Scientific Concepts and Investigative Practice

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Experimentation and the Meaning of Scientific Concepts

Theodore Arabatzis

1. Introduction: Concepts and &HPS

There are encouraging signs that, after a long period of "withering on the vine" (Fuller 1991), integrated history and philosophy of science has begun to pick up steam. Scientific concepts can play a significant role in achieving a synthesis of historical and philosophical perspectives on science, because they are of interest to both fields. On the philosophical side, ever since the heyday of logical positivism concepts have been at the center of scholarly discussions in philosophy of science (Arabatzis/Kindi 2008). This is not surprising, since concepts mediate our cognitive access to the world. Furthermore, ever since the early 1960 and the historicist turn in philosophy of science, concepts have figured large in philosophical debates about the nature of scientific change. The historical character of concepts, their dependence on the context in which they are formed and their change over time, has casted doubt upon the rationality of scientific change and has been among the main challenges faced by scientific realism. Realists favor ontological stability and it is not prima facie clear that concepts, qua historically evolving entities, continue to pick out the same referents throughout their historical development (Arabatzis 2007).

On the historiographical side, a focus on concepts may enhance our understanding of how local intellectual, material, and cultural resources are brought to bear on the production of scientific knowledge (Nersessian 2008). Moreover, because of their historicity concepts lend themselves to become "central subjects" of historical narratives (Hull 1975). Here, however, one has to face the same thorny issue that philosophers have been struggling with: the evolving character of concepts. If concepts change (often beyond recognition), how can we construct coherent historical narratives around them? What keeps together different uses of a term over time if the beliefs and practices associated with it change? If a concept does not retain its identity over time, what is its history the history of (Arabatzis 2006; Dear 2005; Kuukkanen 2008)?

Thus, a focus on concepts has the potential of becoming a vehicle for integrating history and philosophy of science. On the one hand, to deal with the historiographical issue of how to frame a historical narrative we need to engage with philosophical accounts of concepts and conceptual change. Conversely, to come to terms with the philosophical challenge posed by conceptual change we need to do historical research.

One of the pillars of the revival of integrated HPS has been the attempt to redress 'the neglect of experiment' and to scrutinize its intricate relationship to theory. Concepts can provide a fruitful means to that purpose, because they play an important role in experimentation and mediate its interplay with theory. The very detection and stabilization of experimental phenomena goes hand in hand with concept formation (Gooding 1990; Steinle 2005; Andersen 2008; Feest 2010). Furthermore, the explanation of experimentally produced novel phenomena often requires new concepts of the entities and processes that underlie those phenomena. The refinement and articulation of those 'theoretical' concepts play, in turn, an important role in experimental research. Most of the discussion on concepts in philosophy of science, however, has been theory-oriented.

In what follows, I will attempt to redress this imbalance. I will start with a brief review of three salient approaches to concepts: two theory-oriented ones, associated with the 'orthodox' view in philosophy of science and with early Kuhn and Feyerabend; and their rival, causal approach that was put forward by Kripke and Putnam. I will suggest that the causal approach, despite certain shortcomings, opens up space for rethinking concepts from the perspective of the philosophy of experimentation. Philosophers of experiment have come up with important insights about the relationship between theory and experiment and have stressed the autonomy of experimental practice. Those insights may shed new light upon long-standing puzzles about the identity and evolution of concepts. To show this, I will discuss the role of experiment in the formation of new concepts and in the articulation of antecedently available concepts. The focus of my analysis will be on hidden-entity (H-E) concepts, that is, concepts referring to entities that are not accessible to unmediated observation. I will round off the paper with a discussion of the significance of experimentation for tracking the referents of such concepts.

