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Emergence of complementarity and the Baconian roots of Niels Bohr's method

Slobodan Perovic*

Department of Philosophy, Faculty of Philosophy, University of Belgrade, 11000 Belgrade, Republic of Serbia

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ABSTRACT

I argue that instead of a rather narrow focus on N. Bohr's account of complementarity as a particular and perhaps obscure metaphysical or epistemological concept (or as being motivated by such a concept), we should consider it to result from pursuing a particular method of studying physical phenomena. More precisely, I identify a strong undercurrent of Baconian method of induction in Bohr's work that likely emerged during his experimental training and practice. When its development is analyzed in light of Baconian induction, complementarity emerges as a levelheaded rather than a controversial account, carefully elicited from a comprehensive grasp of the available experimental basis, shunning hasty metaphysically motivated generalizations based on partial experimental evidence. In fact, Bohr's insistence on the "classical" nature of observations in experiments, as well as the counterintuitive synthesis of wave and particle concepts that have puzzled scholars, seem a natural outcome (an updated instance) of the inductive method. Such analysis clarifies the intricacies of early Schrödinger's critique of the account as well as Bohr's response, which have been misinterpreted in the literature. If adequate, the analysis may lend considerable support to the view that Bacon explicated the general terms of an experimentally minded strand of the scientific method, developed and refined by scientists in the following three centuries.

1. Understanding complementarity

Bohr's principle of *complementarity* along with *wave–particle duality* has been, albeit controversially, a dominant tenet of quantum mechanics. Bohr (1925) argued for the necessity of conceiving the wave-like properties of quantum systems, already well explored and accounted for in terms of emerging wave mechanics (Schrödinger, 1927), as complementing the particle-like properties accounted for by the newly developed matrix mechanics of Heisenberg (1926). The idea of complementarity between space–time coordination and the laws of dynamical conservation and between the principle of superposition and the laws of dynamical conservation stemmed from and supplemented this more general wave–particle complementarity.

Although Bohr's view appears to be closely tied to the experimental advances of his day, some contemporary physicists, such as E. Schrödinger and A. Einstein, were reluctant to embrace it, seeing it as an obscure attempt to reconcile contradictory concepts of particles and waves, and conflicting quantum and classical mechanics. Bohr's pre-complementarity correspondence principle of classical and quantum states (Bohr, 1913), although central to the so-called Old Quantum Theory, initially created a similar controversy, as it discarded spatial continuity by introducing the "quantum jumps" of electrons from one discrete energy state (i.e. an orbit around the atomic nucleus) to another.

The contention surrounding complementarity has persisted, and philosophers and historians of science continue to explore it. For example, Scheibe (1973) interprets complementarity as an account focusing on complementarity of *phenomena*—the pieces of information or forms of experience that supposedly avoid the ontological contradictions and dilemmas of classical approaches, thereby allowing one to use the classical properties selectively in interpreting experiments.

For his part, Murdoch (1989) interprets Bohr's complementarity as a form of pragmatism with a realist slant. Thus, one needs to make use of classical concepts, albeit within the limits of quantum principles. We can talk about waves but only as these are limited by the quantum of action. In addition, quantum theoretical

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* Tel.: +381621793584.
E-mail address: perovicslobodan@gmail.com

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methods are used only symbolically, and classical concepts cannot be interpreted in a purely realistic sense. Murdoch illuminates this aspect of complementarity by analyzing I. Kant's influence on Bohr through a Bohr family friend, Danish philosopher H. Høffding. Faye (1991) also relies on the Kantian aspect of Bohr's philosophical approach but arrives at the opposite conclusion; he determines that Bohr was a staunch proponent of anti-realism.

Howard (1979, 2004) approaches the issue from a different angle, emphasizing that Bohr's complementarity stems from his insistence on the entanglement of the experimental apparatus and the observer (only later formulated in explicit terms), as a provision under which one can unambiguously ascribe properties to observed objects.

Others have made determined efforts to debunk the principle of complementarity. Bub (1974), for one, argues that the complementarity principle is an obscurantist account open to diverse and mutually exclusive interpretations, and in this respect is akin to Kant or Wittgenstein's seemingly profound but in reality obscure philosophy. In his interpretation of several claims Bohr made at the beginning of the development of the complementarity principle, Bub charitably says some can be interpreted as versions of Heisenberg's theory that “in some sense the measurement process engenders the magnitude measured” (Bub, 1974, 44). An alternative and less charitable interpretation is his assessment that “Bohr's contribution to the Copenhagen Interpretation” is “that of a remarkably successful propagandist” (Bub, 1974, 45). Thus, Bohr's complementarity “endows an unacceptable theory of measurement with mystery and apparent profundity, where clarity would reveal an unsolved problem” (Bub, 1974, 46). In his hands, Bohr emerges as a stern ideologue who sees “the statistical relations of quantum mechanics as the confirmation of an approach to the problem of knowledge that had fascinated him since youth” which disposes of the notion of truth. Sadly, Bohr's uncritical commitment to a brand of unsophisticated instrumentalism was imposed on others. Similarly, Beller (1999) and Bitbol (1996) argue that the success of complementarity does not stem from its insights, but from Bohr's unprincipled imposition of his metaphysical preferences on dissenters.

Landsman (2006) has recently warned that “Bohr bashing” is historically highly problematic. Yet only a comprehensive, plausible, and positive account of Bohr's complementarity can reverse this trend. To this end, I argue that our understanding of Bohr's complementarity and its contentious nature should arise from our reflections on his method. Before asking ourselves what exactly was Bohr's account, we need to understand how and in what context he built the account in the first place. Such analysis will minimally tell us what kind of questions concerning complementarity we should ask and those questions we should sideline or abandon entirely. More specifically, Bohr's work in general and complementarity in particular should not be characterized as a relentless pursuit of any one metaphysical doctrine or a narrow epistemological concept. They were, rather, a result of a general method akin to the synthesis of experimentalism and inductive methodology, as first explicated by Bacon (2000) in the 16th century.[1]

Although, perhaps, Bohr's brand of experimentally minded empiricist methodology can be successfully analyzed independently of any one empiricist account, comparison with Bacon's brand of empiricism will enhance our understanding. Briefly stated, Bacon's foundations of the scientific method in his *Novum Organum* offer a convenient noncontroversial template of Bohr's method. Admittedly, other tendencies can be discerned in it as Bohr valued experimentation with different approaches perhaps as much as he valued the role of experiments in physics, even though they may be less central. Some of the most notable attempts to examine philosophical influences on Bohr's work have tracked certain features of Kantian philosophy within it (Hooker, 1972; Kaiser, 1992; Chevalley, 1994). We will see that some of these features are, at least *prima facie*, reminiscent of the key Baconian elements of Bohr's method. We will turn to this important issue in the concluding section.

There are three major stages in Bohr's work: (1) The Correspondence Principle; (2) Complementarity; (3) Bohr's response to the EPR paper. While all three could be an outcome of the same methodological approach, they differ in their theoretical significance and the roles they played in the development of quantum theory. The correspondence principle was groundbreaking. While less ambitious, complementarity is a reasonable and carefully crafted account that was pivotal in providing direction for further research into and understanding of quantum phenomena. Finally, the response to the EPR paper was an important insight offered in terms of the conceptual critique. In this paper, I will focus on complementarity, which has become the most controversial of the three.

At the time of its emergence, complementarity was not an opaque metaphysical stipulation but the result of a Baconian method that focused on careful inclusive examination of experimental results that shunned generalizations drawn from partial evidence. Bacon suggests that the inductive process consists of the stage of gathering multiple particular aspects of the experimental setup and processes (particulars) by “bare experience”. This gathering must be comprehensive, given the limitations of senses and biases in the selection of relevant observations. The second stage involves careful elicitation of a complex web of axioms (i.e. experimentally elicited hypotheses) from the particulars; such elicitations avoid hasty generalizations based on metaphysical preferences and partial grasp of particulars.

