

## I

There are simple and powerful arguments against the biological reality of race.<sup>1</sup> Although the phenotypic characteristics, the manifest features that have traditionally been used to divide our species into races, are salient for us, they are superficial, indicating nothing about important differences in psychological traits or genetic conditions that constitute some racial essence. Throughout history, allegations of deep differences in temperament and capacity, claims grounded in no evidence, have done incalculable harm. 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

An earlier version of this article was prepared for a symposium at the Eastern Division of the American Philosophical Association in December 2005. I am grateful to my co-symposiasts, Anthony Appiah and Tommie Shelby, for their exceptionally thoughtful presentations, to Macalester Bell for some insightful advance suggestions, and to members of the audience for their questions and comments. A later version was presented at a conference on "Race in the Age of Genomic Medicine," brilliantly conceived and organized by Koffi Maglo, at the University of Cincinnati, where I had the opportunity to learn from scholars in a variety of fields. Although all the speakers at that conference have influenced the final version, the greatest impact on my thinking came from presentations by Marcus Feldman, Keith Ferdinand, and Charles Rotimi. I also owe a large debt to Ian Hacking, both for his incisive writings about issues of classification over many years, and for sharing with me some important forthcoming work. Finally, I would like to thank the Editors of *Philosophy & Public Affairs*, for suggestions that have helped me to improve this article.

1. These have been well presented by many anthropologists in recent decades; see, for example, F. B. Livingstone, "On the Nonexistence of Human Races," in *The Concept of Race*, ed. Ashley Montagu (New York: Free Press, 1962). Useful recent summaries are provided by Stephen Molnar, *Human Variation* (Englewood Cliffs, N.J.: Prentice-Hall, 1992); and by Jared Diamond, "Race without Color," *Discover* 15 (1994): 82–89. Appiah offers lucid philosophical presentation in his contribution to Anthony Appiah and Amy Gutmann, *Color Conscious* (Princeton, N.J.: Princeton University Press, 1997).

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.”<sup>2</sup>

If you have a particular view of natural kinds, the line of reasoning I have just sketched will appear overwhelming. Suppose you believe that natural kinds are distinguished by some special underlying feature that explains the behavior of members of the kind—like atomic number, for example, in the case of the elements—then you will infer directly from the absence of special genetic or chromosomal markers of race to the biological insignificance of racial divisions. But there is a serious mistake here. The essentialist/explanationist approaches to natural kinds that have dominated much philosophical discussion in past decades have always been woefully inadequate as accounts of biological kinds.<sup>3</sup> Indeed, anyone familiar with the writings of two of the greatest evolutionary biologists of the last century, Theodosius Dobzhansky and Ernst Mayr, can only wonder at philosophical insistence on the idea that natural kinds have essences.<sup>4</sup> As Dobzhansky and Mayr tirelessly pointed out, biological taxa are not demarcated by essential differences; in general, there is no analogue of atomic number, no genetic feature, say, that separates one species of mosquito or mushroom from another; there are occasional exceptions, cases in which species of lizards are formed by hybridization or species of grasses result from doubling, or tripling, of chromosomes, but these are relatively rare.

Many of the premises from which eliminativists about race begin are correct, and important enough to repeat, again and again: there are no genes distinctive of the groups we call races, no biological markers of

2. Ian Hacking, “Why Race Still Matters,” *Daedalus* 134 (2005): 102–16; J. Philippe Rushton, *Race, Evolution, and Behavior* (New Brunswick, N.J.: Transaction Press, 1995).

3. Hacking has made it extremely clear that what philosophers call the “Kripke-Putnam” theory of kinds comprises two related, but distinct, approaches (and explicitly not a fully developed theory). See his forthcoming essay, “Putnam’s Theory of Natural Kinds and Their Names Is Not the Same as Kripke’s,” where he points out how Putnam looks to underlying structures as sources of explanation rather than as essences.

4. Their articulation of a nonessentialist approach to species begins in two classic works of the neo-Darwinian synthesis. See Theodosius Dobzhansky, *Genetics and the Origin of Species* (New York: Columbia University Press, 1937; reprint 1982), especially chap. X; and Ernst Mayr, *Systematics and the Origin of Species* (New York: Columbia University Press, 1942; reprint Harvard University Press [Cambridge, Mass.: 1999]), chap. II–V. Mayr reiterated his main arguments, and his defense of the “biological Species concept,” throughout his long career.

psychological or behavioral differences. In their studies of nonhuman organisms, however, biologists typically do not appeal to distinctive genes in their demarcation of taxa. Once this fact is appreciated, the question of race as a biological category should be recast. Is there a biological basis for dividing species into smaller units, and does appeal to this basis generate a division of our own species into races?

## II

The obvious way to approach this question is to begin from the ways in which species are differentiated. Here, as I have argued elsewhere, we discover a number of species concepts, and the significance of this point will occupy us later. For the moment, however, I want to consider the approach to species most popular among naturalists (especially naturalists who study animals that reproduce sexually). Dobzhansky introduced, and Mayr articulated in great detail, the *biological species concept*, according to which species are clusters of populations that would freely interbreed in the wild, separated from other such clusters by *reproductive isolation*.<sup>5</sup> The notion of reproductive isolation is more delicate than philosophers typically appreciate. It does not entail that interbreeding is impossible: the fact that tigers and lions can produce hybrid progeny under conditions of captivity does not undermine their status as distinct species. Nor does it mean that interbreeding never occurs in the wild: there are well-studied cases of "hybrid zones" at the boundaries of species ranges. The important point about these hybrid zones is that they remain stable and relatively narrow; outside the special conditions, usually marked by a low density of the pertinent organisms, breeding is within the species.<sup>6</sup>

What causes reproductive isolation? Sometimes rather striking features of the organisms like incompatibility of the genitalia or a barrier to proper incorporation of genes into the zygote. Most often, however, the

5. In Mayr's classic formulation, "Species are groups of actually or potentially interbreeding natural populations, which are potentially isolated from other such groups," *Systematics and the Origin of Species*, p. 120; it should be noted that this is the abbreviated version of Mayr's definition, even though it is typically repeated as Mayr's analysis.

