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Race and Reference

Abstract The biological race debate is at an impasse. Issues surrounding hereditarianism aside, there is little empirical disagreement left between race naturalists and anti-realists about biological race. The disagreement is now primarily semantic. This would seem to uniquely qualify philosophers to contribute to the biological race debate. However, philosophers of race are reluctant to focus on semantics, largely because of their worries about the 'flight to reference'. In this paper, I show how philosophers can contribute to the debate without taking the flight to reference. Drawing on the theory of reference literature and the history of meaning change in science, I develop some criteria for dealing with cases where there is uncertainty about reference. I then apply these criteria to the biological race debate. All of the criteria I develop for eliminating putative kinds are met in the case of 'race' as understood by 20th century geneticist Theodosius Dobzhansky and his contemporary proponents, suggesting that we should eliminate it from our biological ontology.

Keywords Race; Population Genetics; Theory of Reference, Flight to Reference, Racial Naturalism, Eliminativism

Introduction

Despite many confident claims to the contrary, there is no consensus on the scientific status of 'race'. Various scientific organisations, such as the American Anthropological Association (1999), have issued statements claiming that human populations are not races. However, such statements are often disregarded (Morning 2011). Leonard Lieberman and colleagues (2004) describe the rejection of race as a biological category as "high" in the U.S. and Canada, but only "moderate" in Europe, and "lowest" in Russia and China. If one looks closely at their findings, a complex picture emerges. Often cultural anthropologists (who do not specialise in biological diversity) are surveyed alongside physical anthropologists. And even when rejection of race is described as "high", many survey respondents still accept some minimal notion of biological race. While scholars often suggest otherwise, the biological race debate is ongoing.

Interestingly, there is widespread agreement about the science in the debate (Mallon 2006; Hochman 2016). We now know a lot about human biological (or at least genetic) diversity: how much there is and how it is distributed. Deflationists argue that it is really normative differences that fuel the debate (Mallon 2006; Gannett 2010; Ludwig 2015; McPherson 2015; Lemeire 2016). Whether or not this is true is not my focus (see Hochman 2017a). Given the widespread agreement about the science, the semantic differences are the ostensible barriers to finding a resolution. If we can resolve the semantic disagreements, we will either resolve the debate or it will morph into the kind of normative conversation the deflationists think we ought to be having.

There are many biological approaches to 'race' (e.g. Kitcher 1999; Pigliucci and Kaplan 2003; R. O. Andreasen 2004; Spencer 2014; Hardimon 2017). Responding to all of these versions of racial naturalism is beyond the scope of this paper. I will focus exclusively on a

highly influential view popularised by the great 20th century biologist, Theodosius Dobzhansky. As Lisa Gannett (2013) has documented, Dobzhansky's views about race changed over his career. From 1933–1939 he defined races as 'arrays of forms', then from 1940–1946 as 'genetically identifiable geographical populations', and finally—from 1947–1955—as genetically distinct 'Mendelian populations'. I will focus on the middle view, shortening 'genetically identifiable geographical population' to GIGP for convenience. I focus on GIGP naturalism about race because it has been, and continues to be, highly influential. GIGP naturalism was the version of racial naturalism endorsed by the 1950s UNESCO statements on race (UNESCO 1952), and this approach to 'race' has been revived in the 21st century after a worldwide genetic clustering study by Noah Rosenberg and colleagues (2002) breathed new life into the race debate (e.g. Edwards 2003; Leroi 2005; Sesardic 2013).

According to GIGP naturalism, "Races are defined as populations differing in the incidence of certain genes, but actually exchanging or potentially able to exchange genes across whatever boundaries (usually geographic) separate them" (Dobzhansky 1944b, 252). This is an unusually flexible—one might say weak—race concept. "A systematist", wrote Dobzhansky,

> may or may not find it desirable to break the chain of populations into two or more sections and to designate them by racial or subspecific names. If he does so, the divisions are quite arbitrary. His decision will be guided by considerations of expedience and by nothing else: the difficulties caused by the presence of populations intermediate between the arbitrary racial 'types' may or may not outweigh the convenience of having simple reference names applicable to some of the populations. (Dobzhansky 1944a, 139)

While this view may seem out-of-date, it is still influential. New statistical techniques have been created, and new genetic data have been made available, but when it comes to the *concept* of race, there has been a "return to Dobzhansky", as I will document in this paper.

A range of arguments against racial naturalism have been advanced. To name just a few: most human genetic diversity can be found within any given racialised group (Lewontin 1972); there are no human subspecies, and subspecies is a good candidate for a scientific synonym for 'race' (Hochman 2013a; Templeton 2013); current versions of racial naturalism deviate from the ordinary concept of 'race' and produces racial taxonomies which are vague and arbitrary (Glasgow 2009); racial naturalism does not help to explain social or psychological life, or correspond to folk racial taxonomy (Appiah 1996); and current versions of racial naturalism does not require "racial purity", but the concept of race itself requires that most of us are and have ancestors that were "racially pure" (Zack 2002).

GIGP naturalists want to revise the concept of race to avoid these criticisms. They accept that "races" aren't subspecies, that humans are not very genetically diverse, that racial taxonomy is arbitrary, that essentialism isn't essential to 'race', and so on, but they insist that race is real nevertheless (see Leroi 2005; Sesardic 2013). Noah Rosenberg and colleagues' (2002) work has been important for the revival of GIGP naturalism. They showed that even though human genetic diversity is relatively small, by correlating allele frequencies it is possible to distinguish geographic clusters that roughly map onto conventional racial categories. Rosenberg and colleagues' did not show that human genetic diversity is high, discretely distributed, or that it is captured by a privileged grain of analysis. In fact, they showed that the opposite was, by and large, true (see Rosenberg et al. 2005). What they found was genetically-identifiable-geographic-populations—GIGPs—that roughly resemble conventional racial categories.

