CAN A HISTORIAN OF SCIENCE BE A SCIENTIFIC REALIST?

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Abstract: In this paper I want to address the problems that the historical development of science poses for a realist and to discuss whether a realist construal of scientific activity is conducive to historiographical practice. My aim is to show that the realism problem is relevant to historiography and that the position one adopts with respect to this problem entails a particular historiographical strategy. I will argue that for historiographical purposes an agnostic attitude with respect to scientific theories and unobservable entities is the most appropriate.

1. Introduction. One reads repeatedly in the philosophical literature that scientists, and especially experimentalists, usually adopt a realist perspective towards the entities and processes they investigate (e.g., Hacking (1983, 1984), Franklin (1996)). In Alan Franklin's aptly chosen words, "there are no anti−realists in the laboratory" (Franklin 1996, 131). If active participation in scientific practice forces one to become a realist, the opposite seems to be the result of studying the historical development of science. Both historically−minded philosophers and professional historians, to the extent they address philosophical issues, usually adopt an anti−realist perspective. To paraphrase Franklin, there are few realists in the library or in the archives. In this paper I want to address the problems that the historical development of science poses for a realist and to discuss whether a realist construal of scientific activity is conducive to historiographical practice. That history of science is relevant to the realism problem has been widely recognized (see, e.g., Newton−Smith 1990). Here my aim is different, namely to show that the realism problem is relevant to historiography. In particular, I will attempt to show that a realist position entails a particular historiographical strategy, which faces severe problems. Furthermore, I will argue that for historiographical purposes one should adopt an agnostic attitude with respect to scientific theories and unobservable entities.

2. Scientific realism: how is history of science relevant to the debate? The debate on scientific realism has taken place along several lines; the two most salient are as follows. The first concerns the appropriate epistemic attitude towards contemporary scientific theories. Are they, at least approximately, true descriptions of reality or merely convenient schemes for saving the phenomena? The second and equally important for my purposes concerns the grounds that we have for believing in the reality of the unobservable entities postulated by contemporary science (electrons, photons, fields, atoms, etc.). A realist position amounts to the belief that contemporary scientific theories are approximately true and that the entities postulated by contemporary science exist (Psillos 1999). However, even a cursory examination of the historical record suffices to undermine this position. Successful scientific theories of the past, that had been almost universally accepted (e.g. Newtonian mechanics, or classical electromagnetic theory), were eventually abandoned and replaced by other theories. An even more problematic historical fact, for the realist, is that several unobservable entities that had occupied a central place in the ontology of past science (e.g., phlogiston, caloric, ether) turned out to be fictitious. It is a small step to extrapolate from these aspects of the historical record and conclude that, in all probability, the scientific theories that we currently accept and the unobservable entities that comprise the contemporary ontology of science will have a similar fate. So, it should be evident that history of science plays a crucial role in the realism debate and, prima facie at least, gives rise to anti−realist intuitions.

3. The case for scientific realism: the verdict of history of science. Several arguments have been proposed to support a realist position. First, realism has been portrayed as the best explanation of the success of science. If our theories were not approximately true and if the entities that they postulate did not exist, then the impressive success of science, its enormous range of applications, and the power that it confers for controlling and altering our environment would be truly miraculous and would be left without an explanation (Putnam 1975a, 73). This argument turns out to be problematic, when examined from a historical perspective. Science started having significant applications more than a hundred and fifty years ago (e.g., in the rising chemical and electrical industries) and those applications were based on theories that have been abandoned (e.g. on an ether−based electromagnetic theory). Thus, it is not at all clear that technological success is a straightforward indication of the truth of scientific theories or the existence of theoretical entities.