6. See M. J. Littlejohn and G. F. Watson, "Hybrid Zones and Homogamy in Australian Frogs," *Annual Review of Ecology and Systematics* 16 (1985): 85–112; and N. H. Barton and G. H. Hewitt, "Analysis of Hybrid Zones," *Annual Review of Ecology and Systematics* 16 (1985): 113–48.

mechanisms are subtle. The courtship behavior of the male fly is rejected by the female of a different species, or the species are simply active at different times or in different places. In the Caribbean, lizard species of the genus *Anolis* sometimes differ in the fact that one species lives in the crowns of trees, another on the trunks, and yet another on the ground around the bases.

Much more could be said about this approach to species, but our interest lies with infraspecific units. As Dobzhansky and Mayr both saw, there are occasions on which we might want to divide a species into varieties or “local races,” *species in statu nascendi*.<sup>7</sup> Consistent with the general approach to species, the obvious criterion to employ is one of *reduced* interbreeding. If we discover a population within a species that is mostly inbred, that is, it is considerably more probable that members of the population will mate with one another than with outsiders, then we have an embryonic version of the condition, reproductive isolation, that distinguishes species. Naturalists identify such populations as subspecies, or races.

I want to refine this conception a bit by recognizing explicitly something implicit in the biological practice: the populations are identified, of course, by phenotypic traits, differences that are sometimes slight, and it is assumed that these differences have arisen over generations of inbreeding. The notion of race is thus that of an inbred lineage, where the inbreeding may initially have resulted from geographical isolation that eventually gives rise to differences in phenotype and to some interference in free interbreeding, even when the geographical isolation is overcome. That notion is available for generating infraspecific units within any species, including *Homo sapiens*.

About a decade ago, I proposed that this was the way to make sense of race as a biological category. Quite independently, Robin Andreasen has deployed a different approach to species (the “cladistic species concept”)

7. The term is introduced in Theodosius Dobzhansky and Boris Spassky, “*Drosophila paulistorum*, a cluster of species *in statu nascendi*,” *Proceedings of the National Academy of Sciences* 45 (1959): 419–28. It is taken up in Mayr, *Animal Species and Evolution* (Cambridge, Mass.: Harvard University Press, 1963), and in Theodosius Dobzhansky, *Genetics of the Evolutionary Process* (New York: Columbia University Press, 1970); but it is effectively present in the discussions of notions of race from *Genetics and the Origin of Species* and *Systematics and the Origin of Species* on.

to argue for a similar thesis.<sup>8</sup> Andreasen and I are united in accepting the biological facts to which eliminativists point; we insist on the absence of deep essential differences among biological races. More recently, Michael Hardimon has refined my conception of race in connection with our own species, suggesting that our ordinary concept of race is that of a relatively inbred lineage that emerges from a particular geographical region and manifests distinctive superficial traits that people find salient.<sup>9</sup> Two points are worth making about Hardimon's careful analysis. First, a similar refinement would be available along the lines marked out by Andreasen. Second, there are obvious prospects for a historical explanation of the emergence of racial concepts. The sorts of phenotypic features often used to demarcate races—skin color, hair texture, and so forth—have always been salient for humans (witness literary descriptions from ancient times); racial concepts were forged in the age of discovery, when clusters of these traits came to be associated with groups of people who had descended from ancestors in a particular region.

Suppose, then, that careful study of patterns of human mating discloses that there are inbred lineages, coming from particular regions and having acquired, over generations, slight differences in phenotypic features that people find salient. Suppose, further, that when the lineages come into contact, rates of inter-lineage mating remain significantly lower than those of intra-lineage mating. There are genuinely biological phenomena here, and a division of our species can be grounded in those phenomena.

But to leave matters at that is unsatisfactory. For the causes of the incipient reproductive isolation may be social. Recall the subtleties of isolating mechanisms, and the species of lizards differentiated by their positions on the trees. Perhaps people in one line of descent do not mate as frequently with those in another line of descent because their paths rarely cross, or rarely cross in contexts conducive to courtship, and perhaps the separation of places of activity comes about because those in one group have once made judgments about those in the other, judgments based on no evidence but profoundly consequential in fixing the

8. Robin Andreasen, "A New Perspective on the Race Debate," *The British Journal for the Philosophy of Science* 49 (1998): 199–225.

9. Michael Hardimon, "The Ordinary Concept of Race," *Journal of Philosophy* 100 (2003): 437–55.

socioeconomic status of the descendants of the judged. The biological phenomenon, the incipient isolating mechanism, is an effect of social attitudes, the result of marginalization and prejudice. These people belong to a different race because they were once *labeled*—mistakenly, ignorantly, unreasonably—as intrinsically different, for that initial labeling has given rise to the separation of their way of life from that of the labelers.

If that is so, races are both biologically real and socially constructed.<sup>10</sup>

### III

I have outlined a view of human races that views them as biological kinds, not in the traditional, thoroughly misguided and harmful, way, but in line with the practice of naturalists who try to bring order to the organisms they observe and study. I could now go on to elaborate the view, and to support it with what evidence there is about human practices of mating. Instead, I want to argue that matters are far more complicated than I have portrayed them.

The simple eliminativist argument with which I began goes astray because of a mistaken premise about natural kinds: natural kinds have essences, and, in particular, biological kinds have genetic essences. The mistake is corrected by turning from a bad philosophical account of kinds to taxonomic practice in biology. But the account I developed also made philosophical presuppositions about natural kinds, presuppositions I now find dubious.

Those presuppositions stem from a realist view of natural kinds. Realists believe that nature is divided, independently of us, of our cognitive capacities and our interests. One of the important tasks of the natural sciences is to trace the divisions—in Plato's famous metaphor, to carve nature at its joints—and, in a discussion of progress, I once proposed that conceptual progress consists in adjusting language to those

10. It is worth emphasizing that there is nothing paradoxical here. We can apply either label depending on how deeply we intend to probe the causal history of our practices of racial classification.

objective fault-lines.<sup>11</sup> If you accept a view like this, then you will be interested in the general types of divisions that various sciences draw, in the ways the fault-lines run. Once you see a particular type of division, division by microstructure, say, or division by interruption of free reproduction, then you will trace that sort of division wherever you can, and take the resultant boundaries to demarcate natural kinds. Interruption of free reproduction is important to biologists, and we should thus look for it within *any* group of organisms. A refusal to apply that principle of division to *Homo sapiens* would have to rest on an illicit separation of our species from the rest of the living world.

