

Race: a social destruction of a biological concept

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Abstract It is nowadays a dominant opinion in a number of disciplines (anthropology, genetics, psychology, philosophy of science) that the taxonomy of human races does not make much biological sense. My aim is to challenge the arguments that are usually thought to invalidate the biological concept of race. I will try to show that the way “race” was defined by biologists several decades ago (by Dobzhansky and others) is in no way discredited by conceptual criticisms that are now fashionable and widely regarded as cogent. These criticisms often arbitrarily burden the biological category of race with some implausible connotations, which then opens the path for a quick eliminative move. However, when properly understood, the biological notion of race proves remarkably resistant to these deconstructive attempts. Moreover, by analyzing statements of some leading contemporary scholars who support social constructivism about race, I hope to demonstrate that their eliminativist views are actually in conflict with what the best contemporary science tells us about human genetic variation.

Keywords Race · Social constructivism · Genetics · Geographical ancestry · Human variation · Philosophy of science

Those who subscribe to the opinion that there are no human races are obviously ignorant of modern biology.

Ernst Mayr, 2002

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Introduction

A number of contemporary philosophers, anthropologists, geneticists, evolutionary biologists and psychologists have argued for some time that the concept of race does *not* have a biological reality. But what is actually being denied here? What exactly does it mean that a concept has (or does not have) a biological reality?

To paraphrase the title of Quine's famous article "Three Grades of Modal Involvement", the concept of race has three possible grades of biological involvement. First, the basic meaning of "race" seems to imply that, due to a common ancestry, members of a given race A will display increased genetic similarity, which will make them in some way *genetically* different from individuals belonging to another race, B. Second, it is frequently assumed that A-individuals will also differ systematically from B-individuals with respect to some genetically determined *morphological* characteristics (skin color, hair texture, facial features, etc.), with these morphological differences being the basis for the common-sense racial recognition and classification. And third, A-individuals could differ from B-individuals with respect to some genetically determined *psychological* characteristics as well. So in discussing the concept of race, its three grades of biological involvement are three kinds of racial differentiation that are rooted in biology: genetic, morphological and psychological.

Each of these three ways of connecting race with biology has been widely disputed. Various general arguments against the biological underpinnings of race have convinced a lot of people, although they should not have. Despite the flawed logic of most of these deconstructive attacks, the biological concept was in the end hastily and clumsily dismantled. Learned societies issued statements disowning the concept and many leading experts in the relevant fields joined in by insisting that this pure "social construction" has no counterpart in the subject matter of life sciences, that "the concept of race has no genetic or scientific basis" (Craig Venter), or that it is "biologically meaningless" (Schwartz 2001, 1392). In a very influential article it has even been suggested that the reality of human races is another common sense "truth" destined to follow the flat Earth into oblivion (Diamond 1994, 82). Similarly, in the publicly acclaimed PBS series "Race: The Power of an Illusion" (2003), anthropologist Alan Goodman said "To understand why the idea of race is a biological myth requires a major paradigm shift, an absolute paradigm shift, a shift in perspective. And for me, it's like seeing, you know, what it must have been like to understand that the world isn't flat".

In this paper I will try to bring the notion of race back into the open, dust it off a little and reevaluate its scientific status. I will separately discuss the three grades of biological involvement mentioned above: genetic, morphological and psychological differentiation.

But first a few words about a frequent attempt to undermine the concept of race by arbitrarily adding to its definition some blatantly unacceptable assumptions.

Defining the race away

Naomi Zack claims that those who believe in the existence of human races "to this day... assume the following: (1) races are made up of individuals sharing the *same*

essence; (2) each race is *sharply discontinuous* from all others...” (Zack 2002, 63—italics added).

Zack is wrong. First, as many philosophers have pointed out, the everyday concept of race “has been greatly influenced by science” (Andreasen 2005, 100, cf. Appiah 1999, 269). Therefore there is no reason to think that the ordinary notion of race would “to this day” be so rigidly determined by such a completely outdated and long discredited essentialist taxonomy. Second, it is actually dubious whether the view of races as “sharply discontinuous” groups was ever the received view in science. After all, despite the usual criticisms of the pre-Darwinian “typological” thinking, even the very first systematic scientific work on human races was not essentialist in Zack’s sense, as shown in the following quotation from the book first published in 1776:

For although there seems to be so great a difference between widely separate nations, that you might easily take the inhabitants of the Cape of Good Hope, the Greenlanders, and the Circassians for so many different species of man, yet when the matter is thoroughly considered, you see that all do so run into one another, and that *one variety of mankind does so sensibly pass into the other, that you cannot mark out the limits between them.* (Blumenbach 2005, 98–99—italics added)

Sally Haslanger states that “our everyday racial classifications do not track meaningful biological categories”. She explains: “there are no ‘racial genes’ responsible for the different clusters of physical or cultural differences between members of racial groups...” (Haslanger 2005, 266) Indeed, if our everyday racial classifications required the existence of some special “racial genes”, any connection with currently accepted biological categories would be immediately lost. But since Haslanger gives no support at all for her claim that the common sense notion of race is inextricably linked with such a demonstrably false assumption, it is fair to conclude that she has actually done nothing to show that “our everyday racial classifications do not track meaningful biological categories”.

Philip Kitcher also sets up a similar straw man concept, which he then easily knocks down:

Contemporary genetic studies of human populations have revealed that there are no alleles distinctive of this race or of that, and, although a few researchers like J. Philippe Rushton—“ogre naturalists,” as Ian Hacking aptly dubs them—continue to seek such simple genetic differences, there is a widespread consensus among anthropologists that races are not “biologically real”. (Kitcher 2007, 293–4)

Among contemporary scientists, Rushton is one of the main defenders of the biological reality of the everyday concept of race. Anyone who is even superficially acquainted with his work will recognize how unfair and inappropriate it is to refer to his research program as seeking “alleles distinctive of this race or of that”. In fact, if Kitcher’s parody were true, it would be inexplicable how it could happen that Rushton’s articles containing such scientific nonsense were favorably judged by academic referees and accepted for publication in leading scholarly journals. The

truth, of course, is that Rushton's real ideas bear no resemblance to how Kitcher pictures them. Instead of being discussed and perhaps criticized, Rushton is here only quickly dismissed without his views being as much as presented.

Antony Appiah also explodes the biological concept of race by capriciously inflating it with clearly unacceptable essentialist connotations. In discussing a potential grounding of "race" in biology, the only position that he considers is the view "that there are heritable characteristics, possessed by members of our species, that allow us to divide human beings into a small set of races, in such a way that *all* the members of these races share certain traits and tendencies with each other that they do *not* share with members of *any other* race. These traits and tendencies characteristic of a race constitute, on the racialist view, a sort of *racial essence*" (Appiah 1990, 4–5—italics added; cf. Appiah 2006, 363). Appiah rightly says that such a view is scientifically untenable but he wrongly concludes from this that the concept of race is thereby shown to have no connection with biology. Apparently he never envisages the obvious possibility that his essentialist non-starter is not the only option, and that the notion of race might still be biological, *if interpreted in a more charitable and more sophisticated way*. It is puzzling why Appiah is ignoring the dog that is so loudly barking here and seeking attention: a biologically informed but *non-essentialist* concept of race.

It is true that the everyday concept of race does presuppose some degree of phenotypic similarity between the groups called "races". But surely this phenotypic similarity does not have to be manifested necessarily in such a way that, in case of two distinct racial groups A and B, *all* members of group A would have to share certain characteristic F, and that characteristic F would also have to be absent in *all* members of group B. More complex and much more plausible forms of racial phenotypic similarity are possible (as will be illustrated in "[Morphological differences](#)").

Ron Mallon follows Appiah, and in discussing race focuses only on this crude form of similarity. He says that historically races were believed to be characterized by properties that "are shared by *all and only* the members of a race" (Mallon 2006, 528—italics added). Again, after justifiably rejecting this view, Mallon never goes back to look at some more viable versions of the biological race concept that would be based on similarity. As a result, his overview of different standpoints on race turns out to be seriously incomplete: the only biology-based concept of race that he discusses is the cladistic "racial population naturalism", which does *not* include the claim that races are genetically or morphologically distinguishable from one another. So any similarity-laden view of race simply falls through the cracks. The similarity-driven concept of race is addressed only in its essentialist version ("shared by *all and only* the members of a race") and rightfully dismissed as anachronistic and untenable. But its subtler and much more interesting version (which exists in the literature) never receives any consideration.

Nevertheless, Mallon's sin of omission did not preclude the *Stanford Encyclopedia of Philosophy* from endorsing his views in its recent article on race and concluding, relying mainly on his arguments, that "the biological conception of race is philosophically and scientifically dead" (James 2008).

It appears that those who attempt to deconstruct the concept of race by gratuitously burdening it with essentialist connotations (“discrete”, “non-overlapping”, “discontinuous”, “defined by racial markers”, “racial genes”, etc.) are unaware that their criticism has already been addressed by Dobzhansky more than 40 years ago:

Professor Fried has correctly pointed out that there is no careful and objective definition of race that would permit delimitation of races as exact, nonoverlapping, discrete entities. Indeed, such criteria do not exist because if they did, we would not have races, we would have distinct species. (Dobzhansky in Mead 1968, 165)

In fact, Dobzhansky’s argument should be taken one step further: the essentialist requirement is so unrealistically demanding that, if this criterion were applied, even the species concept would fail to pass muster: “In practice, the characters that define a species will not be present in all members of that species and absent from all members of other species. Nature is too variable” (Ridley 2004, 349).

