The Role of Cognitive Values in the Shaping of Scientific Rationality

Jan Faye
University of Copenhagen, Denmark
faye@hum.ku.dk

Introduction

It is not so long ago that philosophers and scientists thought of science as an objective and value-free enterprise. But since the heyday of positivism, it has become obvious that values, norms, and standards have an indispensable role to play in science. You may even say that these values are the real issues of the philosophy of science. Whatever they are, these values constrain science at an ontological, a cognitive, a methodological, and a semantic level for the purpose of making science a rational pursuit of knowledge. Philosophy of science is in place when one discusses what makes science possible both as a theoretical and a practical discipline.

It is useful to distinguish between the external and internal values of science. On the one hand, the external values are somehow imposed on science from the outside in the sense that they are not inherent in the scientific practise or necessarily for science to be a rational enterprise. They are the demands that society puts on science that its results should be publicly relevant and technologically useful and be to the benefit of mankind. On the other hand, the internal values are immanently situated in the scientific practise and discourse. Scientists take them for granted as they carry on with their research because these values shape the rationality of science. But it is also clear that external values to a certain extent and in certain fields form some of the internal values. Medicine is a typical example of a scientific practise where internal values and external values merge into the same goal.

Some philosophers, e.g. social constructivists, may argue that there are no internal values of science. Values are always of social origin and determined by social demands, and it does not make sense to distinguish between values imposed from outside and values imposed from within. I find this an unreasonable claim. Indeed, values of science are formed and supported in part by individuals, but they are also upheld by the scientific community where they, together with particular theories, form the shared basis of a group of researchers. We therefore see that individual scientists may disagree about which values one ought to sustain. A scientist may diverge from the majority of colleagues concerning some cognitive values, or one group of scientists may deviate from another with respect to their norms and standards. Cognitive values are indirectly established in students through learning, training, and tradition, as they labour to grasp the factual content of the scientific theories. Norms and standards are tacitly presupposed most of the time. They constitute an intimate part of the scientist's rationality, and when scientists disagree, they very seldom realize that it may not be about factual matter, as they believe, but that it is these tacit values which are at stake. This does not mean, however, that these values are arbitrary and that one cannot give reasonable arguments for their constitutive role of scientific rationality.

The kinds of cognitive values of science

In the scientific practise we find all sorts of internal values that both guide and constrain our actions and reasoning. Some of them are important in specifying the aims of science; others are significant in stating the methodological prescriptions that may allow scientists to pursue those aims in the most rational fashion. The goals of science set the standards towards which the scientific practise should aim its activities. We do science with the purpose of acquiring new kinds of propositional knowledge, and therefore the values that guide science towards this aim are those we associate with calling something knowledge.

Truth is often taken as the definitive goal of science because true beliefs are what partly characterise propositional knowledge. Science goes with truth. If scientific statements were not true, they would be worthless as expressions of knowledge and as guidance to technological successes. Thus truth is considered to be the main *epistemic value of science*. This is also how most scientists look upon theories of their field. They believe that theories express what they and their colleagues take to be true about the objective world.

As long as we are talking about ordinary knowledge, it seems to be correct to say that we possess true beliefs and that these constitute the aim of our cognitive activities. But we know that some philosophers have expressed doubt about truth as a totally indispensable standard of scientific inquiry. An epistemic anti-realist, like Bas van Fraassen, has replaced the notion of truth with that of empirical adequacy. He argues that our epistemic commitments go with observational support and not with truth.[1] Scientific theories may or may not be true, what is important is that they are empirically adequate in the sense that they are true with respect to observation. Instrumentalists go even further in their denial of truth as an indispensable epistemic value. For them it is enough if theories are able to organize our observations in a coherent and thought-economical way in order to for us to make empirical predictions. In fact instrumentalist may deny that theories can be ascribed any form of truth. Thus, simplicity, thought-economics and predictability becomes the most important epistemic standards.

