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Test Questions, Economic Outcomes, and Inequality

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Test Questions, Economic Outcomes, and Inequality

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Abstract

Standard achievement scales aggregate test questions without considering their relationship to economic outcomes. This paper uses question-level data to improve the measurement of achievement in two ways. First, the paper constructs alternative achievement scales by relating individual questions directly to school completion and labor market outcomes. Second, the paper leverages the question data to construct multiple such scales in order to correct for biases stemming from measurement error. These new achievement scales rank students differently than standard scales and typically yield achievement gaps by race, gender, and household income that are larger by 0.1 to 0.5 standard deviations. Differential performance on test questions can fully explain black-white differences in both wages and lifetime earnings and can explain roughly half of the difference in these outcomes between youth from high- versus low-income households. By contrast, test questions do not explain gender differences in labor market outcomes.

Keywords: human capital, inequality, achievement gaps, measurement error
JEL Codes: I.24, I.26, C.2

1 Introduction

Human capital is fundamental to understanding labor market success, health, and many other economic outcomes. Group differences in human capital play a key role in common

*Bryce Turner and Danielle Nemschoff provided excellent research assistance. This paper benefited from discussions with Ralf Meisenzahl, Steve Laufer, Neil Bhutta, and seminar participants at the Federal Reserve Board and the NBER Education Program Meeting. The views and opinions expressed in this paper are solely those of the author and do not reflect those of the Board of Governors or the Federal Reserve System. Contact: eric.r.nielsen@frb.gov explanations for group differences in economic outcomes. A key question therefore is to what extent human capital differences drive observed inequalities. Human capital, however, is not directly observable. Researchers and policy makers therefore often turn to achievement test scores calculated using standard psychometric methods as proxies for human capital. These scores correlate with economic outcomes across a variety of contexts, motivating their use as direct measures of human capital.

Standard psychometric achievement scales measure human capital imperfectly because the skills they emphasize need not correspond to the skills most valuable for economic outcomes. Every test scale can be thought of as a method for combining individual test items (questions) into a single index. (I will stick with the terminology "items" throughout the remainder of the text.) Psychometric methods combine items without reference to the economic importance of the skills the items measure. Such methods may therefore yield biased estimates of human capital inequality if different groups perform differentially better or worse on items that are systematically related to later success. This is not a failing of psychometric methods per se – the problem lies using them for a purpose for which they were not designed.

In this paper, I develop new achievement scales that more accurately measure human capital. Using recently available data on individual Armed Forces Qualifying Test (AFQT) items from the National Longitudinal Survey of Youth 1979 (NLSY79), I assess which items are most relevant for predicting various long-run economic outcomes including high school completion, college completion, wages at age 30, and total lifetime earnings. I construct "item-anchored" scores by weighting test items in proportion to how strongly they correlate with these school completion and labor market outcomes, conditional on the other item responses. In other words, I assign to each item a "skill price" based on that item's ability to predict outcomes. I then use these skill prices to compare human capital (achievement) differences across different demographic groups. This use of item data is novel – even where outcomes have been used to anchor scores (Cunha and Heckman

(2008); Bond and Lang (2018); Chetty et al. (2014); Jackson (2018), and many others), item data have not been available or have not been used.[1]

I additionally use the item data in a novel way to assess measurement error in the item-anchored scales. Understanding this measurement error is important for calculating mean achievement gaps – simply taking the naive mean difference in the item-anchored scores between two groups will result in estimates that are biased towards zero. I adapt the empirical approach in Bond and Lang (2018), who use lagged scores and instrumental variables methods to undo this downward bias. I follow a similar approach, but instead of lagged scores, I leverage the item data to estimate multiple item-anchored scales which can then be used to construct the necessary instruments. This method allows me to estimate the reliabilities (signal-to-noise ratios) of the item-anchored scales.

After adjusting for measurement error, I find that achievement gaps calculated using the item-anchored scales are typically, though not always, much larger than gaps calculated using the NLSY79-given scales. For instance, I estimate that white/black and high-/low-income (defined here as the top and bottom parental income quintiles) math and reading gaps are all around 1 standard deviation (sd) using the NLSY79-given scales, roughly in line with prior literature (Neal and Johnson (1996); Reardon (2011); Downey and Yuan (2005), and many others). By contrast, the high-/low-income gaps are 0.2 sd larger and the white/black gaps are 0.2-0.5 sd larger when I anchor on wages at age 30 or lifetime earnings. Anchoring to high school completion leads to more modest, though still sizable, increases on the order of 0.06-0.20 sd. Item-anchoring does not increase all of the gaps, however – the college anchored white/black math gap is a full 0.2 sd smaller than the gap calculated using given math scores.

[1] Jackson (2018) studies teacher value-added on noncognitive dimensions not captured by achievement test scores using an analytic framework that recognizes the possibility of a disconnect between the skills emphasized by test scores and the skills that generate later-life outcomes. Polachek et al. (2015) take a different approach by estimating human capital production function parameters for each survey respondent in the NLSY79. They then relate these “human capital” ability measures to standard measures of cognitive and noncognitive skills. The NLSY79 item-level data is used by Schofield (2014) to argue that the measurement error in the IRT-derived AFQT scores is non-normal.

Achievement does not have natural units, so in some sense the most fundamental way to compare different test scales is through their induced rank orderings of students. Even if the cardinal properties of two scales are different, they might nonetheless rank students equally. However, the item-anchored scales I estimate rank students very differently than the NLSY79-provided scales. Individual student's rankings may differ by 10-20 percentile points or more between the item-anchored and given scales. Some students do well on items that are predictive of later success but poorly on uninformative items, resulting in a low given score, while for other students the situation is reversed. The item-anchored and given scales frequently disagree fundamentally about which students are doing well and which are doing poorly.

The item-anchored gaps can be directly compared to the actual outcome gaps. Consistent with prior literature using standard psychometric test scales (Lang and Manove, 2011), test items predict larger white/black gaps in school completion than are actually observed. By contrast, the item-anchored and actual white/black lifetime earnings and wage at age 30 gaps are very similar – the sizable racial differences in these labor market outcomes can be more or less fully explained by differential item responses. This result strengthens the headline conclusion in Neal and Johnson (1996); I both explain a greater share of the early-adult wage gap, and I additionally explain the gap in lifetime earnings, an outcome not studied in that paper.[2] Notably, I obtain these labor market results using only white men to construct the item-anchored scales, so that my findings do not depend on racial differences in how items correlate with outcomes.

The item-anchored scales do less well at explaining outcome differences for youth from high- and low-income households. The item-anchored scales modestly under-predict school completion differences while dramatically under-predicting wage and total labor market earnings gaps for youth from the top versus bottom quintiles of the household income distribution. The differences are largest for total earnings, suggesting that, con-

[2] To be clear, Neal and Johnson could not have studied lifetime earnings because the NLSY79 respondents were too young to accurately estimate this outcome when their paper was written.

ditional on test items, youth from high-income households both earn higher wages and work more hours. In short, youth from high-income households do substantially better in the labor market than their test items alone would predict.

Finally, I show that the item-anchored scores resolve the "reading puzzle" – the phenomenon in which reading scores, though positively correlated with wages, are weakly or even negatively correlated conditional on math scores (Sanders, 2016; Kinsler and Pavan, 2015; Arcidiacono, 2004). Joint regressions of wages at age 30 on item-anchored reading and math scores suggest a sizable role for reading conditional on math, in contrast to what regressions using given scores find. Reading skills do seem to have explanatory power above and beyond their correlation with math skills, but this is not visible when reading items are combined as they are in standard psychometric models. Nonetheless, the item-anchored reading coefficients are still only about half as large as the math coefficients for most outcome anchors and most regression specifications.

The rest of the paper is organized as follows: Section 2 discusses the test item and outcome data in the NLSY79, while Section 3 presents preliminary evidence that these items correlate quite differently with different outcomes. Section 4 discusses the general empirical and conceptual framework. Sections 5 - 6 present the main empirical estimates, while Section 7 addresses the reading puzzle. Section 8 concludes. Appendix A contains all of the tables and figures referenced in the paper.

2 NLSY79 Data

The NLSY79 is a nationally representative survey that follows a sample of individuals aged 14-22 in 1979 through to the present. Each round of the survey collects extensive information on educational and labor market outcomes that allows me to construct school completion, wage, and lifetime earnings variables. Additionally, survey respondents took a series of achievement tests, the Armed Services Vocational Aptitude Battery (ASVAB), in the base year of the survey. Item response data that code each item as correct or incorrect for each survey respondent are available for the math and reading components of the ASVAB. I now describe each of these pieces, the test items and later-life outcomes, in detail.