Oddly, even the scholars who have been at the very forefront of empirical research on race are prone to use fallacious reasoning in order to downplay the importance of that concept. For instance, Cavalli-Sforza, geneticist and the lead author of the path-breaking *History and Geography of Human Genes* (Cavalli-Sforza et al. 1994), states in a book co-authored with Walter Bodmer: “Races are, in fact, generally very far from pure and, *as a result*, any classification of races is arbitrary, imperfect, and difficult” (Bodmer and Cavalli-Sforza 1976—italics added). Is any classification of races imperfect? Yes. Difficult? Perhaps. But arbitrary? No, this certainly does not follow from the premise, “as a result”.

Speaking about Cavalli-Sforza, it is interesting that he tried to defuse potential political attacks on his research by a simple and sometimes surprisingly effective rhetorical ploy. At one point he just stopped using the term “race” and replaced it with a much less loaded expression “human population”, which in many contexts he actually used more or less with the same meaning as “race”. On one occasion this terminological switch gave rise to an amusingly ironic development, as described in the following episode involving Cavalli-Sforza’s collaborator, Edwards:

When in the 1960s I started working on the problem of reconstructing the course of human evolution from data on the frequencies of blood-group genes my colleague Luca Cavalli-Sforza and I sometimes unconsciously used the word ‘race’ interchangeably with ‘population’ in our publications. In one popular account, I wrote naturally of ‘the present races of man’. Quite recently I quoted the passage in an Italian publication, so it needed translating. Sensitive to the modern misgivings over the use of the word ‘race’, Cavalli-Sforza suggested I change it to ‘population’. At first I was reluctant to do so on the grounds that quotations should be accurate and not altered to meet contemporary sensibilities. But he pointed out that, as the original author, I was the only person who could possibly object. I changed ‘present races of man’ to ‘present populations of man’ and sent the paper to be translated into

Italian. When it was published the translator had rendered the phrase as ‘*le razze umane moderne*’. (Edwards [undated](#), unpublished manuscript)

So, the whole process unfolded in three stages. First, it began with the original text containing the word “race”. Then with time the term started to sound unacceptable or jarring to some people, and consequently the pressure of the new linguistic practice transformed it into “population”. Finally, an easygoing and politically unconcerned Italian translator just went ahead and turned it back into “*razza*”.

Genetic differences

In biology, the concept of “race” is often regarded as synonymous with “subspecies”. Subspecies are populations of organisms that, despite belonging to the same species, differ among themselves with respect to frequencies of alternative alleles at a number of loci. These differences are the result of these populations being at least partly isolated from one another long enough for a genetic differentiation to develop. Research has shown that, indeed, groups of people of significantly different geographical ancestries do differ from one another genetically: when compared on many genetic loci these groups have different frequencies of different alleles.

Is this empirical fact sufficient to vindicate the notion of human subspecies or human races? Many say “No”, offering two main reasons for their skepticism: first, that the genetic variation between the groups is too small (compared to the genetic variation within groups), and second, that the groups formed on the basis of genetic similarity do not actually correspond to common sense races.

Too small to matter?

Since the publication of Richard Lewontin’s widely cited article (Lewontin [1972](#)), it has been a matter of scientific consensus that a much smaller part of the total human genetic variation is between the races than within the races. Lewontin estimated the inter-racial variation comprises only about 7% of the total genetic variation in the human species. Some philosophers think that this numerical fact alone shows that the biological concept of subspecies (or race) is inapplicable to humans (e.g., Machery and Faucher [2005](#), 1208–1209; Richardson [2000](#), 847). Robin Andreasen also uses the relatively small between-group genetic variation to argue 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’” (Andreasen [1998](#), 215, cf. [2000](#), S663; Mallon [2006](#), 529). In a similar vein, Ned Block tries to undermine the importance of race by mentioning that only about 7% of all human genetic variation lies between the major races (Block [1995](#), 112, 115). All these philosophers seem to consider the anti-race import of that low percentage figure so cogent and straightforward that they do not even deem it necessary to clarify how it is supposed to establish their conclusion. Yet their reasoning is fallacious: 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” (see Edwards 2003). An argument that is due to Lewontin and that has been uncritically accepted by almost all philosophers is that racial classification is of virtually no genetic or biological significance just because the genetic differences between the races on a number of arbitrarily selected loci are typically found to be swamped by the corresponding within-race differences. But as Edwards has shown, Lewontin completely ignored the aggregation effect of these inter-group differences in allele frequencies on different loci, which could (and arguably does) support a racial taxonomy—without a need for a very big average variation between the races on a locus-by-locus basis. Even with Lewontin’s condition satisfied (i.e., the within-group variation being much larger than the between-group variation), a clear group structure can still emerge on the basis of these aggregate properties of populations. It should be emphasized that Lewontin’s fallacy was exposed long before Edwards’ article in 2003. An especially clear explanation is given in Mitton (1977) and (1978), the articles that somehow missed the attention of most scholars, including Edwards himself (personal communication).

Look at a simple (fictitious) illustration in Fig. 1. There are two groups of objects, represented in the graph by triangles and squares, respectively. For any of these forty objects one can read off its numerical value on either of the two dimensions (on the X and Y axis). On each of the two dimensions, considered

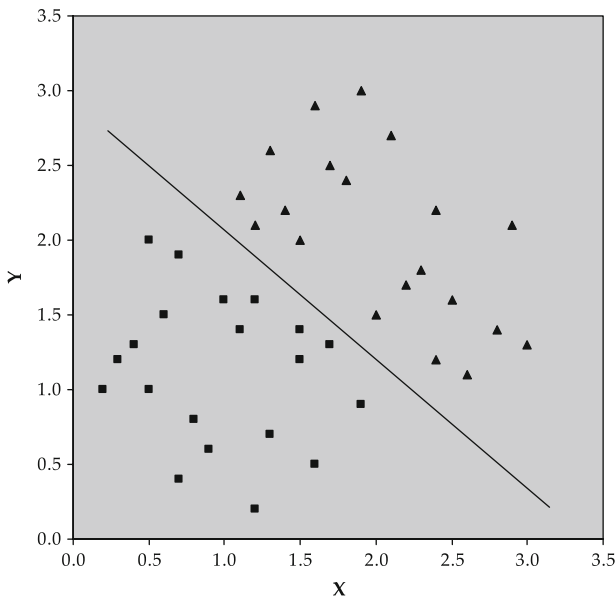


Fig. 1 Clusters emerging in a two-dimensional perspective

separately, triangles and squares overlap a lot and there is no way to segregate the two shapes into two distinct groups. Put differently, any way you choose to split the X axis into two parts, you will never be able to get all and only the triangles having their X -value on one side of that cut (and consequently all the squares with their X values on the opposite side of the cut). The same is true about the Y axis. However, *combining* the two dimensions makes it possible to appreciate that the two groups indeed fall apart and that they can be divided off from each other by a straight line.

It bears emphasis that with respect to each of the two variables (X and Y) the within-group variation is larger than the between-group variation, and yet the group structure emerges already in the two-dimensional graph. Adding one more dimension might further strengthen the group separation and make the segmentation even clearer and more robust. Adding yet another dimension could push the groups still further apart... And so forth. I haven't used some of the existing graphs that make the same point because they assume that there is a X - Y correlation within groups, which is both unnecessary and disanalogous to the case under discussion (differences within and between human races).

Returning to the topic of human genetic variation, we are now in a better position to diagnose the source of Lewontin's fallacy. The figure of only 7–10% of total genetic variation belonging to the variation between the races actually refers to the inter-racial portion of variation that is *averaged* over the *separate* contributions of a number of *individual* genetic indicators that were sampled in different studies. In other words, this information is completely restricted to an iterated single-dimension-point-of-view. Any structure that might exist at the level of *the aggregation* of the inter-group genetic differences is absolutely invisible from that essentially one-dimensional perspective.

Lewontin's univariate approach to the conceptualization of race is particularly clear when he asks: "How much difference in the frequencies of A, B, AB, and O blood groups does one require before deciding that it is large enough to declare two local populations are in separate 'races'?" (Lewontin 1987, 200) This is the wrong question completely. Races are not distinguished from one another by some specially big difference of allelic frequencies in one trait, but rather by a combination of a number of small or moderate differences in many traits. That is, *e pluribus*, not *ex uno*.

Here is another way of explaining Lewontin's fallacy, by using an analogy with two biased coins. Assume that coin 1 is slightly biased toward heads, with its $p(H) = 0.6$, whereas coin 2 is biased in the opposite direction, with its $p(H) = 0.4$. If one of the two coins is randomly chosen and then flipped, observing the outcome (heads or tails) will not help us much in guessing which of these two coins was actually flipped. On average, our best guess (that it was the coin with the bias toward the observed outcome) will be correct only slightly above the chance level of 0.5. To be precise, our success rate with this strategy will be 60%. But with the increasing number of tosses of the selected coin, our predictive ability will become better and better. Consider the situation in Table 1 that represents 11 flips of the unknown coin, which in every trial has the same probability of heads (either 0.6 or 0.4).