In the standard theory of knowledge, it is also a requirement that true beliefs should be justified, i.e., that they can be said to be rationally held. But where a belief may be true by chance, and therefore in addition has to be justified in order to count as knowledge, a belief is empirically adequate only if it is justified with respect to our observation. The claim that a belief must be justified expresses a *methodological value* and the fulfilment of the standards of justification is what makes our beliefs justified. Moreover, any justificatory procedure normally counts as a reliable method in the sense that by following it there is a high chance of getting closer to truth than to falsehood. So being a method of science the procedure must fulfil some standard of reliability, and a scientist would act epistemically responsible in cases where he pursues truth in accordance with these standards. We can just think of the requirements of variation, control, and accuracy posed on the collection of data.

It is well-known that Thomas S. Kuhn questioned the traditional notion of scientific rationality by saying that methodological values and standards change when the scientific community discards a paradigm and replaces it with another. Moreover, Kuhn believed that alternative paradigms are incommensurable. Many philosophers have therefore accused him of denying scientific rationality. Indeed, it is correct that Kuhn rejected that truth could have any important role to play as a guide to scientific rationality and the choice of theories. In this he seems to have had an attitude to empirical adequacy much like van Fraassen's (before the latter gave the concept real consideration.) A scientific theory or a paradigm must agree with observations, but apart from this it does not have to fulfil the epistemic value of being true. Rather a paradigm should deliver material for normal science and puzzle-solving.

Although Kuhn believed that scientific standards are paradigm-dependent, he nevertheless suggested that there might be some methodological values which are paradigm-independent and can be used as guidance for theory choice.[2] Such transparadigmatic values are accuracy, consistency, perceptiveness, simplicity, and fruitfulness. 1) The consequences of a theory have to fit closely to those

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[1] Van Fraassen (1980), p.

[2] See Kuhn's paper "Objectivity, Value Judgment, and Theory Choice" in his (1977)

observations and experiments which the theory is supposed to describe. 2) A theory has to be internally consistent but also externally consistent with respect to other relevant theories. 3) The consequences of a theory have to reach much further than to those observations according to which it was posed to describe at the first hand. 4) A theory must be simple and be able to unite phenomena which would appear otherwise separated. And 5) a theory must be able to predict new phenomena which nobody knew anything about previously. But Kuhn also emphasized that neither the importance nor the weight of them was something agreed on among scientists because different paradigms may satisfy each differently and in various degrees.

Sometimes, however, other kinds of epistemic goals are being substituted for truth, empirical adequacy or simplicity. But there is no reason to think that truth or empirical adequacy as epistemic values can be replaced by or reduced to any other kind of value. Truth is not reducible to, say, political correctness, nor do political ideals reduce to true beliefs. *Political values* are not part of science at all if one takes truth to be the ultimate goal of science. Nevertheless, political norms are sometimes claimed to play an explicit function in the formulation of the aim of science. Marxists, for instance, think that the purpose of economics and sociology are to save the working class from poverty and economical exploitation. In this case the truth of a theory is twisted by political purposes. Most modern societies also spend a lot of money on science. In return these societies want science to pursue goals which help improve the need and the health of their citizens. But these external demands are clearly not part of the scientific practice itself.

Ethical values may also take part in the formulation of the aims of science. Medicine is a clear example of a science in which ethical values play a significant role in selecting what is considered to be acceptable research projects. The goal of pharmaceutical research is not merely truth but also the curing of illnesses and a general improvement of people's health. You can even say that the practical success of bringing a disease to an end is often more important in medicine than having a correct theory concerning the aetiology of disease itself. Indeed, truth and recovery very often go hand in hand, but one should not be misled by this fact to think that the latter can be fulfilled only in the case of the former.

Among scientists one sometimes finds an explicit claim that truth and beauty go hand in hand. An elegant mathematical theory must be true due to the beauty of nature, and since mathematics is nature's own language, it must reflect all the beauty we find in nature. Scientists like Albert Einstein, Herman Weyl, Paul Dirac and Steven Weinberg have all expressed strong belief in a close connection between scientific truth and aesthetics. Dirac, for instance, maintained that "a theory with mathematical beauty is more likely to be correct than an ugly one that fits some experimental data."[3] More scientists would say that *aesthetic values* guide their research in the sense that they prefer an elegant theory rather than a clumsy one. This indicates that the aesthetic values of a theory are taken as evidence of its being true even though truth and beauty may not be considered identical. Such records should indeed be taken serious. Aesthetic properties may, as a matter of fact, sometimes be taken into account in both theory formation and theory choice. But it only makes sense if one has to select between, perhaps temporarily, empirically underdetermined alternatives. A scientist may prefer his own theory on aesthetic grounds in case its empirical success is identical with alternative theories with a similar or identical success. Having worked very hard with his own theory, he would try to vindicate it for other reasons.

