Chapter 2
1913. “On the Constitution of Atoms and Molecules”: Quantum Jumps and Epistemological Leaps

In the spring of 1913, Niels Bohr completed his, now famous, paper, “On the Constitution of Atoms and Molecules,” presenting a new model of the hydrogen atom. The paper built on previous discoveries of Planck and Einstein, mentioned in Chap. 1, concerning the quantum behavior of light. Along with these discoveries, Bohr’s paper revolutionized our understanding of the ultimate nature of both radiation, such as light, and matter, such as electrons. Bohr, back in Denmark after a few years in England, sent the manuscript to his former mentor, a great physicist in his own right (a Nobel Prize laureate by then) Ernest Rutherford, a New-Zealander by birth, but long a member of the British scientific establishment. Rutherford was also 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. I shall return to the irony of the journal’s title, which was unlikely to escape Bohr, below.

Upon reading the paper, Rutherford, first of all, made an important substantive comment reaching to the core of Bohr’s argument, a comment that I shall discuss below. He also added, however, 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. (Letter to Bohr, March 20, 1913, reproduced in “The Rutherford Memorial Lecture,” PWNB 3, p. 41)

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In commenting on this criticism 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. …" (*PWNB* 3, p. 42). Rutherford tried to reason with Bohr again in responding to an expanded version of the paper: "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" (Letter to Bohr, March 25, 1913, cited in Rosenfeld 1963, p. xiv). This is not the end of the story. I shall, however, pause my account of it here and return to it in closing this chapter.

While Rutherford was primarily an experimental physicist, who also made important theoretical contributions, Bohr was 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 such paper, but a significant experimental contribution, 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 started to develop his ideas concerning the atomic constitution of matter, eventually presented in his 1913 atomic theory. Bohr valued experimental physics greatly and championed its significance throughout his life. 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. In my view, he was both, and I would argue that he was at his best as a philosopher when thinking philosophically about physics, rather than in extending, admittedly in a preliminary and tentative way, his ideas, such as complementarity, beyond physics (the subject that I shall address in Chap. 9). Most crucial at the moment is that Bohr understood that not only theoretical concepts and arguments, but also experimental evidence and arguments have philosophical dimensions to them, dimensions that, moreover, acquire a special significance in quantum theory.

While the full measure of this significance became apparent only after the discovery of quantum mechanics, Bohr's 1913 paper and the earlier work of Planck and Einstein, just mentioned, were harbingers of these complexities, new to physics, although relativity already posed some philosophical questions similar to those posed by quantum theory. I would argue that some of the "German" long-windedness of Bohr's paper, resisted by Rutherford, reflected Bohr's struggle with these complexities and his emerging sense that they might ultimately prove to be unavoidable in quantum theory, and in particular that they could not be handled by means of the kind of thinking that defined classical physics and even relativity.

By contrast, although in turn aware of 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 already in conflict, albeit not quite to the degree his later thinking was. Rutherford saw these complexities in classical terms, just as did Einstein, even 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 can be and often are bypassed in the disciplinary practice of physics, essentially because of its mathematical character, which has defined modern physics since Galileo, who established physics as, in his words, a mathematical science of nature. Quantum theory is no exception, radically different as it may be from classical physics epistemologically; and theoretical physicists can, and most do, productively work on the mathematics of quantum theory and relate this mathematics to experimental data, without engaging with epistemological aspects of the theory. However, these complexities do come into play in deeper foundational questions, such as those that were at stake in Bohr's paper, especially, again, when such questions are precipitated by a crisis. Understanding the quantum aspects of matter could not, in Bohr's view, bypass philosophical issues, in particular those at stake in the long standing and still continuing, and, as I said, perhaps interminable, debate concerning quantum theory. This debate was new then, indeed was it not quite a debate, but rather a problem that was expected by most to be solved on classical-like lines. It took its modern shape with the creation of quantum mechanics in 1925, and Bohr's confrontation with Einstein, which ensued in its wake.