Table 1 Coin 1 or coin 2?

	Coin 1	Coin 2
Flip 1	$p(H) = 0.6$	$p(H) = 0.4$
Flip 2	$p(H) = 0.6$	$p(H) = 0.4$
Flip 3	$p(H) = 0.6$	$p(H) = 0.4$
Flip 4	$p(H) = 0.6$	$p(H) = 0.4$
Flip 5	$p(H) = 0.6$	$p(H) = 0.4$
Flip 6	$p(H) = 0.6$	$p(H) = 0.4$
Flip 7	$p(H) = 0.6$	$p(H) = 0.4$
Flip 8	$p(H) = 0.6$	$p(H) = 0.4$
Flip 9	$p(H) = 0.6$	$p(H) = 0.4$
Flip 10	$p(H) = 0.6$	$p(H) = 0.4$
Flip 11	$p(H) = 0.6$	$p(H) = 0.4$

Based on the observed outcomes of these 11 flips, we have to infer which of the two coins was in fact tossed (i.e., which of the two columns represents the reality).

How often will we be right? First, notice that in these eleven tosses either there will be more heads than tails or more tails than heads. Obviously the probability of more heads (MH) is higher with coin 1 than with coin 2. The binomial probability calculator tells us that $p(\text{MH}|\text{C}_1)$ is 0.753, and $p(\text{MH}|\text{C}_2)$ is 0.247. Bayes's theorem then gives us the answer to the specific question we are interested in: what is $p(\text{C}_1|\text{MH})$?

$$p(\text{C}_1|\text{MH}) = \frac{p(\text{C}_1) \times p(\text{MH}|\text{C}_1)}{p(\text{C}_1) \times p(\text{MH}|\text{C}_1) + p(\text{C}_2) \times p(\text{MH}|\text{C}_2)} = \frac{\frac{1}{2} \times 0.753}{\frac{1}{2} \times 0.753 + \frac{1}{2} \times 0.247} = 0.753$$

Relying on the information that in 11 tosses there were more heads than tails (or the other way around, as the case might be) we will be able to guess correctly which coin was selected in approximately 75% of the cases.

How about the case with 101 flips? Here using the same reasoning as above, the probability that we will be right about the coin will rise to 0.979.

What will happen when there are, say, 1,001 flips? In that situation our probabilistic inference will lead us to the wrong conclusion only in about one out of 10 billion cases. We have reached the region of virtual certainty.

The lesson here is that although inferring the identity of the coin from the outcome of *any individual* flip is indeed highly fallible and unreliable, using the *joint* information about *more and more* flips can improve our predictive ability significantly. By just increasing the number of tosses we can approach the limit of perfect accuracy as close as we like.

To see how all this connects with Lewontin's fallacy let us move from coin tossing to genetics. Let us assume that an individual is randomly selected from one of two populations. Our task is to guess to which of these two populations the individual belongs, given that these two groups have different frequencies of two

Table 2 Population 1 or population 2?

	An individual from P ₁	An individual from P ₂
Locus 1	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 2	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 3	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 4	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 5	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 6	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 7	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 8	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 9	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 10	$p(A_1) = 0.6$	$p(A_1) = 0.4$
Locus 11	$p(A_1) = 0.6$	$p(A_1) = 0.4$

alternatives alleles on a number of loci. For simplicity, suppose (following Edwards 2003) that for any particular locus, whichever of its two alleles we take, its frequency will be 0.6 in one group and 0.4 in the other. Now for every of these biallelic loci I will label the allele that is more frequent in population P₁ as allele A₁ (for that locus). And any allele that is alternative to A₁ on a given locus will be called A₂ (for that locus). Obviously, A₂ will always be more frequent than A₁ in population P₂.

Table 2 shows a situation in which eleven loci are considered and which is so structurally similar to the two-coin example that it contains the same numerical values as Table 1. So the same calculation must apply.

If by using the information from *only one* locus we decide to assign a randomly chosen individual to the population in which the actually observed allele is more frequent than its alternative, we will get the wrong result in 2 out of 5 cases. If using 11 loci and assigning the individual to the population in which *the majority* of the observed alleles are more frequent than their alternatives, the rate of error will be around 1 out of 4. With 101 loci, the probability of mistake with this method will be approximately 1 out of 50. Finally, with 1,001 loci the wrong group attribution will be so spectacularly unlikely that for all practical purposes our inference will be error-free.¹

Therefore, *contra* Lewontin, the racial classification that is based on a number of genetic differences between populations may well be extremely reliable and robust, despite the fact that any single of those genetic between-population differences remains, in itself, a very poor predictor of racial membership.

¹ Here is the translation manual for the coin analogy. The two coins with opposite biases (towards heads or tails) correspond to the two individuals from two different populations that have opposite allelic biases (towards A₁ or A₂). Different flips of the coin correspond to different loci. The heads or tails outcome in a particular coin throw corresponds to the A₁ or A₂ outcome on a particular locus.

Races \neq groups based on genetic similarity?

Fine, the argument could go (and actually did), it may well be that heterogeneous human populations indeed cluster into a number of distinct groups based on the multivariate genetic similarity, but it may still turn out that these gene-based clusters do not correspond to common-sense races at all. In that case, the colloquial racial classification would still be left with no support from biology: “While we argue that there likely are a variety of identifiable and biologically meaningful races, these will not correspond to folk racial categories” (Pigliucci and Kaplan 2003, 1161).

Ironically, empirical knowledge about race and genetic is advancing so fast that Pigliucci’s and Kaplan’s prediction was already refuted while the article with their bold claim was still in print. In an important paper that came out in *Science* at the very end of 2002, 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.

Ian Hacking was also unlucky with the following scientific prophecy that he put forward: “Science might have revealed an endless number of differences between the races that are not consequences of the marks by which we distinguish them, namely color and physiognomy. But science has not done so, and almost certainly will not”. (Hacking 2005, 104) As yet another indication of how quickly this research area is moving forward, Hacking has backpedalled considerably in an article published just 1 year later, adopting a much more guarded tone and now explicitly conceding that “stereotypical features of race are associated both with ancestral geographical origin and, to some extent, with genetic markers” (Hacking 2006, 86).

There have been some attempts to downplay the importance of the results presented in Rosenberg et al. (2002). For instance, it was said that the major geographical regions, for which the correlation with genetic clusters was observed, only loosely correspond to social categories of race (Bonham et al. 2005, 12) and that self-reported race (which better represents the common sense notion of race) would not necessarily show the same level of correlation with genetic clusters (Barbujani 2005, 521). These complaints were raised in 2005, but they were put into doubt by a new study that was published in December of the same year.

A group of researchers led by geneticist Neil Risch analyzed genetic data for 326 microsatellite markers on a sample of 3,636 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. Commenting on this result in an interview, Risch said that if the concept of race is regarded as genetically suspect because of this extremely low discordance rate of 0.1%, then any classificatory scheme should be rejected as well because “any category you come up

with is going to be imperfect” (Gitschier 2005, 4). He added that if nothing short of a *perfect* correspondence could legitimate the genetic basis of a common sense category, then it would follow that even the distinction between “male” and “female” has nothing to do with genetics either, since in his study a discordance rate between self-reported sex and markers on the X chromosome was actually higher than the discordance rate between self-reported race and the genetic cluster membership.

A good measure of the robustness of racial genetic differentiation is the answer to the following question: “How often does it happen that a pair of individuals from one population is genetically more dissimilar than two individuals chosen from two different populations?” In fact, if many thousands of loci are used as a basis for judging genetic similarity and when individuals are sampled from geographically separated populations, the correct answer, which many will probably find surprising, is: “Never” (Witherspoon et al. 2007, 357).

To illustrate how, due to recent developments in science, a chasm opened between the consensus in philosophy of biology and views of some cutting edge geneticists, let me juxtapose contrasting statements of two authorities in these two fields. David Hull, philosopher: “The subdivisions of *Homo sapiens* that experts recognize do not come close to coinciding with the ‘races’ of ordinary people” (Hull 1998, 366). Neil Risch and his team of geneticists: “The correspondence between genetic cluster and self-identified race/ethnicity is remarkably high... Accordingly, in this case, major self-identified race/ethnicity and genetic cluster are *effectively synonymous*” (Tang et al. 2005, 271—italics added). In another paper Risch stated that “effectively, these population genetic studies have recapitulated the classical definition of races based on continental ancestry” (Risch et al. 2002).

This does not mean, of course, that Risch’s results by themselves conclusively establish the biological reality of race or refute social constructivism about that concept. Questions can be (and have been) raised about whether the same outcome would be obtained for other racial categories, or with a sample of people with more mixed ancestries, or on a more fine-grained scale, etc. My point is merely that in view of these new studies it becomes harder to accept the widespread but often unsubstantiated claim about the biological meaninglessness of race. Those who support this view ought to stop relying on simplistic and time-worn arguments that have been largely discredited. Instead, they should make contact with the most recent exciting developments in genetics and deal with the best contemporary attempts to rehabilitate the biological foundations of race.

