



What are we to make of the concept of race? Thoughts of a philosopher–scientist

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ARTICLE INFO

Article history:

Available online 18 May 2013

Keywords:

Race
Genotype to phenotype mapping
Intelligence
Ecotype

ABSTRACT

Discussions about the biological bases (or lack thereof) of the concept of race in the human species seem to be never ending. One of the latest rounds is represented by a paper by Neven Sesardic, which attempts to build a strong scientific case for the existence of human races, based on genetic, morphometric and behavioral characteristics, as well as on a thorough critique of opposing positions. In this paper I show that Sesardic's critique falls far short of the goal, and that his positive case is exceedingly thin. I do this through a combination of analysis of the actual scientific findings invoked by Sesardic and of some philosophical unpacking of his conceptual analysis, drawing on a dual professional background as an evolutionary biologist and a philosopher of science.

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When citing this paper, please use the full journal title *Studies in History and Philosophy of Biological and Biomedical Sciences*

1. Introduction: the never ending debate

Scientific and philosophical discussions about the existence and meaning of human races have been going on ever since there have been science and philosophy (James, 2008), and they will likely—one might add, unfortunately—never end. Nonetheless, it is instructive to look at new arguments and new empirical evidence whenever these are presented, if nothing else in order to clear once again the air from misconceptions and ill-founded notions.

I also think that debates about race are an excellent example of the fruitful interaction between philosophy of science and scientific practice (Pigliucci, 2008), though of course contributions from additional allied fields, such as the sociology and the history of science, are just as pertinent. Clearly, talk of race has to be grounded in the relevant empirical evidence, which certainly makes it at least partially the domain of science. However, the concept of race itself has changed over time, it is being deployed differently by biologists working in different areas (e.g., human vs other animals vs plants), and it can be unpacked in different ways—all of which makes it germane to history, sociology and philosophy of science. Moreover, there is the issue of the epistemic warrant of claims made about races on the basis of the available scientific evidence, as well as questions about how (and even why) said evidence is

being gathered, again issues with which philosophers are arguably more familiar than the scientists themselves.

The topic of race is obviously huge, so in order to ground this discussion I will provide an in-depth analysis of a recent paper by Sesardic (2010) which presents us exactly with all the issues mentioned above. Sesardic is a philosopher, and takes the somewhat controversial position that the biological concept of human race has been “socially destructed” by misguided scientists and philosophers, who have yielded to political correctness. Sesardic appropriately builds his case through a combination of philosophical criticism of recent positions and an appeal to the most up to date empirical evidence coming out of both human genetics and anthropometrics.

In the following I will therefore take on Sesardic's major points and discuss both their philosophical and scientific validity, drawing on my experiences as both an evolutionary biologist and a philosopher of science. I will attempt to show that while Sesardic does make some interesting points, his philosophical analysis, and—more to the point—his interpretation of the science, is fatally flawed. I will then provide a brief discussion of the usefulness of these debates, building on an unusual exchange published in *Nature* by Steven Rose on the one hand and Ceci and Williams (2009) on the other, essentially asking the hard (and I'm sure academically

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unpopular) question of why exactly we should even continue to have this debate.

2. Criticizing the critics

Sesardic begins his assault on the alleged social destruction of race by claiming that a number of people have essentially defined the concept away, beginning with Naomi Zack's (cited in Sesardic, 2010, p. 145) assertion that "(1) races are made up of individuals sharing the same essence; (2) each race is sharply discontinuous from all others..." He then goes on to attribute similar essentialist straw man arguments to a number of authors, including Sally Haslanger, Philip Kitcher and Anthony Appiah. He also throws a few biologists, such as Luca Luigi Cavalli-Sforza, into the mix for good measure.

Sesardic is surely correct that if the criteria for recognizing races (and many other biological entities, for that matter) were based on a strict understanding of essentialism (i.e., where essences are defined by a small number of necessary and jointly sufficient conditions) then no way to make sense of it could be found within the framework of modern biology, and all discussion would halt at the starting gate. (Although it needs to be noted that, ironically, there has been a resurgence of essentialism *broadly construed* in the philosophy of biology recently, particularly with respect to species concepts: LaPorte, 2004; Ereshefsky, 2010.) Indeed, he is right on both his baseline criticisms of the critics of race concepts: (a) races (may) make biological sense even though there is no "essence" (*sensu stricto*) defining them; and (b) in order to agree that races exist we do not need to find sharp boundaries distinguishing one race from another, just as sometimes there are no sharp boundaries between species, pretty much regardless of which particular species concept one adopts.