Some have interpreted genetic clustering studies as vindicating racial naturalism without clarifying *why* such groupings count as races (e.g. Edwards 2003). Such authors may be well understood as GIGP naturalists, because they conceptualise "races" as 'genetically-identifiable-geographic-populations'. Quayshawn Spencer is not a GIGP naturalist, because he argues that the *racial* level of analysis "is just the level that corresponds to what ordinary folk in the U.S. mean by 'race'" (Spencer 2014, 41; for a reply, see Hochman 2014). The GIGP naturalist has no means by which to privilege any level of analysis as uniquely racial. If a geographical population is genetically identifiable, it can be called a 'race', if we so choose. Appealing directly to Dobzhansky, GIGP naturalist Neven Sesardic writes that ""major" races cannot be rigorously distinguished from other groups, as they don't have a qualitatively different status from other possible groupings" (2013, 290).

There seems to be something wrong with such a weak concept of race, but it is difficult to locate the error, given that the meaning of theoretical terms can change over time. Are differences between GIGPs *racial* differences, or has there been a change in topic? One way to answer this question would be to endorse a theory of reference—a theory about how words attach to the world—against which to test whether GIGP naturalists are still talking about 'race'. However, this strategy—the 'flight to reference'—is now widely rejected (Stich 1996; Bishop and Stich 1998; Bishop 2003; Braddon-Mitchell 2005; Mallon et al. 2009). The critics of the flight to reference argue that there is no single way in which words refer to things in the world, so trying to find a single determinative theory of reference is a fool's errand. The critique does not imply that theories of reference never capture how reference *happens* to be working in particular cases, but rather that they cannot tell us how reference *ought* to be working in any given case, including the case of 'race'.

In light of the critique of the flight to reference, we may be tempted to reject philosophy of language as irrelevant to metaphysical debate, or even to reject metaphysical debate itself.

These have certainly been common reactions in debates about race (Mallon 2006; Gannett 2010; Ludwig 2015; McPherson 2015; Lemeire 2016; but see Hochman 2016a). I will argue that this takes the criticism of the flight to reference too far: the flight to reference is only one way we might avail ourselves of the theory of reference literature when faced with ontological disagreement.

Drawing on the theory of reference literature, and also on the history of meaning change in science, I develop some criteria for dealing with cases where there is uncertainty about whether a putative kind refers, such as we find in the biological race debate. These criteria can be used as norms for rejecting putative scientific kinds, instead of relying on theories of reference. I propose that putative kinds are *candidates* for elimination when (1) a radical change in definition is proposed, (2) the putative kind is trivialised, and (3) a new and successful theoretical system, with alternative terminology, is introduced. This list is not meant to be exhaustive, and no single criterion is assumed to be sufficient for eliminating putative kinds. However, when all three of the criteria are met in any particular instance, we have a strong case for eliminating that putative kind from our ontology. We can see the criteria as weak norms, with a cumulative force.

I will show that all three of the criteria are met in the case of race-as-GIGP. Race-as-GIGP is thus an excellent candidate for elimination. The argument is not that there are no genetically identifiable geographical populations in our species, but that such populations should not be thought of as human races. GIGP naturalists have changed the topic from 'race' to 'population'. They are not really talking about 'race'. Or rather, we should decide that they are not. If the flight to reference is a mistake—and I will be assuming that it is—then we need to accept that there is no one way that reference works. As such, we need to talk about alternative ways of settling semantic disputes. That is one of the main aims of this paper.

In the first section, I take the flight to reference twice, using the two main theories of reference and race-as-GIGP as my example, and I reach contradictory conclusions about the reality of races-as-GIGPs. If there is no single true theory of reference, as the critics of the flight to reference maintain, then the reality of races-as-GIGPs remains underdetermined. In the following section, I extract criteria from the theory of reference literature for dealing with cases where there is uncertainty about reference. In the final section before the conclusion, I trial these criteria on GIGP naturalism, and I show that all three of the criteria are met, giving us good reason to eliminate race-as-GIGP from our biological ontology.

Taking the Flight to Reference

The flight to reference consists of three main steps. First, one presents an account of how words relate to the world—a theory of reference. Second, one argues that according to that theory of reference a term of interest either refers or fails to refer. Third, one draws an ontological conclusion.

With race-as-GIGP as our example, let us take the flight to reference, to see where it leads us. First, we need a theory of reference. Saul Kripke (1972) introduced the *causal historical theory of reference*, and Hilary Putnam (1975) then devised a more sophisticated extension of the theory to natural kind terms. According to the causal-historical theory, a term is coined after contact with a putative natural kind. The term then extends to everything with the same structure. For example, a term such as 'iron' is introduced after contact with samples of iron and the term then refers to everything with that structure. The term gets "fixed" to something, and then it is "borrowed" by future speakers, resulting in a causal-historical chain of successive users of the term. If the term is "fixed" to something that indeed has some

structure to be explained, and current users of the term have "borrowed" it from early users of the term, then the term can be said—on the causal-historical theory—to refer.

We have completed the first step in our flight to reference—we have our account of reference relations. Now we plug in our term, 'race', to see whether it refers. First, we need to know something about early users of the term. What did they come into contact with that led them to introduce the term? Initially it was traveller reports from around the globe, describing variations in the human form. Because what counts for the causal-historical theory is the underlying causal structure that prompted the naming of the kind, we must ask what that structure was. For iron, we would say "the atomic number 26". But racial classification was not an attempt to describe something with a consistent internal structure—it was an attempt to describe variation. So the object of interest is human biological diversity itself.

According to the causal-historical theory of reference, it does not matter if early racial theories about human biological diversity were mistaken. If there is an historical chain connecting the original and current users of the term, and that term is used to describe some genuine phenomenon, then it can be said to successfully refer to that phenomenon, whatever it may be. Human biological diversity is certainly a genuine phenomenon. There may be no major divisions in the human species and no objective number of human types (Templeton 2013; Maglo, Mersha, and Martin 2016). But there are GIGPs (Rosenberg et al. 2002). Furthermore, contemporary race naturalists who recast 'race' as GIGPs are historically linked to early race theorists, even if their conception of race is radically different (e.g. Edwards 2003; Leroi 2005; Sesardic 2013).

