Hysteria, Race, and Phlogiston.
A Model of Ontological Elimination in the Human Sciences

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Abstract: Elimination controversies are ubiquitous in philosophy and the human sciences. For example, it has been suggested that human races, hysteria, intelligence, mental disorder, propositional attitudes such as beliefs and desires, the self, and the super-ego should be eliminated from the list of respectable entities in the human sciences. I argue that eliminativist proposals are often presented in the framework of an oversimplified "phlogiston model" and suggest an alternative account that describes ontological elimination on a gradual scale between criticism of empirical assumptions and conceptual choices.

Scientific ontologies are constantly changing through the introduction of new entities and the elimination of old entities that have become obsolete. Sometimes the elimination of an entity is an uncontroversial consequence of new empirical evidence. For example, new observational data may lead an astrophysicist to the elimination of a planet or a geographer to the elimination of an island that had previously been assumed to exist. Despite the availability of uncontroversial examples, not all issues in scientific ontologies can be settled easily. Even if we limit ourselves to the human sciences, examples of controversial elimination issues are legion. For example, philosophers have disagreed regarding the ontological status of propositional attitudes such as beliefs and desires (Churchland & Churchland, 1998) as well as more general psychological entities such as the self (Metzinger, 2004). Unsettled elimination controversies are not only found with regard to folk-psychological entities but also entities that have a strong tradition in experimental psychology such as general intelligence (Gardner, 1985; Schlinger, 2003) or basic emotions (Cohen, 2013; Ortony & Turner, 1990). Furthermore, psychiatric debates have been concerned with the elimination of mental disorders in general (Szasz, 2011) as well as more specific psychiatric entities such as hysteria (Micale, 1993) or multiple personality disorder (Hacking, 1996). Finally, elimination debates also occur in human biology as current controversies about the existence of human races (Glasgow, 2008) illustrate.

The ubiquity of elimination controversies in the human sciences raises the general but rarely discussed (an exception is Chang, 2011) question at what point scientists should eliminate an entity from their ontology. Typically, elimination controversies focus on one specific entity and consider other cases of ontological elimination only briefly through analogies to obsolete entities in the history of science such as the élan vital, ether, phlogiston, phrenological organs, or even witchcraft. In this article, I want to argue that this situation is unfortunate as it often leads to the implicit use of an oversimplified "phlogiston model" of ontological elimination (Section 1) that proves inadequate for many debates in the human sciences (Section 2). Furthermore, I will propose a more complex model that interprets ontological elimination as typically located on gradual scale between criticism of empirical assumptions and conceptual choices (Section 3). Finally, I try to show that this gradual model is helpful in the history and philosophy of science by discussing its application to debates about the existence of human races (Section 4).

1. The phlogiston model of ontological elimination

In criticizing ontological assumptions, philosophers and scientists often compare their targets to failed entities in the history of science. For whatever reason, analogies with phlogiston are especially popular as a quick look at the literature illustrates. Some of the best known phlogiston analogies come from debates in philosophy of mind with eliminative materialists arguing that all folk psychological entities such as beliefs and desires will somehow end up being displaced by brain states in a process analogous to the displacement of phlogiston (Churchland & Churchland, 1998, p. 71). Other philosophers remind us that "the 'self' or 'person' is no more real than such outdated scientific concepts as phlogiston" (Jones, 2000, p. 75). Not only folk entities are assumed to share the fate of phlogiston. In the case of psychiatry, Thomas Szasz's influential attack on psychiatric classification ended up in the diagnosis of mental illness as "psychiatry's phlogiston" (2001) while others have singled out specific mental disorders such as hysteria as phlogiston-like entities (Stein, 2001, p. 88). In cognitive science and psychology, different entities such as an innate universal grammar (Tomasello, 2009, p. 304) or basic emotions (Harré & Gillett, 1994) have been compared to phlogiston. Finally, Ashley Montagu shaped both philosophical and biological debates about human races by characterizing them as "the phlogiston of our time" (1964, p. xii).

Although I do not want to suggest that all authors use the analogy in exactly the same way, there is something like a standard story about the "Chemical Revolution" that is historically questionable (e.g. Chang, 2012) but arguably an important point of reference for philosophers and scientists who use the phlogiston analogy. Furthermore, I assume that this standard story can help to formulate a "phlogiston model" of ontological elimination that is often implicit in eliminativist proposals and typically involves four crucial assumptions. The first and most obvious assumption of the phlogiston model is that an eliminated entity x is postulated by some theory T1 but its existence is rejected by an ontologically incompatible competitor theory T2. In order for T1 and T2 to be ontologically incompatible, it is not sufficient that the term x is not part of T2 but also necessary that x can neither be reduced to nor identified with any entity in T2.

(1) T1 and T2 are ontologically incompatible in the sense that existence of an entity x is postulated by T1 but rejected by T2.

Consider, for example, the Churchlands' description of the elimination of phlogiston as "outright displacement, without reduction, of the old phlogiston theory of combustion by Lavoisier's oxygen theory of combustion. The older theory held that the combustion of anybody involved the loss of a spirit-like substance, phlogiston, whose pre-combustion function it was to provide a noble woodlike or metal-like character to the baser ash or calx that is behind after the process of combustion is complete. It was the 'ghost' that gave metal its form. With the acceptance of Lavoisier's contrary claim that a sheerly material substance, oxygen, was being somehow absorbed during combustion, phlogiston was simply eliminated from our overall account of the world." (Churchland & Churchland, 1998, p. 71). The Churchlands' interpretation of the elimination of phlogiston provides a clear example of (1). We are confronted with two theories (phlogiston theory and oxygen theory) that are competitors in the sense that they provide incompatible accounts of processes such as combustion. The appearance of a contradiction between the ontological commitments of both theories could be dissolved, if we were able to reduce phlogiston to an entity that is postulated by the oxygen theory or at least identify phlogiston with such an entity in a non-reductive manner. However, differences between both theories prevent any identification and therefore leave us with an ontological incompatibility between T1 and T2. While the ontological incompatibility of T1 and T2 is necessary for an elimination of x, it is certainly not a sufficient. If an entity is labeled "phlogiston-like", it is not only assumed that its existence is incompatible with a competitor theory T2 but also that this competitor theory is better justified in the light of the available evidence:

(2) T2 is better justified than T1.

