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THE INCOME-ACHIEVEMENT GAP AND ADULT OUTCOME INEQUALITY*

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Abstract. This paper discusses various methods for assessing group differences in academic achievement using only the ordinal content of achievement test scores. Researchers and policymakers frequently draw conclusions about achievement differences between various populations using methods that rely on the cardinal comparability of test scores. This paper shows that such methods can lead to erroneous conclusions in an important application: measuring changes over time in the achievement gap between youth from high- and low-income households. Commonly-employed, cardinal methods suggest that this "income-achievement gap" did not change between the National Longitudinal Surveys of Youth (NLSY) 1979 and 1997 surveys. In contrast, ordinal methods show that this gap narrowed substantially for reading achievement and may have narrowed for math achievement as well. In fact, any weighting scheme that places more value on higher test scores must conclude that the reading income-achievement gap decreased between these two surveys. The situation for math achievement is more complex, but low-income students in the middle and high deciles of the low-income math achievement distribution unambiguously gained relative to their high-income peers. Furthermore, an anchoring exercise suggests that the narrowing of the income-achievement gap corresponds to an economically significant convergence in lifetime labor wealth and school completion rates for youth from high- and low-income backgrounds. JEL Codes: I24, I21, J24, C18, C14.

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1. Introduction

Economists and policymakers frequently use standardized test score data to measure group differences in academic achievement. For example, researchers use achievement test scores to measure changes in the black-white achievement gap over time, to assess school and teacher quality, and to quantify the importance of class size and other school inputs for student achievement. A fundamental question in all of these applications is how best to use test score data to measure student achievement. The typical methods employed in education economics provide a deeply inadequate answer to this question.

In almost all empirical work, researchers first normalize test scores to have a mean of zero and a standard deviation of one within each year/age cohort and then compare these normalized scores (or "z-scores") across different cohorts using standard statistical techniques, such as mean differences or regression. This approach makes two very strong assumptions on the normalized test scores. First, it assumes that the normalized test scale has a constant interpretation throughout the range of scores, so that a score change of x represents the same change in achievement at all starting locations in the test scale. Second, it assumes that a given z-score has a fixed interpretation across all cohorts. Neither of these assumptions is well justified by either economic or psychometric theory, and if either fails, standard methods may produce biased estimates. It is even possible for these methods to misidentify the sign of the achievement change.

In recent work, Reardon (2011) uses such standard methods to argue that the income-achievement gap has increased markedly over the last several decades. This dramatic finding has received substantial press coverage and has been cited frequently in a growing body of academic research.[1] This excitment is understandable, as the income-achievement gap is an important driver of intergenerational mobility and adult outcome inequality. Unfortunately, Reardon's conclusions rely very heavily on the cardinal comparability of test scores, both in the cross section and over time.

[1]In the popular press, see Garland (2013) and Tevernise (2012), among others. In the academic literature, Corak (2013), Jerrim and Vignoles (2013), and many others cite Reardon's work.

This paper makes two important contributions, one methodological and the other empirical. On the methodological side, I introduce several ordinal measures of achievement inequality that are not sensitive to the scale of test scores. I show that there are precise, testable conditions under which these ordinal measures unambiguously identify a decrease in achievement inequality. My method has two key components. First, I use test scores that have been "crosswalked" to ensure that a given score corresponds to the same underlying level of achievement regardless of the cohort. Second, I compute achievement gaps using only the rank order of students in the test score distributions. Empirically, I reexamine Reardon's findings on the income-achievement gap using these ordinal methods and find compelling evidence in the NLSY79 and NLSY97 surveys that the income-achievement gap narrowed substantially between 1980 and 1997.[2] This result stands in sharp contrast to Reardon, who finds no significant change using the same data.

In particular, I estimate Cliff's δ, an easy-to compute ordinal statistic that measures the degree of overlap between two distributions. Estimates of Cliff's δ uniformly suggest that there was a large, statistically significant decrease in the income-achievement gap between 1980 and 1997. These estimates are robust to income and test score measurement error, as well as to various methods for adjusting income to reflect household size and composition. I also test a number of first-order stochastic dominance (FOSD) relationships in the crosswalked test score data. These tests allow me to make a very strong claim for reading achievement: any weighting scheme that places greater weight on higher scores would necessarily measure a smaller achievement gap in 1997 than in 1980. This conclusion follows because the low-income achievement distribution in 1997 is unambiguously higher than it was in 1980, while the high-income achievement distribution is unambiguously lower. I cannot make such a strong claim about math achievement because low-achieving low-income students suffered an adverse shift in their achievement, while high-achieving low-income students improved unambiguously.

[2]The NLSY79 provides achievement data for 1980 while the NLSY97 provides data from 1997.

Nonetheless, any weighting scheme that does not place too much emphasis on the bottom of the achievement distribution would find a smaller math achievement gap in 1997 than in 1980.

Finally, to assess whether these ordinal shifts are economically significant, I use the NLSY79 to anchor test scores on various later-life outcomes. In particular, I use the NLSY79 to estimate a set of skill prices that I then use to translate ordinal test score shifts into economically interpretable units. The results of this anchoring exercise suggest that the achievement test shifts that I document correspond to economically large shifts in both lifetime labor wealth and school completion rates. Using NLSY79 skill prices, the decrease in the reading achievement gap between high- and low-income youth corresponds to a narrowing of the lifetime earnings gap of between $50,000 and $70,000 in present discounted value and a decrease in the high school and college completion gaps of between 0.05 and 0.07 probability units.

My findings should give researchers who use standard methods pause. Ordinal methods disagree with traditional cardinal approaches in at least one important setting, and other findings that use standard methods may be similarly fragile. Ordinal methods are prima facie more credible, as the conditions needed for them to provide valid inference are substantially weaker than those required by cardinal methods. When possible, researchers should use methods that do not rely on the scale of achievement test scores.

The ordinal techniques that I develop in this paper apply to any situation in which a researcher wishes to use test score data to assess group differences in achievement; nothing about the analysis assumes that time is the relevant dimension for comparison. For example, the methods in this paper could be used to compare black-white achievement inequality in the American South versus the North at a given point in time, or the achievement of suburban students versus urban students across different metropolitan areas.

The rest of this paper is organized as follows. Section 2 reviews the literature on using test scores to assess group achievement differences. Section 3 describes the NLSY data. Sections 4 and 5 discuss various ordinal estimates of income-achievement gap changes,

while Sections 6 and 7 show how to reach even stronger conclusions if crosswalked test scores are available. Section 8 presents the anchoring analysis. Section 9 discusses the relationship between my empirical results and the literature on differential investments in children by parental income class. Section 10 concludes. Appendix A contains proofs and additional econometric details. Appendices B-C contain all of the empirical results.

2. Literature Review

Recent research by Reardon (2011) argues that the difference in academic achievement between high-income and low-income students has widened tremendously over the last half century. Reardon defines the income-achievement gap as the difference in average academic achievement, measured in z-scores, between students from families at the 90th percentile and 10th percentile of the household income distribution. He estimates that this gap is 30 to 40 percent larger for those students born in 2001 than those born three decades earlier. His regression-based method assumes that the z-scores are cardinally comparable, both in the cross section and over time.[3]

My research does not address Reardon's larger thesis, as I study a much shorter time period and do not use the full complement of surveys that he employs. The large increases that Reardon documents come from cross-sectional income-achievement gaps estimated using surveys other than the NLSY79 and NLSY97. It is possible that my methods would also find an increasing income-achievement gap with these alternate data. However, Reardon does use both NLSY surveys in his analysis, and he calculates a negligible change in the income-achievement gap with these data.

The literature studying changes in achievement gaps over time focuses mostly on black-white disparities estimated assuming the cardinal comparability of z-scores. For example, Fryer and Levitt (2004, 2006) study the evolution of the black-white achievement gap in the first few years of school by comparing standardized test scores.[4] Neal (2006) uses standardized test scores for much of his discussion, yet he also recognizes

[3]The online appendix contains an exhaustive description of his method.

[4]Other papers in this literature that assume the cardinal comparability of standardized test scores include Clotfelter et al. (2009), Duncan and Magnuson (2006), and Hanushek and Rivkin (2006).

that "[a]chievement has no natural units," and so he also compares percentile rankings between black and white test takers. This procedure is related to the one I employ, although he does not conduct inference using ordinal methods.

A number of authors in economics and psychometrics have focused on the sensitivity of standard statistical approaches to the arbitrary choice of test scale. Lang (2010) and Cascio and Staiger (2012) argue that standard treatment effects and value-added estimates are sensitive to arbitrary scaling decisions. In a 2008 working paper, Reardon (2007) shows that black-white achievement gap estimates are sensitive to the choice of test scale, while Bond and Lang (2013) show that order-preserving transformations of achievement test scores can dramatically effect the estimated evolution of the black-white achievement gap from kindergarten through third grade.[5] Stevens (1946) argues that most psychometric scales are inherently ordinal. Lord (1975) shows that IRT scale scores from 2- or 3-parameter logistic models are ordinal in the sense that there are infinitely many rescalings of these scores that will fit any test data equally well.

Cunha et al. (2010), Cunha and Heckman (2008), and many others create interpretable test scales by anchoring scores to later-life, economically meaningful outcomes. This approach offers an appealing solution to the arbitrariness of achievement test scales. However, anchoring has a number of significant downsides. Most importantly, anchoring requires data on outcomes years or even decades after the test date in order to accurately measure lifetime differences between test subjects. For many pressing questions, waiting 20 to 30 years for an answer is not a viable option. In addition, estimates based on anchored scores may be sensitive to the particular outcome chosen as the anchor and to the functional forms used to implement the anchoring procedure. Despite these shortcomings, I augment my ordinal estimates by anchoring test scores on school completion and labor market earnings in Section 8.

[5]In extreme cases, they find that such transformations can actually reverse the estimated growth in the gap. The online appendix presents a simple, parametric example in which it is always possible to find a rescaling such that the estimated gap change calculated using z-scores has the opposite sign as the true gap change.

3. Data

I use the NLSY79 and NLSY97 surveys for all of my analysis. These are high-quality, nationally representative surveys of young adults that contain detailed family background and achievement test score data. Both surveys follow respondents throughout their adult lives, making the anchoring analysis of Section 8 possible. I restrict my attention to the NLSY surveys simply because they are designed to be comparable to each other. Other data sources have income and achievement measures that do not map easily across surveys, adding an additional layer of complexity and uncertainty to the analysis.

The NLSY79 and NLSY97 collect comprehensive family income information for each respondent. In my empirical work, I define family income as the sum of all sources of income for all household members in the base year of each survey.[6] As a robustness check, I rerun the analysis using the average household income in a three-year window around the base year of the survey and obtain qualitatively similar results.

NLSY respondents took the Armed Services Vocational Aptitude Battery (ASVAB) near the start of each survey. I use a subset of the ASVAB known as the Armed Forces Qualifying Test (AFQT), along with its math and reading subsections, as my measures of academic achievement.[7] The ASVAB test format changed between the NLSY79 and the NLSY97. For reasons that I will explain in Section 6, I would like the achievement test scores I use to maintain a constant relationship over time between the test score and the underlying level of achievement. To this end, I make use of a crosswalk created by Segall (1997) based on a sample of test takers who were randomly assigned to the two versions of the ASVAB.[8]

[6]This definition includes income earned by the respondent herself, in addition to the income earned by her parents. Since the respondents I study are less than 18 years old, their share of total household income is usually negligible. Subsetting on respondents positively identified in the data as being dependent on their parents has a very minor effect on the ordinal estimates.

[7]The definition of the AFQT changed in 1989; throughout, I will use the current, post-1989 definition. Using the old definition, I estimate somewhat larger and more statistically-significant decreases in the math income-achievement gap.

[8]The NLSY79 test is pencil and paper (P&P) while the NLSY97 test follows a computer adaptive design (CAD). The crosswalk is courtesy of Altonji et al. (2011) and is available at the following url: `http://www.econ.yale.edu/~fl88/data.html`. The crosswalk contains a percentile-mapped 1980-equivalent score for each component of the CAD ASVAB for each respondent in the NLSY97.

I restrict my analysis to respondents who were between the ages of 15 and 17 when they took the ASVAB. I select this age range for two reasons. First, the two NLSY surveys have very different age distributions, yet both have large numbers of respondents in this age range. Focusing on this age group therefore reduces the importance of age standardization in my empirical work. My results do not change qualitatively if I eschew age-standardization entirely and instead restrict my analysis to 16-year-old test takers.[9] Second, respondents in this age range are just on the cusp of becoming independent adults. Test scores for such students provide a measure of the cumulative effect of parental income on achievement through high school.

4. Percentile-Percentile Curves

This section uses percentile-percentile curves (PPCs) to document shifts in the income-achievement gap between the NLSY79 and the NLSY97. Let L and H denote youth from low- and high-income households, respectively. The PPC for L relative to H simply plots the percentiles of the group-L scores in the distribution of group-L scores against the percentiles of the group-L scores in the distribution of the group-H scores. Formally, let F_L and F_H be the cumulative distribution functions (cdfs) for low- and high-income students. The PPC for L relative to H is given by $\{(p_i, q_i)\}\ \ i \in L$, where $p_i = F_L(s_i)$ and $q_i = F_H(s_i)$.[10] The PPC summarizes how the low-income scores compare to the high-income scores. If the scores in L tend to be lower than the scores in H, the corresponding PPC will lie below the 45-degree line, since the pth percentile in the group-L score distribution will correspond to the qth $< p$th percentile in the group-H score distribution. The further below the 45-degree line the PPC is, the more the scores in H dominate those in L.

Comparing PPCs across different surveys allows one to assess changes in the performance of low-income compared to high-income youth. Shifts in the PPCs closer

Creating a 1980-equivalent AFQT score by adding these cross-walked subscores together is not strictly valid because it ignores the covariance structure of the different ASVAB components. However, Segall (1999) reports that the AFQT scores resulting from such a procedure are virtually identical to those obtained by crosswalking the AFQT scores directly.

[9]The online appendix shows the main ordinal estimates calculated separately for each age.

[10]In practice, these percentiles must be estimated by their obvious sample analogues.

to or further from the 45-degree line indicate decreasing or increasing differences, respectively, in the score distributions between the two groups. Only relative changes in test scores between groups L and H are detectable in the PPCs; if both groups are experiencing secular increases (or decreases) in their test scores, the curves will show no change. The empirical PPCs created using income in the NLSY surveys look very much like Lorenz curves. This is no accident, as the definition of a PPC is very similar to the definition of a Lorenz curve. Since high-income test scores always dominate low-income test scores in both NLSY surveys, the PPCs I calculate will all be below the 45-degree line. However, unlike Lorenz curves, this is not true by construction.[11]

Figure I in Appendix C displays the math, reading, and AFQT PPCs for the bottom versus the top income quintiles in both NLSY surveys. The large income-achievement gap in both surveys is clear from the great distance between all of the PPCs and the 45-degree line. For example, the median reading score in the high-income distribution of the NLSY79 corresponds roughly to the 90th percentile in the low-income score distribution.

The reading and AFQT PPCs for 1997 lie uniformly closer to the 45-degree line than those for 1980. This indicates that the score distributions for high- and low-income students are more similar to each other in the NLSY97 than in the NLSY79. The shifts are quite large in magnitude. In the 1980 data, the 60th percentile in the low-income reading distribution corresponds roughly to the 13th percentile in the high-income distribution. By 1997, the same point in the low-income distribution corresponds to the 18th percentile in the high-income distribution. The NLSY97 math PPC also lies above the PPC for 1980, but the two curves coincide almost perfectly for low-income score percentiles below 50. The convergence in the math score distributions is smaller and less uniform than the convergence in the reading and AFQT distributions.

Figure II displays black-white achievement inequality by designating white respondents as the H group and black respondents as the L group. Both curves are very

[11]There is also no constraint that the curves start at (0,0) and end at (1,1). These will be the endpoints only if both groups have support on the same range of test scores. Furthermore, PPCs may intersect the 45-degree line any number of times.

far below the 45-degree line, indicating that there is substantial black-white achievement inequality in both surveys. Furthermore, the 1997 curve is always above the 1980 curve, which suggests that the black and white test score distributions became more similar between the two NLSY surveys. This result is consistent with the findings on black-white achievement convergence in Altonji et al. (2011) and Neal (2006).

The narrowing of the black-white gap over this time period is a strong force pushing against a widening income-achievement gap. Since black students tend to come from more economically-disadvantaged families than white students, their relative improvement implies that a large subpopulation of low-income families gained relative to wealthier white families.[12] Low-income white students would have to have fallen much farther behind their wealthier white peers in order for the change in the overall income-achievement gap to have been flat or positive.

