

There's still no good reason to believe black-white IQ differences are due to genes

Our response to criticisms.

By Eric Turkheimer, Kathryn Paige Harden, and Richard E. Nisbett | Updated Jun 17, 2017, 10:45am EDT



US Army recruits taking an intelligence test at Fort Lee, in Virginia, 1917. | Time Life Pictures / Getty

THE BIG IDEA

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It is difficult to decide where to begin among the commentary that followed our recent discussion of **Sam Harris's interview** of Charles Murray on Harris's *Waking Up* podcast. In **the piece**, we argued that Murray was wrong in 1994 in his reading of the evidence for a genetic basis for the black-white IQ difference — and that he is wrong today. We argued that it was misleading, even irresponsible, for Harris to treat Murray as if he were someone who merely passes along scientific facts — facts so sound that they can only be doubted by liberals in the grip of “a politically correct moral panic,” in Harris's words.

All three of us are academic psychologists who have studied human intelligence, and it is our contention that Murray's views do not represent the consensus in our field.

We start by noting that we accepted as facts many claims that are controversial in the academy, if not in psychology — that IQ exists; that it predicts many life outcomes; that there is a gap between black IQ scores and white IQ scores; that IQ is at least partly heritable (as is almost every human trait). We rejected the conclusion that Murray and Harris say is virtually inescapable: that it follows that the black-white difference in IQ must be partly genetic.

Given the response to our first article, we thought it would be useful to clarify the precise boundaries of the dispute, as well as respond to some technical points critics raised.

The central issue at stake is whether the black-white IQ gap is partially genetically determined. We believe there is currently no strong evidence to support this conclusion, whereas Murray presents it as a near certainty, and Harris endorses Murray's position.

To be fair to our critics, it can be a little hard at first to pin down Harris and Murray's position on this point. They both offer broad caveats, like this one, from Harris:

The fact that a trait is genetically transmitted in individuals does not mean that all the differences between groups or really even any of the differences between groups in that trait are also genetic in origin. [43:25 in the podcast]

But the example he then gives is malnourishment producing differences in height. When speaking about IQ, Murray's position eventually becomes clear: Genes play a role in the average difference between the IQs of blacks and whites, and public policy is not going to be able to do much to change levels of cognitive skills.

Referring to the claims he made in *The Bell Curve*, Murray paraphrases the argument that he and co-author Richard J. Herrnstein made, which Murray says created much of the subsequent controversy:

Our crime in the book was to have a single solitary paragraph that said ... if we've convinced you that either the environmental or the genetic explanation has won out, to the exclusion of the other, we haven't done a good enough job of presenting the evidence for one side or the other. It seems to us highly likely that both genes and the environment have something to do with racial differences. And we went no further than that. [59:07]

Harris endorses Murray's contention about partial heritability of the group differences. He says, for example:

This is just straight biology. And because different racial groups differ genetically, to any degree, and because most of what we care about in ourselves — intelligence included — ... also has some genetic underpinnings — for many of these traits we're talking about something like 50 percent — it would be very, very surprising if everything we cared about was tuned to the exact same population average in every racial group. There's just virtually no way that's going to be true. So based purely on biological consideration, we should expect that for any variable, there will be differences in the average, its average level, across racial groups that differ genetically to some degree. [55:12]

Even when accepting an environmental contribution to black-white differences, Harris still implicitly endorses the idea that group differences are due to genes:

But again, what we should come back to here is that genes are almost certainly only just part of the story and there should be very likely an environmental contribution here. [58:19]

With statements like these, Harris executes the same move Herrnstein and Murray made in *The Bell Curve*: They acknowledge all the reasons why the heritability of intelligence doesn't necessarily mean that group differences are due to genes. They then proceed to draw their conclusions as if those reasons don't really matter.

The other side of Murray's repeated assertions that the black-white IQ gap is partially genetic is his claim that there is ultimately very little that can be done about average levels of IQ; even if the environment contributes to IQ, any inequalities are basically intractable. Murray again:

There is this notion that if traits are genetically determined, that's bad, and if traits are environmentally determined, that's good, because we can do something about them if they are environmental. And if there is one lesson that we have learned from the last 70 years of social policy, it is that changing environments in ways that produce measurable results is really, really hard and we actually don't know how to do it, no matter how much money we spend. [38:34]

At another point, Murray and Harris are discussing how genetic tendencies can lead children to reshape their environments, and Murray cautions:

Does that mean that if only you can jack up artificially the environment you're going to make much difference in the child's IQ? And the answer to that is: Not long term. [37:48]

Does adoption count as “jack[ing] up artificially the environment”? In our original post, we pointed out that adoption from a poor home to a well-off home is associated with a 12- to

18-point gain in IQ. Other studies have come up with slightly lower figures, but the general direction of the finding is beyond dispute.