2. Theory-Oriented Approaches to Concepts

A lot of philosophical ink has been spilled on spelling out the meaning of scientific concepts in terms of their location within a systematic theoretical framework.[1] In the "orthodox" view (Feigl 1970), the culmination of logical empiricism, there were two kinds of scientific concepts: observational and theoretical. The meaning of the former was fully specified by their direct association with observable entities, properties, and processes. The meaning of the latter, on the other hand, derived partly from the system of "postulates" in which they were embedded and partly from "correspondence rules" which linked those postulates with a domain of phenomena. Thus, the meaning of theoretical concepts was determined, indirectly, by their links via scientific laws with other theoretical concepts and by their connections, via correspondence rules, to observational concepts. Given that new laws or correspondence rules could always be discovered, it followed that the meaning of theoretical concepts was always "partial" or incomplete (Carnap 1956, 48; 67; cf. Feigl 1970, 5 ff).[2]

The contribution of correspondence rules to the meaning of theoretical concepts allows for some input from experiment. Meaning is partly shaped by experimental procedures and operations. I think though it would be fair to say that, despite its empiricist orientation, the orthodox view downplayed the connection between theoretical concepts and observation and experiment. For instance, in "The Methodological Character of Theoretical Concepts" Carnap admitted, "in agreement with most empiricists, that the connection between the observation terms and the terms of theoretical science is much more indirect and weak than it was conceived … in my earlier formulations" (Carnap 1956, 53; cf. Feigl 1970, 7). Furthermore, as some of its critics pointed out, the orthodox view neglected the use of theoretical concepts in experimental contexts. Theoretical concepts, such as the concept of the electron, are often used in "observation sentences" describing the outcome of experimental interventions. Think, for instance, of experimental reports of positron tracks in a cloud chamber (cf. Feyera-

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[1] For a historical survey of the philosophical literature on the meaning of scientific concepts see Arabatzis/Kindi 2008.

[2] Note, however, that in his earlier work Carnap had suggested that the meaning of scientific concepts derived from their conditions of application in experimental situations (Carnap 1936, 1937).

bend 1960/1999, 18 ff; Putnam 1962/1975, 217; Hempel 1973/2001, 212).

The tenuous connection between scientific concepts and experience was loosened further with the rise of historicist philosophy of science. Feyerabend, for instance, claimed that "the fact that a statement belongs to the observational domain has no bearing upon its meaning" (1962/1981, 52). Rather, "*the interpretation of an observation language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change*" (1958/1981, 31). As regards the meaning of scientific terms, Feyerabend opted for "regarding theoretical principles as fundamental and giving secondary place … to those peculiarities of the usage of our terms which come forth in their application in concrete and, possibly, observable situations" (Feyerabend 1965/1981, 99). Thus, the meanings of scientific concepts (observational and theoretical alike) are "dependent upon the way in which … [they have] been incorporated into a theory" (Feyerabend 1962/1981, 74).

The theory dependence of concepts implies that theory change leads to conceptual change. Moreover, according to Feyerabend and Kuhn, the older concepts and their descendants refer to completely different entities. The very subject matter of scientific investigation shifts along with conceptual change. I would like to stress that the fluidity of scientific ontology over time is not a straightforward consequence of the historical record concerning the development of science. Rather, it follows from an explicit decision to ignore the stability of concept use at the observational (and, I would add, experimental) level and to focus exclusively on the theoretical frameworks in which concepts are embedded. Here is a striking passage from Feyerabend's "On the 'Meaning' of Scientific Terms":

> It may be readily admitted that the transition from *T* to *T'* [classical mechanics to general relativity] will not lead to new methods for estimating the size of an egg at the grocery store or for measuring the distance between the points of support at a suspension bridge. But … we have already decided not to pay attention to any *prima facie* similarities that might arise at the observational level, but to base our judgment [concerning stability or change of meaning] on the principles of the theory only. It may also be admitted that distances that are not too large will still obey the law of Pythagoras. Again we must point out that we are not interested in the empirical regularities we might find in some domain with our imperfect measuring instruments, but in the laws imported into this domain by our theories. (Feyerabend 1965/1981, 100)

This exclusive preoccupation with ‘high’ level theory was bound to overemphasize the unstable characteristics of scientific concepts at the expense of their stable features, associated to a significant extent with ‘low’ level methods of measurement and identification of the referents of scientific concepts in experimental contexts. I will have more to say about this below.