I agree, however, with McAllister's conclusion when he denies that there is any connection between *scientific success* and the use of aesthetic criteria. "Contrary to Dirac, Einstein and others, I see little evidence that aesthetic properties correlated with high degrees of empirical adequacy in theories have yet been identified in any branch of science. If they had, the empirical benefit of choosing theories on particular aesthetic criteria would be far more obvious."[4] Nor is there any argument that connects *truth* and beauty. What is considered to be the aesthetic criteria varies according to the ruling taste. At one point in history, philosophers and physicists considered the circular movement to be the true and perfect motion; thus the circle described the movement of the bodies of heaven. Nevertheless, Kepler had to give up on this idea because of new empirical evidence. So the characteristics which scientists at one time regard as belonging to the aesthetic cannon are rejected at another time. The beauties are never always beauty and therefore never necessarily true. As McAllister points out, scientists' aesthetic taste is inductively induced through their professional training and the taste of the scientific community varies depending on its theories and experience. Like art, like science. What is taken to be aesthetically attractive differs not only through different epochs but also between different scientific disciplines. In contrast, truth and empirical adequacy are cognitive norms that are independent of the taste of beauty. They last longer and make up the rational basis of the scientific practise.

Scientists, indeed, do not aim at truth as such. They need theories to be able to grasp what is true. Truth must be explicated in terms of theories to become a target of empirical investigation. A scientific theory yields the explicit explanation of what is taken to be the truth. But every scientific explanation rests on theoretical interpretations, and theoretical interpretations do not provide the "natural" and only understanding of our experience. Hence theoretical claims go beyond what can be empirically

[3] Kragh, 1990, p. 284
[4] McAllister (1996), p. 102.

settled. Any theoretical interpretation must take its departure in a set of *metaphysical values* that shape our global world-view. Such values are assumptions which state how the foundation of a scientific theory ought to be and which ontological requirements it must meet. They are tacitly present in the given research practise in the sense that they constraint the scientific theories without their validity being discussed. They form the ultimate basis of what a scientist would regard as a possible theory. Values of this sort are associated with commitments to realism and objectivity; in particular, how reality should be understood. We may feel committed to mechanical descriptions, physicalistic descriptions, naturalistic descriptions, or some other form of description. For instance, a physicist may prefer deterministic theories instead of indeterministic theories, mechanical theories instead of non-mechanical theories, or action-at-a-distance theories instead of field theories.

Methodological prescriptions

The Danish physicist Niels Bohr made important contributions to the development of atomic physics, and later he and Einstein were involved in discussions about the interpretation of quantum phenomena. Here I shall show that much of this debate was a debate about values and not about facts. As we shall see, Bohr believed that quantum mechanics was methodologically sound because it was developed based on what he regarded as acceptable values and he therefore thought that determinism had to yield for indeterminism.

Bohr presented his core model of the atomic structure in 1913. The model could explain the spectroscopic lines of the hydrogen atom and ascribed to the atom some strange non-classical features due to Planck's quantum of action. The discrete atomic spectrum is caused by electrons jumping between well-defined stationary energy states, at the same time it was impossible for these electrons to occupy the empty space between the stationary states. The model is called semi-classical since it still presented stationary states as classical orbits around the atomic nucleus.