PPCs calculated using data restricted by race and sex show shifts in the income-achievement gap separately for various demographic subgroups. The PPCs restricted to white respondents suggest that the income-achievement gap for whites narrowed between the two surveys, while the PPCs for black respondents suggest that the income-achievement gap for black youth may have widened. This conclusion for black respondents is sensitive to whether or not income is adjusted for household size and composition. Similarly, the PPCs for women show little evidence of a shift in the income-achievement gap, while the PPCs for men indicate a sharp decrease in achievement inequality between 1980 and 1997.[13]

5. Cliff's δ

5.1. **Estimates and Discussion.** The PPCs provide suggestive evidence that the income-achievement gap narrowed between 1980 and 1997, but they do not readily

[12]For example, the black respondents in my NLSY79 sample have household incomes of $26,000 a year, versus $47,000 for whites. In 1997 the means shifted to $30,000 and $61,000, respectively. In the NLSY79, 37% of the bottom income quintile is black, compared just 3.6% of the top income quintile. In the NLSY97, 29% of the bottom income quintile is black, compared to 7% in the top quintile.

[13]PPCs in which the H category is all men and the L category is all women indicate that there was very little change in male-female achievement inequality between the two surveys. Please refer to the online appendix for the PPCs broken down by race and sex.

admit formal hypothesis testing. Tests of first-order stochastic dominance are feasible, but the resulting test statistics are not interpretable as effect sizes.[14] I therefore seek a test statistic that allows me to conduct inference on shifts in the $\hat{q}$ distributions. Furthermore, since the $\hat{q}$'s are not themselves cardinally interpretable, the statistic should be ordinal.

Cliff's δ is an ordinal statistic that satisfies these requirements. Before defining Cliff's δ, it is worth emphasizing that it is not the only statistic that meets my criteria. Rather, δ is simply an easy-to-compute and easy-to-interpret statistic that ordinally measures the degree of overlap between two distributions.

The definition of Cliff's δ is quite simple. Consider two randomly selected low-income students, one from the NLSY79 and one from the NLSY97. Let $q_{i,97} = F_{H,97}(s_i)$ and $q_{j,79} = F_{L,79}(s_i)$ be each student's respective population test score percentile relative to high-income students in her cohort. Cliff's δ is then defined by

$$\delta_{97,79} \equiv Pr(q_{i,97} \geq q_{j,79}) - Pr(q_{i,97} < q_{j,79}). \tag{1}$$

That is, $\delta_{97,79}$ is the probability that a randomly selected low-income youth from the NLSY97 has a higher q than a randomly selected low-income youth from the NLSY79, minus the reverse probability. The subtraction is simply a normalization to ensure that Cliff's δ always lies between -1 and 1. A positive value of $\delta_{97,79}$ implies that the respondent with higher relative achievement is more likely to come from the NLSY97. In other words, $\delta_{97,79} > 0$ suggests that the income-achievement gap decreased.

Estimating $\delta_{97,79}$ requires two steps. First, one must estimate $q_{i,t}$ for each low-income student i in survey t. Let $\hat{q}_{i,t}$ denote a consistent estimate of $q_{i,t}$ for $t \in \{79, 97\}$. Second, $\delta_{97,79}$ must be estimated from these $\hat{q}$'s. A consistent estimate for $\delta_{97,79}$ is

$$\hat{\delta}_{97,79} = \frac{\sum_{i=1}^{N_{97,L}} \sum_{j=1}^{N_{79,L}} \left[\mathbb{I}\left(\hat{q}_i \geq \hat{q}_j\right) - \mathbb{I}\left(\hat{q}_i < \hat{q}_j\right)\right]}{N_{97,L} N_{79,L}}. \tag{2}$$

[14] The online appendix shows the results of such tests on the low-income score percentiles relative to the high-income score distributions. In the full sample, the relevant null hypotheses are strongly rejected for all three achievement measures.

This estimator does not depend on the scale of the $\hat{q}$'s, and it will be unaffected if the test scores in the two surveys are subjected to distinct, arbitrary rescalings.

I rely exclusively on bootstrapped confidence intervals to conduct inference on $\hat{\delta}_{97,79}$. Asymptotic formulas for $\hat{\delta}_{97,79}$ are available, but they do not account for the fact that both the $\hat{q}$'s and the high- and low-income thresholds are estimated from the data. Adjusting for this first-stage estimation is quantitatively important in this setting; the asymptotic formulas give standard errors that are about half as large as those obtained via the bootstrap.

Table II displays the $\hat{\delta}$ estimates comparing high- and low-income youth. Comparing either income quintiles or deciles, I estimate large, positive $\hat{\delta}$'s for reading and AFQT. I can reject at 1% or 5% the null that $\delta = 0$ against the alternative that $\delta > 0$ for both of these achievement measures. The estimates for math achievement are smaller and less statistically significant. Despite this, I can still reject $\delta = 0$ against $\delta > 0$ at either 5% or 10% for all comparisons. The race-specific $\hat{\delta}$'s show a significant decrease in the income-achievement gap among white youth and a significant increase in the income-achievement gap among black youth, consistent with the PPC analysis of the previous section. Table II subdivides the sample by race before calculating the income thresholds; each comparison is between income categories defined relative to the race-specific income distribution. Since white respondents come from relatively wealthy households, the high- and low-income groups defined in this manner will be somewhat wealthier than their full-sample counterparts. Symmetrically, the high- and low-income groups in the black-only subsample will have lower incomes than their full-sample counterparts. Table III sets income thresholds using the full sample before subsetting on race. As before, the point estimates for white youth are all positive. The reading and AFQT estimates are significantly above 0 at either 5% or 10%, while the math estimates, though positive, are no longer distinguishable from 0. The black-only estimates in Table III have very wide confidence intervals because there are very few black respondents in the upper quintile of the household income distribution. Nonetheless, $\hat{\delta}$ is significantly greater than 0 at 5% for both reading and AFQT.

Thus far, I have defined the high- and low-income categories using percentile cutoffs created separately for each survey. I can also define the income thresholds using the same real-dollar cutoffs for both surveys. If the absolute level of income, rather than relative income, is what matters for creating achievement, then these income categories will come closer to measuring the relevant income-achievement gap.[15] Table IV displays these estimates. Compared to Table II, all of the point estimates for the full sample and the white-only subsample are substantially larger for reading and AFQT and moderately larger for math. The point estimates for the black-only sample are typically smaller than in the baseline case, but they are very imprecisely estimated.

Cliff's δ may be used to measure cross-sectional achievement inequality between high- and low-income youth. The cross-sectional δ is given by the probability that a randomly selected high-income youth has a larger test score than a randomly selected low-income youth from the same cohort, minus the reverse probability. A natural alternative measure for the change in the relative dominance of high-income scores over low-income scores is the difference in the cross-sectional δ's for the NLSY97 and the NLSY79.[16] Although the cross-sectional $\hat{\delta}$'s theoretically can disagree with $\delta_{97,79}$ in both sign and significance, Table V shows that the two approaches lead to similar conclusions in the NLSY data. The differences in the cross-sectional δ's suggest that there was a significant decrease in the income-achievement gap for the full sample and the white-only subsample, while hinting at an increase in the gap for the black-only subsample.

5.2. **Measurement Error.** Both test scores and household income are measured with error. Unfortunately, measurement error in either of these variables can create either positive or negative asymptotic bias in the $\hat{\delta}$'s. For sufficiently extreme measurement

[15]Since there was real income growth between 1980 and 1997, income categories defined in this manner will have relatively many 1997 observations in the high-income category and relatively many 1980 observations in the low-income category.

[16]Please refer to the online appendix for a more formal discussion of this statistic.

error distributions, it is even possible that the probability limit of $\hat{\delta}$ will have the opposite sign as δ.[17] Perverse outcomes like this generally require that the two surveys have very different amounts of measurement error. To see this, suppose that the relationship between household income and expected achievement is monotone increasing. Income measurement error will result in missclassifications at both ends of the income scale. These missclassifications will increase apparent achievement in the low-income group, since the misclassified youth will have higher average incomes and thus higher average achievement than their truly low-income peers. Symmetrically, the missclassifications in the high-income group will decrease that group's apparent achievement. Therefore, income measurement error will bias cross-sectional measures of achievement inequality toward 0. Now suppose that there is more actual achievement inequality and more measurement error in the NLSY97 than in the NLSY79. If the disparity in the amount of measurement error is sufficiently great, it will erroneously appear as though achievement inequality decreased between the two surveys. An analogous argument shows that, test-score measurement error will tend to bias cross-sectional measures of the income-achievement gap toward 0 but can bias gap-change estimates away from 0 if the test scores in the two surveys have very different reliabilities.

I use the observed test score and household income distributions, along with intelligent guesses about the reliabilities of both variables, to simulate the asymptotic bias stemming from each type of measurement error.[18] I use reported reliabilities for each ASVAB component testm in these simulations. The NLSY surveys do not give reliability estimates for their income measures, so I use a range of reliabilities reported from other surveys and data sources. Table VI has the results of these simulations, which suggest that both income and test score measurement error lead to moderate attenuation bias. For a range of plausible reliabilities, the probability limits of the $\hat{\delta}$ estimates are 7 to 25 percent closer to 0 than the true population δ's. The only way to bias the estimates away from 0 is to assume that income in the NLSY79 is much more

[17]This is true even if the underlying test scores and income variables are only subject to classical measurement error. Please see Appendix A for a formal demonstration of these claims.
[18]Please refer to Appendix A for a detailed description of the simulation procedure.

precisely measured than income in the NLSY97. To my knowledge, there is no good reason to suppose that the two income measures differ so much in their reliability. I conclude that my gap-change estimates are probably conservative.

5.3. **Size and Composition Adjustments.** My baseline analysis treats all households equally, regardless of their size and composition. Making no distinctions between households with the same total incomes but very different sizes and compositions ignores the fact that resources must be shared among household members. Holding income fixed, as a household grows larger, the income available per household member falls. Furthermore, adults and children have different consumption needs, so that households with the same size and income but different compositions may have different "real" incomes.

The size and composition of the typical household changed dramatically between the two NLSY surveys. In the NLSY79, roughly 19 percent of 15-17 year-olds reported not having a father-figure in their household.[19] By 1997, almost 28 percent reported that they did not live with a father-figure. The average household size also decreased between the two surveys: in the NLSY79, 42 percent of respondents lived in households with 4 or fewer members, whereas 59 percent of respondents in the NLSY97 lived in such households.[20] The average number of youth (<19 years old) per household fell from 2.8 in 1980 to 2.2 in 1997, while the number of adults per household barely changed.

I adjust for household size and composition by transforming income into equivalency units and then recomputing the $\hat{\delta}$ estimates with the high- and low-income groups defined by percentiles in the transformed income distribution. In particular, I assume that larger households need more income to be equivalently well off, but that the increase is not one-to-one with size because of economies of scale in household production. I also assume that children consume a constant fraction of what adults consume, so that a household with more children will need less income than a household of the same size with fewer children. More specifically, I assume that the equivalency scale for household

[19] "Father-figure" includes step-fathers and adopted fathers. The motherless household rates are very low in both surveys, with the NLSY97 rate only slightly above the NLSY79 rate.
[20] The mean household size is 4.95 in the NLSY79 and 4.41 in the NLSY97.

i with A_i adults and K_i children is given by $E_i = (A_i + \theta K_i)^\gamma$, where $\gamma \in [0, 1]$ gives the returns to scale in household production and $\theta \in [0, 1]$ gives the fraction of an adult's consumption used by a child. I follow Citro and Michael (1995) and set $\gamma = \theta = 0.7$. I also use $\theta = \gamma = 1$, which implies that per capita income is the relevant scale. Finally, I use the equivalency scales used by the U.S. Census.[21]

Table VII displays the Cliff's $\hat{\delta}$'s calculated using each of these three adjustment methods. The estimates are generally quite similar to those calculated using unadjusted income. The math estimates are usually about half as large as the unadjusted estimates. The reading and AFQT estimates are typically slightly larger, with the proportional adjustment producing the smallest increases. The bootstrapped standard errors are also quite similar to the unadjusted estimates for all three achievement tests. For both reading and AFQT, I can reject at 1% or 5% the null that $\delta \leq 0$ against the alternative that $\delta > 0$. For math achievement, only the [90-100] vs [10-20] estimates using either census adjusted or functionally adjusted income are distinguishable from 0. Thus, it does not appear that changes in household characteristics are driving my results.[22]

6. Valuing Achievement Shifts

The previous two sections present compelling evidence that the test score distributions for high- and low-income youth are less dissimilar in the NLSY97 than in the NLSY79. Does this convergence in test scores imply that the *value* of the achievement stocks held by high- and low-income youth also converged? Unfortunately, the answer to this question is no for two distinct reasons.

[21]These equivalency scales are are defined by eligibility criteria for various government transfer programs. The census scale does not adjust for composition and can be modeled roughly by $E_i = (A_i + K_i)^{0.55}$.

[22]I also test an alternative adjustment method in which I regress standardized achievement test scores on a host of demographic variables such as race, sex, and age of parents and then use the estimated residuals as measures of background-adjusted achievement. Using regression-adjusted scores invariably results in smaller estimated shifts in the achievement gap between high- and low-income youth. In each case, however, the adjusted scores still show a sizable decrease in the income-achievement gap between the NLSY79 and the NLSY97, providing further evidence that household size and composition changes are not driving my results. Please refer to the online appendix for a more detailed discussion of this method and results.

The first problem is that the amount of achievement represented by a given $q = F_{H,t}(s)$ may not be constant over time. In the extreme case that the lower end of the high-income achievement support in the NLSY79 lies above the upper end of the high-income achievement support in the NLSY97, $q_{i,1997} > q_{j,1979}$ cannot imply that i has greater achievement than j. The achievement represented by a given percentile in the high-income score distribution needs to either remain constant or increase over time in order for $q_{i,1997} > q_{j,1979}$ to imply that i has greater achievement than j.

Second, the value of an improvement in test scores will not generally be constant throughout the range of scores. The social value of a test score can be viewed as a composition of maps: the map from test score to true, underlying achievement; the map from achievement to life outcomes; and the map from outcomes to social welfare. There is no reason to think that the composition of these maps is linear.

Formally, suppose that $s_{i,t}$ is the achievement test score of student i in year $t \in \{1979, 1997\}$ and that $\psi_t : s \to a$ is the map from test score to underlying achievement.[23] Let $\mathcal{W}(Y) : \mathbb{R}^N \to \mathbb{R}$ be the social welfare that the analyst assigns to the vector $Y \equiv (y_1, \ldots, y_N)$ of life outcomes. Finally, define $f_t^{(j)} : a \to y_j$ to be the map from achievement to outcome y_j in year t, and let F_t denote the vector of these maps. The social value associated with a test score of s in t is then given by $\Gamma_t \equiv \mathcal{W}(F_t(\psi_t(s)))$. This formulation makes explicit both problems with inferring shifts in the value of the underlying achievement gap from shifts in the test score distributions. The first difficulty, that $q_{i,t} > q_{j,t-1} \not\Rightarrow a_{i,t} > a_{i,t-1}$, comes from the fact that ψ_t may not equal ψ_{t-1}. The second problem, that the value of a score improvement may not be constant throughout the range of scores, comes from the fact that Γ_t will not generally be linear in s.

[23]This formulation implicitly assumes that achievement is unidimensional. Allowing achievement to be multidimensional only slightly complicates the theoretical exposition. Empirically, however, such an extension presents significant difficulties because one must take a stand on which dimensions of achievement are measured by a given achievement test. Such a modification is left to future work.

If both ψ and F are held fixed, then $\Gamma_t = \Gamma_{t-1}$ and it is possible to value shifts in test score distributions against a common, interpretable standard.[24] In practice, it will be difficult to know or estimate either ψ or F. Even if these functions are known, different analysts may reasonably assign different weights $\mathcal{W}$ to the outcomes in Y. Pinning down Γ is therefore not straightforward. Despite this difficulty, it is still reasonable to assume that Γ is strictly increasing in s, since test scores ordinally measure achievement and achievement has a positive effect on a wide array of economically important life outcomes.

The above discussion suggests the following requirement for a decrease in the income-achievement gap to be deemed *unambiguous:* all achievement-weighting schemes that place greater weight on higher achievement levels must measure a smaller difference in achievement in the later period, holding skill prices constant. Atkinson (1970) showed that a given distribution $\Omega_1(s)$ will be strictly preferred to another distribution $\Omega_2(s)$ for *any* Γ precisely when Ω_1 first-order stochastically dominates Ω_2. The conditions required for a gap change to be unambiguously positive are likewise based on FOSD.

Theorem 1. *The income-achievement gap unambiguously decreased between $t-1$ and t if both $\Omega_{t-1,H} \succsim \Omega_{t,H}$ and $\Omega_{t,L} \succsim \Omega_{t-1,L}$ and at least one of these relationships is strict. Moreover, in this case, the PPC for t will lie uniformly above the PPC for $t-1$.*

Proof. Please refer to Appendix A for a proof of this statement. □

Theorem 1 says that the income-achievement gap will have decreased unambiguously only when high-income youth performance declines unambiguously and low-income youth performance improves unambiguously, in the sense of FOSD.

7. PPCs and Crosswalked Test Scores

Whether one can infer an unambiguous shift in the income-achievement gap from an unambiguous shift in the high- and low-income test score distributions depends critically on the map from test scores to underlying achievement in each survey. In particular, if

[24] Throughout, I assume that $\mathcal{W}$ is fixed. Since $\mathcal{W}$ simply represents the preferences of the analyst, such an assumption is both natural and justified.

this map is fixed (that is, if $\psi_t = \psi_{t-1}$), then tests of first-order stochastic dominance on the high- and low-income test score distributions will be sufficient to show that any reasonable system for assigning weights to test scores would measure a decrease in the income-achievement gap. If the relationship between ψ_t and ψ_{t-1} is not known, one cannot reach such strong conclusions from ordinal shifts in high- and low-income test score distributions.

