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Abstract

I cite text clarifying the position of Herrnstein & Murray's (1994) "The Bell Curve" on heritability, present evidence that the independent effect of IQ on social and economic outcomes is stable across independent methods of controlling for family background, and propose that Reifman's (2000) target article assigns a curiously short time frame for assessing the outcome of the controversy over the book.

Commentary on: [Reifman, Alan \(2000\) Revisiting the Bell Curve, Psycholoquy: 11,#99 Bell Curve \(1\)](#)

Keywords: [IQ](#), [adoption studies](#), [behavior genetics](#), [bell curve](#), [crime](#), [education](#), [intelligence](#), [nature/nurture](#), [poverty](#), [twin studies](#), [uterine environment](#)

1. I will confine myself to two topics raised by the target article and a comment on its framework. The two topics respond to several of Reifman's (2000) more specific points.

I. THE HERITABILITY OF IQ.

2. An overarching confusion about Herrnstein & Murray's (1994) "The Bell Curve" (TBC), one that has led to many subsidiary confusions, concerns with importance of the heritability of IQ. The received wisdom about TBC in the mass media and in many technical discussions has been that its conclusions depend on a particular level of heritability of IQ, and that particular level is unusually high. That widespread view of the book is incorrect. The conclusions assume only "substantial"

heritability and depend primarily on observed relationships between IQ and a variety of social phenomena. Many of those relationships do not involve passage of IQ from generation to generation. Others do, but whether that relationship is the result of heritability in the technical sense (genetic) or in a loose sense (being passed from parents to children through any mechanism) is usually irrelevant.

3. The reputation of TBC is such that many readers will find this characterization hard to accept. I therefore ask the indulgence of presenting verbatim the significant passages that present our view of the magnitude of heritability and the relative importance of genes and environment.

4. TBC has two extended discussions of heritability. One is in Chapter 13 and (with the exception of a passage reprinted below) is concerned with the state of knowledge about a genetic role, if any, for observed ethnic differences in test scores. The other extended discussion of heritability occurs earlier in the book, in Chapter 4, pp. 105-108, and deals with magnitude of heritability. The concluding paragraphs of that initial discussion are reprinted below, deleting the footnotes:

The technical literature is filled with varying estimates of the heritability of IQ, owing to the varying models being used for estimation and to the varying sets of data. Some people seem eager to throw up their hands and declare that, "No one knows (or can know) how heritable IQ is." But that reaction is as unwarranted as it is hasty, if one is content, as we are, to accept a range of uncertainty about the heritability that specialists may find nerve-wracking. We are content, in other words, to say that the heritability of IQ falls somewhere within a broad range, and that, for purposes of our discussion, a value of .6 to .2 does no violence to any of the competent and responsible recent estimates. The range of .4 to .8 includes virtually all recent (since 1980) estimates competent, responsible, or otherwise.

Recent studies have uncovered other salient facts about the way IQ scores depend on genes. They have found, for example, that the more general the measure of intelligence-the closer it is to *g*-the higher the heritability. Also, the evidence seems to say that the heritability of IQ rises as one ages, all the way from early childhood to late adulthood. This means that the variation in IQ among, say, youths ages 18 to 22 is less dependent on genes than that among people ages 40 to 44. Most of the traditional estimates of heritability have been based on youngsters, which means that they are likely to underestimate the role of genes later in life. Finally, and most surprisingly, the evidence is growing that whatever variation is left over for the environment to explain (i.e., 40 percent of the total variation, if the heritability of IQ is taken to be .6), relatively little can be traced to the shared environments created by families. It is, rather, a set of environmental influences, mostly unknown at present, that are experienced by individuals as individuals. The fact that family members resemble each other in intelligence in adulthood as much as they do is very largely

explained by the genes they share, rather than the family environment they shared as children. These findings suggest deep roots indeed for the cognitive stratification of society.

5. We reiterate our position on heritability elsewhere in the book. Here are the relevant additional passages.

From Chapter 5, p. 130:

In discussions of intelligence, people obsess about nature versus nurture, thinking that it matters fundamentally whether a person with a low IQ at, say, age 15 came by that IQ through a deficient environment or by bad luck in the genetic draw. But it does not matter for the kinds of issues we consider in Part II. The AFQT test scores for the NLSY sample were obtained when the subjects were 15 to 23 years of age, and their IQ scores were already as deeply rooted a fact about them as their height.