Bohr's model gave a successful description of the spectral properties of hydrogen atom but it was incomplete when applied on atoms of any higher atomic number. Bohr eventually realized that his model, and Sommerfeld's modification of it, was only a first step in the direction of a coherent theory, and that a future theory possibly would require an even more radical deviation from classical notions than his earlier ideas.[5] Helping him in the search for such a new theory, he thought that it was necessary to find some general methodological requirements which might serve him or other physicists as guidance in the formulation of a better theory. He advocated what he called the correspondence rule according to which the prediction of the behaviour of a free electron based on classical theory and on the new theory should be numerically the same. In the beginning Bohr thought of the correspondence argument as a purely formal argument, which requested only the existence of syntactic or structural features between the two theories, but already in the 1920s he realized that a new theory should be subjected to intelligibility or semantic constraints as well. Before we can put questions to quantum phenomena and provide answers, there has to be many things about common sense, the experimental apparatus, and physical knowledge as such, which we cannot doubt in the actual situation of inquiry. A theory must be meaningful and relevant with respect to what we know and in general assume to be true. As a consequence, he believed that the use of classical concepts, developed by classical physics to describe our experience and experimental practise, was necessary for any appropriate understanding of quantum phenomena.

In her recent book, the Italian philosopher and historian of science Michela Massimi (2005) gives us a painstaking description of the development from the success of the Bohr-Sommerfeld model of atomic structure to its failure to cope with the anomalous Zeeman Effect and many other spectroscopic phenomena. Massimi follows their struggles to understand the spectroscopic data within the atomic core model. It was not until Pauli suggested that some of these data could be interpreted such that in an atom two electrons with the same quantum numbers were impossible that it became obvious that the core model was in severe crisis. The immediate consequence was the abandonment of the atomic core model. Instead a young American physicist Ralph Kronig considers s, one of the two angular momenta, l and $s = ½$, as an intrinsic angular momentum due to a rotation of the individual electron about its axis. This interpretation was first really accepted nine months later in 1925 when George Uhlenbeck and Samuel Goudsmit published a similar conclusion. But Pauli's proposal meant a lethal blow to the core model; but also "Bohr's correspondence principle was left out: how to reconcile the classical periodic motions presupposed by the correspondence principle with the classically non-describable *Zweideutigkeit* of the electron's angular momentum?"[6]

Although the exclusion rule and the introduction of spin broke with the attempt of explaining the structure of the basic elements along the lines of the correspondence argument (as Pauli pointed out in a letter to Bohr) Bohr continued to think of it as an important methodological principle in the attempt to establish a coherent quantum theory. In fact, he repeatedly expressed the opinion that Heisenberg's matrix mechanics came to light under the guidance of this very principle. In his Faraday Lectures from 1932, for instance, Bohr emphasizes: "A fundamental step towards the establishing of a proper *quantum mechanics* was taken in 1925 by Heisenberg who showed how to replace the ordinary kinematical concepts,

[5] Some of Bohr's reflections are mentioned in Faye (1991), pp. 113-119.
[6] Massimi (2005), p. 73.

in the spirit of the correspondence argument, by symbols referring to the elementary processes and the probability of their occurrence."[7] Bohr acknowledged, however, that the correspondence argument failed too in those cases where particular non-classical concepts have to be introduced into the description of atoms. But he still thought that the correspondence argument was indispensable for both structural and semantic reasons in constructing a proper quantum theory as a generalised theory from classical mechanics.

Indeed spin is a quantum property of the electrons which cannot be understood as a classical angular momentum. Needless to say, Bohr fully understood that. But he didn't think that this discovery ruled out the use of the correspondence rule as guidance to finding a satisfactory quantum theory. Allow me to give a lengthy quotation from Bohr's paper "The Causality Problem in Atomic Physics" (1938):

> Indeed, as adequate as the quantum postulates are in the phenomenological description of the atomic reactions, as indispensable are the basic concepts of mechanics and electrodynamics for the specification of atomic structures and for the definition of fundamental properties of the agencies with which they react. Far from being a temporary compromise in this dilemma, the recourse to essentially statistical considerations is our only conceivable means of arriving at a generalization of the customary way of description sufficiently wide to account for the features of individuality expressed by the quantum postulates and reducing to classical theory in the limiting case where all actions involved in the analysis of the phenomena are large compared with a single quantum. In the search for the formulation of such a generalization, our only guide has just been the so-called correspondence argument, which gives expression for the exigency of upholding the use of classical concepts to the largest possible extent compatible with the quantum postulates.[8]

This shows that, according to Bohr, quantum mechanics, as formulated by Heisenberg, was a rational generalization of classical mechanics when the quantum of action and the spin property were taken into account.