A related argument for realism has been advanced by Ian Hacking, whose position is based on a close examination of experimental practice. A satisfactory resolution of the problem of scientific realism would be possible, Hacking claims, only if we stopped being preoccupied with scientific theorizing and shifted instead our focus of analysis towards experimentation. Such a shift of emphasis would be enough to make us all realists with respect to some unobservable entities, but would not weaken our anti−realist convictions with respect to the theories which postulate those entities. This peculiar mix of realism about entities and anti−realism about theories follows from two central aspects of experimental practice. On the one hand, the manipulation of unobservable entities in the laboratory provides sufficient grounds for believing in their existence. On the other hand, the fact that experimentalists use, according to the purpose at hand, a variety of sometimes incompatible theoretical models of those entities generates strong doubts that any of those models accurately represents reality. All these models, however, have some aspects in common, namely a core of statements about the causal properties of the corresponding entities, properties which we have come to know by manipulating those entities in various experimental contexts. One can (should) be realist about this common core which, however, does not deserve to be called a ’theory’ (Hacking 1983, 1984).

Hacking’s entity realism can be summarized in his aptly chosen slogan: "If you spray [e.g.] electrons then they are real." (Hacking 1983, 24) Notwithstanding the charm of Hacking’s slogan, it fails to impress philosophers in the empiricist tradition. Van Fraassen, for instance, when asked to evaluate Hacking’s argument, responded in the following way: "If they are real then you spray them." (Personal communication) Van Fraassen does not imply, of course, that everything real can be sprayed. Rather, his point is that one can use the expression ’spraying of electrons’ (as the best available description of a given experimental situation) without committing oneself to believing in the existence of electrons. The coherence of his position stems from the possibility that a new theory, which would not include electrons in its ontology, could adequately account for the experimental situation which Hacking, following contemporary experimentalists, describes as ’spraying of electrons’.

An example that illustrates this possibility can be found in a well−known episode from the history of chemistry, the so−called Chemical Revolution.[1] Before the establishment of the oxygen theory of combustion, late eighteenth century chemists explained several chemical phenomena (e.g., the formation of acids) by postulating an entity called phlogiston. Furthermore, they could manipulate that entity, since they could transfer it to a substance. Georg Stahl, for instance, had discovered that "vitriolic acid [sulfuric acid] can be converted to volatile sulfurous acid by transferring to it some of the inflammable principle [phlogiston]" (Holmes 1989, p. 100). After the Chemical Revolution and the concomitant disappearance of ’phlogiston’ from the chemical vocabulary, however, the process that had been previously described as ’transfer of phlogiston’ was redescribed in terms of a different entity named oxygen. It is similarly conceivable that the process that is now described as ’spraying of electrons’ might be redescribed in terms of an alternative theory based on a different ontology. Thus, Hacking has not provided a conclusive argument for the existence of electrons since he has not excluded the possibility of an alternative, empirically adequate theory whose ontology would not include electrons.

Another criticism has been raised against Hacking’s claim that manipulability is a sufficient criterion for establishing the existence of an unobservable entity (Morrison 1990). An example from the history of physics will illustrate the inadequacy of manipulability as a sufficient criterion for scientific realism.[2] When J. J. Thomson was experimenting with cathode rays he was able to manipulate them in various ways. For example, he could deflect them by means of electrostatic and magnetic fields. It is well established today that cathode rays consist of electrons. Now given that Thomson manipulated cathode rays and that cathode rays are streams of electrons, it follows that Thomson manipulated electrons. However, at the time the existence of electrons was still controversial and no choice had yet been made between a model of cathode rays which portrayed them as waves in an all−pervading ether and an alternative model which represented them as streams of particles (see Arabatzis 1996). It is clear that in that context the manipulability of cathode rays would not by itself establish the validity of the latter model and thus the reality of electrons.