Of course, the evaluation of competing scientific theories is a notoriously complicated issue. On the one hand, one can appeal to epistemic values such as empirical adequacy, explanatory power, simplicity, and so on. And indeed, many philosophers have insisted that the elimination of phlogiston can be interpreted along these lines. Kitcher, for example, has argued that Lavoisier's oxygen theory provides "a general account which deals in a unified and consistent way, with a far greater range of the experimental results than any extant version of the phlogiston theory" (1993, p. 278). On the other hand, it has become almost a truism in post-Kuhnian history and philosophy of science that the reality of theory change is often much more complicated and the Chemical Revolution has become a much discussed example for the question if and to what degree epistemic values such as empirical adequacy and explanatory power account for theory change in the history of science. In a more recent discussion of the issue, Chang (2010) has suggested that the entire debate about the justification of the Chemical Revolution is somewhat misguided because it is based on a misleading historical picture. According Chang, Lavoisier's oxygen theory was not better justified but there was also no revolution in the late 18th century that needs to be explained. Instead, Chang suggests that both the phlogiston theory and Lavoisier's oxygen theory had a serious proponents in the late 18th century and both accounts were roughly equally well justified despite different strengths and weaknesses.

Ironically, the phlogiston model may therefore fail to be applicable to the historical debates about phlogiston in the late 18th century. However, (2) still remains an important aspect of the phlogiston model in the sense of common phlogiston analogies in elimination debates: if a philosopher or scientist compares an entity to phlogiston, she does not only want claim that there is a competitor theory in the sense of (1) but also that this competitor theory is in a better position. Usually, phlogiston analogies indicate an even stronger claim, as it is not only used to describe the implications of different scientific theories and their justification but rather to make an ontological commitment to the non-existence of an entity along the following lines:

(3) T1 was wrong in postulating the existence of x and the term x fails to refer to anything in reality.

There can be little doubt that an ontological commitment in the sense of (3) is often involved in phlogiston analogies as it is assumed that the term *phlogiston* was meant to refer to a hypothetical substance that turned out to be non-existent and that chemists before Lavoisier simply failed to refer to anything when talking about phlogiston. As Sankey puts it: "If oxygen is what causes fire, then 'phlogiston' refers to oxygen. But phlogiston does not exist, so that rather than mistakenly referring to oxygen, the term 'phlogiston' fails to refer to anything at all." (Sankey, 2008, p. 67). Usually, an analogous claim also constitutes the core of phlogiston comparisons: If propositional attitudes, the self, universal grammar, basic emotions, races, and so on turn out to be phlogiston-like, then the entities simply don't exist and the corresponding terms fail to refer.

So far, my characterization of the phlogiston model has been solely negative but there is also a positive side. Recall my claim in the introduction that scientific eliminations are sometimes uncontroversially implied by empirical evidence. For example, consider the elimination of "phantom islands" such as Sandy Island that had first been charted by James Cook in 1774 (Seton, Williams, & Zahirovic, 2013) and survived in maps and data sets for more than 200 years. Claims that the island did not exist were first made by in 2000 and confirmed by a scientific expedition in 2012. Arguably, all three features (1)–(3) are present. (1) Sandy Island had been been postulated by T1 but is rejected by a new competitor theory T2. Furthermore (2), T2 is clearly better justified as it relies on much more reliable data. Finally (3), the island simply doesn't exist and the name of the island fails to refer. Still, the analogy between the island and phlogiston appears somewhat weak as phlogiston (contrary to the island) was postulated on the basis of a quite elaborate theory that was considered of crucial importance for the explanation of phenomena such as combustion and the rusting of metals. Phlogiston-analogies are therefore typically directed against entities that are at least of some theoretical importance and come with the appearance explanatory potential.

(4) x is postulated by an elaborate theory and comes with the appearance of explanatory value.

To sum up, the phlogiston model suggests the following picture of ontological elimination: A scientific theory T1 postulates the existence of an entity x that is used to explain some natural phenomena. Despite its explanatory potential, T1 becomes challenged by a competitor theory T2 that rejects the existence of x. As x cannot be reduced to or identified with any of the entities that are postulated by T2, we end up with an ontological incompatibility of T1 and T2. Furthermore, T2 turns out to be the better justified theory so that we are led to the ontological conclusion that x simply does not exist and that the theoretical term x fails to refer.

2. Limits of the phlogiston model: hysteria

The overall goal of this article is to provide an alternative to the phlogiston model. Still, it seems attractive to interpret at least some prominent cases of ontological elimination in terms of the phlogiston model. For example, consider Franz Joseph Gall's phrenology which was proposed as a theory of mental organs such as "faithfulness", "numbers", "thievery", "inductive reasoning", or "good humor" (Bloede & Gall, 1807). Gall assumed that these mental organs were realized in circumscribed areas of the brain and that the size of brain areas correlates with characteristics of the mental organ. Finally, he argued that the size of the brain areas influences the shape of the cranium so that it becomes possible to determine personality traits by measuring the form of the skull. Unfortunately, Gall's claims did not only lack conclusive positive evidence but were also soon challenged by competitor theories. For example, Pierre Flourens physiological animal experiments suggested that there is no functional specialization in the cerebral hemispheres and certainly no neurally located mental organs in the sense of Gall (e.g. Tesak, 2001, pp. 56–60). Although Flourens' rejection of functional specialization came itself under pressure due to aphasiological research of Paul Broca and Carl Wernicke (e.g. Ludwig, 2012), the history of phrenology seems to match the phlogiston model reasonably well. First (1), Gall's phrenology postulated the existence of neurally located mental organs that were rejected by competitors theories. Furthermore (2), Gall's theory lacked conclusive positive evidence while competitors had the results of Flourens' animal experiments on their side. Finally (3), we know today that phrenological organs do not exist and Gall's organs failed to refer although (4) they were based on a quite elaborate theory.

