Erkenn (2011) 75:413–429
DOI 10.1007/s10670-011-9340-9

# The Persistence of Epistemic Objects Through Scientific Change

**Hasok Chang**

Received: 27 September 2011 / Accepted: 27 September 2011 / Published online: 27 October 2011


**Abstract** Why do some epistemic objects persist despite undergoing serious changes, while others go extinct in similar situations? Scientists have often been careless in deciding which epistemic objects to retain and which ones to eliminate; historians and philosophers of science have been on the whole much too unreflective in accepting the scientists' decisions in this regard. Through a re-examination of the history of oxygen and phlogiston, I will illustrate the benefits to be gained from challenging and disturbing the commonly accepted continuities and discontinuities in the lives of epistemic objects. I will also outline two key consequences of such re-thinking. First, a fresh view on the (dis)continuities in key epistemic objects is apt to lead to informative revisions in recognized periods and trends in the history of science. Second, recognizing sources of continuity leads to a sympathetic view on extinct objects, which in turn problematizes the common monistic tendency in science and philosophy; this epistemological reorientation allows room for more pluralism in scientific practice itself.

## 1 Prologue: The Historicity of Epistemic Objects

The world as we know it is populated by *epistemic objects*, by which I mean entities that we identify as constituent parts of reality. I use the designation "epistemic" as relating to the human process of seeking knowledge, as an indication that I wish to discuss objects as we conceive them in our interaction with them, without a presumption that our conceptions correspond in some intractable sense to the shape of an "external" world that is entirely divorced from ourselves. Therefore my usage follows that of Hans-Jörg Rheinberger (1997), rather than the custom common

H. Chang (✉)
Department of History and Philosophy of Science, University of Cambridge, Free School Lane, Cambridge CB2 3RH, UK
e-mail: hc372@cam.ac.uk

among analytic philosophers according to which "epistemic" implies "truth-bearing". Along vaguely Kantian lines, we may say that metaphysical objects-in-themselves are forever out of our reach, while epistemic objects constitute nature-as-phenomena, or nature as we know it. Yet, in a very un-Kantian way, as Rheinberger has stressed, epistemic objects (or, epistemic things, as he prefers to call them) have a historicity about them. As we continue to learn about nature, various epistemic objects come into being, and they change and evolve; in this process they have a capacity to surprise us by revealing new and unexpected aspects about themselves.

Rheinberger's view is that once epistemic objects/things are completely established and understood, they turn into *technical objects* with stable and reliable properties, which can be used in the study of other epistemic objects (1997, pp. 28ff, esp. p. 33). Without denying the existence of this petrifying tendency, I would like to focus my attention in this paper to the possibility of epistemic objects becoming *less* stable, sometimes getting phased out altogether. There are enough well-known cases of extinct epistemic objects: ether, caloric, phlogiston, the four humors, entelechy, etc., which "died" as a result of significant scientific change. Meanwhile, certain other epistemic objects have survived through equally serious changes, even revolutions, which have actually introduced serious changes in their meanings. Electrons provide a prime example of these survivors, as Theodore Arabatzis (2006) has discussed in detail; there are many others, such as atoms, genes, energy, acids, and oxygen.

In considering the historicity of epistemic objects, I start with a puzzle: why do some epistemic objects die, and others survive? Now, since epistemic objects are objects as existing in our conceptions, what I say about their life and death has no implication about anything coming in and out of existence in a mind-independent and metaphysical sense. Rather, what I have in mind is the epistemic decisions we make about what we *presume* to be real in our dealings with the world. If I simply said "concepts" instead of "epistemic objects", many of the arguments made in this paper would still go through. However, it is important to recognize epistemic objects as objects; what we presume to be real functions as such, not only in our reasoning but also in our material practices. Here, again, I follow Rheinberger (2005, p. 406) in stressing "the power of material objects—in contrast to ideas or concepts—as driving forces in the process of knowledge acquisition." Epistemic objects serve that role "by virtue of their opacity, their surplus, their material transcendence…, which is what arouses interest in them and keeps them alive as targets of research."

Yet it is also important to keep in mind that epistemic objects are our inventions (allowed by the cooperation of nature), and how we try to manage them is also a matter for our choice: we can let them continue to develop in unpredictable ways, or fix their meaning precisely by definition, or eliminate them altogether, and so on. The consideration of this choice enhances Rheinberger's own sense (1997, pp. 29–30) that the distinction between epistemic things and technical objects is a fluid and contextual one. Decisions regarding the cultivation of epistemic objects are important enough to demand careful and conscious deliberations. Yet scientists have often been careless in retaining certain epistemic objects despite serious meaning-changes and discarding others despite significant continuities, and historians and philosophers of science have been much too unreflective in accepting the scientists' decisions in this regard. My underlying stance is that we need to re-examine, challenge and disturb the commonly accepted continuities and discontinuities in the lives of epistemic objects—for the sake of better historiography, better epistemology, and better science. I hope that the discussion in the remainder of this paper will illustrate and validate that stance.

## 2 Is Lavoisier's Oxygen Our Oxygen?

Much of my thinking on the subject of epistemic objects first arose from my current work revisiting the history of the so-called Chemical Revolution.[1] As knowledgeable historians of chemistry are well aware, the popular image of Antoine-Laurent Lavoisier discovering oxygen as we know it is seriously misleading. There are many aspects to the misconception, even if we set aside the well-known question of priority. Two points are particularly striking. First, what Lavoisier conceived as "oxygen gas" was full of caloric, by virtue of which oxygen served as the source of heat released in combustion according to his theory. Second, if we set caloric aside, what is left is "oxygen base", which was for Lavoisier the essence of acidity. Within half a century of Lavoisier's death, these pillars of his chemistry were knocked out. John McEvoy puts the point even more strongly (1997, pp. 22–23): it is "a simple fact" that already "by the end of the eighteenth century, almost every major theoretical claim that Lavoisier made about the nature and function of oxygen was found wanting." The meaning of "oxygen" changed dramatically after Lavoisier's death, without anyone crying "revolution". Whether anything much was left of Lavoisier's original concept of oxygen by the latter half of the nineteenth century is a serious question. Therefore we need to ask whether there has been a coherent and lasting epistemic object called "oxygen", or merely a linguistic term that has been retained without a continuity of epistemic meaning; my answer will be that there has been a sufficient continuity of meaning to warrant the preservation of the same term, but only at the operational level and not at the theoretical level.