3. Problems of Theory-Oriented Approaches to Concepts

I see three problems with this exclusive preoccupation with theory. First, quite often concepts have a priority over theories. Especially in the frontiers of research, concepts are formed and used in the absence of a fully developed, or even consistent, theoretical framework. For instance, in the early 18th century, well before the development of a systematic theory of electrical phenomena, the concept of two electricities was formed by Charles Dufay in the process of detecting and stabilizing various regularities. The formation of that concept and the genesis of facts about (not a theory of) electricity went hand in hand (Steinle 2005, 2009a). Another more recent example can be found in the investigation of atomic structure during the 1910s and early 1920s. In deciphering the riddle of the atom physicists made heavy use of the concept of the electron, even though they lacked a consistent and systematic theory of the electron and its behavior inside the atom (Arabatzis 2006).

A second problem for theory-centered accounts of concepts is created by their synchronic and diachronic stability. Scientific concepts have a trans-theoretical character and enable the formulation of different contemporary theories about their referents. Sometimes they even have a trans-disciplinary dimension, as testified to by the existence of “boundary objects” that are coveted by different disciplines (Star/Griesemer 1989; Arabatzis 2006, ch. 7). Furthermore, scientific concepts persist across theoretical change, transcending the theoretical frameworks in which they are embedded at particular times. This is an insight that we owe to several philosophers of science, including Hilary Putnam, Dudley Shapere, and Nancy Nersessian.

A third problem is that the exclusive preoccupation with the role of fundamental theories in concept formation has led to a neglect of the interplay between concepts and experimentation. On the one hand, experimental interventions are often crucial for the formation, articulation, and sometimes the failure of scientific concepts (cf. Steinle

2009b). On the other hand, concepts frame and guide experimental research. Furthermore, experimentation is often crucial for the identification of the referents of H-E concepts. Experimental procedures, robust across changes in high-level theory, enable the identification and measurement of H-E on the basis of their (purported) manifestations in experimental settings.

4. Coming to Terms with the Trans-Theoretical Character of Concepts

The most notable response to theory-oriented approaches to concepts has been the causal theory of reference (CTR). It was first suggested by Saul Kripke as an account of proper names, but shortly afterwards it was extended by Kripke and Hilary Putnam to natural kind concepts.[3] While Putnam acknowledged the theory-dependence of those concepts, he stressed their persistence across theory change, a persistence that he attributed to the stability of their reference. This stability derives from the way the reference of a natural kind concept is picked out: not by the full theory in which the concept is embedded, but through a specification of the phenomena that are causally associated with its referent. For instance,

> [n]o matter how much our theory of electrical charge may change, there is one element in the meaning of the term 'electrical charge' that has not changed in the last two hundred years … and that is the reference. 'Electrical charge' *refers to the same magnitude* even if our theory of that magnitude has changed drastically. And we can identify that magnitude in a way that is independent of all but the most violent theory change by, for example, singling it out as the magnitude which is causally responsible for certain effects. (Putnam 1975, ix)

The CTR has its problems. It makes mastery of a scientific concept dependent on "contact" with its counterpart in nature (Putnam 1973/1975, 205). This requirement is problematic when a concept refers to a hidden or fictitious entity, where the required 'contact' is either indirect or altogether missing. In the latter case, the lack of contact between the users of a concept and its purported referent would seem to undermine their linguistic competence. Thus, the realist character of the

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[3] For some salient differences between Kripke's and Putnam's versions of the causal theory of reference see Hacking 2007.

CTR introduces an implausible double-standard in the semantics of scientific concepts: the competence of a concept user depends on whether that concept has a referent (see Arabatzis 2007, 53 ff; Arabatzis/Kindi 2008, 360 f).

Having said that, the CTR opens up space for examining the role of concepts in experimental research, where their purported referents become objects of investigation and manipulation. A focus on experimentation may, in turn, elucidate the 'contact' requirement that, as we saw above, is necessary for the CTR to get off the ground. In the laboratory sciences the presumed contact is achieved, if at all, in artificially produced experimental situations.[4]

5. Experimentalism and Its Implications for Understanding Scientific Concepts

For some time now, experimentation has become the object of sustained philosophical scrutiny. Philosophy of experiment has focused on the validation of experimental knowledge by means of a variety of epistemological strategies. However, despite some notable exceptions,[5] the importance of experimentation for concept formation and concept articulation has not received the attention it deserves.