Thus, within actual scientific practice manipulability did not (and should not have) provide(d) adequate reasons for belief in the entities that were supposedly manipulated. In the previous example the act of manipulation could be described without even mentioning the entities that, according to present−day physics, were manipulated. One could describe the experiment in terms of cathode rays as opposed to electrons. Moreover, an antirealist could give an even less theory−laden description, by avoiding the term ’cathode rays’ and using instead the phenomenological expression ’spot on a phosphorescent screen’. The only thing that we know, the antirealist would argue, is that by the application of electric and magnetic fields Thomson could move a spot on a phosphorescent screen. To the extent that an act of manipulation can be described without mentioning the unobservable entity that is (supposedly) manipulated, this act does not by itself imply the existence of the entity in question. Thus, given that experiments can be (re)described in phenomenological terms, manipulability cannot be employed, to the satisfaction of an antirealist, for existential inferences. Whereas for Hacking manipulability justifies existence claims, for the antirealist it is the other way around: It is the belief in the existence of, e.g., electrons, prior to the act of manipulation, that allows us to interpret that act as a manipulation of electrons (as opposed to something else) (cf. Feyerabend 1960, 64). To paraphrase van Fraassen's reversal of Hacking's slogan: If they exist then we manipulate them.

Another problematic aspect of Hacking's realist position is associated with his 'home truths'(low−level generalizations) about, e.g., electrons that we supposedly know independently of any high−level theory. Hacking doesn't specify what kind of electron properties he has in mind but one could guess that his 'home truths' would include well−known causal properties of electrons, like their charge, mass and spin, which enable us to manipulate them in order to investigate other less well−known aspects of nature.[3] It is difficult to see, however, how one could isolate those properties from the background theory in which they are embedded. To use a concrete example, it is difficult to obtain an understanding of 'charge' independently of any high−level theory, especially in view of the fact that 'charge' meant different things for different scientists. Within Maxwellian electrodynamics, for instance, charge was not an independent substance but merely an epiphenomenon of the electromagnetic field. In Lorentz's electromagnetic theory on the other hand it was an independent entity which interacted with, but could not be reduced to, the electromagnetic field.

Hacking could argue that all those different and incompatible conceptions of charge had certain aspects in common, i.e., the causal properties that we have associated with electric charge all along (e.g., the ability of charges to attract, or repel each other). His attempt to isolate the causal properties of electrons from any background theory was motivated by the plurality of incompatible models about electrons. However, it turns out that the causal properties themselves have been interpreted via several, incompatible theories. He could, of course, search again for a common core shared by those theories, but it is not clear that there would be an ending point to this process.

So far, we have seen that two central arguments for scientific realism, the no−miracle argument from the success of science and the emphasis on manipulability as a sufficient proof of the existence of a theoretical entity, are undermined by the historical record. Let us now turn to arguments against scientific realism that capitalize on certain aspects of the history of science.

4. The case against scientific realism: arguments from history of science. In the early 1960s Kuhn and Feyerabend pointed out that the meaning of scientific terms changes over time. At that time, it seemed that the instability of scientific concepts had very negative implications for scientific realism. If the meanings of scientific terms which denote unobservable entities are subject to change, then to what extent are we justified in believing that the referents of these terms are unaffected by theoretical instability? To give a concrete example, what sense does it make to believe in the existence of electrons if the 'electrons' of Thomson, Lorentz, Bohr, Pauli, and Schroedinger differed significantly from each other? The problem is even worse when one accepts, as many philosophers did at the time, that the meaning of a term is specified by a set of conditions which are individually necessary and collectively sufficient for the correct application of that term. The slightest change in those conditions (meaning change) would imply that the term as previously used was vacuous, i.e., it referred to nothing at all. This problem is, in my opinion, the most serious challenge that realists face and the one with the most direct historiographical implications.

Realists have tried to respond to this challenge in two ways. First, by criticizing the meaning−variance thesis (in its most extreme version, incommensurability) and, second, by putting forward the so−called causal theory of reference, which sustains a central realist intuition, namely "that there are successive scientific theories about the same things: about heat, about electricity, about electrons, and so forth" (Putnam 1975b, 197). I have discussed elsewhere the potential of the latter proposal for allowing a realist interpretation of the historical development of science (see my 1995, 1998). Here I will examine the former line of response to the argument from meaning−variance. The focus of my discussion will be Kuhn's recent work, where he explicated and defended the well−known thesis of incommensurability (Kuhn 1982, 1989, 1993).