Another way to illustrate how recent advances in empirical knowledge have changed the terms of the debate about race is to contrast the statements of the two high-profile geneticists, Craig Venter and Francis Collins. In June of 2000, at the White House press conference in celebration of the completion of the sequencing of the human genome, Craig Venter famously said that “the concept of race has no genetic or scientific basis”. It was only 4 years later that Francis Collins (the leader of the rival research team in the Human Genome race) decided that the studies that have appeared in the meantime made it necessary for him to disagree: “*Increasing scientific evidence*, however, indicates that genetic variation can be used to make a reasonably accurate prediction of geographic origins of an individual, at least if that

individual's grandparents all came from the same part of the world. As those ancestral origins in many cases have a correlation, albeit often imprecise, with self-identified race or ethnicity, *it is not strictly true that race or ethnicity has no biological connection*" (Collins 2004, S13, italics added).

Morphological differences

In everyday life we classify people into different races on the basis of their external appearance. The morphological features used for this purpose include skin color, hair texture, bone structure, facial features, etc. Although we are in general pretty accurate in assigning people to different racial groups, there is an influential argument purporting to show that any such phenotypic categories are actually arbitrary and lack objective validity. This so-called "independent variation argument" or "the fact of discordance" has been accepted by a number of philosophers (e.g., Andreasen 2004, 428, 2005, 94, 2008, 484; Machery and Faucher 2005, 1209; Glasgow 2009, 88; Appiah 1996, 99; see also Appiah and Gutman 1998, 116). These philosophers rely (disjunctively) on the following sources and treat them as entirely authoritative:

If one genetic character is used, it is possible to divide a species into subspecies according to the variation in this character. If two characters are used, it may still be possible, but there will be some "problem populations", which, if you are an anthropologist, will be labeled composite or mixed. As the number of characters increases it becomes more nearly impossible to determine what the "actual races really are". (Livingstone 1962).

There are many different, equally valid procedures for defining races, and those different procedures yield very different classifications... Faced with such differing classifications, many anthropologists today conclude that one cannot recognize any human races at all. (Diamond 1994, 84)

The success of racial classification depends on the number of traits used in ordering the races. A single trait such as skin color will result in a classification system that is easily determined. Add another trait and classification becomes a more difficult task, and there usually are groups that cannot be classified. As you increase the number of traits, the problems in racial classification become insurmountable. (Brown and Armelagos 2001, 34)

It is interesting that none of these sources cites any empirical evidence for these race-undermining claims. Furthermore, these claims clearly go against the entrenched common sense belief that racial recognition is not actually based on a single trait (like skin color) but rather on a number of characteristics that are to a certain extent concordant and that jointly make the classification not only possible but fairly reliable as well (a point that Diamond himself actually acknowledges *sotto voce* but which, buried at the end of his article, has been completely lost on most readers amidst his thundering denunciations of the race concept). Worse still, forensic anthropologists are quite successful in correctly inferring a person's race from the skeletal characteristics of human remains, which would of course be

impossible if the statements in the above quotations were true. This 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?” (Sauer 1992).

Sauer explained that in forensic anthropology race is assigned with high probability on the basis of an algorithm that combines a series of measurements. According to him, it is taken for granted among forensic anthropologists that race is determinable from the skull and postcranium, and “if such a determination is not possible, the problem is usually attributed to the incomplete nature of the remains or mixed ancestry” (ibid. p. 109).

Indeed, a quick look into the literature confirms this. For instance, a study that covered 17 populations over the world and that relied on 34 different measurements managed to assign 98% of the specimens to their correct major racial group (Brues 1990, 6). Another more recent study had a success rate of 80% in distinguishing between American Whites and Blacks, although it used just two variables. With seven variables, however, it reached the reliability of 95%, and with 19 variables the probability of correct classification rose to 97% (Ousley et al. 2009). Also, estimating generally the reliability of attributing a given data point to one of the five racial categories, another team of experts calculated that under some realistic conditions it is sufficient to use as few as 13 characteristics to have the posterior probability of the correct classification attain the value of 99% (Königsberg et al. 2009).

The empirical reality appears to refute decisively the claim so confidently advocated by many philosophers that “as the number of traits increases, racial classification becomes increasingly difficult” (Andreasen 2004, 428), or that “multiplying phenotypic racial traits has the result ... that ... they correlate with one another in no particular order, throwing the alleged features for biological racial reality into an unorganized mess” (Glasgow 2009, 88). This is exactly backwards: multiplying relevant phenotypic racial traits brings more order and structure, and indeed lays ground for an *objective* biological classification.

Here is another example of conflicting statements coming from philosophers and working scientists. Philosophers: “Assigning an individual to a race does not buy the inferential power you are usually warranted to expect from a biological kind term” (Machery and Faucher 2005, 1209). Geneticists: “It may be possible to infer something about an individual phenotype from knowledge of his or her ancestry” (Witherspoon et al. 2007, 358).

Philosophers have put too much trust in those strongly worded proclamations against the reality of race that are often issued to the wider public by high-profile scientists or learned societies. The problem with these public statements about such a politically sensitive issue is that they are not always driven just by a desire to transmit current knowledge. They do not necessarily correspond to what is really going on in scientific discussions in peer-reviewed journals. As mentioned above, it is particularly in physical anthropology that there is this acute clash between declarations directed to the public at large and the actual scientific practice, with the former often being governed less by science and more by “the reasons of the heart”. This kind of a *homo duplex* situation produces a lot of frustration and confusion, but it has also once prompted a comment that those anthropologists who

deny that races exist “have their heart, but not their heads in the right place” (cited in Gill 1990, viii).

One of the leading physical anthropologists warned about the tendency of some scientists to misrepresent the true view of their scientific community. He said that the idea widely propagated by many scientists that race is only skin deep “is simply not true, as any experienced forensic anthropologist will affirm” (Gill 2000). He went further and stated that the bias of the race-denial faction “seems to stem largely from socio-political motivation and not science at all” (ibid.). And finally: “At the beginning of the twenty-first century, even as a majority of biological anthropologists favor the reality of the race perspective, not one introductory textbook of physical anthropology even presents that perspective as a possibility. In a case as flagrant as this, we are not dealing with science but rather with blatant, politically motivated censorship” (ibid.).

There is also evidence from a content-analysis of the scientific literature that in the very period of the widespread public denigration of the race concept, the racial terms were actually used with increased frequency in scientific publications: “Based upon my findings I argue that the category of race only *seemingly* disappeared from scientific discourse after World War II and has had a *fluctuating yet continuous* use during the time span from 1946 to 2003, and has even *become more pronounced from the early 1970s on*” (Gissis 2008, 438—italics in the original).

Some other scientists agree with Gill that race denial is not based merely on scientific considerations: “The proposal to scrap the concept of race altogether is currently only one extreme in a range of views. It is certainly not shared by all anthropologists and is by no means the majority opinion of the public at large. It appears to be a conclusion reached more on the basis of political and philosophical creeds than on scientific arguments” (Klein and Takahata 2002, 384).

These are strong words and I am not suggesting that we should immediately agree with them. But if, as seems to be the case, many serious and responsible scientists complain that the scientific opinion in their disciplines has been badly misrepresented because of the pressure of political correctness, there is every reason to be very cautious and make an effort to study primary sources, rather than rely too heavily on scientists’ pronouncements and proclamations addressed to the general public. Given this smoke and mirror situation in the debate about such a politicized issue as race, where emotions run high and where huge dangers of a wrong step are obvious to everyone, do not take at face value what scientists merely say about these topics. Instead, look at what they actually do about it in their real work.

As Henry Harpending astutely observed: “A poll about views of race would be like a poll about Marxism in East Germany in 1980. Everyone would lie” (Harpending 2000).

Psychological differences

Is there some reason to believe that some of the psychological differences between the races (like, for instance, the observed IQ differences between racial

groups) are at least partly due to genetic differences between them? I will argue here that the answer to that question is *not* an unequivocal and resounding “No”, as many philosophers and scientists are asserting. The affirmative answer cannot be rejected out of hand or treated as completely unfounded (as is often done), because it is actually supported by a number of empirical arguments that have considerable strength and legitimacy. For a recent overview of the literature and a forceful defense of the hereditarian position in this debate see Rushton and Jensen (2005).

Both in philosophy in general and in philosophy of science it is the received wisdom that the hereditarianism about group psychological differences lacks any plausibility and should not be considered as a serious empirical hypothesis. The problem, however, is that this received wisdom is not the result of a detailed analysis of all the existing arguments pro and contra. With the exception of the maverick Michael Levin, a philosopher who has an excellent command of the relevant literature and is also an outspoken critic of pure environmentalism (Levin 1997), other philosophers who discuss this topic merely gesture at the alleged weaknesses or a “pseudo-scientific” nature of hereditarianism, but typically without engaging at all with the specifics of the controversy between hereditarianism and pure environmentalism about racial psychological differences.