The ASVAB consists of a series of subject- and skill-specific tests which are primarily used by the United States military in making enlistment and personnel decisions. I make use of the item response data from the math and reading components of the ASVAB that together comprise the oft-studied Armed Forces Qualifying Test (AFQT). The math items come from the arithmetic reasoning (30 items) and mathematics knowledge (25 items) ASVAB component tests, while the reading items come from the paragraph comprehension (15 items) and word knowledge (35 items) components.[3] Thus, in total, I use 50 reading items and 55 math items to construct the item-anchored scales. I drop the subsample of the initial sampling frame that was designed to be representative of the military population, as the ASVAB is intended specifically for military recruits. However, my results change very little if this group is included.

The math and reading ASVAB items in the NLSY79 are particularly well-suited to anchoring to economic outcomes. The tests were administered to youth on the cusp of adulthood – completing high school, enrolling in college, starting to work full time, and so forth. As such, the item responses may be viewed as summary measures of the skills these youth took into adulthood. Moreover, the ASVAB was a low-stakes and unfamiliar test for most of the the NLSY79 sample, particularly with the military subsample already dropped. The item responses therefore are unlikely to reflect differential coaching/practice by race and socioeconomic background as might be a concern with a more widely-used, high-stakes assessment.

I use longitudinal data in the NLSY79 to construct various school completion and labor

[3] The constituent ASVAB components defining the AFQT changed after the administration of the test in the NLSY79. I use the current definition of math and reading, rather than the definition which held in 1980.

earnings outcomes.[4] For school completion, I use the highest grade completed reported at any point in the first 15 years of the survey. I define "high school" as an indicator for 12 or more grades completed and "college" as 16 or more grades completed.

The first labor market outcome I construct is average hourly wage at age 30 (wage_30). I divide total annual labor income by total hours worked for each survey round. I then average over the three rounds closest to each individual's age-30 round to to smooth out transitory wage/earnings fluctuations. I restrict myself to 30 because almost all schooling is completed by this age. Additionally, 30 falls after the typical crossing point past which more educated adults earn more on average than less educated adults.

The second labor market outcome I construct is the present discounted value of lifetime labor income (pdv_labor). The construction of pdv_labor is complicated because I do not observe labor income in all years for all survey respondents, either due to missing data, unemployment, or labor-force non-participation. To deal with such missing observations, I adopt a "pessimistic" imputation rule which assigns to each missing value the minimum labor income observed for the individual over the life of the survey. This rule is not meant to be realistic; rather, it will tend to compress the earnings distribution.[5] The NLSY79 respondents are only in their late 40's in the most recent survey round; I therefore assume that each individual's earnings growth after the latest survey round follows the education-specific growth rates from a pseudo-panel of male earnings constructed from the 2005 American Community Survey. Additionally, I assume each respondent retires 20 years after the most recent survey round, when the respondents are in their late sixties.[6] I discount future income exponentially at a 5% annual rate.

Table 1 presents the summary statistics of the main variables used in the analysis. The calculations here and elsewhere use cross-sectional weights designed to make the sample

[4] Please refer to Nielsen (2015b) for more details.

[5] In Nielsen (2015b), I study this measure along with others which adopt alternative fill-in rules. Though different in levels, the resulting income measures using different fill-in rules are strongly correlated with each other. I therefore stick with pessimistic imputation to simplify the discussion.

[6] A final complication is that the NLSY79 moved to a biennial format after 1994. I impute labor earnings for the odd-numbered years using linear interpolation *after* applying the pessimistic imputation rule.

nationally representative. My final analysis sample consists of 11,406 youth.[7] The college completion rate in this sample is about 24%, while the high school completion rate is 89%. The average hourly wage earned at age 30 is about \$19.43 with a standard deviation of \$11.27. The pdv_labor variable has an average of \$440,880. Roughly 9% of the sample is missing each of the ASVAB components – I drop these roughly 1,500 individuals from the anchoring analysis.[8]

3 Item-Outcome Correlations

Before delving into the anchoring analysis, I first present some simple evidence that test items differ widely in how strongly they predict economic outcomes. I then show that the predictive items are not easily identified using the parameters that characterize each item in item response theory (IRT), the popular psychometric framework used to construct the math and reading scales in the NLSY79.

Figure 1 shows the distributions of the estimated coefficients for bivariate regressions of indicators for each test item on school completion and labor market outcomes (in natural logs). The panels of Figure 1 show that the items are all positively correlated with every outcome. However, there is quite a range in the estimated coefficients. For instance, using college completion as the outcome yields math item coefficient estimates that range from 0.1 to 0.3. For each outcome, one can easily reject 0 for most item coefficients, and one can also easily reject that all of the coefficients are equal.[9] Comparing the distributions, there

[7] I use the full range of ages in my analysis to get as large a sample as possible. However, restricting the analysis sample to youth less than 18 years old at the survey start yields similar empirical results but with lower estimated reliability in the item-anchored scales.

[8] These individuals are also all missing IRT-estimated ASVAB component scores. In cases where the items are not entirely missing, I set the missing items to 0, corresponding to “incorrect.” For individuals who took the assessment (so that not all items are missing), blank (unanswered) items are coded as missing by the NLSY79. The assumption I make therefore is that leaving a question blank and getting the question incorrect are equivalent.

[9] Regressions that include all items simultaneously yield similar conclusions, although of course the individual item coefficients are smaller and some are negative. Similarly, plotting the R^2 for each bivariate regression used in Figure 1 shows that the share of variance explained by each item also varies widely.

are more math than reading items that are strongly correlated with college completion. The situation for high school completion and lifetime earnings is reversed – reading items seem to be particularly predictive of these outcomes. For ln(wage_30), the reading item distribution has greater variance than the math distribution, with more low-correlation and more high-correlation items.

Items that are predictive of one outcome are often not predictive of other outcomes. This is clear in Figure 2, which plots the item-level coefficients for different outcomes against each other. The top panel shows that the college item coefficients are essentially uncorrelated with the high school item coefficients. Even the math items in the right tail with college coefficient estimates greater than 0.25 have high school coefficients no higher or lower than the average. By contrast, the bottom panel shows that the ln(wage_30) and ln(pdv_labor) item coefficients are positively correlated – the items that predict lifetime earnings also, on average, predict wages at age 30. Even here there is substantial variation, with some items strongly predictive of only one of these two labor market outcomes.

A natural question is whether the highly predictive items have any discernible commonalities. My ability to investigate this question is very limited – the only item-level information available in the NLSY79 comes from the estimated IRT parameters that are used in the construction of the given scales.[10] In detail, the ASVAB scales are constructed using the three parameter logistic (3PL) IRT model, which supposes that a student with achievement θ answers item i with probability (i.e., has item response function)

$$P(D_i = 1|\theta, \alpha_i, \beta_i, \gamma_i) = \gamma_i + \frac{1-\gamma_i}{1+e^{-\alpha_i(\theta-\beta_i)}}.$$

The parameters $(\alpha_i, \beta_i, \gamma_i)$ characterize the item. The discrimination, α_i, gives the maximum slope of the item response function – the higher is α_i, the more sharply does the item distinguish between individuals with similar θs. The item difficulty, β_i, gives the

[10] The IRT parameters are not included as variables in the NLSY data download program, Rather, I transcribed them manually from the NLSY79 codebook.

location of this maximum slope – difficult (high-β) items distinguish between high-θ individuals. Finally, γ_i gives the guessing probability – the probability that a test taker with minimal achievement answers the item correctly. Importantly, these IRT parameters are not connected to the topics or skills covered by the item and are not estimated using any data on economic outcomes. Two items that have the same parameters will be treated equivalently, regardless of how differentially well they predict outcomes.

I assess how these IRT parameters are related to outcomes by estimating regressions which relate the item-level outcome coefficients to the IRT parameters. Specifically, I estimate regressions of the form

$$\hat{W}_i = \delta_0 + \delta_1 \text{discrimination}_i + \delta_2 \text{difficulty}_i + \delta_3 \text{guessing}_i + \varepsilon_i, \tag{1}$$

where $\hat{W}_i$ is the first-stage regression coefficient from a model relating an indicator for test item i to an economic outcome. I pool math and reading items in these regressions – the results are similar if math and reading are considered separately.