Fortunately, it is possible to examine directly how ψ changed between the NLSY79 and the NLSY97 for each achievement measure I use. As described in Section 3, there is a crosswalk between the ASVAB component scores in the NLSY79 and the NLSY97. The crosswalk makes use of a sample of test takers who were randomly assigned to take one of the two versions of the test by equating their scores based on percentile rank.[25] Since the two groups of test takers have roughly equal achievement distributions, the percentile-equated test scores will map scores that correspond to the same underlying level of achievement to each other. $\psi_{1979} \approx \psi_{1997}$ for the crosswalked scores.

FOSD tests on the crosswalked score distributions imply that the conditions specified in Theorem 1 hold for reading and AFQT achievement but do not hold for math achievement. Table VIII displays the p-values from these tests for all three achievement tests for both high- and low-income students.[26] For reading achievement, these tests show that low-income students' scores improved unambiguously and high-income students' scores declined unambiguously. In contrast, the FOSD tests on the math test score distributions do not suggest than any of the needed dominance relationships hold.

Figure III plots the PPCs for high-income students in 1980 relative to high-income students in 1997 and the PPCs for low-income students in 1980 relative to low-income students in 1997. For reading and AFQT, these plots simply confirm the results of the FOSD analysis. The high-income PPCs lie everywhere above the 45-degree line, while the low-income PPCs lie everywhere below. The high- and low-income PPCs for math present a more complex picture. The high-income math PPC is always very close to

[25]For example, if a 90th percentile student in the NLSY79 earned a score of x and the 90th percentile student in the NLSY97 earned a score of y, the crosswalk would map x to y.
[26]I implement the test described in Barrett and Donald (2003).

the 45-degree line; the math achievement distribution for high-income students does not appear to have shifted much between the two surveys. The low-income PPC for math is above the 45-degree line for scores below the 30th percentile and below the 45-degree line for scores above the 30th percentile. This suggests that the low end of the performance distribution shifted down among low-income students, while the high end shifted up.[27] It is the downward shift at the bottom end of the low-income achievement distribution that is driving the rejection of FOSD; a weighting scheme that placed a lot of emphasis on the bottom end of the achievement distribution would assess a larger income-achievement gap in math in the NLSY97 than in the NLSY79.

8. Anchoring and Later-Life Outcomes

8.1. **Motivation.** I have thus far been able to make several very strong claims about changes in the income-achievement gap using only ordinal methods. The ordinal analysis is limited, however, in that it cannot say whether a given test score shift corresponds to an economically important change in achievement. The reading and AFQT achievement gaps narrowed unambiguously, but did they narrow by an interesting amount given a plausible set of achievement weights? The FOSD analysis of math scores shows that there exist achievement weights that would measure a larger achievement gap in 1997 than in 1980. Given this ambiguity, would a realistic set of weights assess an increase or a decrease in the gap for math?

This section estimates the economic importance of the convergence in achievement between high- and low-income youth by mapping crosswalked achievement test scores to various life outcomes. My basic approach uses the NLSY79 to flexibly estimate the reduced-form relationship between crosswalked achievement test scores and a given later-life outcome. Holding this relationship constant, the empirical distribution of crosswalked test scores for low- and high-income youth in the NLSY97 can be used to compute "counterfactual" outcome distributions for the NLSY97 respondents. These

[27] Although it looks as though the high-income PPC is below the 45-degree line above the 80th percentile, confidence intervals for percentiles in this range cannot reject that the PPC is at or above the 45-degree line.

counterfactual distributions answer the following question: "If the relationship between achievement and a given outcome were unchanged between the NLSY79 and the NLSY97, what would be the distribution of that outcome for the NLSY97 cohort given their observed test scores?"

8.2. **Formal Discussion.** Carrying over the notation from Section 6, the value of test score distribution $\Omega(s)$ in survey t is given by $V(\Omega, \mathcal{W}, \psi_t, F_t) = \int \mathcal{W}(F_t(\psi_t(s)))d\Omega(s)$. Using crosswalked test scores guarantees that $\psi_t = \psi_{t+1} \equiv \psi$. This implies that $V(\Omega, \mathcal{W}, \psi, F_t) \neq V(\Omega, \mathcal{W}, \psi, F_{t+1})$ only if $F_t \neq F_{t+1}$. There are two natural "fixed-price" comparisons that measure changes in V due to shifts in Ω: $\Delta(F_t) \equiv V(\Omega_{97}, \mathcal{W}, \psi, F_t) - V(\Omega_{79}, \mathcal{W}, \psi, F_t)$ for $t \in \{79, 97\}$. Similarly, there are two "achievement-constant" comparisons that quantify the value of the change from F_{79} to F_{97}: $\Delta(\Omega_t) \equiv V(\Omega_t, \mathcal{W}, \psi, F_{97}) - V(\Omega_t, \mathcal{W}, \psi, F_{79}),\ t \in \{79, 97\}$. Although these expressions provide a convenient theoretical framework for thinking about evaluating changes in Ω, they are of little practical use because a full list of the outcomes that enter into $\mathcal{W}$ will not be available in even the richest data sets. Using only observable outcomes to calculate $\Delta(F_t)$ and $\Delta(\Omega_t)$ will provide valid estimates only if $\mathcal{W}$ is separable in the observed and unobserved outcomes.

Given these difficulties, I pursue a much more modest objective: I ignore $\mathcal{W}$ altogether and focus on computing gap changes denominated in the units of some particular outcome j. The j-denominated value of distribution Ω is given by $v\left(\Omega, \psi, f^{(j)}\right) \equiv \int f^{(j)}\left(\psi(s)\right) d\Omega(s)$. I define four gap changes denominated in the units of y_j:

$$\begin{aligned}
\Delta(f_t, j) &\equiv v\left(\Omega_{97}, \psi, f_t^{(j)}\right) - v\left(\Omega_{79}, \psi, f_t^{(j)}\right),\ t \in \{79, 97\} \\
\Delta(\Omega_t, j) &\equiv v\left(\Omega_t, \psi, f_{97}^{(j)}\right) - v\left(\Omega_t, \psi, f_{79}^{(j)}\right),\ t \in \{79, 97\}
\end{aligned}$$

In practice, $f_{97}^{(j)}$ will typically not be estimatable because the NLSY97 respondents are not currently old enough to accurately measure differences in lifetime outcomes. Therefore, I mostly report $\Delta(f_{79}, j)$ for various outcomes j.

8.3. **Empirical Method.** This section outlines my empirical method. For brevity, I will omit technical details and suppress the dependence of my estimates on demographic

and background characteristics.[28] My approach for the expected value of outcome y consists of the following steps:

(1) Estimate $\hat{F}_{79}(y|s)$, the conditional distribution of y given s in the NLSY79.

(2) Use $\hat{F}_{79}(y|s)$ to estimate $\hat{\mathbb{E}}_{79}[y|s] = \int y d\hat{F}_{79}(y|s)$.

(3) Estimate $\hat{\Omega}_{t,G}(s)$ for each income group $G \in \{H, L\}$ and survey $t \in \{1979, 1997\}$.

(4) Estimate the counterfactual mean of y in group G: $\tilde{\mathbb{E}}_{97,G}[y] \equiv \int \hat{\mathbb{E}}_{79}[y|s] d\hat{\Omega}_{97,G}(s)$.

(5) Estimate the y-denominated gap change for G by $\widehat{\Delta\mathbb{E}_G[y]} \equiv \tilde{\mathbb{E}}_{97,G}[y] - \hat{\mathbb{E}}_{79,G}[y]$.

(6) Estimate the y-denominated gap change by $\widehat{\Delta(y)} \equiv \widehat{\Delta\mathbb{E}_H[y]} - \widehat{\Delta\mathbb{E}_L[y]}$.

I can also use $\hat{Q}_{79}(y; \tau, s)$, τth quantile of y conditional on s estimated from $\hat{F}_{79}(y|s)$, as the skill-pricing function. Section 8.5 reports gap-change estimates using both skill-pricing functions.

The techniques I employ to implement steps 2 - 6 are standard. For the continuous outcomes in step 1, I construct $\hat{F}_{79}(y|s)$ using a large number of quantile regressions that predict different quantiles of y as polynomial functions of s. These fitted regressions give an estimate of the quantile function of y conditional on s, which can then be inverted and smoothed to obtain an estimate of $\hat{F}_{79}(y|s)$. I estimate $\hat{F}_{79}(y|s)$ for binary outcomes such as high school and college completion using a large number of probit regressions.[29]

It is important to emphasize that this approach does not allow me to make any causal claims. A given skill-pricing relationship is a complex equilibrium object that depends on on many different, endogenously-chosen factors that are not explicitly modeled here. Making causal statements using any of these relationships would require a much more complete model of the labor market. When I make statements like, "The improvement in achievement among low-income white men corresponds to an increase of $\$X$ of lifetime wage income," I am not arguing that the improvement in achievement caused an increase

[28]For most of my empirical work, I simply calculate gap change estimates separately for each race/sex bucket. I only discuss using NLSY79 skill prices to calculate counterfactual gap changes in the NLSY97. Please refer to Appendix A for a detailed discussion of the anchoring methodology.

[29]I also estimate gap changes for binary outcomes using local polynomial regressions. This procedure produces very similar gap-change estimates as those reported here. Please refer to the online appendix for these estimates.

in wage income of $\$X$ for low-income white men. Rather, I am simply translating test score shifts to wage income shifts using the same set of skill prices for both surveys.

8.4. **Data.** I carry over all of the data restrictions from the previous sections. Both NLSY surveys collect longitudinal data on income, education, employment, and many other outcomes annually or biennially. I can observe the NLSY79 respondents through ages 45-47 and the NLSY97 respondents through ages 29-31. Since a large share of the total heterogeneity in labor market outcomes has yet to be revealed by age 30, I estimate skill-pricing equations mostly using the NLSY79. The exception is high school and college completion; very few people change their school completion status after age 30, so I can use either NLSY survey to estimate skill-pricing relationships for these outcomes.

The primary outcome that I study is the present discounted value of lifetime labor wealth (pdv_labor).[30] The labor/leisure decision faced by workers complicates the analysis of pdv_labor because the lifetime budget set of an individual depends on the degree to which she controls her labor supply. To see this, consider two extreme scenarios: one in which there is no voluntary unemployment and one in which there is no involuntary unemployment. If no worker is ever voluntarily unemployed, then the discounted sum of observed annual wage incomes yields the economically relevant measure of lifetime labor wealth. In contrast, if there is only voluntary unemployment, then the present value of the observed income flows will understate true lifetime labor wealth. In this case, the correct measure of annual income is given by an individual's hourly wage rate multiplied by the total number of hours she could possibly work in a year. Of course, the truth is probably somewhere between these two extremes: workers have some control over their labor supply, but they may also face involuntary unemployment. To address this indeterminacy, I estimate pdv_labor under both extreme unemployment assumptions and compare the resulting gap-change estimates.

[30] I convert all dollar values to a 1997 basis using the CPI-U prior to any calculations. I use a discount rate of 5% throughout.

Annual earnings data are often missing in the NLSY surveys. Rather than model selection explicitly, I compare gap-change estimates computed assuming either extreme positive or extreme negative selection. Pessimistically, I impute earnings equal to the minimum ever observed for an individual; optimistically, I impute the maximum.[31] I also focus my analysis on white males, as this group has comparatively high labor force participation.

The optimistic and pessimistic imputation rules do not bound the change in pdv_labor associated with a given change in the distribution of achievement. The size of the estimated gap change depends on the slope of the reduced-form skill-pricing relationship. The steeper the pricing relationship is in the regions where group G's test score density is greatest, the larger the estimated change for group G will be. Similarly, the gap-change estimates assuming all or no voluntary unemployment do not generally bound the true gap-change. Despite this, these various methods for estimating gap-changes are still informative. As the next section will discuss, these different methods all produce qualitatively similar estimates. The robustness of my results suggests that the true population changes are likely close to those I report here. Please refer to Appendix A for a more detailed discussion of the construction of these data.

Both NLSY surveys collect annual data on the highest grade completed by each survey respondent. Using the highest-grade-completed variable from either NLSY survey, I construct two school-completion variables. The indicator "college" is equal to 1 if the respondent has completed college and equal to 0 otherwise. Analogously, "high school" is equal to 1 if the respondent has at least completed high school and 0 otherwise. As with income, I can define optimistic and pessimistic imputations for respondents with missing highest grade completed data in all years. There are very few such respondents,

[31]These optimistic and pessimistic imputation rules are quite extreme. Suppose that a respondent reports wage income of \$100 in 1983 at the age of 20, and then reports wage income of \$100,000 for 2000, 2002, and 2006, but has no wage income recorded for 2004. The pessimistic imputation rule will assign to this individual a wage income for 2004 of at most \$100. Similarly, if the same individual has wage income missing in 1982, when she was 19 years old, the optimistic imputation rule will assign her a wage income of at least \$100,000 for that year. Under mild conditions, the optimistically and pessimistically imputed conditional means will bound the true population mean: $\mathbb{E}_{t,\text{pess}}[y|s] \leq \mathbb{E}_t[y|s] \leq \mathbb{E}_{t,\text{opt}}\,[y|s],\ \forall s$.

so the imputation method matters very little for the gap-change estimates. Table IX has the summary statistics for the outcome variables that I use.

8.5. **Estimates and Discussion.** Table X contains the white male mean gap-change estimates for lifetime labor wealth estimated using NLSY79 skill prices. These estimates show that the decrease in the reading income-achievement gap corresponds to an economically meaningful decrease in the lifetime labor wealth gap between white men from high- and low-income households. Assuming workers have full control over their labor supply, the improvement in reading achievement among low-income white males corresponds to an increase of $8,000-$11,000 in lifetime labor wealth, while the adverse shift in achievement among high-income white males translates to a decrease of $34,000-$42,000. Together, these estimates imply that the decrease in the reading income-achievement gap translates to a decrease of $43,000-$52,000 in the lifetime labor wealth gap between these groups. The gap-change estimates increase to $56,000-$69,000 if workers cannot control their own labor supply. The AFQT gap-change estimates are quite similar to those for reading: $30,000-$35,000 with flexible labor supply and $47,000-$51,000 with fixed labor supply. Unfortunately, the bootstrapped standard errors for these gap changes are quite large, so I cannot reject that the true gap change is $0 at any standard significance level.

The ordinal estimates do not conclusively show that the math income-achievement gap decreased. The ambiguity is driven by the polarization of low-income achievement: low-performing, low-income youth suffered an adverse shift in math achievement, while high-performing, low-income youth experienced clear gains. The mean gap-change estimates for pdv_labor reflect this polarization. With flexible labor supply, the shifts in math correspond to a narrowing of lifetime wealth gap of $10,000-$22,000. In contrast, if labor supply is fixed, the gap-change estimates range from -$7,000-$13,000. Strikingly, the estimated changes in both cases for high- and low-income white men are all negative; the total value of the achievement stocks held by these groups decreased, but the decrease may have been larger for low-income men. The standard errors for the

math point estimates are also quite large and preclude rejection of the null that the true gap change is \$0.

I also use $\hat{Q}_{79}(\text{pdv_labor}; \tau, s)$, the estimated τth percentile of pdv_labor conditional on s, as an alternative skill-pricing function. Through the examination of different test score and outcome percentiles, I can get a more nuanced picture of how changes in high- and low-income achievement have translated to changes in outcomes. An additional benefit of using $\hat{Q}_{79}$ is that the resulting gap-change estimates are much less sensitive to outliers than the estimates using the conditional mean as the skill-pricing function. This results in substantially smaller standard errors that allow me to reject the null that the true gap change is \$0 at 1% significance in almost all cases.