Similarly, we argued in our initial piece that Murray was not forced to grapple sufficiently with the implications of the Flynn Effect — that is, the remarkable increase in average IQ over generational time: 18 points in the US between 1948 and 2002. These very large increases demonstrate massive, population-level, environmentally caused changes in IQ. Like adoption, the Flynn Effect remains a powerful rebuttal of the idea that IQ cannot be budged by environmental factors.

Harris brought up the Flynn Effect, and even briefly described it as a challenge, until Murray produced a vague citation to a paper by **Wicherts et al. (2004)** and Harris gave up. Murray noted that the paper in question is quite complex, and he is right. Wicherts's analysis shows that across different IQ subtests, the pattern of larger and smaller changes produced by the Flynn Effect is different from the pattern of differences between blacks and whites.

Wicherts's finding has some interesting technical implications, but the important question remains whether it discredits the Flynn Effect as a challenge to the notion of inborn group differences in cognitive ability. We don't think it does. The Flynn Effect demonstrates massive, population-level environmental changes in average IQ scores; the exact nature of the structure of these changes is an interesting question, but it is a side issue in this context.

So here, then, is where we differ with Murray, and, as we understand it, with Harris: 1) we think there is currently no good reason to believe that the black-white difference in average IQ is due to genetic differences between racial groups; and 2) rather than thinking there is no way to influence intelligence by improving the environment, we think there *is*, in fact, good reason to believe that improving children's environments will improve their cognitive skills.

With the terms of the debate established, we now move on to some more technical questions raised about the topic. Nisbett is primarily responsible for the first section, Harden for the second, and Turkheimer for the third, although we are all in agreement on the main points.

Richard Nisbett: who is cherry-picking?

Charles Murray did not write a response to our piece, but he did endorse, on Twitter, the work of several critics. He **suggests** he might have written something along the lines of **this blog post**, which attacked the article on several points. I respond to several of those points here:

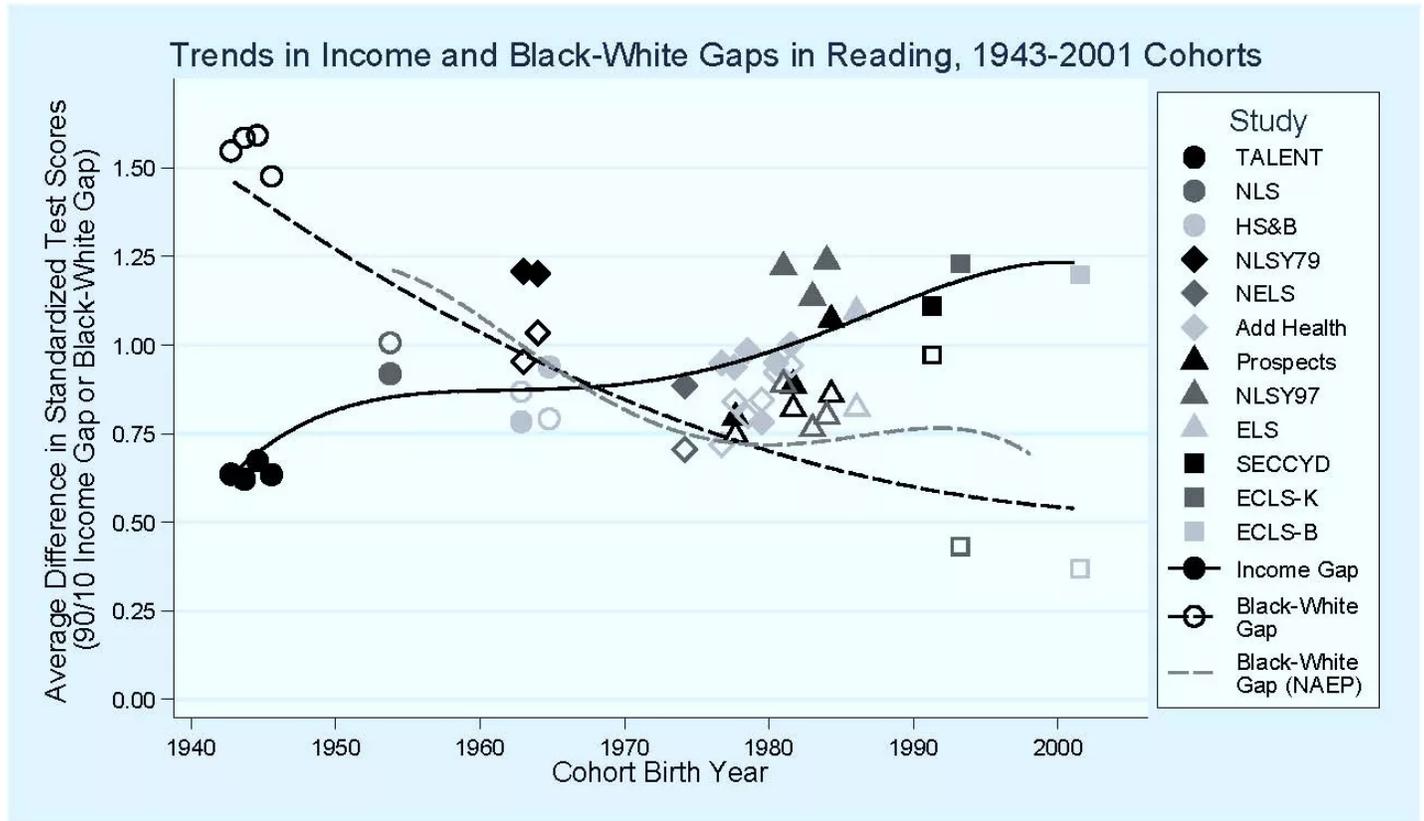
Do most experts think genes make a substantial contribution to the black-white difference in intelligence? There have been several surveys of expert opinion over the years. Perhaps the first was described in a 1988 book by **Snyderman and Rothman**. The most recent was described in a **2013 blog post** about a conference presentation. The survey described in that post has resulted in two **published articles**, neither of which presents data on opinions regarding the black-white difference. The studies do, however, report that only about 5 percent of people who were invited to participate responded to any one set of items. Given this very low response rate, along with the potential for bias in which scientists were invited in the first place, we doubt that these results are an accurate representation of the field.