Perhaps the main lessons of experimentalist philosophy of science have been the relative autonomy of experimentation and its complex non-reductive relationship with various levels of theoretical knowledge, from specific models of phenomena to phenomenological laws to 'deep' unifying principles. One of the manifestations of this autonomy, I would like to suggest, is the relative independence of the concepts employed in experimental settings from the wider theoretical environment in which they 'live'. To put it another way, concepts have a life in experimentation. They frame experimental research and are shaped by it. Sometimes they even fail, by becoming incoherent as a result of experimentally obtained information.

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[4] I should stress though that the 'contact' in question is a fallible assumption. As the examples of phlogiston and caloric show, even concepts which have played a fruitful role in experimental research may turn out to have no counterpart in nature.

[5] Besides the works I've already mentioned, these exceptions include Jed Buchwald's and Hasok Chang's contributions (Buchwald 1992; Chang 2004).

The failure of concepts can be particularly instructive as to the surplus content they obtain when they are used in experimental contexts and out of the theoretical context in which they originally obtained their meaning.[6] When new experimental phenomena are discovered their very description and explanation is often achieved in terms of antecedently available concepts. This process, however, may sometimes lead to tensions and paradoxes that indicate the limitations of those concepts and the need for their revision.

Take descriptive failure first. A fascinating example can be found in the history of low-temperature physics. Helium was liquefied by Heike Kamerling Onnes in 1908. Some years later, during the 1930s, there were attempts to describe the behavior of liquid helium by using the established concept of viscosity. That concept was associated with the internal friction of fluids and had an operational dimension. There were two distinct methods for measuring viscosity: rotating a disc and observing the rate of its deceleration; and letting a liquid pass through tiny capillaries. Up to that point those methods had led to identical results. However, in the case of liquid helium below a certain temperature (2 degrees Kelvin) it turned out that those two methods led to fantastically different results. The "first gives a value that is a million times larger than the second" (Gavroglu 2001, 165). This discrepancy undermined the coherence of the theoretical and the operational dimensions of the concept of viscosity. The internal friction of liquid helium manifested itself only under the specific circumstances associated with the first method of measuring it. Under different circumstances, such as those associated with the second method, it vanished without a trace. This paradoxical situation indicated that liquid helium was not a normal fluid; rather it had to be reconceptualized as a "superfluid" (cf. Gavroglu/Goudaroulis 1988).[7]

The failure of the viscosity concept in providing a coherent description of low-temperature phenomena can be understood if we take into account the two-dimensional character of scientific concepts. Scientific concepts have a theoretical dimension—a description of the characteristics of their referents, and an operational dimension—specific ways of measuring those characteristics. Of course, these dimensions are not independent; rather, the latter is the "material realization" (Radder 1995, 69) of the former. Furthermore, if different material realizations are as-

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[6] Here I'm drawing on Gavoglu and Goudaroulis (1988).

[7] I would like to thank Kostas Gavroglu for a helpful discussion on this point.

sociated with the same concept, they should lead to the same results. For instance, there shouldn't be a discrepancy between two different ways of measuring temperature, using mercury and resistance thermometers. If that happened the coherence of the concept of temperature would be undermined. The emergence of incoherence in a concept is a sign of its failure to be applicable to "situations that are too distant from the kind of situation for which they were designed" (Kroon 2011, 182). As a result, the concept may split into two, or more, different concepts.

