RE-FRAMING THE CONCEPTUAL CHANGE APPROACH IN LEARNING AND INSTRUCTION

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Chapter 5

Conceptual Change and Scientific Realism: Facing Kuhn's Challenge

Theodore Arabatzis

Introduction

The impact of Thomas Kuhn's work on history and philosophy of science has been unparalleled. His classic *The Structure of Scientific Revolutions* (Kuhn, 1970) has changed permanently the historiographical and philosophical landscape. Its influence on historians of science has been pervasive but, for the most part, indirect. It is a striking fact that very few historians have employed the conceptual apparatus of *The Structure* (i.e., "paradigm", "normal science", "crisis", "revolution", "gestalt switch", "incommensurability") to illuminate past scientific developments. Philosophers of science, on the other hand, have been directly affected by Kuhn's historical philosophy of science and have debated endlessly the issues that he and his fellow traveler Paul Feyerabend raised. Those issues (e.g., the rationality of theory-choice, the reality of the ontology of science) remain at the forefront of contemporary philosophical reflection on science.

One of the most far-reaching claims of Kuhn and Feyerabend concerned the nature of scientific concepts. They promoted a contextual view of concepts, according to which concepts obtain their meaning from the theoretical framework in which they are embedded. It follows that when a theoretical framework changes the concepts embedded in it change too (Feyerabend, 1962; Kuhn, 1970). Given that theory change has been quite common in history of science, conceptual change must have been a ubiquitous phenomenon. Several problematic philosophical consequences seemed to ensue from conceptual change, most notably a relativist view of theory-choice and an anti-realist stance toward the ontology of science. It was widely believed that rational choice between scientific theories could take place only against a stable, shared conceptual framework. With such a framework in place the claims of competing scientific theories could be formulated in a common language and then subjected to comparative evaluation. On the other hand, in the absence of such a framework objective theory-choice seemed impossible, since there would be no common language for formulating and comparing rival scientific theories. Furthermore, the realist requirement for a stable scientific ontology was hard to reconcile with conceptual change.

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If scientific concepts change, then it is not clear how they can continue to refer to the same entities, properties, or processes.

This chapter focuses on the latter issue, namely the implications of conceptual change for scientific realism.[1] The debate on scientific realism has taken place along several lines.[2] The most important, for my purposes here, concerns the grounds that we have for believing in the reality of the unobservable entities postulated by contemporary science (atoms, electrons, photons, fields, etc.). These entities are represented by theoretical concepts. If these concepts are evolving, then what sense does it make to believe that they continue to refer to the same, and *ipso facto* real, entities?[3] In what follows, I will discuss some of the main developments concerning this question in post-positivist philosophy of science. I will then argue that the evolution of theoretical concepts need not throw doubt, under certain conditions, on their stable identity. When theoretical concepts change they may continue to refer to the same entities, properties, or processes.

Historicizing Concepts: Kuhn's and Feyerabend's Anti-Realist Theses

As I noted above, Kuhn and Feyerabend launched a novel and powerful attack against a realist reading of the historical development of science, that is, the view that science aims at

> a permanent fixed truth, of which each stage in the development of scientific knowledge is a better exemplar. (Kuhn, 1970, p. 173)

On this view,

> the progress of science … consist[s] in ever closer specification of a single world, the actual or real one. (Kuhn, 1989, p. 24)

Here I will focus on Feyerabend's views, since they were more radical and developed in more philosophical detail than Kuhn's early views.[4]

Feyerabend (1962) presented his views on conceptual change in a seminal paper. There he argued that the concept associated with a scientific term

> is not an intrinsic property of it, but is dependent upon the way in which the term has been incorporated into a theory. (Feyerabend, 1962, reprinted in Feyerabend, 1981, p. 74)[5]

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[1] What follows draws on material I have presented in more detail in Arabatzis (2006). I thank the University of Chicago Press for granting me permission to adapt and use portions of Chapter 9 of this book, entitled "Identifying the electron: Meaning variance and the historicity of scientific realism".

[2] For a comprehensive analysis of this debate, from a realist point of view, see Psillos (1999).

[3] cf. van Fraassen (2002, p. 56).

[4] I have discussed Kuhn's recent and more philosophically articulated views on scientific realism in Arabatzis (2001).

[5] Note that Feyerabend framed the issue in terms of meanings, as opposed to concepts.