It may be argued, however, that this debate, especially Bohr's confrontation with Einstein, and even Bohr's exchange with Rutherford concerning his article, is a continuation of a much older 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, for example and in particular, as concerns the empiricist and positivist view of physics (especially along Machian lines) versus, first, the realist view, such as that of Einstein, and second, the non realist view, such as that of Bohr, in some respects, already apparent in 1913. Importantly, however, a non realist view need not be positivist or empiricist, and as I explained in the Introduction, that of Bohr was not. The Bohr–Einstein debate is a confrontation between two fundamentally different philosophies of physics and nature, and of our interaction with nature by means of experimental and theoretical physics—a realist and an anti-realist philosophy (keeping the complexities of each, such as those signaled by a possible departure from positivism or empiricism in either case). The still ongoing debate concerning quantum theory continuously replays this confrontation, and brings various versions of these two philosophies and other philosophical positions, such as empiricism and positivism, to the stage of this debate. 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 method in physics. If so (it is difficult to be 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 the pertinence of philosophy in physics in general.[1] This is why I said that Bohr might have taken the title "philosophical magazine" seriously, or at least might have been aware of the ironies involved. 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.

It is not that Rutherford did not understand or was hostile to Bohr's argument, at least Bohr's physical argument; quite the contrary, he clearly saw its significance and radical nature, although Bohr's most radical ideas troubled him. Indeed, he remained cautious as to how definite Bohr's argument was for quite some time, expecting a more classical solution of the problem of the atomic constitution, a hope that became even more frustrated with the subsequent developments of quantum theory and made even less likely to be realized by quantum mechanics. It is not clear whether Rutherford ever really reconciled himself to the kind of thinking in physics that Bohr and then Heisenberg adopted. As Rutherford's letter makes clear, he also realized Bohr's desire for and even obsession with clarity and precision, a hallmark of all of Bohr's writings. Rutherford's position appears to have been essentially empiricist or positivist philosophically, and from this position, he may not have perceived the relevance of Bohr's philosophical thinking creeping into his paper (at this stage it was no more than that) to his physical argument. For Rutherford, philosophical thinking was not sufficiently significant or even relevant for physics—it was only part of the long-windedness of the German method. For Bohr philosophical thinking was essential to physics, especially for quantum physics, and his 1913 papers already began to reflect his more philosophical style of thinking in physics. Bohr's most radical, revolutionary epistemological move—the impossibility of offering an ultimate explanation of some physical processes in the atoms—was, I would contend, a product of this fusion of physics and philosophy in his thinking. While, as I said, this move gave Rutherford a pause, later on it inspired Heisenberg and helped his discovery of quantum mechanics, which placed all physical processes inside atoms beyond the reach of explanation.

Bohr's theory of atomic constitution ambitiously aimed to remedy the difficulties of Rutherford's own earlier "planetary model" of the atom, with electrons orbiting atomic nuclei. Although a remarkable conception in turn, this model was inconsistent with classical electrodynamics, which would dictate that the electrons would nearly instantly spiral down into the nucleus, and hence that atoms could not be stable, while they are in fact manifestly stable. Bohr's theory was based on Planck's and Einstein's theories, which, as I explained in Chap. 1, postulated the possibility, in certain circumstances, of the discontinuous emission of light in the form of light quanta (or energy), $h\nu$, eventually understood as photons, particles of light. Making a 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,

[1] On Bohr's philosophical background in general, see (Favrhold 1992).

resulting in the emission of Planck's quanta of radiation, without electrons radiating continuously while remaining in orbit, thus, conflicting 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. Bohr also abandoned as hopeless an attempt to offer a mechanical explanation for such transitions, as opposed to the stationary states themselves. The latter, he said, "can be discussed by help of the ordinary mechanics, while the passing of the system between different stationary states cannot be treated on that basis" (Bohr 1913, p. 7). 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, in Bohr's theory, electrons would or, in the case of the lowest energy states, would not radiate energy). Bohr's postulates, however, proved to be correct and have remained part of quantum theory ever since, thus proving that classical theory and laws do not apply to the ultimate quantum constitution of matter. The postulates were given a proper mathematical theory with quantum mechanics.