Table XI shows the estimated gap changes for various projected pdv_labor percentiles for the median values of s in the high- and low-income groups. I compute standard errors via the bootstrap. The interpretation of this table is somewhat subtle. Consider the top leftmost entry of \$13,378. This value equals $\hat{Q}_{79}\left(\text{pdv_labor}; 10, s_{L,97}^{(50)}\right) - \hat{Q}_{79}\left(\text{pdv_labor}; 10, s_{L,79}^{(50)}\right)$ where $s_{L,t}^{(50)}$ is the math test score corresponding to the median of the survey-t test score distribution of low-income white men. Table XI shows that the shifts in median achievement for high- and low-income white men translate to a narrowing of the median labor wealth gap between these two groups of \$41,000-\$43,000 for reading and \$38,000-\$40,000 for AFQT. The estimated gap changes are larger for higher projected wealth percentiles and smaller for smaller projected wealth percentiles. In sharp contrast with the mean estimates, the median gap-change estimates for math all suggest a narrowing of the labor wealth gap between high- and low-income white men. Indeed, the point estimates for math are often somewhat larger than those for reading. Unlike the conditional mean estimates, gap-change estimates for wealth percentiles less than or equal to the median assuming either fixed or flexible labor supply are almost identical for all three achievement measures. For wealth percentiles above the median, the gap change estimates assuming no voluntary unemployment are larger for all three achievement measures. All of the point estimates are significantly different from 0 at 1% for each projected wealth percentile and each achievement measure. The

estimates using either optimistically or pessmistically imputed data are very similar and can be found in the online appendix. However income is computed, the changes in achievement at the medians of the high- and low-income score distributions correspond to economically large shifts in projected lifetime labor wealth.[32]

Figure IV plots the math and reading gap-change estimates for test scores at the 10th, 50th, and 90th percentiles of the high- and low-income test score distributions for a wide range of projected income percentiles. These curves show substantial heterogenaity for different scores and different projected incomes. For math and reading, the curves comparing the medians of the high- and low-income distributions lie always above the curves comparing the 10th and 90th percentiles. This pattern suggests that the convergence in median achievement corresponds to greater convergence in later-life inequality than achievement convergence in the tails. Strikingly, the 90th percentile math curve is always negative, while the corresponding curve for reading almost never is. This discrepancy helps explain why the mean gap-change estimates for math are small and insignificant; these estimates roughly correspond to the average of the projected income changes for the median, 10th, and 90th score percentiles. The 10th percentile changes are close to 0, while the median and 90th percentile changes are virtually mirror images of each other and therefore nearly cancel each other out. In contrast, no reading curve is ever significantly negative, so the resulting mean gap-change estimates are positive and large. Figure V plots these curves with pointwise 95% confidence intervals estimated via the bootstrap. The confidence intervals confirm that the differences in the math and reading curves are statistically significant.

Table XII displays the gap-change estimates for high school and college completion. Using NLSY79 skill prices, I calculate that the improvement in any of the three achievement measures between 1980 and 1997 corresponds to a decrease of about 0.05-0.06 in the high school graduation gap for white men. The high school gap changes for math

[32]Please refer to the online appendix for gap-change estimates computed at the 25th and 75th percentiles of the high- and low-income test score distributions.

and AFQT are marginally distinguishable from 0, while the change for reading is significant at 1%. The estimates for white men using NLSY97 skill prices paint a similar picture, though with slightly smaller and less statistically significant point estimates. The college gap-change estimates are around 0.07 for both sets of skill prices and all three achievement measures. In all cases, the college gap changes are distinguishable from 0 at 1% significance. The college and high school point estimates for white women are all close to 0 and statistically insignificant. For black men and women, the gap-change estimates for both high school and college are negative but insignificant. Overall, these results suggest that the decrease in the income-achievement gap between 1980 and 1997 corresponds to a large decrease in the high school and college completion gaps for white men. The estimates are inconclusive for other demographic groups, although there is some evidence that the changes correspond to large increases in both the high school and college completion gaps among black youth.

9. A Puzzle: The Parental Income-Investment Gap

The gap in childhood investment expenditures between high- and low-income parents increased dramatically over the last several decades. Data on parental time use and direct monetary expenditures show that while all parents substantially increased their investments since 1970, high-education and high-income parents increased their expenditures much more rapidly. For example, Duncan and Murnane (2011) calculate that the parents in the top income quintile increased their enrichment expenditures per child by 150% between 1972 and 2006, while parents in the bottom quintile increased their expenditures only 57%. Looking at time diaries, Ramey and Ramey (2010) estimate that college-educated mothers increased their childcare time by almost 9 hours per week in the 1990s, while less-educated mothers increased their childcare time by only 4 hours.[33]

[33]Gautier, Smeeding, and Gauthier et al. (2004) find evidence that educated mothers increased their time spent with children more than did low-education mothers. Guryan et al. (2008) estimate that Canadian college-educated mothers spend 16.5 hours per week on childcare tasks, while women with only a high school degree spend 12.1 hours. Hill and Stafford (1974) and Leibowitz (1975) reach similar conclusions about cross-sectional differences in time investments. Aguiar and Hurst (2007) find that

Given my finding that the income-achievement gap decreased between 1980 and 1997, these results are quite puzzling. High-income parents dramatically increased their investments relative to low-income parents but seem to have less than nothing to show for it. The time use results do not actually directly contradict my results because high-income parents only began to differentially increase their time investments around 1993; the NLSY79 and NLSY97 youth probably received similar parental time investments at least through age 11-13. We would only expect to see a widening income-achievement gap between the NLSY97 youth and even younger cohorts if in fact parental time expenditures are effective at generating achievement. In contrast, the parental goods investment evidence does imply that the gap in enrichment expenditures between high and low-income households should be much larger in the NLSY97 than in the NLSY79. If parental investments are subject to decreasing returns, it is logically possible for the investment gap to increase and for the achievement gap to simultaneously decrease. However, my results comparing high-income youth in the two surveys using the crosswalked test scores show that the achievement of the high-income group actually decreased in absolute terms between 1980 and 1997. This is not consistent with a decreasing-returns explanation for achievement convergence, as such an explanation implies that both groups in 1997 should outperform their like-income peers in 1980.

There are a number of explanations that could rationalize the enrichment expenditure results with my estimates of the income-achievement gap. The parental expenditure data may be misclassifying consumption spending as enrichment spending. Art camp, trips to the science museum, and similar activities may simply not be effective at improving achievement test scores. Alternatively, perhaps the kind of enrichment spending high-income parents differentially engage in has payoffs along dimensions not well-measured by achievement tests. For example, colleges like to see well-rounded students with diverse lists of extracurricular activities. Spending on these activities by parents may not improve achievement test scores, but may nevertheless provide a large

parental time with children increased by roughly 2.0 hours per week between 1965 and 2003. Bianchi (2000) and Ramey and Ramey (2010) reach similar conclusions.

benefit. These explanations are speculative; without more research, my results have uncovered a genuine puzzle.

10. Discussion and Conclusion

Ordinal methods using test score data show that the gap in academic achievement between youth from high- and low-income households decreased dramatically between 1980 and 1997. These results are robust to measurement error, composition adjustments, and various data-inclusion criteria. Using percentile-equated test scales, I find strong evidence that the ordinal shifts in reading and AFQT must correspond to unambiguous decreases in the true income-achievement gap. The ordinal shifts in math achievement do not necessarily correspond to a decrease in the underlying achievement gap, although low-income students above the 30th percentile of the low-income math-achievement distribution unambiguously gained. Anchoring reading and AFQT test scores on various later-life outcomes shows that these ordinal shifts correspond to economically-important shifts in underlying achievement. For white men, the narrowing of the income-achievement gap translates to a narrowing in the lifetime wealth gap of roughly $50,000 and a narrowing of the high school and college completion gaps of 0.05 to 0.08 probability units. The results are less clear-cut for math. Changes in math achievement correspond to an ambiguous change in the mean labor wealth gap but a large decrease in the median wealth gap for median high- and low-income white males.

My results should give pause to economists and policymakers who analyze achievement inequality using test score data. The typical methods used to quantify differences in academic achievement between groups assume that test scores are cardinally comparable. This assumption is not well justified, and cardinal methods are often quite sensitive to order-preserving transformations of the test score data. For instance, recent research using standard methods finds a negligible change in the income-achievement gap using the same NLSY data that I employ. Cardinal methods can lead to conclusions about changes in achievement inequality that are not supported by the ordinal content of the test scores.

Given recent findings on changes in parental investments in children by income class, my finding that the income-achievement gap has narrowed is puzzling. High-income parents have increased their enrichment spending on their children much more rapidly than low-income parents have over the last three decades, yet my estimates imply that the distribution of high-income reading achievement shifted down while the low-income reading distribution shifted up. Even in math achievement, where the ordinal analysis leads to less clear-cut conclusions, I find no evidence that the achievement distribution for high-income youth shifted up between 1980 and 1997. Testing various hypotheses that could resolve this puzzle is a worthwhile avenue for future research.

Holding skill prices fixed, the anchoring estimates imply that the convergence in achievement between high- and low-income should have been a powerful force reducing adult outcome inequality. This does not imply, however, that inequality in outcomes between youth from high- and low-income households will be lower in the NLSY97 than in the NLSY79. If the returns to achievement become more convex over time, for example, smaller true achievement differences may well translate to larger absolute outcome differences than in the past. Unfortunately, the young age of the NLSY97 respondents precludes directly examining their lifetime labor market outcomes. As more data becomes available over the next decade, it will be fascinating to track adult outcome inequality for the youth in this more recent cohort.

Appendix A. Additional Discussion and Proofs

A.1. **Calculating $\hat{\delta}$ Using Weighted Data.** Define $W_X = \sum_{i \in X} w_i$ and $W_Y = \sum_{j \in Y} \omega_j$, where $\{w_i\}$ are the weights for sample X and $\{\omega_j\}$ are the weights for sample Y. $\hat{\delta}_{x,y}$ is given by:

$$\hat{\delta}_{x,y} = \frac{1}{W_Y W_X} \sum_{i \in N_X} \sum_{j \in N_Y} w_i \omega_j \left[\mathbb{I}(x_i > y_j) - \mathbb{I}(x_i < y_j) \right]. \tag{3}$$

A.2. **Bootstrap Procedure for $\hat{\delta}$.**

(1) For each observation i in the NLSY97 and j in the NLSY79, define $p_i \equiv \frac{w_i}{W_{97}}$ and $p_j \equiv \frac{w_j}{W_{79}}$ similarly.

(2) For each $t = 1, \ldots, T$ for some large number T of bootstrap iterations:

(a) Draw samples of size N_{1979} and N_{1997} from the two NLSY surveys, using $\{p_i\}$ and $\{p_j\}$ as the sampling probabilities.

(b) Estimate the unweighted $\hat{\delta}_t$ from the resulting pseudosample.

(3) Use the distribution of $\{\hat{\delta}_t\}$ for hypothesis testing.

A.3. **Measurement Error.** Let s_i denote a generic true achievement test score in a population $\mathcal{X}$ and s_j a generic true score in a population $\mathcal{Y}$. Assume the observed test scores $\tilde{s}$ are equal to the true test scores plus classical measurement error: $\tilde{s}_k = s_k + \eta_k,\ \mathbb{E}[\eta_k|s_k] = 0, \quad k \in \{i, j\}$. Define $\tilde{\delta}_{x,y} \equiv Pr(\tilde{s}_i > \tilde{s}_j) - Pr(\tilde{s}_i < \tilde{s}_j)$ and $\delta_{x,y} \equiv Pr(s_i > s_j) - Pr(s_i < s_j)$. Theorem 2 states that under some symmetry conditions, $\tilde{\delta}_{x,y}$ will have the same sign as $\delta_{x,y}$ but be smaller in magnitude.

Theorem 2. *Suppose that the following hold: (1) $(s_i, s_j, \eta_i, \eta_j)$ are mutually independent for all pairs (i, j), (2) the distribution of $\eta_i - \eta_j \equiv \Delta\eta_{i,j} =_D -\Delta\eta_{i,j}$, (3) $s_i - s_j \equiv \Delta s_{i,j}$ follows a single-peaked distribution, and (4) both $\Delta s_{i,j}$ and $\Delta\eta_{i,j}$ have density everywhere on their supports.*[34] *Then $sign(\delta_{x,y}) = sign(\tilde{\delta}_{x,y})$ and $|\delta_{x,y}| > |\tilde{\delta}_{x,y}|$.*

Proof. $\delta_{x,y}$ can be decomposed as follows:

$$\begin{aligned}
\delta_{x,y} &= \tilde{\delta}_{x,y} + \underbrace{\left(-Pr(s_i > s_j)[\gamma^+ - 1] + Pr(s_i < s_j)[\gamma^- - 1]\right)}_{\text{Bias}_{x,y}} \\
\gamma^+ &\equiv [Pr(\tilde{s}_i > \tilde{s}_j | s_i > s_j) - Pr(\tilde{s}_i < \tilde{s}_j | s_i > s_j)] \\
\gamma^- &\equiv [Pr(\tilde{s}_i < \tilde{s}_j | s_i < s_j) - Pr(\tilde{s}_i > \tilde{s}_j | s_i < s_j)] .
\end{aligned}$$

Suppose that the peak of the $\Delta s_{i,j}$ distribution is greater than 0. Let $\Phi(\cdot)$ denote the cdf of $\Delta\eta_{i,j}$, and let f_Δ and F_Δ denote the pdf and cdf of $\Delta s_{i,j}$, respectively. Further, suppose that $Support(\Delta s_{i,j}) = (-a, b) \subseteq (-\infty, \infty)$ and $Support(\Delta\eta_{i,j}) = (-l, h) \subseteq$

[34] These assumptions are merely sufficient for the conclusion in Theorem 2 to hold. The necessary conditions are

$$\begin{aligned}
\delta_{x,y} > 0 \quad &\Longrightarrow \quad A_1 > A_2,\ \delta_{x,y} < 0 \Longrightarrow A_1 < A_2 \\
A_1 \quad &= \quad Pr(\Delta s_{i,j} > 0 | \Delta s_{i,j} + \Delta\eta_{i,j} < 0) Pr(\Delta s_{i,j} + \Delta\eta_{i,j} < 0) \\
A_2 \quad &= \quad Pr(\Delta s_{i,j} < 0 | \Delta s_{i,j} + \Delta\eta_{i,j} > 0) Pr(\Delta s_{i,j} + \Delta\eta_{i,j} > 0).
\end{aligned}$$

$(-\infty, \infty)$. By assumption, l and h are both greater than 0. Based on assumptions 1-3, γ^+ and γ^- can be rewritten as

$$\gamma^+ = \frac{1}{1 - F_\Delta(0)} \int_0^b [2\Phi(k) - 1]\, f_\Delta(k) dk, \quad \gamma^- = \frac{1}{F_\Delta(0)} \int_0^b [2\Phi(k) - 1]\, f_\Delta(-k) dk.$$

Substituting these expressions into the formula for $\text{Bias}_{x,y}$ yields

$$\text{Bias}_{x,y} = \left(\int_0^b 2\Phi(k) \left[f_\Delta(-k) - f_\Delta(k) \right] dk \right) + 2\left[1 - 2F_\Delta(0)\right].$$

The single-peaked assumption on F_Δ implies that $[f_\Delta(-k) - f_\Delta(k)] < 0, \ \forall k \geq 0$. Therefore, the integral in the above expression for $\text{Bias}_{x,y}$ is negative. At the same time, the single-peaked assumption also implies that $2F_\Delta(0) < 1$, which implies that the second term in the $\text{Bias}_{x,y}$ formula is positive. Since $\Phi(k)$ is strictly increasing in k everywhere on $(-l, h)$, $\int_0^b 2\Phi(0) \left[f_\Delta(-k) - f_\Delta(k) \right] dk > \int_0^b 2\Phi(k) \left[f_\Delta(-k) - f_\Delta(k) \right] dk$ must hold. Symmetry in $\Delta\eta_{i,j}$ implies $\Phi(0) = \frac{1}{2}$, so that $\int_0^b 2\Phi(0) \left[f_\Delta(-k) - f_\Delta(k) \right] dk = 2F_\Delta(0) - 1$. Therefore, $\text{Bias}_{x,y} < 1 - 2F_\Delta(0) > 0$. Since $\Phi(k) \leq \Phi(\infty) = 1$, $\int_0^b 2\left[f_\Delta(-k) - f_\Delta(k) \right] dk > \int_0^b 2\Phi(k) \left[f_\Delta(-k) - f_\Delta(k) \right] dk$. The RHS of this inequality simplifies to $2\left[2F_\Delta(0) - 1\right]$. But combined with $\text{Bias}_{x,y} < 1 - 2F_\Delta(0) > 0$, this implies that $\text{Bias}_{x,y} > 0$. Since $1 - 2F_\Delta(0) \in (0, 1)$, the bias must lie strictly between 0 and 1. This establishes that $\delta_{x,y} > 0 \implies \tilde{\delta}_{x,y} \in (0, \delta_{x,y})$. The argument for $\delta_{x,y} < 0$ is symmetric and omitted for brevity. □

Theorem 3. *Define $\delta \equiv Pr(q_i > q_j) - Pr(q_i < q_j)$ and $\tilde{\delta} \equiv Pr(\tilde{q}_i > \tilde{q}_j) - Pr(\tilde{q}_i < \tilde{q}_j)$. If the true test score and measurement error distributions are all normal and independent, there exist means and variances for these distributions such that $|\tilde{\delta}| > |\delta|$. There also exist means and variances such that $sign(\tilde{\delta}) = -sign(\delta)$.*

Proof. For each $s_{i,L,x}$ and $s_{j,L,y}$, define m_i and m_j as the probit transformations of q_i and q_j.[35] Since $\Phi^{-1}(\cdot; 0, 1)$ is monotone increasing, $\delta_{x,y} = Pr(m_i > m_j) - Pr(m_i < m_j)$.

[35] That is, $m_i \equiv \Phi^{-1}\left(\Phi(s_{i,L}; \mu_{H,x}, \sigma^2_{H,x}); 0, 1\right)$ and $m_j \equiv \Phi^{-1}\left(\Phi(s_{j,L}; \mu_{H,y}, \sigma^2_{H,y}); 0, 1\right)$.