Of course, none of the above amounts to anything like a positive defense of the meaningfulness of the concept of race, which is why we need to turn to Sesardic's three-part treatment of human races, in terms respectively of their genetics, morphology, and behavior. It is here that Sesardic draws most heavily—and, I will argue, most incorrectly—from the newest scientific findings.

3. The genetic case for races

Here Sesardic (2010, p. 148) makes a crucial mistake right off the bat: he strongly hints at an equation between human races and biological sub-species. But there is a large—if certainly *not* unanimous—literature in biology distinguishing the two, as well as making the case that while human races exist (in some biologically meaningful sense) they most certainly are *not* subspecies (see Pigliucci & Kaplan, 2003; Templeton, 2003). The term "race" when applied to animal systems (the terminology is different, though analogous, for plants, where researchers tend to use "ecotype") most often refers to groups of geographically and/or ecologically differentiated populations. "Subspecies," on the other hand, marks deeper divisions that have cladistic (i.e., phylogenetic) valence, and subspecies are often interpreted as incipient species. It is unquestionable, even on the basis of the biological literature cited by Sesardic, that human subspecies—so construed—simply do not exist (Templeton, 2003). This, to be sure, is an accident of human phylogeography, not a biological necessity, but it is the case nonetheless. The existence of geographically differentiated human populations is equally unquestionably a fact, though its significance is what is usually under (heated) discussion.

As we shall see immediately, once the distinction between subspecies and races is understood and acknowledged, and once we accordingly recast the debate in terms of human races—*not* of human subspecies—most of the points made by Sesardic either

remain true but lead to drastically different conclusions than he imagines, or simply no longer hold.

Which brings us to the so-called "Lewontin fallacy." Population geneticist Richard Lewontin (1972) famously observed that the overwhelming majority of the genetic variation characterizing the human species is found within, not among populations. The exact numbers vary according to when the estimate was made and which genetic markers were used, though the figures have stayed remarkably similar since Lewontin's first assessment (which put inter-population variation at about 7%, and correspondingly intra-population variation at about 93%).

Sesardic claims that this statistic has been much misused by critics of the race concept: "the mere fact that the between-group genetic variation is many times smaller than the within-group variation does not actually preclude racial categorization from making a lot of genetic sense. To think otherwise is to commit a statistical mistake that has recently been labeled 'Lewontin's fallacy'" (Sesardic, 2010, p. 149). Why would this be a fallacy? Because, as Sesardic adds shortly thereafter: "a clear group structure can still emerge on the basis of these aggregate properties of populations."

This is most certainly true, and it is essentially the *only* empirically based argument Sesardic can marshal in support of his thesis, at both the genetic and morphological levels of analysis (at the behavioral one, as we shall see below, things become somewhat bizarre). But Sesardic is equivocating on a variety of terms here. For instance, as I will discuss below, "group structure" can be recovered from almost any biological sample, as long as there are consistent inter-group differences, no matter how small. But why should we be concerned with group structure, unless it indicated a deep difference, such as a phylogenetic one? And the latter is, again, clearly not the case for human populations. As for "racial categorization," again, it depends on what one means by that term. If it is to be used simply as synonymous with population differences, then I'm pretty sure Lewontin himself wouldn't deny that there are differences among human populations. So what?

Let us examine the evidence that Sesardic cites in favor of his conclusions about racial "group structure" in humans. Two of the major (somewhat) recent works discussed by Sesardic are by Rosenberg et al. (2002)—ironically, the senior author of which, Marcus Feldman, is a longtime collaborator of Lewontin—and by Tang et al. (2005); accordingly, I will focus my analysis on those. The Rosenberg et al. paper is a study of 52 human populations, whose genetic diversity was characterized using 377 autosomal (i.e., not sex-linked) microsatellite loci scored in 1056 individuals. It is by all means a large sample of genetic variation, and its empirical conclusions are robust (Rosenberg et al., 2005). The significance of the Rosenberg et al. study for Sesardic is that it "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" (Sesardic, 2010, p. 153). Yes, but this is an interestingly (and possibly revealing) exercise in selective quotation on Sesardic's part.