This summary offers a more or less standard distillation of the essence of Bohr's 1913 atomic theory, which brought him a Nobel Prize in 1922. Thirty years later, Einstein commented on Bohr's "miraculous" achievement as follows: "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 principle 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, I would argue, still reflects Einstein's unease with the "foundations" upon which Bohr and then Heisenberg built their theories. These foundations never became secure or even acceptable as foundations for Einstein (he may no longer have seen them as contradictory), as his overall reflections on the same occasion make clear.

His earlier view of the situation in 1917 in a related context and Rutherford's immediate response are worth briefly commenting upon here. In the letter to Bohr, cited above, Rutherford said (before proceeding to his remark on Bohr's "arrangement of the paper"): "There appears to me one grave difficulty in your hypothesis, which I have no doubt you fully realize, namely, how does 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" (Letter to Bohr, March 20, 1913, reproduced in "The Rutherford Memorial Lecture," *PWNB* 3, p. 41). Pais, who cites this passage as part of his account of Bohr's work on his paper and reactions to it in his biography of Bohr, comments on this question and on Einstein's related question a bit later. Pais says: "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. In 1917 Einstein would add a related question: How does an individual light-quantum, emitted in an atomic transition, know in which direction to move?" (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 later in this study), both Rutherford's and Einstein's statements clearly represent the classical (causal and, in the first place, realist) way of thinking, which neither ever gave up and with which Bohr was willing to part. 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, which answered these questions more fully, albeit not to Rutherford's or, especially, Einstein's satisfaction. Pais concludes: "These questions [of Rutherford and Einstein] were to remain unresolved until … quantum mechanics gave the surprising answer: they are meaningless" (Pais 1991, p. 153). Not to Rutherford and Einstein, or many others following them! Accepting this answer requires a very different epistemological attitude, which is still uncommon and, if accepted as unavoidable, is often seen as an unfortunate and, hopefully, temporary imperative.

Bohr's paper and his "1913 trilogy" of papers devoted to his theory contain further inklings of this epistemological attitude. The limits in this study do not permit me to offer a proper discussion of Bohr's paper. However, Bohr's elaboration reflecting the situation just discussed merits further attention in the context of Heisenberg's discovery of quantum mechanics, to be discussed in the next chapter. 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," which occasioned Rutherford's comment cited above, since it poses, quite dramatically, the question of causality. It is this radical approach that inspired Heisenberg, who echoes this statement when he says: "a geometrical interpretation of such quantum-theoretical phase relations by analogy with those of classical theory seems at present scarcely possible" (Heisenberg 1925, p. 265). Heisenberg's remark is, however, also a reflection on Bohr's sentence as whole, and his 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, again, always invested a major effort in each of his sentences. I suspect, however, that Heisenberg had read his paper 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 I shall explain, Heisenberg's own approach in his creation of quantum mechanics in effect 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 become infinite matrices, infinite square tables of quantities, with a proper rule of algebraically manipulating them, instead of regular functions of coordinates and time, as in classical physics. Physically, these new variables are linked to the probabilities of the occurrences of certain observable phenomena, in this case, spectra, instead of describing the motion of quantum objects on the model of classical mechanics. In this sense, Heisenberg's mechanics was *symbolic* mechanics, as Bohr often referred to it, thus echoing his earlier thinking concerning his 1913 atomic theory and in effect extending it to his interpretation of quantum mechanics, and to his philosophy of physics and nature, a philosophy that quantum theory made imperative for him. Heisenberg's approach reflects the epistemology that made Rutherford's and Einstein's questions, stated above, meaningless, at least to Bohr and Heisenberg, and those (not very many) who were willing to follow them.