Using the causal-historical theory of reference it seems legitimate to say that 'race' refers to GIGPs because the human biological diversity that caused the introduction of racial terms can be understood—if a little gene-centrically—as due to the existence of GIGPs, and there is a causal chain historically connecting early race theorists to 20th and 21st century GIGP

naturalists (Appiah 1996). And here we take the third and final step in the flight to reference, which is to make an ontological claim—that 'race exists', or 'there are human races'.

Unfortunately for the GIGP naturalist, one does not get the causal-historical theory of reference for free. There are other, competing theories of reference.

Let us consider the other major account of reference relations, the *descriptive theory of reference*. The descriptive theory, as applied to natural kinds, was formally introduced by David Lewis (1970). According to this approach, a kind term refers when the world contains the entities that answer to its associated description. Let us take the flight to reference again, this time using the descriptive theory.

Does 'race' refer, according to this account of reference relations? To answer this, we need to know the description associated with the term. According to the descriptive theory, theoretical terms are "implicitly defined by the causal patterns specified in the theory that introduces the terms" (Stich 1996, 31). This presents a difficulty when it comes to 'race', because the race concept evolved over time from a cluster of ideas with no single origination point (Hannaford 1996). As such, any definition of race will have a stipulative, as well as a descriptive, element. That is, it will stipulate which of the many early conceptualisations of race we *ought* to be thinking about.

In 1950, Walter Scheidt argued that Immanuel Kant produced "the first theory of race which really merits that name" (1950, 372). Scheidt's argument has stood the test of time. Robert Bernasconi has made a convincing case that Kant has the dubious honour of being the "inventor" of the scientific race concept, in the sense that he "gave the concept sufficient definition for subsequent users to believe that they were addressing something whose scientific status could at least be debated" (Bernasconi 2001, 11). It is now widely accepted that Kant gave the concept its first stable scientific definition, rigorously distinguishing it from other concepts such as 'species' and 'variety' (Scheidt 1950; Eze 1997; Bernasconi

2001; Larrimore 2008; Sloan 2014). Even Claude-Olivier Doron, who emphasises Buffon's role in the formation of the scientific race concept, acknowledges that "Kant… gave Buffon's claims a decisive epistemological status" (Doron 2012, 91).

Consider the "competition". François Bernier (1684) did not distinguish between 'race' and 'species'. Linnaeus (1750) did not use the term race, but rather 'variety'. Importantly, 'variety' had no genealogical dimension, and was intended to be a mere category of convenience, rather than a scientific category. Buffon (1749) added a genealogical dimension, but where Bernier failed to distinguish between 'species' and 'race', Buffon seemed to use 'race', 'variety', 'species', and 'nation' interchangeably. Johann Friedrich Blumenbach is another contender. However, in the first two editions of *On the Natural Variety of Mankind* Blumenbach used the Linnean category 'variety' almost exclusively. As Bernasconi notes, "even when in the 1790s Blumenbach came to adopt Kant's terminology it is not clear he fully understood" (personal communication). Furthermore, as Nicholas Hudson explains, "in the work of Blumenbach, it [race] specifically denoted visible differences of physiology rather than a common stock" (1996, 257).

Kant theorised 'race' in contradistinction to 'variety'. He distinguished between *Naturbeschreibung*, the description of nature, and *Naturgeschichte*, the history of nature. According to Kant, 'race' does not belong in a systematic description of nature because such a description needs to deal with clearly defined and primarily constant characteristics. The description of nature, when directed to the human species, produces only varieties. What distinguishes the "races" from these innumerable varieties is a history of inheritance that gives the "races" constancy over time. Natural history promised to "transform the presently overly detailed artificial system for the description of nature into a physical system for the understanding" (Kant 1777, 13). Varieties were associated with artificial descriptions of

nature, whereas "races" were associated with the genealogical and scientific practice of natural history.

In order to determine whether the descriptive theory of reference entails that 'race' refers, we ought to draw our description from Kant, for the simple reason that he gave race a coherent place in conceptual space by conceiving of it as a within-species category that was distinct from other categories such as 'variety'. However, how tightly we keep to Kant's description of 'race' is an open question. Mark Larrimore explains that, "When first invented, race was…conceived in terms of schemes of human diversity we barely remember" (2008, 342). For instance, Kant believed that humans contained "racial seeds" (*keime*) that were triggered by the climate, just as a cold climate triggers growth of a second layer of feathers in some birds. Furthermore, one might argue that Kant's theory was immediately refuted. Kant relied on skin colour to classify the "races". In 1775, the year Kant published his first essay on 'race', John Hunter showed that people of different so-called races could have the same skin colour, and Georg Forster made criticisms along similar lines (see Bernasconi 2001, 17). "It is ironic", writes Bernasconi, "that at the very time that Kant was giving the concept of race intellectual coherence, his criterion for distinguishing the different races was collapsing" (2001, 17).

Of course, it is not surprising that Kant's biological theory and taxonomic methodology are outdated. In determining whether 'race' refers according to the descriptive theory of reference, we might pull back from the fine-grained specifics of Kant's racial theory, to see if there is a general idea worth investigating. I believe that there is, and that it can be expressed in the following stipulative definition of 'race': races are major within-species biological

groups and lineages, formed due to reproductive isolation, in which membership is transmitted through biological descent.[1]

A benefit of this definition is that it is suitably broad, allowing for flexibility and change in the concept. For instance, it allows for a genetic focus, which was not a possibility in the 18th century. It also allows for change in what counts as a 'major' biological difference between groups (by introducing thresholds) and change in how we approach the genealogical dimension (with phylogenetic analysis). The above definition simply retains what makes the idea of race interesting and conceptually distinct. It describes a substantive theory about human biological diversity, which is intuitively plausible.

If we conceive of Kant's theory as expressing this general idea, does 'race' refer, on a descriptive approach to reference? To answer this question, we would need to adopt some methodology for determining whether our species splits into major biological lineages or subgroups. The obvious approach would be to test for human subspecies, because subspecies are major divisions within a species, just as Kant imagined races to be. While there are significant differences in how Kant understood 'race' and how biologists currently understand the subspecies category, Kant could be read (in contemporary terms) as defending *something like* the view that there are human subspecies.