Still, in both the Snyderman and Rothman book and in the more recent survey, more than half of respondents selected one of two response categories that included zero (one option was “0 percent of [black-white] differences due to genes” and the other was “0-40 percent of differences due to genes”). Much more important, however, is that respondents were not allowed to endorse what in my view is the only reasonable response: It is not possible to give a meaningful estimate of the percentage.

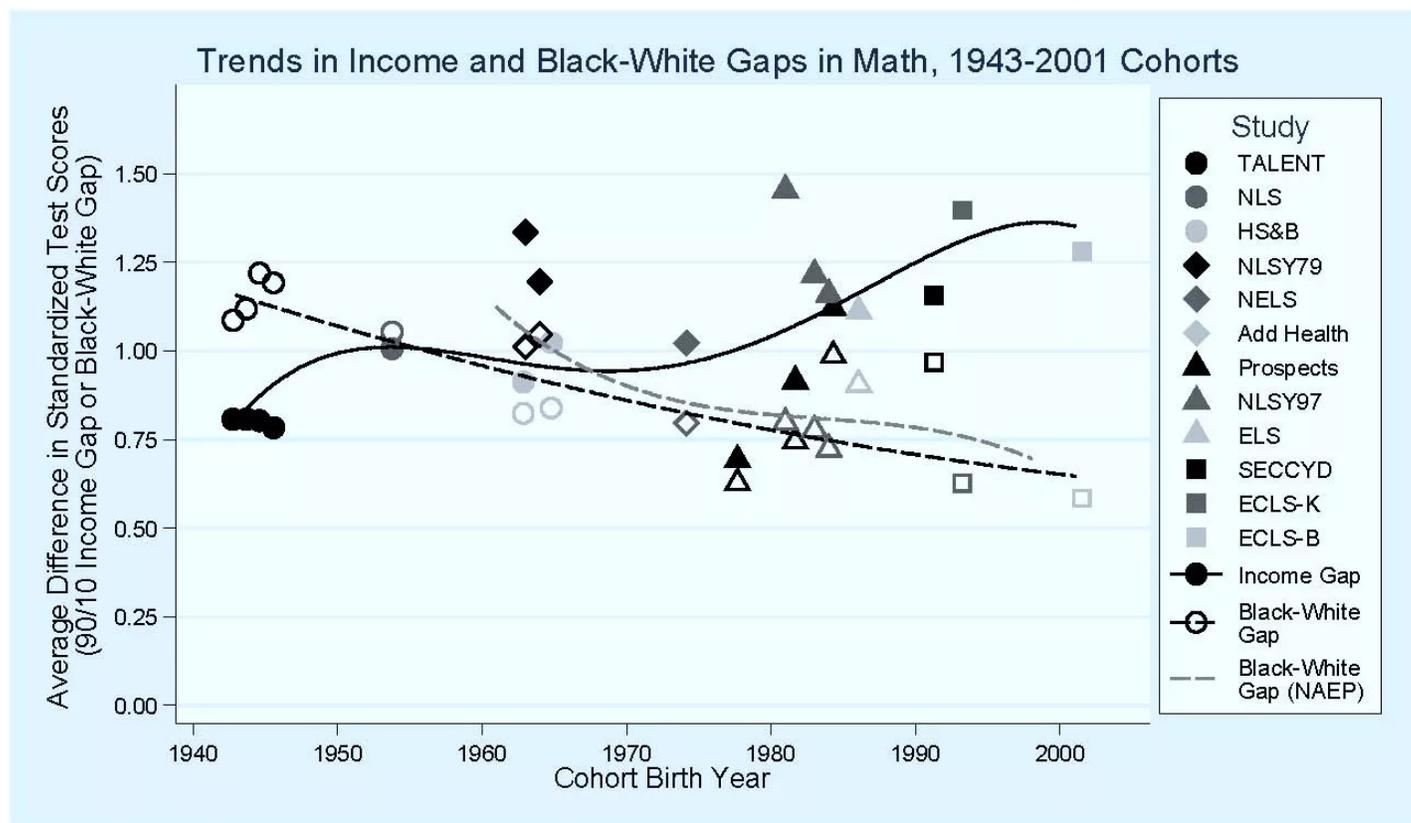
Has the black-white gap in test scores narrowed in the past 25 years? Below are the results of a very large number of psychometric tests of academic achievement assembled by sociologist **Sean Reardon**. Along the X-axis is the birth year of the cohort. On the Y-axis are the black-white gap and the gap between children of families at the 90th percentile in income and families at the 10th percentile of income, in standard deviation terms (one standard deviation of IQ is equal to 15 points).