Concepts may also fail in the process of explaining new experimental results. An instance of this type of failure is provided by the 'discovery' of spin in 1925. Spin was suggested by the Dutch physicists Samuel Goudsmit and George Uhlenbeck in an attempt to make sense of the 'anomalous' Zeeman Effect, the patterns of magnetic splitting of spectral lines that did not conform to the predictions of the classical theory of electrons. Those patterns could be accommodated by the 'old' quantum theory of the atom if one assumed that the electron was a tiny charged sphere, whose internal rotation (spin) gave it magnetic properties. The experimentally indicated magnitude of the electron's magnetic moment was such that any point on the electron's surface should travel with a velocity about ten times the velocity of light! In other words, the new property attributed to the electron in the process of interpreting experimentally obtained information was incompatible with relativity theory. Thus, the concept of spin failed to meet certain theoretical constraints. That failure led to a reinterpretation of spin as a quantum mechanical property with no classical counterpart.

These examples show that experimentation is crucially involved in the formation and articulation of concepts. Even 'theoretical' concepts, such as the concept of the electron, are 'laden' with information obtained through observation and experiment. A focus on the experimental content of concepts will make it possible to understand their trans-theoretical character, the extent to which their meaning is independent from theory.

Before I proceed a distinction is in order. Scientific concepts come in, at least, two varieties. The first variety comprises concepts that are formed in the early, exploratory stages of the development of a field with a primarily descriptive and classificatory aim, namely to impose order in a domain of natural or experimentally produced phenomena. I have in mind concepts such as Dufay's 'two electricities' or Faraday's 'lines of force'. The generation of these concepts and the establishment of observable facts and regularities are two aspects of a single process.

For lack of a better term, we could call them 'phenomenal' concepts. A number of scholars, David Gooding and Friedrich Steinle among others, have ably discussed how such concepts are born, stabilized, and refined. So my focus here will be on a second variety of concepts that emerge in later, and perhaps more mature, stages of the investigative process. Their purpose is primarily explanatory, namely to account for previously established facts and regularities. Typically, they refer to hidden entities and processes that lie deeper than (and give rise to) the observable realm. Their articulation goes hand in hand with the construction of theories specifying the mechanisms or laws that govern the hidden realm in question.[8]

In the rest of this paper I will chart the various roles of experimentation in the articulation of the meaning of H-E concepts and in the stabilization of their reference. Departing from traditional philosophical accounts of the semantics of scientific terms, I will argue that essential aspects of the meaning and reference of H-E concepts can be understood only by examining their role in experimentation.

6. Experimentation and the Meaning of H-E Concepts

Experimentation plays a significant role in the formation and articulation of H-E concepts. New H-E concepts are introduced to make sense of experimentally produced phenomena and are, in turn, shaped by the information provided by the latter. For instance, after J. J. Thomson had put forward the concept of 'corpuscle' in order to account for various phenomena observed in the discharge of electricity in gases at low pressures, he inferred on the basis of quantitative information extracted from those phenomena that the corpuscle had a minute mass-to charge-ratio (three orders of magnitude smaller than the atom). Similar inferences were drawn by other experiments (e.g., by Zeeman on the magnetic influence on spectral lines) for related concepts, such as H. A. Lorentz's concept of 'ion'. The convergence of such experiment-driven results led to a unification of the 'corpuscle' and the 'ion', under the umbrella term 'electron', and a unified understanding of different phenomena as manifestations of electrons (Arabatzis 2006).

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[8] Sometimes, of course, the functions of phenomenal and hidden entity concepts may overlap. The former may play an explanatory role and the latter may facilitate the detection and description of novel phenomena.

Furthermore, when H-E concepts are created for theoretical (explanatory, predictive) purposes they are not fully articulated, either qualitatively or quantitatively.[9] The qualitative features that an H-E must have in order to bring about its purported effects are specified only to the extent that is required in order for them to play their explanatory role in the given context. Furthermore, the magnitude of those features is not determined in advance; rather it is inferred by the magnitude of the effects under investigation. Thus, H-E concepts are (forever?) incomplete and provisional in at least three ways. First, they do not specify exhaustively all the properties of their referents. When new experimental information is obtained it often turns out that the concepts in question have to be either refined or enriched in order to fulfill their explanatory function.[10] As an example of refinement consider Lorentz's 'ion'. Originally it referred to both positively and negatively charged particles, but as a result of Zeeman's magneto-optic investigations 'ions' were endowed solely with negative charge. Various examples of enrichment can be provided by the career of the concept of the electron in the 'old' (pre-1925) quantum theory. The challenges of experimental spectroscopy led to the incorporation of new properties, such as spin, in the concept of the electron. Second, H-E concepts are incomplete with respect to their quantitative characteristics. Experimental research often provides the information used to articulate quantitatively the concepts in question. For instance, as I already mentioned, experiments in magneto-optics and with cathode rays enabled the calculation of the charge to mass ratio of corpuscles/ions and their identification with electrons.[11] Third, experiment may lead to the retraction of some established properties of a H-E and, thus, to the adjustment of the associated concept. One may argue, for instance, that the experiments on electron diffraction in the late 1920s undermined the electron's particulate character.