My purpose for bringing Bohr's statement to our attention is a further point which Massimi makes in her book. She accepts Kuhn's claim that there was a "revolutionary transition from the old quantum theory to the new quantum theory around 1921-5."[9] This revolution came about as a result of a crisis of the old quantum theory between 1922 and 1925. Kuhn also thought that the old quantum theory could not be called a full-blown theory but rather a set of algorithms to solve problems and paradoxes. Massimi uses this revolutionary transition as an argument against Friedman's suggestion that a rational continuity of revolutionary transitions originated from a well-established fact that, at a later time, is elevated to the

[7] Bohr (1998), p. 48.
[8] Bohr (1998), p. 96.
[9] Massimi (2005), p. 73.

constitutive a priori principle of a new theory. Her reason is that in the case of the Pauli rule there was no such well-established fact, nor was the rule as a 'phenomenological' law elevated to the status of a constitutive principle of the new quantum mechanical framework. But what if there was no revolution between 1922 and 1925? If Kuhn and she are wrong, it seems that she has no argument against Friedman.

This depends very much on how we characterize a scientific revolution: How can an event of this sort be identified? Kuhn presented something like a definition of a scientific revolution. It is a historical change of incommensurable paradigms. The defining feature of a revolution in scientific thoughts, according to Kuhn, is discontinuities and gaps between these thoughts which make them incommensurable. In his *The Structure of Scientific Revolutions* the concept of incommensurability covered meaning variance, epistemic standards, and psychological attitudes. He therefore made the famous remark about a change of paradigm that the world does not change, but that the scientists afterwards work in a different world. Later he attempted to articulate incommensurability in terms of the untranslatability of lexical taxonomies. But Massimi does not have this definition of a scientific revolution at hand. Rather than reading lexical taxonomies as constitutive, as Kuhn did as a philosopher of science, she argues that it makes much more sense to understand lexical taxonomies as having a regulative task. As she says: "a mild Kantian reading of lexical taxonomies allows us to reformulate the new-world problem in a way that does not license incommensurability, and, on the other side, vindicates Kuhn's 'post-Darwinian Kantianism'"[10]I think her reading reconciles Kuhn as a philosopher with Kuhn as a historian, but at the same time deprives her from talking about scientific revolutions. Indeed, this may explain why Kuhn stuck to his concept of incommensurability.

Massimi's analysis of the conceptual changes along these lines squares well with the fact that there was no incommensurability between the atomic core model and the spin model, and therefore no revolution in Kuhn's sense. It seems as if Massimi has an unarticulated view on scientific revolutions. She continues to talk about revolutions, but she also talks about demonstrative induction in which non-classical concept are derived from some relevant theoretical assumptions of old quantum mechanics and spectroscopic anomalies: "The electron's *Zweideutigkeit* was not plucked out of the air as a bold conjecture. It rather followed from spectroscopic anomalies with the help of theoretical assumptions (a)-(f), i.e., it came out in a non-ampliative way from the interplay between the old quantum mechanics and anomalous phenomena."[11] A historiographic term like 'revolution' is not a natural kind term, so I wonder how this quotation reports a revolution. At least I imagine that the lexical taxonomy of historiography should not be interpreted as constitutive.

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[10] Massimi (2005), p. 97.
[11] Massimi (2005), p. 106.

The historiographic term 'revolution' is taken from political history. Its present meaning signifies a change of power which happens against the constitution through the violent actions on the part of the people. Such events are often very rapid and short termed. But we also think of revolution more broadly when we talk about the Glorious Revolution, the Industrial Revolution and a scientific revolution, and Lenin even thought of the Permanent Revolution. So it cannot be the length of time that defines a revolution. But how do we then distinguish a revolution from an evolution? Just how normative these notions are, is clearly testified by the fact that Kuhn, the historian of science, didn't see Copernicus' introduction of the heliocentric system as a revolution where other historians have considered it to be the scientific revolution par excellence. The important thing is, if we think of a scientific revolution analogous to a political revolution, that one should require that there be a total replacement of a whole conceptual system rather than a mere generalization of a conceptual system in terms of change and addition of concepts in the direction of increased richness and precision. This, I think, leaves us with very few, if any, scientific revolutions.