[1] This definition is very close to the definition of 'populationist race' offered by Michael Hardimon (2017) in *Rethinking Race: The Case for Deflationary Realism*. However, Hardimon clarifies that "populationist race is a minor principle of human genetic structure and that populationist race is a minor principle of human variation" (2017, 124). As the title of Hardimon's book indicates, his approach to 'race' is *deflationary*. Mine is not only deflationary in the sense that it does not involve any claims about "racial" hierarchy. Otherwise, my account is substantive.

Some biologists use a genetic diversity threshold for subspecies division. While chimps can be divided into subspecies using the threshold, humans are well below it (Gonder et al. 2011; Templeton 2013). Of course, the threshold is subjective. Those who worry about such subjectivity may be persuaded by E. O. Wilson and W. L. Brown's (1953) scepticism about subspecies. However, Quayshawn Spencer has recently argued that,

> In the case of subspecies, what constitutes taxonomic difference is being an incipient species. So, even if a threshold of genetic difference is biologically arbitrary, that fact does not imply that the geographic race itself is biologically arbitrary, just that our quantitative tests for subspecific difference are biologically arbitrary… a good qualitative genetic test for subspecific difference would be identifying genes for incomplete hybrid sterility. When two groups can produce viable and fertile hybrids, but viable and fertile hybrids are limited to a particular type (e.g., a particular sex), that is known as incomplete hybrid sterility. (Spencer 2018, 3)

While 'subspecies' may be an objective category on this view, there is no incomplete hybrid sterility in humans, so there are no human subspecies on Spencer's measure.

Another approach to subspecies treats them as evolutionary lineages within a species. Robin Andreasen (1998) defends the view that races are subspecies in the sense that they are monophyletic groups or clades: ancestor-descendent sequences of breeding populations that all trace back to a common origin. Spencer (2018) offers a sympathetic evaluation of Andreasen's cladistic approach, but argues that it is ultimately unsuccessful: "there is nothing about Andreasen's boundary for breeding populations that guarantees that breeding populations are genealogical groups" (Spencer 2018, 7). Moreover, Andreasen's central claim is that there were human subspecies *in the past*. She accepts that "if we focus on the synchronic question—is there any justification for dividing current populations into races—

the answer may very well be 'no'" (1998, 215). Insofar as the biological race debate is about whether there are *currently* biological races, Andreasen holds an agnostic position.

Even if we were to focus on the past, Alan Templeton's reconstructions of human evolutionary history suggest that human populations are not branches on an evolutionary tree. "A treelike structure among humans has been falsified whenever tested" (Templeton 2013, 262). In his analysis, "there is not one statistically significant inference of splitting during the last 1.9 million years" (Templeton 2013, 269). Human evolutionary history is more bush-like than tree-like. There is only one human lineage: *the* human lineage.

So, to take the last step in our second flight to reference, this time using the descriptive theory of reference, we conclude that racial theory has no realization, and that GIGP naturalists must be talking about something other than 'race', because race does not exist.

After the Flight to Reference

We have now applied the two major accounts of reference relations and come to conflicting conclusions about the existence of human races. How are we to decide which one of these (or indeed any alternative) is the right account, and hence whether or not race is real? In *Deconstructing the Mind* Stich (1996) argues—convincingly, in my view—that there is no single determinative theory of reference. Arguments for and against theories of reference tend to be based on our intuitions, and people have conflicting intuitions about reference (Machery et al. 2004). If there were a science of reference relations that told us how reference *really* works, then perhaps this could be overcome. But there is no such science, and one does not appear to be forthcoming (Stich 1996). As such, we cannot appeal to a purportedly determinative theory of reference to solve metaphysical disputes.

One reaction to the critique of the flight to reference would be to insist that a science of reference relations is forthcoming, and that it will likely involve a hybrid approach to reference, such as that defended by Philip Kitcher (1993). However, a hybrid account is not immune to Stich's critique. Indeed, Kitcher's theory is one of the main targets of Michael Bishop and Stich's (1998) article on the flight to reference.

Another response would be to ignore issues to do with reference in metaphysical debate. If it is philosophically dubious to use a putatively determinative theory of reference to settle ontological disputes, then this seems like an appropriate response. Yet philosophy of language may still have an important role to play. The narrative behind *Deconstructing the Mind* supports this claim. In this book Stich describes his realisation that he had been defending a view for many years—eliminativism about beliefs—without realising that it was premised on a descriptive approach to reference, and that one does not get a theory of reference for free. "This book", writes Stich, "is about the unravelling of a philosophical position" (Stich 1996, 3). With Bishop, Stich later describes the flight to reference as a recipe for "How *Not* to Make Progress in the Philosophy of Science" (Bishop and Stich 1998). This may be right, but note that Stich's realisation about the implicit role the descriptive theory of reference played in his argument for eliminativism about beliefs led directly to the re-examination of his long-held eliminativist position. This narrative suggests that it might be useful to think more about reference, rather than less.

The relevance of the theory of reference literature becomes obvious when we consider that in defining terms *we cannot help but make use of some approach to reference*. If we define a putative kind by associating it with some phenomenon ('let's call this thing x') we are implicitly using something like the causal-historical theory. If we stipulate that a putative kind refers if the world contains the entities that answer to its associated description ('this thing, x, must have the following characteristics…') then we are using something like the

descriptive theory of reference. We can't avoid making assumptions about reference when we define our terms: it is a part of the act of defining. Given this fact, it may be useful to abstract some lessons from the theory of reference literature—where the pros and cons of different theories of reference have been analysed—and apply these lessons to live debates. That is, we may be able to draw some norms from the theory of reference literature that could be applied to help us decide whether we should eliminate or conserve putative kinds, such as 'race'.