We need to think carefully before blithely repeating that Lavoisier (or anyone else) discovered oxygen in the late eighteenth century. What Lavoisier considered the most definitive characteristics of oxygen are not regarded as characteristics of oxygen at all by modern chemists. Oxygen is now defined by the number of protons that each of its atoms has; from that basic microphysical configuration other properties follow, such as the number and configuration of electrons in each oxygen atom, and consequently most of the chemical properties of oxygen. If we consider how Lavoisier invented and used the concept of "oxygen", we can discern two essential features. (1) Oxygen, or *oxygen base* to be precise, has a strong affinity for caloric (the material fluid of heat), yet an even stronger affinity for combustible substances. Therefore oxygen gas, which consists of oxygen base combined with a great amount of caloric, supports combustion: when oxygen base unites with a

[1] For further details, see Chang (2009b), Chang (2010), and Chang (2012), chapter 1.

combustible substance, it releases the caloric (and light) that it was previously combined with. (2) For Lavoisier (1965, pp. 51, 64–65), oxygen base is also the principle of acidity; that is to say, acids are made through the composition of oxygen with other substances, and oxygen is the ingredient responsible for the acidity of the compound. If this Lavoisierian concept of oxygen is essentially linked to the modern one, the link is at least not a simple one.

At first glance, it would seem that chemists should have at least re-named oxygen after they rejected Lavoisier's theory of acidity (though I will argue partly against this intuition later). Lavoisier's neologism "oxygen", meaning acid-generator, is an embarrassingly loud advertisement for an outmoded chemical theory. The term comfortably survives in English only because most English speakers have no idea of its etymology. What really puzzles me is how Germans have allowed the term *Sauerstoff* to persist.[2] When I hear people say Lavoisier discovered oxygen and heralded modern chemistry, I am reminded of a story I once read as a child, about an itinerant at a time of famine. Arriving at a certain village, he declared to the villagers: "I can show you how to make a soup with stones. Just get me a big pot, some water, and some stones. Then we will have a nice meal together." He started boiling the stones, then asked for some seasoning. Then he said, "Oh, it would be better if we had a few vegetables to enhance the flavor. And do you have any kind of meat as well? That would be even better. Potatoes or dumplings would add a nice touch, too." So all these things were added, and a delicious soup was made. "Let's eat", said the beggar happily, taking a large bowl of the soup for himself. "What do we do with the stones?", someone asked. Casually, the man answered, "Oh, you can't eat the stones. Just take them out."

But there is a standard philosophical response to this kind of situation, in a tradition of philosophy of language reaching as far back as Frege: although the theoretical meaning of "oxygen" (or its sense, or stereotype) has changed, its *reference* is still the same. For generic substance terms, the constancy of reference comes down to the idea that the extension of the term has remained constant. In other words, whichever substances Lavoisier called "oxygen", we still call "oxygen", and vice versa, even though our understanding of the nature of the substance so designated differs significantly from Lavoisier's understanding. But this is not so straightforward. First of all, as already mentioned, Lavoisier did not have just one term for oxygen, but two: "oxygen gas" and "oxygen base", which clearly had different extensions. So if we want to say that our "oxygen" has the same reference as Lavoisier's "oxygen", we first need to know for which Lavoisierian oxygen we would like this to be true. This is not an easily removable ambiguity, as we can see from Lavoisier's famous table of elements (or rather, simple substances) published in his definitive textbook of chemistry in 1789, shown in Fig. 1. In this table he lists old terms corresponding to the new terms proposed by him and his colleagues, and for their neologism "oxygen" he lists the corresponding old terms "Air déphlogistiqué, Air empiréal, Air vital, Base de l'air vital"—both vital air and its base![3]

[2] It would have been better to go with Scheele in calling the stuff "fire air", or to follow Oersted's example in coining a more sensible term in one's own language.

[3] Lavoisier does the same for hydrogen and nitrogen, too.

Modern chemistry does not assign any meaning to Lavoisier's phrase "oxygen base", as it does not recognize oxygen gas as a compound of oxygen base and a prodigious amount of caloric. So if we want to know the extension of "oxygen base", we have to go back to Lavoisier's own system of chemistry. Now, Lavoisier considered that pure oxygen base was not obtainable (somewhat like free quarks in today's quantum chromodynamics), as there was always going to be some caloric combined with it. So it was impossible to specify the extension of "oxygen base" by ostension (simply pointing to tokens of oxygen base when they occurred), and the best method would have been to point to the presumed composition of observable substances containing oxygen base. In fact Lavoisier could only ever obtain oxygen in its gaseous form, though he theorized that the extraction of sufficient amount of caloric would turn any gas into liquid and then solid. Taking heed of the rest of Lavoisier's theory, too, we can see that oxygen base would be co-extensive with the following set:

1. oxygen gas − caloric
2. acid − radical
3. calx − metal
4. water − hydrogen,

where ($a - b$) indicates "what one obtains by removing $b$ from $a$" (disregarding the ubiquitous residual caloric contained even in liquids and solids).