Moreover, I think that Massimi's reconstruction of the development of the atomic core model into the spin model as a demonstrative induction comes close to how the major partisans themselves realized the development during those years. Her reasons for dismissing the correspondence principle are not convincing. Being the most dominating figure from the creation of the first atomic theory in 1913 to the interpretation of the second theory in 1927 Bohr did not consider the transition from the Bohr-Sommerfeld model to the Heisenberg matrix mechanics as a revolution. His methodological prescriptions in terms of correspondence rule were very different from what Kuhn's retrospectively observed as a philosopher. Where Kuhn saw revolutions and incommensurability, Bohr (and Heisenberg) looked for rational generalizations and commensurability. Where Kuhn saw himself as a post-Darwinian Kantian, we may characterize Bohr as a Darwinian Kantian. Were Bohr and his younger disciple wrong? I think that Friedman's view is closer to Bohr's; it also seems more evolutionary than revolutionary. Planck's discovery of the discontinuity of the black body spectrum was considered by Bohr as a well-established empirical fact which was elevated to a constitutive (a priori) principle in his model of the atom. This semi-classical model applied to the hydrogen atom, and not much else. From then on, a continuous struggle began of enlarging the model to deal with atoms with higher numbers and to reach a proper theory which could explain the dynamics of elementary particles. One may therefore argue that the period between 1913 and 1925 was one long process of conceptual adaptations which involved a collection of the most brilliant physicists at that time. This process only partly ended in 1925 when Heisenberg established a proper *theory* of quantum mechanics in which the quantum of action still formed the constitutive a priori principle.

Thus I believe it is correct to say that the correspondence rule with its syntactic and semantic constraints formed a major role of shaping the rationality of theory construction of quantum mechanics, and because the principle of correspondence stayed intact as a methodological prescription during the critical years there was no scientific revolution when atomic physics moved from Bohr's atomic core model to Heisenberg's new quantum mechanics.

The Bohr-Einstein debate

It is well-known that Albert Einstein was not the least happy with quantum mechanics, and that he had some intense discussions with Bohr during the 1930s. In my opinion the discussions arose because these prominent physicists were divided with respect to their fundamental cognitive and metaphysical values. They did not themselves realize the foundation of their disagreement; instead of having an open methodological discussion about cognitive and metaphysical values, they buried their intellectual conflicts in physical considerations which might have helped them to understand each opinion but instead hid the real issue.

Gerald Holton was among the first who focused explicitly on individual presuppositions in the process of theory formation; he calls them themata and sets up a theoretical framework to analyse them.[12] Themata are concerned with metaphysical, epistemological, methodological, ethical or aesthetic issues. Most of these issues are clearly different kind of values as they have been described above. In his study of Einstein's thematic guidelines to theory construction, Holton isolates a number of motivating issues on which the investigator makes some presuppositions or takes a stand: formal rather than materialistic explanations; unity and unification, logical parsimony and necessity; symmetry; simplicity; causality and determinism; completeness, continuity; and constancy and invariance. In addition one could also mention: value-definiteness; locality; separability; and the objectivity of theoretical descriptions. Other scientists may hold opposing themata. Niels Bohr did not hesitate to accept theories motivated by discontinuity, indeterminacy, value-indefiniteness, superposition, non-separability and entanglement, and intrinsic probabilities. Moreover, Bohr preferred physicalistic rather than formal explanation; he regarded the classical concepts to be essential for any unambiguous communication of experimental results in physics, and he denied objectivity in the sense that the theoretical description represents the state of the system as it really is.[13]

As mentioned before, Bohr developed his sets of norms and standards as a reaction to the discovery of the quantum of action when he recognized that this feature, in the same way as the

[12] Holton (1973).
[13] See Faye (1991), pp. 197-210.