First off, Rosenberg et al. actually found a *variable number* of major clusters (6, 5, 4 and even 3), depending at what level one stops the analysis. Why pick a particular one as the major finding of the paper, other than because five clusters happen to fit the author's predilection for the true number of human races? At the very least this is blatant cherry picking of the relevant evidence. Second, and far more damning, Sesardic entirely ignores that Rosenberg and colleagues go on to say (even in the abstract of their paper!) that "we identified... subclusters that often correspond to individual populations." Are each and all of these subclusters also races, in Sesardic's opinion? One assumes not, but Sesardic has not given us any compelling reason to think that $K = 5$ is *the* racial level because his own basic meaning of 'race' (a genetically identifiable cluster of individuals) is compatible with multiple levels of

human population substructure. Under a referentialist semantic framework this suggests that ‘race’ as intended by Sesardic has no referent, not that ‘race’ refers to $K = 5$ but not to $K = 6, 4, 3$ etc.

What of Tang et al.’s paper? Sesardic summarizes its import thus: “A group of researchers led by geneticist Neil Risch et al., (2002) analyzed genetic data for 326 microsatellite markers on a sample of 3636 subjects from the United States and Taiwan. The subjects identified themselves as belonging to one of the four racial groups (white, African American, East Asian and Hispanic). The genetic cluster analysis of the data produced four major clusters, whose correspondence with the four self reported races was near-perfect: the genetic cluster membership and self-identified race coincided in as many as 99.9% of the cases.” Besides the fact that there are specific methodological issues with the Tang et al.’s survey (if one looks closely at their sample, one realizes that all of their Hispanics are Mexican Americans from a single county in Texas, which makes the otherwise surprising “Hispanic” cluster a reflection of mere geographical proximity), again, one can identify legitimate genetic clusters of human populations at a variety of hierarchical levels, but Sesardic offers no principled reason for identifying one such clustering as more fundamentally indicative of races.

So, yes, there is structured genetic variation in human populations. But this is hardly a surprising or controversial notion among human population geneticists, and it does not at all imply any strong correspondence between the available genetic data and folk concepts of races.

4. The morphometric and behavioral cases for races

Sesardic then turns to the morphometric and behavioral evidence concerning the existence of human races, once again beginning by citing a number of critics who deny the objectivity or relevance of race when it comes to human morphometry, and then asking rhetorically (by quoting Sauer): “This [skepticism] prompted one bewildered and exasperated scientist to write an article with a provocative title: ‘If Races Do Not Exist, Why Are Forensic Anthropologists So Good at Identifying Them?’”

That is indeed a good question, though the answer is actually well known in statistics, and need not invoke the existence of well characterized and biologically meaningful human races. It is a truism of multivariate statistical analysis that membership in individual clusters of pretty much any heterogeneous collection of objects can be ascertained with a fairly high degree of accuracy if one has a sufficient number of discriminatory variables to play with. Indeed, Sesardic approvingly quotes a study by Ousley, Jantz, and Freid (2009) which concludes that seven variables are sufficient to achieve a classificatory accuracy (i.e., attributing the right geographic origin to a given individual) of 95%, and that 19 variables raised that probability to 97%. Very impressive, until one looks at Table 4 of the Ousley et al. (2009, p. 74) paper. It turns out that the groups that their analysis identifies include not just (and not surprisingly) “races” identified by large geographical provenances, but also more local geographical provenances (just as in the continuous hierarchy that emerged from the Rosenberg et al. paper discussed above), and even tribes (of Native Americans) and time differences (as in the differences between white males born from 1840 to 1890 vs white males born between 1930 and 1980).

Now, again, Sesardic owes us (and, needless to say, does not provide) a principled account of why we should not take the Ousley et al.’s results to indicate not only that Arikar and Sioux Native Americans actually belong to distinct races, but that late 19th century white males apparently also belonged to a different race from white males from the middle part of the 20th century. Yes, I know, the notion is laughable, but it follows from a consistent application of Sesardic’s logic.

Things become downright bizarre when we come to the last of the three major types of differences among human populations that allegedly help us make sense of the concept of race: psychological differences. Of course, anyone who has seriously looked into this endless debate knows very well that here is where the stakes really lie: it is not about small genetic differences that may or may not help build a more individualized medicine; it is not about forensic anthropologists and how well they do their work; it is about claims that one race has superior or inferior intellectual capabilities than other ones, and it is because of this possibility that debates about race in biology so easily become over-heated (see, for instance: Herrnstein & Murray, 1996; Kincheloe, Steinberg, & Gresson, 1997).