Both m_i and m_j are normally distributed:

$$m_i \sim N\left(\underbrace{\frac{\mu_{L,x}-\mu_{H,x}}{\sigma_{H,x}}}_{\mu_{m,x}}, \underbrace{\frac{\sigma^2_{L,x}}{\sigma^2_{H,x}}}_{\sigma^2_{m,x}}\right), \quad m_j \sim N\left(\underbrace{\frac{\mu_{L,y}-\mu_{H,y}}{\sigma_{H,y}}}_{\mu_{m,y}}, \underbrace{\frac{\sigma^2_{L,y}}{\sigma^2_{H,y}}}_{\sigma^2_{m,y}}\right).$$

Define $(\tilde{m}_i, \tilde{m}_j)$ analogously to (m_i, m_j), but with the noisy test scores. These $(\tilde{m}_i, \tilde{m}_j)$ are also jointly normal:

$$\tilde{m}_i \sim N\left(\underbrace{\frac{\mu_{L,x}-\mu_{H,x}}{\sqrt{\sigma^2_{H,x}+\sigma^2_{\eta,x}}}}_{\tilde{\mu}_{m,x}}, \underbrace{\frac{\sigma^2_{L,x}+\sigma^2_{\eta,x}}{\sigma^2_{H,x}+\sigma^2_{\eta,x}}}_{\tilde{\sigma}^2_{m,x}}\right), \quad \tilde{m}_j \sim N\left(\underbrace{\frac{\mu_{L,y}-\mu_{H,y}}{\sqrt{\sigma^2_{H,y}+\sigma^2_{\eta,y}}}}_{\tilde{\mu}_{m,y}}, \underbrace{\frac{\sigma^2_{L,y}+\sigma^2_{\eta,y}}{\sigma^2_{H,y}+\sigma^2_{\eta,y}}}_{\tilde{\sigma}^2_{m,y}}\right).$$

Define

$$\tilde{R} \equiv \frac{\tilde{\mu}_{m,x}-\tilde{\mu}_{m,y}}{\sqrt{\tilde{\sigma}^2_{m,x}+\tilde{\sigma}^2_{m,y}}} = \frac{\frac{\mu_{L,x}-\mu_{H,x}}{\sqrt{\sigma^2_{H,x}+\sigma^2_{\eta,x}}} - \frac{\mu_{L,y}-\mu_{H,y}}{\sqrt{\sigma^2_{H,y}+\sigma^2_{\eta,y}}}}{\sqrt{\frac{\sigma^2_{L,x}+\sigma^2_{\eta,x}}{\sigma^2_{H,x}+\sigma^2_{\eta,x}} + \frac{\sigma^2_{L,y}+\sigma^2_{\eta,y}}{\sigma^2_{H,y}+\sigma^2_{\eta,y}}}} \tag{4}$$

$$R \equiv \frac{\mu_{m,x}-\mu_{m,y}}{\sqrt{\sigma^2_{m,x}+\sigma^2_{m,y}}} = \frac{\frac{\mu_{L,x}-\mu_{H,x}}{\sqrt{\sigma^2_{H,x}}} - \frac{\mu_{L,y}-\mu_{H,y}}{\sqrt{\sigma^2_{H,y}}}}{\sqrt{\frac{\sigma^2_{L,x}}{\sigma^2_{H,x}} + \frac{\sigma^2_{L,y}}{\sigma^2_{H,y}}}}. \tag{5}$$

Normality implies $\tilde{\delta}_{x,y} = 2\Phi\left(\frac{\tilde{\mu}_{m,x}-\tilde{\mu}_{m,y}}{\sqrt{\tilde{\sigma}^2_{m,x}+\tilde{\sigma}^2_{m,y}}}; 0, 1\right) - 1$ and $\delta_{x,y} = 2\Phi\left(\frac{\mu_{m,x}-\mu_{m,y}}{\sqrt{\sigma^2_{m,x}+\sigma^2_{m,y}}}; 0, 1\right) - 1$. R and $\tilde{R}$ thus determine the relative signs and sizes of $\tilde{\delta}_{x,y}$ and $\delta_{x,y}$. Comparing these quantities in general is quite complicated, so I consider a number of special cases that delineate scenarios in which $\tilde{\delta}_{x,y}$ is biased toward or away from 0, as well as cases in which the sign of $\tilde{\delta}_{x,y}$ is different from the sign of $\delta_{x,y}$.

Case 1: $\sigma_{H,x} = \sigma_{H,y} = \sigma_H$, $\sigma_{L,x} = \sigma_{L,y} \equiv \sigma_L$, and $\sigma_{\eta,x} = \sigma_{\eta,y} \equiv \sigma$. Let $\Delta\mu_y \equiv \mu_{H,y} - \mu_{L,y}$ and $\Delta\mu_x \equiv \mu_{H,x} - \mu_{L,x}$. These assumptions imply that $R = \frac{(\Delta\mu_y - \Delta\mu_x)}{\sigma_L\sqrt{2}}$ and $\tilde{R} = \frac{(\Delta\mu_y - \Delta\mu_x)}{\sqrt{\sigma^2_L + \sigma^2}\sqrt{2}}$. The signs of both R and $\tilde{R}$ are determined by the sign of $(\Delta\mu_y - \Delta\mu_x)$. Since $\sqrt{\sigma^2_L + \sigma^2} > \sigma_L$, it must be that $|\tilde{R}| < |R|$. Measurement error must lead to asymptotic attenuation bias.

Case 2: $\sigma_{H,x} = \sigma_{L,x} \equiv \sigma_x$, $\sigma_{H,y} = \sigma_{L,y} \equiv \sigma_y$, and $\sigma_{\eta,x} = \sigma_{\eta,y} \equiv \sigma$. Under these assumptions, $R = \frac{1}{\sqrt{2}}\left(\frac{\Delta\mu_y}{\sigma_y} - \frac{\Delta\mu_x}{\sigma_x}\right)$ and $\tilde{R} = \frac{1}{\sqrt{2}}\left(\frac{\Delta\mu_y}{\sqrt{\sigma_y^2+\sigma^2}} - \frac{\Delta\mu_x}{\sqrt{\sigma_x^2+\sigma^2}}\right)$. Suppose that $R > 0$. This implies that $\sigma_x^{-1}\Delta\mu_x < \sigma_y^{-1}\Delta\mu_y$. Differentiating $\tilde{R}$ with respect to $\tilde{\sigma}^2$ yields $\frac{\partial\tilde{R}}{\partial\tilde{\sigma}^2} \propto \frac{\Delta\mu_x}{(\sigma_x^2+\sigma^2)^{\frac{3}{2}}} - \frac{\Delta\mu_y}{(\sigma_y^2+\sigma^2)^{\frac{3}{2}}}$. Evaluated at $\sigma^2 = 0$, the derivative is proportional to $\frac{\partial\tilde{R}}{\partial\sigma^2}|_{\sigma=0} \propto \frac{\Delta\mu_x}{\sigma_x^3} - \frac{\Delta\mu_y}{\sigma_y^3}$. This derivative is positive when $\frac{\Delta\mu_x}{\Delta\mu_y} > \frac{\sigma_x^3}{\sigma_y^3}$. However, in order for $R > 0$, it must be that $\frac{\Delta\mu_x}{\Delta\mu_y} < \frac{\sigma_x}{\sigma_y}$. Therefore, if $\frac{\sigma_x}{\sigma_y} > \frac{\Delta\mu_x}{\Delta\mu_y} > \frac{\sigma_x^3}{\sigma_y^3}$, both $R > 0$ and $\frac{\partial\tilde{R}}{\partial\sigma^2}|_{\sigma=0} > 0$. But then, by continuity there must exist an interval $(0, \bar{\sigma})$ such that for all $\sigma \in (0, \bar{\sigma})$, $\tilde{\delta}_{x,y} > \delta_{x,y} > 0$. When $\sigma > \bar{\sigma}$, the measurement error will bias $\tilde{\delta}_{x,y}$ toward 0: $\delta_{x,y} > \tilde{\delta}_{x,y} > 0$. If $\frac{\Delta\mu_x}{\Delta\mu_y} < \frac{\sigma_x^3}{\sigma_y^3}$, then for all values of σ, $\tilde{\delta}_{x,y}$ is attenuated toward 0 relative to $\delta_{x,y}$. In either case, as σ becomes arbitrarily large, $\tilde{R} \to 0$.

Case 3: $\sigma_{H,x} = \sigma_{H,y} = \sigma_{L,x} = \sigma_{L,y} \equiv \upsilon$ and $\sigma^2 = \sigma_{\eta,x}^2 = \alpha^{-1}\sigma_{\eta,y}^2$. These assumptions imply $R = \frac{(\Delta\mu_y - \Delta\mu_x)}{\upsilon\sqrt{2}}$ and $\tilde{R} = \frac{1}{\sqrt{2}}\left(\frac{\Delta\mu_y}{\sqrt{\upsilon^2+\sigma^2}} - \frac{\Delta\mu_x}{\sqrt{\upsilon^2+\alpha\sigma^2}}\right)$. Evaluating $\frac{\partial\tilde{R}}{\partial\tilde{\sigma}^2}$ at $\tilde{\sigma} = 0$ gives an expression proportional to $\frac{1}{\upsilon^3}(\Delta\mu_x - \alpha\Delta\mu_y)$. Note that $R > 0 \implies \Delta\mu_y > \Delta\mu_x$. However, if $\alpha < \frac{\Delta\mu_x}{\Delta\mu_y}$, then $\tilde{R} > R$ will hold for all σ in some neighborhood of 0. If, by contrast, $\alpha > \frac{\Delta\mu_x}{\Delta\mu_y}$, then $\tilde{R}$ will always be less than R. If $R < 0$, then if $\alpha > \frac{\Delta\mu_x}{\Delta\mu_y}$, $\tilde{R} < R < 0$ for all σ in a neighborhood of 0. Therefore, both positive and negative bias is possible regardless of the sign of R. Furthermore, it is now possible for $\tilde{R}$ to have a different sign than R. Suppose that $R > 0$. $\tilde{R}$ is strictly decreasing in α and, as $\alpha \to \infty$, $\tilde{R} \to \frac{-\Delta\mu_x}{\sqrt{\upsilon^2+\sigma^2}}$. Therefore, for a large enough α, $R > 0 > \tilde{R}$. Analogously, as $\alpha \to 0$, $\tilde{R} \to \infty$, which means that for any $\delta_{x,y}$, $\tilde{\delta}_{x,y} \to 1$ as $\alpha \to 1$.

□

A.4. **Asymptotic Bias Simulation Procedure.** The online appendix reports test score reliabilities in the NLSY surveys. For each test s, I estimate $(\hat{\mu}_{s,t,G}, \hat{\sigma}_{s,t,G})$ for $G \in \{H, L\}$ and $t \in \{1979, 1997\}$ using the sample means and standard deviations. I draw random pseudo-samples of test scores of size N, for N large, from each distribution $N(\hat{\mu}_{s,t,G}, \hat{\sigma}_{s,t,G})$. I use these pseudosamples to estimate $\tilde{\delta}$. Then, if $R_{s,t}$ is the reliability of assessment s in year t, I generate "noiseless" pseudosamples drawn from $N(\hat{\mu}_{s,t,G}, \sqrt{R_{s,t}}\hat{\sigma}_{s,t,G})$ and use these pseudosamples to compute δ. I compute δ and $\hat{\delta}$ for

each achievement test for the whole range of possible reliabilities reported in Reardon (2011) and report the extrema of this procedure.

To simulate the bias stemming from income measurement error, I suppose the true distribution of income is lognormal with mean μ_t and variance σ_t^2 in both surveys t. I also assume that observed log income $\tilde{m}_{i,t}$ is equal to true log income plus a normally distributed classical measurement error $\eta_{i,t}$ with variance $\sigma_{\eta,t}$. Finally, I suppose that observed standardized test scores are linear in true log income: $s_{i,t} = a_t + B_t m_{i,t} + \varepsilon_{i,t},\ \mathbb{E}[\varepsilon_{i,t}|m_{i,t}] = 0,\ \varepsilon_{i,t} \sim N(0, \sigma_{\varepsilon,t})$. Under these assumptions, a linear regression of test scores on observed log income will recover an asymptotically biased estimate of B_t: $plim\ \hat{B}_t^{ols} = B_t R_t = B_t \left(\frac{\sigma_t^2}{\sigma_t^2 + \sigma_{\eta,t}^2}\right)$. If R^2 is the true share of variance explained, then the asymptotic share explained in the noisy regression is $\tilde{R}^2 = R_t R^2$. These facts imply that the following procedure will provide valid approximations for δ and $\tilde{\delta}$:

(1) Estimate $\hat{\tilde{\mu}}_t$ and $\hat{\tilde{\sigma}}_t$ from the sample means and standard deviations of the log income distributions. Then, for some large $N \in \mathbb{N}$, draw a sample $\{m_{i,t}\}$ of size N from $N(\hat{\tilde{\mu}}_t, R_m \hat{\tilde{\sigma}}_t^2)$. These $\{m_{i,t}\}$ are the "clean" income values.

(2) Run a linear regression of $s_{i,t}$ on the observed log income values. Using the $\tilde{R}^2$ from this regression, simulate a population of errors $\{\varepsilon_{i,t}\}$ of size N by drawing a random sample from $N\left(0, \frac{1-\tilde{R}^2}{R_m}\right)$. Then, for each $m_{i,t}$ in the created sample from step 1, simulate a test score via $s_{i,t} = R_m \hat{B}_t^{ols} m_{i,t} + \varepsilon_{i,t}$.

(3) Create a virtual population of noisy incomes $\{\tilde{m}_{i,t}^v\}$ of size N via $\tilde{m}_{i,t}^v = m_{i,t} + \eta_{i,t}$, where the $\eta_{i,t}$ are iid draws from $N(0, (1 - R_m)\hat{\tilde{\sigma}}_t^2)$. Repeat these steps for the other survey.

(4) For the clean data, calculate $\hat{\delta}$ from the scores that correspond to incomes in the top 20 percent and bottom 20 percent of the true income distributions for years t and $t+1$. For the noisy data, calculate $\hat{\tilde{\delta}}$ analogously using the noisy income distribution and compute $\text{Bias}(R_m) = \frac{\hat{\delta} - \hat{\tilde{\delta}}}{\hat{\delta}}$.

A.5. **Proof of Theorem 1.**

Proof. The income-achievement gap in year $k \in \{t-1, t\}$ is $V(\Omega_{k,H}, \Gamma) - V(\Omega_{k,L}, \Gamma)$, where $V(\Omega, \Gamma) \equiv \int \Gamma(s) d\Omega(s)$. Suppose that $\Omega_{t-1,H} \succsim \Omega_{t,H}$. Since Γ is strictly increasing, it must be that $V(\Omega_{t-1,H}, \Gamma) \geq V(\Omega_{t,H}, \Gamma)$ with the inequality strict eactly when $\Omega_{t-1,H} \succ \Omega_{t,H}$. Similarly, if $\Omega_{t,L} \succsim \Omega_{t-1,L}$ then $V(\Omega_{t,L}, \Gamma) \geq V(\Omega_{t-1,L}, \Gamma)$ with the inequality strict exactly when $\Omega_{t,L} \succ \Omega_{t-1,L}$. Putting this together, the change in the income achievement gap is

$$[V(\Omega_{t,H}, \Gamma) - V(\Omega_{t-1,H}, \Gamma)] - [V(\Omega_{t,L}, \Gamma) - V(\Omega_{t-1,L}, \Gamma)] \leq 0$$

This inequality will be strict if either FOSD relationships is strict. To prove the claim about PPCs, note first that the y-axis of the PPC for survey $k \in \{t-1, t\}$ can be written as a function of the low-group test score percentile p: $PPC_k(p) \equiv \Omega_{k,H}(\Omega_{k,L}^{-1}(p))$. The two FOSD conditions on high- and low-income score distributions imply $\Omega_{t,L}^{-1}(p) \geq \Omega_{t-1,L}^{-1}(p)$, $\forall p$ and $\Omega_{t,H}(s) \geq \Omega_{t-1,H}(s)$, $\forall s$. Together, these inequalities imply that $\Omega_{t,H}(\Omega_{t,L}^{-1}(p)) \geq \Omega_{t-1,H}(\Omega_{t-1,L}^{-1}(p)), \forall p$. Moreover, these inequalities will be strict on $(0, 1)$ if both FOSD conditions are strict.

□

A.6. **Life Outcomes Data and Method.**

A.6.1. *Data.* Calculating pdv_labor for each respondent is complicated by three forms of missing data. First, only some respondents are asked about their income in each year, and not every respondent who is asked provides a valid response. Second, not every survey respondent is in the labor force in a given year. Third, after 1994, the respondents were only interviewed every other year, so income data is missing for odd-numbered years between 1994 and 2010. I address the first two kinds of missing data through the imputation rules described in Section 8. I address the third form of missing data by linearly interpolating wage income values for the odd-numbered years between 1995 and 2009 after applying one of my three imputation rules.

The NLS computes hourly wage rates for each wave of both NLSY surveys using reported annual wage income and reported annual hours worked. I use the "hourly rate

of pay – job #1-5" variables in my analyses. I construct an annual average hourly wage rate for each respondent by simply averaging the reported hourly wages across all of the respondent's jobs.