This picture no longer seems to be persuasive. I find it hard to envisage nature as prescribing the forms our language should take, as coming nicely organized with fence-posts that our concepts must respect. There is, as I see it, no feasible project of inquiry (singular) that aims at a complete account of our world, but rather many *inquiries* driven by specific questions we find it important to answer. We make conceptual progress by devising concepts that prove useful for us, with our particular capacities and limitations, to deploy in answering the questions that matter to us, and we should recognize that those questions are historically contingent and culturally variable. To use an analogy I find useful, there is a nondenumerable infinity of possible accurate maps we could draw for our planet; the ones we draw, and the boundaries they introduce, depend on our evolving purposes.<sup>12</sup>

Abandoning a strong realism about kinds does not mean giving up completely on realism. There is a world, one world, containing what is completely independent of us, and that world resists some of our efforts to draw boundaries within it; indeed, it resists the overwhelming majority of divisions we might try to find. There are nondenumerably many choices for demarcating objects within it, however, choices that the world would not resist. Other sentient and sapient beings with different capacities might be led to different choices; similarly for human beings with different interests. Even given a choice of boundaries for objects, a

11. See Philip Kitcher, *The Advancement of Science* (New York: Oxford University Press, 1993), chap. 4. In a forthcoming essay, "Plato's Joints," Laura Franklin-Hall subjects Plato's metaphor to devastating scrutiny.

12. See Philip Kitcher, *Science, Truth, and Democracy* (New York: Oxford University Press, 2001), chap. 5. I attempt to explicate the analogy further in "Scientific Realism: The Truth in Pragmatism" (forthcoming).

decision about where Manhattan stops or just how much space is included within a star, there are nondenumerably many ways to sort objects into kinds, and these, too, depend on our capacities and our purposes. In the sense that the world contains the so-far undifferentiated totality of what is independent of us, there is just one world. In the sense that the world is a collection of objects, assorted into types, there are many worlds, and we choose the one, or ones, in which we live.

The position I am sketching, probably too indistinctly, plainly recapitulates the views of Nelson Goodman.<sup>13</sup> Yet there are affinities with earlier authors, especially with James and Dewey, both of whom want to combine a version of realism with an insistence on the indefinitely multiple possibilities of classification.<sup>14</sup> That insistence, rather than the famous, and famously problematic, slogan about truth, seems to me to be the core insight of pragmatism with respect to issues about truth and realism. So, for brevity, I shall call the position I have sketched a *pragmatist* account of natural kinds (I use an indefinite article because it may not be the only one deserving the label).

This version of pragmatism about kinds can be defended by focusing on the pluralistic character of taxonomic practices in the sciences, especially within biology. A couple of decades ago, John Dupré and I argued, independently, that there were lots of different ways in which the world of living things can be divided up, according to the things human beings find salient and according to the purposes they have.<sup>15</sup> Dupré called his view “promiscuous realism” and I referred to mine as “pluralistic realism”; in essence, we continued to take the Platonic metaphor of a beast with joints seriously, and campaigned for multiple-jointedness. So, for example, I proposed that there were many different species concepts, appropriate for different purposes of inquiry. Both Dupré and I, however, tended to think in terms of manageable pluralism, or limited promiscuity; for my part, I took the Biological Species Concept to be one

13. The obvious link is to his *Ways of Worldmaking* (Indianapolis, Ind.: Hackett, 1978). But the same general view is also present much earlier in the dependence of kinds on practices of projection that appears in Nelson Goodman, *Fact, Fiction, and Forecast* (Indianapolis, Ind.: Bobbs-Merrill, 1956).

14. See William James, *Pragmatism*, Lecture VII; John Dewey, *The Quest for Certainty*, chap. 5, *Experience and Nature*, chaps. 1–2.

15. John Dupré, “Natural Kinds and Biological Taxa,” *Philosophical Review* 90 (1981): 66–90, and *The Disorder of Things* (Cambridge, Mass.: Harvard University Press, 1993); Philip Kitcher, “Species,” *Philosophy of Science* 51 (1984): 308–33.



among a number of contenders. The real trouble, however, is that the Biological Species Concept itself allows for indefinitely many ways of development, depending on how one approaches the notions of population and of reproductive isolation. Even worse, it is quite evident that the multiplicity of species concepts I considered, and the folk divisions that Dupré rightly emphasized, pick out only a tiny subset of the possible ways in which people might shape their divisions of the natural world to different purposes. Promiscuity becomes rampant, and, as you appreciate that, the thesis that there is a vast number of ways to carve the beast at its joints, a vast number of privileged fault-lines in nature, topples over into the position that there are no privileged fault-lines at all, that the divisions are drawn to suit our purposes. Really promiscuous realism drops the realism and becomes pragmatism.<sup>16</sup>

I shall say no more by way of motivation and defense. My aim is to explore the consequences of this pragmatic approach to kinds for the naturalistic proposal about races outlined above. The first thing to note is that pragmatism renders suspect a crucial part of the argument for grounding races in biology. Once you move to pragmatism, you lose the general license to introduce a subdivision of *Homo sapiens* on the grounds that the principle of division accords with the infraspecific distinctions biologists make in other cases. The fact that it is useful for certain purposes to use reduced gene flow in a widespread species of oaks to talk about local varieties, or local races, does not mean that it will be useful to mark out similar divisions in the case of our own species. Pragmatism insists that the usefulness be demonstrated in the particular case at hand.