Sesardic must have been running out of steam at this point, since he begins the section on psychological differences with this strange argument: “Is there some reason to believe that some of the psychological differences between the races are at least partly due to genetic differences between them? The affirmative answer cannot be rejected out of hand” (Sesardic, 2010, p. 158). Well, that is correct, though it must be noted that the same observation also goes for unicorns and a host of other unlikely claims. But the nadir is reached shortly thereafter, with the following piece of evidence brought in to show a conspiracy among philosophers to “socially destruct” the very idea of race:

“For instance, on the website AskPhilosophers.org, where a group of highly respected philosophers answers questions about various philosophy-related issues, someone recently asked: ‘If people of different races can have clear physical differences, could this not also mean there could be differences in ability to learn, or mental differences altogether?’

A very brief answer that left no room for doubt came from Richard Heck, a leading philosopher of language and logic: ‘Of course there could be all kinds of differences between races, including differences in native intelligence, ability to learn, and so forth. The only significant question is whether there are such differences, and there has never been any decent reason to believe that there are.’” (Sesardic, 2010, p. 159)

Not only Sesardic is quoting a philosopher of *language* (as opposed to, say, a philosopher of science) as representative of the philosophical community’s opinion on race, but he is doing so using as his source a web site intended for popular audiences, not scholarly discussions. If this is the best that he can do, I submit that the burden of proof on Sesardic’s side has not even begun to be discharged, and that not much else need be said about his attempt to “show” that philosophers have engaged in a radical social attack on race.

5. The real problems with the race concept

Sesardic’s discussion of race is not atypical of this seemingly endless diatribe among philosophers, scientists and social scientists. The arguments remain remarkably constant, almost despite the parallel accumulation of empirical evidence, evidence that points to two fundamental flaws underlying most discussions of race: the (often implicit) assumption of a relatively simple genotype to phenotype map; and the misunderstanding of the actual biological nature of races. Let me briefly comment on both.

Biologists have been tackling the so-called genotype to phenotype mapping function for a while now (Alberch, 1991; Pigliucci, 2010). This refers to the causal pathways by which phenotypes are related to genotypes. The consensus among biologists who have seriously looked into the matter is that said map is exceedingly complex and non-linear, and that it is highly affected by the environment through a by now well studied phenomenon known as phenotypic plasticity (Pigliucci, 2001). Lewontin has

shown long ago that phenotypic plasticity has some interesting effects that undermine much discussion about the import of the relationship between genetic and behavioral/psychological differences among humans (Lewontin, 1974). Specifically: phenotypic plasticity makes it impossible (in principle, not just in practice) to disentangle genetic from environmental effects in determining the phenotype (including behavioral and cognitive traits). We can quantify the statistical (i.e., population-level) contributions of *variation* in genes and *variation* in environments, but that tells us precisely nothing about how the two come together to build phenotypes. To use Lewontin's analogy, one can measure the weight of bricks vs lime used to build a house, but houses are built by a *specific patterning* of bricks and lime, not by just mixing together the two piles.

While a detailed discussion of genotype to phenotype mapping and phenotypic plasticity is beyond the scope of this paper, it is about time for philosophers like Sesardic—who are interested in a phenotypically-based (as opposed, to say, a cladistic) concept of human races to seriously delve into these aspects of the biological literature. This will help raise the sophistication of the discourse and will avoid naïve statements about the significance of measurable differences in genetic or phenotypic characteristics among human populations. Most importantly, taking phenotypic plasticity on board means recognizing that while it is obviously possible to measure differences in genetic markers in humans, we do not have any reasonable means to causally disassociate environmental effects from behavioral differences, even when those behavioral differences are known to be affected by one's genetic background.

Take, for instance, the famous case of a clearly genetically “determined” human trait: phenylketonuria (Kaplan, 2000). This is a well known condition that causes severe mental retardation due to the accumulation of the amino acid phenylalanine, since the subjects affected by the disease lack a crucial enzyme necessary to metabolize the amino acid. However, the symptoms can be prevented by making sure that affected individuals do not incorporate significant amounts of phenylalanine in their diet, especially during development (this is why soda cans display a prominent warning to phenylketonurics). The lesson here is that although the disease (and the corresponding morphological and behavioral phenotypes) is certainly genetic, a relatively simple change in the environment largely neutralizes its deleterious effects. We do not know how much this example generalizes, but it is worth noting that most traits of interest in discussions about races have a much more complex genetic and environmental causation than phenylketonuria. One would think this to be sufficient reason to tread extremely carefully. Whereof one cannot speak, thereof one must be silent indeed.