Taking (3) and (4) alone, it may seem that we could identify the reference of Lavoisier's "oxygen base" with that of the modern expression "oxygen atom". But (1) and (2) are meaningless formulations in modern chemistry, with the empty set as the extension in each case. For example, the extension of Lavoisier's "oxygen base" includes the substance one would get by subtracting the "muriatic radical" from "muriatic acid" (which we now call hydrochloric acid); Lavoisier was so sure about the existence of the muriatic radical that he included it in the table of chemical elements (see Fig. 1). The extension of "oxygen" today certainly does not include this presumed component of hydrochloric acid, since we don't think there is such a thing as the muriatic radical. The point can be put even more strongly: today we would not give any conceivable constituent of $HCl$ a name that had anything to do with oxygen. One might say that the extension of "oxygen" has remained the same except in such strange cases, but that only comes to saying that the extension of "oxygen" has remained the same except where it hasn't. (And, of course, Lavoisier was a genius, except when he wasn't.) In fact (1) and (2) above are not aberrant cases at all (like albino tigers and such); on the contrary, they give the two most essential theoretical meanings of oxygen in Lavoisier's system.

Do we have better hope at referential continuity if we consider oxygen gas rather than oxygen base? The case seems more promising at first glance, since we presume that Lavoisier must have had jars of stuff that we, too, would surely call "oxygen gas" if we could have meaningful contact with it. But what is the source of our confidence here? One can follow the causal theory of reference if one likes, and say that the extension of "oxygen gas" is the set of all the bodies that "bears a particular 'sameness relation'" to the initial samples that Lavoisier christened "oxygen gas". For natural kind terms, Kyle Stanford and Philip Kitcher cash out this "sameness"

| | *Noms nouveaux.* | *Noms anciens correſpondans.* |
|---|---|---|
| *Subſtances ſimples qui appartiennent aux trois règnes & qu'on peut regarder comme les élémens des corps.* | Lumière | Lumière. |
| | Calorique | Chaleur.<br>Principe de la chaleur.<br>Fluide igné.<br>Feu.<br>Matière du feu & de la chaleur. |
| | Oxygène | Air déphlogiſtiqué.<br>Air empiréal.<br>Air vital.<br>Baſe de l'air vital. |
| | Azote | Gaz phlogiſtiqué.<br>Mofete.<br>Baſe de la mofete. |
| | Hydrogène | Gaz inflammable.<br>Baſe du gaz inflammable. |
| *Subſtances ſimples non métalliques oxidables & acidifiables.* | Soufre | Soufre. |
| | Phoſphore | Phoſphore. |
| | Carbone | Charbon pur. |
| | Radical muriatique. | Inconnu. |
| | Radical fluorique | Inconnu. |
| | Radical boracique | Inconnu. |
| *Subſtances ſimples métalliques oxidables & acidifiables.* | Antimoine | Antimoine. |
| | Argent | Argent. |
| | Arſenic | Arſenic. |
| | Biſmuth | Biſmuth. |
| | Cobolt | Cobolt. |
| | Cuivre | Cuivre. |
| | Etain | Etain. |
| | Fer | Fer. |
| | Manganèſe | Manganèſe. |
| | Mercure | Mercure. |
| | Molybdène | Molybdène. |
| | Nickel | Nickel. |
| | Or | Or. |
| | Platine | Platine. |
| | Plomb | Plomb. |
| | Tungſtène | Tungſtene. |
| | Zinc | Zinc. |
| *Subſtances ſimples ſalifiables terreuſes.* | Chaux | Terre calcaire, chaux. |
| | Magnéſie | Magnéſie, baſe du ſel d'Epſom. |
| | Baryte | Barote, terre peſante. |
| | Alumine | Argile, terre de l'alun, baſe de l'alun. |
| | Silice | Terre ſiliceuſe, terre vitrifiable. |

**Fig. 1** Lavoisier's table of simple substances, from Lavoisier (1789, 192); p. 175 in the English translation (1965)

relation in terms of having the same "inner constitution" that is causally relevant to producing the characteristic observed properties of the substance (Stanford and Kitcher 2000, pp. 108, 114). In order to satisfy modern chemistry, we would have to say that the inner constitution referred to here is having atomic number 8. But why should we think that Lavoisier was getting at anything like that, when he did not have a chemical atomic theory, not to mention the concept of atomic number?

Stanford and Kitcher (2000, p. 114) give a refined version of the causal theory of reference which allows "people who are ignorant of underlying structures to partition the total cause"; according to this conception, "term introducers make stabs in the dark", and "*conjecture* that there's some underlying property (or 'inner structure') that figures as a common constituent of the total causes of each of the properties" that are typically exhibited by the substance in question. But there is some futility here, as Lavoisier was not even making any conjectures about the inner structure of oxygen (except that oxygen gas consists of oxygen base and caloric),[4] and our confidence about the referential continuity does not arise from any confidence we might have about Lavoisier's ideas about the inner structure of oxygen.

The continuity about the meaning of "oxygen gas" exists at the *operational* level, or in the realm of *epistemic activities* (again in line with Rheinberger's kind of way of thinking about scientific practice).[5] And what we have here is not merely (presumed) referential continuity, but semantic continuity of a broader, more tangible and more secure kind. All of the procedures that Lavoisier had used for producing and identifying oxygen gas are still repeatable and valid; that is also to say, most of the observable properties of oxygen gas noted by Lavoisier are also still recognized today. Heat some red oxide of mercury intensely; collect the evolving gas in a glass jar; see things burn with special vigor in that gas, and animals live longer; breathe it and feel a lightness in the lungs; explode it together with hydrogen gas and make water. This operational stability is what is responsible for fixing the extension or reference of "oxygen", to the extent that it has been fixed over the centuries. Reference-fixing, at least in this case, is fully tied to observable properties.