Since scientific concepts are dependent on the theory in which they are embedded, conceptual change is a necessary consequence of theory change. Concepts "change their meaning with the progress of science", since

> extension of knowledge leads to a decisive modification of the previous theories both as regards the quantitative assertions made and as regards the meanings of the main descriptive terms used. (Feyerabend, 1962, reprinted in Feyerabend, 1981, pp. 80–81)

What are the implications of conceptual change for scientific realism? To use a concrete example, emphasized by both Kuhn and Feyerabend, if the

> physical referents of these Einsteinian concepts [space, time, and mass] are by no means identical with those of the Newtonian concepts that bear the same name (Kuhn, 1970, p. 102)

then what should we conclude about the ontological status of the referents of those concepts?

There are two options available to the realist. First, he or she could accept the discrepancy between Newtonian and Einsteinian concepts and argue that only the Einsteinian concepts have genuine physical referents. However, if the difference between Newtonian and Einsteinian concepts implies that the former do not have a referent then, similarly, future theoretical developments may reveal that Einsteinian terms are non-referential too. Second, the realist could deny that Kuhn and Feyerabend have provided an accurate characterization of conceptual change in science and, thus, dispute their claim that Newtonian terms do not have a referent. An argument along these lines would be that, despite the differences between Newtonian and Einsteinian concepts, there are sufficient similarities between the two sets of concepts to enable us to maintain that Newtonian terms refer to the manifestations of relativistic entities at low velocities compared with the velocity of light.[6]

Finally, another realist response would be to point out some difficulties in the view of concepts favored by Kuhn and Feyerabend and thus to cast doubt on their conclusions. As I mentioned above, according to that view, the concepts associated with scientific terms are theory dependent. One needs to know then what counts as part of a theory and what kinds of theory change amount to a transformation of the concepts of the theory. In this respect, as has been pointed out by Dudley Shapere, Kuhn's and Feyerabend's views are open to objection (see Shapere, 1966).[7] "Theory" is usually understood as a systematic and articulated body of knowledge. Feyerabend on the other hand has a more inclusive conception of "theory":

> The term "theory" will be used in a wide sense, including ordinary beliefs (e.g., the belief in the existence of material objects), myths (e.g., the myth of eternal recurrence), religious beliefs, etc. In short, any sufficiently general point of view concerning matter of fact will be termed a "theory". (Feyerabend, 1965a, p. 219)

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[6] cf. Shapere (1991, p. 656).

[7] The critique of Feyerabend that follows is based on Shapere's paper. The attempt, however, to improve on Feyerabend's views so as to meet Shapere's criticism is mine.

Given this notion of "theory" and the theory dependence of concepts, it is not clear what aspects of the theoretical context are relevant to specifying a concept. Feyerabend, to the best of my knowledge, has not provided a satisfactory resolution of this difficulty. Nevertheless, this difficulty can be, at least partly, alleviated if we narrow Feyerabend's conception of "theory" so as to exclude ordinary beliefs, myths, religious beliefs, etc. and to retain scientific theories only. In such a case a concept would be specified by the set of properties ascribed to its referent (a purported entity in nature) by the scientific theory in which the concept is embedded.[8]

One may object that not all the properties attributed to an entity are constitutive of the concept associated with it. The attribution of some of those properties, on such a view, is a merely factual assertion about the entity and is, therefore, irrelevant to the concept associated with it. A satisfactory rebuttal of that objection would require an extensive discussion of the analytic/synthetic distinction, a task that is beyond the scope of this chapter. Here I can only appeal to the widely accepted view, originating from Quine, that a clear-cut distinction of this kind is not possible.[9] Furthermore, the main realist opponent of Feyerabend, Hilary Putnam, has emphasized

> the impossibility of separating, in the actual use of the word, that part of the use which reflects the 'meaning' of the word [or, I would add, the concept associated with a word] and that part of the use which reflects deeply embedded collateral information. (Putnam, 1962, pp. 40–41)

I will have more to say about this issue below.