The first graph gives the results for reading, the second for math. For reading, the black-white gap for the 1943 cohort was approximately double the gap associated with family income. The black-white gap then shrank from substantially more than a standard deviation for the 1943 cohort to roughly a standard deviation for the 1963 cohort to slightly more than half a standard deviation for the 2003 cohort. For math, the black-white gap went from around slightly more than a standard deviation to slightly more than half a standard deviation.

IQ is highly correlated with these measures of academic achievement, so it is almost surely the case that the black-white IQ gap has been very substantially reduced. (The race gap in IQ itself has not to our knowledge been investigated since 2006, when **Dickens and Flynn** found that it was around 9.5 points, close to what is suggested by Reardon's achievement data. In the podcast, Murray asserts that the gap is on the order of 15 points.)



Russell Sage Foundation



Russell Sage Foundation

It should be noted that the data for 17-year-olds is comparable to the data overall. (The blog post Murray endorses suggests that the test scores of 17-year-olds reflect genetic influence more than the test scores of 10-year-olds.) The reading gap for 17-year-olds was reduced by 9 points between 1975 and 2012; the math gap was reduced by 4.5 points.

It is true that the average SAT score of blacks has not changed over the past 20 years. However, black adolescents are much more likely to take the SAT today than in the 1990s: The number of black people in the US increased by 4 percent from 1996 to 2015, while the number of black SAT takers doubled, far more than the 17 percent increase in the number of white SAT takers. If the average black IQ is increasing, but the black adolescents from the lower portion of the IQ distribution are increasingly likely to take the test, this will result in a static mean score.

Are there significant limitations to studies on the effect of adoption on IQ? In our original post, we pointed out that adoption from a poor home to a well-off home is associated with a 12- to 18-point gain in IQ. This point was challenged from several angles.

First, even when adoption produces substantial gains in the average IQs of adopted children, the magnitudes of the individual gains are better predicted by the IQs of the

children's biological parents than by the relative quality of the adoptive environment. This is true but irrelevant: It is merely evidence that IQ is partly heritable, which no one disputes. That effect (one more time) has no implications for understanding group differences. (The authoritative reference on this phenomenon, by the way, is **Turkheimer, 1991**.)

What we care about is how high their IQs are, not whether the correlation between their IQs and their biological parents is higher or lower than the correlation with the IQs of the adoptive parents. The IQs of those adopted children are substantially higher than they would have been if they had been raised by their biological parents.