Thus, experimentally obtained phenomena direct the articulation of H-E concepts by indicating, under certain theoretical assumptions, the kinds of properties that the referents of those concepts should have. Various features of experimental phenomena eventually find their counter-

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[9] Cf. Carnap's claim that the meanings of concepts become more fully specified as science develops (Carnap 1959 / Psillos 2000, 171).

[10] Cf. Radder 2006, 121: "[T]he meaning of concepts needs to be articulated when they are being extended or communicated to a novel situation." Cf. also Rouse 2011.

[11] Cf. van Fraassen (2008; 2009) on the "empirical grounding" of scientific theories.

parts in the putative properties and behavior of the H-E associated with them. For instance, when Niels Bohr put forward his model of the hydrogen atom, he made several assumptions about the properties of the electrons' orbits (e. g., only certain orbits, corresponding to the discrete structure of the hydrogen spectrum, were allowed). As Robert Millikan noted, "if circular electronic orbits exist at all, no one of these assumptions is arbitrary. Each of them is merely the statement of the existing experimental situation" (Millikan 1917, 209). To put it another way, on the assumption that a H-E exists, scientists construct the associated concept with an eye to the particularities of experimentally obtained information. In that sense H-E concepts are constructions from experimental data (see Arabatzis 2006, ch. 2).

7. The Experimental Life of H-E Concepts

Extending a point made by experimentalist philosophers of science about the relative independence of experimentation from theory, I would now like to suggest that the experimentally derived part of the meaning of a H-E concept is, to a significant extent, independent from theory. The experimentally produced component(s) of H-E concepts is often remarkably immune to changes in theoretical perspective. This is because the experimentally obtained information that is incorporated in H-E concepts can be robust across theory change. In the case of the electron, for example, its experimentally determined properties, such as its charge and mass, remained stable features of a concept which, in other respects, was in flux.

To understand the autonomy of the experimental life of H-E concepts, we need to understand the complexity of 'theory' and its relation to experiment. As philosophers of experiment have insisted, 'theory' is an accordion term covering various kinds of knowledge, ranging from general principles or laws that unify entire domains of nature to particular models of a phenomenon or an instrument. In the case of H-E concepts we should distinguish, I think, the following three levels of 'theory': First, the high-level theoretical framework in which those concepts are embedded. For instance, the concept of the electron was originally embedded within the framework of classical electromagnetic theory (Maxwell's laws plus Lorentz's force). The second level of theory concerns the representation of H-E, which provides an account of their nature. To stick with the same example, electrons were originally represented as sub-atomic singularities in the ether. Finally, the third level of 'theory' consists of the low-level knowledge that makes possible the identification of H-E in different experimental situations and the purported manipulations performed on (and with) them in the laboratory.

This final level of 'theory', whose robustness has been stressed by experimentalist philosophers of science (Hacking 1983; Cartwright 1983), is crucial for my argument about the autonomy of the experimental life of H-E concepts. Its significance for experimentation on (and with) H-E could be revealed by a mere glance at scientists' experimental reports. One is struck, for instance, by the paucity of high-level theory in C. T. R. Wilson's reports of his early 20th century experimental work on β-rays and x-rays. Wilson associated different cloud-chamber tracks with different particles on the basis of low-level considerations concerning the effect that the velocity of a particle and its scattering by atoms would have on its trajectory (Wilson 1923). In other cases cloud-chamber tracks of positrons were distinguished from those of protons by the width and length of their paths after they had been slowed down by lead. The "length [of a positron track] above the lead was at least ten times greater than the possible length of a proton path of this curvature" (Millikan 1947, 330). The inference from the manifest characteristics of a track to the identity of the underlying H-E was enabled by low-level facts about the differential deceleration of differently sized particles by a dense substance.