To return to my proposed modification of Feyerabend's notion of concepts, a difficulty remains: What is the relationship between theory change and conceptual change? In other words, what kinds of modifications in a theory affect the concepts that the theory specifies? One could think of changes in a theory that would be too minor to affect the concepts embedded in it. For example, a refinement of the gravitational constant in Newton's law of gravity would hardly change the meaning of any Newtonian terms. Feyerabend tried to alleviate this difficulty by proposing that

> we shall diagnose a change of meaning [conceptual change] either if a new theory entails that all concepts of the preceding theory have zero extension or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself. (Feyerabend, 1965b, reprinted in Feyerabend, 1981, p. 98)

The former "diagnostic" procedure is far too strict. After all, one would want to diagnose a case of conceptual change if a new theory entails that *some* of the concepts of the preceding theory are vacuous and refer to nothing at all.

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[8] A similar view is held by Dudley Shapere. The only difference is that, in Shapere's view, the concept associated with a term is identified with the set of properties attributed by the *group of scientists who are using the term* to the entity that the term denotes (see Shapere, 1982, p. 21).

[9] For some useful reflections on this issue, see Papineau (1996).

The latter concerns the taxonomic function of scientific theories and it presupposes the existence of unambiguous rules of classification, which can be used to collect objects, events, entities, etc. into sets. If a new theory attributes additional properties to the entities denoted by the concepts of the previous theory but preserves its taxonomic rules, one should conclude, according to Feyerabend, that no conceptual change has taken place. First, one might want to dispute his claim that the properties of an entity, to the extent that they are not related to any rules of classification, are irrelevant to the concept associated with the corresponding term. However, even if every feature associated with an entity is relevant to the specification of the corresponding concept, it would still be the case that any conceptual change brought about by the addition of new properties to a *fixed* set of entities would lack any anti-realist connotations. On the contrary, conceptual change of this kind would support a realist position, since it would have taken place against a stable ontological background.

Furthermore, the existence of unambiguous taxonomic rules in science has been denied by Shapere (1966). Scientific classifications, in his view, are usually based on pragmatic criteria, which do not reflect any intrinsic properties of the entities classified. For example, the question

> Are mesons different "kinds of entities" from electrons and protons, or are they simply a different subclass of elementary particles? … can be answered either way, depending on the kind of information that is being requested for there are differences as well as similarities between electrons and mesons … [It] can be given a simple answer ("different" or "the same") only if unwanted similarities or differences are stipulated away as inessential. (Shapere, 1966, pp. 51–52)

Thus, an appropriate choice of taxonomic criteria might leave the concepts associated with the entities in question unaffected by theory change.

Shapere's criticism is based on his emphasis on the pragmatic character of scientific classifications, an emphasis that, I think, is overstated. Physics, for instance, has produced an unambiguous classification of the unobservable realm into various particles (electrons, protons, neutrons, etc.). It is true that their occasional similarities enable the classification of otherwise different entities under the same category. For example, electrons, protons, neutrons, and quarks are all classified as fermions, since they obey Fermi–Dirac statistics. But these entities, despite their occasional similarities, are clearly distinguished from one another according to their intrinsic properties (e.g., the magnitude of their charge, their mass, etc.). At least some classifications are predicated on the intrinsic properties of the entities classified and, thus, Feyerabend's notion of conceptual change could escape Shapere's criticism if one limited its applicability to classifications of that kind. With these qualifications, the above difficulties in the criterion of conceptual change favored by Feyerabend disappear and, thus, cannot be employed to support a realist position.

An anti-realist stance is implicit in those of Feyerabend's views that I have discussed so far. In his early work he did not adopt an explicit position on the ontological status of unobservable entities, an aspect of the realism debate with which he was not directly concerned. He was occupied, rather, with the phenomenon of meaning change and its implications for the then-current theories of scientific explanation and reduction. In his later writings, however, he formulated unambiguously the anti-realist perspective that was implicit in his early papers. The phenomenon of conceptual change at its most extreme (incommensurability) implies, in his view, that past scientific theories, which have been replaced by their incommensurable successors, were based on a non-existent ontology. For example,

> *prerelativistic* terms … are pretty far removed from reality (especially in view of the fact that they come from an incorrect theory implying a nonexistent ontology). (Feyerabend, 1970, p. 87)

Feyerabend's view of conceptual change, a view strikingly parallel to Kuhn's, presented a serious challenge to scientific realists. It seemed that the instability of scientific concepts had as an immediate corollary the collapse of a realist attitude toward their referents. Given the then-prevalent belief (inspired by Frege) that a concept is specified by a set of conditions that are necessary and sufficient for its correct application, the slightest change in those conditions (conceptual change) would imply that the concept as previously used was vacuous, that is, it referred to nothing at all. Hence Kuhn's recent remark that "the history of science is the history of developing vacuity" (Kuhn, 1989, p. 32).