Here I find common ground with some criticisms that people have offered against the biologically grounded proposals made by Andriessen and me, criticisms that are especially cogent against my version. A common objection runs something like this: "You point to reduced interbreeding between human lineages that have been geographically separated and inbred during long periods. But this is likely to be a temporary phenomenon. Sooner or later, and hopefully sooner, the social barriers will be broken down; people will respond to the beauties of

16. Some critics of my proposals about species came close to seeing this point; see, for example, P. Kyle Stanford, "For Pluralism and Against Realism about Species," *Philosophy of Science* 62 (1995): 70–91.

others without regard to the trivial differences of the phenotypic markers used in racial discriminations. Nobody seriously thinks that, in the human case, we can talk of '*species in statu nascendi*'—indeed, it would be a horrific thought."<sup>17</sup> I agree that the phenomenon of reduced interbreeding is likely to be temporary, and, indeed, I think the significance of elaborating the biological approach to race is to raise consciousness about the phenomenon so as to hasten the day when the social barriers disappear. I have been inclined to respond to the criticism by leaning heavily on a realist conception of kinds. Biologists pursuing evolutionary studies have shaped their concepts of species and subspecies to conform to the genuine fault-lines in nature; even in cases where there is no intention of pursuing an evolutionary project, of picking out incipient speciation within *Homo sapiens*, there are similar fault-lines; hence, without supposing that the notion of biological race in human beings is valuable to the same ends, it is still a legitimate biological category.

This response will not do. Given pragmatism about kinds, it is necessary to point to particular purposes that drawing racial divisions in this way would serve, purposes that can themselves be defended. If no such defensible purposes can be identified, then we should simply acquiesce in eliminativism. Indeed, the criticism can surely be strengthened. Given the immense harm that use of racial concepts has generated in the past, insisting on race as a legitimate biological category, even though that concept is linked to no valuable biological project, can seem irresponsible and even perverse. Moreover, even if the concept of race plays a role in some lines of biological inquiry, the values of those lines of inquiry, and of pursuing them through retention of the concept of human race, would have to be sufficiently great to outweigh the potential damage caused by deploying this concept in the other contexts in which it plays so prominent a role, namely in our social discussions.

In assessing this criticism, I think it helps to start with some clear instances of related biological categories that can be defended on pragmatic grounds. With respect to sexually reproducing organisms, a division according to reproductive isolation is valuable in pursuing certain kinds of evolutionary questions, precisely because, when two populations become reproductively isolated, changes in gene frequencies

17. Concerns along these lines were offered independently by Anthony Appiah, Amy Gutmann, and Michele Moody-Adams.

within the one are no longer reflected within the other. The ways in which descendant populations respond to selective pressures may thus be quite different. When the focus shifts to asexual organisms, it is not possible to make the division in exactly the same way, nor is it always appropriate to suggest that the distinctions be made according to the features that accompany division by reproductive isolation in the closest sexually reproducing relatives of the asexual organisms under study (a suggestion that Mayr has repeatedly offered in his attempts to claim a universal priority for the Biological Species Concept).<sup>18</sup> Consider, for example, asexual microbes, including some viruses, bacteria, and parasites.<sup>19</sup> Here taxonomic divisions are reasonably based on the molecules that give these organisms their distinctive ways of attacking the bodies of their hosts, or on the genotypes that underlie the production of those molecules. In effect, you look at what the microorganism *does* to the plant or animal it infects and then group together those microbes with the same crucial structures. It would be mad dogmatism to worry, in this context, about protecting some principle that genuine taxa can only evolve once: if a virus is completely wiped out, but researchers at a bioterrorism agency subsequently use the recorded sequence of its genome to synthesize an exactly similar organism, we would quite properly see them as having subverted the original program of eradication and as having reintroduced the very same virus. For the purposes that drive taxonomy here are medical; we need ways of classifying the microorganisms in terms of the structures that underlie the tricks they use to do harm to their hosts.

The challenge for someone who intends to defend a biological approach to human races is to develop a similar account for the utility of picking out those inbred lineages that descend from populations once geographically separated, in which, as a result of the separation, there are differences in superficial phenotypic traits, characteristics which, despite their superficiality, are salient for human beings.

18. See *Systematics and the Origin of Species*, p. 122, for recognition of the problem with asexuality; for a succinct statement of Mayr's later attempts to deal with it, see *The Growth of Biological Thought* (Cambridge, Mass.: Harvard University Press, 1982), pp. 283–84.

19. For penetrating discussion of the taxonomic issues that arise with respect to bacteria, see Laura Franklin-Hall, "Bacteria, Sex, and Systematics," forthcoming in *Philosophy of Science*.

## IV

Contemporary research on genetic variation within human populations offers what initially appears to be a way of meeting that challenge.<sup>20</sup> Our recently acquired capacities for genomic analysis, coupled with a commitment to understanding human diversity, have enabled biologists to identify subspecific units within the human species—"clusters" as the researchers call them—based on measures of overall genetic similarity. In effect, studies of this kind are using techniques of statistical analysis that critics of the biological species have previously deployed at the phenotypic level, to discern groups that have probably been separated from other such groups for a large part of their ancestry. It is crucial to emphasize that the recognition of different clusters in no way contradicts the received wisdom that there are no racial essences: as the researchers point out, 93 to 95 percent of human genetic variation is found within the clusters (rather than between clusters); each cluster, then, is itself genetically quite heterogeneous.

Faced with the statistical analysis, and especially with the illuminating figures that present the data, it is tempting to say that here we have a completely objective division of the human species into infraspecific groups. We have put the question, and nature has spoken: there are races, or something akin to them. That conclusion, however, has to be hedged with qualifications. First, it is important to understand the question that has actually been put. Given rich data about individuals and bits of their DNA sequences, computer programs have sought divisions, *being told in advance how many clusters they are to find*. So, for example, we might ask, "If our species were to be divided into just two groups on the basis of genetic similarity, how would geographical populations be assigned to those groups?" and we would discover that the two clusters are "anchored by Africa and America" (Eurasian populations would be lumped with the African ones). Ask for three groups, and Eurasia is split off; ask for four, and East Asian populations form a distinct fourth group;

20. The landmark article is Noah Rosenberg et al., "Genetic Structure of Human Populations," *Science* 298 (2002): 2381–85. Effectively, this article is the culmination of the "respectable" biological theorizing about infraspecific—"racial"—divisions that proceeds from the work of Dobzhansky and Mayr to the contemporary achievements of L. L. Cavalli-Sforza and Marcus Feldman. Feldman is in fact the last-named author of the Rosenberg et al. study.

ask for five, and Oceania is separated from the other East Asian populations.<sup>21</sup> So there is a genuine issue about *level* or *fineness of grain*, one that can only be settled on pragmatic grounds: the clusters, or races, will be picked out by fixing the number so that the resulting division best accords with the inquiries we find valuable.