If we accept the operational source of the continuity of oxygen as an epistemic object, a couple of interesting consequences follow. The first consequence of locating the continuity of oxygen at the operational level is a further question: how does it come about that there is such a degree of operational stability? That comes down to the fact that the operations of eighteenth century pneumatic chemistry were such that they still survive to this day, and I think this has something to do with the fact that they are not very different in kind from operations that we carry out in everyday life. This line of thinking is explored further in another place.[6] Secondly, it is a straightforward matter to observe that the operational meaning of Lavoisier's "oxygen" was pretty much the same as that of Joseph Priestley's "dephlogisticated air", or Carl Wilhelm Scheele's "fire air". So it makes sense that the chemists of the late eighteenth century had little trouble communicating with each other about their research regarding these epistemic objects, and that there was no strong semantic

[4] This makes an important contrast to the case of the term "acid" (from Arrhenius onward), which Stanford and Kitcher (2000, pp. 115–120) discuss at some length.

[5] What I mean by "operational" goes back to Percy W. Bridgman's work, as explicated in Chang (2009a). My own conception of "epistemic activity" is yet to be fully spelled out, but some indication is given in Chang (2008).

[6] For a preliminary attempt, see "Acidity: The Persistence of the Everyday in the Scientific", presentation at the joint meeting of the Philosophy of Science Association and the History of Science Society, 4 November 2010.

incommensurability between their paradigms even as they maintained serious theoretical disagreements. This is also why we can quite comfortably celebrate all of Scheele, Priestley and Lavoisier as co-discoverers or independent discoverers of oxygen.

## 3 What if Phlogiston had been Kept?

Everything I have said so far should be relatively uncontroversial, though perhaps unusual and hopefully refreshing. But it does lead directly to a much more controversial point. If the lasting integrity of oxygen as an epistemic object was provided by the stability of operations involving it and not by anything else, then we have to admit that *phlogiston* had just the same type of basis for lasting integrity. We can still successfully repeat most of the operations that gave phlogiston its meaning in the eighteenth century, such as the reduction of metallic calxes by means of charcoal, or the production of inflammable air from metals. (I am currently carrying out some of these operations myself in reproductions of historical experiments.) In other words, there was no convincing reason for chemists to kill phlogiston in the late eighteenth century—at least no more convincing reason than there was to kill oxygen in the early nineteenth century. This bold claim, of course, needs a full justification, which is provided in the first chapter of my forthcoming book, *Is Water $H_2O$?* For now, it may be sufficient to wonder what would have happened to phlogiston, if Priestley had remained advisor to Lord Shelburne until the latter became prime minister, and had the kind of political inclination to embed in general society the use of his nitrous air test as a way of measuring the goodness of air. (And what would have happened to the luminiferous ether, if it hadn't been for the unique scientific and cultural phenomenon whose name was Albert Einstein?)

So much is a question of possibility, and of "rights"—now comes the question of desirability, or usefulness. Consider the oxygen side of the story first: why shouldn't chemists have killed off oxygen as an epistemic object as they eliminated both the Lavoisierian theory of combustion and the Lavoisierian theory of acidity? Why shouldn't they have moved on to a new epistemic object with a new name and a new meaning, consigning Lavoisierian oxygen to the dustbin of history just as they did dephlogisticated air and fire air? Now, I am willing to grant that oxygen contributed to chemistry by surviving and adapting, rather than dying. But that willingness comes with two obligations: first, to spell out what exactly post-Lavoisier oxygen did contribute to chemistry, and second, to try to see if we can plausibly extend the same courtesy to phlogiston.

So, first, how did the retention of oxygen help chemistry? The comparison-and-contrast is with the hypothetical situation in which scientists killed off Lavoisier's oxygen and instead put in a chemical element "negyxo", which had the operational meaning of oxygen but without the theoretical meaning linked to Lavoisier's theories of combustion and acidity. This question is actually quite difficult to answer, and I will not attempt a full answer here, but just note a couple of tentative points. First, the concept of "oxidation" was good to preserve because it was later usefully extended to become something deeper and more general than "combination with oxygen". Second, Lavoisierian oxygen played an important role in J. J. Berzelius's system of chemistry, leading to the generalized theory of radicals transcending its origin in Lavoisier's conception of an acid as a dualistic combination of oxygen with a radical (the latter being specific to each type of acid). Neither of these benefits would easily have followed if chemists had simply dropped Lavoisier's oxygen and adopted a rather featureless "negyxo"; the developmental potential was realized because chemists only *gradually* moved away from Lavoisier's outdated concepts. I am not entirely convinced by this line of thinking, but that is the best I can do.

Surprisingly, it is somewhat easier to discern what good it might have done to keep *phlogiston* beyond the time of its actual death. I will make two points relatively briefly, invoking a couple of famous dead chemists along the way to generate an air of authority.

First, phlogiston would have served as a reminder that there was more to chemical reactions than the grouping and re-grouping of gravimetric building-blocks. Whiggishly speaking, phlogiston served as an expression of chemical potential energy, which the weight-obsessed oxygen theory completely lost sight of. The Lavoisierian tradition was actually quite unstable on this count. For example, Lavoisier sowed the seed of the destruction of his own theory of combustion, by putting so much emphasis on weight and then assigning no weight to caloric. Lavoisier's theory of combustion in fact never got very far in explaining the release of heat and light in combustion, without the concept of energy available. But chemists should not have needed to wait for the likes of Mayer, Joule and Helmholtz to help them think about energy. If phlogiston had lived, it could have given chemists a productive open end to start thinking about something like energy. As Douglas Allchin (1992) stresses, some people during the time of the Chemical Revolution, particularly in Germany, did try to preserve this avenue of thought by accepting oxygen for weight considerations but keeping phlogiston for what we would now call energy considerations. J. R. Partington and Douglas McKie (1937–1939) already pointed to a large number of people in this category, including Gren, Crell, Richter and Gadolin.