From Chapter 13, p. 314:

Aren't genetic differences passed down through the generations, while environmental differences are not? Yes and no. Environmentally-caused characteristics are by definition not heritable in the narrow technical sense that they do not involve genetic transmission. But nongenetic characteristics can nonetheless run in families. For practical purposes, environments are heritable too. The child who grows up in a punishing environment and thereby is intellectually stunted takes that deficit to the parenting of his children. The learning environment he encountered and the learning environment he provides for his children tend to be similar. The correlation between parents and children is just that: a statistical tendency for these things to be passed down, despite society's attempts to change things, without any necessary genetic component. In trying to break these intergenerational links, even adoption at birth has its limits. Poor prenatal nutrition can stunt cognitive potential in ways that cannot be remedied after birth. Prenatal drug and alcohol abuse can stunt cognitive potential. These traits also run in families and communities and persist for generations, for reasons that have proved difficult to affect.

From Chapter 15, p. 342:

We will refer to this downward pressure as dysgenesis, borrowing a term from population biology. However, it is important once again not to be sidetracked by the role of genes versus the role of environment. Children resemble their parents in IQ, for whatever reason, and that immigrants and their descendants may not duplicate the distribution of America's resident cognitive ability distribution. If women with low scores are reproducing more rapidly than women with high scores, the distribution of

scores will, other things equal, decline, no matter whether the women with the low scores came by them through nature or nurture.

6. When one finds that others are widely misinterpreting one's text, it is natural to ask how the point could have been made more clearly. Were Herrnstein and I writing anew, I would favor expanding our discussion of the nonshared environment and linking it with findings about the increasing heritability of IQ with age. Even though everyone, including Herrnstein and I, believe that environment plays a substantial role in IQ, the net result of age and nonshared environment leads to findings of near-zero correlations between the IQs of adoptive siblings by their mid and late teens (Scarr & Weinberg 1978; Loehlin, Horn, and Willerman, 1989). Since all of TBC's discussion of the relationship of IQ to social and economic outcomes bears on behaviors that occur in the teenage years and beyond, perhaps elaborating on this combination of age and the role of the shared environment would have been helpful in underscoring why the narrow genetic understanding of heritability is less important than the de facto transmission of IQ from generation to generation. Perhaps not, however. The passages quoted above are not really ambiguous even as they stand.

7. To sum up: The scientific assertion of TBC regarding nature vs. nurture is that IQ is substantially heritable, between 40-80 percent, that the environmental component is dominated by the nonshared environment, and that environmental influences can become as hard-wired by adolescence as genetic heritage. None of these views was exceptional in the technical literature at the time we wrote them, nor are they now.

II. THE INDEPENDENT EFFECT OF IQ ON SOCIAL AND ECONOMIC OUTCOMES.

8. A large cluster of issues regarding TBC centers on the book's estimates of the independent effect of IQ on various social and economic outcomes after taking socioeconomic status into account, using an SES index comprised of parental education, occupation, and income entered as an independent variable in a regression analysis. Once again, an extended direct quotation from TBC may be helpful in understanding what TBC set out-and did not set out-to demonstrate. From the introduction to Part II, pp. 122-123:

In most of the chapters of Part II, we will be looking at a variety of social behaviors, ranging from crime to childbearing to unemployment to citizenship. In each instance, we will look first at the direct relationship of cognitive ability to that behavior. After observing a statistical connection, the next question to come to mind is, What else might be another source of the relationship?

In the case of IQ, the obvious answer is socioeconomic status. To what extent is this relationship really founded on the social background and economic resources that shaped the environment in which the person grew up—the parents' socioeconomic status (SES)—rather than intelligence? Our measure of SES is an index combining indicators of parental education, income, and occupational prestige (details may be found in Appendix 2). Our basic procedure has been to run regression analyses in which the independent variables include IQ and parental SES. The result is a statement of the form: "Here is the relationship of IQ to social behavior X after the effects of socioeconomic background have been extracted," or vice versa. Usually, this takes the analysis most of the distance it can sensibly be pushed. If the independent relationship of IQ to social behavior X is small, there is no point in looking further. If the role of IQ remains large independently of SES, then it is worth thinking about, for it may cast social behavior and public policy in a new light.

We do not have the choice of leaving the issue of causation at that, however. Because intelligence has been such a taboo explanation for social behavior, we assume that our conclusions will often be resisted, if not condemned. We can already hear critics saying, "If only they had added this other variable to the analysis, they would have seen that intelligence has nothing to do with X." A major part of our analysis has accordingly been to anticipate what other variables might be invoked, and seeing if they do in fact attenuate the relationship of IQ to any given social behavior. This was not a scattershot effort. For any given relationship, we asked ourselves if evidence, theory, or common sense suggests another major causal story. Sometimes it did. When looking at whether a new mother went on welfare, for example, it clearly was not enough to know the general socioeconomic background of the woman's parents. It was also essential to examine her own economic situation at the time she had the baby: whatever her IQ is, would she go on welfare if she had economic resources to draw on?

At this point, however, statistical analysis can become a bottomless pit. It is not uncommon in technical journals to read articles built around the estimated effects of a dozen or more independent variables. Sometimes the entire set of variables is loaded into a single regression equation. Sometimes, sets of equations are used, modeling even more complex relationships, in which all the variables can exert mutual effects on one another.