We are told, for instance, that science has shown that morphological differences between the races are “not correlated with traits that are humanly significant” (Hardimon 2003, 455), that “the belief that racial differences are anything more than superficial is a repugnant error” (Hacking 2005, 102–103), that “apart from a small handful of arbitrarily selected visible characteristics, the members of different races are not all that different” (Andreasen 2000, S663), that if “race” is supposed to refer to inherent behavioral and temperamental differences then “race is an illusion” (Glasgow 2009, 118), and that race is “at most a matter of physical characteristics that can have no bearing on anyone’s abilities or moral character” (Dummett 2004, 31). No citations are ever provided to support these empirical claims.

Even when an argument against hereditarianism is offered, it is often weak and completely misses the point. For reasons of space I will here give just two characteristic illustrations of how cavalierly scientists and philosophers cut this Gordian knot in order to reach a desirable conclusion.

After raising his famous “Yali’s question” about the economic backwardness of New Guineans (“Why white people have so much cargo and New Guinean so little?”), Jared Diamond briefly considers a possibility that a genetic difference in cognitive ability between the two groups might partly account for the observed disparity in their economic development. But although most studies indeed put the IQ of New Guineans consistently far below 100 (the white mean), Diamond immediately rejects the genetic hypothesis for the following reason: “To me, any explanation based on race is absurd. I know too many really smart New Guineans to believe there is anything genetically inferior about them” (Diamond 2005). It is easy to see that Diamond’s objection to hereditarianism has no force, and that such a reasoning would be at once recognized as a blatant fallacy in any other context. Just consider an entirely analogical and equally faulty inference: “Any explanation of the difference in height between men and women that is based on genetics is absurd.

I know too many tall women to believe that there is anything genetic about their comparatively lower height”.

Another illustration is related to a well known fact that, statistically, the incidence of criminal activity is significantly higher in some racial groups than others. How can this be explained? Is this *entirely* the result of different environmental influences, or is at least a part of that disparity perhaps due to genetic differences between the two groups? There is a huge literature on this issue, which is notoriously dominated by uncertainties and honest disagreements among social scientists. Philip Kitcher, however, proposes a quick resolution of this debate: “Young men with dark skin are not more likely to commit crime because of the darkness of the skin or because the alleles that code for proteins that increase melanin concentrations in the skin have some psychological side effect, but because they are poor, undereducated, given fewer opportunities, and so on” (Kitcher 2007, 310). Notice how hereditarianism is presented here in a wildly implausible form as hypothesizing that a disposition toward criminal behavior is perhaps caused by the darkness of the skin (?) or is just a side-effect of those alleles that increase melanin concentration. Needless to say, no scholar has ever defended such a silly explanation.

Similarly, Kitcher writes elsewhere: “Unless we are profoundly deceived, there are some readily identifiable features of the physical and social environment that have major impact: rates of crime are much higher in decaying inner cities, but I doubt that there is a ‘violence’ allele that has the pleiotropic effect of sending its bearers into grim urban environments” (Kitcher 2003, 294).

Again, Kitcher presents the debate between the two rival explanations of criminality in such a way that his environmentalist hypothesis has a certain air of plausibility, while the genetic causal scenario he considers is too crude to deserve any discussion. “Is there a *single gene* that both causes its bearer to become violent and also sends him to live in a decaying inner city?” Well, no!

Pitting an initially promising environmentalist account against a glaringly ridiculous hereditary alternative creates the appearance that the debate is effectively over and that only people with some sinister non-cognitive motivation could continue to support a genetic explanation. With one side in the controversy being distorted beyond recognition and pictured as a ludicrous non-starter, the other side wins by default.

It is precisely in this way, through a highly tendentious presentation and without making any connection with the relevant scholarly literature, that the received wisdom about the absolute defeat of hereditarianism has spread across philosophy as a field with such intensity that even people with no demonstrated expertise in biology, psychology or behavior genetics feel entitled and confident enough to inform the public about the “correct” scientific view.

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 difference (appearance, or even immunities to certain diseases), 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*” (the last emphasis mine).

Despite the authoritative tone of voice, Heck has never published anything even remotely related to the topic of racial differences in IQ. Nor is there any indication that he is sufficiently acquainted with pertinent research to be in the position to make such a sweeping condemnation of one whole side in this long raging scientific controversy. Besides, Heck’s claim that there has never been any decent reason to accept hereditarianism in this debate is hard to square with the fact that the view he dismisses so hastily has been supported not only by the big names in the history of biology like Charles Darwin, H. J. Muller, R. A. Fisher, J. B. S. Haldane, A. H. Sturtevant, Julian Huxley, W. D. Hamilton, James Watson and Francis Crick, but also by many reputable and very sophisticated contemporary scholars like Arthur Jensen, Linda Gottfredson, John Loehlin, Richard Herrnstein, Charles Murray, David Rowe, Vincent Sarich, David Bartholomew, James Crow, etc. Now it is possible, of course, that all these scientists were just motivated by visceral racism and that they had “no decent reasons” for their views, but I will assume that this possibility should not be taken seriously.

Conclusion

My aim in this paper was not to prove the biological reality of race. Rather, more modestly, I have tried to show that typical attempts to disconnect the concept of race from genetics have too quickly and too uncritically been accepted by many “race critics”, including most philosophers of science who have discussed this issue. The arguments for deconstructing race are fundamentally unsound because they ignore, misinterpret or distort relevant scientific facts. Therefore, it is time to abandon the mantra about the biological meaninglessness of race. Instead of wasting our time on “refuting” straw-man positions dredged from a distant past or from fiction, we should deal with the strongest contemporary attempts to rehabilitate race that are scientifically respectable and genetically informed. Philosophers (and others) have too long tried to destroy the scientific notion of race in different ways; the point, however, is to understand it.

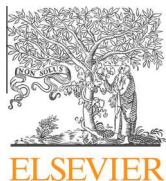
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Discussion

Confusions about race: A new installment

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In his criticism of my paper on the concept of race (Sesardic, 2010), Adam Hochman raises many issues that deserve further clarification. First, I will comment on Hochman's claim that I attack a straw man version of racial constructionism. Second, I will try to correct what I see as a distorted historical picture of the debate between racial naturalists and racial constructionists. Third, I will point out the main weaknesses in Hochman's own defense of constructionism about race. And fourth, I will briefly comment on why I think that Hochman unjustifiably dismisses one of the potential sources of racial differentiation that were suggested in my paper.

Before I start, though, a preliminary clarification is in order. Hochman kindly calls my article "one of the strongest defenses of racial naturalism in recent times", which might suggest to the reader that my goal was to offer a full-fledged biological explication of the concept of race. But in fact my ambition was more limited. As I explained:

My aim in this paper was *not* to prove the biological reality of race. Rather, *more modestly*, I have tried to show that typical attempts to disconnect the concept of race from genetics have too quickly and too uncritically been accepted by many "race critics". (Sesardic, 2010, p. 160; italics added)

I will continue defending the same position in this article.

1. A straw man comes to life

Hochman's central claim is that my defense of the biological notion of race fails because I allegedly equivocate between two interpretations of race: a weak interpretation that, according to Hochman, no one actually disputes (which would show that I am here arguing against a mere straw man), and a strong interpretation, which he regards as hopeless on the grounds that it has been conclusively ruled out on empirical grounds.

I will dispute this diagnosis on both counts. In this section I will try to show that the so-called "weak" interpretation is not a straw man. And later, much more importantly, I will argue that the

"strong" interpretation of race is not undermined by Hochman's arguments.

Take the following proposition:

- (1) Classifying people into commonsense races tells us *absolutely nothing* informative about biological characteristics of these people.

I think this proposition is false. Hochman seems to agree. But he also claims that everyone else rejects (1) as well, that there are no examples of racial constructionists who subscribe to that proposition. Attributing this view to anyone, he argues, just creates a straw man.

Well, let's see. Consider typical statements made repeatedly by leading racial constructionists that race is biologically "meaningless" (AAA, 1994; Fish, 2002, p. 138; Gould, 1996, p. 379; Marshall, 1998, p. 654; Rose, 2002; Schwartz, 2001), that "race as biology is fiction" (Smedley & Smedley, 2005), that "race is the phlogiston of our time" (Montagu, 1964, p. xii; similarly Hirschfeld, 1998, p. 36), that "race" is a concept like unicorn (Fish, 2002, p. 138), that "the reality of human races is [...] destined to follow the flat Earth into oblivion" (Diamond, 1994; a similar claim is also made by physical anthropologist A. Goodman in the 2003 PBS educational documentary "Race: The Power of an Illusion"), etc.

How is the ordinary reader expected to interpret these statements? As accepting (1)? Or as denying it? Or as being agnostic about it? I think it is quite obvious that most people would take these statements as implying (1). The ideas of phlogiston, unicorn and flat earth were rejected because they had no correspondence with reality whatsoever. In equating race with phlogiston, unicorn and flat earth, it is hard to see how Diamond, Goodman, Fish and Montagu could have intended to communicate to their readers anything less than (1). And yet (1) is the view that, according to Hochman, no one defends.