Two main criticisms had been raised against that thesis. The first rested on the presumption that incommensurability implies incomparability and precludes theory−choice on the basis of evidence. Given that the advocates of incommensurability had often talked about comparisons between incommensurable theories, their critics accused them of incoherence. The second criticism interpreted incommensurability as synonymous with untranslatability and declared it incompatible with historiographical practice. If the incommensurability thesis were true, then a translation of scientific theories that are incommensurable with their contemporary descendants to a modern scientific idiom would be impossible. But, as a matter of fact, historians (like Kuhn himself) have been engaged successfully in the translation of past scientific texts to equivalents that can be understood by a modern audience. Thus, the success of historiographical practice speaks against the validity of the incommensurability thesis.

Kuhn rejected both of these critiques. He denied an essential premise of the first one, namely that incommensurability implies incomparability. In geometry, from which the term ’incommensurability’ was borrowed, incommensurable magnitudes can be compared. The same is true of incommensurable theories. All that the incommensurability thesis maintains is that there is no common language in which the assertions of both theories can be expressed. As such, it does not preclude the comparison of incommensurable theories, a task that does not presuppose the existence of such a common language.

Furthermore, he denied a crucial premise of the second criticism, namely that the activity that historians are engaged in is equivalent to a process of translation. The historians’ task, according to Kuhn, is a hermeneutic one that aims at understanding and not translating past scientific texts. The impossibility of translation, that is indeed implied by the incommensurability thesis, does not imply the impossibility of understanding past scientific texts, an understanding that can be achieved by acquiring from scratch the language in which those texts were written.

Having defended the incommensurability thesis against the attacks that had been launched against it, he offered an explanation of the phenomenon of incommensurability and its immediate corollary, the impossibility of translation. Incommensurability, in his view, is a manifestation of underlying structural differences between two scientific languages. The language that one acquires by learning a scientific theory embodies a taxonomy of the world in natural kinds. The phenomenon of incommensurability arises when some of the natural kinds of two such taxonomic structures partially overlap. The overlap is not such that one natural kind subsumes another (as, for example, the natural kind ’metal’ subsumes ’gold’). Even though two incommensurable natural kinds have some members in common, the one has members that do not belong to the other and vice versa. If the overlap were complete (i.e., if the two taxonomies coincided) or if there was no overlap at all, one could express the statements of the two theories in a common language. In the former case this would be a trivial task; in the latter one could construct a language that would embody a taxonomy subsuming each of the taxonomic structures in question. When there is a partial overlap, however, no such language can be found. The natural−kind terms in the one structure cannot be translated into natural−kind terms in the other. Furthermore, any language that would embody one of the taxonomies could not be coherently extended so as to accommodate the other. Such an extension would require the addition of natural−kind terms whose referents would overlap with the referents of natural kind terms already in place. Thus, since it is a necessary condition for any system of classification that no entity belongs to more than one natural kinds,[4] the whole taxonomic structure would collapse (cf. Hacking 1993). For example, due to the partial overlap between the referents of the term ’element’, as employed in 18th century chemistry, and the same term, as employed in 20th century chemistry, no single−word translation of the 18th century term can be given in the language of contemporary chemistry. Moreover, one cannot enrich the modern chemical vocabulary with a new term, whose meaning would coincide with the meaning of the 18th century term ’element’, without rendering the language of contemporary chemistry incoherent.