Second, a **previous study co-authored by Turkheimer** found an adoption effect of only about 4.4 points. However, the magnitude of the increase afforded by adoption depends on the difference between the biological and adoptive homes. This particular adoption study was conducted in Sweden, using children adopted from homes of slightly less than average economic status into homes that were slightly higher than average. Krona for krona, the IQ gains were just about the same. Again, adoption into improved environments, even in a country with a strong social safety net and relatively slight economic differences between the social classes, increases IQ.

Can educational programs increase IQ? In our original post, we stated that the best early childhood education programs greatly **increase educational attainment and labor force participation**. A critic alleged that "this was a strange straw man," because would Murray disagree that the best educational programs could raise "social capital"? But throughout the podcast, Murray and Harris are quite skeptical about the possibility that any policy or intervention could be successful. Their remarks begin as a discussion about IQ specifically, but drift into what sounds like pessimism about social policy generally. Murray again:

And if there is one lesson we've learned from the last 70 years of social policy, it is that changing environments in ways that produce measurable results is really, really hard. And we actually don't know how to do it, no matter how much money we spend. [Harris readily agrees:] Right. [38:49]

I do not deny the problem of IQ gain fade-out, or the difficulty of designing successful social policies. Indeed, we commented in our original post that IQ gains from programs "tend to regress once the program ends *and environmental disadvantages reassert themselves*" [emphasis added]. But fade-out on IQ gains does not justify making sweeping statements that we are largely helpless to remedy social inequalities — a claim that Murray has made, in different forms, throughout his career.

Work by the Nobel Prize-winning economist James Heckman has demonstrated that the best early childhood interventions have a benefit-cost ratio of somewhere between 3:1 and 9:1 by virtue of their effect on such things as lifelong earnings, health costs, crime, and dependence on welfare.

Is the heritability of intelligence “more or less the same” across social classes? In our original post, we wrote, “The heritability of intelligence, although never zero, is markedly lower among American children raised in poverty,” and linked to a **2003 study by Turkheimer and colleagues**. That finding suggests that low-income children have fewer opportunities for their genetic potential to flourish.

Critics have noted that in a more recent meta-analysis by **Tucker-Drob and Bates**, the effect size estimated by Turkheimer et al. (2003) was the largest of the studies that tested the interaction. We are quite familiar with that paper, as Tucker-Drob is Harden's spouse. However, the same meta-analysis unequivocally demonstrated that the heritability of intelligence is lower among poor children raised in the United States (estimated to be ~26 percent) than among children from wealthy families (estimated to be ~61 percent).