In the remaining part of this paper, I will suggest that we can capitalize on the experimental life of H-E concepts to resolve some of the long-standing difficulties concerning their synchronic and diachronic identity. Experiment may provide the key for stabilizing the referents of evolving H-E concepts.

8. Experimentation and the Identity of H-E Concepts: A Role for History

Let me start with an observation that can find ample documentation in the historical record. Scientists who disagree about the ultimate nature of a H-E may come to agree about its experimental manifestations and its experimentally determined properties. J. J. Thomson and H. A. Lorentz, for instance, disagreed about the ultimate nature of the H-E they had postulated; that is why they gave them different names (corpuscles versus ions). Thomson thought that corpuscles were structures in the ether, whereas Lorentz believed that ions were ontologically distinct from the ether. They both came to agree, however, on their experimental manifestations (e. g., in magneto-optics and in cathode-ray tubes) and on their experimentally determined properties (their charge and their mass).

The differentiation between the different levels of theory involved in the description and identification of a H-E can help us understand how this partial agreement between otherwise disagreeing scientists is possible. Disagreement is limited to some of those levels, whereas agreement is made possible by shared, usually low-level, background knowledge. In the Thomson and Lorentz case, their high-level disagreement concerned the deeper nature of electrons and did not preclude a low-level agreement about their observable effects. Their high-level disagreement about the irreducible or merely epiphenomenal character of, say, charge was compatible (or even made possible by) their agreement on how charged particles would behave under the influence of electromagnetic forces. This latter agreement made possible the unproblematic identification of electrons in different experimental situations.

An agreement about the identity of a H-E in an experimental setting can be possible even when there is disagreement about the high-level theoretical framework in which the associated H-E concept should be embedded. Walther Kaufmann's early 20th century experiments on β-rays provide a particularly instructive example of this possibility. Kaufmann's experiments aimed at resolving a disagreement among theoreticians about the laws obeyed by the electron. Different laws had been proposed about the precise effect of the electron's velocity on its mass and its shape. This disagreement, however, did not extend beyond a certain level. In particular, it did not involve the (rest) mass and charge of the electron, and the alteration of its motion by electromagnetic forces. Again, this shared background was responsible for the agreement of all concerned parties about the identity of the H-E in Kaufmann's apparatus (Staley 2008; Arabatzis 2009).

The different levels of 'theory' involved in experimentation and their different temporalities can elucidate a condition I have suggested for the referential stability of H-E concepts (Arabatzis 2006, 2011; cf. Psillos 2001, 85). According to that condition, an evolving H-E concept continues to refer to the same 'thing' as long as its experimental manifestations remain stable or grow (more or less) cumulatively.[12] This stability or cumulative expansion, even across ruptures in high-level theory, can be explained along three different lines. First, it may result from the robustness of low-level knowledge of the behavior of a H-E in various experimental settings. Second, it may be related to the enduring attribution of some key properties to a H-E. And, third, it may be the outcome of high-level theoretical continuity, where the theory that specifies the laws obeyed by a H-E is preserved, as a limiting case, in subsequent theories (Bartels 2010). Which of these possibilities obtains has to be investigated on a historical case by case basis.

Be that as it may, I hope to have shown that experiment provides various ways of establishing 'contact' with the referents of H-E concepts. In contrast with the CTR, however, these experimental ways do not fix the reference of H-E concepts once and for all. Future developments may always reveal flows in the knowledge that kept together different phenomena as manifestations of the same H-E. If the bonds between those phenomena dissolve, the H-E that purportedly gave rise to them will turn out to have been a merely useful fiction (cf. Feest 2011).

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[12] The qualifier "more or less" is meant to allow some room for error. A mistaken attribution of an experimental phenomenon to a H-E need not throw doubt on its identity.

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