In the paper that I have been discussing, Kuhn did not draw the implications of incommensurability for the problem of scientific realism. In a recently published essay, however, where he articulated his view of scientific development in terms of possible−world semantics, he made clear that incommensurability is incompatible with a realist position (Kuhn 1989). Every scientific theory, according to him, embodies a certain lexicon. That lexicon gives access to an infinite number of possible worlds (i.e., those situations that can be described in terms of the lexicon) and, at the same time, precludes access to other possible worlds. Normal science consists in the elimination of all those possible worlds permitted by the theory that do not conform with experience. If scientific development were exclusively of this kind, it would support the realist view that "the progress of science ... consist[s] in ever closer specification of a single world, the actual or real one." (Kuhn 1989, 24)

However, normal scientific development is sometimes interrupted by revolutionary periods which lead to the replacement of a theory by its incommensurable successor. Such a replacement entails the replacement of the older lexicon by a new one that gives access to a new set of possible worlds and, at the same time, prevents access to the possible worlds that one could access with the older lexicon. On this reading of the development of science, realism becomes an impossible doctrine, since the realist presupposition of a single world that our theories approximate is denied. Since the structure of the world is lexicon−dependent, when the lexicon changes the structure of the world changes with it. A realist who subscribed to this view would have two options: Either to suggest that the development of science has taken place against the background of a single lexicon; or to accept that scientific development has been characterized by changes of lexicon, but to deny that such changes will ever take place again. I presume that none of those options would be acceptable to Kuhn (Kuhn 1993).

Kuhn’s anti−realist view of scientific development has significant historiographical implications. If the language of the science of the past cannot be translated in modern terms, then the historian should avoid as much as possible contemporary vocabulary and modern categories when practicing his craft. Realists, on the other hand, are in a precarious historiographical predicament. Given their belief that (mature) science has developed against a stable ontological background, they are forced to portray past scientific terms and their modern descendants as referring to the same entities. For instance, when looking at the developments in late 19th century electromagnetic theory, the realist has to be able to reinterpret those developments in terms of contemporary ontological assumptions. First, one must be able to show that, say, Joseph Larmor’s theory of the electron, which by the way was a very successful theory, was approximately true, i.e., an approximation of the currently accepted theory of the electron. Second, one has to show that Larmor’s ’electron’ referred to the entity that we now designate with that term.

The challenge for the realist becomes more difficult when we start looking at the more remote scientific past. Should we interpret , say, Maxwell’s "ether" as referring to the same entity as its contemporary counterpart, the "electromagnetic field" (cf. Psillos 1999)? If Maxwell referred to the electromagnetic field, when he used the term ’ether’, then the historian could and, perhaps, should explain to his modern audience what

Maxwell was up to, by using contemporary knowledge. Could we really claim that Newton was referring to light−quanta when he used the term "light−particles"? Historians reject such attempts to interpret past scientific theories from a contemporary perspective and for good reason, since they are bound to lead to anachronisms and misinterpretations of past scientific practice. And this brings me to the historiographical implications of the realism debate, which will dictate the position that a practicing historian should adopt with respect to that debate.

5. Implications of the realism debate for historiography. As I already said realism can be considered a historiographical strategy whose aim would be to translate past scientific terms, statements etc. into the currently accepted scientific idiom and show how past scientific beliefs were approximate versions of contemporary ones.

Another historiographical implication of the realism debate concerns the historiography of scientific discovery. In order to identify an event or a process as the discovery of an unobservable entity, one needs a criterion (or a set of criteria) that would enable one to say that such a discovery has taken place. Several possible stances to the problem of what constitutes a discovery can be adopted. First, one might favor an anti−realist perspective, i.e., deny that we have any good reasons for believing in the existence of unobservable entities. From such a point of view discoveries of unobservables never take place. To quote from an eminent contemporary representative of this approach, "scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable." (Van Fraassen 1980, 5) This view of scientific activity is compatible with (but not necessary for) the approach to the issue of scientific discovery that I favor; but more on this later.