Bohr's statements on and his arguments in favor of complementarity match this two-stage division of the inductive process, as well as the characterization of the stages, while offering further specifications according to the context of the phenomena at stake. He insists that since they belong to the groundwork of the theory based in experience, rather than to theoretical accounts subsequently drawn from them, the reports on the observations of the experimental setup and processes are expressed in “common language” that adheres to locality and discreteness. (Because the theoretical concepts of classical physics need not depart from such language, they can be used to elaborate such reports.) The theoretical accounts are only subsequently elicited from the groundwork (particulars). Only in the second stage of the process are the hypotheses that renounce locality and discreteness introduced (Newton and Maxwell fortuitously never needed these to elicit their theories.)

The process of gradual elicitation from a comprehensive experimental base produces the “axiom” of complementarity. The process adequately synthesizes hastily generalized wave-mechanical and quantum-particle accounts, treating them as “imperfect axioms”—Bacon's term for axioms of limited use, the generalization of which is motivated by ready-made ontological preferences. Finally, the complementarity axiom, as much as any other such general axiom, appears unintuitive because it is a result of a dogged commitment to the comprehensive reliance on the experimental base.

As my goal is to reveal the method that resulted in complementarity, rather than to offer a full-blown account of it, I will focus primarily on its emergence in 1926. Yet, in light of my analysis of different aspects of Bohr's method, I will address some specific differences that the development of complementarity over

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[1] I will explain shortly why the work of other empiricists is not as relevant as Bacon to the understanding of Bohr's method.

the years might have brought about, as well as looking at diverging views on whether Bohr adhered to wave/particle complementarity in a strong sense at all, whether his view was inconsistent, and whether complementarity concerned experimental phenomena or descriptions of atomic states.

It may not be surprising that there is a major undercurrent of Baconian experimentally oriented inductivism in Bohr's work. Bohr's method is nothing more than a continuation of a method, first explicitly illuminated by Bacon, praised, practiced and improved upon by the members of the Royal Society, which became common and indispensible in the general scientific enterprise.[2] One could ask whether Bohr picked up Baconian method from being acquainted with Bacon's work. Although I will not pursue this issue here, it should be noted that this is not unlikely given that philosophy was discussed in Bohr family. For instance, the most distinguished Danish Kantian philosopher at the time, namely H. Høfding, was a Bohr family friend. It also seems that the Baconian method was a subject of some discussion in Rutherford's laboratory in Manchester where Bohr worked in the early phase of his career.[3]

Having said that, if Bacon explicated some central aspects of what the practice of science has been all along, Bohr did not need to be acquainted with the experimentalist inductive method through Bacon's work at all. In fact, unlike Einstein and Schrödinger, two main opponents of his complementarity account, he was trained as an experimentalist; and in his first published paper (Bohr, 1909) he suggests an account based on his own experiments.

2. Bacon's experimental inductivism

We are interested in Bacon's approach to induction rather than in the issues that surrounded this concept in the work of other empiricists for a couple of reasons.[4] His account, unlike that of other empiricists, is closer to scientific practice and is developed as a study of the scientific method. It is concerned with the methodological foundations of science in a very specific sense, while other empiricists are concerned with general epistemological issues relevant to everyday knowledge, even though the success of science has basically motivated them. This is why Bacon developed a detailed methodological normative account of induction in science rather than a general epistemological account. Nevertheless, it has been repeatedly conflated with induction as discussed by Hume, even though it clearly addresses a different concern that can be considered independently from the issues that Hume's arguments raise (Hacking, 1974). It certainly is not primarily concerned with enumerative induction as a method—it is not even clear to what extent, if at all, Bacon was interested in this issue.

In general terms, Bacon's inductive method requires comprehensive gathering of experimentally arrived at phenomena in *Stage* 1 of the process, and the gradual devising of axioms of different abstraction from them in *Stage* 2. Such a process ultimately aims at deriving the most general axiom that adequately captures the entire multitude of relevant experimental phenomena and reconciles all intermediate axioms. Such a final axiom becomes a central tenet of the theory until new axioms questioning it are devised from novel experiments.

As will quickly become clear, Bacon's *axioms* are what we might call today *experimentally based hypothesis*.[5] As they are supposed to be drawn directly from experimental results, Bacon labels them "axioms" to distinguish them from the hypotheses arrived at solely by speculation and only subsequently superficially squared with partially selected experimental results.[6]

It is crucial that all the formulated axioms, including the most abstract ones, are closely tied to relevant experimental set-ups in the following three ways. First, the axioms should be *elicited or drawn from sense and particulars* (Bacon, 2000, 36, XIX) in experiments. Particulars sorted into the so-called tables refer to the elements of various experimental situations and processes examined by our senses, not to metaphysical/ontological particulars, as the term is more commonly understood today. Thus, they are gathered observationally in *Stage* 1, while the activation of the intellectual capacity that tries to connect them in axioms belongs to *Stage* 2 of the process. Particulars, the "effects" (Bacon, 2000) or "the works" (Bacon, 1874), as Bacon terms them as well are a carefully characterized specific notion reflecting different aspects of the experimental practice and processes. They are an experiential store of experimentation. (ibid. 80–82) (It is certainly not a generic philosophical notion introduced simply in contrast to generalizations allegedly drawn from particulars by enumerative induction.) In *Stage* 1, one thus confines one's activity to recording different aspects of the experimental process as they appear to one's senses. Even the lowest axioms are slightly above the "bare experience" (Bacon, 2000, 83, CIV) where particulars dwell. Gathering of particulars is a process autonomous from, and thus open to different interpretations by intellect that constructs axioms. Thus, in contrast to the "actual measures, signs or stages of a process which are visible in bodies" in *Stage* 1, *Stage* 2 seeks a process that "for the most part escapes the senses" (ibid. 106, VI) by connecting particulars. And particulars are recorded as "written experience" (ibid. 82, CI) that enables intellect to construct axioms in a more precise manner than if it relied on memory alone.[7]

Second, as much as possible, the elicitations of axioms in *Stage* 2 should be based on the *comprehensive gathering of particulars* in *Stage* 1. This does not merely concern the repetition of the experiments of the same sort, the main point of concern for empiricists pursuing the Humean problem of induction. Of course, repetition is necessary when gathering experimental particulars because "by itself the sense is weak and prone to error" (Bacon, 2000, 45, L). But a more complex difficulty is that the senses are *partial* as well—dull, limited

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[2] It is a Baconian method only insofar its initial explicit elaboration is concerned. Even though it is hard to deny that certain key aspects that are essential to my analysis constitute scientific methodology in one way or another, I will not argue this here. I will rather demonstrate how Bohr's most controversial work appears uncontroversial in light of Baconian theory. Although this is not a central goal of my analysis, we might gain some useful insights into Bacon's work along the way. If my interpretation of Bohr's method is satisfying, it follows that Bacon got something fundamentally right about the scientific method: more specifically, it can help us adequately characterize a key episode in modern physics.

[3] During his strife with Bohr over the correspondence principle, Rutherford pointed out that Bohr's approach conflicted with Bacon's methodological postulates. This allegation reflects both Rutherford's need to make a strong point as he was taken aback by Bohr's correspondence principle, as much as the perception of the importance of the Baconian method for both Rutherford and Bohr. See also footnote 23 in this paper suggesting a context of Rutherford's critique.

[4] The view of Bacon's philosophy that I invoke is a standard view in the literature on Bacon that does not portray Bacon as either an inductivist of Mill's type or a Humean empiricist. His notion of induction was substantially different from both, a kind of experimentalist induction. The key differences are pointed out by McMullin (1970, 1990), Sargeant (2001), Hacking (1974), Hattiangadi (2005; forthcoming) and others. Naturally, I focus on the key points of Bacon's method which ground my analysis.

[5] For more interpretative details see Stargen (2001) and McMullin (1990; 2001).

[6] Today, the notion of "axiom" is mainly used in the context of mathematics, while "hypothesis" has multiple, often subtly different, meanings. Yet these two terms that I will use interchangeably refer to Bacon's notion of axiom and all the subtleties it implies.