While it is attractive to analyze cases such as phrenology in terms of the phlogiston model, I want to suggest that it is ill-suited as a general model of ontological elimination. Before I turn to what I consider the most substantial problem of the phlogiston model, I briefly want to mention a more obvious issue: the phlogiston model has clear limits in the history and sociology of science as there are also instances of ontological elimination that are not due to a better justified theory in the sense of (2) and in which we are not willing to claim that eliminated terms fail to refer in the sense of (3). Although, for example, much of genetics was eliminated in the Soviet Union in the wake of Lysenkoism (e.g. Gordin, 2012), this elimination was not due to a better justified theory in the sense of (2) and certainly did not stop many of the eliminated genetic terms from referring in the sense of (3). Or, to put the problem in more general terms: the phlogiston model seems to overrationalize actual cases of elimination that are influenced by a whole range of social factors and do not necessarily come with a better justification of the competitor theory or even a reference failure. While this worry indicates an important limit of the phlogiston model in the history and sociology of science, one may still hold that the model works just fine in cases of well-justified and successful elimination, i.e. cases in which entities are rightly eliminated for the right reasons. In the remainder of this section, I argue that there are important limitations of the phlogiston model even in these cases of well-justified and successful elimination as becomes clear when we consider the complex structures of elimination controversies about entities such as hysteria.

Hysteria has a long and complicated history with some elements of its diagnosis having evolved at least since the medical canon of ancient Greece. Despite this tradition, the late 19th century clearly constitutes the "the heroic period" of hysteria (Raymond, 1907, cited after Micale, 1993, p. 497) with the diagnosis becoming of crucial importance in the psychological discourse of fin de siècle societies and with hysteria's inflationary appearance as a ubiquitous nervous disorder. Theoretical debates about hysteria towards the end of the 19th century were dominated by the French neurologist Jean-Martin Charcot who made the Parisian Salpêtrière Hospital the center of European hysteria research (Huberman, 2004). Although Charcot had a clear etiological theory and characterized hysteria as caused by a functional lesion of the brain, the main of focus of his hysteria research was the characterization and systematic description of symptoms with the constant creation of new hysteric subcategories "such as traumatic hysteria, hysterical catalepsy, hysterical fugue, hystero-neurasthenia, toxic hysteria, hysterical heart, hysterical anorexia, hysterical tic, hysterical fever, and hysterical gastralgia. In short, as hysteria became the object of more medical investigation, the accumulation of observations did not led not to a more rigorously defined clinical category, but only to more encompassing descriptive definitions. As a result, by the end of the nineteenth century the diagnosis resembled an oversized and slightly vulgar late Victorian edifice– highly articulated in detail and impressive to contemplate from afar butimpractically large and with extremely shaky etiological foundation" (Micale, 1993, p. 504).

While hysteria arguably became one of the most visible and important psychiatric entities during the late 19th century, its importance quickly declined throughout the 20th century with an increasing number of psychiatrists suggesting to get rid of *hysteria* all together. An editorial of Canadian Medical Association Journal nicely captures a widespread attitude towards hysteria by 1970 as it suggested a "progress of the term 'hysteria' towards the graveyard of outworn nomenclature already occupied by lunacy, neurasthenia and shellshock" (Editorial, p. 1187). Two years before the publication of this editorial, hysteria had indeed made a big step´towards the graveyard of abandoned psychiatric entities by being eliminated from the Diagnostic and Statistical Manual of Mental Disorders of the American Psychiatric Association which in its third edition DSM III did not use the term *hysteria* at all anymore and instead replaced it with a variety of diagnoses such as somatoform disorder, conversion disorder, and psychogenic pain disorder. A more recent study by Stone, Hewett, Carson, Warlow, and Sharpe (2008) provides further evidence of the disappearance of hysteria from the psychiatric literature. Examining general neurological textbooks in UK libraries that were published between 1877 and 2005, Stone et al. found that the proportion of text concerned with hysteria was steadily declining from 3.7% between 1877 and 1900 to 0.5% between 1950 and 2005.

While the disappearance of hysteria from the official nomenclature, research articles, and textbooks is well documented, it is far less clear why hysteria became virtually extinct. One possible explanation is that the diagnosis of hysteria disappeared because hysteric behavior disappeared over the course of the 20th century. Maybe hysteric symptoms of the late 19th century were so closely entangled with fin de siècle culture that the developments of Western societies in the 20th century simply caused hysteria to disappear. While this is an intriguing hypothesis, there is little reason to believe that it is true. Of course, it is reasonable to assume that the prevalence of hysteric symptoms has changed over the past 150 years and it is undoubtedly true that every culture comes with its own transitory pathologies (Hacking, 1998). Furthermore, the entanglement of hysteric symptoms with the sexually repressive Victorian and Wilhelmian societies has been a thoroughly discussed topic since Freud's (1908) treatment of the issue. Still, there is ample evidence from hysteria's successor entities such as somatoform disorder or conversion disorder that many symptoms that 19th century psychiatrists were concerned with have clearly not disappeared (e.g. Feinstein, 2011; Stone et al., 2008). If hysteria disappeared while many typical "hysteric symptoms" still exist, one may be tempted to explain the situation in terms of the phlogiston model. Indeed, the processes that phlogiston was supposed to explain such as combustion and rusting of metals still exist while phlogiston has been eliminated because provided a flawed account of these processes. In analogy: The symptoms that hysteria was supposed to explain still exist while hysteria has been eliminated because provided a flawed account of these symptoms.

In order to apply the phlogiston model to hysteria, we would first have to identify a competitor theory in the sense of (1). Arguably, modern psychiatric accounts that do not mention hysteria and instead rely on diagnoses such as somatoform disorder or conversion disorder are the best candidates for a competitor theory.

However, the phlogiston model does not only require that the competitor theory does not mention hysteria but also that we cannot identify hysteria with any of the entities that are postulated by the competitor theory. First, one may suggest that an identification is possible as we can simply identify hysteria with its numerous successor entities. However, it is not difficult to see why this will not work. Identity is a transitive relation and if we would (for example) claim hysteria = somatoform disorder as well as hysteria = conversion disorder, we would end up with a false identity statement of somatoform disorder = conversion disorder. Second, one may suggest that we can identify hysteria with the set of all successor entities and therefore avoid problems with the transitivity of identity relations. However, if we would identify hysteria with the set of its successor entities, we would not only have to include entities such as somatoform disorder and conversion disorder but also highly disparate disorders such as some forms of epilepsy, schizophrenia, and even advanced neurosyphilis. However, hysteria in the sense of 19th century psychiatrists was clearly not meant to be a set of disorders with vastly different etiologies, symptoms, and prognoses. It therefore seems at least reasonable to reject any identification of hysteria with contemporary disorders and to follow Hacking in claiming that in modern psychiatry "one taxonomy replaces another to the point that we simply do not know what hysteria was anymore" (Hacking, 1998, p. 72).