Furthermore, even if one rejects the 'necessary and sufficient conditions' view of concepts, as most philosophers (and, as far as I know, all psychologists) nowadays do, a problem remains. If a concept evolves over time it is not clear how it can refer to the same entity.[10] Why is it, for example, that Faraday's field concept and Einstein's field concept have the same counterpart in nature? (cf. Nersessian, 1984). If they do not, as Kuhn and Feyerabend argued, then one is forced to admit that there are no entities in nature with the properties of Faraday's field. Only the Einsteinian field concept may have a genuine referent. And even that possibility may be unlikely. Consider the following alarming scenario, due to Hilary Putnam:

> What if *all* the theoretical entities postulated by one generation (molecules, genes, etc., as well as electrons) invariably do not exist from the standpoint of later science? … One reason this is a serious worry is that eventually the following meta-induction becomes overwhelmingly compelling: *just as no term used in science of more than fifty (or whatever) years ago referred, so it will turn out that no term used now refers* (except maybe observation terms, if there are such). (Putnam, 1984, pp. 145–146)

Thus, Putnam saw clearly the, *prima facie*, anti-realist implications of conceptual change. Let me now turn to his significant attempt to evade those implications.

Putnam on Conceptual Change: A Realist Way Out?

In the early 1970s Putnam tried to disentangle concepts from their referents by suggesting that the latter are independent and essential constituents of the former. That is, a concept

[10] Remember that the requirement for a stable ontology is crucial for realism.

is determined, to a large extent, by its referent and not *vice versa*. Whatever changes the other components of a concept may undergo the "referential" component will remain unaffected. Putnam was not particularly concerned with theoretical concepts denoting unobservable entities. Following Kripke, whose ideas were developed in the context of modal logic, he was occupied, rather, with natural kind concepts in general. The purpose of his arguments was to show that the referents of natural kind concepts have not been affected by the development of scientific knowledge. Even though

> the concept [of a natural kind, e.g., fish] is not exactly correct (as a description of the corresponding natural kind)…that does not make it a *fiction*. …The concept is continually changing as a result of the impact of scientific discoveries, but that does not mean that it ceases to correspond to the same natural kind … . (Putnam, 1973, p. 197)

I have two comments here. First, I do not think that one can exclude the possibility that future changes in the concept of, for example, fish will affect its reference. The concept may change so that organisms that are now classified as fish will cease to be thus classified. Second, the example of "fish", or any other observable natural kind concept, is not very helpful when it comes to questions of scientific realism. Even if the reference of that concept changed, the existence of the individual organisms that we classify as fish would not be questioned. And that is because the existence of those organisms is established on grounds independent of our system of classification. That is not the case, however, when the natural kind concept refers to unobservable entities. Besides the fact that several such concepts have been abandoned,[11] even when they have been retained, the lack of independent, physical access to the entities denoted by those concepts makes problematic the claim that, in case of conceptual change, their referents remain stable. Whatever the merit of Putnam's arguments for the referential stability of observable natural kind concepts, I will argue that, when it comes to theoretical concepts, which denote unobservable entities, his theory of reference does not remove the threat that conceptual change poses to scientific realism.[12]

Putnam's view of concepts was motivated by a basic realist intuition, namely "that there are successive scientific theories about the *same* things: about heat, about electricity, about electrons, and so forth". For instance,

> Bohr would have been referring to electrons when he used the word "electron", notwithstanding the fact that some of his beliefs about electrons were mistaken, and we are referring to those same particles notwithstanding the fact that some of our beliefs — even beliefs included in our scientific "definition" of the term "electron" — may very likely turn out to be equally mistaken. (Putnam, 1973, p. 197)

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[11] For a very long list of concepts that have been abandoned, see Laudan (1981).

[12] Several critics of Putnam have also argued, for different reasons than I will suggest below, that his theory does not work for theoretical concepts (see Enç, 1976; Kroon, 1985; Nola, 1980).