[7] One could argue that particulars, in part, at least implicitly, include theoretical physical entities such as particles, along with their capacities and properties which are not directly observed. Even though Bacon may have conflated the observables of the experimental process with the entities postulated by the theory (axioms), one could develop his account in different directions. We are interested in the clear distinction between observable particulars of the experimental setup on the one hand, and the axioms that introduce theoretical posits by combining particulars on the other.

and deceptive. (ibid.) "[t]here are so vastly many particulars … and … they are so scattered and diffuse that they confound understanding." (Bacon, 2000, 82, CII). In the experimental process, the senses are exposed to a "variety of shapes" (ibid. 227, V) and "things that strike the senses [often] have greater influence than even powerful things which do not directly strike the senses" (ibid. 45, L).[8] Bacon's work is not that of a naïve empiricist who believes that the *selection of particulars* is immune to biases, including metaphysical and theoretical preferences, even though he believes they constitute "bare experiences". (Their content is autonomous from the intellect, but they are not isolated from it, as it can control the selection process of the senses.) Rather, he insists on the comprehensiveness of experimental research as a remedy to its partiality and a gradual methodical selection of experiments to be performed. To elicit general axioms from particulars, one must rely on "suitable and relevant experiments" (ibid. 45, L) that probe various experimental "instances" (ibid.). Axioms based on only a few particular experimental situations will likely be hasty and skewed, especially if one elicits them in ignorance of other key experimental particulars that might elicit other contradictory axioms.

Third and finally, then, *Stage* 2, devising axioms from a comprehensive table of particulars, should be approached with *a moderately skeptical attitude* to prevent such hasty generalizations. One must "apply afresh to particulars a scoured and level intellect" (ibid. 79, XCVII) and try to do "completely without common theories and common notion." (ibid.) Otherwise, one runs the risk of embracing what Bacon calls the less desirable method in science. He writes:

> There are, and can be, only two ways to investigate and discover truth. The one leaps from sense and particulars to the most general axioms, and from these principles and their settled truth, determines and discovers intermediate axioms … The other elicits axioms from sense and particulars, rising in a gradual and unbroken ascent to arrive at last at the most general axioms; this is the true way. (ibid. 36, XIX)

Thus, the undesirable way of investigating satisfies one's intuitions and ontological preferences at the expense of eliciting axioms from a comprehensive recording and understanding of particulars: "For one merely brushes experience and particulars in passing…" and "forms certain abstract and useless generalities" (Bacon, 2000, 37, XXII). It exploits the biases in recording particulars, rather than trying to overcome them.

Such "anticipations", as Bacon labels such an approach, are more likely to initially win assent than "interpretations." On the one hand, anticipations "are gathered from just a few instances, especially those which are common and familiar, which merely brush past the intellect and fill the imagination" (Bacon, 2000, 28, XXVIII). On the other hand, "interpretations … are gathered piece by piece from things which are quite various and widely scattered, and cannot suddenly strike the intellect." (ibid.) One's gradual ascent to deriving more general axioms, always wary of ready-made ontological preferences and models (which too easily and prematurely lead to the "settled truth"), and guided by a comprehensive grasp of particulars, should help one remove the "mask" that obscures natural objects.

A central question in *Stage* 2 is how to formulate an adequate axiom from the tables of relevant phenomena. One plausible view is that a key aspect of the process consists of choosing appropriately from among rival or alternative suggestions[9]. Scholarship on *eliminative induction*, as motivated by Bacon (and Mill), has focused on this. For example, several authors (Worall, 2000; Weinert, 2001; Earman, 1996; Laymon, 1994) have attempted to explain why a scientist will choose one account over another and what kind of criteria guide her choice. The idea is that various empirical and perhaps some non-empirical constraints determine such choices. And each choice is often represented as a dilemma between two rival or alternative accounts or hypotheses,[10] one of which is dismissed in the process of deliberation. Several case-studies purport to demonstrate the use of eliminative induction in the history of science. Weinert (2001) analyses theories of atoms of the early 20th century, arguing that Rutherford and Bohr arrived at their respective models via such a method. He argues, for instance, that it led to Bohr's dismissal of Thomson's model and incorporation of Rutherford's instead.

While the choice between two hypotheses might be part of Baconian method in certain instances, the following key point is often missing: A scientist who is particularly good at utilizing Baconian inductive method simultaneously keeps her eye on the variety of experimental results she deems relevant. It is thus very questionable whether she proceeds sequentially in a step-by-step procedure deciding between pairs of rival or alternative hypotheses just to move on to the next dilemma. She is more likely to be doing it holistically, albeit gradually, as she refines her hypothesis to accord with multiple experimental considerations. This will be apparent in the case we will discuss in due course.

Actually, the key decisions stemming from clear-cut dilemmas could be detrimental as one would need to stick to a ready-made hypothesis rather than focus on formulating a novel, refined one based on a comprehensive grasp of experimental particulars. The actual choices are much more tentative, in part because they are triggered by fear of being stuck with a hypothesis that favors one set of experimental results at the expense of another. Most importantly, the decisions one makes concern the ways of *constructing new suitable axioms* from multiple particulars, rather than making choices between the pairs of ready-made rival or alternative hypotheses.[11]

With respect to our main goal, reconstructing the method that resulted in Bohr's account is best understood in the overall context of mutually dependent multiple theoretical and experimental considerations, rather than as a sequence of isolated choices applied to pairs of ready-made hypotheses. My analysis neither necessarily presupposes that a version of eliminative induction constitutes Bohr's method nor focuses on analyzing the method in such terms, even though one could undertake such a task without contradicting my analysis. I suspect, however, that it might be both elusive and unrewarding to try and pin down Bohr's method in such terms. Thus, analysis predicated on the Baconian method understood in that particular way is not likely to demonstrate that Bohr's complementarity was not obscure, which is our main goal.[12] [13]

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[8] To use a more recent example that illustrates Bacon's point, it has not been uncommon in high energy physics laboratories that a technician fails to record, or a graduate student dismisses as a background noise, a peak at an energy level that is not anticipated by a theory she is trained to adhere to, thereby failing to make an important discovery.

[9] In the relevant literature, rival accounts are taken to contradict each other (e.g. the systems of Copernicus and Ptolemy), while alternative accounts, although not contradictory, offer alternative explanations (e.g. the systems of Copernicus and Kepler).

[10] Although there are substantial differences between hypotheses and models, the argument discussed is applicable to both. A somewhat vague notion of an "account" is taken to encompass both here.

[11] The distinction between alternative and rival hypotheses central to the analysis in terms of eliminative induction is sensitive to the broader context.

[12] Weinert's explanation of Bohr's method deals with the intricacies of Bohr's model of the atom but does not offer insight into the key dilemmas of its development. An explanation in terms of eliminative induction understood in the above-outlined way can offer only partial insights in any case.

[13] Not all accounts of eliminative induction are committed to the above-outlined premises. For example, Norton's (1995) analysis of the development of General Relativity does not rely on the view that Weinert advocates in his analysis of Bohr and Rutherford's theories of atoms.

3. Bohr's inductive method—*Stage* 1: Observing and recording experimental particulars

A main goal of the complementarity account was reconciliation of quantum and classical concepts. A heuristic move, it was intended to help build a satisfying theory, and it formed the basis of Bohr's understanding of quantum phenomena. When Bohr started devising complementarity, diverse experimental results that had accumulated over the course of two decades had convinced the vast majority of physicists that the foundations of quantum theory were very different from those of Newtonian mechanics and the dominant version of electrodynamics. The points of contention concerned the exact foundational elements that had to be reformulated.

Bohr's major contribution to the early debate was his atomic model. It combined the concept of orbits borrowed from classical mechanics with the discontinuous nature of their energy states resulting in quantum leaps from one orbit to another. The latter concept conflicted with the presuppositions of classical theories. A number of novel experiments in the first half of the 1920s, as well as the emergence of wave mechanical and matrix mechanical formalisms, led the community to work towards a new quantum theory. In response to the challenge, Bohr introduced the idea of complementarity of classical and quantum states, along with the notion of wave-particle dualism.