Picking out new clusters preserves, in an important sense, the boundaries that have already been drawn. You may find new subdivisions within a previously identified unit, but you do not generate new clusters that straddle earlier ones. If two populations are assigned to different clusters at one value of the parameter, they remain separated at all higher values. On this basis, one might conclude that the pragmatic component in dividing the species is relatively insignificant, just a matter of finding the appropriate level in an objective tree-structure. There is, however, a second way in which the goals of inquiry affect the whole enterprise, one that elaborates the general points of the previous section. Why, we might ask, does clustering according to genetic similarity identify the significant units within the human population? The obvious answer is that hypotheses about genes, about genetic differences and genetic similarities, play important explanatory roles in addressing questions that matter to us, so that division on a genetic basis yields categories that are more valuable than, say, dividing people up according to the curvature of their eyebrows or the length of time for which they can stand on one leg. Yet here we should tread carefully, for the emphatic disavowal of racial essences already signals the fact that the clusters demarcated on the basis of genetic similarity are not going to play a significant role in the explanation of shared phenotypic features or susceptibilities to various types of disease. Indeed, the authors of the study *do* tread very carefully, linking the categories they introduce, not to some ("ogre naturalist") project of understanding differences in phenotypes, but to understanding the history of human migrations.<sup>22</sup> The fact that

21. Interestingly, as the authors point out, the sixth population is a relatively isolated group from Pakistan; at this stage the association of clusters with major geographic regions breaks down. See Rosenberg et al., "Genetic Structure of Human Populations."

22. Here they continue in a direct way the inquiries carried out by Cavalli-Sforza. It is also worth noting that the kinds of rationale for introducing genetically based classifications that emphasize the causal role of genes in giving rise to human phenotypes (rationales most evident in the writings of "ogre naturalists") are quite alien to biologists like Cavalli-Sforza and Feldman, both of whom have been persistent and subtle critics of tendencies to crude genetic determinism.

contemporary science takes the question “How did our species reach its current distribution?” as a significant one does not entail that there is a list, Nature’s Agenda, on which it figures. It is posed because we find it significant: because of a fact about us. In principle, we might discover, on reflection, that it is not something we need to know, and, if that were to occur, then the enterprise of tracing genetically similar “clusters” would lose its principal rationale.

The pragmatic dimensions of our concepts are frequently invisible to us because we are so used to certain kinds of inquiries that they come to feel natural, externally given. Only when science changes dramatically, or when we realize that some lines of research have damaging social effects, do we pause to wonder if those inquiries are genuinely justified. On the face of it, the genetically similar clusters discerned in the brilliant work of Rosenberg, Feldman, and their associates are well adapted to the pursuit of important issues about human history. The pragmatism I commend would simply involve awareness of the fact that importance is conferred by us, and that the status can, in principle, be retracted.

v

The difficulty with biological projects of subdividing our species is that they appear to introduce a conceptual framework that can easily revive unjust and damaging social practices. Although contemporary research may speak of “clusters” rather than “races,” it is relatively easy to foresee that the old, loaded word will often substitute for the aseptic scientific terminology.<sup>23</sup> As the researchers themselves note, self-reported ancestry (itself entangled with folk racial categories) can sometimes serve as a good proxy for an identification grounded in genetics.<sup>24</sup> The places where divergence is most likely to occur are in practices of classification that appeal to extraneous and superficial markers, where tangled prejudices easily come into play. Yet, of course, where prejudice still exists, overtly or disguised, there is ample motivation for assimilating the scientific classification as a cover for continued assertions about the reality of race.

23. As it did, almost instantly. The *New York Times* rightly saw this as extremely important scientific work, and, ignoring the cautious language of the article, reported it as a regrouping of the concept of race.

24. Rosenberg et al., “Genetic Structure of Human Populations.”

This means that the notion of race is likely to continue to straddle the divide between well-motivated science (for example, the quest to trace patterns of human migration) and social applications. Any pragmatic assessment of its value will have to deal in a synthetic and balanced way with both types of context. We shall need an overall evaluation, one that takes into account all its potential uses and abuses. Recent debates about the continued deployment of 'race' and cognate terms are full of contending voices that emphasize selected aspects of the picture.

One might maintain, at this point, that these contending voices can be ignored, at least insofar as we are concerned with the legitimacy of a notion that has shown itself valuable in connection with a serious scientific project: once we know that talk of "clusters" is valuable in the study of human migrations, debate ends and the concept stands vindicated.<sup>25</sup> That version of pragmatism strikes me as too anemic. As I insisted above, the significance of scientific questions is conferred by us, and, in recognition of the problems associated with continued usage of a concept, it might be reasonable to suggest that, when all the consequences of using that notion are taken into account, we would be better off to give up on particular lines of research.

I anticipate obvious questions and worries. Does this strong pragmatic test set standards for justified scientific research that are impossibly demanding? I believe not. We would rightly worry about the continued deployment of a concept in fundamental physics, if thinking about nature in terms of that concept could lead, relatively directly, to the discovery of principles about the release of energy that would make massively destructive bombs available to anyone.<sup>26</sup> Similarly, if a concept, valuable to some investigators pursuing a particular research question, might cause, in the social world into which that concept is likely to make its way, considerable burdens for many people, then one ought *at least* to raise the question of whether such research is warranted. I emphasize that this is not a matter of *censorship*—the idea of a "thought police" that supervises research and issues interdictions against some programs is obviously counterproductive (as well as being distasteful);

25. I am indebted to an Editor of *Philosophy & Public Affairs*, who suggested that I should confront directly the issues raised in the next few paragraphs.

26. This possibility is explored in Dürrenmatt's play *Die Physiker*. I have elaborated on the moral in chapter 8 of *Science, Truth, and Democracy*.

the ethical question “Should this research be done?” needs to be differentiated from the sociopolitical question “Should there be a public ban on exploring some types of investigations?”<sup>27</sup> We might answer both questions in the negative.