invariance of the velocity of light with respect to the special theory of relativity, should be given a constitutive role in a new theory of atomic physics. But because the introduction of the quantum of action as the foundation of a future theory led to radical deviations from the ontology of classical mechanics, the physicists needed some methodological standards which could guide them safely through to the new theory. This was the prescriptions of correspondence. The theory which came out in the other end was nonclassical, and it did not fit easily with the norms of classical physicists. Bohr accepted that quantum mechanics did not allow ascription of conjugate variables to an atomic object at one and the same time because it did make sense to attribute a property to an atomic object which could not in principle be measured. As he once said:

> The emphasis on permanent recordings under well-defined experimental conditions as the basis for a consistent interpretation of the quantal formalism corresponds to the presupposition, implicit in the classical account, that every step of the causal sequence of events in principle allows verification.[14]

Hence Bohr looked at the quantum formalism to be complete and consistent; he also believed that it did not give us a true picture of reality.

Some philosophers, myself included, see Bohr as a realist about systems but an antirealist about states. In many places Bohr refers to the mathematical formalism of quantum mechanics as the mathematical *symbolism*, and he talks about *symbolic operators*. Concerning the aim of science Bohr says: "In our description of nature the purpose is not to disclose the real essence of phenomena, but only to track down as far as possible relations between the manifold aspects of our experience."[15] Furthermore he stated that "within the frame of the quantum mechanical formalism, according to which no well defined use of the concept of "state" can be made as referring to the object separate from the body with which it has been in contact, until *the external conditions involved in the definition of this concept* are unambiguously fixed by a further suitable control of the auxiliary body"[16]– in other words, it makes no sense to say that a quantum system has a definite kinematical or dynamical state prior to any measurement. Hence we can only ascribe a certain state to a system given those circumstances where we epistemically have access to their realization. Based one these and other considerations, I still think it makes good sense to argue that Bohr was a realist with respect to atomic systems but antirealist with respect to their states.

Thus, Bohr was an entity realist who believed that the aim of science is not to provide true theories which explain quantum phenomena, but theories which are empirically adequate and useful for

[14] Bohr (1963), p. 6.
[15] Bohr (1929[1985]), p. 18.
[16] Bohr (1938b[1998]), p 102, my emphasis.

regimenting and describing our experimental experience. The value of science is unambiguous communication about the experimental phenomena in question. Bohr had no scruples of accepting a description of physical objects which contain discontinuity, indeterminism, value-indefiniteness and entanglement if this was what it takes to set up such an unambiguous communication.

Einstein came to quantum mechanics with a very different experience. He had discovered the theories of relativity in 1905 and 1915, and in the following years he was constantly working on a uniting theory of gravitational and electromagnetic fields. Einstein really believed that the aim of science was to give us true theories of physical nature. Although he contributed as earlier as 1905 to the atomic physics with the explanation of the photoelectric effect, the discontinuity of this description was something he never gave serious thought as a formative principle. Because the object of relativity was continuous fields, Einstein automatically took the notion of continuity in space and time to be the norm in terms of which any coherent theory of quantum should be constructed. Therefore he wanted to show that quantum mechanics was only a limiting case of a theory which remains to be discovered but which was based on a field concept.[17] Before then he had hoped to prove that quantum mechanical formalism was inconsistent. This happened at the Solvay conference in 1927 and 1930 in discussion with Bohr. In 1930 Bohr made his famous touché when he used the theory of relativity against Einstein's objections. But when Einstein finally agreed to the fact that quantum mechanics was consistent, he then attempted to show that the theory was incomplete. This happened with the publication of the paper of Einstein, Podolsky, and Rosen.

Besides consistency, completeness is another methodological value which we would like our theories to hold because this means that they do not leave anything out that might have led to new knowledge. In their common paper Einstein, Podolsky, and Rosen argued that a physical theory is complete only if "every element of the physical reality has a counterpart in the physical theory" and we may find out whether it has "if, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity."[18] They then constructed a thought experiment by which they hoped to show that it was possible in principle to find such states. Today, we also know that they didn't succeed as demonstrated by the experimental attempts to prove Bell's theorems.