In order to understand the nature of Bohr's proposal and assess its role in the overall debate, we need to clarify Bohr's understanding of a classical concept. This has been a troubling obstacle to students of Bohr's complementarity. However, the question becomes much less controversial, although perhaps outdated in light of subsequent developments, if we understand it in light of Bohr's overall Baconian method. It is essential to understand that there are two distinct stages to Bohr's account that are analogous to those of Bacon's account: *Stage* 1, experimental observations and measurements and immediate reports on them (gathering experimental particulars *via* senses); *Stage* 2, axiom formation from gathered experimental results (particulars).

Now, *Bohr's elaboration of the nature of Stage* 1 is guided by the context of his own research. Thus, the context of quantum theory led him to: (1) explicitly characterize the nature of experimental particulars, observations and observational records in a satisfyingly precise fashion—*hence the analogy with and utilization of locality and discreteness of classical states*; and (2) *clearly distinguish Stage* 1 (gathering particulars) *from Stage* 2 (connecting particulars into axioms) that *involves quantum principles and introduces the general axiom of complementarity*.

Bohr insists that classical concepts are an indispensible starting point in formulating quantum theory. Thus, "[t]he unambiguous interpretation of any measurement must be essentially framed in terms of classical physical theories, and we must say that in this sense the language of Newton and Maxwell will remain the language of physicists for all time" (Bohr, 1931, 692). The adamant tone, more often absent from Bohr's pronouncements, stems from the fact that this and similar statements are important clarifications of what Bohr considers the level on which physical theory is built, namely, the level of the observational records and reports in experiments (i.e. *Stage* 1). Once we realize that he dwells here on the experience as the groundwork for the subsequent elicitation of axioms, it does not puzzle us that he deems this entirely isolated from quantum concepts. And it renders perfectly understandable his insistence on classical descriptions, even though we might well disagree.[14] Only Stage 2 that exceeds the experientially based gathering of particulars can involve the concepts of entanglement of objects and instruments, quantum entanglements and nonlocality.

Thus, as far as the experimental reports directly based on our *sensations* go, it is not likely at all that "the fundamental concepts of the classical theories will ever become superfluous for the description of physical experience" (Bohr, 1929). Bohr is resolute that insofar as the basic observational level is concerned, the account of *particulars*, or in other words, "the experimental arrangement and the record of the observations of experimental situations," "must always be expressed in common language supplemented with the terminology of classical physics" (Bohr, 1948, 313). For instance, in the case of the double-slit experiment, in *Stage* 1, the dots on the screen are observed and their positions recorded, along with the energy of the source and the nature of the light ray, the screen and the double-slit.[15] Similarly, in the cloud chamber, the tracks are recorded and measured, and their quality noted (i.e. whether they are continuous; whether splits appear and of what sort; whether they are straight or curved, and in the latter case, the angle of the curve). These reports subsequently constitute axioms in *Stage* 2. Thus, the dots can be accounted for as hits by particles or as discrete traces of a continuous wave-front. Similarly, the tracks in the cloud chamber can be accounted for as traces of a particle of particular properties that whizzes through the cloud, or as a particular ionization track left by a wave-front. As will be shown shortly, *Stage* 2 is a complex process of relating particulars and formulating axioms; its early steps already relate particulars from multiple experiments.

In *Stage* 1, however, the observations concerning particulars are expressed in the "common language" used and elaborated on by classical physics. Everyday language can be unambiguous in its characterization of the properties of observed particulars. Any experimental particular (i.e. "the experimental arrangement and the record of the observations of experimental situations") can come in the form of "a well-defined meaning in the sense of classical mechanics" (Bohr & Rosenfeld, 1933, 359). The notion of "classical" is defined to the extent to which it is contrasted with the properties of quantum states. Thus, quantum states cannot be straightforwardly characterized as individual states that assume discrete values. For example, a microphysical state with the momentum p lacks a discrete value of position, and vice versa. The account of such state has to involve both the state of the observer and the object. And a state of an entangled system depends on another spatially separated state.[16] In contrast, the position, momentum, force, field potential, etc. in classical mechanics and electrodynamics are unambiguous: they cannot be quantized, entangled or principally mutually exclude each other. *Nor can the experimental particulars*. To use more recent terms, both experimental particulars and classical states are *localized and separable (or discrete)*.[17]

In other words, *Stage* 1 is limited to unambiguous reports of measurements: dots on the screen, tracks in the cloud chamber, clicks of a Geiger counter and recording of their separation in time, acknowledgements of coincidental events, as well as events separated in time, and more complex but essentially similar "works".

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[14] See footnote 19.

[15] Strictly speaking, the "dots" have dimensions, and one can perform measurements on them.

[16] Based on only a couple of observations, we do not know whether two events occurring simultaneously and in a correlated manner are a classical entanglement (the so-called mixed state) or a quantum entanglement (the so-called pure state). We observe and report on two spatially separated states; we do not observe them as entangled. It takes more than two observations to establish whether we are dealing with a quantum state.

[17] See also Dickson (2002) and Bokulich & Bokulich (2005).

The concepts of momentum, position, force or field potential in classical mechanics and electrodynamics stem from such common language describing measurements. The discreteness and localization of momentum and position were always there; they are the foundation of our everyday observations and reports on them. And when their work was compared to quantum theory, Newton and Maxwell were fortunate that properties introduced in their theoretical concepts did not need to depart from properties of localizability and discreteness of observations and immediate reports on them:

> The argument is simply that by the word "experiment" we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics. (Bohr, 1961, 39)

Even though Bohr is quite explicit about what he sees as the classical properties that characterize observational experimental particulars and "written experience", one could question whether classical physics, especially electrodynamics, has relied on the concepts that square with the discreteness that characterizes them (experimental particulars). For instance, is a wave-front discrete, even though it is localizable? When one accounts for the wave-front in classical electrodynamics, one does so by treating its parts as localized and discrete—in as much as the entire wave-front is a localized and discrete compound. Nothing suggests that this analogy cannot be pursued in individual processes that underlie the dynamics of the wave front. As a matter of fact, Bohr is clear about this: "The measurement of electromagnetic field quantities rests by definition on the transfer of momentum to suitable electric or magnetic test bodies" (Bohr & Rosenfeld, 1933, 368). This is the case where such a body is treated as an average component of the electric field, spatiotemporally localized, assuming discrete values, and without any principled limitations on its measurement.

These are the analogies that seem unsatisfying in *Stage* 2's axiom building in the case of quantum phenomena and the nature of the experimentally examined "individual processes." Also, with respect to observations, although we can, for instance, experience a hazy light occurrence in our experiments, we report on it as a single event that can only subsequently be related in multiple manners to another single event observed simultaneously but observationally untangled from it. This is a region whose more detailed properties could be examined and more detailed reports on the states of its components provided in discrete terms (e.g. intensity, color etc. of the components). Arguably, we do not always observe discrete values, although we can try to discern such states in our observations and record them as such. For example, we can observe the dissipation of light but may analyze the process in discrete terms.[18]

Only following the first stage of gathering particulars will one try to formulate axioms. This will consist of attempting to relate the measurements, expressed in classical language, to a variety of suggestions of tentative general axioms that will eventually be reformulated to provide a better grasp of the particulars. The axioms will introduce ontological concepts and models that, in the end, do not have to square with the classical nature of basic states. Thus, the quantum axioms turn on the entanglement of the observed objects and instruments,[19] and they violate the requirement of localization. But these concerns belong to *Stage* 2 as it is "the application of these [classical] concepts alone," i.e. entirely isolated (by the very nature of the process of gathering experimental particulars) from the concepts introduced in *Stage* 2, to the gathering of particulars "[that] makes it possible to relate the symbolism of the quantum theory to the data of experience" (ibid.).

4. Towards complementarity—*Stage* 2: Connecting experimental particulars to axioms

As Bacon tells us, *Stage* 2 of the inductive process is complex:

> For the lowest axioms are not far from bare experience … the intermediate axioms which are the true, sound, living axioms … and also the axioms above them, the most general axioms themselves, are not abstract but are given boundaries by these intermediate axioms (Bacon, 2000, 83, CIV).