Why should we not press forward? Why not also ask if religious background has an effect on the decision to go on welfare, for example? It is an interesting question, as are fifty others that might come to mind. Our principle was to explore additional dynamics when there was another factor that was not only conceivably important but for clear logical reasons might be important **BECAUSE OF DYNAMICS HAVING LITTLE OR NOTHING TO DO WITH IQ**. This last proviso is crucial, for one of the most common misuses of regression analysis is to introduce an additional variable

that in reality is mostly another expression of variables that are already in the equation.

9. The italics were in the original, and they point to my broad objection to many of the reanalyses of TBC. The addition of independent variables to try to explain away the independent role of IQ has commonly included variables that are worse than irrelevant; they soak up variance that logic tells us is likely to be attributable to parental IQ or the subjects own IQ.

10. Working out the technical debates on the appropriate treatment of causally related independent variables is complex and seldom decisive. There is, however, a way of bypassing some aspects of the debate by using an independent means of analysis. The sample for the National Longitudinal Survey of Youth (NLSY) used for many of the analyses in TBC included 5,863 subjects who shared the same household with at least one other NLSY subject as brother or sister. A sample this large permits analysis of sibling pairs, thereby controlling not only for socioeconomic status but for the entire constellation of variables that go into the shared environment. Korenman and Winship (2000) have conducted such an analysis, using a siblings fixed-effects model. I also used sibling comparisons in a monograph on income inequality and IQ (Murray 1998).

11. The Korenman and Winship paper offers a nice illustration of where the debate can and cannot be resolved. Part of their paper involves the kind of elaboration of independent variables that I find unrealistic, adding to regression equations variables that certainly have a correlation with, and are probably a function of, IQ. On the interpretation of these analyses, we disagree. But part of their analysis of sibling pairs simply replicates TBC's method, asking how the independent effect of IQ using sibling pairs compares to the independent effect of IQ when an SES index is used as a control. They present a direct comparison of the results for virtually all of the dependent variables used in the Part II of TBC. (Korenman and Winship 2000 Table 7.2). Thus, for example, the OLS estimate of the independent effect of IQ on annual wages for year-round workers is \$5,548 using TBC's control for parental SES compared to \$5,317 in the siblings fixed-effects model. For years of schooling, the respective coefficients are .59 and .45. The logit coefficients for the TBC control for parental SES and the siblings fixed-effects model are, respectively, 1.76 and 1.87 for attainment of a BA degree, 1.39 and 1.72 for being in a high-IQ occupation, .34 and .30 for probability of being out of the labor force for a month or longer in the most recent year, and .52 and .47 for probability of being unemployed for a month or longer in the most recent year. These are examples from a long list, but not atypical ones. As Korenman and Winship observe as they introduce Table 7.2, "[w]ith few exceptions, the fixed-effects estimates for AFQT are remarkably similar to the standard OLS and logit estimates." (Korenman and Winship 2000: 146).

12. I emphasize that the sibling results do not demonstrate that socioeconomic status, or family background more generally, are unimportant in determining social and economic outcomes. Herrnstein and I accepted that they often are. The sibling analysis simply gives us a way of pushing these factors out of the picture and asking whether differences in IQ still make a difference among children from the same family. They do make such a difference, of the same magnitude as the independent effect claimed in TBC. This puts a burden on those who would dismiss the importance of IQ in social and economic outcomes that few critics of TBC have taken up. Many of them seem to be trying to refute a claim ("IQ is important and other factors aren't") that TBC never made.

III. A GENERAL COMMENT.

13. Professor Reifman frames the target article by referring to my expectation, expressed in the Afterword to the soft cover edition of TBC, that the attacks on TBC would eventually prove embarrassing to many of the critics. He sets out to ask whether I have been proved right five years later and concludes that I was wrong.

14. I did not specify a timetable for the process, but even so I am puzzled that Reifman could think the three-stage sequence I specified in the Afterword has gotten past the first stage. Certainly the sources cited by Reifman are overwhelmingly part of the first stage of the process. Many of them were already in draft form within six months of the publication of TBC. Apart from that, it didn't occur to me that anyone could imagine that the dust would settle within five years. Can anyone think of another social science controversy generating as much emotion as this one that has been resolved so quickly? As for the special case of the heritability of IQ, we can all sit back and relax. The answer is on the way, not from psychometrics but from genetics, and the wait should not be more than another decade or so. If I were to have to set a timetable, I suppose another ten years would also be a good time to revisit TBC.

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ID Code: 105

Deposited By: INVALID USER

Deposited On: 11 July 2002

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PSYCOLOQUY (ISSN 1055-0143) is sponsored by the American Psychological Association (APA).

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