The same applies to the meaninglessness claim. On the face of it, saying that race is biologically meaningless is logically incompatible with denying (1). Denying (1) would entail that race is at least minimally informative about biological characteristics, but a

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biologically meaningless concept cannot be informative about biology. Hochman tries to avoid this implication by a torturous piece of reasoning:

Race offers a poor, misleading representation of human biological diversity. Does this mean that race is biologically meaningless? Well, it depends on what one means by that. If it means that race fails to capture the most basic features of human biological diversity—our predominantly clinal variation, the richness of diversity within sub-Saharan Africa—then yes, race is biologically meaningless. (Hochman, 2013)

Sorry, but no. Saying that race is biologically meaningless means much more than merely that race “fails to capture the most basic features of human biological diversity”. It entails that race is not relevant *at all*, i.e. it entails not merely that race doesn’t capture the most basic features of human biological diversity, but that it captures *no* biological features *whatsoever*. For if it did, it couldn’t be biologically meaningless. So we end up, as before, with the conclusion that these racial constructionists are indeed saying something that in ordinary English is equivalent to (1). Hochman’s “straw man” comes to life again.

But maybe when these people said that race is biologically meaningless they didn’t mean it literally! Maybe. Nevertheless, they must have been well aware that, given the way they chose to express themselves, the public was bound to take them to mean something like (1). Why did they do it then? One possibility is that they were guided by what they regarded as the noble goal of fighting racism and that they intentionally overstated their case in the attempt to downgrade the importance of race as much as possible, even if this involved pushing race denial beyond what is justified by current biological knowledge.

Be that as it may, we should be able to agree about this: if many a public statement by racial constructionists looks like (1) and quacks like (1), we may not know for certain that it “really” means (1), but it certainly deserves to be treated as expressing (1). And discussed accordingly.

2. A consensus that never was

Hochman invests a lot of effort trying to show that recent studies pointing to genetic clustering of world populations (Rosenberg et al., 2002; Tang et al., 2005) do not support racial naturalism. I will address this part of his argument in a moment, but let me discuss something else first.

Hochman seems to think that these new studies are the only small dark cloud that recently appeared on the horizon, and that they represent the key obstacle to further advancement of racial constructionism. Before that, he thinks, social constructionism was clearly winning:

The UNESCO Statements on Race of the early 1950s are understood to have marked a consensus amongst natural scientists and social scientists that ‘race’ is a social construct. Human biological diversity was shown to be predominantly clinal, or gradual, not discreet, and clustered, as racial naturalism implied. From the seventies social constructionists added that the vast majority of human genetic diversity resides within any given racialized group. (Hochman, 2013)

This is a highly distorted picture of how the debate about race historically developed. Undoubtedly there has constantly been an effort to create the appearance of a scientific consensus through race-denigrating proclamations of experts, statements of learned societies and popular science publications. It is also undeniable that there was a lot of political pressure to minimize the biological importance of race. Understandably, many scholars were reluctant

to express their opinion publicly if it deviated from the message that most people obviously wanted to hear and the line that many scientists were eagerly pushing.

But despite all this, it is easy to refute the story of the triumph of racial constructionism. It is ironic that Hochman sees the UNESCO Statements on Race of the early 1950s as having “marked a consensus amongst natural scientists and social scientists that ‘race’ is a social construct”, because just a cursory glance into these documents would immediately dispel any illusion about the existence of a consensus.

Moreover, even before turning to the contents of the statements, a very obvious question arises: why did UNESCO release *two* statements on race in quick succession, one in 1950, and then another in 1951? Answer: because “geneticists and physical anthropologists immediately attacked the [first] statement vehemently” (Provine, 1986, p. 874). So the aim of the 1950 statement may have been to present the authoritative and collective opinion of science on the sensitive issue of race but “the objections of major scientists constituted a severe blow to the credibility of the whole enterprise” (Provine, 1986, p. 874).

Contrast the two UNESCO statements in one key respect. The 1950 statement says: “For all practical purposes ‘race’ is not so much a biological phenomenon as a social myth.” In the 1951 statement that sentence disappeared (it was presumably one of those “important deletions”) while in a newly added text we read: “The physical anthropologist [...] knows that races exist [...] from the scientifically recognizable and measurable congeries of traits which he uses in classifying the varieties of man.” This sounds much more like racial naturalism than social constructionism.

Furthermore, some claims in the first statement that survived revisions and remained in the second version were also strongly criticized by a number of leading scientists who read the draft before publication. The resulting “consensus” was more socially manufactured than real.

With later developments as well, reports about a social constructionist consensus should be taken with a large helping of salt. Among other reasons for healthy skepticism about such stories, it is good always to remember the strong pressure that very often came outside of science to create the appearance of a scientific repudiation of race. Even Peter Medawar, a mainstream scientist with moderate views and hardly a crusader against political correctness once said, recommending the publication of John Baker’s naturalist treatise on race: “*We all know* that the idea of race or raciality has been *systematically depreciated for political or genuinely humanitarian reasons*, and it was high time that someone wrote about race as Baker does, i.e. in the spirit and style of a one-man Royal Commission” (Kenny, 2004, p. 413; italics added).

3. Numerology about race

Let us look at some of the reasons that, according to Hochman, justified the alleged anti-race consensus. Racial constructionists (including Hochman) put a lot of trust in the well-known conclusion against racial classification, which Lewontin once put this way: “We found that there were practically no genetic differences between [human] groups except skin color and body form and a few things like that” (Lewontin, 2003). His claim was that we classify people into races on the basis of a few superficial genetically mediated characteristics, and that we then found that there are *no other* differences between these groups. But how exactly did we “find” that? (Besides, it is unclear how such a negation of an existential statement could be proved with currently available empirical methods.) Also, is it really true that there are no other differences? Answering these questions is a good way to start our discussion.

The main support for Lewontin's claim comes from the discovery that, with respect to human genetic variation, the between-group variation is much lower than within-group variation. The variation between groups is only 15% of total genetic variation, with the remainder (85%) coming from the variation within groups. Commenting on this argument Hochman says: "[M]ost scientists and philosophers thought [Lewontin] was right. Most still do, I think..."

The unfortunate thing is that many scientists and philosophers agree not only with Lewontin's figures but also with a highly dubious implication that he derives from them. For example, in a fairly recent edition of a widely used textbook in physical anthropology the authors still rely on Lewontin's mantra in dismissing the concept of race: "Anthropologists recognize that race isn't a valid concept, especially from a genetic perspective, because the amount of genetic variation accounted for by differences *between* groups is vastly exceeded by the variation that exists *within* groups" (Jurmain et al., 2009, p. 273).

Hochman appears to think that Lewontin's argument gave strong support for constructionism and that racial naturalism is nowadays making a resurgence largely due to a new factor in the debate, namely the influence of A. W. F. Edwards' 2003 article on the so-called "Lewontin's fallacy", as well as some recent empirical work on genetic differentiation of human groups. But in fact there were good grounds for being suspicious about Lewontin's argument even before these new developments.

First, the calculation of the low between-group contribution to total variance refers to differences that were *averaged* over many genetic loci that were taken into account. With respect to some of the examined loci the between-group contribution is actually much larger than 15%. For example, Lewontin's own values for the between-group contribution for the genes Duffy, Lutheran and Rh are higher than 30% (Lewontin, 1972, p. 396).

And second, it is totally unclear how this kind of strange numerical reasoning (within-group variation is 85%, between-group variation is "only" 15%, ergo...) could ever help us infer that, *just because of this "low" percentage of the between-group variation*, differences between the groups must be so small that they should be treated as *quantité négligeable*. This is a huge logical fallacy.

A similar appeal to numerology is made by Craig Venter. Hochman describes it as the claim that "all humans have genome sequences that are 99.9% identical" and "that 0.1% genetic difference between people is not enough to support racial naturalism". It is puzzling that so many scholars (and, to make things worse, many philosophers) could write about this kind of reasoning without their logical alarm bells being set off. (For an exception and good discussion of this issue see Tal (2013).)

After all, recall that we were being told for years that humans share about 99% of their DNA with chimpanzees. (This estimate has been recently downgraded a little.) But of course this made no one think that the DNA difference of 1% between humans and chimpanzees was so small that it precluded any important biological differences between the two species. Yet we are urged to believe that the discordance of 1% comfortably allowed for the existence of substantial group differences, but that somehow the discordance of 0.1% absolutely excludes any such possibility. Why? On what grounds? This is hocus-pocus with numbers, not an argument.

4. "I can't say breakfast!"

The bulk of Hochman's paper is devoted to difficulties with attempts to delineate the concept of race. For instance, he regards it as a problem for racial classification that forensic anthropologists are able to distinguish between groups that are separated not only racially but also "culturally, linguistically, politically, and tempo-

rally, and at a finer grain than a racial taxonomy offers". He adds: "Racial categories are only one way of grouping humans, and an imprecise one at that." Why should this be an objection to race as a biological classification? Surely those who defend that view don't have to be committed to the claim that racial categories are the *only* way of grouping humans, nor should they deny that racial categories are imprecise. They actually never make either of these two manifestly wrong claims.

Also, although Hochman is right that "forensic anthropologists are biased towards racial classification [...] because we, the public, tend to classify (our missing persons) racially", this "bias" doesn't make their findings any less objective or less naturalistic. The fact that they are so successful in classifying people racially shows that there are *real* biological differences underlying "social" race categories. This correspondence of "race" with skeletal and other empirically discovered differences validates the biological underpinnings of that concept, despite the social "bias" without which that knowledge might not have been obtained. Surely the social "bias" cannot produce that kind of result by itself. For example, in former Yugoslavia the public tended to classify people ethnically (Serbian, Croatian, Bosnian, ...), but I bet that despite the strong role ethnicity played in that social context there were virtually no morphological differences to be found along these lines.