The first thing to note is that causal-historical and descriptive theories of reference are understood to enjoy inverse advantages and suffer from inverse problems (Devitt and Sterelny 1999; Lycan 2000). Let us first consider the advantages of the respective theories. The causal-historical theory captures an important lesson from the history of science: the fate of a putative kind does not always depend on whether the world contains the entities that answer to its associated description. 'Atom' and 'electron' are thought to refer, despite nontrivial changes in their associated descriptions. The descriptive theory, on the other hand, captures the inverse lesson: sometimes the description associated with a putative kind is so mistaken that it (and/or the theory to which it belongs) is rejected as non-referring. 'Phlogiston' and 'electric effluvia' are examples of such putative kinds.

Let us now consider the associated disadvantages of these theories of reference. According to the descriptive theory, the terms 'atom' and 'electron' do not refer because the initial descriptions of them were radically mistaken. This seems implausible given their ongoing centrality in physics and chemistry. On the other hand, the causal-historical theory allows that 'phlogiston' refers to oxygen, because it is really oxygen that is responsible for the processes (combustion and calcination) that phlogiston was supposed to explain (Enć 1976, 267). The idea that phlogiston-talk successfully referred to oxygen seems just as mistaken as the claim that there are no atoms or electrons. The descriptive theory of reference makes it too easy (in

the long run) to eliminate putative kinds, while the causal-historical theory makes it, under most circumstances, very difficult to eliminate them.[2]

Criteria for eliminating putative kinds ought to incorporate these lessons from the theory of reference literature. Successful reference should not be too easily assured, or too difficult to achieve. This needs some qualification. There are posits of well-established theories that everyone agrees refer, and in such cases reference does seem easily assured. However, these cases are different from those where there is a live debate, and reference is uncertain. In those cases, we should attempt to strike a balance. If the conditions for successful reference are too strict, this will favour eliminativism. If they are not strict enough, this will favour conservationism. All else being equal, we should not favour either the conservation or elimination of putative kinds; as I will now explain, there are problems with favouring either position.

When we tightly maintain the description of the kind offered when it was first introduced, leaving no room for changing the description as we find out more about the thing described, eliminativism is favoured. This is not only a problem because it does not fit with the history of science. It is also a problem because, as Ernst Mayr observed, "Conceptual systems and research traditions… often change drastically and rapidly and in these cases it would be necessary [when using a descriptive theory of reference] to select a new type every time the system in question has changed to such an extent that the type no longer typifies it. And every selection of a new type would presumably require a new name for the system. Such procedure might theoretically salvage unambiguity, but it would be dreadfully cumbersome" (1982, 510). In some cases, ambiguity might be such an issue that we accept a more or less cumbersome solution, but in general, the consequence of accepting the descriptive theory of

[2] Kripke (1972) suggested that if the original sample turns out to be too heterogeneous, then the term does not refer. However, what counts as "too heterogeneous" varies with context.

reference (thus making long-term successful reference quite difficult) would be an endless proliferation of kind terms.

The conditions for successful reference should not be too easy to meet, either, favouring conservationism. This can happen when we associate a putative kind with some phenomenon (e.g., stars are the bright lights in the evening sky), rather than with descriptive content (e.g., stars are holes in the celestial sphere). As the causal-historical theorists point out, this is sometimes just what happens. There is nothing "wrong" with this in a general way, but it can be a problem when the causal-historical theory is adopted by conservationists in a live metaphysical debate. As long as there is a genuine phenomenon to explain, and a historical chain between those who introduced the term and its current users, the term can be said to "refer" even when the definition of that term changes radically. This would make for successful reference, but at the cost of trivialising such success.

Trivialising reference is a serious problem for the causal-historical theory. As Larry Laudan argues, the causal-historical theory:

> countenances as genuinely referring all manner of ill-developed theories. It can be argued, for instance, that Aristotle's conception of natural place plays many of the same causal and explanatory roles as Newtonian gravitational forces. Cartesian vortices perform a similar function. Yet is anyone seriously prepared to maintain that Aristotle and Descartes, whose conceptual frameworks were so antithetical to action-at-a-distance, were really 'referring', had they but known, to gravitational attraction? (Laudan 1984, 160)

The causal-historical theory entails that when Aristotle wrote about "natural place" and when Descartes wrote about "vortices" they were really talking about gravitational attraction, and indeed that Newton and Einstein were co-referring with 'gravity'. Even if one were to accept the latter, the former cases seem wildly implausible. It trivialises progress in science to

suggest, as the causal-historical theory does, that Aristotle and Descartes were successfully referring to gravitational attraction, in either Newton or Einstein's sense.

Yet there is not a clear line between cases where the same thing is being talked about in a new way and cases where there has been a change of topic, as we seem inclined to say there was between Aristotle, Descartes, and Newton. Perhaps there is nothing very deep behind our intuitions about what counts as a change of topic. Take the stock example of a failed kind, phlogiston. Lavoisier introduced the oxygen theory and it replaced the phlogiston theory. But it need not have been this way; history could have been different. It has been suggested that the replacement of phlogiston with oxygen may have depended on something to do with Lavoisier's personality:

> had Lavoisier wished to be viewed less as a radical innovator and more as a conservative, he might have retained a venerable old term rather than introducing a new one. And rather than maintaining that there is no such thing as phlogiston, he might instead have claimed that Priestley, Stahl, and earlier theorists were simply mistaken about lots of the properties they attributed to phlogiston. So, if Lavoisier had a somewhat different personality, what we now call "oxygen" would be called "phlogiston" instead. (Stich 1996, 68)

There are two important things to note here. First, the act of defining a new term offers the opportunity to break with an old one, and it is bad news for proponents of the old term, especially when the new term is attached to a successful new theory. Second, our ontologies may not be as rational as we might like them to be, and there might not be anything deep behind our intuitions about reference.

Some may find something worrying about the idea that personality could play such a large role in determining the fate of theoretical posits. Stich's example suggests that if history were different and Lavoisier had retained the term phlogiston (radically changing its meaning so that its description fits what we now think of as oxygen) we would probably think it just as

natural as the retention of kinds such as ‘atom’ and ‘electron’ despite changes in their descriptions. What drives our intuitions may have a lot to do with past consensus formation. How we end up using terms just seems intuitively correct. And how we end up using scientific terms is, according to Stich, a mixture of very sensible principles of rational ontological inference (Donovan, Laudan, and Laudan 1988), and more or less arbitrary factors: implicit previous agreements about the properties necessary to a posited entity and social and political factors both internal and external to the relevant science (Stich 1996, 66–70).

