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

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[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 (*hν*-relations), hence I do not see any geometrically-controllable sense in the statement "falling into the nucleus." It is really my conviction that an interpretation of the Rydberg formula in terms of circular and elliptical orbits (according to classical geometry) does not have the slightest physical significance. And all my wretched efforts are devoted to killing totally the concept of an orbit—which one cannot observe anyway—and replace it by a more suitable one. (Heisenberg to Pauli, 9 July 1925; cited in 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:

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

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[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:

[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 $H(X) = EX$, 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

[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.

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