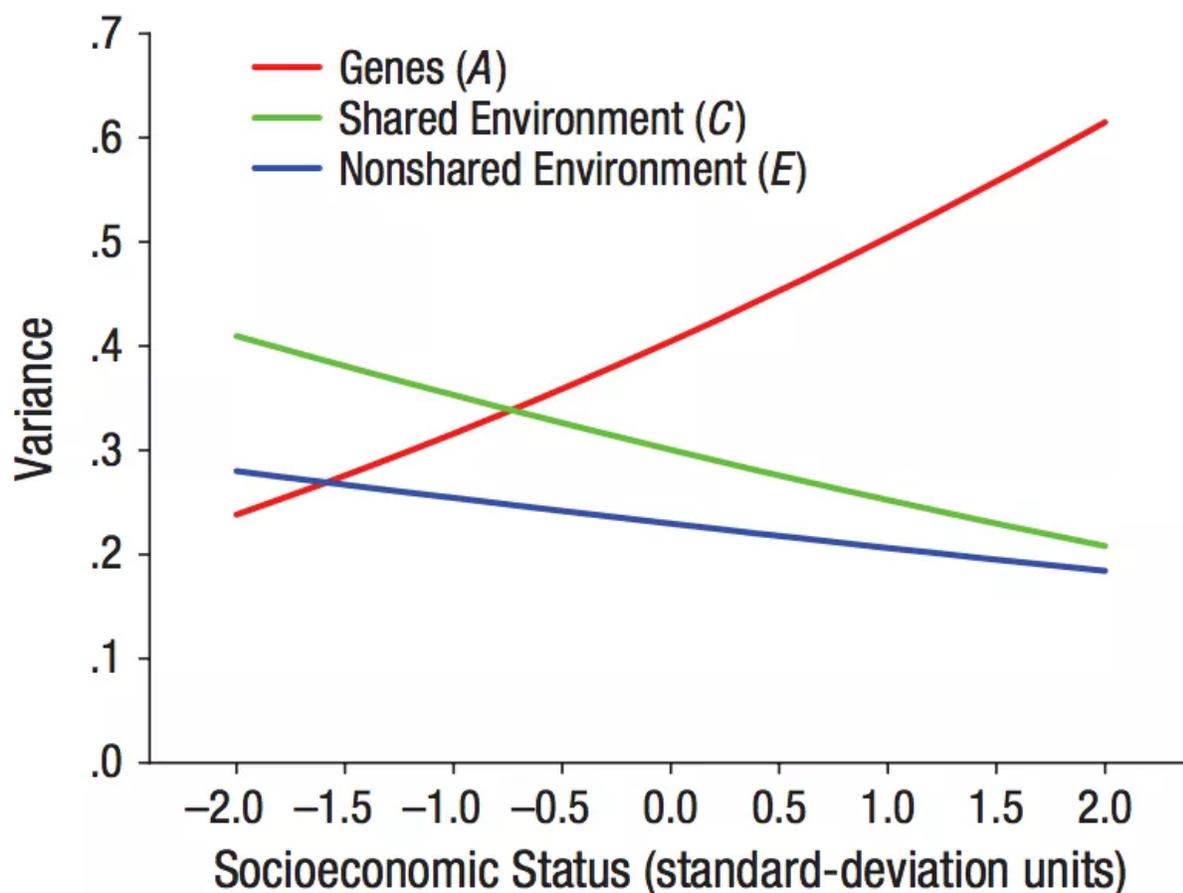


Fig. 1. Variance in cognitive-test performance for the U.S. sample accounted for by genetic and environmental factors, graphed as a function of socioeconomic status (SES). Cognitive test scores were standardized to a z scale within each data set prior to model fitting. This plot is very close to (but not identical with) a plot in which the y -axis represents the instantaneous proportion of variance for each level of SES.

From Tucker-Drob and Bates, 2016, *Psychological Science*

Furthermore, the meta-analysis tested whether Turkheimer et al. (2003) was a statistical outlier, and it was not; it tested whether the average reduction in heritability was still significant when Turkheimer et al. (2003) is left out, and it was; it made the same test leaving out every study Turkheimer had anything to do with, and the effect was still significant.

So despite the misleading impression given by the critics, the meta-analysis was a confirmation of the reduction in heritability among poor Americans. This is important,

because it undermines the hereditarian argument that twin studies show family environment doesn't matter for IQ: For poor children in the US, in particular, the family environment seems to matter quite a bit.

Paige Harden: race and ancestry are not synonymous

Our piece did not contain much information about the relationship between genetic ancestry and race, but the brief paragraph that was included motivated objections, most prominently from the author Razib Khan **on his blog, Gene Expression**.

To back up, in the podcast, Murray states that he has changed none of his views on race and IQ since writing *The Bell Curve*. In fact, he says (emphasis added):

Now that the genome has been sequenced and so much has been learned since it has been sequenced, the whole discussion of ethnicity-slash-race is being conducted at a much higher level of sophistication. ... Now, the ability of the geneticists to simply look at variation over a million SNPs [single nucleotide polymorphisms] across populations and do really fascinating cluster analysis. ... **The word “populations” is what the geneticists like to use now instead of race, and I don't blame them, and I'm happy talking about populations, too.** That's just being done at a huge level that we never considered. In *The Bell Curve*, we simply said, if they call themselves black or Latino or white, we're going to believe them, and they are going to be our samples. [56:28]

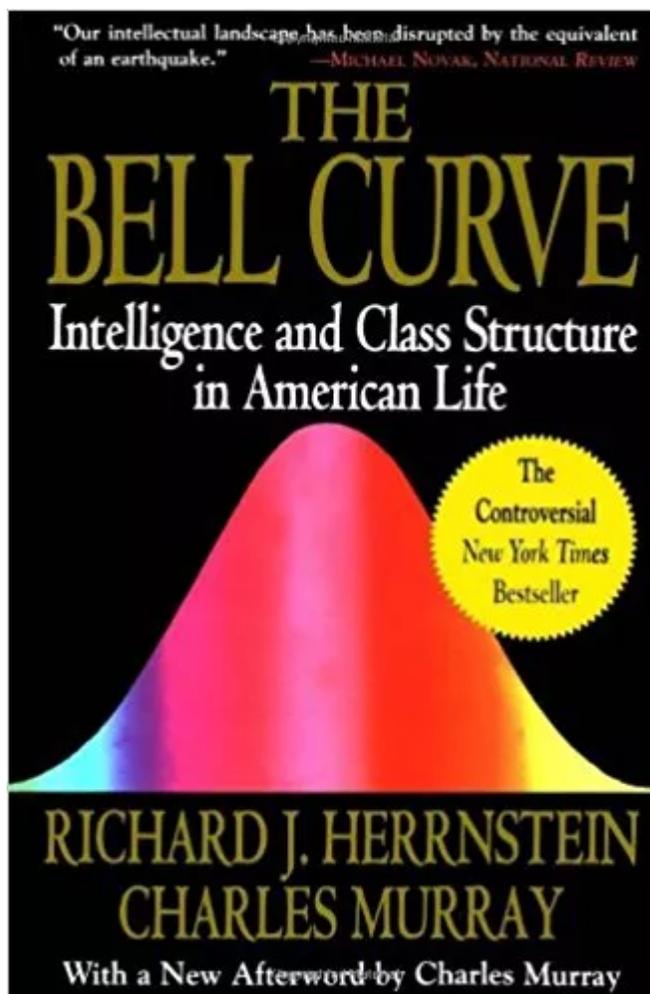
This description inappropriately implies that “populations” defined from genetic analyses and “race” as defined by the US Census categories used in *The Bell Curve* are essentially the same thing. Elsewhere, Murray speaks of genetic ancestry differences between races as “signal”; the “blurriness” of race is “noise” that “contaminates” the search for genetically based group differences. [57:55]