To flesh out this realist intuition Putnam had to reject the 'necessary and sufficient conditions' view of concepts. In his view, membership in the extension of a term is not determined by a set of necessary and sufficient conditions, which constitute the concept associated with that term. Rather the reference of a term (concept) is fixed through our causal interaction with the world:

> things which are given *existentially* … help to fix reference. Actual things, whatever their description, which have played a certain causal role in our acquisition and use of terms determine what the term refers to. (Putnam, 1983, p. 73)

Thus, the reference of a concept is an independent component of it and is not affected by (even drastic) changes the other components might undergo.[13]

One of the main arguments that Putnam directed against the necessary and sufficient conditions view of concepts was the following thought experiment.[14] Consider our familiar concept of water. The reference of the concept of water, on the necessary and sufficient conditions view, is determined by the phenomenological properties we attribute to water (transparent, tasteless, thirst-quenching, etc.). Putnam, however, pointed out that the discovery of a substance with the same phenomenological properties as our familiar water but with a totally different structure (XYZ) is conceivable. In that case the reference of our concept of water would not include the novel substance (XYZ). Thus, the "stereotypical" features of an entity or substance, despite being part of the concept associated with it, do not determine the concept's reference. It follows that the evolution of a concept need not affect the stability of its reference.

Putnam's rejection of the necessary and sufficient conditions view neutralizes, to some extent, the anti-realist implications of conceptual change. If a concept could refer to the same entity(ies) despite the fact that it has evolved, anti-realism would not be an inescapable consequence of the instability of scientific concepts. I put the qualifier "to some extent" because a belief in the existence of, say, electrons would make sense only if a core of the concept of the electron (i.e., some of our beliefs about electrons) has remained stable since the initial proposal of the electron hypothesis. Cataclysmic changes of scientific concepts, which would amount to the abandonment of every belief about the corresponding entities, would be indeed a threat to scientific realism. Barring such radical cases, Putnam's rejection of the necessary and sufficient conditions view of concepts should be an indispensable ingredient of any realist position.

Nevertheless, Putnam's success in disentangling the reference of a concept from the rest of it does not extend to cases where a concept refers to an unobservable entity (or a class of

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[13] Here it is worth pointing out that Putnam's views on reference have remained intact, even though his more recent views on scientific realism depart considerably from the realist intuitions that motivated his earlier attempts to articulate a theory of meaning. In a recent discussion of this issue, for example, he insists that "the reference of the terms 'water,' 'leopard,' 'gold,' and so forth is partly fixed by the substances and organisms themselves… [while] the 'meaning' of these terms is open to indefinite future scientific discovery" (Putnam, 1990, pp. 109–110).

[14] See Putnam (1975a, pp. 223ff).

such entities). In order for the referent of a concept to be an essential and stable constituent of the concept in question, as Putnam's theory requires, it should be possible to identify the referent without relying on the other constituents of the concept. To that effect, one should have independent, physical access to the entities denoted by a concept, independent of (most of) our beliefs about them. This independent access guarantees, extreme skepticism excluded, that those entities are real. However, in the case of unobservable entities no such direct epistemic access exists. All that we have is a set of indirectly confirmed beliefs about those entities and it is not clear that we can identify them independently of those beliefs.[15] Furthermore, since the problem under examination is the ontological status of unobservable entities, it goes without saying that a theory like Putnam's, which presupposes their existence, cannot be employed without circularity to support a realist position.

My evaluation of Putnam's theory as a potential realist tool was based on the assumption that unobservable entities are beyond direct empirical access. This assumption has been forcefully challenged by Ian Hacking, whose views on scientific realism are the subject of the next section.

Hacking's Entity Realism

Hacking (1983) has advanced a realist position, which 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 toward 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 that 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 in hand, a variety of sometimes incompatible theoretical models of those entities, generates strong doubts that any of those models accurately represent 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 a realist about this common core, which, however, does not deserve to be called a "theory".

Hacking's entity realism can be summarized in his aptly chosen slogan: "If you spray [e.g.] electrons then they are real" (Hacking, 1983, p. 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

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[15] I will suggest below a recipe for identifying the reference of theoretical concepts denoting unobservable entities that would allow one to include reference as a component of a concept. However, it is not applicable to entities that have not been (and perhaps cannot be) subject to experimental investigation (e.g., black holes). Furthermore, even where it is applicable, the referential stability of a concept requires historical investigation and should not be taken for granted prior to such an investigation.

real then you spray them".[16] 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 that Hacking, following contemporary experimentalists, describes as "spraying of electrons". It is conceivable that the process now described as "spraying of electrons" might be re-described in terms of an alternative theory based on a different ontology.