Creating a web of axioms depends on the comprehensiveness of observed particulars. As for Bohr and his approach to quantum theory, even though reports on observations are formulated in common language and elaborated in classical terms, very early on in the process, such reports elicit interpretations which can contradict each other. As soon as they start observing and recording, experimentalists already conceptualize lower axioms, closest to the "works." And possible discrepancies in lower axioms can strike experimentalists as soon as they start gathering data. For instance, one interprets hits on the screen in the double-slit experiment either as marks left by a group of particles that only appear to be traces of a wave, or as traces of the wave that appear to be marks of particles. Similarly, initially a track in a cloud chamber is interpreted as either a pattern of the atom's ionization or as a trace of a particle that whizzes by. Thus, the formulations of opposing axioms will invoke the deceptive nature of one's senses by assimilating some aspects of observations (particulars) as mere appearances, while emphasizing others as capturing the actual physical process. The question that physicists try to resolve is not whether our senses are partial—they assume that, as does Bacon—but in what exact way they are partial in particular experimental situations.

Naturally, preliminary accounts (lower axioms) elicited from experimental data tend to initially favor particular interpretations that accord with particular ontological and theoretical assumptions. These assumptions are often the framework within which the experiment is conceived as well. For instance, the experiment leading to the discovery of the Compton Effect, the follow-up of

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[18] One could critique such an assessment by stating that we actually experience quantum as much as classical states and we cannot separate the two phases of observation. We actually see entangled states in the indirect form of simultaneous and correlated events and record them as entangled. But that is, at best, a coherent alternative to Bohr's approach, not a self-evidently superior approach. Moreover, it is an empirical question whether our vision can pick up entangled states (Barbosa, 2012), and thus, neither viewpoint has an *a priori* advantage concerning the "real nature" of our observations. Bohr insisted that the conclusion about the nature of the phenomenon comes after such reports are provided and stems from them, even though one might conflate the two in practice. Failing to make the distinction will result in hasty theoretical judgments of the sort, as Bohr believed, Schrödinger made at the time.

[19] In the following (seemingly obscure) passage discussing the observer-object entanglement motivated by Heisenberg's indeterminacy principle, Bohr clearly separates the particulars-level from the axiom elicitation of such entanglement: "We must on the one hand, realize that the aim of every physical experiment leaves us no choice but to use everyday concepts… On the other hand, it is equally important to understand, that just this circumstance *implies* that *no result* of an experiment, concerning a phenomenon that lies outside the range of classical physics *can be interpreted as* giving information about independent properties of the objects" (Bohr 1938, 25–26; italics mine). Howard's (1979) analysis, even though recognizing the role of the observer-object entanglement in Bohr's theoretical stance, does not recognize the meaning and importance of this separation: the entanglement thesis concerns how an already established "classical" record of observational particulars (results of *Stage 1*) can be *interpreted*—i.e. it concerns the nature of the axiom elicited by the results, without commitment to a lack of ambiguity.

which turned out to be essential in eliciting the complementarity account (to which we will turn shortly), was initially conceived as a repeated measurement of the angles under which quanta presumably scatter off the electrons, and it elicited the treatment of both quanta and electrons as particle-like.

Now, such preferred conceptualizations in an experiment motivate attempts to apply formulated or similar axioms *to other experiments*. Failures of such attempts shed new light on the initial experiment and challenge the initial axioms. Thus, the judgments on the scattering experiments were not made independently of other experiments with similar light phenomena (e.g., Compton related them directly to his experiments with X-rays). Similarly, an account of the nature of the tracks in a cloud chamber was not independently made of the accounts in the light interference experiments. Thus, it turned out that one could treat the initially supposed particle-like entities involved in the scattering as the outcome of the interference of radiation waves and the electron as a standing wave. (This was the interpretation advocated by Schrödinger, Mott and others to whom we will turn shortly.) In such interpretation, the observed value is not simply a value of an angle of scattering; rather, it is the result of the waves interfering with each other.[20]

Experimentation continues until the experimental results suggest an axiom that is acceptable in other experiments as well. Efforts of this sort led, in the case of the scattering experiments, to a much more refined analysis of, theoretical conclusions on, and further experimental consequences. Thus, Compton elicited the possibility of the existence of the electron recoil from the quantum-corpuscular assumption, something lacking under the above-mentioned alternative wave-mechanical assumption. The hypothesis turned out to agree with the follow-up on the scattering experiments (Stuewer, 1975).

Having all this in mind we may be in a better position to address the worries concerning Bohr's seemingly mysterious statements that quantum mechanics is complete, implying that such joint completion of phenomena renders any single phenomenon incomplete in some sense. Held (1994) 886 paraphrases and comments on this view: “In a clear quantum mechanical sense one well-defined phenomenon does ‘exhaust the possibilities of observation', while in another mysterious sense it is ‘complementarity in the sense that only the totality of the phenomena exhausts the possible information about the objects.'” In light of our analysis, phenomena, as experimental particulars, are always incomplete and gathered as such in Stage 1. Stage 2 aims at an axiom that is all-encompassing (in terms of particulars). Furthermore, it need not present an adequate picture according to any ready-made ontological criterion. It is sufficient that it introduces complementary physical features through “imperfect axioms” (wave-mechanical and quantum-corpuscular) to which we turn in the following section.

5. Bohr versus Schrödinger: Interpretation versus anticipation

One should expect that an initial hypothesis arising from a single experiment, when tested against other experimental situations, will typically elicit a subtler hypothesis, often contradicting the more “intuitively” appealing initial hypothesis (i.e. the hypothesis in agreement with one's preferred ontological assumptions). A less desirable way of investigating is marked by hasty and partial induction from particulars that serve the purpose of satisfying such intuitions and ontological preferences. In the 1920s, Bohr was faced with multiple experimental situations, each more or less *prima facie* suggestive of different compelling interpretations in accord with particular ontological principles. His *moderately skeptical* attitude led him to select a middle road in interpreting the experimental results by understanding the “pull” of the opposed axioms without pronouncing them general if he deemed relevant particulars to elicit doubt.

The complementarity account was truly a product of the experimental context as its elicitation disposed of the requirement to come up with a single coherent ontology if some of the relevant experimental particulars precluded it. While the coherence of the account was something that might eventually emerge from connecting particulars into axioms, even such a soft coherence requirement was secondary to the requirement of comprehensiveness, i.e. the inclusion of relevant particulars. Ideally, one can obtain axioms as coherent as our characterizations of observations and reports on them. In this sense, Newton and Maxwell were lucky. But if this is not possible, one should go as far as possible in formulating axioms, as long as none of the experiments is “suitable and relevant,” and the particulars characterizing them are overlooked.

Bohr played a mediating role in the “division of labor” in the community of physicists working on quantum mechanics, toning down those who more hastily pursued their preferred accounts. Arguably, Schrödinger's early insistence on the wave-mechanical approach to quantum phenomena as superior to the alternatives is a prime example. Contrasting Bohr's Baconian “interpretation” with Schrödinger's “anticipation” clarifies the context of the debate. The latter had a limited role to play within the broader methodological context provided by Bohr, but perhaps it is not an overstatement to say that at the time of pursuing it, Schrödinger was somebody who “just merely brushes experience and particulars in passing …” (Bacon, 2000, 37, XXII) and “forms certain abstract and useless generalities from the beginning.” Perhaps this is too harsh; nevertheless, unlike any other physicist involved in the debate at the time, except Einstein, Schrödinger insisted on the primacy of clear metaphysical ramifications of scientific enterprise as a starting point. And this commitment led him to pursue the wave-mechanical interpretation of quantum phenomena based on *the principle of spatio-temporal continuity*[21] (Schrödinger, 1926, 27). Moreover, Schrödinger's insistence on the *anschaulichkeit* of an account of microphysical states seems to be a pursuit of anticipation that “fill[s] imagination.” It should not be surprising, then, that Schrödinger was the most vocal of Bohr's critics.