Table 2 presents estimates of Equation 1 and shows that the IRT parameters can partially predict which test items will be strongly correlated with economic outcomes. In particular, columns (1) and (3) show that the items that are strongly associated with labor market outcomes tend to have high discrimination, low difficulty, and low guessing probability. Column (5) shows similar results for high school completion. Column (7), by contrast, finds that items predictive of college completion are both more discriminating and more difficult, while still being difficult to guess. The even-numbered columns repeat the analysis for coefficients estimated in joint regressions of outcomes on all of the items. Very few significant links between the IRT parameters and the estimated outcome coefficients are apparent in these "full" regressions other than item difficulty, which is negatively related to most of the outcome coefficients yet positively related to the college coefficients. This shows that the importance of an item in the item-anchored scale is not strongly related to the item's psychometric properties.

It is intuitive that item discrimination matters for predicting economic outcomes. A highly discriminating item operates like a threshold – test-takers with achievement above some cutoff are very likely to get the item correct, while those below are very unlikely. A highly discriminating item therefore serves as a clear, discrete measure of the skill being tested and should thus have a strong correlation with outcomes if the tested skill is itself predictive.

Perhaps more surprising is the finding that easier items are more predictive of earnings and wages than harder items. More difficult items may be more purely "academic" and may thus simply not measure economically relevant skills. The positive relationship between item difficulty and college completion is consistent with this hypothesis – difficult items predict advanced school completion but not earnings. Alternatively, easier items might measure general, widely applicable skills, while difficult items might be more specialized, with more context-dependent value.

Finally, the consistent negative relationship between the guessing parameter and the item-outcome correlations is entirely natural. Easy-to-guess items have a high "measurement error" in that they do not distinguish clearly between high achievers, who got the question correct because they knew the answer, and low achievers, who happened to guess well. Such items should therefore have comparatively weak correlations with outcomes.

4 Conceptual Framework

This section presents the framework I use to analyze test items and economic outcomes. For ease of exposition, I refrain here from discussing the techniques I employ to handle measurement error in the calculation of item-anchored achievement gaps. That analysis is presented in Section 6.

Let $j \in \{1, \ldots, M\}$ index a sample of test-taking students drawn independently from some population, and let S_j denote the economic outcome of interest for student j. All other observable characteristics of the student (race, gender, family background, etc.) are denoted by X_j. Students take an achievement test with N dichotomous items. Let $\mathbf{D}_j$ denote the vector of item responses from student j: $\mathbf{D}_j = [D_{1,j}, \ldots, D_{N,j}]$ where $D_{i,j} = 1$ if j gets question i correct, and 0 otherwise. These items are combined using some framework (item response theory in the NLSY79) to produce a standardized (mean 0, standard deviation 1) test score z_j. It is these given scores that are often treated in social science and policy analysis as direct measures of human capital rather than as estimated proxies. The use of such scores introduces two distinct problems, both of which are remedied by item-level anchoring.

First, achievement has no natural scale – there is no way to determine whether a given score represents a lot of achievement or relatively little. Anchoring scores by estimating the relationship between z_j and the outcome S_j solves this indeterminacy by rescaling so that test scores are in directly interpretable units. Interpretability is not the only reason to prefer the anchored scale, however. If the transformation from the given scale to the anchored scale is nonlinear, as is often the case empirically, statistics calculated using the two scales may disagree dramatically – they may even differ in sign (Nielsen, 2015a; Schroeder and Yitzhaki, 2017; Bond and Lang, 2013).

Second, and the key insight in this paper, the given scores represent a particular choice about how to map each of the 2^N possible sequences of item responses to a scalar measure of achievement. Since this map is chosen without reference to economic outcomes, the scoring procedure may obscure useful information about human capital contained in the item responses. Note that anchoring given scores to outcomes (z_j to S_j), as has been done in prior literature, does not address this second concern.

I propose a framework that overcomes both of these conceptual challenges. As in Bond and Lang (2018), I guarantee interpretability by *defining* achievement A_j as the expected value of the outcome S_j:

$$S_j \equiv A_j + \eta_j, \ \mathbb{E}[\eta_j] = \mathbb{E}[\eta_j A_j] = 0. \tag{2}$$

Note that η_j is orthogonal to A_j by construction. Because only S_j is observed for each student j, A_j must be estimated. Rather than estimating A_j directly from z_j, I instead estimate it directly from the item-level responses: $\hat{A}_j = \hat{\mathbb{E}}[S_j|\mathbf{D}_j]$. In other words, I allow test items to enter directly into the anchoring relationship rather than only through the given score. In particular, for some function f, I suppose that

$$S_j = f(\mathbf{D}_j) + \varepsilon_j. \tag{3}$$

I then use estimates of f to construct outcome-denominated achievement scores. These item-anchored scores, given by $\hat{A}_j = \hat{f}(\mathbf{D}_j) = \hat{\mathbb{E}}[S_j|\mathbf{D}_j]$, are responsive to the relationship between individual test items and outcomes.

It is necessary in empirical work to place restrictions on the class of functions considered for f because $\mathbf{D}_j$ can take on many possible values with even a moderate number of items. I consider only linear regression and probit models in this paper. Although there is no *a priori* reason to rule out interactions between items, I find that allowing for such interactions produces anchored scales similar to what I report here.[11] The item-anchored scales I estimate are therefore based on models of the form

$$S_j = \mathbf{D}_j'\mathbf{W} + \varepsilon_j, \text{ or } \ S_j = \Phi(\mathbf{D}_j'\mathbf{W} + \varepsilon_j). \tag{4}$$

It is important to emphasize that the estimated item coefficient vector $\hat{\mathbf{W}}$ from Equation 4 should not be interpreted structurally – the estimated coefficient on an individual item is not indicative of any causal relationship between that item and the outcome S. Rather, the

[11] Specifically, I estimate models that allow for two-way interactions, and I employ regularization techniques (LASSO and elastic net) to keep the number of estimated parameters reasonable relative to the sample size.

goal is simply to estimate $\mathbb{E}[S_j|\mathbf{D}_j]$ flexibly.[12] Individual elements of $\hat{\mathbf{W}}$ are unconstrained and may therefore be negative, even though it may not be plausible that the true causal effect of any of these items should be less than zero.

5 Empirical Results – Rank Comparisons

I compare in this section the rank orderings induced by various item-anchored test scales to the rank orderings induced by the NLSY79-given scales. I also compare the item-anchored scales to "given-anchored" scales that use regression and probit models to relate the given scores to outcomes. This exercise produces two important results. First, economic outcomes are not linearly related to the given scores. Second, the item-anchored scales rank students very differently than the given scales.

Figure 3 plots the item-anchored scores against the given scores. The x-axis for each panel plots the given scores in standard deviation units. The y-axis plots the mean (solid black line) as well as the middle-50% and middle-90% ranges of the item-anchored scores, in standard deviation units, for each ventile of the given score distribution. For comparison, the figure also shows the mean of the given-anchored scores (dashed line) in each ventile.

The first thing to note in Figure 3 is that the item-anchored and given-anchored scales have nonlinear relationships to the given scales, particularly for high school and college completion. As noted in previous literature (Bond and Lang, 2013; Schroeder and Yitzhaki, 2017; Jacob and Rothstein, 2016; Nielsen, 2015a), this nonlinearity means that standard calculations (mean differences, OLS, etc.) using the given scale may produce severely biased estimates. Notably, the item-anchored and given-anchored scales line up very well at the mean for each ventile of the given score distribution.

The nonlinearities in the school completion scales are intuitive. The college math and

[12] In fact, the only reason to assume a linear model, or any model at all, is because the sample size is not large enough for non-parametric anchoring. Given a very, very large sample, the anchored scale could be estimated simply as the sample average of S for each possible realization of item responses.

reading scales have convex relationships to the given scales; differences in achievement at the bottom ends of the given scales do not translate to differences in college completion, while differences at the top do. The situation is reversed for high school – improvements at the bottom ends of the given scales translate strongly changes in high school completion, while improvements in the top are not very valuable. Low-achievement youth are not likely to be on the margin for completing college, so the college scales should be flat for such students. At the same time, these students are likely to be on the high school margin, explaining the steep anchored relationship for below-mean given scores.

The item-anchored scales are closer to being linearly related to the given scales for ln(wage_30) and ln(pdv_labor). Of course, this means that the relationship between observed scores and lifetime earnings or wages in levels is convex. Improvements at the top of the given math and reading scales yield out-sized gains in predicted wages and lifetime earnings.