Another objection has been raised against Hacking's claim that manipulability is a necessary and sufficient criterion for establishing the existence of an unobservable entity, namely that it does not do justice to actual scientific practice (see Morrison, 1990). In particular, his exclusive emphasis on manipulability fails to capture the variety of evaluative criteria that are employed *within* scientific practice for demonstrating the reality of an unobservable entity.[17] The most important of those criteria seems to be the empirical adequacy of the theory that postulates the entity in question. Even though manipulability is one of the criteria that scientists often employ, it fails to carry conviction in all contexts. Furthermore, scientists need to have a fairly definite idea of what it is that they are manipulating before invoking manipulability as a demonstrative principle. In many experimental contexts we know that we are manipulating *something* without knowing what it is that we are manipulating. Manipulability, however, was supposed to provide adequate grounds for the existence of, for example, electrons and not merely for the existence of an "I know-not-what" something.

Thus, Hacking's manipulability criterion fails as a descriptive account of the plurality of principles that scientists employ in the construction of "existence proofs" for unobservable entities. It also fails as a normative account, since even within actual scientific practice manipulability does not (and should not) provide adequate reasons for belief in the entities that are supposedly manipulated.

A further problematic aspect of Hacking's realist position is associated with his "home truths" (low-level generalizations) about, for example, electrons that we supposedly know independently of any high-level theory. Hacking does not 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. It is difficult to see, however, how one could isolate the concepts denoting 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.

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[16] Personal communication.

[17] One could deny that Hacking's criterion needs to capture the richness of scientific practice, by interpreting it as a normative as opposed to a descriptive criterion. However, Hacking himself stresses the descriptive (as well as the normative) dimensions of his criterion. For him, the main reason that *scientists themselves* believe in the reality of some unobservable entities is their ability to manipulate those entities: "The vast majority of experimental physicists are realists about some theoretical entities, namely the ones they *use*. I claim that they cannot help being so" (Hacking, 1983, p. 262).

Hacking could argue that all those different and incompatible conceptions of charge had certain aspects in common, that is, the causal properties that we have associated with electric charge all along (e.g., the ability of charges to attract, or repel each other). But even those properties may vary across theoretical frameworks. In classical electromagnetic theories, for instance, one of the causal properties of a charged particle was that it emitted radiation when performing accelerated motion. In the old quantum theory of the atom, on the other hand, charged particles in accelerated motion (electrons orbiting around the nucleus) did not radiate. Hacking's 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.

A closely associated problem is Hacking's selective realism. Let us grant for the sake of argument that belief in the existence of the "well-known causal properties" of some entity does not depend on how we interpret those properties theoretically. As I already mentioned, in his view, one should maintain a realist position with respect to those properties, but, nevertheless, should not commit oneself to believing that the theories involved in the interpretation of the relevant experiments are true.[18] However, as was pointed out by Duhem, experimental results falsify (or, I would add, confirm) whole chunks of knowledge consisting of high-level theory(ies), an understanding of the instruments involved in the given experimental context, statements about initial conditions, etc. In view of this very plausible holistic epistemology, which Hacking has not by any means discredited, his selective realism does not make much sense. To put it another way, it is far from clear why our ability to make sense of certain experimental situations confers a privileged status on the entities supposedly involved in the experiments but, nevertheless, cannot be employed as an argument for a realist stance toward the theory(ies) involved in the interpretation of those experiments. The following example would suffice to illustrate my point: The manipulation of electrons requires knowledge of their behavior in various experimental contexts (e.g., in a cloud chamber). To control that behavior one has to know, in addition to the causal properties of electrons, several background theories (electromagnetism, mechanics, etc.), which predict how an entity with those properties would behave in a given context. It is those background theories along with Hacking's "home-truths" that enable intervention. Thus, if our ability to intervene is an argument for realism, that realism should be of a very broad kind covering entities and theories alike.

Having discussed some significant positions that have been advanced with regard to the implications of conceptual change for scientific realism, I will now put forward my own approach to this problem.

How to Rescue Scientific Realism from Conceptual Change

To present my own view on conceptual change and scientific realism, it is necessary to adopt a particular view of scientific concepts. In what follows, I take a concept to be the

[18] Perhaps one should be a realist about the phenomenological theories of the instruments involved.

set of features that are ascribed to its putative referent, by the theory in which the concept is embedded. In case that no such systematic theory has been developed, a concept would consist of the characteristics attributed to its counterpart in nature, by the group of scientists who are using it. These characteristics consist of two interrelated kinds: the properties of the entity in question and the laws obeyed by it. The former are usually specified via the latter. For instance, what charge is depends on the laws obeyed by charged bodies, for example, the laws of classical electromagnetism. Conversely, the laws obeyed by an entity depend on its properties. For example, the electrons qua charged particles are supposed to obey Coulomb's law.