But what was Bohr's reaction? He realized, perhaps surprisingly, in his contribution to the volume on Einstein in *The Library of Living Philosophers* that it was a question of rationality. He said: "The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a *rational* account of physical phenomena of the type with which we are concerned in

[17] See Pais (1982), p. 460 ff.
[18] Einstein, Podolsky, and Rosen (1935)

quantum mechanics."[19] Shaping a new form of physical rationality required a new criterion of reality. Bohr regarded the causal account of the physical phenomena to be the rational account of the same phenomena. This account constitutes the criterion of reality. But since the application of that criterion is severely restricted because of "the necessity of a final renunciation of the classical *ideal* of causality," it follows that we are forced to accept "a radical revision of our attitude towards the problem of physical reality." [20]

Both Bohr and Einstein shared the same Kantian idea that the subsumption of physical phenomena under as causal description is what makes nature intelligible. But Einstein was fond of the classical ideal of causality according to which physical processes take place in a continuous manner between different states with well-defined values. So he believed that a physical description of a physical system which obeys the classical ideal of causality is objective in the sense that the description represents the system as it really is. Bohr, however, did not feel obliged to sanction any of these commitments. Based on his idea about complementary description, he gave up the idea that a quantum system is in a definite dynamic state except when measured. Therefore, he believed that quantum mechanical formalism does not represent the truth, the real truth, and nothing but the truth. Science should pursue something more humanely important – unambiguous description.

Conclusion

Today we have to recognize that science is saturated by epistemic values, methodological prescriptions and metaphysical principles in order to make it intelligible, rational, and objective. The rationality of science is not given by God. It is installed by us in the form of an epistemic and a methodological obligation towards the treatment of beliefs and the possession of knowledge. Adjusted to our cognitive faculties and constantly imposed on our belief-processing system we are entitled to hold that these cognitive norms guarantee that science is rational. The norms are not eternal but changeable depending on the context of the scientific practise. More often than not they are invisible for the working scientists, since they form an integral part of the scientific enterprise. It is only as long as we dissect the scientific practise of belief acquisition that we may be able to discover their role. I think, however, that a good place to look for them is in the rise of quantum mechanics and in the debate between Bohr and Einstein on its interpretation, not because similar cognitive values are not shaping scientific rationality elsewhere, but because they surface in the debate whenever a new revolutionary paradigm is about to take over the scene. In some sense it makes science less sacrosanct. However, it also makes science more interesting.

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[19] Faye (1991), p. 178, my emphasis.
[20] *Ibid.*

References

Bohr, N. (1985) *Atomic Theory and the Description of Nature. The Philosophical Writings of Niels Bohr*. Vol. 1. Woodbridge, Conn.: Ox Bow Press.

Bohr, N. (1963), *Essays 1958-1962 on Atomic Physics and Human Knowledge*. London: J. Wiley & Sons.

Bohr, N. (1998), *Causality and Complementarity. The Philosophical Writings of Niels Bohr*. Vol. 4. Edited by Jan Faye and Henry Folse. Woodbridge, Conn.: Ox Bow Press.

Einstein, A., Podolsky, B., and Rosen A. (1935) "Can Quantum-Mechanical Description of Physical Theory be Considered Complete" in *Physical Review*, **47**, 777-80.

Faye, J. (1991), *Niels Bohr: His Heritage and Legacy. An Antirealist View on Quantum Mechanics*. Dordrecht: Kluwer Academic Publisher.

Faye, J. (2003), *Rethinking Science*. Ashgate

Holton, G. (1973), *Thematic Origins of Scientific thought: Kepler to Einstein*. Cambridge: Harvard University Press. Rev. Ed. 1988.

Kragh, H.S. (1990), *Dirac: A Scientific Biography*. Cambridge: Cambridge University Press.

Kuhn, T.S. (1977), *The Essential Tension. Selected Studies in Scientific tradition and Change*. Chicago: University of Chicago Press.

Massimi, M. (2005) *Pauli's Exclusion Principle. The Origin and Validation of a Scientific Principle*. Cambridge: Cambridge University Press.

McAllister, J.M. (1996) *Beauty and Revolutions in Science*. Itacha, N.Y. Cornell University Press.

Pais, A., (1982) *Subtle is the Lord. The Science and the Life of Albert Einstein*. Oxford: Oxford University Press.

Van Fraassen, B. (1980) *The Scientific Image*. Oxford: Claredon Press.