William Odling made the same point in a most interesting paper from 1871. Although not a household name today, Odling was one of the leading theoretical chemists of Victorian Britain, and at that time the Fullerian Professor of Chemistry at the Royal Institution. According to Odling (1871, p. 319), the major insight from the phlogiston theory was that "combustible bodies possess in common a power or energy capable of being elicited and used", and that "the energy pertaining to combustible bodies is the same in all of them, and capable of being transferred from the combustible body which has it to an incombustible body which has it not". Lavoisier had got this wrong by locating the energy in the oxygen gas (in the form of caloric), without giving a convincing account of why caloric contained in other gases would not have the ability to support combustion. Odling (1871, p. 322) thought that "the Stahlians, though ignorant of much that has since become known, were nevertheless cognizant of much that became afterwards forgotten." He also cited Alexander Crum-Brown as having the same view that "there can be no doubt" that potential energy was what the earlier chemists "meant when they spoke of phlogiston". (This I admire as a *properly* whiggish view of the history, compared to the triumphalism of celebrating Lavoisier just because he won.[7])

Admitting that phlogistonists tended to conceive of phlogiston as a material substance, Odling questioned whether this was meant in such an ordinary way: "though defining phlogiston as the principle or matter of fire,… they [Stahlians] thought and spoke of it as many philosophers nowadays think and speak of the electric fluid and luminiferous ether." In any case, Odling (1871, pp. 323–324) thought this substance-talk could be pardoned:

> That Stahl and his followers regarded phlogiston as a material substance, if they did so regard it, should interfere no more with our recognition of the merit due to their doctrine, than the circumstance of Black and Lavoisier regarding caloric as a material substance, if they did so regard it, should interfere with our recognition of the merit due to the doctrine of latent heat.

Although phlogiston was clearly not exactly chemical potential energy as understood in his own time, Odling (1871, p. 325) argued that "the phlogistians had, in their time, possession of a real truth in nature which, altogether lost sight of in the intermediate period, has since crystallized out in a definite form." He ended his discourse by quoting Becher: "I trust that I have got hold of my pitcher by the right handle." And that pitcher, the doctrine of energy, was of course "the grandest generalization in science that has ever yet been established."

The other point I would like to make about the possible benefits of keeping phlogiston concerns its connection with electricity. There was one clear area of "Kuhn loss" in the Chemical Revolution: one important thing that the phlogiston theory did well and the oxygen theory did not do so well was to explain the common properties of metals, by saying that all metals were rich in phlogiston.[8] As Paul Hoyningen-Huene puts it (2008, p. 110): "Only after more than a hundred years could the explanatory potential of the phlogiston theory be regained in modern chemistry. One had to wait until the advent of the electron theory of metals towards the end of the nineteenth century." But the phlogistic account has a close resonance with the modern notion, that all metals share metallic properties because they all have a "sea" of free electrons. If we were to be truly whiggish, we would recognize phlogiston as the precursor of free electrons.

The phlogiston–electricity connection is actually not at all a retrospective fabrication by whiggish historians or philosophers. Allchin (1992, p. 112), following William M. Sudduth (1978), identifies no fewer than 23 people who postulated a close relationship between phlogiston and electricity in the eighteenth century. There were some good motivations for this identification (even aside from the common desire to find a grand unity among all the imponderables): for example, it was found that electricity could be used to reduce calxes to metals, which was a role performed by phlogiston. For such reasons, the English chemist John Elliott (1780, p. 92) even proposed that phlogiston should be called "electron".[9] Later on, when

7 See Chang (2009b) for further reflections on this historiographical point.

8 Kuhn (1970), 157.

9 This quirky fact I owe to Partington and McKie (1937–1939, p. 350).

the electrolysis of water in 1800 ended in a puzzle about why the oxygen and hydrogen gases were produced at separate places, Johann Wilhelm Ritter's answer was that hydrogen gas was a compound of water and negative electricity, and oxygen a compound of water and positive electricity; this lined up exactly with Cavendish's earlier notion that hydrogen was phlogisticated water, on making the identification of phlogiston with negative electricity.[10]

If phlogiston had survived, and its association with electricity maintained, I am confident that nineteenth century scientists would have made attempts to isolate the electric fluid from phlogiston-rich substances such as metals, using any plausible means at their disposal. Would it not have occurred to someone to hit the surface of a metal with powerful ultraviolet rays (already discovered in 1802) in an attempt to disengage phlogiston? As soon as there were sensitive enough electrometers, the photoelectric effect would have been detected. What about trying to run an electric current between two electrodes across a near-vacuum, a very familiar sort of thing from the traditional practice of drawing sparks from static electricity? Is it too irresponsible to speculate that cathode rays would have been discovered and investigated very early on in this way? Elliott would have been pleased to congratulate my imaginary investigators for the experimental isolation of the "electron".

To show, again, that it is not only mad philosophers of science who have these wild thoughts about phlogiston, I quote the American chemist Gilbert Newton Lewis (of the "octet rule"), who said (1926, pp. 167–168):

> If they [the phlogistonists] had only thought to say "The substance burning gives up its phlogiston to, and then combines with, the oxygen of the air," the phlogiston theory would never have fallen into disrepute. Indeed, it is curious now to note that not only their new classification but even their mechanism was essentially correct. It is only in the last few years that we have realized that every process that we call reduction or oxidation is the gain or loss of an almost imponderable substance, which we do not call phlogiston but electrons.[11]

All in all, the survival of phlogiston into the nineteenth century would have sustained a vigorous alternative tradition in chemistry and physics, which would have hastened the birth of other useful epistemic things like energy and electrons. It would have been at least no less productive than the retention of oxygen was.