The middle 50% and 90% ranges in each panel of Figure 3 show that there is a fairly wide distribution of item-anchored scores associated with each given score ventile. Individuals whose item responses led to the same (or similar) given score might have very different predicted outcomes based on their particular item responses. For college completion, this variation is greater at the top of the observed score distribution. For instance, among students with given math scores about 1 sd above the mean, the middle 90% of the item-anchored scores cover almost 2 sd on the item-anchored scale, while for those 1 sd below the mean, the corresponding range is only about 0.2 sd. Some apparently high-performing youth are actually forecast to have very low rates of college completion, while others are substantially more likely to finish than their given scores would indicate. Low-performing youth, however, are always forecast to have low rates of college completion. The pattern for high school is reversed – there is a lot of variation in the item-anchored scores at the bottom of the given scale and very little variation at the top. In contrast, the middle 50% and 90% ranges of the log labor scales appear to be fairly constant across the given score distributions, although they are a bit more spread out in reading for lower given scores. The range of item-anchored scores in each given score ventile again tends to be quite large. As a representative example, the range of log(pdv_labor) scores for youth at the mean of the given reading distribution covers about 2 sd. Some youth with low observed scores are predicted to have high labor market earnings, while others with high observed scores are forecast to perform fairly poorly in the labor market.

The range of item-anchored scores depicted in Figure 3 implies that they rank students differently than the given scores. This is not true for the given-anchored scales, as these are just monotone transformations of the given scores. The item-anchored scores do not just disagree with the given scores about how valuable achievement is, they disagree fundamentally about which students are performing well and which students are not.

The relatively wide 90% ranges depicted in Figure 3 suggest that the ranking differences between the item-anchored and given scales might be quite substantial. Indeed, Figure 4, which plots the absolute value of the difference in percentiles according to the item-anchored and given scales, shows that it is not uncommon for the rankings to differ by 10 - 20 percentile points or more between the two. Notably, the labor outcome scales display more rank shuffling than the school completion scales. This is intuitive, as math and reading test items should be more closely aligned to academic performance than to success in the labor market.

6 Empirical Results – Achievement Gaps

I now turn to the measurement of achievement gaps using the item-anchored test scales. I first show how to leverage the item data to construct multiple, independent item-anchored scales in order to estimate the amount of measurement error in said scales. That is, I use the item data in two ways: first to estimate the item-anchored scales and second to estimate the reliability (signal-to-noise ratio) of these scales. After adjusting for measurement error, I find that the item-anchored scales generally show much more achievement inequality than the NLSY79-given scales. Moreover, I find that the item-anchored scales can explain significant fractions of the observed differences in school completion and labor market success by race and parental income.

6.1 Mean Achievement Gaps and Measurement Error

The item-anchored test scales are estimated with error. This error biases achievement gaps estimated using the item-anchored scales towards zero. I adapt the approach in Bond and Lang (2018) to handle this problem. The basic idea is to construct two independent anchored scales that can be used in an instrumental variables setting to undo the bias generated by the measurement error. The strategy of using instruments to recover the relevant "shrinkage term" (to be explained below) is from Bond and Lang's work. My innovation lies in leveraging the item data to construct the necessary instruments.

Let h and l denote two groups of students whose achievement we want to compare. The goal is to estimate $\Delta A_{h,l} \equiv \bar{A}_h - \bar{A}_l$, where $\bar{A}_g$ is the average achievement of group g. Each individual's achievement is measured with error: $\hat{A}_j = A_j + \nu_j$. If $A \sim N(\bar{A}, \sigma_A^2)$ and if $\nu_j \sim N(0, \sigma_\nu^2)$ iid in the population,[13]

$$\mathbb{E}[S_j|\hat{A}_j] = \frac{\sigma_A^2}{\sigma_A^2 + \sigma_\nu^2}\hat{A}_j + \frac{\sigma_\nu^2}{\sigma_A^2 + \sigma_\nu^2}\bar{A}. \tag{5}$$

Equation 5 says that the expected outcome of student j conditional on the item-anchored achievement $\hat{A}_j$ is "shrunk" towards the population mean $\bar{A}$. This is intuitive – tests are noisy, so the best guess about a student's true achievement gives weight to both the realized score and the population expected value, with the observed score weighted more heavily the less noisy it is.

[13] See Bond and Lang (2018) for an extensive discussion and demonstration of the robustness of the general approach outlined here to violations in the normality assumption.

A naive estimator for $\Delta A_{h,l}$ is the mean difference in item-anchored scores. However, Equation 5 shows that this estimator will be biased towards 0. Letting $\hat{A}_h - \hat{A}_l$ denote the sample mean difference in item-anchored scores,

$$\text{plim}(\hat{A}_h - \hat{A}_l) = R_{A,\nu}\left(\Delta A_{h,l}\right), \quad R_{A,\nu} = \frac{\sigma_A^2}{\sigma_A^2 + \sigma_\nu^2}. \tag{6}$$

The intuition for Equation 6 is that, for an individual test-taker, the measurement error in testing implies that the best guess of achievement should be shaded to the population average. However, for a group mean, this measurement error is immaterial, so the shading towards 0 is not needed. Given some way to consistently estimate $R_{A,\nu}$, one could use Equation 6 to recover a consistent estimate of $\Delta A_{h,l}$. A biased estimator of $R_{A,\nu}$ is given by γ in the regression

$$\hat{A}_j = \kappa + \gamma S_j + \varepsilon_j. \tag{7}$$

The OLS estimate of γ is biased toward 0 because S_j is a noisy measure of A_j. This classic errors-in-variables problem is solvable with an appropriate instrument for S_j.

The item-level construction allows one to construct many such instruments simply by estimating different item-anchored scales using disjoint subsets of the test items. For example, if the items are partitioned into two groups (1) and (2), Equation 4 can be estimated separately on each group to produce anchored scores $\hat{A}_j^{(1)}$ and $\hat{A}_j^{(2)}$. Each of these scores is a noisy measure of A_j. Now consider estimating Equation 7 using $\hat{A}_j^{(1)}$. An instrument for S_j in this equation is the average S among students who are not j but who nevertheless have the same value of $\hat{A}_j^{(2)}$. That is, an instrument using item group (1) as the "base" is

$$\zeta_j^{(1)} = N_j^{-1} \sum_{j' \neq j:\ \hat{A}_j^{(2)} = \hat{A}_{j'}^{(2)}} S_{j'}. \tag{8}$$

This instrument is relevant because achievement is persistent – test-takers should do similarly well on tests with similar items. The exogeneity condition is satisfied thanks to the leave-one-out construction.

There are several things to note about this method. First, groups (1) and (2) can be interchanged in the construction, with either used to construct the "main" scale and the other used to construct the instrument. Second, there are many different ways to partition the items into two groups. Third, there is no reason to restrict the partition to two groups. For now, I restrict the analysis to only 2 groups with equal numbers of test items where I assign odd-numbered items to group (1) and even numbered items to group (2). To the extent that the ASVAB component tests organize items by content, this procedure ensures that items from each content area are included in both groups (1) and (2). Given this approach, it does not appear to matter very much in my setting which of the two groups is used to construct item-anchored test scale and which is used to construct the instrument. A more comprehensive treatment of the possibilities suggested by the above discussion is left for future work.

A technical point concerning the construction of the instrument $\zeta_j^{(1)}$ is the selection of the other test-takers j' such that $\hat{A}_j^{(2)} = \hat{A}_{j'}^{(2)}$. With 25 or more items in each group, it will frequently be the case that no one else will have the exact same $\hat{A}^{(2)}$ as j. Therefore, I divide the sorted $\hat{A}_j^{(2)}$s into 100 equally sized bins and estimate $\zeta_j^{(1)}$ using the observations in the same bin as j. Putting everything together, my approach consists of the following steps:

1. Divide the items into groups (1) and (2) such that each group has roughly half of the total items.
2. Estimate $\hat{A}_j^{(1)}$ and $\hat{A}_j^{(2)}$ separately on groups (1) and (2) using Equation 4. Let $\Delta\hat{A}_{h,l}^{(1)}$ be the raw (unadjusted) mean achievement gap estimated using the $\hat{A}_j^{(1)}$.
3. Construct the $\zeta_j^{(1)}$ as in Equation 8. Estimate $\hat{\gamma}^{(1)}$ from Equation 7 using instrumental variables regression.

4. Estimate the *h*-*l* achievement gap using $\frac{\Delta \hat{A}^{(1)}_{h,l}}{\hat{\gamma}^{(1)}}$.

The final econometric question is how to construct the standard errors for the anchored achievement gaps. The issue is whether to treat the inflation factors $1/\hat{\gamma}^{(1)}$ and the anchored scales as estimated or known. The main results tables treat these factors as known, consistent with work that uses psychometrically-derived reliability estimates and consistent with the given-anchored estimates I produce which use such reliabilities. I also generate bootstrapped standard errors which take the sampling distribution of $1/\hat{\gamma}^{(1)}$ and the item-anchored scales themselves into account. Table 3 demonstrates that bootstrapped standard errors that incorporate these additional sources of error are 25-50% larger than the baseline standard errors which do not. Using the bootstrapped standard errors would not change any of the important empirical conclusions of the paper.