If we grant proponents of the phlogiston model the existence of an ontologically incompatible competitor theory in the sense of (1), its better justification in the sense of (2) may be considered less problematic. Indeed, modern psychiatric theories that have replaced hysteria with more specific disorders have a large range of explanatory and clinical benefits. For example, more specific disorders allow more fine-grained descriptions, more precise accounts of etiologies and more reliable predictions of the effects of medical treatment. Getting rid of *hysteria* and replacing it with a more fine-grained psychiatric ontology therefore clearly comes with explanatory benefits. So far, so good for the phlogiston model. Hysteria arguably has (1) ontological competitors that have (2) distinct advantages and can be considered to be better justified. The real trouble lies in (3) and the assumption that *hysteria* does not refer to anything in reality. The analogy between phlogiston and hysteria arguably breaks down at this point as there is no reason to believe that *hysteria* "fails to refer". In the phlogiston model (whether historically accurate or not), the term phlogiston is supposed to refer to a hypothetical substance that turns out to be non-existent and we are therefore left without any reasonable referent of the term. *Hysteria*, however, was never meant to refer to a hypothetical substance or object that latter turned out to be non-existent. Instead, psychiatrists used *hysteria* to refer to a variety of symptoms and syndromes that still continue to trouble patients and psychiatrists. Indeed, theoretical accounts of hysteria in the 19th century were often too simple and some of the claims of scientists such as Charcot turned out to be outright wrong. However, at no point does it seem justified to claim that *hysteria* failed to refer to anything in reality. Even during its most inflationary use at the end of 19th century, *hysteria* referred to perfectly real psychological phenomena despite the fact that today's psychiatric taxonomy that has largely gotten rid of *hysteria*.

If *hysteria* did not disappear due to failed reference, we need an alternative explanation of its elimination. Furthermore, it is not difficult to at least sketch the outlines of such an alternative: *hysteria* was not eliminated because it failed to refer but rather because the referents became redescribed through concepts that psychiatrists found more useful and left *hysteria* without any function in research or clinical practice. Indeed, hysteria and its successors such as somatoform disorder or conversion disorder often refer to the same symptoms but psychiatrists find the latter entities more useful because they are more precise, fine-grained, and homogeneous regarding shared etiology, symptoms, and prognoses (although there are also problems with these new diagnoses, e.g. Feinstein, 2011). And if we have redescribed the referents in terms of these new entities, there is simply no good reason to hold on to hysteria. From a research perspective, *hysteria* is too vague and heterogeneous to come with any epistemic payoffs. From a practitioner's perspective, *hysteria* has a huge historical baggage especially in its discriminatory use as a "female disorder". One way or another, there seems little reason in clinging to hysteria instead of accepting its extinction.

I believe that there is a more general lesson to learn from this example. So far, I have argued that the elimination of scientific entities is usually understood in terms of the phlogiston model which implies that an eliminated term fails to refer. I have suggested that *hysteria* was not eliminated because it failed to refer but rather because the referents became redescribed through new and more fine-grained diagnoses that made any further use of *hysteria* superfluous. It is not difficult to see how this example can be generalized into a model of elimination through redescription instead of the phlogiston model's elimination through failed reference. Further examples of the former are not hard to find. In the case of psychiatry, even a cursory look at the changes from the fourth to the fifth and latest edition of Diagnostic and Statistical Manual of Mental Disorders (DSM) leads to many examples that are less prominent than hysteria but have a similar structure. For example, "the DSM-IV subtypes of schizophrenia (i.e., paranoid, disorganized, catatonic, undifferentiated, and residual types) are eliminated due to their limited diagnostic stability, low reliability, and poor validity" (American Psychiatric Association, 2013, p. 3) Clearly, psychiatrists did not fail to refer when they used terms such as *paranoid schizophrenia* prior to the publication of the DSM-5 in May 2013 but the phenomena became redescribed due to clinical concerns.

3. A gradual model of ontological elimination

My discussion so far seems to indicate two different kinds of ontological elimination. On the one hand, there are cases of elimination that are correctly described by the phlogiston model and in which eliminated terms fail to refer. For example, Gall's *phrenological organ*s fail to refer and were therefore eliminated by subsequent neurological and psychological research. On the other hand, there also cases of elimination that are based on redescription instead of failed reference. For example, hysteria became virtually extinct in psychiatry not because the term failed to refer but rather because the referents became redescribed through new diagnoses that left no productive role for the old and colorful entity hysteria.

Clearly the distinction between two different kinds of elimination has many advantages compared to an insistence on the phlogiston model as the only account ontological elimination. For example, the distinction can help in making sense of cases that do not fit the idea of elimination through failed reference such as the elimination of psychiatric entities from the DSM. Furthermore, it gives both empirical and conceptual issues a clear role in ontological elimination without reducing one to another. Sometimes, empirical evidence convinces scientists of the non-existence of an entity as implied by the phlogiston model. At other times, ontological elimination is due to redescriptions that are preferable in one way or another even if the eliminated term did not fail to refer.

Still, I want to suggest that the dichotomy between two types of elimination is misleading and that a convincing account should consider both models as opposing and idealized ends on a gradual scale. When we consider actual cases of ontological elimination in the human sciences, discussions are often considerably more complex than suggested by both models as they include criticism of both empirical assumptions and conceptual choices. To make this point, we do not even have to consider new examples but only have to have a more careful look that the cases discussed sofar.

For example, I have presented the elimination of hysteria from much of the psychiatric literature as a case of redescription in which a vague and heterogeneous term was replaced by more precise diagnoses such somatoform disorder or conversion disorder that also have more homogenous (e.g. etiological and symptomatic) properties and therefore turned out to be preferable in research and academic practice. But surely this is not the whole story. Hysteria did not only come under pressure in a process of reconceptualization but also through new empirical evidence that cast doubt on important elements of 19th century theories of hysteria. Micale, for example, points out that "Charcot possessed a clear etiological theory of hysteria. He believed that the disorder traced a physical defect of the nervous system, such as a tumor or spinal lesion that resulted either from direct physical injury or defective neuropathic heredity" (1993, p. 503).

Charcot's etiological assumptions soon became untenable as his expansive concept of hysteria included disorders with vastly different causes such as traumatic events, physical brain lesions, and even infections such as syphilis. However, if Charcot postulated an etiologically at least somewhat unified disorder which turned out to be non-existent, what stops us from saying that *hysteria* failed to refer?