One reaction to such a conclusion would be to throw one’s hands up and forget about trying to influence the fate of theoretical terms. This may be the right attitude in one situation and the wrong attitude in another. It may be the right attitude for cases where the semantics have already been settled. While we may be willing to argue that there are *some* cases in the history of science where scientists should have kept or abandoned a particular theory or theoretical entity but did not, on the whole it is sensible to work with the ontology we have inherited. Few would be willing to endorse the wholesale upheaval of scientific ontology which would be the consequence of adopting either the descriptive or the causal-historical theory of reference.

When the semantics have not been settled, however, it makes good sense to think about the conditions under which we should say that particular kind terms refer. Drawing on the discussion so far, I will now propose some criteria for the elimination of putative kinds. I do not assume that the list is exhaustive or that any single criterion gives reason enough for eliminativism. However, if all three criteria are met, there is strong reason to eliminate the putative kind from our ontology.

Criterion 1. A radical change in definition is proposed.

Sometimes when there is a substantial change in the way some phenomenon is understood, the original term used to describe it is rejected as non-referring. Despite the empirical successes of phlogiston theory, there is no such substance emitted in combustion; there is no phlogiston. And there is no caloric fluid or electric effluvia, either. A major problem for the causal-historical theory is that it allows for the radical redefinition of putative kinds when anti-realism about them might be more appropriate. David Braddon-Mitchell puts the problem forcefully: "Since *something* has caused our uses of the terms of the theory, it seems that our terms may end up referring even in cases where the theory is so hopeless, and the causes of our use of these terms so unlike how we imagined them, that we should surely conclude that they don't refer" (2005, 158). Putative kinds are thus candidates for elimination when their proposed new definition differs radically from their original definition.

Criterion 2. The putative kind is trivialised

One way of dealing with large differences between the original and the current best description of some phenomenon is by changing the meaning of the associated term. Sometimes this means changing one substantive meaning for another; sometimes this means trivialising the term. However, by trivialising a term, we turn something that promised to be explanatorily useful into something that is not. Often the reason for introducing a putative kind is that it promises to be explanatorily useful, so if it turns out not to be we have cause to eliminate it from our ontology. For this reason, trivialisation is to be avoided, and putative kinds are candidates for elimination when their definitions are weakened to the point where successful reference is so well ensured that it would be trivially true to say that they refer.

Criterion 3. A new and successful theoretical system, with alternative terminology, is introduced

Putative kinds are candidates for elimination when they can be replaced by the kinds associated with successful new theoretical systems. The idea of "natural place" was replaced by Newtonian gravitational forces, just as oxygen replaced "phlogiston". The introduction of a new term is not necessarily a death knell for an old one, but it does not bode well, especially when the new term is connected to a theory that can better explain the target phenomenon and is free of historical baggage.

When one or two of these criteria are met, there is a prima facie case for elimination. When all three are met, that case is strong. In the following section, I demonstrate the use of these criteria through applying them to the biological race debate.

Are Genetically-Identifiable-Geographic-Populations Races?

If the above criteria are useful, we should be able to apply them to a case study and move that debate in the direction of a consensus. The best cases will be those where there is agreement about the properties of the target phenomenon, but disagreement about how it should be described, so that we can bracket the science and focus on the semantics. There is a tendency to think of such cases as philosophically uninteresting, as "merely semantic". This is a mistake. There are, as David Chalmers (2011) argues, substantive semantic disputes—semantic disputes that really matter. The debate about the reality of race is one of them: whether or not race is biologically real matters in a wide range of domains.

While some leading philosophers of race, such as Andreasen (2000) and Anthony Appiah (1996), have drawn on theory of reference to defend (respectively) realism and anti-realism about race, there is increasing scepticism about such an approach. Following Stich, Ron Mallon argues that, "approaching the metaphysics of race via finding a *determinative theory of reference* for racial terms or concepts is unlikely to be fruitful" (2006, 549, emphasis added). Gannett (2010), Olivier Lemeire (2016), David Ludwig (2015), and Lionel McPherson (2015) all make similar arguments. Most of these theorists pose this argument as a challenge to the metaphysics of race as a field (for a reply, see Hochman 2016a). However, my use of the theory of reference literature does not depend on there being a determinative theory of reference. Instead, I have drawn norms from the theory of reference literature that can be applied to help us decide whether we should eliminate or conserve putative kinds, such as 'race'.

Criterion 1 applied to race-as-GIGP

Let us now see whether the criteria developed in the previous section are met in the case of race-as-GIGP. Recall that the first criterion for the elimination of putative kinds is met when the proposed new definition of a term differs radically from its original definition. GIGP naturalism is indeed very different to the race theory Kant advanced in the late 18th century, which is a good candidate for the first scientific theory of race, according to Bernasconi's (2001) criteria. I will now discuss some of the major differences.

Difference 1. The number of races

Kant argued that "we only need to assume four races in order to be able to derive all of the enduring distinctions immediately recognizable within the human genus" (Kant 1777, 11). The four "races" changed between the original 1775 essay and the 1777 revision. In the latter, they are listed as:

Lineal root genus
White of brownish colour

First race
Noble blond (northern Europe) from humid cold

Second race
Copper-red (America) from dry cold

Third race
Black (Senegambia) from humid heat

Fourth race
Olive-yellow (Asian-Indians) from dry heat

Kant believed that "once a race has established itself as the result of a long residency…no further climatic influences could cause it to change into another race. For only the lineal formation can turn into a race" (Kant 1777, 11). Kant allowed for mixes and for instances "when a people has not yet lived long enough in a specific climate to take on fully the character of the race peculiar to the climate" (1777, 11), but these groups do not count as "genuine races" (1777, 13).