The age-earnings profiles of men with different education levels are not simply log-level shifts of each other. Highly educated men experience much more rapid wage income growth in percentage terms between the ages of 20 and 50. To account for these differences, I use Census data from 2005 to construct synthetic age-earnings profiles for men with different education levels.[36] I use the mean earnings of men in several age buckets (18-24, 25-34, 35-44, 45-54, 55-64, and 65+) crossed with several education categories (<high school, high school, and college+). Since the synthetic data are computed for full-time, year-round workers over the age of 18, they are more directly applicable to the estimates that assume no involuntary unemployment. As a robustness check, I also use median earnings data for white men from the ACS unconditional on full-time status. The estimates made using these alternate data tell qualitatively the same story about the value of achievement shifts between high- and low-income white men.[37]

I use data bucketed into 5- and 10-year increments, as doing so appears to affect my final estimates very little relative to using more granular age-earnings data. Let $m_{e,a,a+1}$ be the slope of the earnings line connecting the labor income in age buckets a and $a+1$ for education category $e \in \{<\text{ high school, high school, college+}\}$, and let $\tilde{w}_{i,t,k}$ be the (imputed) annual wage income for respondent i in survey wave t using imputation rule $k \in \{\text{Reas}, \text{Pess}, \text{Opt}\}$. The final wave I observe in the NLSY79 is $t = 31$ (if 1979 is $t = 0$), at which point the NLSY79 respondents in my sample are between 45 and 47 years old. Since most workers retire between the ages of 60 and 70, I assume that each of my NLSY respondents will work until age 65 and then retire. I calculate the expected annual wage income of i in year 2011, $\hat{w}_{i,2011,k}$, by running a

[36]These data are available at `https://www.census.gov/hhes/www/income/data/historical/people/`.

[37]The online appendix shows the age-earnings profiles from these two data sources.

quadratic regression of t on $\tilde{w}_{i,t,k}$ for $t \in \{2000, 2010\}$ separately for each i.[38] I assume that i's yearly income increases and decreases from $\hat{w}_{i,2011,k}$ between the ages of 45 and 65 in accordance with the slopes $\{m_{e(i),a,a+1}\}$, where $e(i)$ is the education level of i. Putting all of this together, the pdv of a youth who was 15 at the start of the NLSY79 is given by

$$\begin{aligned} PDV_{i,k} \equiv & \underbrace{\sum_{t=0}^{t=31} (0.95)^t \, \tilde{w}_{i,t,k}}_{\text{observed}} + \underbrace{\hat{w}_{i,2011,k} \sum_{j=1}^{10} (0.95)^{31+j} \left(1 + m_{e(i),35,45} j\right)}_{\text{projected, age 46-55}} \\ + & \underbrace{\hat{w}_{i,2011,k} \left(1 + 10 m_{e(i),35,45}\right) \sum_{j=1}^{10} (0.95)^{41+j} \left(1 + m_{e(i),45,55} j\right)}_{\text{projected, age 56-65}} . \end{aligned}$$

As a robustness check, I rerun the pdv_chosen estimates while assuming that wage growth for each individual tracks the average (or median) growth in her educational group in levels, not slopes. This alternate projection procedure does not qualitatively affect the gap-change estimates.

Both NLSY surveys record the highest grade completed for each respondent in each survey wave. I construct a new variable for each survey wave t equal to the largest highest grade completed observed in any wave up to and including t. Occasionally, the highest grade completed for a respondent will decrease between one survey and the next. These data are difficult to interpret; my "fill-in" rule assumes that the lower value is incorrect. I only use the grade-completion variables up to 14 years after the start of the survey, as this is as far out as I can go in the NLSY97. Very few people change their education status after age 30 in the NLSY79, so this restriction should have little effect on my estimates.

[38] That is, if $(\hat{a}_{i,k}, \hat{\beta}_{1,i,k}, \hat{\beta}_{2,i,k})$ are the OLS estimates of $\hat{w}_{i,t,k} = \alpha_{i,k} + \beta_{1,i,k} t + \beta_{2,i,k} t^2$, $t \in \{2000, \ldots, 2010\}$, the projected income for i in 2011 is $\hat{w}_{i,2011,k} = \hat{\alpha}_{i,k} + \hat{\beta}_{1,i,k} 2011 + \hat{\beta}_{2,i,k} 2011^2$.

A.6.2. *Method.* Implementing the procedure outlined in Section 8 requires that I estimate the conditional cdf $F_{79}(y|s)$ for each $s \in [\underline{s}, \bar{s}]$ and the marginal test score distributions $\Omega_{t,G}$ for each $t \in \{79, 97\}$ and $G \in \{H, L\}$. I estimate the marginal test score distributions using a smoothed kernel density estimator. I estimate $\hat{F}_{79,t}(y|s)$ in two steps. First, I estimate polynomial quantile regressions of the form $y^{(\tau)} = \alpha^{(\tau)} + \beta_1^{(\tau)} s + \beta_2^{(\tau)} s^2 + \ldots \beta_n^{(\tau)} s^n$ for each $\tau \in \{\tau_1, \ldots, \tau_M\}$, where $0 < \tau_i < \tau_{i+1} < 1,\ 1 \leq i \leq M-1$. My baseline estimates use cubic quantile regressions ($n = 3$); higher-order polynomials produce very similar estimates. I use the estimated quantile regressions to estimate $\tilde{Q}_{79}(u|s)$, the quantile function of y conditional on s. Since the quantile regressions do not guarantee that the resulting $\tilde{Q}_{79}(u|s)$ is monotone in u, I estimate $\hat{F}_{79}(y|s) = \int_0^1 \mathbb{I}\left(\tilde{Q}_{79}(u|s) \leq y\right) du$. Even if $\tilde{Q}_{79}(u|s)$ is not monotone, $\hat{F}_{79}(y|s)$ will be. Finally, I use cubic b-splines to smooth out the stepwise function defined by the above integral. The derivative of this smoothed cdf yields a smooth estimate of the conditional density of y given s, $\hat{f}_{79}(y|s)$.

The above method will not work for binary outcomes. The simplest method for dealing with binary outcomes is to estimate a probit (or logit) of y on a polynomial in s. The estimated parameters of this probit can then be used to directly estimate $\hat{\mathbb{E}}[y|s]$ because $\hat{\mathbb{E}}[y|s] = \widehat{Pr}(y = 1|s) = \Phi\left(\hat{\alpha} + \hat{\beta}_1 s + \ldots + \hat{\beta}_M s^M\right)$. This method is less flexible than the one I employ for continuously distributed outcomes.

My empirical work allows the estimated relationship between s and y to depend on student characteristics. In particular, I estimate $\hat{\mathbb{E}}[y|s, x]$ for a student with characteristics x via two methods. My baseline specification estimates $\hat{\mathbb{E}}_x[y|s]$ separately for each $x \in \{\{\text{Black}, \text{White}\} \times \{\text{Male}, \text{Female}\}\}$. If y is continuously distributed, this amounts to estimating the quantile regressions $y^{(\tau,x)} = \alpha^{(\tau,x)} + \beta_1^{(\tau,x)} s + \ldots + \beta_N^{(\tau,x)} s^N$ separately for each x. I also run specifications in which I include race and gender dummies in the (quantile) regressions used to construct $\hat{F}(y|s)$. For continuously distributed y, this amounts to estimating quantile regressions of the form $y^{(\tau)} = \alpha^{(\tau)} + \beta_1^{(\tau)} s + \beta_2^{(\tau)} s^2 + \ldots + \beta_n^{(\tau)} s^n + \gamma_x^{(\tau)} \mathbb{I}(X = x)$. The estimated gap changes using

the dummy methodology for white males are generally slightly closer to 0 than the fully-interacted estimates.

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Appendix B. Tables

Table I. Summary Statistics

Variable	Survey	N	Mean	Median	S.D.
Math	NLSY79	3,277	96.77	95	18.23
Math	NLSY97	2,833	98.74	99	18.82
Reading	NLSY79	3,277	94.19	98	19.32
Reading	NLSY97	2,833	93.41	98	20.39
AFQT	NLSY79	3,277	142.57	146	26.94
AFQT	NLSY97	2,833	142.88	147.4	28.11
Income	NLSY79	3,388	$44,000	$39,800	$28,700
Income	NLSY97	3,570	$54,700	$43,100	$49,500
Age	NLSY79	3,388	16.08	16	0.78
Age	NLSY97	3,570	15.76	16	0.72
Black	NLSY79	3,388	0.14	0	0.35
Black	NLSY97	3,570	0.15	0	0.36

Note: Respondent ages are restricted to 15-17 as of ASVAB test date. All dollars have been converted to a 1997 basis using the CPI-U. The N shown for a variable is the sample size used in calculations involving that variable.

Table II. $\hat{\delta}$ Estimates

Income Percentiles	Race	Math	Reading	AFQT
[80-100] vs. [0-20]	All	0.08* (-0.05, 0.21)	0.19*** (0.08, 0.34)	0.17*** (0.06, 0.32)
[80-100] vs. [20-40]	All	0.13** (0.01, 0.26)	0.13** (0.02, 0.28)	0.14*** (0.02, 0.29)
[90-100] vs. [0-10]	All	0.04 (-0.16, 0.28)	0.20*** (0.04, 0.38)	0.17** (0.00, 0.37)
[90-100] vs. [10-20]	All	0.18** (-0.02, 0.37)	0.26*** (0.08, 0.41)	0.24*** (0.06, 0.41)
[80-100] vs. [0-20]	White	0.05 (-0.08, 0.23)	0.17***(0.03, 0.30)	0.15**(0.01, 0.30)
[80-100] vs. [20-40]	White	0.23***(0.09, 0.35)	0.28***(0.14, 0.40)	0.28***(0.13, 0.40)
[90-100] vs. [0-10]	White	0.12 (-0.12, 0.33)	0.23***(0.05, 0.40)	0.19**(0.01, 0.39)
[90-100] vs. [10-20]	White	-0.03 (-0.25, 0.24)	-0.01 (-0.20, 0.20)	-0.01 (-0.21, 0.21)
[80-100] vs. [0-20]	Black	-0.27**(-0.45, 0.01)	-0.23** (-0.43, 0.02)	-0.26** (-0.44, 0.02)
[80-100] vs. [20-40]	Black	-0.16**(-0.41, 0.09)	-0.09 (-0.36, 0.12)	-0.11 (-0.38, 0.09)
[90-100] vs. [0-10]	Black	-0.24 (-0.45, 0.24)	0.02 (-0.29, 0.43)	-0.05 (-0.35, 0.41)
[90-100] vs. [10-20]	Black	-0.20 (-0.48, 0.25)	-0.14 (-0.46, 0.26)	-0.23 (-0.50, 0.25)

Note: 95% confidence intervals calculated using 2,500 bootstrap iterations shown in parentheses. (***) = one-sided hypothesis test significant at 1%; (**) = significant at 5%; (*) = significant at 10%. All calculations use cross-sectional weights for the ASVAB test year. Race-specific results subset on race prior to defining the high- and low-income categories.

Table III. $\hat{\delta}$ Estimates with Income Categories Defined First

Income Percentiles	Race	Math	Reading	AFQT
[80-100] vs. [0-20]	White	0.05 (-0.10, 0.22)	0.16** (0.02, 0.33)	0.14** (0.00, 0.32)
[80-100] vs. [20-40]	White	0.10* (-0.04, 0.24)	0.09* (-0.04, 0.26)	0.10* (-0.03, 0.26)
[90-100] vs. [0-10]	White	0.04 (-0.17, 0.30)	0.19** (-0.02, 0.40)	0.16* (-0.06, 0.37)
[90-100] vs. [10-20]	White	0.13 (-0.10, 0.37)	0.19** (0.01, 0.39)	0.18** (-0.03, 0.40)
[80-100] vs. [0-20]	Black	0.07 (-0.22, 0.40)	0.21* (-0.07, 0.53)	0.21* (-0.07, 0.51)
[80-100] vs. [20-40]	Black	0.15 (-0.17, 0.45)	0.21* (-0.06, 0.53)	0.21* (-0.09, 0.53)
[90-100] vs. [0-10]	Black	0.15 (-0.11, 0.59)	0.10 (-0.29, 0.62)	0.14 (-0.27, 0.63)
[90-100] vs. [10-20]	Black	0.20 (-0.17, 0.63)	0.21 (-0.33, 0.73)	0.26 (-0.24, 0.73)

Note: 95% confidence intervals calculated using 2,500 bootstrap iterations shown in parentheses. (**) = one-sided hypothesis test significant at 5%; (*) = significant at 10%. "Income Categories Defined First" means that the H and L groups are defined relative to the full sample income distribution rather than the race-specific income distribution. All calculations use cross-sectional weights for the ASVAB test year.

Table IV. $\hat{\delta}$ Estimates Using Common Income Cutoffs

Income Percentiles	Race	Math	Reading	AFQT
[80-100] vs. [0-20]	All	0.10* (-0.03, 0.24)	0.26*** (0.16, 0.38)	0.24*** (0.13, 0.37)
[80-100] vs. [20-40]	All	0.16*** (0.04, 0.28)	0.19*** (0.09, 0.30)	0.20*** (0.09, 0.31)
[90-100] vs. [0-10]	All	0.08 (-0.14, 0.36)	0.35*** (0.22, 0.50)	0.31*** (0.16, 0.48)
[90-100] vs. [10-20]	All	0.20** (-0.04, 0.41)	0.34*** (0.16, 0.47)	0.33*** (0.14, 0.48)
[80-100] vs. [0-20]	White	0.07 (-0.07, 0.24)	0.20*** (0.07, 0.33)	0.19*** (0.06, 0.32)
[80-100] vs. [20-40]	White	0.24*** (0.10, 0.38)	0.29*** (0.14, 0.41)	0.30*** (0.16, 0.42)
[90-100] vs. [0-10]	White	0.17* (-0.05, 0.38)	0.31*** (0.12, 0.47)	0.28*** (0.09, 0.46)
[90-100] vs. [10-20]	White	0.02 (-0.22, 0.27)	0.06 (-0.14, 0.25)	0.07 (-0.15, 0.28)
[80-100] vs. [0-20]	Black	-0.17* (-0.42, 0.04)	-0.16* (-0.39, 0.07)	-0.20* (-0.44, 0.07)
[80-100] vs. [20-40]	Black	-0.04 (-0.33, 0.20)	-0.07 (-0.35, 0.16)	-0.08 (-0.35, 0.17)
[90-100] vs. [0-10]	Black	-0.02* (-0.28, 0.37)	0.30*** (0.08, 0.61)	0.26** (0.00, 0.57)
[90-100] vs. [10-20]	Black	0.00 (-0.28, 0.38)	-0.01 (-0.23, 0.39)	0.03 (-0.24, 0.41)

Note: 95% confidence intervals calculated using 2,500 bootstrap iterations shown in parentheses. (***) = one-sided hypothesis test significant at 1%; (**) = significant at 5%; (*) = significant at 1%. "Common Income Cutoffs" means that the H and L groups are defined using percentile cutoffs from the combined income distribution of the two NLSY surveys. All calculations use cross-sectional weights for the ASVAB test year. Race-specific results subset on race prior to defining the high- and low-income categories.

Table V. Cross-Sectional $\hat{\delta}$'s

Income Percentiles	Race	Math	Reading	AFQT
[80-100] vs. [0-20]	All	0.07** (-0.01, 0.16)	0.13*** (0.05, 0.24)	0.11*** (0.03, 0.21)
[80-100] vs. [20-40]	All	0.11** (0.00, 0.22)	0.10** (-0.01, 0.24)	0.11** (0.01, 0.25)
[90-100] vs. [0-10]	All	0.05 (-0.05, 0.17)	0.11** (0.01, 0.22)	0.10** (0.00, 0.20)
[90-100] vs. [10-20]	All	0.14** (0.02, 0.26)	0.17*** (0.04, 0.29)	0.16*** (0.04, 0.28)
[80-100] vs. [0-20]	White	0.06 (-0.04, 0.19)	0.13** (0.02, 0.25)	0.12** (0.00, 0.24)
[80-100] vs. [20-40]	White	0.21*** (0.07, 0.33)	0.26*** (0.13, 0.38)	0.26*** (0.13, 0.38)
[90-100] vs. [0-10]	White	0.10* (-0.05, 0.26)	0.20** (0.02, 0.34)	0.17** (0.01, 0.31)
[90-100] vs. [10-20]	White	-0.02 (-0.17, 0.17)	-0.03 (-0.20, 0.15)	-0.02 (-0.18, 0.16)
[80-100] vs. [0-20]	Black	-0.25*** (-0.41, -0.06)	-0.20** (-0.36, -0.01)	-0.22** (-0.37, -0.03)
[80-100] vs. [20-40]	Black	-0.11 (-0.33, 0.07)	-0.07 (-0.30, 0.10)	-0.09 (-0.31, 0.08)
[90-100] vs. [0-10]	Black	-0.24* (-0.42, 0.06)	-0.05 (-0.26, 0.23)	-0.09 (-0.28, 0.21)
[90-100] vs. [10-20]	Black	-0.22 (-0.42, 0.09)	-0.16 (-0.39, 0.13)	-0.19 (-0.40, 0.11)

Note: 95% confidence intervals calculated using 2,500 bootstrap iterations shown in parentheses. (***) = one-sided hypothesis test significant at 1%; (**) = significant at 5%. All calculations use cross-sectional weights for the ASVAB test year. Race-specific results subset on race prior to defining the high- and low-income categories.