Some authors (Beller, 1999; Bitbol, 1996) suggest that history would have taken a different turn were Schrödinger free of the Copenhagen pressure to pursue his supposedly philosophically more acceptable interpretation of quantum phenomena. Given that Quantum Mechanics was just forming at the time, the difference this would have made could have been tremendous, given that the debate concerned both the meaning and the plausibility of the application of formalisms to the experiments. It was not merely an “academic debate” on the preference for a particular ontology of an already established theory. There was no metaphysical underdetermination of distinct ontological interpretations based on a formally unified theory. Actually, there was no underdetermination of any kind, as two

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[20] In the subsequent experimental strand that was crucial in eliciting the complementarity account, physicists relied on the notion of *coincidences* in Bothe & Geiger's (1926) scattering experiments. The initial values of the presumed energy wave fronts turned out to be more suggestive of the energies in particle-like interactions, in light of the results of a different group of related experiments performed in the cloud-chamber (Stuewer, 1975; Perovic, 2006). (Schrödinger and others tried to stick to the wave-mechanical hypothesis, as we will see shortly.).

[21] Heisenberg's instrumentalism, inspired by the philosophical views of Logical Positivists, was another method of coping with the puzzling quantum phenomena that were at odds with Bohr's views. This aspect of the debate can be understood within the ramifications of our analysis but we will not pursue it here. Suffice it to say that Bohr dismissed Heisenberg's insistence on the principle failure to understand individual microphysical processes as a confounding of his instrumentalism, not as an outcome of analysis based on experiments.

ontologically, empirically, and formally distinct approaches (matrix mechanics and wave mechanics) were equivalent only in a very limited sense (Muller, 1997a, b; Perovic, 2008). The stakes were high in the debate.

A generally accepted historical account (Stewer, 1975) will tell us that seeing discontinuity as an aspect of microphysical processes (Einstein argued for it after 1915) resulted from the above-mentioned second series of scattering experiments. The first series of Compton's experiments left open the question of the exact nature of the individual processes that could resolve the particle/wave dilemma. They could be interpreted in two apparently dichotomous ways. The first hypothesis was that the incident wave interfered with the electron wave, and this resulted in various angles of scattering. Alternatively, the quantum-corpuscular hypothesis, suggested initially by Einstein in 1915, predicted the angles of scattering and recoil that a particle scattering would produce.

The second series of scattering experiments performed independently by Bothe and Geiger with the help of newly invented Geiger counters (Bothe & Geiger 1926) and Compton and Simon (1925) using recently invented cloud chamber , however, brought surprising results. The scattering in individual interactions was in agreement with Einstein's prediction and was seen as demonstrating the impossibility of wave-like "communication" between atoms posited by the Bohr–Kramers–Slater theory (Ibid.). Bohr argued that the results indirectly but convincingly pointed out the implausibility of Schrödinger's insistence on the continuity of micro-physical processes that he defended in his seminal 1920s papers, as the momentum in interactions between light and matter turned out to be particle-like (Perovic, 2006). [22] Other physicists eventually agreed.

While sticking to the wave-hypothesis, however, one could still insist that discontinuities are only appearances. Thus, in general, the tracks in the cloud chamber are not α-rays at all and cannot be explained without mentioning the particles that produce them. They were determinate tracks left by a spherical wave. Neither is it necessary to conclude that discrete segments of the wave (including those after a scattering takes place) make these tracks analogous to a particle (implying the particle-like interactions of radiation and matter) whizzing through the cloud chamber. What we really observe in such a case, the argument goes, is a trace of the entire atom that ionizes according to the probability given by the wave-equation. Following this line of argumentation, in the scattering experiments performed by Bothe and Geiger, the observed scattering angles could be interpreted as particle-like interference, rather than the statistically distributed interference of wave fronts, even though the latter was expected (and failed) to be a mark of the interaction between continuous wave fronts (Kidd, Ardini, & Anton, 1985). In fact, Mott (1926) developed such an alternative explanation in the late 1920s, and Schrödinger followed his lead in the early 1930s.[23]

Despite its apparent subtlety, Bohr sidelined this line of Schrödinger's argument in his pursuit of complementarity. Bohr's argument can be easily misinterpreted as intellectual bullying (Bub, 1974; Beller, 1999) if one does not understand his method. Yet as noted above, Bohr's attempt was much closer to Baconian "interpretations" which "by contrast [to anticipations] are gathered piece by piece from things which are quite various and widely scattered, and cannot suddenly strike the intellect." Bohr states that the results of the Compton–Simon experiments demonstrates "the connection demanded by the light-quantum theory between the direction in which the effect of the scattered radiation of the velocity of the recoil electrons [is] accompanying the scattering" (Bohr, 1925, 848). This is something Schrödinger's account fails to acknowledge.[24] Even so, Bohr argues that "the suggestion [on light-quantum approach] does not offer a satisfactory escape from the dilemma" between the wave approach and the light-quantum approach. The conjunction of these two statements, the first embracing light-quantum hypothesis and the second cautioning against its generality, was eventually epitomized in the principle of complementarity. Some critics see the conjunction as strong evidence of Bohr's obscurantism and syncretism. Others argue that he should not have blocked alternative approaches that advocate one of the two coherent accounts, by imposing a wishy–washy syncretism. Contrary to what critics claim, however, these statements are a direct and fortunate result of his inductive method.

When he was formulating his complementarity account, Bohr had to deal with a sizeable body of experimental evidence which supported seemingly very diverse hypotheses. Besides the above-discussed scattering experiments, he considered the experiments with electron collisions, Ramsauer's (1921) experiments with the atoms of gases, and Stern-Gerlach's (1922a, b) experiments with the spectral lines in magnetic fields. As Heisenberg (Bohr, 1985, vol. 6, pp. 20–1) suggests, Bohr was committed to "the requirement of doing justice at the same time to the different experimental facts which find expression in the corpuscular theory on the one hand and the wave theory on the other."

By aiming at "doing justice to the different experimental facts" he aimed at nothing other than deriving an account "from sense and particulars" mitigated by a moderate skepticism. One should not prematurely explain away discrepancies in two different experimental situations by postulating entities clearly suggested by only one of them; the *rationale* for a more general axiom encompassing both experiments should not be purely or primarily metaphysical. Thus, in the scattering experiments in the cloud chamber, one should not forcibly interpret the tracks as patterns of ionization motivated by wave-like appearances in the light interference experiments. Although the ontologically preferred wave-mechanical approach appeared to be applicable in general, and although it swayed Schrödinger, it was shown valid only for the angle of 90 degrees of the incoming light (Kidd et al., 1985, 643). It was not clear that the scattering under non-standard angles could have been interpreted using Mott's or a similar approach, nor whether it should be extended to the other group of experiments.

The logic of wave-mechanical ontology certainly suggested such an interpretation and offered the formal tools to refine it, but the details of the experimental situation gave Bohr good reason to pause, as such an approach would not carefully enough, in his judgment, acknowledge the details of the scattering experiments. It is possible that the tracks are patterns of ionization, and that physicists see them as traces of rays only because they have acquired classical–mechanical treatment habits. Nor is this at odds with the appearance of a wave front hitting the screen in the interference experiments. But one may alternatively and as convincingly interpret the traces using the axiom of another set of experiments, namely those with spectroscopy, where the particle-like entities supposedly enter the spectrometer and leave behind well-segmented lines of spectra.

When faced with such choices, Bohr decided to suspend judgment, while listing the possibilities formulated in a precise manner as "imperfect axioms," i.e. intermediate experimentally based hypotheses of a limited grasp; the wave-mechanical and light-quantum hypothesis in the case at stake. They are useful as long as they are

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[22] An exhaustive analysis of this episode appears in Stuewer (1975) and Perovic (2006).

[23] Von Neumann could have argued it but didn't because of Bohr/Copenhagen pressure; see Bub, 1974(49,129).

[24] Schrödinger tried but failed to assimilate this experimental fact into his account (Schrödinger, 1927, 35; Perovic, 2006).

provisional: “Imperfect axioms as they occur to us in the course of the inquiry … are … useful if not altogether true” (Bacon, 1874, vol. 5, 136).