## 4 Historiographical Implications

The cases of oxygen and phlogiston discussed above are highly suggestive. It seems that that the scientific community at the time of the Chemical Revolution did not act on the basis of consistent principles or policies, or even with any clear epistemic awareness, in deciding which of these epistemic objects to retain and which to

[10] For full details on this episode, see chapter 2 of Chang (2012).

[11] I thank Patrick Coffey for alerting me to this passage.

discard, in the face of changes that threatened the identity of each. In such cases, historians and philosophers of science have an intellectual obligation to disturb the rather groundless decisions made by scientists, even just to see what consequences follow from such disturbance. If the cases of oxygen and phlogiston are typical at all, there will be a great deal of such consequence-seeking work to do.

There are immediate historiographical consequences. Recently many historians of science have stressed that an object-focus gives us useful novel historiographical perspectives (e.g., Rheinberger 1997; Daston 2000; Klein and Lefèvre 2007). This can only be even more so if we are also willing to challenge the accepted history of the objects in question. The Chemical Revolution is by no means the only episode that deserves such re-examination. Historical epochs are marked out by epistemic objects just as much as by people, institutions or theories, so where we recognize continuities and discontinuities in epistemic objects does affect our historiography in substantive ways. It will not do simply to follow "actors' categories" in a superficial way, any more than we can make an adequate framework for good political history by observing that the United Kingdom has been called a "kingdom" for many centuries. As historians, we need to scrutinize the continuities and discontinuities implied in the terminology used by scientists.

First of all, we need to examine whether terminological continuity is matched by an actual stability and coherence of theoretical and experimental practices. It might seem that there is no harm in keeping the same word as long as everyone understands that its meaning has changed, but terminological inertia has significant consequences. In the case of oxygen, if it had been generally accepted that Lavoisierian oxygen (just like phlogiston) ceased to exist as a cogent epistemic entity by the early nineteenth century, would people have felt such a strong temptation to call Lavoisier "the father of modern chemistry"? As Bernadette Bensaude-Vincent has noted relatively recently (1996, p. 482), the "intense scholarship in the historiography of the Chemical Revolution has not sufficed to discredit" this image of Lavoisier, which "still reigns supreme in the collective memory of professional chemists, as least in France." I suspect that the mythology of oxygen and phlogiston has been an important factor in the perpetuation of the popular notion that Lavoisier made a decisive break from previous chemistry. Without the underlying idea that the coming of oxygen and going of phlogiston represented some radical change, how happy would we be to talk about the Chemical Revolution, which James Bryant Conant (1957) famously summed up as "the overthrow of the phlogiston theory"? The best-informed historians today are quite wary of the revolutionary label, and this historiographical maturity could have been reached more easily by a more direct critical view on the continuity and discontinuity in the lives of oxygen and phlogiston.

I will further illustrate these historiographical consequences by means of another example: atomism. On the one hand, we need to emphasize the discontinuities masked by the persistence of the term "atom". We certainly do not have an unchanged epistemic object from the ancient Greek discourse about "atoms". And a *little* knowledge would tell us that it is incorrect to trace the origin of modern chemical atomism even as far back as John Dalton, whose atoms had plenty of caloric, no electrons, and all the wrong weights and sizes.[12] On the other hand, there *is* actually some continuity that justifies the use of the same term through the ages. The nature of this continuity needs to be recognized clearly: the common core of all the different notions of "atom" is that atoms are discrete building-blocks which make up more complex things while preserving their identities so they can be taken out again intact. What all the different concepts of "atom" share is not *ultimate* indestructibility or indivisibility, but robustness within each of the particular epistemic activities that each concept is involved in—whether it be the mental construction of explanatory models of phenomena, or down-and-dirty operations of analytic and synthetic chemistry. Once again, the stable core of meaning, for "atom" as much as "phlogiston" or "oxygen", is rooted in something quite concrete and practical, even if sometimes mental: operations of decomposition and recomposition.

Looking back at the history of atomism, then, it is really not so important whether the historical actors used the word "atom" or not. They might have variously said (and did say) "corpuscles", "particles" or "molecules". Whatever the terminology, the big question is whether and how people employed atomistic building-blocks in their experimental and theoretical operations. We can observe a few major trends from the early modern period onward. The mechanical philosophers of the seventeenth century made a conscious effort to employ atomistic building-blocks, overly speculative as they may have been. But much successful chemistry in the eighteenth century took place in the tradition of "principlism", which was not atomistic; meanwhile in physics flourished various theories of subtle fluids, which were quantifiable (even conserved in quantity) yet usually not made up of identifiable and robust discrete units. The big wave of experimental atomism arose slowly and gradually, eventually outstripping principlism and displacing non-atomic subtle fluids; key figures in that atomistic takeover include Geoffroy, Lavoisier, Haüy, Dalton, Berzelius, and many others.[13] Two centuries of highly successful reductionistic chemistry and physics followed, during which even many philosophical anti-realists have reasoned and experimented in terms of atoms while they were doing their science. These suggestions need to be backed up by detailed historical research, but I think already they illustrate how much productive new work may be elicited by the fresh perspective proposed here, even on a subject that has been "done to death".

## 5 Epistemic Pluralism

So much for historiography. What epistemic and scientific consequences follow from challenging commonly accepted continuities and discontinuities in the lives of epistemic objects? Of course, that depends on *how* we challenge them. In this section I would like to convey just one point, which I put into a rhetorical and

[12] For details about various aspects of Dalton's atomism, see Cardwell (1968).

[13] Various historians have traced the rise of the building-block ontology in chemistry; for example, see Siegfried (2002) and Klein (1994, 1996).

metaphorical question: why are we so ready to kill? So far I have indicated that sometimes scientists retain an epistemic object (with modifications) when they could also decide to eliminate it, and sometimes they eliminate it when they could retain it. However, I think there has been an unwarranted and unproductive tendency toward elimination. This is why the history of science looks like a graveyard of dead epistemic objects. The frequency of elimination has more to do with scientists' predilections than anything about the nature of nature, or anything inevitable about the course of scientific development.