Table VI. Simulated Measurement Error Bias

Test	Type	Minimum Bias	Maximum Bias
Math	Test Score	-16%	16%
Reading	Test Score	-11%	-23%
AFQT	Test Score	-7%	-23%
Math	Income	-7%	-17%
Reading	Income	-8%	-17%
AFQT	Income	-7%	-20%

Note: The lower and upper limits are calculated by taking optimistic and pessimistic estimates for the assessment reliabilities for each NLSY assessment. The narrow range of bias in math is due to the narrow range of reported reliabilties for the AFQT math subtest.

Table VII. $\hat{\delta}$ Estimates Using Size and Composition Adjusted Income

Income Percentiles	Adjustment	Math	Reading	AFQT
[80-100] vs. [0-20]	$\theta = \gamma = 1$	0.01 (-0.12, 0.15)	0.17*** (0.06, 0.29)	0.16*** (0.04, 0.29)
[80-100] vs. [20-40]	$\theta = \gamma = 1$	0.00 (-0.13, 0.12)	0.08 (-0.04, 0.19)	0.06 (-0.06, 0.18)
[90-100] vs. [0-10]	$\theta = \gamma = 1$	0.09 (-0.12, 0.30)	0.28*** (0.12, 0.46)	0.27*** (0.08, 0.44)
[90-100] vs. [10-20]	$\theta = \gamma = 1$	0.16 (-0.07, 0.35)	0.23*** (0.07, 0.40)	0.24*** (0.05, 0.40)
[80-100] vs. [0-20]	$\theta = \gamma = 0.7$	0.04 (-0.09, 0.17)	0.23*** (0.10, 0.33)	0.22*** (0.07, 0.31)
[80-100] vs. [20-40]	$\theta = \gamma = 0.7$	0.03 (-0.10, 0.14)	0.11** (-0.01, 0.22)	0.09* (-0.02, 0.21)
[90-100] vs. [0-10]	$\theta = \gamma = 0.7$	0.07 (-0.14, 0.30)	0.33*** (0.16, 0.50)	0.29*** (0.10, 0.46)
[90-100] vs. [10-20]	$\theta = \gamma = 0.7$	0.18** (-0.02, 0.39)	0.29*** (0.11, 0.44)	0.28*** (0.09, 0.44)
[80-100] vs. [0-20]	Census	0.04 (-0.09, 0.17)	0.21*** (0.08, 0.32)	0.20*** (0.07, 0.32)
[80-100] vs. [20-40]	Census	0.03 (-0.10, 0.14)	0.10* (-0.02, 0.20)	0.09* (-0.04, 0.19)
[90-100] vs. [0-10]	Census	0.09 (-0.11, 0.33)	0.34*** (0.16, 0.49)	0.30*** (0.11, 0.48)
[90-100] vs. [10-20]	Census	0.19** (-0.01, 0.39)	0.28*** (0.11, 0.45)	0.27*** (0.11, 0.45)

Note: 95% confidence intervals calculated using 2,500 bootstrap iterations shown in parentheses. (***) = one-sided hypothesis test significant at 1%; (*) = significant at 10%. All calculations use cross-sectional weights for the ASVAB test year.

Table VIII. FOSD Tests of Crosswalked Score Distributions

	Low	High	White, Low	White, High	Black, Low	Black, High
Math, 1980	0.00	0.49	0.09	0.75	0.85	0.00
Math, 1997	0.00	0.47	0.38	0.09	0.00	0.52
Reading, 1980	0.03	0.87	0.65	0.81	0.98	0.03
Reading, 1997	0.31	0.05	0.66	0.01	0.03	0.89
AFQT, 1980	0.00	0.72	0.54	0.77	0.99	0.01
AFQT, 1997	0.06	0.10	0.66	0.01	0.00	0.84

Note: Each cell represents the probability of observing the empirical score distributions under the null that the row distribution dominates the column distribution. The column distribution is just the same achievement test from the other NLSY survey.

Table IX. Life Outcome Summary Statistics

Variable	Group	Imputation	Sample	Mean	Median	S.D.
pdv_chosen	White Men	R	1979	$660,622	$464,11	$704,703
pdv_chosen	White Men	O	1979	$744,669	$539,103	$1,047,215
pdv_chosen	White Men	P	1979	$446,719	$385,020	$259,882
pdv_fixed	White Men	R	1979	$578,148	$438,825	$563,953
pdv_fixed	White Men	O	1979	$657,067	$528,408	$485,578
pdv_fixed	White Men	P	1979	$425,897	$351,897	$338,197
High School	White Men	O	1979	0.86	1	0.34
High School	White Men	O	1997	0.89	1	0.31
College	White Men	O	1979	0.25	0	0.43
College	White Men	O	1997	0.31	0	0.46

Note: All figures deflated to 1997 basis using CPI-U. PDV calculations use a 5% discount rate. Statistics shown only for white males. R = linear imputation, O = optimistic imputation, P = pessimistic imputation. pdv_chosen = pdv_labor when labor supply is fully chosen, pdv_fixed = pdv_labor when labor supply is never chosen. All calculations use cross-sectional weights for the ASVAB test year.

Table X. Mean Lifetime Labor Wealth Gap Changes for White Men

Outcome	Income	Subject	Pessimistic	No Selection	Optimistic
pdv_chosen	L	Math	-$44,237 ($37,214)	-$44,481 ($37,459)	-$56,277 ($47,832)
pdv_chosen	H	Math	-$33,769 ($39,153)	-$29,891 ($39,718)	-$34,500 ($35,726)
change	–	Math	-$10,468 ($59,951)	-$14,591 ($55,623)	-$21,777 ($70,343)
pdv_fixed	L	Math	-$22,389 ($30,927)	-$35,710 ($43,380)	-$42,382 ($42,545)
pdv_fixed	H	Math	-$35,634 ($41,380)	-$45,495 ($54,154)	-$35,684 ($48,281)
change	–	Math	$13,245 ($56,675)	-$9,785 ($77,755)	-$6,698 ($64,386)
pdv_chosen	L	Read	$9,249 ($36,002)	$8,918 ($37,858)	$10,322 ($46,387)
pdv_chosen	H	Read	-$36,969 ($44,872)	-$34,804 ($47,518)	-$41,412 ($57,010)
change	–	Read	$46,218 ($61,201)	$43,722 ($62,763)	$51,734 ($75,139)
pdv_fixed	L	Read	$11,768 ($32,074)	$13,486 ($41,082)	$13,891 ($44,068)
pdv_fixed	H	Read	-$44,375 ($48,545)	-$55,863 ($60,344)	-$48,123 ($59,619)
change	–	Read	$56,144 ($61,612)	$69,349 ($79,375)	$62,015 ($76,561)
pdv_chosen	L	AFQT	$1,564 ($32,296)	$1,451 ($33,876)	$1,757 ($41,201)
pdv_chosen	H	AFQT	-$33,401 ($40,171)	-$30,247 ($40,717)	-$34,564 ($50,286)
change	–	AFQT	$34,965 ($35,448)	$31,699 ($51,757)	$36,321 ($61,802)
pdv_fixed	L	AFQT	$7,517 ($25,478)	$5,571 ($34,713)	$6,979 ($36,519)
pdv_fixed	H	AFQT	-$39,601 ($42,074)	-$45,926 ($52,045)	-$40,276 ($53,437)
change	–	AFQT	$47,118 ($49,639)	$51,497 ($61,591)	$47,255 ($58,573)

Note: Based on 500 quantile regressions and a grid of 1500 evenly-spaced points on the outcome measure. All figures deflated to 1997 basis using CPI-U. PDV calculations use a 5% discount rate. Standard errors based on 50 bootstrap iterations assuming normality. All calculations use cross-sectional weights for the ASVAB test year.

Table XI. Median Lifetime Labor Wealth Gap Changes for White Men

Outcome	Subject	10th	25th	50th	75th	90th
chosen, L	Math	$13,378 ($4,819)	$13,840 ($4,001)	$17,926 ($4,630)	$26,435 ($7,008)	$40,105 ($16,183)
chosen, H	Math	-$15,215 ($4,951)	-$16,141 ($4,373)	-$32,141 ($5,219)	-$41,343 ($9,161)	-$74,731 ($21,853)
change	Math	$28,592 ($5,529)	$29,981 ($4,675)	$50,068 ($6,906)	$67,778 ($10,067)	$114,836 ($28,995)
fixed, L	Math	$11,406 ($7,084)	$22,958 ($6,960)	$36,025 ($7,090)	$41,258 ($11,111)	$69,917 ($38,863)
fixed, H	Math	-$16,552 ($5,240)	-$16,458 ($6,686)	-$20,547 ($8,907)	-$43,334 ($17,488)	-$77,583 ($34,163)
change	Math	$27,958 ($7,662)	$39,417 ($8,008)	$56,572 ($9,467)	$84,592 ($18,953)	$147,501 ($35,031)
chosen, L	Read	$6,735 ($2,929)	$9,226 ($1,579)	$11,620 ($3,131)	$18,750 ($4,237)	$23,289 ($9,038)
chosen, H	Read	-$19,291 ($7,504)	-$16,141 ($5,039)	-$31,474 ($6,000)	-$47,407 ($11,173)	-$117,202 ($39,230)
change	Read	$26,026 ($7,342)	$25,367 ($5,663)	$43,095 ($6,312)	$66,157 ($12,606)	$140,491 ($39,612)
fixed, L	Read	$11,874 ($3,069)	$13,211 ($2,402)	$20,960 ($5,044)	$17,294 ($67,22)	$25,971 ($22,053)
fixed, H	Read	-$17,636 ($9,083)	-$20,982 ($9,835)	-$20,100 ($9,987)	-$67,565 ($22,935)	-$193,521 ($48,260)
change	Read	$29,510 ($9,430)	$34,193 ($9,075)	$41,059 ($9,447)	$84,859 ($23,444)	$219,491 ($48,927)
chosen, L	AFQT	$12,335 ($4,448)	$13,9367 ($2,312)	$17,801 ($5,470)	$22,422 ($6,563)	$31,929 ($9,864)
chosen, H	AFQT	-$8,724 ($4,733)	-$10,685 ($4,075)	-$20,331 ($4,866)	-$31,707 ($7,151)	-$76,343 ($21,126)
change	AFQT	$21,059 ($4,336)	$24,622 ($4,579)	$38,132 ($5,807)	$54,129 ($9,480)	$108,272 ($24,932)
fixed, L	AFQT	$11,036 ($5,190)	$21,731 ($3,763)	$30,666 ($5,454)	$29,467 ($8,719)	$53,985 ($29,584)
fixed, H	AFQT	-$9,928 ($5,707)	-$13,682 ($5,782)	-$9,831 ($5,988)	-$35,519 ($11,372)	-$176,736 ($33,965)
change	AFQT	$20,964 ($7,290)	$35,413 ($6,365)	$40,497 ($6,966)	$64,986 ($13,748)	$230,721 ($40,213)

Note: Based on 1,000 quantile regressions and a grid of 1,000 evenly-spaced points on the outcome measure. All figures deflated to 1997 basis using CPI-U. PDV calculations use a 5% discount rate. Standard errors in parentheses are calculated from 200 bootstrap samples assuming normality. All calculations use cross-sectional weights for the ASVAB test year.

Table XII. Probit-Estimated Mean School Completion Gap-Change Estimates

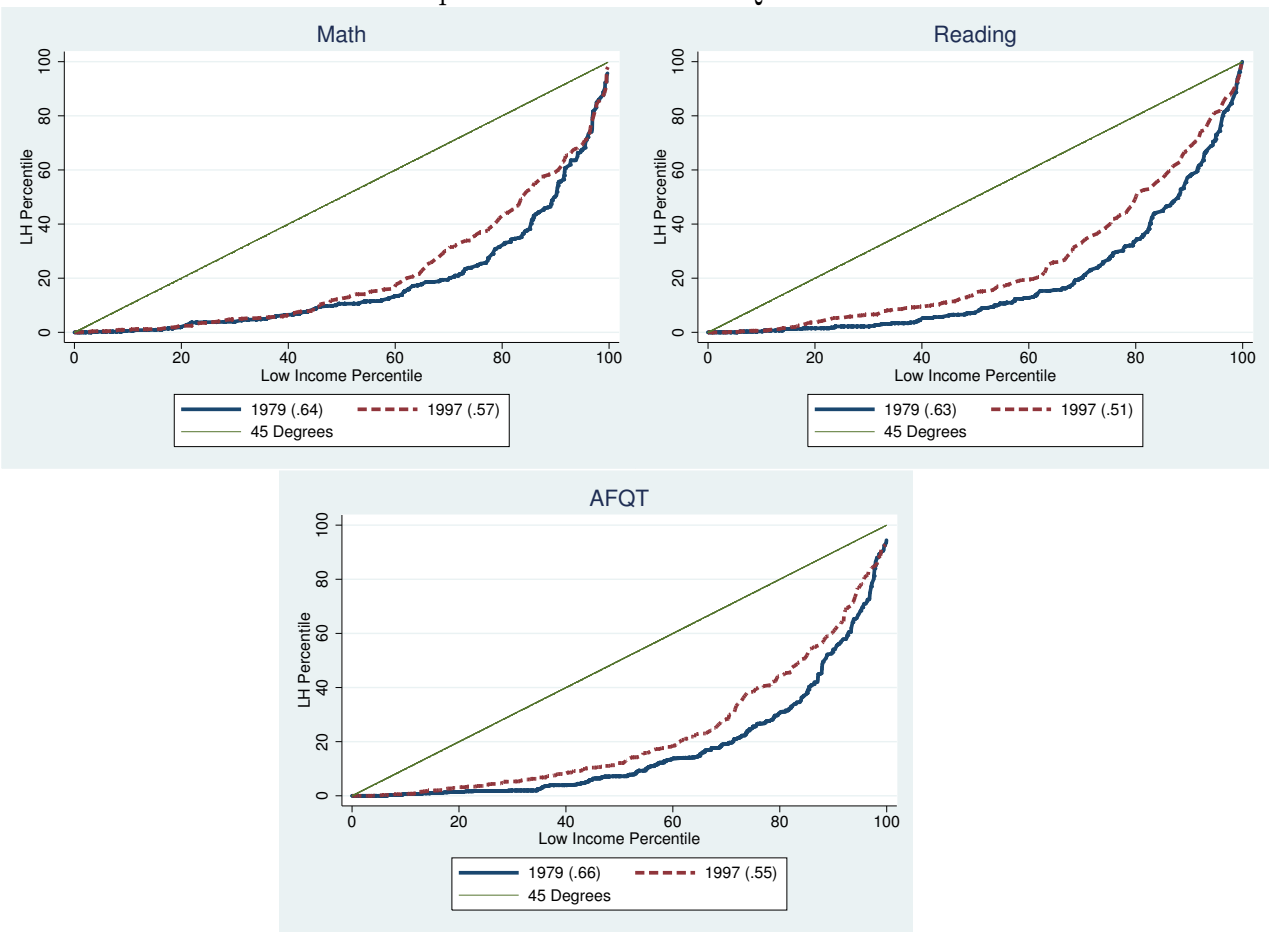
Outcome	Group	Skill Prices	Math	Reading	AFQT
High School	White Men	NLSY79	0.05 (0.03)	0.06 (0.03)	0.06 (0.04)
High School	White Men	NLSY97	0.02 (0.02)	0.05 (0.03)	0.04 (0.03)
College	White Men	NLSY79	0.07 (0.03)	0.07 (0.03)	0.07 (0.03)
College	White Men	NLSY97	0.06 (0.02)	0.07 (0.03)	0.07 (0.03)
High School	White Women	NLSY79	-0.02 (0.03)	-0.01 (0.03)	-0.01 (0.03)
High School	White Women	NLSY97	-0.02 (0.02)	-0.01 (0.02)	-0.01 (0.02)
College	White Women	NLSY79	0.02 (0.02)	-0.01 (0.03)	0.02 (0.03)
College	White Women	NLSY97	0.02 (0.03)	-0.02 (0.03)	0.01 (0.03)
High School	Black Men	NLSY79	-0.08 (0.08)	0.00 (0.08)	-0.02 (0.08)
High School	Black Men	NLSY97	-0.05 (0.08)	0.01 (0.08)	-0.00 (0.09)
College	Black Men	NLSY79	-0.03 (0.07)	0.04 (0.07)	0.01 (0.07)
College	Black Men	NLSY97	-0.01 (0.07)	0.00 (0.04)	-0.01 (0.04)
High School	Black Women	NLSY79	-0.04 (0.06)	-0.04 (0.06)	0.00 (0.06)
High School	Black Women	NLSY97	-0.03 (0.06)	-0.04 (0.07)	0.00 (0.07)
College	Black Women	NLSY79	-0.10 (0.07)	-0.06 (0.05)	-0.07 (0.06)
College	Black Women	NLSY97	-0.09 (0.08)	-0.06 (0.07)	-0.07 (0.08)

Note: Based on 500 probit regressions and a grid of 3000 evenly-spaced points on the outcome measure. Results use only optimistically-imputed data. Confidence intervals based on 200 bootstrap iterations assuming normality. All calculations use cross-sectional weights for the ASVAB test year.