6.2 School Completion Item-Anchored Gaps

I first discuss the school completion item-anchored gaps. Table 4 shows three sets of gap estimates: white/black, male/female, and high-/low-income (defined here as the top and bottom quintiles of the base-year household income distribution). I present the item-anchored gaps in standard-deviation units for comparability to the NLSY79-given gaps and in outcome (school completion probability) units for economic interpretability.

The item-anchored white/black math and reading gaps are quite different than the given math and reading gaps, which are both around 1 sd. The math and reading high school item-anchored gaps and the college item-anchored reading gap are 0.17-0.23 sd larger than their given counterparts. By contrast, the college item-anchored math gap is 0.17 sd smaller than the given gap. Black youth do comparatively well on math items that are particularly predictive of college completion, although the anchored gap is still quite large in absolute terms. At the same time, black youth perform relatively poorly on reading items that are predictive of high school or college completion and math items that are predictive of high school completion.

Turning to gender differences, the given scores imply that males have a 0.18 sd advantage in math and a 0.11 sd deficit in reading, roughly consistent with prior literature (Fryer and Levitt (2010); Dee (2007); Le and Nguyen (2018), and many others). Anchoring to school completion at the item level lowers the male advantage in math – the college-anchored gap is 0.13 sd and the high school-anchored gap is only 0.05 sd. Similarly, item-anchoring to high school completion shrinks the female advantage in reading to 0.08 sd, while item-anchoring to college completion removes the female advantage entirely – the gap shifts to an insignificant 0.01 sd. Achievement scales which emphasize items associated with school completion reduce, or in some cases remove, apparent male-female achievement differences.

Item-anchoring to school completion also shifts the estimated achievement gaps between youth from high- and low-income households. Specifically, Table 4 shows the given and item-anchored gaps for students in the top and bottom quintiles of the base-year household income distribution. Given scores show achievement gaps of about 1 sd in math and reading, while the high school item-anchored gaps are roughly 0.10 sd larger. The college results again show a divergence between math and reading; the college item-anchored math gap is about 0.05 sd smaller than the corresponding given gap, while the college item-anchored reading gap is 0.30 sd larger.

Table 4 also presents the estimated reliabilities for the item-anchored scales. The math reliabilities are 0.81 for high school and 0.87 for college; these are both quite close to the 0.85 reliability reported in the NLSY79. The high school reading reliability, at 0.86, is larger than the reported value of 0.81, while the college reading reliability is smaller, at 0.74. A consistent finding is that the item-anchored reliabilities are different from, and often smaller than, the reliabilities reported by the NLSY79 and also quite different for the same test items across different anchoring outcomes.[14]

[14] These reliabilities are mostly larger than what Bond and Lang (2018) find anchoring to high school completion with prior-year lagged test scores used to adjust for shrinkage. One possible explanation for

The second column of Table 4 shows that anchoring at the item-level, as compared to anchoring using the given scores, is important for my empirical results. The column shows gaps calculated by anchoring the given scores directly to outcomes, adjusted using the NLSY79-reported test reliabilities.[15] These given-anchored gaps are generally larger than the item-anchored gaps, sometimes substantially so, and they are never much smaller. The differences between the item- and given-anchored results are driven both by differences in the estimated test reliabilities and differences in the unadjusted gaps. One extreme example is the college male/female math gap where the given-anchored estimate is the same as the raw gap, at 0.1 sd, while the item-anchored estimate is 0. This same general pattern repeats for the log labor outcomes – the given-anchored and item-anchored gaps are often quite different from each other.

Finally, I compare group differences in predicted versus actual school completion. The predicted white/black gaps are uniformly much larger than the actual gaps. Math and reading items predict college gaps of 0.20 and 0.25, respectively, both much larger than the 0.13 gap observed in the data. One possible explanation for this finding is that black youth may on average be attending lower quality schools where the probability of graduating is higher for a given level of achievement. For gender, the predicted gaps are usually small and positive in favor of men, while the actual gaps slightly favor women. Men would be expected to complete school at slightly higher rates purely on the basis of their item responses, but women have higher observed completion rates than men. Test items under-predict achievement gaps by parental income by roughly 0.05-0.06, off a base of 0.29 for college and 0.19 for high school. High-income youth have an even larger advantage in school completion than what would be predicted on the basis of their item responses alone.

this difference is that any skill that is predictive of outcomes within a given year that is not predictive across years will be viewed by Bond and Lang as measurement error but not by my "within year" procedure. Additionally, the assessments studied by Bond and Lang are different than the AFQT and the students are much younger, both of which could independently explain their lower reliability estimates.

[15] The ad hoc use of the reported test reliabilities is the best one could do using only given scores.

6.3 Wage and Lifetime Earnings Item-Anchored Gaps

I now repeat the analysis of the previous section using test scales anchored at the item level to ln(wage_30) and ln(pdv_labor). Table 5 presents my preferred estimates which use only white males to construct the anchored scales. White men have greater labor force attachment in the NLSY79, so selection plays less of a role in the estimates. Moreover, to the extent that discrimination and other barriers for women and racial minorities are operative in the labor market, item-anchored achievement scales estimated using only white males will be more interpretable. These achievement gaps thus answer the question: "If test items correlated to outcomes for everyone as they do for white men, what would be the achievement gap between these two groups?" For completeness, I also report estimates anchored on the full sample (not just white men) and find broadly similar results.

The white/black results are particularly striking, as the item-anchored scales show greater achievement inequality than the NLSY79-given scales while simultaneously explaining more of the observed differences in labor market outcomes. The item-anchored white/black gaps are 0.15-0.23 sd larger for ln(wage_30) and 0.29-0.51 sd larger for ln(pdv_labor). Moreover, the predicted white/black gaps in both of these outcomes are almost exactly equal to the observed gaps – the large mean differences in wages and lifetime earnings by race can be fully predicted by differential item responses.[16] This result is consistent with Neal and Johnson (1996), who find that adding AFQT scores to log wage regressions reduces the white/black gap for men by about two-thirds in the NLSY79. In order to compare my estimates to theirs, Table 7 estimates race and income gaps on the male-only subset of my data. The item-anchored scores explain between 77%-84% of the observed white/black wage and labor income gaps for men – less than what I found in the full sample but more than what Neal and Johnson report. It is worth emphasizing again that my results apply to white/black gaps in both wages in early adulthood and total

[16] It is important to note that I condition on labor earnings of wages being observed in all of these comparisons. The latent (selection-corrected) wage gaps may well be different than what I present. However, the predicted gaps using the full sample are fairly similar to what I report here.

lifetime earnings, whereas Neal and Johnson (1996) present results only for early-adult wages due to the young age of the NLSY79 respondents in the mid-1990s.

Turning to differences by parental income, the item-anchored scales again find more achievement inequality than the given scales, with the item-anchored high-/low-income gaps about 0.16-0.20 sd larger than the given gaps. However, in contrast with the white/black results discussed above, the math and reading items predict wage and lifetime earnings gaps that are roughly half as large as the gaps that are actually observed. This echos the findings for school completion – skill differences appear to be an important part of the story for understanding adult inequality for high- and low-income youth, but other factors must also be playing a significant role.

Unlike with race and parental income, test items do a poor job of predicting the sizeable gender gaps in wages and lifetime earnings. Using the item-anchored scales instead of the given scales increases the male advantage in math by about 0.07 sd while decreasing the female advantage slightly. These differences translate to modest predicted gaps in earnings, with the reading results predicting that women should (slightly) outearn men. However, the actual wage gap is more than four times what even the item-anchored math gap would predict, while the lifetime earnings gap is more than six times as large.

Using the full sample, rather than just white men, to estimate the anchored scales yields broadly similar results. Table 6 shows that while the differences between the given gaps and the anchored gaps are typically (though not uniformly) smaller, the qualitative conclusions are largely unchanged. The one exception is the male/female reading gap. The item-anchored scales estimated on the full sample suggest that men have a sizable advantage in both math and reading, while the given scores and the item-anchored scores using just white men find a significant reading advantage for women and a significant math advantage for men. This is perhaps intuitive – women are half the sample and earn far less than men. The item-anchored scale estimated on the full sample will thus tend to pick out particularly "male" items in order to predict earnings, making it appear as though men have a sizable advantage in both reading and math.