An analogous problem can be raised by questioning the claim that terms such as phrenological organ fail to refer. As mentioned in the beginning of the last section, phrenology quickly crumbled in academic debates under the influence of several factors such as Flourens' animal experiments which led to the assumption that there are no functionally specialized brain areas that correspond with mental organs. However, Flourens' equipotentiality thesis also soon came under pressure by new aphasiological research of Broca and Wernicke who insisted that correlations between brain lesions and aphasiological symptoms supported the functional specialization of the brain (Tesak & Code, 2008). Indeed, aphasiological researchers of the late 19th century were careful in distinguishing their positions from the discredited phrenological tradition (Greenblatt, 1995). Wernicke, for example, insisted in his groundbreaking Der Aphasische Symptomkomplex that only the"most basic mental functions can refer to specific locations of the cortex" (1874, p. 4, translation by David Ludwig) while the phrenologists' went wrong in trying to locate complex personality traits such as "faithfulness" or "good humor" in the brain and in assuming that these traits can be determined by measuring the shape of the cranium. Still, phrenologists were clearly not wrong about everything but had some important and at their time highly innovative insights about the functional specialization of the brain. The point can be further stressed by considering positive references to phrenology in more recent debates in cognitive science. Jerry Fodor's The Modularity of Mind, for example, starts out with a discussion of Gall and the surprising claim that "much of what follows in this section will be an elaboration of Gall's vertical organ idea, since it seems to me that there is much in this notion that modem cognitive science would do well to ponder" (1983, p. 17).

Arguably, both Charcot's hysteria theory and Gall's phrenology got some things right and others wrong. This result threatens to blur the lines between both cases in one way or another: if we insist that the development from Charcot's hysteria to contemporary psychiatric disorders is only a case of redescription– why shouldn't we also claim that the development from Gall's phrenological organs to contemporary modules of cognitive neuroscience is only a case of redescription? If we insist that Gall's *phrenological organ*s failed to refer because of false assumptions such as measurable crania differences– why shouldn't we insist that Charcot's hysteria failed to refer because of false assumptions such as a unified etiology?

I do not want to suggest that the lesson is that there are no differences between both cases. Instead, I want to propose that we have understand ontological elimination on a gradual scale whose idealized ends are marked by the models discussed so far. At the one end is the phlogiston model which in its idealized form states that everything that a theory T1 assumed about a postulated entity turned out to be wrong. Sometimes, sketchy philosophical presentations of historical analogies take the form of such an idealized phlogiston model. For example, it is assumed that all theoretical
claims about phlogiston or phrenological organs were falsified by subsequent research which therefore clearly established that phlogiston or phrenological organs failed to refer. The other end of the spectrum are idealized cases of redescription without the correction of any flawed empirical assumptions. Appeals to this idealized end of the spectrum may be found when scientists attempt to convince their colleagues that the elimination of a term is merely a classificatory issue that comes with the introduction of a more helpful taxonomy.

The reality is virtually always more messy and it is not difficult to see how the gradual model can help making sense of cases such as phrenological organs or hysteria. Neither of the cases fits the idealized models but that does not mean that there are no important differences between them. In the case of phrenological organs the idealized phlogiston model fails because Gall and colleagues were right with respect of some important assumptions such as the functional specialization of the brain. At the same time they were wrong with respect to other assumptions such as the idea that functionally specialized brain areas shape the cranium in a way that one can detect personality traits by examining the skull. Clearly, these flawed assumptions were crucial in providing the foundation for phrenological research. By eliminating the idea that mental organs shape the cranium, the core of phrenological practice which revolved around skull measurements breaks down, no matter whether phrenologists were right about some other assumptions. The situation is notably different in the case of hysteria research. Although Charcot assumed an at least somewhat unified etiology, much of his practice focused on the description, classification and treatment of symptoms that was not dependent on his etiological speculation.

A second difference between phrenological organs and hysteria becomes obvious when we consider the development of both concepts over time. In the case of hysteria, the emergence of new evidence motivated at least some psychiatrists to reshape the concept. Even today, there are psychiatrists who disagree with the elimination of hysteria from the DSM and ICD and instead suggest conceptual frameworks that save hysteria in psychiatric taxonomies (e.g. Ávila et al., 2012). The situation is strikingly different in the case of phlogiston theories that were quickly dropped by the academic community without any attempts of reformulation in the light of new evidence. No one tried to save phrenological organs by reinterpreting them as functionally specialized brain areas in the Broca–Wernicke-tradition. Instead, phrenological organs made a career as popular spectacles while later localizationist theories in neurology carefully avoided associations with the discredited phrenological tradition.

A gradual model allows to account for differences between elimination debates without requiring that they are separated by a clear line between the phlogiston model and redescription model. Gradual differences between cases such as hysteria and phrenological organs are all we need and indeed usually also all we get when we engage with complex elimination debates in the human sciences. Still, one may be object that this such a gradual account does not answer the crucial question at what point a term such as *hysteria* or *phrenological organ* fails to refer. Recall that the idealized phlogiston model assumes that a term fails to refer while the redescription model assumes that the referents only become reconceptualized. Even if this much is uncontroversial– what shall we say about all the cases that lay somewhere in the middle on the gradual scale between both models?

While it is true that the gradual model as I have presented it so far does not answer this question, I also assume that it is actually helpful to avoid commitment to a philosophical theory of reference failure when describing structures of ontological elimination. By not making any substantive philosophical assumptions about reference failures, the gradual model remains compatible with a variety answers to the question at what point eliminated terms fail to refer. On the one hand, the model can be combined with accounts of reference that draw a line between referring and non-referring terms at some point on the gradual scale. Debates about reference of scientific kinds (e.g. Devitt & Sterelny, 1987; Stanford & Kitcher, 2000) have moved beyond simple variants of a descriptive (e.g. gold refers to whatever is a metal + shiny + yellow + ductile) and causal-historical (e.g. gold refers to whatever has the same inner constitution as the original sample of gold) theories of reference and may be able to handle even complex examples from the history of science such as phlogiston, phrenological organs, or hysteria.

On the other hand, one can also combine the proposed model with a deflationist account that rejects the assumption of one correct account of reference (e.g. Mallon, Machery, Nichols, & Stich, 2009). Deflationist accounts of reference are currently especially popular in the context of experimental philosophy and often motivated by psychological evidence about diverging referential intuitions regarding proper names (e.g. Machery, Mallon, Nichols, & Stich, 2004). Roughly, the basic deflationist argument takes the following form: Intuitions about reference are cross-culturally variable. These intuitions cannot be further justified and therefore should be considered equally acceptable. Theories of reference crucially rely on intuitions about reference. A variety of equally acceptable referential intuitions therefore implies a variety of equally acceptable theories of reference and there is "no correct substantive theory of reference" (Mallon et al., 2009, p. 343).