I am now ready to finish my Bohr and Rutherford story. Upon receiving Rutherford's letter, cited earlier, which urged Bohr to follow "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," Bohr took a ship from Copenhagen to Manchester. As he recollected in his Rutherford Memorial Lecture after noting the "embarrassing" nature of the situation:

> 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. (*PWNB* 3, p. 42)

Well, perhaps! But then something reflecting the character of Bohr's thinking, both physical and philosophical, would be lost as well, and besides we do not know the details of these negotiations and what Rutherford aimed to cut or change. Be that as it may, both Bohr's determination and Rutherford's patience deserve credit for bringing Bohr's paper to publication. Eventually, Bohr received his Nobel Prize for the work presented there. More importantly, it changed the course of the history of physics and the philosophy of physics. So, sometimes physics and philosophy do become reconciled in practice to the benefit of both and their relationships.

They can also be brought together within a given theory, such as quantum mechanics, although it takes the likes of Bohr or Heisenberg to do so. Bohr's thinking concerning quantum phenomena may be said to be both fundamentally physical and fundamentally philosophical. It is fundamentally physical, and not only theoretical but also experimental, *empirical*, one might even say, because, as Heisenberg said, “its every detail can be subjected to the inexorable test of experiment.” However, and this is why Bohr’s thinking is fundamentally philosophical, no such test, Bohr argues, is possible apart from our conceptual, philosophical determination of both such tests themselves, and what they tell us about how *nature* makes the outcomes of these tests possible. I underline nature here, because while we can always control the setups of such tests, we can never fully control their outcomes, especially in quantum physics, which leads to the irreducibly probabilistic nature of our predictions concerning even primitive individual quantum processes.

In this respect, 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 as well, not the least by being philosophical enemies, because their confrontation helped to advance their conflicting philosophical views. Einstein, as I said, believed, specifically against Mach’s views, that we 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 on the basis of experience. In commenting on Heisenberg’s (more Machian) contention in his paper introducing quantum mechanics to the effect that the paper aimed to deal only with “quantities which in principle are observable” (Heisenberg did not quite follow this principle in his argument itself), Einstein told Heisenberg that our concepts and theories decide what could be observed (Heisenberg 1971, p. 63). Einstein’s argument impressed Heisenberg and, in part, guided his work on the uncertainty relations. Einstein’s insight is crucial because it leads to a questioning of the uncritical use of the idea of observation, an idea that has been a subject of much discussion throughout the history and philosophy of science. He argued against the empiricist or positivist “philosophical prejudice,” which “consists in the belief that facts by themselves can and should yield scientific knowledge without free conceptual construction.” He added: “such a misconception is possible only because one does not easily become aware of the free choice of such concepts, which, through success and long usage, appear to be immediately connected with the empirical material” (Einstein 1949a, p. 47).

Bohr follows Einstein insofar as free conceptual constructions, mathematical and philosophical, are seen by Bohr as decisive as well: there could be no quantum mechanics or his interpretation of it otherwise. What else is the concept of complementarity, or in his 1913 theory, the concept of a quantum jump? However, he departs from Einstein insofar as this free conceptual construction is, in his interpretation, no longer in the service of describing the ultimate nature of quantum objects and processes, Einstein’s key desideratum for a physical theory. In Bohr’s scheme quantum objects and processes are placed beyond the reach of our thought altogether. Our conceptual construction is now only in the service of our predictions concerning the outcomes of observable and constructible quantum experiments, or quantum phenomena, made possible by uncircumventably unconstructible quantum objects. Einstein found this way of thinking about physics "logically possible without contradiction," but "very contrary to his scientific instinct," and this was also, and in the first place, his philosophical instinct (Einstein 1936, p. 375). That may be, and yet it was this type of thinking that, as Einstein, as we have seen, in fact acknowledged in invoking its insecure and contradictory foundation, made Bohr's 1913 thinking "the highest musicality in the sphere of thought." This highest musicality had, I would contend, never left Bohr's thinking about quantum physics. If anything its harmonies had become ever more complex, without losing any of their highest musicality.

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