The second problem with some discussions of race is a lack of appreciation for what Pigliucci and Kaplan (2003) have proposed to be the best biologically based interpretation of human races: as geographical ecotypes. The term “ecotype” was originally proposed by botanist Turesson (1922), ironically precisely within the context of disentangling genetic from environmental effects on an organism's morphology. In modern parlance, the idea is that natural selection favors particular phenotypes under specific sets of environmental conditions (e.g., short and branchy plants at high elevation, because they are more drought and wind resistant; or dark skin, eyes and hair in humans living closer to the equator because of better protection against UV damage). However, biologists also recognize that the same (or relevantly similar) phenotype can be achieved by a variety of underlying genetic makeups (because of the complexity and non-linearity of the genotype to phenotype mapping function discussed above). This means that the “same” ecotype (meaning the same morphology) can evolve independently several times in response to similar environmental conditions, but will likely do so via whatever combination of genes

happens to be available in the local populations at that particular time.

Pigliucci and Kaplan (2003) have therefore proposed that human races—to the extent that they exist—could be thought of from a biological perspective as ecotypes. There are several implications to this proposal, the most fundamental being the following two: (a) there is little relation between human races *qua* ecotypes and the folk concept of race, because the same folk “race” may have evolved independently several times in response to local environmental conditions, and be characterized by different genetic makeups; (b) ecotypes (and hence races) are only superficially different from each other because they are usually selected for only a relatively small number of traits that are advantageous in certain environments. This means that races are nothing like phylogenetically divergent subspecies, and that racial differences are literally skin deep. Somewhat ironically, a number of philosophers (e.g., Glasgow, 2009) have criticized our approach on the ground that applying the biological technical sense of ecotypes to human races ends up showing that races (in the folk sense) do not really exist, at least not in the relevant sense of the term used in the race debate. That is indeed a correct interpretation of Pigliucci and Kaplan (2003), and the validity of such contribution lies precisely in the fact that it shows that Sesardic-like accounts of race are ill-informed *scientifically*, so that we can all move on and concentrate on the more relevant and complex issue of the social construction of the concept of race, which has well known and quantifiable consequences in its own right.

So what we are left with is that human races do exist (as ecotypes), but in nothing like the sense that is used in Sesardic-type discussions about race; and that the complexity and non-linearity of the genotype to phenotype mapping function, together with the phenomenon of phenotypic plasticity, make much debate about the genetic basis of behavioral and cognitive traits in humans moot. Why, then, are we still talking about this?

6. When is enough enough?

As I mentioned at the beginning, discussions about race in science and philosophy just do not seem to go away, no matter how many cogent arguments and how much high quality empirical evidence is brought to bear on the issue. One may reasonably begin to wonder whether it is productive to keep engaging in these debates, or whether we should encourage scholars to get busy doing something else instead. Interestingly, precisely that question has not so long ago been posed by the editors of the prestigious *Nature* magazine and framed as “Should scientists study race and IQ?” (Rose, Ceci, & Williams, 2009). I take that the word “should” here is interpreted as a suggestion that there may be better things to do, not as a prescriptive injunction that would surely violate basic principles of academic freedom. Bearing that assumption in mind, it is instructive to briefly review the arguments that the two sides listed in favor of their respective answers.

The “Yes: The scientific truth must be pursued” side was represented by Stephen Ceci and Wendy M. Williams, who are in the Department of Human Development at Cornell University. They brought up the following issues: (a) Academic freedom; (b) The idea that controversies over IQ advance our understanding of intelligence; and (c) The suggestion that IQ scores can be used to further socially worthy goals. Point (a) is actually not in play at all, since as I mentioned I do not think anyone—and certainly not the editors of *Nature*—is suggesting that academics should be prohibited from doing research on race and IQ. That said, of course, the entire academic research system is based on multiple levels of peer review (both of articles before publication, and of grant proposals before funding), one main object of which is precisely to determine whether a given research program is worth the effort and

resources (be they printed pages in a journal or dollars from a granting agency). So the question of whether research on race and IQ should be funded and/or published is legitimate, and the burden is on interested researchers to make a case for it, just as it is in all other fields of scholarship.