There are at least two reasons why it is tempting to combine a gradual model of elimination with a deflationist account of reference. First, eliminated scientific kinds that have to be located somewhere in the middle of the gradual elimination scale typically elicit very heterogeneous referential intuitions and even in supposedly clear-cut cases such as phlogiston, there has been a confusing variety incompatible claims about successful or failed reference (cf. Chang, 2011; Lewowicz, 2011). A deflationist account of reference dissolves worries about reference by denying that there is a substantial issue to be solved. Second, the gradual model arguably offers a way of thinking about elimination controversies that does not require a definitive answer to questions of reference. One potential problem with deflationism about reference is that it appears to imply an overly strong deflationism about scientific existence debates. For example, Mallon et al. (2009) suggest that "there is no contradiction when a member of A says truly 'Beliefs do not exist' and when a member of B says, also truly, 'Beliefs do exist'" (p. 346) as long as members of A (e.g. Churchland & Churchland, 1998) insist on a descriptivist account of reference while members of B (e.g. Lycan, 1988) rely on a causal theory of reference. Still, one may object that such a deflationism leaves too little room for serious scientific existence debates as we can always deflate a discussion by appealing to different accounts of reference.

The gradual model provides a possible rejoinder by making sense of substantive elimination controversies without relying on a substantive account of reference. On the one hand (and towards the phlogiston-end of the spectrum), elimination controversies are due to different empirical assumptions. For example, the elimination of phrenological organs was largely due to the rejection of Gall's empirical assumptions such as the possibility of making psychological predictions on the basis of cranial measurements. On the other hand, (and towards the redescription-end of the spectrum), elimination controversies are motivated by different conceptual choices. For example, the elimination of hysteria was largely motivated by the attempt to reconceptualize disorders in a manner that is preferable in psychiatric research and clinical practice. Furthermore, many elimination controversies involve criticism of both empirical assumptions as well as conceptual choices and we can describe all of this in detail without making any claims about successful or failed reference. In an important sense, one may therefore hold that the gradual model offers an alternative which allows us to make sense of the elimination of terms such as *phlogiston*, *phrenological organ*, or *hysteria* without having to tackle the question whether these terms really refer or fail to refer. Or, to make the point with Mallon et al.'s example of the elimination of belief: There are usually substantive disagreements between eliminative materialists and defenders of folk psychology. On the one hand, there are empirical disagreements regarding issues such as the explanatory power of folkpsychology. On the other hand, there are conceptual disagreements regarding the question whether it is desirable to replace a folk-psychological framework with a neuroscientific framework. Both types of disagreement are perfectly reasonable and doubts about the substantive character of the elimination controversy only occur if both sides agree on these issues while still disagreeing on whether belief refers. Only in these cases can we move from a deflationism about reference to deflationism about elimination controversies.

Although it may be attractive to combine the gradual model with a deflationist attitude towards questions of reference, one may as well stick with one of the many substantive philosophical theories of reference. This agnosticism towards issues of reference is possible because the gradual model does not equate ontological elimination with failed reference. Failed reference is arguably a sufficient but clearly not a necessary condition for ontological elimination and elimination controversies in the human sciences can be reconstructed along the gradual model without even raising the question whether a term "failed to refer".

4. Applications in history and philosophy of science: race

A model is only as good as its applications. In this last section I want to illustrate the strengths of the gradual model by applying it to debates about the elimination of human races from biology. Debates about existence of human races have undergone highly complex metamorphoses in the past 150 years. In the 19th century, we find most race theories fairly close to the idealized end of the phlogiston model. Indeed, there is not one definitive theory of race in the 19th century but rather a large number of often inconsistent proposals. First, there has been considerable variation with respect to the extension of assumed races. While Blumenbach originally (1775) followed Linnaeus in distinguishing four races (American, Caucasian, Mongoloid, Negroid), he latter added a Malay race and his five-race system became an important point of reference for biologists of the late 18th and the 19th century (Gould, 2002, chap. 26). Still, biologists often reduced or inflated the number of races. For example, Cuvier (1798) postulated three races (Caucasian, Mongolian, Ethiopian) while Agassiz suggested eight races such as an Arctic race or a Hottentot race (1854, cf. Irmscher, 2013, chap. 6). Furthermore, race theorists did not only disagree on the number but also on the nature of human races. The most visible dispute was about the descent of races with monogenists such as Blumenbach arguing for a common origin of all humans and polygenists such as Agassiz insisting on races descending from different ancestral populations.

Despite these differences, race theories of the late 18th and the 19th century shared crucial empirical assumptions that turned out to be flawed and make it attractive to locate them near the idealized phlogiston-end of the elimination scale. Two features are especially obvious and important for their latter rejection. First, races were usually assumed to have essences: all members of a race share the same essential properties that unambiguously define their racial membership. Second, 19th-century theories postulated far-reaching differences between races on a morphological, intellectual, and "temperamental" level. Remarks of respectable 19th-century scientists illustrate how far off the mark much of mainstream biology was in its speculations about racial differences. For example, Haeckel claimed "that the unprejudiced comparative student of nature, seem to manifest a closer connection [of "lower races of man"] with the gorilla and chimpanzee of that region than with a Kant or a Goethe" (Haeckel, 1869, p. v).

Both assumptions were not only widely rejected in the 20th century but also contrasted with populationist approaches in early post-war biology. Although there are legitimate historical worries about the dichotomy between two completely distinct approaches (Gannett, 2001; Lipphardt, 2012), the rejection of essentialist and racist speculations of 19th-century theories is sufficiently obvious in the works of the most visible proponents of the new "population thinking" including Theodosius Dobzhansky (e.g. 1951) and Ernst Mayr (e.g. 1973). While essentialism was a direct target of populationist accounts, the rejection of far-reaching mental differences between races was another important reason for drawing a sharp line between race science and new populationist approaches. Indeed, reproductive isolation will lead to some genetic and phenotypic differences between populations but at least the mainstream of populationist post-war research rejected far-reaching intellectual or "temperamental" differences as the 1950 UNESCO declaration on race nicely illustrates: "Whatever classification the anthropologist makes of man, he never includes mental characteristics as part of those classifications. It is now generally recognised that intelligence tests do not in themselves enable us to differentiate safely between what is due to innate capacity and what is the result of environmental influences, training and education [. . .] As for personality and character, these may be considered raceless." (UNESCO, 1950, p. 9).