Many areas of scientific research would survive this stronger pragmatic test, for, although there are often uncertainties about the intellectual and practical consequences, the occasions on which one can confidently predict that damage is likely to be done are quite rare.<sup>28</sup> When such occasions arise, the obvious tactic is to try to find ways of insulating the research so that potentially damaging consequences do not occur. Precisely this sensible tactic is prefigured in the use of the term ‘clusters’ by the researchers on human migrations. Unfortunately, the pressure on science journalism, even in the most apparently respectable media, to sensationalize recent findings, led quickly to the demolition of the barrier that the investigators had hoped to erect.<sup>29</sup> So, to recapitulate my earlier conclusion, we need a thorough survey that considers all the potential uses and abuses.

## VI

There is at least one type of social issue for which we might seem to require a notion of race based on separation of inbred lineages. People belong to two kinds of lineages, one biological and one cultural. The former relates us to our biological ancestors and descendants, the latter to those who pass on to us parts of our distinctive mix of ideas and ideals, lore and law, as well as to those to whom we pass on our traditions. When

27. See *Science, Truth, and Democracy*, chap. 8, and also “An Argument about Free Inquiry,” *Noûs* 31 (1997): 279–306.

28. This also means that we only rarely have to confront the obviously difficult issues about how to weigh intellectual values (greater understanding of some aspect of nature) against practical concerns.

29. In the *New York Times* article that rightly celebrated the beautiful research, the term ‘cluster’ immediately gave way to ‘race.’ (It is unclear whether the substitution resulted from a connection that might appear natural to well-meaning people, or whether it should be charged to culpable carelessness.) I heartily sympathize with the tactic pursued by Rosenberg, Feldman, and their colleagues, but any effective use of this tactic will have to come to terms with the ways in which social interests and prejudices distort the transmission of knowledge. I discuss related issues in “Knowledge and Democracy,” *Social Research* (2006). It is also worth noting that Möbius, the central figure of Dürrenmatt’s *Die Physiker*, also tries an insulating strategy—and that he fails.



the biological line in which we stand belongs to a population whose lineages are inbred, and when the principal *cultural* ancestors and descendants of people in these lineages tend to be people who belong to the *biological* lineages, then we have a use for the notions of race and ethnicity, the one pointing to the line of biological descent and the other to the line of cultural descent. This provides a basis for exploring mismatches between race and ethnicity, to pose questions about the desirability of viewing members of a particular race as bearers of its culture. At the heart of claims about cosmopolitanism lies the thought that *cultural descent* should be liberated from the patterns associated with *biological descent*, that individuals should not be confined to the ethnicity associated with their race.<sup>30</sup>

There are genuine questions in this area, ranging from large issues about the survival of cultural traditions and the responsibility of biological descendants to preserve the lore of their ancestors to debates about the desirability of transracial adoption. 'Race' and 'ethnicity' provide convenient shorthand terms for exploring them, and for marking out the places in which concerns about the coincidence of biological inheritance and cultural inheritance coincide. All this, however, may seem far too slight to serve as a counter to the damage that is likely to be done by retaining a notion of race. For, after all, there are obvious and familiar costs to the continued use of racial distinctions.

The most obvious of these is the practice of stereotyping, whether it is manifest in the police practice of rounding up the usual suspects or in a teacher's forming a premature judgment about a young schoolchild. Sometimes the stereotype is imposed on the basis of a folk generalization, a claim that people of a certain race are more likely to have some undesirable trait, where not only do the appliers of the stereotype have no evidence for that claim but there is also in fact, absolutely no evidence for it to be found. On other occasions, however, there may indeed be a correlation that would stand up to serious investigation: evidence would disclose that people with a particular cluster of superficial phenotypic traits, who belong to a relatively inbred lineage that was once separated

30. I discuss issues of this sort at greater length in sections VI and VII of my essay "Race, Ethnicity, Biology, Culture," in *Racism*, ed. Leonard Harris (Amherst, N.Y.: Prometheus Books, 1999), pp. 87–117; reprinted as chap. 11 of Philip Kitcher, *In Mendel's Mirror* (New York: Oxford University Press, 2003).

from other such lineages for many generations, are more likely to have the trait in question. Even here the practice is pernicious, for the correlation is readily mistaken for a causal diagnosis. Despite all our knowledge of the triviality of the genotypic differences between racial groups, the singling out of some racial groups as more likely to engage in criminal behavior (say) encourages the myth that there are deep features of membership in such groups that explain the increased probability. So the practice of stereotyping fosters backsliding into the ugly racial theses that have disfigured past centuries, and they recur in modern dress as searches for behavioral genes associated with criminality, genes alleged to be differentially distributed among the races.

In fact, the practice is even more hideous than I have represented it as being, for a better explanation of the correlations involves the *past* application of racial concepts. Where the correlations are sustained, where, for example, young men with particular phenotypes are more likely to engage in criminal behavior than young men with other phenotypes, nothing hangs on the phenotypes themselves, the textures and colors of skin and hair, nor on the distribution of alleles responsible for such traits. The accidental association occurs because of a past history of poverty and deprivation, one that continues into the present: 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. Behind these conditions, of course, we can trace a past history of discrimination. So, at the root of the causal story are past practices of identifying some people by the superficial characteristics, viewing them as belonging to a special race, and, in consequence, cramping and confining their aspirations and their lives. Crude essentialist notions of race, often committed to prejudiced speculations about the “biological bases” of various cognitive and behavioral traits, have played crucial roles in these practices. Application of the notion of race is thus ultimately responsible for the correlations adduced to “defend” the current practices of stereotyping; the old errors have unjustly generated conditions that now differentially affect people with different phenotypes, and racial stereotyping is likely to maintain the difference, enabling future generations of stereotypers to mount the same defense.

To abandon the recording of data in terms of racial categories would undermine an ability to support stereotyping by appeal to evidence of correlations, but it would probably not terminate the *beliefs* that prompt the application of stereotypes. Folk generalizations are likely to live on, and even to be reinforced by resentment of the decision not to collect data couched in racial categories. The eliminativist thought that the damage done by current employment of the concept of race can be undone by jettisoning the concept is surely too simple. Conceptual reform is no substitute for the serious work of ameliorating social conditions, and it is an empirical issue how much good conceptual reform alone can do.