6.4 Median Regression Wage Anchored Gaps

The baseline wage results discussed in the previous section use the subsample of white men with non-missing wages. Although white men have relatively high labor force participation, wage data are still missing for roughly 20% of this group, raising concerns about selection. In this section, I present alternative anchored gap estimates where median regression is used to estimate the anchoring relationships on the full sample of white men, not just those with non-missing wages. The key assumption underlying my approach is that, conditional on the item responses, the latent wages of the missing men are below the median. With this assumption, I can identify the median, and, under further assumptions, the mean of the latent wage distribution for white men conditional on the item responses.

Let $\tilde{S}$ be the latent wage, so that $S = \tilde{S}$ if the individual is working and missing otherwise. Suppose that $\tilde{S} = \mathbf{D}'W + \varepsilon$ with $\text{median}(\varepsilon) = \text{mean}(\varepsilon)$. These conditions imply that $\text{median}[\tilde{S}|\mathbf{D}] = \mathbb{E}[\tilde{S}|\mathbf{D}]$. If one can identify the conditional median, then one can also identify the conditional mean and proceed exactly as before to construct item-anchored scales and achievement gaps.

Two conditions are sufficient to identify the median. First, less than half the data must be missing for each $\mathbf{D}$. Second, conditional on $\mathbf{D}$, the missing latent outcomes must be below the median: for all j with S_j unobserved such that $\mathbf{D}_j = \mathbf{D}$, $\text{median}[\tilde{S}|\mathbf{D}] > \tilde{S}_j$. This second condition just says that selection into the labor market is always positive, a plausible assumption for white men. Given these two conditions, the median can be identified by creating a new outcome variable, $\check{S}$ equal to S_j when the outcome is non-missing and $\min\{S_{j'}\}$ otherwise. It is straightforward to see that $\text{median}[\tilde{S}|\mathbf{D}] = \text{median}[\check{S}|\mathbf{D}]$. One can thus estimate the necessary conditional medians (and hence means) by running a median regression of $\check{S}$ on $\mathbf{D}$. Once the predicted values from this regression are in hand, the rest of the analysis proceeds exactly as outlined in Section 6.

Table 8 presents the ln(wage_30) item-anchored gaps estimated on the white male sample using median regression. As before, the item-anchored gaps are typically substantially larger than the given gaps. Notably, the median-anchored math gaps tend to be much larger than the regression-anchored gaps, with the white/black math gap ballooning from 1.21 sd to 1.48 sd and the high-/low-income gap increasing from 1.19 sd to 1.44 sd. These larger math gaps are partially due to the lower estimated reliability of the median-anchored math scale (0.64 vs 0.75). The median-anchored reading estimates, despite a similarly lower reliability, are generally much closer to the regression-anchored estimates. Overall, these results do not suggest that selection is driving my main findings.

Turning to the actual versus predicted comparisons, the reading item responses again perfectly predict the black/white log wage gap while predicting a bit less than half of the high/low-income gap. However, for math, item responses now predict a log wage gap that is about 0.1 larger that what is actually observed. Similarly, math items predict a higher share of the high-/low-income log wage gap (about two-thirds versus one half previously).

7 The Reading Puzzle and Item-Anchoring

Prior research (Sanders, 2016; Kinsler and Pavan, 2015) has noted that in multivariate regressions of labor outcomes on math and reading scores, the coefficients on reading are often much smaller than the coefficients on math. In some cases, the reading coefficients are even significantly negative. These findings are puzzling – reading and math skills are distinct, and reading skills seem like they should be quite valuable economically. This section demonstrates that using item-anchored scores can resolve this puzzle. Item-anchored math and reading scores both have large and statistically significant coefficient estimates in the types of joint regressions that give rise to the reading puzzle. Nonetheless, the coefficient estimates on item-anchored reading achievement are still quite a bit smaller than those on item-anchored math.

I assess the effect of item anchoring on the reading puzzle by estimating for different definitions of math and reading (given, item-anchored, etc.) models of the form

$$ln(\text{wage\_30}) = \alpha + \beta_1\text{math} + \beta_2\text{reading} + \text{controls} + \varepsilon.$$

Table 9 presents the math and reading regression coefficients for various model specifications that include or exclude parental income and highest grade completed dummies as additional controls. Echoing prior literature, columns (1) and (4) show that the estimated coefficients using the given scores are large and statistically significant for math and small and insignificant for reading. A one standard deviation increase in the given math score is associated with a 0.1-0.18 increase in ln(wage_30), while the same increase in the given reading score corresponds to a -0.01 to 0.03 change in ln(wage_30). Given-anchored scores do not resolve the puzzle – columns (2) and (5) show that given-anchored math is significantly associated with ln(wage_30), while given-anchored reading is not.[17]

Item-anchored scores, however, do resolve the reading puzzle. Columns (3) and (6) in Table 9 show that the item-anchored math coefficients are very similar to the given-scale coefficients, while the item-anchored reading estimates are much larger: a one standard deviation in item-anchored reading corresponds to a 0.05-0.09 increase in ln(wage_30). Although these reading estimates are about half the size of the math estimates, they are still large and are statistically distinguishable from 0 at a 1% level.

These results are not tautological – the math and reading scales are constructed independently of each other. It could have been the case that the component of item-anchored reading orthogonal to item-anchored math was uncorrelated with ln(wage_30). Nonetheless, each scale is constructed to be maximally predictive of ln(wage_30). This may make their significance less surprising. However, Table 10 shows that using school completion item-anchored scales yields similar results. In particular, columns (2) and (4) show that

[17] The given-anchored scales use linear regression, which Figure 3 suggests should fit the data quite well. Indeed, using quadratic or cubic models to anchor the given scores alters the results very little.

high school and college item-anchored scales result in smaller, though still significant, math and reading estimates.[18] Finally, columns (3) and (5) show that the reading puzzle persists using school completion given-anchored scores. The scales in these columns are non-linear (probit) transformations of the given scores, but they are order-preserving. In each case, the reading coefficient is small and not distinguishable from 0, while math remains significantly positive.

The results in this section also highlight the more general point that the item-anchored scales relate to each other and to various outcomes differently than the given scales. Item-anchoring therefore has the potential to change not just achievement gap estimates. Any calculation using test scores as an outcome or as a control might yield strikingly different results with item-anchored scores. Findings such as the reading puzzle, which have been interpreted as saying something interesting about the relationship between math and reading skills and economic outcomes, may in fact only reflect arbitrary and poorly motivated measurement choices.

8 Discussion and Conclusion

In this paper, I argue that test scales anchored at the item-level to economic outcomes are more plausible measures of human capital than the standard psychometric scales widely used in social science and policy analysis. I show that the choice of scale, item-anchored or psychometric, matters in a variety of settings. Item-anchored scales rank students very differently and yield gaps by race, gender, and parental income that are typically 15-50% larger in standard deviation terms. Item-anchored scales can explain the entire white/black earnings gap but only half of the earnings gap between youth from high- and low-income households. Finally, item-anchored scales can resolve the "reading puzzle" in that item-anchored reading scores have significant explanatory power

[18] The smaller college-anchored reading estimates may reflect the weaker relationship between reading items and college completion.

for labor market outcomes beyond their correlation with item-anchored math, in contrast to psychometric reading and math scales. These empirical results make use of a novel method for calculating test reliability that leverages item-level data to construct multiple, independent, item-anchored scales.

Overall, the results in this paper suggest that social scientists and policy makers would do well to consider more closely the alignment between the achievement scales they are using and the economic outcomes that they are ultimately interested in. Although psychometric achievement scales are related to economic outcomes, they are not designed with these outcomes in mind. Such scales are thus imperfect measures of human capital and their use appears to obscure important patterns in data.

Although my focus in this paper is on math and reading achievement, similar critiques apply to standard measures of noncognitive skills. As these skills are thought to be important in determining later-life outcomes, a natural question is to what degree our understanding of these skills is being driven by the economically arbitrary aggregation of individual noncognitive items.

Policy interventions are frequently evaluated by their causal effects on standard psychometric scores. My results further suggest that a focus on such effects may be misplaced. An intervention might improve observed scores by shifting items that are not predictive of later success. Conversely, an intervention that improves items that are predictive of success may appear to be a failure if those items are not emphasized by the psychometric scale. Moreover, causal effects estimated on psychometric scales may mask economically important heterogeneity by race, gender, and socioeconomic status. This discussion suggests yet again that economists and policy makers would do well to focus greater effort on the construction of achievement scales that are more closely aligned with economic outcomes.