I am not going to make an exhaustive survey of all epistemic objects in the history of science in an attempt to support my claim that unnecessary killing is more prevalent than unwarranted preservation. Instead, I will make an observation about a philosophical–psychological ideal held by many scientists which explains why killing would be rife. This ideal is a certain kind of monism: that we should have only one theory about a given domain of nature.[14] Not every scientist is a monist in this sense, but it is a sufficiently widespread presumption that if we have the correct theory in place, all other (genuinely different) theories in that domain must be eliminated. Even admitting that they do not know whether they are in possession of the ultimately true theory, scientists still tend to think that if one of the competing theories is clearly better than the others, then the latter need to be eliminated.

Philosophers are often wedded to this kind of monism, too. Consider, for example, the widespread discourse on the inference to the best explanation: inferring to the best theory is usually taken as an indication of the falsity of all the less-good theories, or at least a recommendation for their rejection. Even among philosophers who do not think science deals in "truth", there is a notion that scientists ought to work with only one theory at a time. The emblematic example here is Kuhn, with his insistence that a paradigm does and should enjoy a monopoly within a given field of science in its "normal" phases; extraordinary science, in which competing paradigms co-exist, is a temporary and uncomfortable phase which inevitably settles into another phase of normal science. When a theory or a paradigm is eliminated, the epistemic objects that populate it are threatened. Thus, whenever there is revolutionary change in science, it is likely that there will be dead epistemic objects.

Imre Lakatos is the exception that proves the rule here: against Kuhn he maintains that there should always be multiple research programmes in a field of science; however, this is only so that these programmes can *compete* with each other, so that scientists can choose the best (most progressive) one at the end of the process. Lakatos does not explain why there should be an "end" to the process of scientific research; that just comes as part of the conceptual framework of theory-choice. But why are we so obsessed with *choice*? Why do we need to choose between different alternatives in a strong, exclusive sense? Why can't "choice" be a more relaxed matter of each scientist or each group of scientists deciding which avenue of investigation to take, without implying that all the other avenues are inferior and should be closed off? And if it is a question of which options society should support, why can't it be a matter of degrees and amounts of support spread

[14] For a helpful definition of monism, see Kellert et al. (2006, p. x).

out over several alternatives, instead of a decision about which basket we should put all of our eggs in? Again inspired by the cases of oxygen and phlogiston, I wish to advance a general hypothesis that there is some benefit to be had in retaining epistemic objects once they have been established through some successful epistemic activities.

If we take an operational view, it is difficult not to have sympathy for extinct epistemic objects. If an epistemic object once had a cogent meaning, then why shouldn't that meaning be lasting, especially if it was based on repeatable and robust operations? Unless it has somehow become impossible or inadvisable to perform those operations, there is no compelling case for eliminating from scientific discourse and practice the epistemic objects that were once rendered meaningful through those operations. This is a lesson I first articulated in thinking about phlogiston and oxygen, but I think I already knew it vaguely from my work on heat and temperature (in Chang 2004, among other places). Heat and temperature provide us with another instructive story of survival, achieved in spite of their association with all manner of disreputable theories ranging from the Aristotelian 4-element cosmology to various caloric theories. Heat was once an indestructible subtle fluid, then a distinct form of energy interconvertible with mechanical work, and then just a macroscopic manifestation of the mechanical energy of molecules. Temperature went from the density of (free) caloric, to a couple of unspeakably abstract things defined by Kelvin in terms of the Carnot cycle (first on a scale stretching to negative infinity, and then on a scale having a zero), then to something proportional to the average kinetic energy of molecules. What ties all these concepts together is their respective links to the operational concept of temperature, which rests in a thickly weaved and robust tradition of practical thermometry. And with our quotidian and industrial necessities to worry about temperature as anchored in practical thermometry, there is little chance that the temperature concept would be discarded altogether, however much it keeps on changing.

This is not just a matter of semantics, or of the fortunes of epistemic objects. The point is more general. Epistemic objects come bound up with systems of knowledge in which they play a role; once a system of knowledge becomes well-established, it is difficult to see how it would suddenly become invalid, short of a genuine, metaphysical change in the very laws of nature. In fact scientists often do preserve and use systems of knowledge that are supposed to be invalid in an ultimate sense. Newtonian mechanics, with its absolute space and time, is still in use in most practical applications. Orbitals still form the basis of much work in chemistry, although they are not supposed to exist according to up-to-date quantum theory. Geometric optics still has its uses; classical wave optics even more so, although there is officially no acknowledged medium in which the waves can exist and even the status of classical electromagnetic fields seems uncertain in the face of photons and quantum electrodynamics. It is of course acknowledged that the old theories do not apply well outside the domains in which they are well-established, but it is also acknowledged in practice that they still function in their own right and the in-principle reductions to newer theories are either merely promissory notes or useless currency. Scientists may pay lip-service to an overreaching monism, but their actual practices tend to be much more pluralistic, even in many areas of theoretical physics. Only the "dreams of a final theory" make our once-respectable systems of knowledge suddenly appear shabby and not worth keeping.

What these reflections suggest, as an alternative to an exclusive monism, is a curious kind of conservative pluralism, or better, *conservationist pluralism*. What I advocate is more of the kind of preservation-and-development of epistemic objects that we witness in the cases of oxygen, atoms and heat, and less of the hasty elimination seen in the cases of phlogiston and ether. I think scientists have tended toward theoretical and ontological monism, which most philosophers and many historians of science have also shared. This increases the risk of a hasty elimination of epistemic objects. As new facts and ideas spring almost irrepressibly in science, monism is liable to lead to faddishness, or fickleness: if the latest thing is any good, then monists are prone to assuming that anything old that competes with the new must be eliminated. Why should Popperians, for example, have an ideal of a refuted theory being eliminated, rather than a new theory arriving to complement the old? If we really discard established epistemic objects, we are also likely to discard the knowledge and valuable epistemic practices embodied in them.