Which brings us of course to points (b) and (c). Ironically, controversies over IQ and its across-race variation have indeed contributed to our understanding of human intelligence (point (b)), but have led to conclude that we should be weary of the whole idea of strong biological determinism of human cognition. Philosophers have shed light on the conceptual issues involved in defining “intelligence” (e.g., Block & Dworkin, 1978) and perhaps more importantly, an entire research program in social psychology has come out of the intelligence test debates, namely, stereotype threat research. Steele and Aronson (1995), among others, looked at IQ tests and at ATS tests (e.g. SATs, GREs, etc.) to see whether human intellectual performance can be manipulated with simple psychological tricks priming negative stereotypes about a group that the subjects self-identify with. Notoriously, the trick worked, and as a result we can explain almost all of the gap between whites and blacks on intelligence tests as an artifact of stereotype threat, a previously unknown testing situation bias. It turns out that our intellectual performances are highly sensitive to which stereotypes about our performances are salient in society and to a performer at the time of performance. Studying race and IQ in careful, responsible ways can indeed lead to deeper insight about intelligence.

What about Ceci and William’s third point, that IQ scores can be used for social improvement? It is somewhat ironic that IQ tests were indeed originally designed by Alfred Binet precisely in order to accomplish positive social goals, such as to identify young children who were falling behind at school so that they could be given personalized help and recover (Binet, 1905). However, given the well known ensuing history, it is hard to imagine a serious defense of the idea that that intended use has in fact been the major one for IQ tests, and it is even more problematic to defend the entirely separate contention that IQ scores analyzed by *groups* rather than individuals are justified on the likelihood that they will bring positive social outcomes. (Not to mention, of course, that mixing up group- and individual-level variation would be an example of an elementary statistical fallacy.)

What of the case against engaging in further research on IQ and races? In the *Nature* article this position was defended by Steven Rose, a neuroscientist and Emeritus Professor at the Open University in the UK. He made essentially five points: (i) We always make decisions about which research to fund based on both intrinsic and social values; (ii) The IQ-race project is incoherent because it is difficult to define intelligence and to relate it quantitatively to IQ; (iii) Races as normally defined are biologically meaningless; (iv) It is exceedingly difficult to do nature-nurture studies in humans; and (v) Too often IQ-race research boils down to ideology masquerading as science.

I have already commented on the first point, as it is a straightforward undeniable fact of academic research. The problem, if anything, is that research priorities are seldom *explicitly* discussed outside of the confines of a narrow specialty, but of course they should (particularly considering that a great part of funding for scientific research comes from the public purse). Rose’s first point could be interpreted positively as a call for scholars to engage in public discourse over what they want to research and why. It would be the right thing to do in an open society. I have also already agreed with the second point on Rose’s list. I wouldn’t use the word “incoherent,” with its strong connotations in formal logic, but the definition of intelligence—and its relation to IQ measures—is certainly fraught with a number of possibly irreducible difficulties that, again, at the very least ought to be faced squarely instead of sidestepped. Let me skip to the fifth point next: the charge of

ideology masquerading as science may seem like an *ad hominem* attack, but it seems *prima facie* to be on target to me at least when applied to some prominent authors who have participated in the debate (a similar objection, and on similar grounds, has been moved to neurobiological research on gender differences: Fine, 2011). This is not to say, of course, that all—or even the majority—of scholars can reasonably be so charged, particularly because ideological motivations often lurk below the surface of one’s conscious motives. Ultimately, this is an area where history and sociology of science, as well as science criticism, can legitimately weigh in.

Finally, Rose’s third and fourth points speak to the core of the present essay. I have argued that, indeed, it is exceedingly difficult to carry out informative nature-nurture studies in humans, in part because of the complexity of the genotype to phenotype map (especially when the phenotype is behavioral/cognitive), and in part for obvious logistical and ethical reasons (unlike with, say, plants and rats, we cannot artificially create genetically homogeneous lines of humans and “grow” them under sufficiently controlled conditions). As for the biological interpretation of the concept of race, I have reiterated Pigliucci and Kaplan’s (2003) suggestion that it is not meaningless, but it does have a sufficiently different meaning from that of folk races to create serious problems for most of the published scientific and philosophical literature on biological differences among “races.”

In the end, as a practicing scientist and philosopher I am constantly baffled by the perniciousness of some academic stances about race, by the sort of lackadaisical use of statistical concepts and scientific empirical evidence displayed by Sesardic and others, as well as by the biological naiveté of some of their arguments. This is despite the partial counterbalance offered by serious scholarship on race, which has certainly been produced by both philosophers and scientists alike. It may be too much to suggest that some positions about human races are approaching the status of pseudoscience (and pseudophilosophy), but it is hard to avoid the distinct feeling that if there is a demarcation line between legitimate intellectual discourse and pseudoscientific nonsense (Pigliucci & Boudry, *in press*), discussions on race too often seem to square people who properly belong on opposite sides of that threshold.

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