Moreover, we may look at the harms and injustices caused by past use of racial concepts somewhat differently, inquiring whether retention of some, appropriately sanitized, notion of race is needed to correct them. Might sociological research not require a concept of race to identify the damage that has been done by various forms of racial discrimination? Perhaps repairing that damage may require policies of compensation, explicitly crafted in racial categories: think of programs of affirmative action. Even more importantly, the political struggle for remedying the injustices of the past may turn on developing racial concepts that foster forms of solidarity among those who now suffer from the effects of those injustices, as well as from the racism that is still perpetrated. Tommie Shelby has argued eloquently for redeploying a notion of race in these ways.<sup>31</sup>

Although the harm that accrues from the use of racial stereotypes surely outweighs the usefulness of deploying the notion of race to explore issues about race and ethnicity, not to mention the value of the concept of human race in biological inquiries, the pragmatic evaluation of the concept turns on a host of intricate questions for which it is hard to assemble empirical evidence.

In any event, however, scientific work in the past few years has added further complications.

31. Tommie Shelby, *We Who Are Dark* (Cambridge, Mass.: Harvard University Press, 2006). In his APA symposium presentation, Shelby gave a concise but forceful account of the uses of racial categories, along the lines I give here.

## VII

Although there are no distinctive *alleles* found in the relatively inbred lineages we might mark out as races, there is significant variation in the *frequencies* with which alleles occur in different human groups.<sup>32</sup> It is well known, for example, that the allele for Tay-Sachs occurs with greatest frequency among Ashkenazi Jews (as well as in some French Canadian populations), and that the mutations associated with cystic fibrosis are most common among people whose ancestors hail from northwestern Europe. Recently, however, it has become evident that the alleles that affect receptivity to bone marrow transplants are distributed in ways that reflect some traditional racial divisions; in particular, because of the variance in African American populations, it is important not only to use a racial category in classifying potential donors but also to appeal to people, identified by race, to donate. For those involved in trying to help people who urgently need a bone marrow transplant, the eliminativist proposal appears dangerously misguided. As Hacking rightly notes, they view the continued employment of racial categories as a matter of life and death.<sup>33</sup>

This is a striking instance of what we can expect to be a general phenomenon, one likely to become ever more evident. Because of the geographical isolation of some populations for long stretches of our human past, there are differences in the frequencies with which different alleles occur within those populations. As genomic studies reveal the variations in DNA sequences, and in the frequencies with which particular sequences occur in different relatively isolated populations, and as the medical significance of certain variants becomes known, it is to be expected that differential diagnosis can be facilitated by data on the rates at which particular sequences are found in different races. In many instances, the statistical information might be superseded by identifying

32. To acknowledge this is *not* to embrace essentialism. I note this because discussions with philosophers who have made outstanding contributions to our understanding of racial concepts have convinced me that there are serious misunderstandings of any proposals that recognize this kind of genetic variation—in some instances, I have even found an inability to hear the words that present recent genomic findings. For those who have difficulty, Hacking's lucid explanation in "Why Race Still Matters" ought to be required reading.

33. Ian Hacking, "Why Race Still Matters," *Daedalus* (2005): 102–16, at p. 108.

the patient's sequences at the pertinent loci, but when treatment is needed immediately, or when the recommended approach depends on information about others (as in the example of transplants), the partitioning of the statistics according to race may be crucial.

In fact, there is an important difference between the issues that arise in tailoring prescriptions to patients who have different genotypes and recruiting donors for transplant programs. Suppose a doctor must prescribe for a patient. *Assume* it is already known that the disease for which relief is sought is associated with two different genotypes, one that is very common in one racial group (a lineage that has been relatively isolated for a significant chunk of human history) and another that is very common in a different racial group. There are two treatments, one good for cases that are associated with the first genotype and the other good for cases associated with the second genotype. Initially, knowing the person's race would seem valuable in deciding which treatment to prescribe. Yet a moment's reflection reveals a better approach: for the patient is *at hand* and (insurers permitting) can be tested to determine which genotype is present. Prescription can go better by moving beyond the racial classification to finer-grained sorting by genotypes.

In recruiting transplant donors, however, the people an agency wishes to attract are *not at hand*. Instead, one must appeal to markers that raise the probability of finding matches for members of particular groups, markers that are available to the intended audience. So, registry websites contain phrases like the following:

Because tissue type is inherited, patients are most likely to match someone of their same race and ethnicity. There is a special need to recruit more donors who identify themselves as: Black or African American, American Indian or Alaskan Native, Asian, Hawaiian or Other Pacific Islander, Hispanic or Latino.<sup>34</sup>

We are currently trying to recruit more African, African Caribbean and Mixed Race potential donors in our efforts to offer patients the CHANCE OF LIFE.<sup>35</sup>

34. <http://www.katiasolomonfoundation.org/CordandMarrowDonation.html>. I conjecture that the appeal statement uses both terms 'race' and 'ethnicity' not because of any confusion about the relation of the cultural concept of ethnicity to genotypes, but because the foundation simply wants to maximize the number of responses.

35. <http://www.aclt.org/details/d.aspx/16>. Capitals in original.

Because African, African American, and African Caribbean populations are genetically diverse for the pertinent loci, the chances of finding a match are smaller than those for many other groups. This intensifies the need to recruit a large number of potential donors. If everyone had been tested, and knew *and remembered* his or her genotype at those loci, then the appeal could be couched in terms of requests for those with particular allelic combinations to volunteer. But it is utterly unrealistic to hope that we can replace self-identification by race with anything like that. From a practical point of view, the use of the racial category is necessary.