Kant's introduction of only four human "races" was motivated by his desire to transform overly detailed and artificial *descriptions* of nature into a scientific and precise system representing the *history* of nature. When the Rosenberg et al. (2002) worldwide population-genetic study was published, it was met with great interest by race naturalists because it

seemed to vindicate something like a Kantian picture of a handful of geographic races. "In an important paper that came out in Science at the very end of 2002", writes Sesardic:

> a group of geneticists showed that the analysis of multilocus genotypes of 1,056 individuals from 52 populations did allow an inference of group structure and that, furthermore, five clusters derived from that analysis of purely genetic similarities corresponded largely to major geographic regions (Rosenberg et al. 2002). This is an important discovery that makes it much more difficult than before to claim that race is entirely disconnected from genetics. (2010, 153)

The Rosenberg study has, in John Hartigan's words, "become a touchstone in the efforts to argue that there is a genetic basis for race" (2008, 175).

There has, unsurprisingly, been criticism of racial interpretations of the Rosenberg study (e.g. Hochman 2013b, 2016b; Maglo, Mersha, and Martin 2016; Atkin 2017; Wills 2017). Elsewhere, I have critiqued the "new racial naturalism" on a number of grounds (Hochman 2013a). For current purposes, what I call the "grain-of-resolution problem" is relevant. The Rosenberg study is often interpreted as a vindication of the view—defended by Kant and others over 200 years ago—that there are a mere handful of "races". However, Rosenberg and colleagues did not show this. They do not claim that $K = 5$ (five clusters) is a privileged grain of analysis. Their finest grain-of-resolution in their worldwide analysis is $K = 6$, and they also conducted a within-continent analysis because, as they explain, multiple clustering solutions appeared for $K = 7$ because of the complexity of their data (Rosenberg et al. 2002 p. 1 of Supplementary information).

What justifies the claim that there are a handful of "races", and not hundreds, or even thousands of them? Nothing, according to GIGP naturalists. "In some sense it is odd that the objection based on the number of races keeps reappearing because Dobzhansky defused it already half a century ago", writes Sesardic. "In principle we might introduce names for

hundreds or even thousands of human groups that we could call races on the grounds of their genetic differentiation" (Sesardic 2013, 290). The same view is expressed by Armand Leroi: "there is nothing very fundamental about the concept of the major continental races", he writes, "they're just the easiest way to divide things up. Study enough genes in enough people and one could sort the world's population into 10, 100, perhaps 1000 groups, each located somewhere on the map" (Leroi 2005, 4). Here the "return to Dobzhansky" is its most explicit.

Sesardic is correct that Dobzhansky believed that there was no way to number the races. "Boyd has recognized five, and Coon, Garn, and Birdsell nine or thirty or thirty-two races", wrote Dobzhansky. "Does it follow that some of these classifications are necessarily wrong? No, all may be right" (Dobzhansky 1962, 266). But did Dobzhansky "defuse" the objection, as Sesardic claims? Clearly he did not. GIGPs sound more like Linnaean 'varieties' than Kantian 'races'. It is ironic that GIGP naturalists understand 'race' as something closer to 'variety' than 'race' in Kant's sense, and allow that there could be races in the thousands. This is just the kind of subjective and artificial approach to taxonomy that natural history, with its now scientifically respectable category of race, was supposed to supersede.

Difference 2. The amount of difference between 'races'

GIGP naturalism about race allows that one can apply "the term 'racial differences' to distinctions as small as those that can be found between populations of neighboring villages and as large as those between populations of different continents" (Dobzhansky 1946, 101). This is a divergence from Kant's race theory, according to which races were major human groupings. Dobzhansky wondered, "Might one modify the definition of race by specifying that the differences in gene frequencies be above a certain minimum magnitude?" He thought

that this would be undesirable: "since all magnitudes of difference are found among populations, any specified minimum can be only arbitrary" (Dobzhansky 1946, 101). This is true, but herein lies the problem. Rather than saying that racial difference may be miniscule, we might want to say that we are no longer talking about racial difference when we talk of minor genetic differences between geographic populations. And it is widely accepted that human genetic diversity is comparatively very low, and residing primarily *within* any given racialised group (Lewontin 1972; Rosenberg et al. 2002).

Difference 3. How 'racial' variation is distributed

Kant (1777) divided the four "races" according to colour. We now know that skin colour is not distributed in discrete racial bio-packages. It is smoothly distributed in a clinal fashion (Barsh 2003). Clinal or gradual distribution of characteristics has traditionally been understood to be incompatible with racial classification. As Frank Livingstone famously wrote in his debate with Dobzhansky, "There are no races, there are only clines" (1962, 279).

One of the reasons that the genetic clustering study by Rosenberg and colleagues was received with such enthusiasm by race naturalists was that it represented human population structure in relatively discrete groups, or clusters. However, it soon became clear that this was a product of how the clustering software works. Geographic distance alone explains at least 75% of the genetic variance between human populations, and only an extra ~2% is captured by adding information on genetic clusters (Handley et al. 2007). As Rosenberg et al. explain, "allele frequency differences generally increase gradually with geographic distance. However, small discontinuities occur as geographic barriers are crossed, allowing clusters to be produced" (Rosenberg et al. 2005, 661). Human genetic diversity may be best explained by a synthetic model of clusters and clines, but if we have to choose, a clinal model captures

the nature of human biological diversity and population structure much better than a clustered model.

The first criterion for eliminating putative kinds is met in the case of race-as-GIGP. The proposed new meaning for 'race' differs radically from the first scientific definition of race. This gives us some reason to say that GIGP naturalists have changed the topic.

One might raise the following criticism at this point. I have argued that a radical change in definition counts against race-as-GIGP. Am I not endorsing the descriptive theory of reference, and taking the flight to reference myself, the very move that I argue against? This criticism is not successful. I do not endorse the descriptive theory as a determinative theory of reference. If I did I would have only needed the first criterion for the elimination of putative kinds. However, the first criterion is not sufficient. The meaning of kind terms can change. If only the first criterion were met, this may not be enough to justify anti-realism about race-as-GIGP. However, if all three criteria are met, we have a strong case for eliminating race-as-GIGP from our biological ontology. Let us move on to the second criterion.