On the second stance, one might propose certain epistemological criteria whose satisfaction would provide adequate grounds for believing in the existence of a particular entity. From this point of view a discovery takes place when an individual or a group has managed to meet the required criteria. As an example consider Ian Hacking’s proposal that a belief in the reality of an, in principle, unobservable entity is justified to the extent that the entity in question can be manipulated. It follows then that an unobservable entity has been discovered only if a scientist has found a way to manipulate this entity. The adequacy of the proposed recipe for deciding when something qualifies as a genuine discovery depends on the adequacy of the epistemological criterion for what constitutes unobservable reality. Any difficulties that might plague the latter would cast doubt on the adequacy of the former. While, in principle, I do not find any difficulty with this approach to the issue of scientific discovery, this specific manifestation, i.e. Hacking's proposal, as I have already argued, leaves much to be desired.

If these two possibilities capture the philosopher's main stances towards the issue of scientific discovery, a third possibility captures the scientist's perspective. This perspective, which can also be considered a realist one, usually characterizes scientists who write retrospective accounts of scientific discoveries. The recipe in this case involves two steps. First, one identifies the most central aspects of the modern concept associated with the particular entity in question. Second, one looks at the historical record and tries to identify the scientist who first articulated those salient aspects and who, furthermore, provided an experimental demonstration of the validity of her conception. This person (or persons) would then qualify as the discoverer of the given entity. There is a serious difficulty, however, that undermines this approach. Any entity that forms part of the accepted ontology of contemporary science is endowed with several properties. The electron, for instance, has a given mass, a certain charge, an intrinsic magnetic disposition (spin), a dual nature (particle versus wave), and many other features. The question then arises how many properties must one have discovered in order to be granted the status of the discoverer of the entity in question. Until the proponents of this approach provide the necessary selection principle, a principle flexible enough to be applicable to a wide variety of discoveries, this approach will remain indefensible.

A final approach − and the one I favor − takes as central to the account the perspectives of the relevant historical actors and tries to remain as agnostic as possible with respect to the realism debate. The criterion that this approach recommends is the following: since it is the scientific community (or its most eminent representatives) which adjudicates discovery claims, an entity has been discovered only when consensus has been reached with respect to its reality. The main advantage of this criterion is that it enables the reconstruction of past scientific episodes without presupposing the resolution of pressing philosophical issues. For historiographical purposes, one does not have to decide whether the consensus reached by the scientific community is justifiable from a philosophical point of view. Furthermore, one need not worry whether the entity that was discovered (in the above weak sense) can be identified with its present counterpart.

Concluding Remarks: Agnosticism as the most appropriate historiographical approach. I have argued that history of science is relevant to the realism debate in two ways: First, an examination of history of science undermines certain realist arguments and supports an anti−realist outlook. More importantly, historiographical practice is best served by an agnostic attitude with respect to the existence of unobservable entities and the truth of scientific theories. Such an attitude facilitates the avoidance of anachronisms and allows a sophisticated treatment of historiographical issues like scientific discovery. It amounts to a mild anti−realism, which maintains the integrity and autonomy of past scientific life.
sion.

NOTES

1 I am indebted to Nancy Nersessian for this example.

2 This example is not taken from Morrison's article and aims at countering an objection that could be raised against her criticism of Hacking. In the example that she put forward, scientists did not manipulate directly quarks, but more complex entities (hadrons) that were composed of quarks. One could object that Hacking's criterion requires the manipulation of, say, electrons per se, as opposed to a more complex entity that is composed of electrons. Hacking could not have meant that the manipulation of, e.g., a table, which is also (according to him) made up in part of electrons, proves the existence of electrons. The example that follows is not open to this objection, since it concerns the manipulation of cathode rays, which are electrons.

3 In some cases, of course, one can manipulate electrons without knowing all of these properties. In the Thomson case, for example, the only property that was needed for the purpose of manipulation was the mass to charge ratio of the electron.

4 Unless, of course, one natural kind subsumes another (e.g., the referents of the term ’cat’ are also referents of the term ’animal’).

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