Moreover, recruitment by race may have a special force. When the racial groups involved have a history of marginalization (or worse), members of those groups may see themselves as joining together to tackle a problem that arises from their genealogical relationship. Here, in a medical context, racial solidarity may play a valuable role.<sup>36</sup>

I have starkly distinguished recruitment of donors from prescription of medicines by taking advantage of convenient idealization: I assumed that the causes of differential effectiveness were genetic, and that these were already known. In many instances, however, doctors are aware of an effect that correlates with racial classifications, but are ignorant of the causes. They see the decision to prescribe differently for members of different races, where races here are demarcated in everyday ways and are available to patients in their own self-identification, as an interim measure, valuable in a condition of imperfect information. Perhaps at some future time the causal factors responsible for differential reactions to alternative drugs will be understood, perhaps they will be genetic, and the physician's decision can be taken in the more fine-grained way I imagined. In arriving at that knowledge, however, epidemiological data will be required, and the crude correlation may prove helpful in arriving at the causal explanation. So the use of racial categories here is not just a stopgap measure to treat patients, but part of an investigative strategy for doing better.

Recent debates about "race-based medicine" (see, for example, the BiDil<sup>®</sup> controversy) bring out two further complications. First, skeptics

36. Here, evidently, I echo the arguments that Shelby has constructed for the *social* uses of solidarity.

about the role of race in medicine argue that the racial classifications that appear in the alleged correlations are unlikely to be good indicators of genetic differences. Their caution is justifiable, given that the racial groups across which differential responses are supposed to occur—"African Americans" and "Whites"—are unlikely to accord very closely with clusters demarcated by genetic analysis; these are just the sorts of cases in which social criteria are likely to distort class membership and prevent self-identified ancestry from serving as a good proxy for genetically distinctive populations. So, although critics may concede the point that relying on a crude correlation is the best available strategy for treating patients in the here and now, they are skeptical of the value of racial categories as vehicles for refining our ignorance about the causal factors responsible.

The second complication arises as a response to this line of criticism. It is quite possible that *environmental* differences may affect aspects of the human phenotype that determine the efficacy of a drug, and that those environmental differences may themselves be caused by the social practice of assigning people to different races. It is very clear that African Americans (the people inclined to designate themselves in this way) are markedly less well served by U.S. medicine than other major groups. Quite plausibly, part of that difference results from their having been identified in this way from the time of their birth, through chains of causation that give rise to unhealthy living conditions, limited prospects, and alienation from institutions that tend to promote the health of more fortunate people. To the extent to which hypotheses of this sort are correct, self-identified racial membership is an important causal variable, not because it serves as a proxy for *genes*, but because it is a reliable indicator of ways in which racial discrimination survives in environmental conditions that decrease health.

"Race-based medicine," conceived as the reiteration of the familiar theme that different races have different alleles and thus different propensities for disease, is rightly criticized. Understood differently, it may involve an appreciation of the ways in which social discrimination acts through the physical environment to diminish health. More than a temporary measure, a way of coping with sick people in a situation of relative ignorance, it can be viewed as a commitment to understanding the causes of differential morbidity and mortality, and even as a method of creating trust among people who have been neglected by and who have

become alienated from the institutions of U.S. medicine, a tacit promise that, at last, their plight is being taken seriously.<sup>37</sup>

## VIII

Let us take stock. In rejecting a realist approach to natural kinds, I have suggested that the legitimacy of notions of race has to depend upon the suitability of those notions to our purposes. At first sight, the damage that racial concepts have caused, and continue to cause, makes it look as though we come to eliminativism by a nonstandard route. I have been trying to suggest, however, that matters are far more complicated than they initially appear. Not only are there uses that pull in different directions, but there are also serious, unresolved empirical issues, I believe, about what conceptual reform might accomplish.

How, then, to go on from here? My answer is in the spirit of the pragmatism I have been espousing, and also of the plea for a more democratic science that I have tried to defend in recent years.<sup>38</sup> The phrase “the suitability of the notion of race to our purposes” is radically incomplete as a characterization of any test to which racial concepts might be subjected. For, although one can pick particular contexts and uses as they seem salient—as I have done by pointing to questions about human migrations, about race and ethnicity, about racial stereotypes, and about medical uses of racial categories—these are a poor substitute for a systematic survey of the variety of uses to which racial concepts might be put, an investigation of their effects, and an exploration of what might be achieved by eliminating the concepts. There is much here that is unknown, unknown not simply to academic philosophers but to anyone. A responsible verdict on the notion of race must await the elaboration of information about all the uses, their consequences, and the prospects of doing better without racial categories.

Although that is necessary, it is hardly sufficient. For the fact that notions of race have surfaced both in scientific inquiry and in socially

37. This point parallels Shelby’s case that racial notions may be needed to understand, and correct, patterns of past prejudice. In the medical context, it has been made very eloquently by Keith Ferdinand.

38. Philip Kitcher, *Science, Truth, and Democracy*; also “What Kinds of Science Should Be Done?” in *Living with the Genie*, ed. Alan Lightman, Dan Sarewitz, and Christina Dresser (Washington, D.C.: Island Press, 2003), pp. 201–24.



consequential debates means that the continued viability of these notions should not be decided by any group of academic researchers. As so often, the glib first-person plural, "our purposes," disguises the heterogeneity of perspectives that different groups of people might bring, even when presented with the ideal elaboration of information. If there are any groups whose voices should be heard in rendering the verdict the pragmatic test demands, then they should surely be those who have suffered most from the past employment of the categories. This strikes me as a clear case in which the declaration of independence of scientific inquiry rings hollow, an exemplar of the need for that involvement of the judgments of informed outsiders for which I have argued elsewhere. 'Race' is a viable concept just in case it would be hailed as such by a set of ideal deliberators, inclusive with respect to variant human perspectives, fully informed by the systematic elaboration I have seen as a necessary part of the pragmatic test, and mutually engaged. At present, we can only speculate about how that discussion would come out.

Are races natural kinds? I believe not, because I am dubious of the notion of natural kind. There are biological phenomena that can be connected with infraspecific distinctions biologists find it useful to make in nonhuman cases, and, more to the point, that are valuable for research on human historical geography. That does not clinch the case for making infraspecific divisions within our own species. There is a genuine issue about whether the category of race is worth retaining. I hope to have said enough to show that settling that issue is harder than it might appear, that there are considerations pulling in different directions. Beyond that, I have tried to argue that the pragmatic test of racial concepts will depend upon systematic explorations, and the amassing of information nobody yet has, and, most importantly, that it should involve people who have usually been left out of the discussion.