The continual coming-into-being of new epistemic objects should lead to greater diversity and abundance of scientific knowledge and practice, not to an ever-shifting exclusive orthodoxy. And preserving old epistemic objects is not only a matter of preserving old knowledge. As stressed by Rheinberger, each object also has its own distinctive heuristic power—producing new phenomena, suggesting new experiments, and opening up new avenues of thinking. Similarly, each object has a unique potential to change and develop in response to new facts and ideas. There can also be fruitful interactions between the different traditions that different objects embody—yes, let a hundred flowers bloom, and let them cross-fertilize, too. All in all, an impoverished ontology will limit the developmental potential of science. It is beyond the remit of this paper to give a full-blown argument for pluralism, but I hope to have presented some key ingredients for such an argument.[15]

**Acknowledgments** I would like to thank Uljana Feest and Thomas Sturm for inviting me to the workshop on historical epistemology at the Max Planck Institute where this paper was originally presented, and also for their effective guidance through the publication process. I also thank many other participants and hosts of the workshop for their helpful comments and kind encouragement, especially Philip Kitcher, Paul Hoyningen-Huene, Hans-Jörg Rheinberger, and Lorraine Daston.

## References

Allchin, D. (1992). Phlogiston after oxygen. *Ambix, 39*, 110–116.

Arabatzis, T. (2006). *Representing electrons: A biographical approach to theoretical entities*. Chicago and London: University of Chicago Press.

Bensaude-Vincent, B. (1996). Between history and memory: Centennial and bicentennial images of Lavoisier. *Isis, 87*, 481–499.

Cardwell, D. S. L. (Ed.). (1968). *John Dalton and the progress of science*. Manchester: Manchester University Press.

Chang, H. (2004). *Inventing temperature: Measurement and scientific progress*. New York: Oxford University Press.

[15] For the full argument, see Chang (2012), chapter 5.

Chang, H. (2008). Contingent transcendental arguments for metaphysical principles. In M. Massimi (Ed.), *Kant and philosophy of science today* (pp. 113–133). Cambridge: Cambridge University Press.

Chang, H. (2009a). "Operationalism", *Stanford Encyclopedia of Philosophy*. Online at http://plato.stanford.edu/entries/operationalism.

Chang, H. (2009b). We have never been whiggish (about phlogiston). *Centaurus, 51*, 239–264.

Chang, H. (2010). The hidden history of phlogiston: How philosophical failure can generate historiographical refinement. *HYLE—International Journal for Philosophy of Chemistry, 16*, 47–79.

Chang, H. (2012). *Is water $H_2O$? Evidence, pluralism and realism*. Dordrecht: Springer.

Conant, J. B. (1957). The overthrow of the phlogiston theory: The Chemical Revolution of 1775–1789. In J. B. Conant (Ed.), *Harvard case histories in experimental science* (Vol. 1, pp. 65–115). Cambridge, MA: Harvard University Press.

Daston, L. (Ed.). (2000). *Biographies of scientific objects*. Chicago: The University of Chicago Press.

Elliott, J. (1780). *Philosophical observations on the senses of vision and hearing; to which are added, a treatise on harmonic sounds, and an essay on combustion and animal heat*. London: J. Murray.

Hoyningen-Huene, P. (2008). Thomas Kuhn and the Chemical Revolution. *Foundations of Chemistry, 10*, 101–115.

Kellert, S. H., Longino, H. E., & Waters, C. K. (Eds.). (2006). *Scientific pluralism*. Minneapolis: University of Minnesota Press.

Klein, U. (1994). Origin of the concept of chemical compound. *Science in Context, 7*(2), 163–204.

Klein, U. (1996). The chemical workshop tradition and the experimental practice: Discontinuities within continuities. *Science in Context, 9*(3), 251–287.

Klein, U., & Lefèvre, W. (2007). *Materials in eighteenth-century science*. Cambridge, Mass: The MIT Press.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: The University of Chicago Press.

Lavoisier, A.-L. (1789). *Traité élémentaire de chimie*. Paris: Cuchet.

Lavoisier, A.-L. (1965). *Elements of chemistry, with an introduction by D. McKie*. New York: Dover (reprint of R. Kerr's English translation (1790) of Lavoisier 1789).

Lewis, G. N. (1926). *The anatomy of science*. New Haven: Yale University Press.

McEvoy, J. G. (1997). Positivism, whiggism, and the Chemical Revolution: A study in the historiography of chemistry. *History of Science, 35*, 1–33.

Odling, W. (1871). On the revived theory of phlogiston (address at the Royal Institution, 28 April 1871). *Proceedings of the Royal Institution of Great Britain, 6*, 315–325.

Partington, J. R., & McKie, D. (1937–1939). Historical studies on the phlogiston theory. *Annals of Science, 2*, 361–404; 3, 1–58, 337–371; 4, 113–149 (in four parts).

Rheinberger, H.-J. (1997). *Toward a history of epistemic things: Synthesizing proteins in the test tube*. Stanford: Stanford University Press.

Rheinberger, H.-J. (2005). A reply to David Bloor: 'Toward a sociology of epistemic things'. *Perspectives on Science, 13*, 406–410.

Siegfried, R. (2002). *From elements to atoms: A history of chemical composition*. Philadelphia: American Philosophical Society.

Stanford, P. K., & Kitcher, P. (2000). Refining the causal theory of reference for natural kind terms. *Philosophical Studies, 97*, 99–129.

Sudduth, W. M. (1978). Eighteenth-century identifications of electricity with phlogiston. *Ambix, 25*, 131–147.