In response, we wrote: “Murray talks about advances in population genetics as if they have validated modern racial groups. In reality, the racial groups used in the US — white, black, Hispanic, Asian — are such a poor proxy for underlying genetic ancestry that no self-respecting statistical geneticist would undertake a study based only on self-identified racial category as a proxy for genetic ancestry measured from DNA.”

In his critique, Khan responded that “the Census categories are pretty bad and not optimal (e.g., the ‘Asian American’ category pools South with East & Southeast Asians, and that has caused issues in biomedical research in the past). But the claim is false.”

This criticism is confusing, because our claim is essentially the one Khan makes: “Census categories [involving race] are pretty bad and not optimal.” At the same time, our observation — that statistical geneticists could not publish a study that only controlled for self-identified race rather than genetic ancestry as measured from DNA — is certainly true. Controlling for multiple dimensions of ancestry derived from genome-wide genotyping is **standard practice** in genetic research.

I suspect that Khan’s reflexive criticism comes from a place of exasperation with the idea, still in circulation among some social scientists, that race is “just” a social construct or that the racial categories used in the US today are entirely meaningless. I am sympathetic to this objection to pure social constructivism, and we said in our post that lay notions of race are not wrong or useless. Self-reported racial categories, coarse as they are, also generally reflect underlying differences in genetic ancestry. For instance, **in a 2015 paper by Neil Risch et al.**, which Khan cites extensively, more than 99 percent of people who reported being African American had some proportion of African ancestry.



But even this close correspondence between African ancestry, as measured from DNA, and self-reported race does not undermine our claim — race is not the same as ancestry. For one, there can be a range of ancestral backgrounds within any one self-identified racial group. If someone has any African ancestry, you can probably tell with a reasonable degree of confidence that he or she will identify as black, but the reverse is harder: If you know someone is black, you do not know what percentage African versus European versus American ancestry he or she has.

Ancestry also allows for more continuous and granular distinctions than our relatively crude categories of race. The ancestry components that geneticists are most commonly including in their analyses are making fine-grained distinctions between people who would all be lumped together as “white” in the US today.

Finally, we ignore some ancestral differences and focus on others when we categorize people into races. As a historical example, consider Carl Brigham’s 1923 book, *A Study of American Intelligence*. In a section titled “**The Race Hypothesis**,” Brigham attempts to classify people from different European countries in terms of their “Nordic,” “Alpine,” and “Mediterranean” blood: The Italians are estimated to be 70 percent Mediterranean; the English as 80 percent Nordic.

The effort to divide Europe’s inhabitants by “blood” is crude, but in one respect, Brigham wasn’t wrong — with modern technology, **you could certainly differentiate a person with English ancestry from a person with Italian ancestry**. But sometime in the past century, we stopped conceptualizing the differences between the English and the Italians in terms of race. We elevate to the status of “race” the distinctions that are our current political and cultural preoccupations, while eliding others.

Ironically, the genetic differences between racial groups are a big part of why it’s methodologically difficult to resolve the persistent questions about the origins of group differences to anyone’s satisfaction. **Populations and sub-populations** don’t just differ in the frequency of certain genetic variants; they also differ in which variants are present at all, and in the pattern of correlations between genetic variants. Currently, everything we know about the specific genetic variants associated with intelligence has been discovered in people of European ancestry, but because of these genetic differences between populations, applying genetic discoveries gleaned from one population to understand another turns out to be **very difficult**.