Baconian science is inherently socialized science, and a division of labor in the scientific community is crucial. Different stances are advocated, making indispensable the work of those who pursue “imperfect axioms” (an aspect of the inductive method that Bacon did not always appreciate but that practice has rendered indispensible). Bohr's task was to present experimental facts and imperfect axioms without prematurely leaning towards any one of them; that is, he avoids generalizing an imperfect axiom by being compelled by the logic of preferred ontology of any imperfect axiom. Bohr's inquisitive tone and his open-ended statements, later often misinterpreted as ultimately confused, result from what I call above his moderately skeptical attitude towards ready-made ontology and its principles (the continuity principle and the *anschaulichkeit* requirement in Schrödinger's case) and his reluctance to prematurely make a final judgment. Instead, Bohr lists seemingly opposing accounts, always trying to explain how they contribute to the explanation at a more general level (a more general axiom). Such analysis refines the concepts that imperfect axioms utilize by putting them into a broader context of multiple experimental situations, thus gradually deflating the stark contrast between them (imperfect axioms) (see Bohr, 1925). The subtleties of various experimental situations expose the rigidity, or “limitations” in both Bohr's and Bacon's parlance[25], of commitments to the ontological vocabulary of initially mutually exclusive accounts.

Bohr insisted on tentative rationalizations in connecting particulars, refusing to accept ready-made accounts, as these are experimentally inadequate as general accounts.[26] Ideally, there should be a comprehensive axiom composed of the “rational utilization of all possibilities of unambiguous interpretation of measurements.” Thus, each possibility, i.e. the wave account and the quantum-corpuscular account, is an interpretation stemming from certain unambiguous particulars. But as each leaves out certain relevant particulars, the task is to utilize both in a more general axiom, uniting the two imperfect axioms in a general hypothesis of complementarity, the comprehensive experimental basis of which was presented in an elaborate form very similar to Bacon's tables.[27]

In contrast, Schrödinger's interpretation of the scattering experiments is ontologically rather than experimentally motivated to indirectly reinterpret the results. (The experimentally motivated account devises new ontological categories instead of committing to existing ones.) As such, it is illustrative of what Bacon characterizes “axioms formed by argumentation.” In his view, “[general] axioms formed by argumentation cannot be good at all for the discovery of new results, because the subtlety of nature far surpasses the subtlety of argumentation” (Bacon, 2000, 37, XXIV).

The relentless pursuit of an imperfect axiom as a general hypothesis is usually not a good idea, as it becomes entangled in singular problems, requiring the postulation of singular, limited concepts that cease to contribute to the understanding of the overall set of experiments. Thus, Mott's general approach is satisfying with respect to the limited measurements in the cloud chamber, but in the case of Compton–Simon experiments, an explanation along the same lines is satisfying only if the incident wave attacks the electron at an angle of 90 degrees (Kidd, Ardini and Anton). The explanation of scattering at angles other than 90 degrees, predicated on the wave-mechanical hypothesis, has to be amended by ad hoc interpretations and concepts hardly applicable to other experimental situations, as Schrödinger himself acknowledges in his paper on Compton effect (Schrödinger, 1927, 35). As each experimental situation requires one to develop singular, limited accounts connecting particulars, while at the same time, such explanations should be brought into an agreement with each other at a more general level, it is not wise to choose a candidate for the general hypothesis which is clearly applicable to only some of the experimental situations, and which can become a general hypothesis only by being forced into other experimental situations. Bohr's alternative suggestion is premised on an attitude that the general hypothesis should arise from multiple experimental situations and be developed within the overall context.

Even if the wave-mechanical approach could have been worked out fully, Bohr erred on the cautious side: he pursued a successful long-term strategy. If Schrödinger had his way, Heisenberg, whose account was perceived as a rival to Schrödinger's at the time, would have been perceived as a loser. Heisenberg's approach begins with the particle-like properties as basic, motivated by the existence of the spectral intensity lines. Not surprisingly, Schrödinger complained about its unintuitive nature (the violation of the principle of *anschaulichkeit*) and its violation of the continuity principle. If the particle-like properties were deemed mere appearances right at the start, rather than phenomena that might reflect an aspect of the observed systems, it would be hard to motivate development of the approach based on matrix mechanics. This could have been an undesirable outcome in the given context.[28]

If one is keen to debunk Schrödinger's approach, one could identify it as an example of Bacon's harsh characterization of the “imperfect way” of settling a scientific debate: “If [such] men betake themselves to philosophy and universal speculation, they distort and corrupt them to suit their prior fancies” (Bacon, 2000, 46, LIV). It is true that Schrödinger was not accused of conservatism because of his innovative stand-point as Beller and others suggest, but because of his traditionalist insistence on the “imperfect axiom” motivated by metaphysical considerations. However, Bohr reminded Schrödinger of the limitations of his hypothesis without diminishing its importance as an “imperfect axiom.”

Thus, Bohr's inductive approach is not immediately as attractive to a philosophically trained eye as is Schrödinger's. Yet, it was accepted at the time, even by Bohr's leading critics, eventually including Schrödinger himself. The approach was an “umbrella” for other approaches, giving rise to a tightly knit community of socialized science. It enabled the formulation of a satisfying view closely tied to the abundant experiments concerned with a number of very different phenomena, examined by diverse experimental techniques, which elicited distinct hypotheses and resulted in *prima facie* irreconcilable results.

The concluding axioms of a truly inductive approach “being gathered here and there from very various and widely dispersed facts, cannot suddenly strike the understanding … [and] cannot help seeming hard and incongruous, almost like mysteries of faith” (Bacon, 2000, 38, XXVIII). This might be the reason why physicists who expected from the general axiom a transparent coherence set by the criteria they deemed intuitive found Bohr's argument unsatisfying. And it might be why Einstein characterized Bohr's approach as “religion.” In a similar vein, Schrödinger criticized it for defying our limits of explanation (the limits being the spatio-temporal continuity of physical processes). Both agreed that a general characterization of microphysical processes had to postulate either waves or particles as basic.[29] Yet the majority was satisfied, as Bohr fulfilled the main objective of the inductive method in developing a tentative ontology

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[25] See the end of the quotation at the beginning of this section.

[26] The second wave of the scattering experiments was not an instance of crucial experimentation in physics, as it proved one (wave-mechanical) of the two competing hypothesis to be incorrect and the other (corpuscular) to be likely correct. It was crucial in the sense that the wave-mechanical hypothesis turned out not to provide a general account of all relevant experimental situations.

[27] Bohr's, 1925 paper published in *Nature* has precisely such a structure.

[28] Heisenberg and Schrödinger's approaches were far from established at the time. It was not yet clear if there was any substantial relationship between the two, nor whether either was operational with respect to the experimental results; see (Perovic 2009).

[29] Ten years earlier Rutherford criticized Bohr's correspondence principle on the same general philosophical grounds.

from a balanced assessment of the experimental results, which also suggested direction for further work.

Devising ontology that squares with widely appealing metaphysical principles is a secondary and often unattainable objective. Thus, the results of the method are likely to be deemed obscure or even incoherent and accordingly dismissed by those whose understanding is primarily guided by their insistence on received metaphysical notions and who view the interpretation of the results as simply choosing between a few ready-made ontological standpoints. Bohr's arguments appear puzzling, obscure or perhaps even downright inconsistent (Held, 1994) only in light of metaphysical preferences, but they lose their aura of obscurity if we understand them as results of the inductive method and its aims.

In theory, one could expect a satisfying axiom drawn from experiments to be as close to the nature of our measurements as classical theories are. But this has not turned out to be the case. As Bohr states, "It is just this entirely new situation as regards the description of physical phenomena that the notion of complementarity aims at characterizing." The best one can do given the comprehensive analysis of the experimental results and the intermediate axioms stemming from them is to suggest a general axiom that microphysical properties exhibit such nature only partially (i.e. in interactions of light with matter). Its other aspects in different circumstances exhibit a holistic nature, defying localization and discreteness (which made a great impression on Schrödinger). Hence, complementarity is "the most general" axiom "of which those intermediate axioms," namely wave-mechanical and quantum-corpuscular, "are really limitations."