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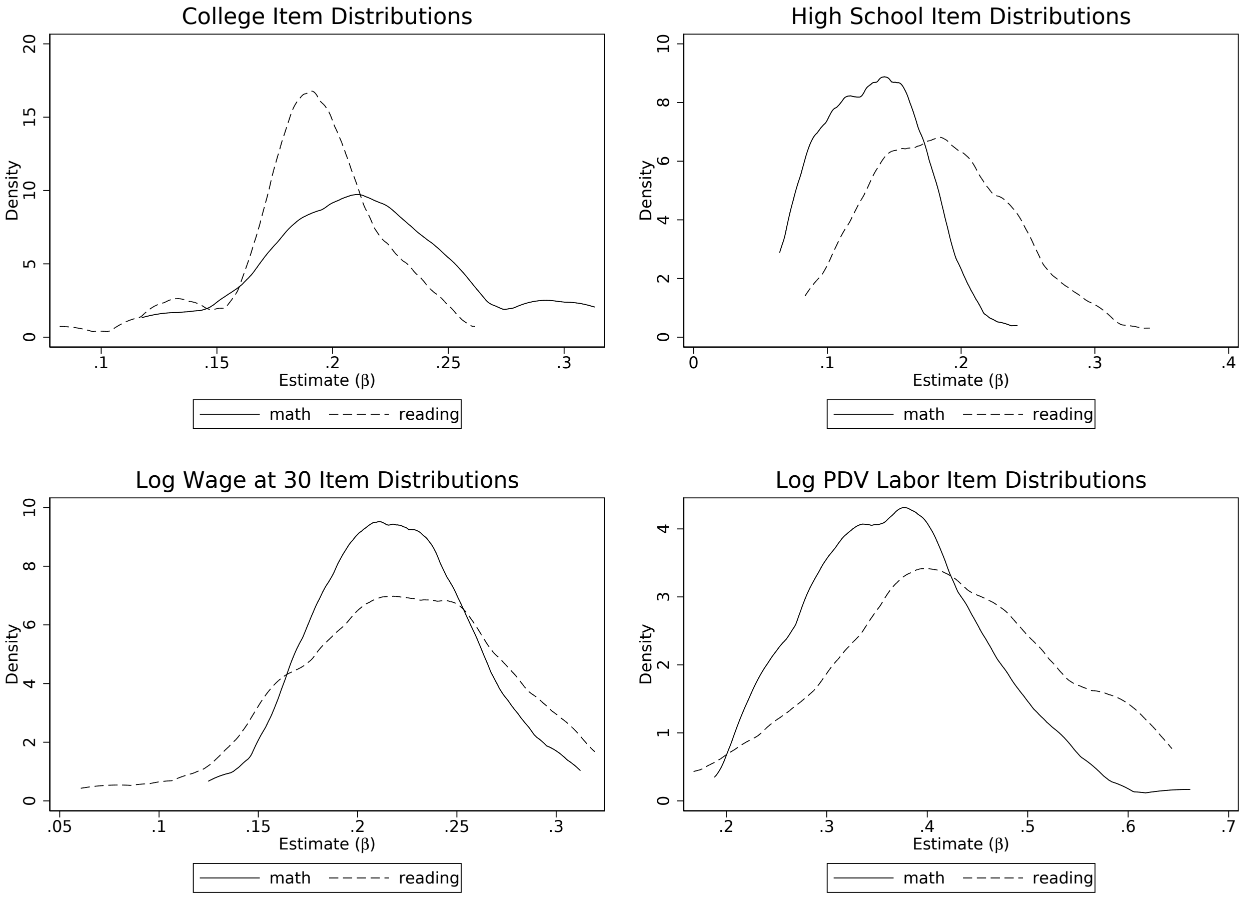
A Appendix

Table 1: NLSY79 Sample Summary Statistics

Variable	Mean	Std. Dev.	N
age (base year)	17.73	2.32	11406
male	0.50	0.50	11406
black	0.14	0.34	11406
hh income (base year, $1,000)	70.84	46.4	10985
high school	0.89	0.32	11406
college	0.24	0.43	11406
highest grade completed	13.28	2.5	11406
pdv labor ($1,000)	440.88	332.2	11406
wage at 30	19.43	11.27	8521
math	99.05	18.99	10721
reading	98.23	19.38	10721
afqt	147.75	27.25	10721
ar missing	0.08	0.28	11406
wk missing	0.08	0.28	11406
pc missing	0.09	0.28	11406
mk missing	0.09	0.28	11406

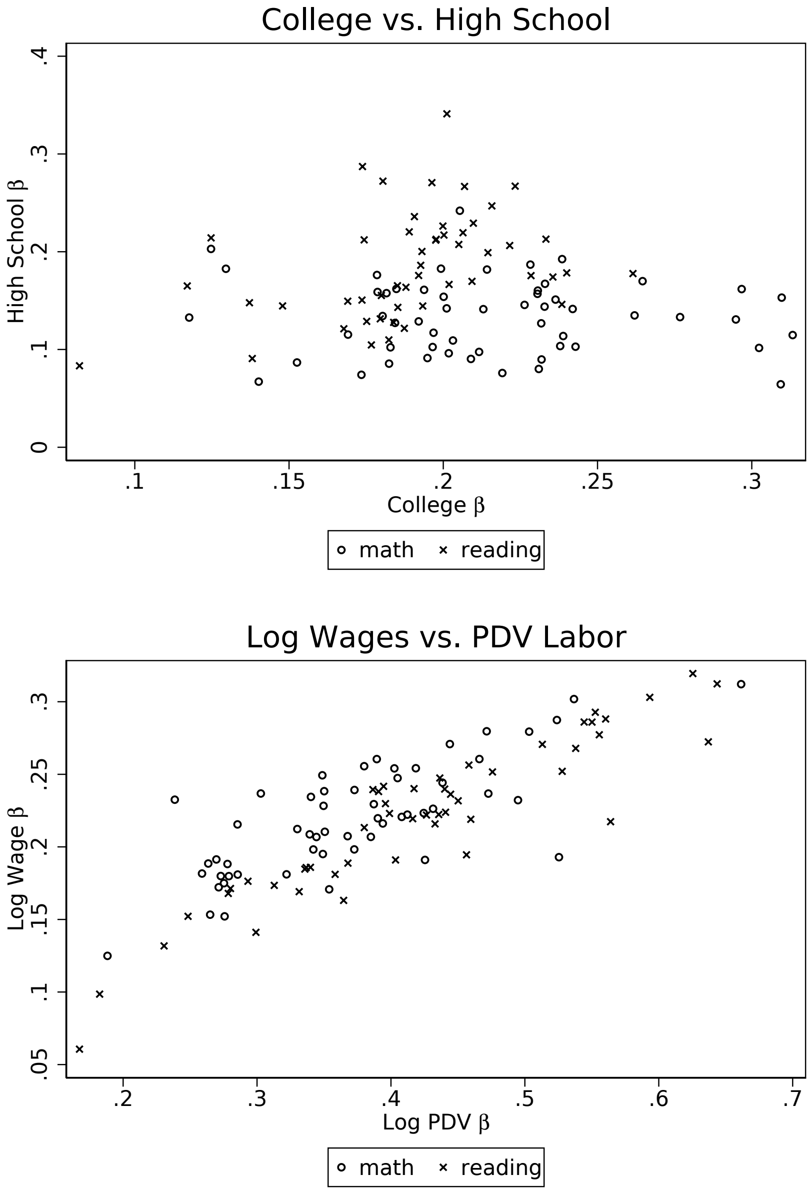
Notes: All statistics use base-year sampling weights. Dollar values in 2017-constant dollars deflated using the CPI-U.

Figure 1: Item-by-Item Regression Coefficient Distributions



Notes: Panels plot kernel densities across test items (i) of estimated regression coefficients ($\hat{W}_i$'s) from regressions of the form $y_j = \alpha_i + W_i D_{i,j} + \varepsilon_{i,j}$, where y_j is either a school completion indicator (high school or college) or a labor market outcome (ln(wage_30) or ln(pdv_labor)).