Eric Turkheimer: reasonable and unreasonable conclusions about group differences

A widely expressed criticism of our piece is that we misrepresented Murray's (and Harris's) conclusions about the degree to which IQ differences among racial groups are partly based in genetic differences. As we've made clear, there is no question on this point: Both Murray and Harris conclude that racial differences in IQ are at least partly genetic in origin, and base this conclusion on the heritability of IQ scores within populations. As Harris put it, "This is just straight biology."

As we noted in our original post, Murray uses a rhetorical move to make a genetic account of the IQ gap seem more reasonable: All Harris and Murray are saying is that the difference is probably partly genetic and partly environmental, whereas their opponents insist that it is not genetic at all. Murray says:

There is an asymmetry between saying probably genes have some involvement and the assertion that it's *entirely* environmental. And that's the assertion that is being made [by critics]. If you are going to be upset at *The Bell Curve*, you are obligated to defend the proposition that the black-white difference in IQ scores is 100 percent environmental, and that's a very tough measure. [59:41]

Unfortunately, Murray's proposal that the IQ gap is the result of a little genetics and a little environment does not offer a way out of the **scientific and ethical dilemma faced by the (alleged) science of race and behavior**. Scientifically, there is no method that can apportion group differences in this way, no empirical analysis that might assign IQ differences between racial groups to one or another source, much less come up with a meaningful balance between the two.

There is not a single example of a group difference in any complex human behavioral trait that has been shown to be environmental or genetic, in any proportion, on the basis of scientific evidence. Ethically, in the absence of a valid scientific methodology, speculations about innate differences between the complex behavior of groups remain just that, inseparable from the legacy of unsupported views about race and behavior that are as old as human history. The scientific futility and dubious ethical status of the enterprise are two sides of the same coin.

To convince the reader that there is no scientifically valid or ethically defensible foundation for the project of assigning group differences in complex behavior to genetic and environmental causes, I have to move the discussion in an even more uncomfortable direction. Consider the assertion that Jews are more materialistic than non-Jews. (I am Jewish, I have used a version of this example **before**, and I am not accusing anyone

involved in this discussion of anti-Semitism. My point is to interrogate the scientific difference between assertions about blacks and assertions about Jews.)

One could try to avoid the question by hoping that materialism isn't a measurable trait like IQ, except that it is; or that materialism might not be heritable in individuals, except that it is nearly certain it would be if someone bothered to check; or perhaps that Jews aren't really a race, although they certainly differ ancestrally from non-Jews; or that one wouldn't actually find an average difference in materialism, but it seems perfectly plausible that one might. (In case anyone is interested, a biological theory of Jewish behavior, by the white nationalist psychologist Kevin MacDonald, **actually exists**.)

If you were persuaded by Murray and Harris's conclusion that the black-white IQ gap is partially genetic, but uncomfortable with the idea that the same kind of thinking might apply to the personality traits of Jews, I have one question: Why? Couldn't there just as easily be a science of whether Jews are genetically "tuned to" (Harris's phrase) different levels of materialism than gentiles?

On the other hand, if you no longer believe this old anti-Semitic trope, is it because some scientific study has been conducted showing that it is false? And if the problem is simply that we haven't run the studies, why shouldn't we? Materialism is an important trait in individuals, and plausibly could be an important difference between groups. (Certainly the history of the Jewish people attests to the fact that it has been considered important in groups!) But the horrific recent history of false hypotheses about innate Jewish behavior helps us see how scientifically empty and morally bankrupt such ideas really are.

If Murray and Harris want to make a science out of their intuitions about how different groups of people have been "tuned" to behave, they will need to come up with a coherent biological account of what exactly genetic "tuning" of behavior entails and how it might be assessed empirically. It is, I acknowledge, a deeply complex question, both philosophically and scientifically.

In fact, I will close by noting that not even the three of us are completely in agreement about it: I (Turkheimer) am convinced that the question is irredeemably unscientific; Nisbett accepts it as a legitimate scientific question, and thinks evidence points fairly strongly in the direction of the black-white gap being entirely environmental in origin; while Harden questions the quality of the existing evidence, but thinks more determinative data may be found in future genetic knowledge.

We agree on this, however: Murray and Harris's current endorsement of a genetic contribution to the black-white IQ gap is based on a weak brew of unexamined intuition and sketchy empirical evidence. In a free country and a free academy, scientists can speculate about whatever they want, but their speculations should not be mistaken for a scientific consensus or a legitimate basis for social policy.

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