In general, the key objective of axioms is to be comprehensive in gathering relevant experimental results or particulars. Thus a dilemma whether complementarity concerns phenomena, i.e. incompatible observables, or a much more metaphysically committed descriptions of atomic objects (Held, 1994; Grünbaum, 1957) may be a false dilemma after all. It points, however, to the key features of complementarity. The higher the axiom, the more comprehensive it should be with respect to initially incompatible observables gathered in lower axioms. Yet intermediate axioms already try to provide descriptions as well. And the axiom above them may not succeed in synthesizing such descriptions to everybody's liking in the end, but it certainly should not violate the basic requirement of comprehensiveness.

Held (1994, 872) points out that Bohr "tries to develop a consistent version" of complementarity, although it is not clear he ever succeeded. One should keep in mind, however, that, if required, ontological neatness is always sacrificed to experimental comprehensiveness. And since the wave or particle picture are never fully adequate on their own, it is questionable how Bohr could have accepted wave/particle complementarity in a stronger sense, as a consistent metaphysical thesis (Held, 1994; Grünbaum, 1957). Perhaps he never intended it that way, even though it is possible he turns this sort of complementarity into a thesis on incompatible observables only later on. In light of our analysis the former possibility is much likelier.[30] [31]

Thus, the final axiom Bohr produced is not much of an axiom when measured by the criteria of ready-made neat ontological accounts, but given the experimental results available at the time it could not have gone much further. It is nothing more than the outcome of a comprehensive and careful relating of particulars into intermediate axioms, each in isolation leading to the exclusion of certain experimental situations (described in classical terms in *Stage* 1) and the favoring of others.

In fact, the axiom was intended to assess the situation but also to suggest direction for further research. This is exactly what a satisfying axiom should do "for the road is not flat, but goes up and down — up first to the axioms, then down to the effects [works]" (Bacon, 82, CIII). As Bacon reminds us, the **"**arguments duly and properly abstracted from particulars readily indicate and suggest new particulars" (Bacon, 2000, 37, XXIV**).** Thus, complementarity motivated the proofs of the equivalence of matrix mechanics and wave mechanics (Perovic, 2008). As its crucial intermediate axiom was closer to the experimental results than the complementarity principle it inspired novel experimental elaborations.

6. Conclusion

Bohr's method is a continuation of a long tradition. It is not the only approach in science, but it has been central to many developments, especially in complex revolutionary contexts such as the development of quantum theory, where its ramifications become apparent when physicists directly reflect on them.[32] Despite its centrality, complementarity has not been as pivotal as the correspondence principle, even though it is, arguably, a result of the same method. Its insistence on experimental comprehensiveness that Bohr understood so well, having started as an experimental physicist himself, was understandably a priority at the time it emerged. Its controversial afterlife is a result of different experimental and sociological contexts that we will not discuss here, but it may not be surprising that in general experimentalists still find it a very agreeable account.

Once we have reflected on Bohr's method we are in a better position to interpret complementarity. Minimally, this could lead to the reconsideration of radical critiques, such as Bub's (1974) commentary. Such critiques are based on the unrealistic expectation that one can avoid misconstruing Bohr's view if it is approached head on—even though one does not understand its historical context or the way it was generated. More methodological analysis of the type offered here can give us a hint as to what kind of questions might be fruitful, refine some existing questions and perhaps even suggest satisfying answers. This kind of study supplements the existing understandings of Bohr's work, but it favors certain understandings over others.[33]

As we mentioned at the beginning of our analysis, perhaps the most thoroughgoing account on philosophical underpinnings of complementarity to date has been developed by a group of authors who tracked some key similarities between Bohr's complementarity and Kant's philosophical views. They recognized

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[30] Although it is possible that Bohr developed complementarity as it concerns phenomena at a later stage, in the early 1930 s, and that his initial formulation concerned descriptions of atomic objects alone (Held, 1994), my analysis suggests that from the beginning the former approach was basic. Even his Como lecture (quantum mechanics 'forces us to regard the space–time co-ordination and the claim of causality, the union which characterizes the classical theories, as *complementary but exclusive* features of the description'), that may appear to favor the aforementioned interpretation opposed to mine, indicates adherence to the two-stage approach. Thus, the above statement states that we coordinate particulars in various ways in Stage 2, which can force us, albeit unwillingly, to dispose of kinds of interactions that rely on the concepts (locality) indispensible in Stage 1.

[31] One may run into a danger of validating the claim of the alleged absence or subdued function of complementarity of observables/phenomena in the early

(*footnote continued*)
phase by Bohr's brining forth this basic aspect of complementarity in its fullest force in a later phase, in his argument concerning the EPR. (Held, 1994) The response to the EPR was effective precisely because Bohr made explicit how the EPR argument brushed over particulars, but this does not imply that he didn't develop that aspect of complementarity much earlier.

[32] All Bohr's statements originating after that period that I used clarify the complementarity account but do not extend its meaning to other areas (e.g. biology). Bohr did not change the core of his account. Rather, the afterlife of the account—how it was treated and criticized—is the result of multiple scientific and social factors.

[33] For example, Scheibe's interpretation of phenomena seems quite plausible based on what we now know about Bohr's method.

in Bohr's work the Kantian distinction between *phenomena* (objects of possible experience) and *noumena* (things outside the domain of possible experience), and the underlying distinction of the faculties of sensibility and understanding, reminiscent of, and perhaps reinforcing the sort of Baconian two-stage distinction that we argued forms the foundation of Bohr's method.

Only a thorough comparative analysis of both Bacon's and Kant's positions can reveal the true nature of this similarity and show whether the two may even coexist in Bohr's account. Such analysis must take into account, however, that Bacon treats intuitions as biases to be minimized, while Kant sees them as a constitutive transcendental aspect of perception. The reason for this discrepancy may be only their different philosophical goals, methodological in Bacon's case and epistemological in Kant's (the former primarily motivating Bohr's work). We cannot turn here to a comparison of their respective notions of intuitions and the role of intellect each implies, or to related important issues. But it is worth emphasizing that analysis of Bohr's work from either perspective seems to point to an underlying two-stage methodological and epistemic framework, the understanding of which may be critical for our understanding of Bohr's work and perhaps the experimental method in general.[34] It should be also emphasized that even authors who trace Kantian influences in Bohr's work admit that Bohr did not accept Kant's apriorism. Perhaps the “new epistemological framework” that goes beyond the naïve empiricism and Kantian foundationalism that Chevalley (1994) refers to, emerged long ago with the emergence of the experimentalist method—the main elements of which Bacon initially grasped and Bohr developed further.

Another important question is whether Bohr was hegemonic in pursuing his complementarity. Metaphorically speaking, was he a Stalin or a Gandhi of physics? If one remembers that Bohr pursued a method, not a metaphysical view, the answer to this question seems obvious, given the inter-subjective core of his method of socialized-science. One might wonder, however, whether Bohr was hegemonic in pursuing a synthetic approach at any cost. The answer is yes, if we believe that the supremacy of experimental results over “argumentation” is a mark of hegemonic thinking in science. But one must remember that this kind of method allows the opposite method, the “argumentative” one favored by Schrödinger, to flourish and significantly contribute, while this would not be the case if the situation was reversed. And as I have hinted herein, this is where Bohr's method improves upon the method as initially explicated by Bacon—who did not fully understand the important role of the “argumentative” aspect.

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[34] Another suggestion which has been worked out in the same literature is that Bohr's insistence that we cannot make judgments concerning quantum states the same way that we do with respect to classical physics is akin to Kant's idea of the so-called conceptual containment, i.e. acknowledgement of the conditions of the validity of an objective judgement. The claim is that Kant and Bohr both argue that one can pass judgements on observables only under restriction that they are limited to possible experiences—i.e. that they concern *phenomena* rather than *noumena*. Yet, if we take a closer look at relevant passages in Bohr, some of which we have cited, we notice that conditions and limitations he is talking about, as much as those that Bacon insists on, concern the experimental apparatus, setup, and circumstances, rather than any abstract principled ways of delineating descriptions. Again, it is conceivable that Kant's account may turn out to be a satisfying epistemological elaboration of Bacon's methodological reflections and Bohr's thinking about quantum phenomena along the same lines.

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