought and technologies conventionally assumed as exterior to thought, beginning with the technology of writing, without which it would be difficult to do mathematics, for example. On the other hand, it is difficult to argue rigorously that one thinks *with* the technology of the LHC, although we might think with mental images of photographs enabled by this technology or possibly of other material components of the LHC. These remarks are, admittedly, hardly sufficient to make the case they suggest, but doing so will require an argument that cannot and need not be pursued here. In any event, my primary concern is the technology, specifically mathematical technology, of thought itself, which may, I admit, never be entirely separable or independent from one or another exterior technology, but it is, I would also argue, not reducible to such exterior technologies either. Nor of course is thought reducible to its technological aspects, insofar as thought may create objects, such as phenomenal or mathematical spaces that are not in themselves technological (fibrations or cohomology and homotopy groups that one uses to study spaces are), even though they can be made part of technologies of thought. To assume this reduction would merely (or naively) reverse the classical dualist and hierarchical view of thought and technology, which assumes technology to be merely auxiliary to thought. This type of reversal, which unconditionally subordinates thought to technology, as against the previous phase of the unconditional independence of thought from technology, is not sufficient to critically reexamine and ultimately change the fundamentals of our thinking concerning the relationships between thought and technology. The same claim, it follows, would apply to the opposition between mental and materially embodied technologies of thought. Neither is unconditionally separable from or unconditionally subordinate to the other.

less germane in my context) of his essay "The Question Concerning Technology" (which cites Heisenberg's along the way [Heidegger 2004, p. 23]):

> There was a time when it was not technology alone that bore the name *technē̃*. Once that revealing that brings forth truth into the splendor of radiant appearing also was called *technē̃*.
>
> Once there was a time when the bringing forth of the true into the beautiful was called *technē̃*. And the *poiē̃sis* of the fine arts also was called *technē̃*.
>
> In Greece, at the outset of the destining of the West, the arts soared to the supreme height of the revealing granted them. … And art was simply called *technē̃*. It was a single, manifold revealing. It was …, *promos*, i.e., yielding to the holding-sway and the safekeeping of truth.
>
> The arts were not derived from the artistic. Art works were not enjoyed aesthetically. Art was not a sector of cultural activity.
>
> What, then, was art—perhaps only for that brief but magnificent time? Why did art bear the modest name *technē̃*? Because it was a revealing that brought forth and hither, and therefore belonged within *poiē̃sis*. It was finally that revealing which holds complete sway in all the fine arts, in poetry, and in everything poetical that obtained *poiē̃sis* as its proper name. …
>
> Whether art may be granted this highest possibility of its essence in the midst of the extreme danger [of modern technology], no one can tell. Yet we can be astounded. Before what? Before this other possibility: that the frenziedness of technology may entrench itself everywhere to such an extent that someday, throughout everything technological, the essence of technology may come to presence in the coming-to-pass of truth.
>
> Because the essence of technology is nothing technological, essential reflection upon technology and decisive confrontation with it must happen in a realm that is, on the one hand, akin to the essence of technology and, on the other, fundamentally different from it.
>
> Such a realm is art. But certainly only if reflection on art, for its part, does not shut its eyes to the constellation of truth after which we are *questioning*.
>
> Thus questioning, we bear witness to the crisis that in our sheer preoccupation with technology we do not yet experience the coming to presence of technology, that in our sheer aesthetic-mindedness we no longer guard and preserve the coming to presence of art. Yet the more questioningly we ponder the essence of technology, the more mysterious the essence of art becomes. (Heidegger 2004, pp. 34–35)

I would argue that experimental and mathematical technologies of quantum physics, or the mathematical technologies of mathematics itself, are *technē̃* in the sense that Heidegger wants to give this term here. They certainly were in the hands of Heisenberg, Schrödinger, and Dirac, and many of their followers, or their predecessors, such as Einstein and Bohr. This is equally true about many experimenters involved in the discoveries of quantum physics, that of the Higgs boson, among them. By the same token, however, contrary to Heidegger's view, which appears to be implied here (although it is difficult to be certain), fundamental physics, experimental and theoretical, and hence, mathematics, is "an experience of the coming to presence of technology" that "guards and preserves the coming to presence of art," the art of physics. This returns us from the spirit of Heidegger's philosophy to the spirit of Copenhagen, because the spirit of Copenhagen is the spirit of guarding and preserving the art of physics, which, however, can only be preserved by transforming physics itself, experimental and mathematical.

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Author Index

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Subject Index[1]

[1] Due to the frequency of their occurrence in the book, certain terms, such as mathematics, nature, principle, physics, quantum mechanics, quantum physics, quantum theory, and quantum objects and quantum phenomena, are only indexed when referring to basic definitions or contrasts.

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