Speaking about forensic anthropology, Hochman wonders why I quoted some claims from anthropologist Norman Sauer's article (1992) in support of racial naturalism when, in the text itself, Sauer actually disavows the concept of race. Hochman thinks that the most obvious explanation is that "Sesardic favors persuasion over other academic virtues".

This is strange because immediately before giving this unfriendly diagnosis Hochman quoted a conclusion from Sauer's article which clearly shows that the main thrust of Sauer's antipathy toward the concept of race does not come from his scientific expertise but from rather irrelevant considerations that can be legitimately ignored. Sauer says:

Perhaps we could avoid the term "race" in our communications about cases, substituting 'ancestry' or some other word that has less baggage than race. Perhaps we could be more explicit about the social or cultural concepts of race. Certainly we can teach the non-existence of race in the classroom and do our best to clarify the use of races in forensic anthropology. (Sauer, 1992, p. 110)

So, it turns out that what mainly bothers Sauer is ideological connotations of the word "race" and it is this worry that leads to his proposal that "race" be replaced with another word with "less baggage", like "ancestry". This is a typical, purely verbal maneuver that we so often find in discussions about race. Notice that Sauer actually proposes that the non-existence of race be taught in the classroom and that at the same time the use of race in forensic anthropology be *clarified*, not abandoned. The message seems to be: deny the existence of the category but continue using it! This is very similar to what Frank Ramsey once described as the absurd position of the child in the following dialogue: "Say breakfast." "Can't." "What can't you say?" "Can't say breakfast." (Ramsey, 1990, p. 6)

Interestingly, it is precisely in connection with Sauer (and some other physical anthropologists) that George Gill tried to explain to his puzzled students how these scientists managed to continue with their practice that seems so blatantly to belie their social constructionist beliefs:

Some of my students ask, how can these people, who can on a random sample of skeletons given to them out of context and who can classify them accurately by region (or "race" if forced to use this despised "social construct"), claim that they do not

believe in race? My answer is that we can often *function* within systems that we do not believe in. (Gill, 1998, p. 4)

Many other scholars also try to have it both ways, i.e. deny race and at the same time study it. And it is not just racial realists who complain about this apparent incoherence. Sometimes even racial constructionists chastise their colleagues who ardently espouse politically correct views about race, but appear to make use of race in their actual scientific practice. A nice example is the sarcastic way Debra Harry and Jonathan Marks criticized Cavalli-Sforza and his co-researchers:

We learn, on the one hand, that the Human Genome Diversity Project will prove that races do not exist . . . , and on the other hand, [we see on the cover page of the *History and Geography of the Human Genes*] that its results can be summarized in widely publicized color-coded maps in which “Africans are yellow, Australians red, [Mongoloids blue], and Caucasoids green”. (Harry & Marks, 1999, p. 304)

5. Race counting

An old argument that is often used to support social constructionism about race is connected with the question about the “true” number of human races. Hochman also thinks that this is a major difficulty for the biological concept of race. For example, Neil Risch and his collaborators talk about five major continental races: Africans, Caucasians, East Asians, Pacific Islanders and Native Americans (Risch et al., 2002). But each of these five groups could be divided further into subgroups of different ancestry, which would apparently have a good claim to be also called “races”. So, how many races are there really: 5, or perhaps 10, or maybe 25, or some other number? Suppose that none of these answers is intrinsically more plausible than any other. Would it follow from this that there is something wrong with the idea that race is a biological concept? Not at all.

Take, analogously, the classification of people into age groups: children, adolescents, young adults, middle-aged people, old people. This seems to be a perfectly valid and objective taxonomy, despite the fact that each of these five age groups could also be subdivided further, or that people could actually be classified into age groups in a different way. But being just one of many ways to categorize people by age does not make the system fade into a realm of mere social constructions.

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: “Boyd has recognized five, and Coon, Garn, and Birdsell nine or thirty or thirty-two races. Does it follow that some of these classifications are necessarily wrong? No, all may be right” (Dobzhansky, 1962, p. 266).

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. Why do we not do this? Dobzhansky again explains: “Obviously it would not be convenient to give racial names to inhabitants of the different counties of England or of the different departments of France. But *everyone will agree* that the Negroes, the Europeans, and the American Indians are *clearly distinct*” (Dobzhansky, 1951, p. 661; italics added).

Notice that the three groups chosen by Dobzhansky as illustration are defined by the large continental or sub-continental areas of their origin (sub-Saharan Africa, Europe and America). In a similar vein, scientists speak of “three major human races: African, Caucasian and Oriental” (Bodmer & Cavalli-Sforza, 1976, p. 563), or they say that “[t]he emerging picture is that populations do, generally, cluster by broad geographic regions that correspond

with common racial classification (Africa, Europe, Asia, Oceania, Americas)” (Tishkoff & Kidd, 2004, p. S26; similarly Risch et al., 2002), etc.

This “continental” approach is characteristic for the ordinary concept of race but it has also been adopted by many scholars who believe that seas, oceans, deserts and major mountain chains give rise to larger inter-group differences because they serve as more effective barriers to gene-flow than do occasional intra-continental divides: “. . . numerous human population genetic studies have come to the identical conclusion—that genetic differentiation is greatest when defined on a continental basis” (Risch et al., 2002, p. 3).

Yet it remains true that these “major” races cannot be rigorously distinguished from other groups, as they don’t have a qualitatively different status from other possible groupings. Simply, there are no races with the capital R, and therefore the question about the “true” number of races has no principled answer. Hochman quotes racial naturalist Armand Leroi who is also very happy to concede that “there is nothing fundamental about the concept of the major continental races” and that the world’s population might as well be divided into 10, 100, perhaps 1000 groups (Leroi, 2005). Hochman wrongly takes this as evidence that Leroi must be confused. In fact, racial naturalism does *not* entail that there must be a clear and precise answer to the question about the exact number of human races. After all, as far as I know, no racial naturalist has ever defended this kind of answer in the literature.

Hochman is gratuitously burdening racial naturalism with a dubious implication, and then he takes this as a reason to reject racial naturalism. Perhaps here lies a chance of a possible rapprochement between the two rival positions. If social constructionists stopped imputing to racial naturalists this implausible and rather arbitrary claim they would perhaps no longer see the view of their opponents as so manifestly wrong or untenable.

Taking a broader perspective, I should add that if the impossibility of unequivocal race counting undermines the concept of race, a similar argument could then be made against the concept of species as well. As Ernst Mayr explained, there is no clear-cut answer to the question about the number of species either: “Since most species originate as geographical isolates, one should expect that a certain percentage of such isolated populations are on the borderline between subspecies and species status. The decision whether or not to call such populations species is by necessity somewhat arbitrary. The existence of such borderline cases is what is to be expected if one believes in evolution” (Mayr, 1982, p. 282).

Mayr’s point that the ubiquity of borderline cases is to be expected if one believes in evolution is usually well appreciated by philosophers of biology. They often emphasize the fact that biological reality is messy, that the laws of biology “lie” more frequently than the laws of physics do (assuming that laws of biology exist at all!), and that, especially in life sciences, “the integrity of a subject is not thrown in doubt if the phenomena it addresses cannot be isolated with absolute clarity” (Sober, 2000, p. 5). And yet when it comes to the issue of human biological diversity the criteria for admitting a biological concept suddenly stiffen and sometimes become so demanding that the concept is promptly condemned without even looking into empirical details.

At least in some cases resistance to race naturalism springs from people’s tendency to read too much into that standpoint. Race naturalists do not have to believe (and usually do not believe) that races are clearly delineated groups, easily distinguishable from one another, readily countable, effortlessly applicable to almost anyone, marked by a recognizable genetic signature, etc. One can be a race naturalist without subscribing to any of these views. Moreover, one can be a race naturalist even if one concedes that race is a crude, course-grained and imperfect category. In fact I don’t see why race naturalists shouldn’t be able to agree with the following picture:

I suggest that typical uses of the concept of geographic race today are simply crude labels imposed upon this geographically structured variation. In that sense, race is culturally constructed, as all labels are, *but it is also based on an underlying reality of biological variation.* (Relethford, 2009, p. 20; italics added)

Relethford explains that crude labels may be correct and useful as far as they go, but that it would be ridiculous to criticize these labels by over-interpreting their true meaning:

My point is that we tend to use crude labels in everyday life with the realization that they are fuzzy and subjective. I doubt anyone thinks that terms such as “short,” “medium,” and “tall” refer to discrete groups, or that humanity only comes in three values of height! (Relethford, 2009, p. 21)

6. From clusters to races?

I argued that race naturalism received a boost from some recent studies that showed a nearly perfect correspondence between self-identified race and genetic clusters. The discordance rate in one of those studies (Tang et al., 2005) was around 0.1%, and it seems fair to say that no one would have expected in advance such a high level of agreement between the two taxonomies. Hochman says that not all scholars doing this research interpreted the results of genetic cluster studies as legitimizing the concept of race. That’s true. But then again some did.