As I already mentioned, one may find objectionable the suggestion that all the characteristics ascribed by a theory to an entity are part of the corresponding concept. In particular, one may call for a distinction between the concept-constitutive and the merely factual aspects of the information concerning the referent of a concept. I think, however, that such a distinction cannot be drawn in a satisfactory way. To begin with, no distinction of this kind is inherent in the scientific representation of an entity. For example, electrons are represented as subatomic particles, with a specific amount of charge, a specific rest mass, spin, etc. There is nothing in this representation, however, that explicitly distinguishes some of these properties as constitutive of the concept of the electron.

Could one impose such a distinction and on what grounds? I can think of two alternatives, none of which seems adequate. First, one could argue that only those properties that are relevant to the classification of an entity are part of the corresponding concept. For example, only the subset of the properties of electrons that enables us to distinguish them from other elementary particles should be considered part of the concept of the electron. This recipe has the undesirable consequence that a concept may change if the domain of classification is enriched. Consider, again, the case of electrons. Until the early 1910s there was no need to distinguish them from other charged particles, because they were the only particles postulated. When the concept of the proton was introduced, however, the electron's negative charge, or the small value of its mass became crucial for distinguishing it from a proton. Should we say that with the postulation of protons the concept of the electron changed? Would someone who learned the concept before the proposal of protons learn something different from someone who learned about electrons after that proposal?

Second, one could argue that only those properties that are used for identifying the reference of a term belong to the associated concept. These are properties, as I will suggest below, which enable the identification of an entity in an experimental situation. In the case of electrons, their charge-to-mass ratio was crucial for deciding whether various phenomena (cathode rays, Zeeman effect, etc.) were their observable manifestations. Other properties, for example, the size of electrons, were not significant in that respect (see Arabatzis, 2006). The problem with this suggestion is that the properties employed for identifying the reference of a term may not suffice to convey the associated concept. The concept of the electron, for instance, is not exhausted by the electron's charge-to-mass ratio. One has to know much more than that to know what an electron is.

The view of concepts I have suggested differs both from the necessary and sufficient conditions view and from Putnam's notion. It differs from the former in that the properties associated with an entity and the laws it is supposed to obey are neither necessary nor sufficient conditions that any such entity should fulfill. The advancement of science may very well eliminate several of those characteristics, reveal further properties of the entity in question, or alter the laws it is supposed to obey without thereby affecting the reference of the corresponding concept.

The view of concepts I advocate also departs from Putnam's in that it does not require the referent of a term to be a stable and independent component of the associated concept. Dropping this requirement has two advantages. First, no problem arises when a concept denotes unobservable entities with no experimental manifestations, whose reference, therefore, cannot be identified independently of a description. Second, the stability of a concept's reference is left open and, as I will argue below, becomes an issue that has to be settled through historical work.

Given the above view of concepts a realist position is compatible with the phenomenon of conceptual change to the extent that a core of the evolving concept in question has survived changes in theoretical perspective.[19] Thus, histories of scientific concepts can play a seminal role in evaluating the tenability of a realist attitude toward the corresponding entities. On the one hand, a realist stance toward a particular entity would be discredited if the historical record revealed that the corresponding concept has undergone a radical transformation, which has affected even the most central features previously associated with that entity. On the other hand, if historical analysis shows that a core of the concept has remained unaffected by changes in theoretical perspective, this would be an argument (albeit not a conclusive one) for maintaining a realist position with respect to the entity under examination. Such a position would involve the inductively grounded belief that the core of properties attributed to the entity will survive changes in high-level theory.

One might want to challenge the view that the survival of a conceptual core enables a realist perspective to get off the ground. In particular, one might ask for the specific criteria that privilege this set of beliefs (the "core") over the rest of a concept that proved highly unstable. I will not attempt to respond to this line of criticism, because I think that there is another way out for the aspiring realist. In particular, there is a way to identify the reference of a theoretical concept, denoting an unobservable entity, without relying heavily on that entity's description. Theoretical concepts are usually introduced and articulated to make sense of experimentally obtained phenomena. The concept of the electron, for instance, was initially developed to account for the behavior of cathode rays in a vacuum tube and for the magnetic splitting of spectral lines (see Arabatzis, 2006). When presented with such a concept, one has to identify the experimental situations that are taken to manifest the presence of (are causally attributed to) its referent, the corresponding unobservable entity. Thus, the experimental situations associated with a concept provide a way to track its referent.