Criterion 2 applied to race-as-GIGP

The second criterion for eliminating putative kinds is met when the putative kind is trivialised. GIGP naturalists do not merely change the meaning of 'race': they weaken it. They weaken the criteria necessary for inclusion under the term, extending its application enormously. To illustrate this, we can ask, "What would it take, on the GIGP approach, for race *not* to be real?" According to Dobzhansky *every* difference in gene frequencies between groups could be understood as a racial difference. However, only species with just one living member, species of clones, and certain panmictic species are without genetic variation. Such

species are rare, and nobody thinks that humans are one of them. Therefore, to say that there is racial difference in humans on the GIGP definition of 'racial difference' tells us *nothing of any interest at all about human biological diversity*.

Dobzhansky made a distinction between 'racial difference' and 'race'. He believed that "whether races should or should not be named, and if they should, how many should be recognized, is a matter of convenience and hence of judgment" (1962, 280). For Dobzhansky, "it does not follow that any racially distinct populations must be given racial (or subspecific) labels" (1962, 280). On the GIGP account, populations count as races if we choose to name them as races, but their reality would be trivial in the sense that it tells us nothing of any substantial biological interest. The triviality of race on the GIGP account is another reason to eliminate 'race' from our biological vocabulary, and to conclude that GIGP naturalists about race have changed the topic from race to something else.

Note that my claim is that race-as-GIGP trivialises 'race', not that population differences themselves are necessarily trivial. Consider the following example. Mylopotamos is a municipality in northern Crete where the population has a diet that is high in animal fat. This should cause serious health problems, but it doesn't, because the population has a variant, rs145556679, that is cardioprotective (Southam et al. 2017). This is not a trivial population difference: it is a lifesaver. However, it would trivialise 'race' to say that the rs145556679 variant constitutes a *racial* difference.[3] Historically, 'race' was meant to describe *major* human groups.

[3] Thanks to Rachael Brown for this example

Criterion 3 applied to race-as-GIGP

Recall that the third criterion for eliminating putative kinds is met when an alternative term, associated with a new and successful theory, is introduced. In the case of GIGP naturalism, this term is 'population'. As Gannett explains, "many geneticists and anthropologists have abandoned the term 'race' and there is no doubt that Dobzhansky's definition of 'race' as any genetically distinct population facilitated its own demise given that the far less controversial and more technical-sounding 'population' could so easily substitute for it" (2013, 486). However, many geneticists, anthropologists and others did not abandon racial classification, as Gannett is aware. "It is widely assumed", she writes, "that as a result of the evolutionary synthesis and rise of population genetics, populations replaced races in genetic research, with race becoming an illegitimate concept in biology" (2013, 250). However, as she observes, "The assumption is not wholly accurate", and in fact, "The evolutionary synthesis and rise of population genetics saw race redefined in populational terms" (2013, 250).

In response to Dobzhansky, the British geneticist L. S. Penrose wrote that he was unable to "see the necessity for the rather apologetic retention of the obsolete term 'race', when what is meant is simply a given population differentiated…merely by a gene frequency peculiarity. The use of the almost mystical concept of race makes the presentation of the facts about the geographical and linguistic groups…unnecessarily complicated" (Penrose 1953, 252). Even if one does not think of 'race' as a mystical concept, it still seems an unnecessary concept in biology if what is meant is 'population'.

The use of 'population' as a synonym for 'race' may go some way to explain why racial naturalism continues to be endorsed by many specialists (Lieberman et al. 2004; Wagner et al. 2016). What may look like empirical refutations (e.g., Lewontin 1972; Templeton 2013) leave GIGP naturalism unharmed. GIGP naturalists reject the claim that 'race' necessarily

still implies (1) a small number of groupings, (2) great difference between groupings, and (3) group homogeneity over internal heterogeneity. However, once these concessions are made, it is unclear why we should call GIGPs 'races'. It seems sensible to eliminate race from our biological ontology if what is really meant by 'race' is 'GIGP' and—as GIGP naturalists accept—continent-sized populations have no special status. Calling genetically-identifiable-geographic-populations 'races' only confuses matters.

All three of the criteria for eliminating putative kinds are met in the case of race-as-GIGP. The proposed new meaning for the term differs radically from its original meaning, the putative kind is trivialised, and a new term has been introduced which can replace the old term. This provides a strong case for eliminating race-as-GIGP from our ontology.

Conclusion

The biological race debate has never been settled. Many race naturalists claim that their view has been vindicated by findings in population genetics, and these claims have been critiqued by scientists and philosophers alike. In response, some race naturalists have implicitly or explicitly appealed to Dobzhansky's view of race-as-GIGP. This can also be understood as an implicit appeal to the causal-historical theory of reference. On a causal-historical approach to reference, we may be forced to accept that when we talk about races, we are really talking about genetically-identifiable-geographic-populations.

There is something wrong with this move, and the critique of the flight to reference helps to pin down exactly what is wrong with it. If there is no single true theory of reference that will tell us whether terms refer, then we should be sceptical of any metaphysical argument that is premised on the truth of any particular theory of reference.

Yet we should be careful not to overextend the critique of the flight to reference. While avoiding the flight to reference, we can abstract some lessons from the theory of reference literature and apply them to live metaphysical debates. Drawing on the theory of reference literature I have proposed that putative kinds are candidates for elimination when (1) the proposed new meaning of the kind term differs radically from its original meaning, (2) the putative kind is trivialised, and (3) a successful new theoretical system, with alternative terminology, is introduced. This is not an exhaustive list and no single criterion is sufficient grounds for eliminating putative kinds. However, if all three are met, there is a strong case for eliminativism.

By applying my criteria for eliminating putative kinds, the problems with race-as-GIGP become clear. All of the three criteria are met, so the case for eliminating race-as-genetically-identifiable-geographic-population is strong. GIGP naturalism departs radically from the original scientific definition race, trivialises the concept, and seems unnecessary, since the population concept offers an alternative to the concept of race. Indeed, concepts such as 'population', 'ancestry', and 'racialised group' can—and in my view should—all serve as alternatives to 'race' (Darder and Torres 2003; Blum 2010; Hochman 2017, 2018).

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