One reason why Hochman thinks “we should not be impressed” by the high agreement between self-identified race and genetic clusters in that study is that “the populations sampled have such distinct, geographically distant, ancestries”. But another study was conducted several years later that sampled 27 worldwide populations from sub-Saharan Africa, Europe, India and Asia. When the number of populations was set to four, the genetic clusters that were inferred from the data were “*identical to the four continental groups*” and all individuals were “correctly assigned to their self-identified continental groups *without exception*” (Xing et al., 2009, p. 818; italics added).

Hochman claims that a racial reading of clustering studies ignores the clinal, or gradual, distribution of genetic structure and diversity. I disagree. No one “ignores” anything here. Different scientists just make different inferences from the known empirical facts. Everyone is perfectly aware of the partially clinal or gradual distribution of genetic diversity. The only question, however, is whether the clinal aspect still leaves room for clustering (and then eventually also for racial taxonomy).

Some scholars believe that the appearance of clusters is just a statistical artifact that is more the result of a study design than the existence of either clusters or races. Others argue that the clusters are perfectly compatible with the clinal variation and that they do reflect the really existing genetic structure. Here is the best known formulation of that view:

For population pairs from the same cluster, as geographic distance increases, genetic distance increases in a linear manner, consistent with a clinal population structure. However, for pairs from different clusters, genetic distance is generally larger than that between intracluster pairs that have the same geographic distance... Loosely speaking, it is these small discontinuous jumps in genetic distance—across oceans, the Himalayas, and the Sahara—that provide the basis for the ability of STRUCTURE to identify clusters that correspond to geographic regions. (Rosenberg et al. 2005)

I should add that Rosenberg et al. claim to be agnostic about implications of this discovery for the status of commonsense races.

Yet others embrace genetic clusters as nothing less than recapitulations of ordinary races. Where does all that leave us?

I would argue that at the end of the day this whole study of genetic clusters has considerably strengthened the case for race naturalism simply because it is the first time that it happened that some cutting-edge researchers claim that newly acquired genetic knowledge points to the existence of human groups that largely correspond to main divisions along the lines of traditional continental races. Not all agree with that, of course, but still this kind of scientific validation of the race concept is an important new development, even if it is coming just from a part of the relevant scientific community. Besides, this view can hardly be regarded as a position of a small minority. Asked to comment on different views on this issue the editor of the top journal *Nature Genetics* said in her official capacity: “Risch’s point that there is a high and useful degree of correlation between ethnicity/race and genetic structure, is well taken, and one with which we agree” (Wade, 2002; italics added).

Another clear sign that the race concept is not exactly on the way out is the fact that the use of race categories in medicine has recently received an endorsement from an unexpected source: the United States Government. In June of 2005, the Food and Drug Administration approved the first drug (BiDil) that was intended for one racial group, African-Americans. The implication of this move was not lost on some critics who (according to the *New York Times*) complained “that endorsing a drug for one race gave official government imprimatur to the discredited notion of race as a biological category”. Indeed.

The whole issue is not thereby settled, of course, for it is well known that the introduction of race-based medicine has been opposed by many scholars and medical experts. The point is merely that after the FDA’s approval of the drug targeting a particular race it becomes much more difficult than before to deny any biological relevance of race.

This development gains additional importance if we remember that for a long time there has constantly been a strong public pressure on everyone to downgrade or completely dismiss any interesting connection between race and biology. Even if one happened to think that empirical evidence clearly points to the conclusion that racial membership gives useful biological information, one would often still be reluctant to voice this opinion publicly given that, for example, Craig Venter, one of the highest-profile scientists in the world, warned in no uncertain terms that “it is *disturbing* to see reputable scientists and physicians even categorizing things in terms of race”.

Why would this be “disturbing”? Well, not because this step would necessarily be highly controversial or dangerous per se but mainly because it is feared that it could open the door to some highly undesirable possibilities:

Some African-Americans fear that if doctors start to make diagnoses by race, then some in the public may see that as a basis for imputing behavioral traits as well. “If you think in terms of taxonomies of race, you will make the dangerous conclusion that race will explain violence,” says Dr. Troy Duster, a sociologist at New York University... “Anything that invites the perception of African Americans as biologically different is a huge worry,” said Dr. Gregg Bloche, a Georgetown University physician who studies racial disparities in health care. (Wade, 2004)

The widespread presence of such worries and nervousness about potential biologicization of race shows that the decision of many scientists, physicians and politicians to accept the relevance of race for medicine could not have been taken lightly. They must have had specially strong reasons for persevering in their opinion in the face of vehement and often strident opposition from many influential circles.

Moreover this is what some of these scholars explicitly say: “[T]here is a need for stringent criteria for drawing conclusions regarding the contribution of genes to between-group differences. Generalizations and assumptions are unwarranted and may exacerbate group disparities. We therefore advocate standards for statements regarding genetic contributions to between-group differences” (Mountain & Risch, 2004, p. S52; italics added).

7. Keep away: a “particularly pungent” smell!

In my 2010 article I considered three biological bases for potential racial differentiation: genetic, morphological and psychological. Hochman has no qualms discussing the first two (genetic and morphological differences) but he seems to regard the third (psychological differences) as somehow inappropriate or even completely out of bounds. This is odd because the psychological question has been an inextricable and important part of the controversy about race and biology from the very beginning of the contemporary debate (see, e.g. (Provine, 1986)). Moreover, even when it was not mentioned at all, this topic was often the proverbial 800 pound elephant in the room that determined the dynamics of the discussion and especially how the participants expressed their views for the public.

Whether one is ready to admit it or not, the possibility of a biological impact on group differences is not present only at the morphological or physiological level, while being somehow a priori ruled out at the psychological level. It is all part of the same debate. Hochman, however, disagrees. He says that this “third prong” in my criticism of social constructionism “is not really an argument” because I do “not describe any empirical studies” that would speak to the issue.

Well, isn't it an argument if I criticized philosophers and scientists for the widespread practice of offering (and accepting) logically atrocious “proofs” for the non-existence of psychological differences between races? And isn't it an argument if I supported this claim by giving several specific illustrations of renowned scholars defending manifestly fallacious inferences in that context? And isn't it legitimate (and useful) to warn about the closed minds of many scholars who are no longer interested in reading empirical studies because they have committed themselves to a particular view on the basis of demonstrably inadequate reasons?

Those who have read Hochman's article but not mine will probably wonder why I didn't go into a detailed analysis of relevant empirical studies given that I raised the issue of possible psychological differences among races. The explanation I gave in the paper is very simple: typically the hypothesis about inter-racial psychological differences is dismissed with sophisticated arguments that wouldn't be tolerated in almost any other area of scholarly debate. Therefore, I argued that people would be more likely to approach the whole issue with an open mind if these widely accepted but fallacious “refutations” of the hereditarian hypothesis were first exposed.

Here is another example to illustrate that very point. A few years ago a multidisciplinary group of prominent scholars from Stanford University published an open letter in *Genome Biology* in which they proposed ten statements that should guide the use of racial and ethnic categories in research into human genetic variation. Their fifth statement was: “We caution against making the naive leap to a genetic explanation for group differences in complex traits, especially for human behavioral traits such as IQ scores, tendency towards violence, and degree of athleticism” (Lee et al., 2008). The only argument given in support of that statement was the following: “Current evidence suggests that for most complex behavioral traits, contribution of any one gene to normal variation is small and these traits may be more fully explained by variation in environmental factors” (ibidem; italics added).

An attentive reader will easily spot the fallacy here, even without the help of my italicization. In the conflict between two rival explanations of psychological group differences—pure environmentalism vs. genetic explanation (which allows for some influence of environmental factors)—we are warned not to leap to the genetic explanation by being told that the impact of any one gene is small and that a better explanation might be in terms of many environmental factors. Why this switch from the singular (when talking about genetic explanation) to the plural (when talking about environmental explanation)? This is a highly tendentious and illogical comparison that cannot advance the debate in any way. What makes it worse is that this obviously flawed inference has been endorsed by distinguished scientists like Marcus Feldman and Luigi Luca Cavalli-Sforza, and even by the notable philosopher Debra Satz. Now isn't there at least some value in pointing to such fallacious reasoning in the hope that this would move the discussion away from obfuscations and in the direction of really interesting questions that might eventually be empirically resolvable?

No referee, no journal editor, and none of many readers has expressed any concern about the part of my paper on psychological differences. It is unclear why making the point about the widespread use of bad arguments in that discussion would be inappropriate. Also, why is there so much repugnance toward the topic that it is called “a particularly pungent red herring”?

But this is not all. Commenting on my take on race and psychology Hochman says that “claims of moral and intellectual superiority should be opposed”. Notice how Hochman manages here to turn my neutral and purely *factual question* “Are there psychological differences between races?” into an *ideological statement*, i.e. that some races are morally and intellectually superior to others. Needless to say, even if it happened that racial psychological differences existed, this by itself would not establish racial superiority, whatever that phrase meant.

Finally, Hochman goes even so far as to accuse me of writing the paper in the attempt to legitimize claims of racial superiority. What to say? In discussions about race and biology, morphological and genetic differences between human groups can still be debated with impunity. But raising the same issue about psychological differences among groups apparently crosses the line.

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