Putnam has anticipated this proposal by suggesting that we can identify a magnitude (e.g., charge)

> by, for example, singling it out as the magnitude which is causally responsible for certain effects (Putnam, 1975b, p. ix)

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[19] A similar suggestion has been defended by Putnam (1974, p. 275). For some other options that are open to the realist, see below. cf. also Psillos (1999, p. 295).

However, he takes for granted that the reference of scientific concepts does not change over time. In my view, on the other hand, the realist intuition that (mature) science has developed against a stable ontological background has to pass the test of historical scrutiny. It is an open question whether the reference of, say, the concept of charge has remained stable. Having provided a criterion for identifying reference, the stability of the reference of a specific concept over time should be investigated historically. Historical research, thus, obtains an essential role for evaluating the tenability of a realist attitude toward the ontology of science.

Furthermore, Putnam does not address a question that, I think, is important: What binds certain effects together as manifestations of a single entity? (cf. Kuhn, 1979, p. 411). Different effects can be attributed to the same entity only if they have some qualitative or quantitative features in common. First, they may share some common qualitative feature, which indicates that a single entity is "behind" all of them. For example, it was known since the 17th century that the processes of combustion, calcination (oxidation), and respiration took place only in the presence of atmospheric air. That fact was explained by the hypothesis that air was necessary for absorbing phlogiston, the entity that was given off in all those processes. Second, from the quantitative features of an experimental situation it may be possible to infer the value of some property of the hidden entity involved in it.[20] If inferences from different situations converge to the same results, then this agreement binds them together as effects of the same entity. Furthermore, the value in question provides a way of identifying the entity in novel experimental situations. For example, at the turn of the 19th century several experimental phenomena (the Zeeman effect, cathode rays, β-rays, etc.) were attributed to the presence and action of hypothetical charged particles. It was not *a priori* evident that the particles involved in all those different phenomena were the same. Physicists were led to that conclusion when it turned out that the particles that were responsible for the Zeeman effect had approximately the same charge-to-mass ratio with the particles which constituted cathode rays. From that point on, the charge-to-mass ratio of the electron functioned as a criterion for identifying it in novel experimental situations (for details see Arabatzis, 2006).

To be a realist about a particular entity one has to establish the referential continuity of the corresponding concept. That is, to show that over the history of the concept the experimental situations that were assumed to be the observable manifestations of its referent have remained stable or, at least, exhibited a cumulative development (i.e., the previous set of situations associated with the concept was a subset of the current set). This expansion may happen in three ways. First, new situations may get attributed to a familiar entity, without any modification in the theoretical description of that entity. In that case the corresponding concept will remain unaffected. Second, the accommodation of new situations as manifestations of a familiar entity may require the attribution of additional properties to that entity, which, moreover, do not conflict with its previously established properties. In that case, the concept will expand in a cumulative way and its reference will remain the same. Third, the accommodation of novel situations may require the attribution of novel

[20] This type of "backward" inference is sometimes called "deduction from the phenomena". I think that this expression is misleading, because these deductions always involve additional theoretical assumptions about, for instance, the entity's properties or the laws it obeys. See the very enlightening discussion in Worrall (2000).

properties to the entity, which, however, are incompatible with some of its previously accepted properties. The latter will, in the process, get rejected. In that case, the concept will change in a non-cumulative fashion. Despite that change, the concept may still refer to the same entity, provided that the experimental situations previously associated with the concept continue to be attributed to its referent. As long as that "family" of experimental situations remains invariant (or expands in a cumulative fashion) a realist stance toward the entity involved in them is not threatened by changes in that entity's description.

Thus, the question whether realism is a defensible approach *vis-à-vis* a certain entity requires a historical reconstruction of the evolution of the corresponding concept (which requires considering the experimental situations that track its reference). Unless the historical and philosophical community undertakes successfully the enormous historiographical task of showing the referential continuity of most scientific concepts, any realist position will make sense only with respect to specific entities (local realism).

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