Appendix C. Figures

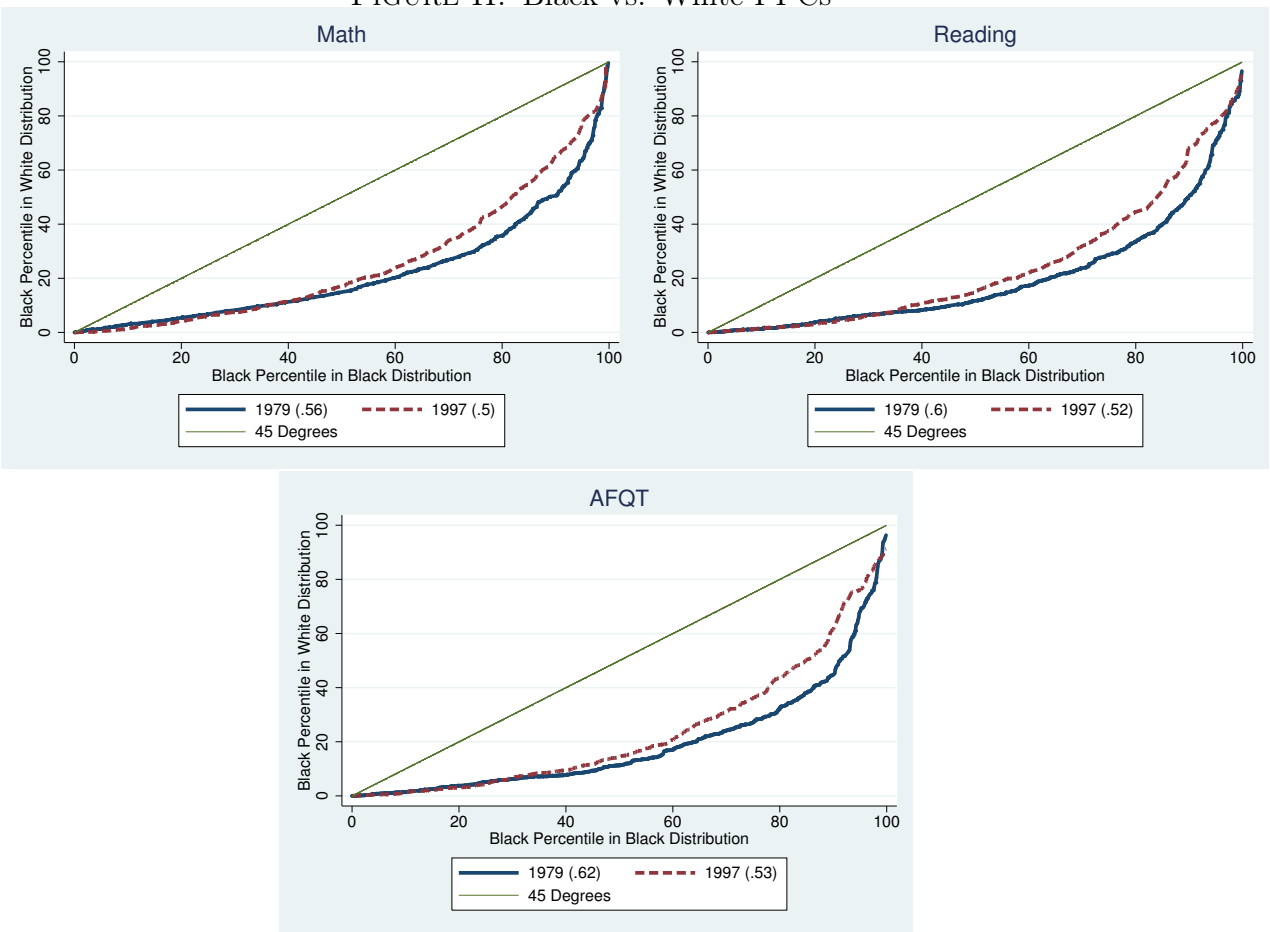
Borrowing terminology from Lorenz curves, I define the pseudo-Gini coefficient for a PPC by $1-2\int_0^1 PPC(z)dz$. The pseudo-Gini coefficient provides a convenient measure of the degree of inequality represented by a given PPC.

Figure I. Top vs. Bottom Income Quintile PPCs



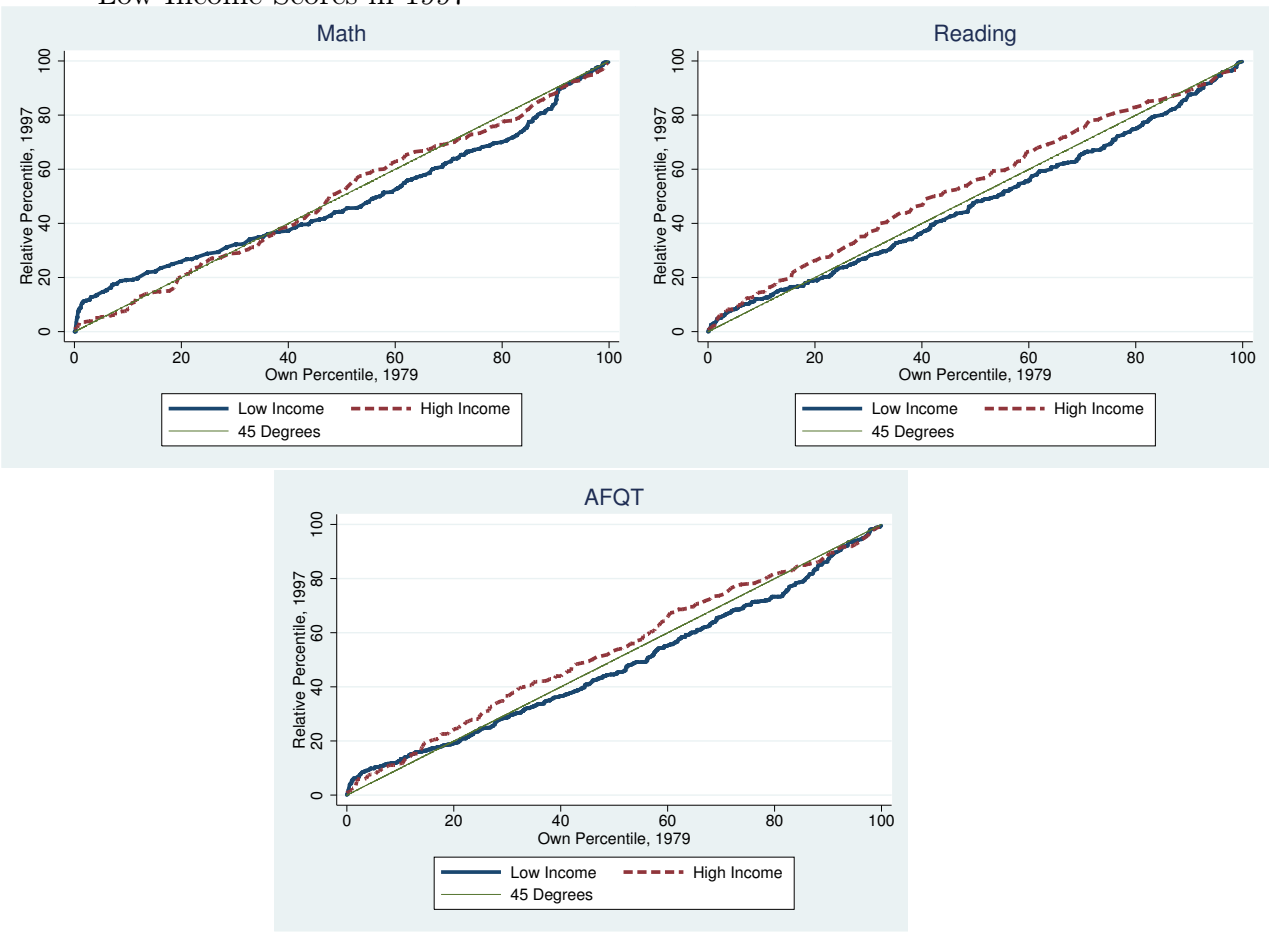
Note: Missing test scores and household incomes omitted. Pseudo-Gini Coefficients in parentheses after the line marker in the legend. All calculations use cross-sectional weights for the ASVAB test year.

FIGURE II. Black vs. White PPCs



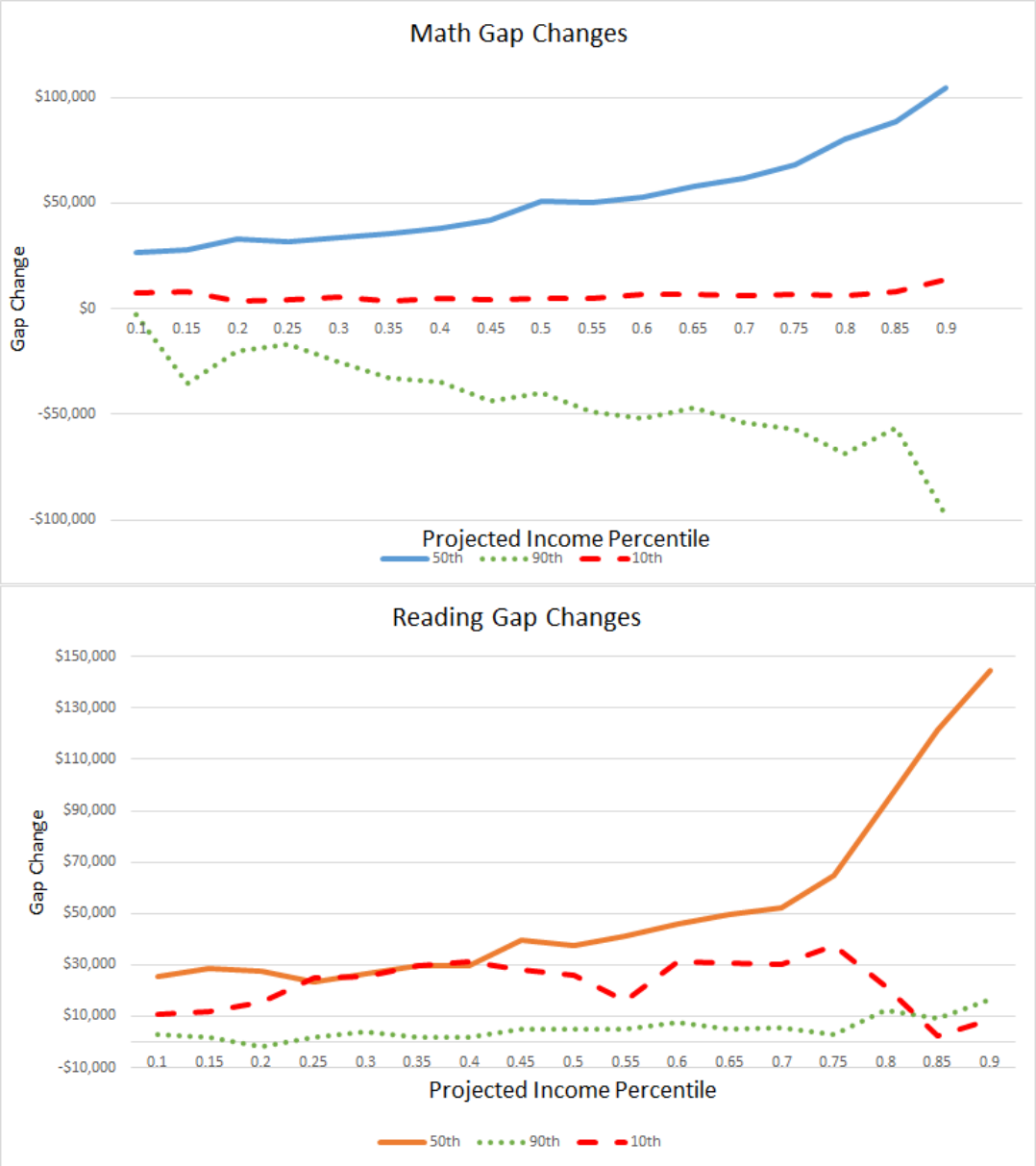
Note: Missing test scores and household incomes omitted. Pseudo-Gini Coefficients in parentheses after the line marker in the legend. All calculations use cross-sectional weights for the ASVAB test year.

FIGURE III. High- and Low-Income Scores in 1980 Relative to High- and Low-Income Scores in 1997



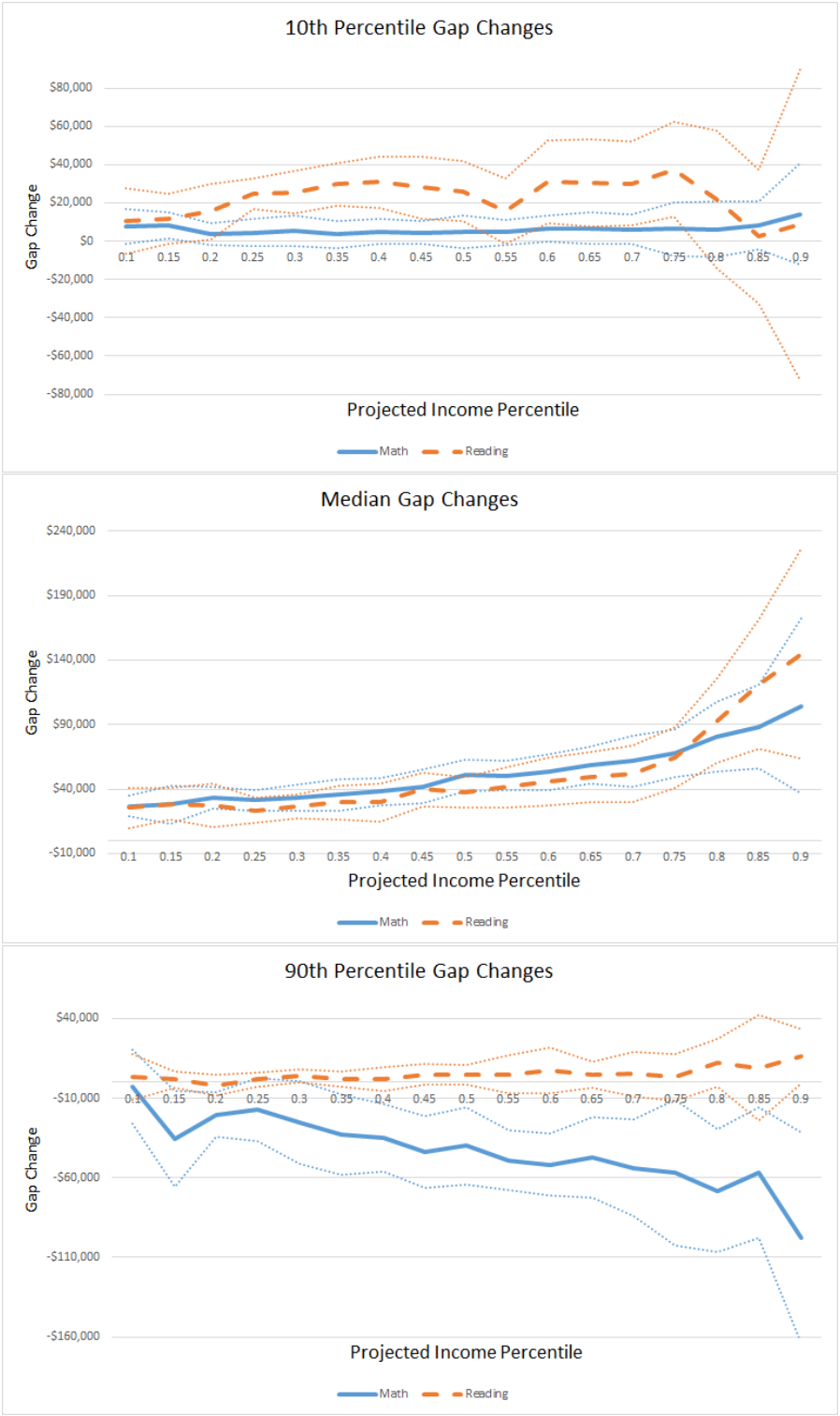
Note: Missing test scores and household incomes omitted. All calculations use cross-sectional weights for the ASVAB test year.

FIGURE IV. Labor Wealth Denominated Income-Achievement Gap Changes for Math and Reading



Note: Graphs use linearly interpolated annual income and assume labor supply is fully chosen. 95% confidence intervals calculated pointwise from 50 bootstrap iterations assuming normality. All calculations use cross-sectional weights for the ASVAB test year.

FIGURE V. Labor Wealth Denominated Income-Achievement Gap Changes for Math and Reading at Various Test Score Percentiles



Note: Graphs use linearly interpolated annual income and assume labor supply is fully chosen. 95% confidence intervals calculated pointwise from 50 bootstrap iterations assuming normality. All calculations use cross-sectional weights for the ASVAB test year.