With essentialism and mental distinctions gone, there was also little left of 19th-century entities such as Agassiz' Mongolian Race or Hottentot Race and one may quite comfortably locate them near the idealized phlogiston-end of the elimination spectrum. Still, race didn't disappear. Instead, the campaign for an anti-essentialist population thinking also led to a new "genetic race concept" (e.g. Dobzhansky, 1951) that defined races as populations with sufficient genetic differences but no essences and also no requirements regarding mental or behavioral differences between them. Although my claim of one new genetic race concept clearly oversimplifies the situation given a diversity of race concepts in early post-war biology and even in the work of Dobzhansky (Gannett, 2013), there is an obvious contrast between these new conceptual proposals and the races of the 19th century. While races as they were assumed by Dobzhansky avoided many empirical objections regarding racial essences and alleged mental differences, they also implied new conceptual issues. One conceptual worry becomes clear in the exchange between the anthropologist Frank B. Livingstone and Dobzhansky in Current Anthropology 1962 (compare Relethford, 2010). In his short article on the "Non-

Existence of Human Races", Livingstone did not question the empirical results of Dobzhansky's research but rather the assumption that we should identify human populations with human races. According to Livingstone, differences in gene frequencies cannot be sufficient for racial differences as that would mean that the difference between gene frequencies in the "town of Orosei in the lowlands of Sardinia and the town of Desulo in the highlands fifty kilometer away is a racial difference [. . .] One could also speak of a high color-blind and a low color-blind race" (1962, p. 280). According to Livingstone, empirical findings of genetic differences between human populations are not sufficient to justify a continued use of the term *race*. Livingstone suggests that populations should only be called races if they are sufficiently discrete units to "accord with the general use of the term race as a concept within the Linnaean system of biological nomenclature" (282). As Dobzhansky (1962, p. 279) explicitly acknowledges that human populations are not discrete units in this sense, we should eliminate human races from biological taxonomies.

The Livingstone–Dobzhansky exchange illustrates how the debate about races had become at least partly about conceptual choices as the question was not what we know about differences between human populations but rather at what point we should call different populations *races*. A second example of the importance of these conceptual issues in the second half of the 20th century comes from comparative accounts of academic communities in different post-war societies. Gissis (2008), for example, has investigated the occurrences of *race* in genetic, medical, and epidemiological journals and found a more or less constant use of *race* in American-authored articles while "except for for a single article in 1959, the race category was never used between 1946 and 2003" (p. 445, emphasis in original) in Israeli-authored articles. Gissis suggests that this difference was not due to different empirical assumptions but rather due to different conceptual strategies. Where American scientists were often used racial categories in their research, Israeli colleagues refused to follow and used alternative concepts instead.

While these examples show the importance of conceptual issues, it would be wrong to assume that the fate of *race* had become merely about conceptual choices as indicated by the idealized redescription model of elimination. Instead, there remained important empirical reasons to doubt the existence of races given the framework of post-war population thinking. The most influential challenge of populationist accounts of human races comes from Lewontin's (1972) study which showed that only a small portion of genetic variation in the human species is found between human populations. As Lewontin put it: "The mean proportion of the total species diversity that is contained within populations is 85.4% [. . .] Less than 15% of all human genetic diversity is accounted for by differences between human groups! Moreover, the difference between populations within a race accounts for an additional 8.3%, so that only 6.3% is accounted for by racial classification. "(1972, p. 392) Lewontin concludes that the low level of genetic variation that is captured by racial distinctions undermines their relevance for taxonomy. As Hochman (2013, cf. Pigliucci, 2013) has recently stressed, this argument was so forceful because it only applied common standards of subspecies classification in non-human biology to the issue of human races. By using the common framework of subspecies classification, the data suggested that racial divisions are relevant in many non-human species with a longer history of reproductive isolation but as a matter of contingent empirical fact, they are not relevant in the case of *homo sapiens*.

In order to leave room for human races in biological ontologies, *race* had to transform once again to become compatible with the kind of data that was presented by Lewontin. Unsurprisingly, this is exactly what happened. Although Lewontin's arguments were successful in convincing many scholars that race was a dead issue in the biological sciences, the debate has made another turn in the past 15 years or so. While contemporary proponents of a continued use of *race* do not agree on its definition, all serious proposals (e.g. Andreasen, 1998; Edwards, 2003; Hardimon, 2012; Kitcher, 1999) share the feature of empirical modesty by only making reasonable and sometimes even trivial empirical assumptions about issues such as genetic diversity and reproductive isolation. Revamped genetic race concepts often rely on a 2002 study by Rosenberg et al. that used data from the Human Genome Diversity Cell Line Panel (HGDP-CEPH) to identify six genetically similar clusters of which five roughly match continental regions. Although Rosenberg et al. do not interpret their clusters as races, they have been quickly identified with the major "continental races" in subsequent discussions. Edwards, for example, has challenged Lewontin's eliminativism by pointing towards the possibility of multi-locus analysis as it is used in genetic clustering contrary to Lewontin's single-locus analysis and by arguing that such a "proper analysis of human data reveal substantial amount of information about genetic differences" (2003, p. 801). Other recent race concepts focus of reproductive isolation instead of genetic similarity. Andreasen's cladistic race concept, for example, identifies races as "ancestordescendant sequences of breeding populations" (1998, p. 200) without making any assumptions about their phenotypic or genetic properties. In other words: Andreasen proposes to understand races as groups of humans that have common ancestry and have become reproductively isolated from each other no matter if and to what degree there are non-historical biological differences between them.

I do not want to endorse or criticize any of these recent accounts of race in this paper but only argue that recent reconceptualizations of race have shifted the elimination debate almost entirely to the conceptual side of the gradual scale. Although there may be some problematic empirical assumptions in Edwards' or Andreasen's proposals, both are arguably cautious not only in avoiding outdated assumptions of the racialist tradition but also in accepting data about human variation as it has been stressed by Lewontin and other eliminativists. While this strategy makes them almost immune to empirical falsification, it makes them equally vulnerable to conceptual objections. No one doubts the existence of human populations in general as the human sciences constantly distinguish between groups of humans along biological features. However, most of these distinctions have nothing to do with race– for example, no one suggests the existence of an albino and a non-albino race, a male and female race or eye-color races. Edwards and Andreasen do not only have to show that there are human populations but also that we should identify some of these populations with races. And indeed, there are reasonable objections against this conceptual strategy. Hochman (2013), for example, argues that a continued use of *race* in biology requires that we deliberately reduce the requirements for subspecies classification as much of non-human biology still uses criteria that led Lewontin to the rejection of human races, i.e. relevant differences in a single-locus analysis instead of a multi-locus analysis as proposed by Edwards.

While this debate may be decided in one way or another, it will have to be decided on conceptual grounds by discussing what race concept we should employ. This is a striking contrast to the empirical shortcomings of race theories in the 19th century and suggests that the debate about the elimination of human races has greatly transformed from being mostly empirical and therefore close to the phlogiston end of the elimination scale to being mostly conceptual and therefore close to the redescription end. To sum up, there seems to be a historical shift on the gradual scale as illustrated in Fig. 1.

Fig. 1. Highly schematic location of different debates about race on a spectrum between the phlogiston model and redescription model, An elimination of Agassiz' (1854) races will be mostly an empirical issue while a possible rejection of Andreasen's (1998) and Edwards' (2003) account will have to be largely based on conceptual objections.

Of course, Fig. 1 is highly schematic and I do not want to claim that my sketchy presentation is sufficient for an evaluation of any of the discussed positions. Still, the general idea should be clear enough: Near the phlogiston-end are essentialist and polygenist theories that assume far-reaching mental differences between races. Agassiz' Arctic Race or Hottentot Race do not fare better than Gall's Organ of Faithfulness or Organ of

Good Humor. Apart from a very general statements ("there are biological differences between groups of humans" and "there are functionally specialized brain areas"), virtually every assumption turned out to be wrong. Arguably, Gall's phrenology fares even better than Agassiz' race theory as his general claim about functional specialization in the brain was at least non-trivial and rejected by many of his contemporaries. Somewhere in the middle, we find early post-war genetic races that had gotten rid of many flawed assumptions of 19th-century race theories but still remained vulnerable to empirical criticism as Lewontin et al.'s studies of the 1970s illustrate.

In their most recent reincarnations, human races are characterized in empirically modest ways that often make empirical objections and worries about the existence of races superfluous. For example, there is little reason to doubt the results of genetic cluster analysis and if we identify human races with genetic clusters, there is also little reason to doubt that human races exist. The tricky question is whether we should identify human races with genetic clusters. I assume that this interpretation of *race* on the gradual elimination scale has obvious advantages for discussions in the history and philosophy of science. On the historical side, the model not only confirms but also specifies the characterization of *race* as a "nonstable, hybrid, contextual category" (Gissis, 2008, p. 438, emphasis in original) by at least partly explaining its non-stable, hybrid, and contextual characteristics. This is not only true in a diachronic but also in a synchronic perspective that tries to understand the different fates of *race* in countries such as the USA and Israel. Gissis, for example, suggests that the Israeli "avoidance of the term [*race*] after the war indicates a consistency and perseverance worth questioning" and further "the existence of a powerful, cultural-emotional barrier concerning the use of 'race' in post-war Israeli society" (2008, p. 446). While Gissis' assumption of such a barrier is certainly well-justified, she does not make any attempt of understanding its role in scientific practice and therefore leaves readers with the impression of an illegitimate "cultural-emotional" bias in research.

My claim that the debate about the existence of races gradually shifted towards becoming a conceptual issue suggests a different interpretation of Gissis' "cultural-emotional barrier "as it is by no means surprising that different academic communities opt for different conceptual strategies on at least partly non-epistemic grounds. Conceptual choices in the human sciences are often based not only on considerations of research interests but also of a variety of social factors. Furthermore, it seems entirely legitimate to consider social factors such as effects in science communication, in education, or in clinical practice when the issue turns out to be largely about conceptual choices. This consideration of different uses of *race* also brings us to one crucial philosophical benefit of analyzing elimination debates in terms of a gradual model that incorporates empirical and conceptual issues. Often, debates about race (or intelligence, mental disorder, and so on) suffer from an insufficiently understood entanglement between factual and normative questions. On the one hand, eliminativism about race is often normatively motivated and many proponents of a continued use of *race* suspect eliminativist proposals to constitute ideological intrusions upon proper scientific research. At the same time, there is also a deep suspicion among many eliminativists that it would be naive to treat the question of the biological reality of races as an innocent factual question that has a purely empirical and objective answer. The proposed model can clarify at least some of this confusion by specifying roles for both empirical and normative considerations. If we fix the meaning of *race*, the question whether races exist has an empirical answer. For example, if we define races as populations with significant differences in a multi-locus genetic cluster analysis (as proposed by some contemporary proponents of racial disctinctions), we can empirically confirm the existence of races. If we define *race* as populations with a significant differences in a single-locus analysis (as common in many areas of non-human biology), we can empirically confirm the non-existence of races. The choice of a specific concept of race, however, is at least partly normative and leaves room for non-epistemic considerations such as Gissis' "cultural-emotional barrier". Indeed, this is the case for many elimination controversies and has already been mentioned in the context of hysteria. The extinction of hysteria is at least partly connected to social issues such as its limited use for patients given the availability of more specific diagnoses and its discriminatory uses as an allegedly female disorder.

Another benefit of framing philosophical debates about race in the gradual model is to disentangle often equated issues of reference failure and elimination by allowing a reasonable debate about the elimination of *race* without requiring prior commitment to a philosophical theory of reference. On the one hand, the debate about human races raises empirical questions regarding issues such as genetic differences or reproductive isolation between populations. On the other hand, it raises conceptual issues such as the question at what point we should call human populations *race*s. As both issues can be discussed without a commitment to a specific theory of reference, one can also have a meaningful debate about the elimination of races without addressing the question whether *race* fails to refer.

My sketchy presentation of the debates about race obviously leaves many questions open that need to be addressed in more careful historical and philosophical research. The same is true for my even sketchier presentations of phlogiston, phrenological organs, and hysteria. Still, I hope to have shown that discussions about elimination have to move beyond the common but often implicit phlogiston model and that it is helpful to approach these issues with a gradual model that incorporates both empirical and conceptual issues in scientific ontologies.

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