Figure 2: Item-by-Item Regression Coefficient Comparisons



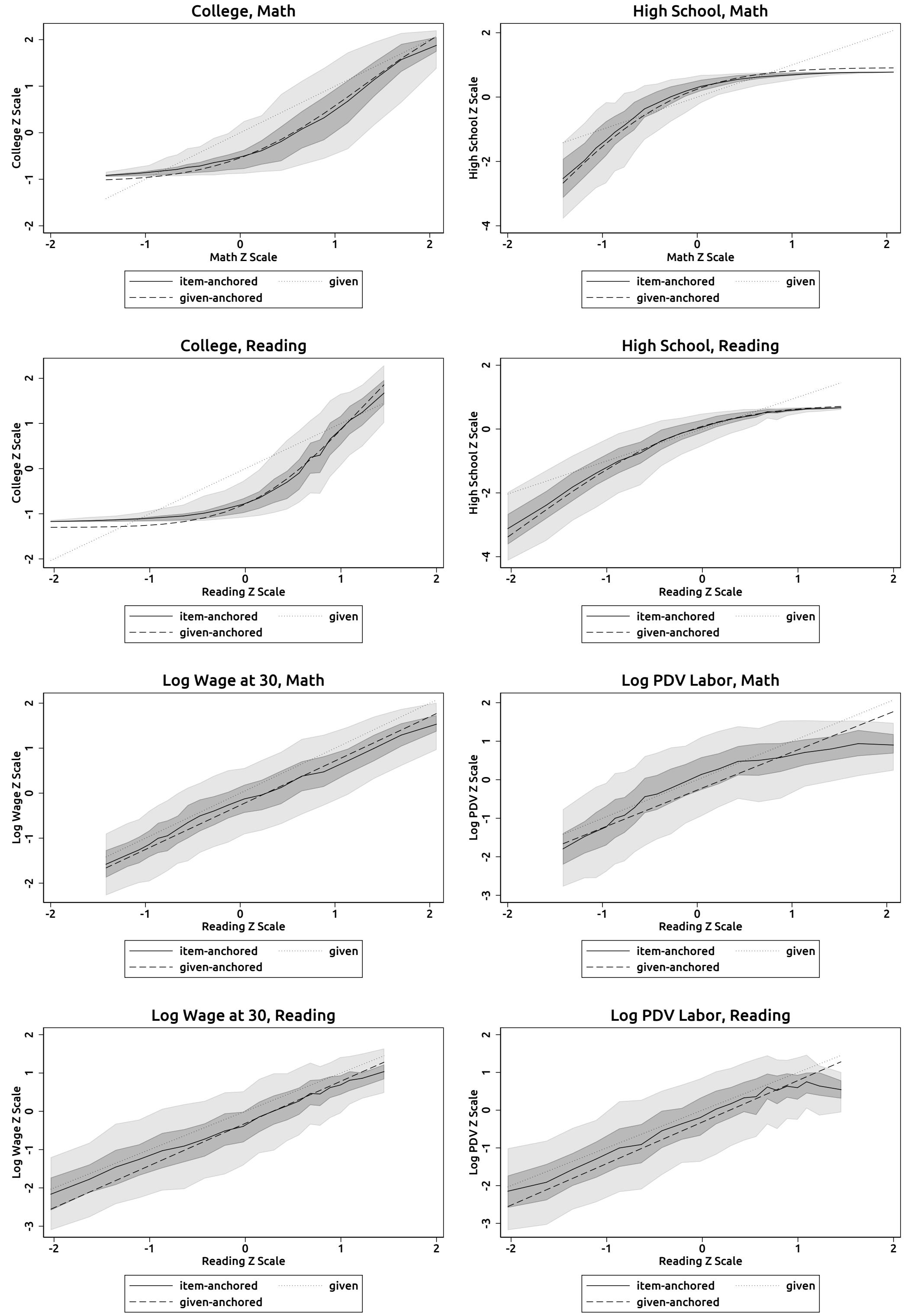
Notes: Panels plot for each test item i pairs of estimated regression coefficients ($\hat{W}_i$'s) from regressions of the form $y_j = \alpha_i + W_i D_{i,j} + \varepsilon_{i,j}$, where y_j is either a school completion indicator (high school or college) or a labor market outcome (ln(wage_30) or ln(pdv_labor)).

Table 2: Item-Outcome Correlations and IRT Parameter Regressions

	(1) wage item	(2) wage full	(3) pdv item	(4) pdv full	(5) hs item	(6) hs full	(7) col item	(8) col full
discrimination	0.04***	0.00	0.05***	-0.00	0.02***	-0.00	0.03***	-0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
difficulty	-0.03***	0.00	-0.12***	-0.02***	-0.06***	-0.01***	0.01**	0.02***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
guessing	-0.08*	-0.02	-0.07	0.00	-0.00	0.01	-0.10**	-0.01
	(0.05)	(0.03)	(0.09)	(0.06)	(0.03)	(0.02)	(0.04)	(0.03)
Obs.	105	105	105	105	105	105	105	105
Adj. R^2	0.372	-0.022	0.566	0.086	0.738	0.317	0.482	0.205

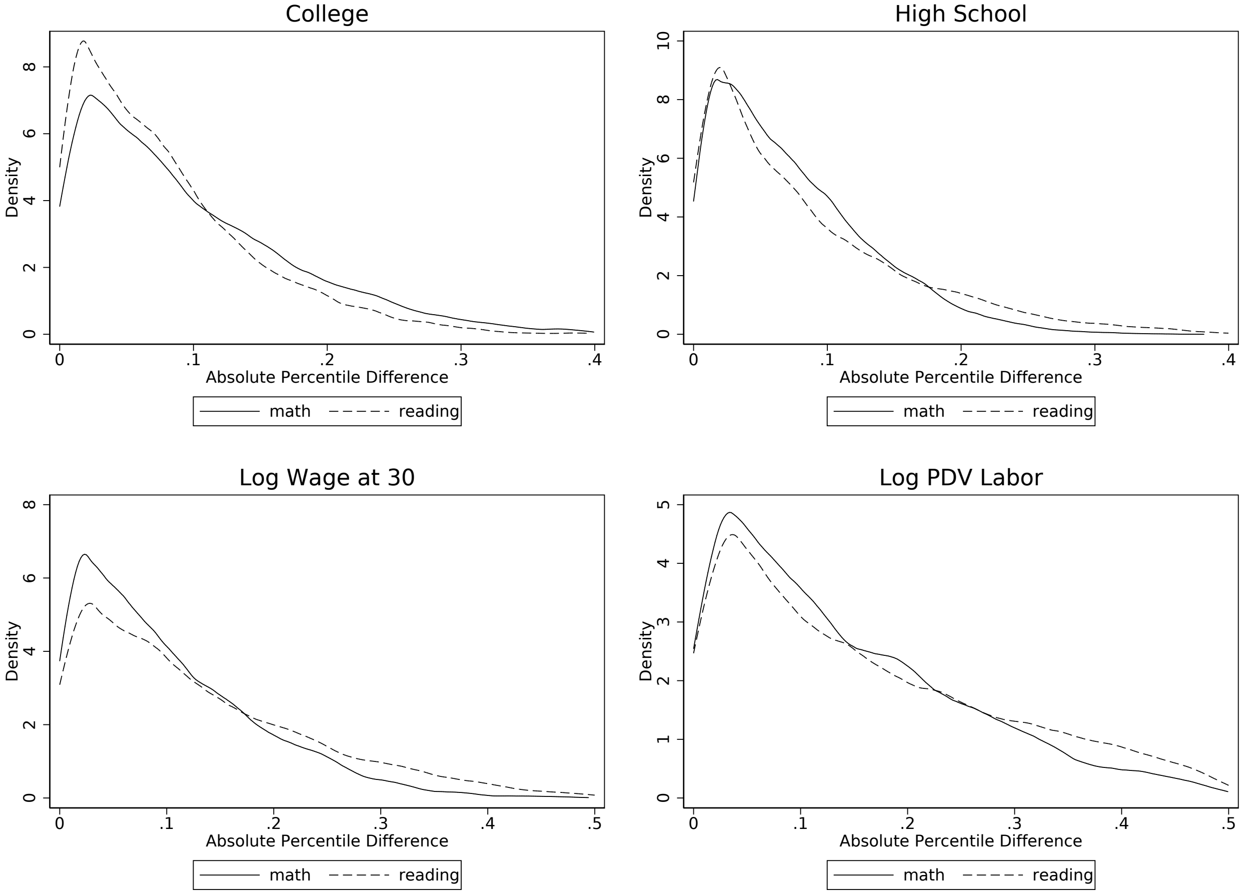
Notes: Each column represents a regression of the form $\hat{W}_i = \delta_0 + \delta_1 \text{discrimination}_i + \delta_2 \text{difficulty}_i + \delta_3 \text{guessing}_i$. All regressions pool math and reading items together. $\hat{W}_i$ is the estimated regression coefficient for item i in a regression of an economic outcome on item(s) and discrimination$_i$, difficulty$_i$, and guessing$_i$ are the irt-estimated discrimination, difficulty, and guessing probability for item i. The odd-numbered columns ("item") use $\hat{W}_i$'s estimated separately item-by-item, while the even-numbered columns ("full") use $\hat{W}_i$'s estimated jointly across all math or reading items. *** denotes significance at the 1% level; ** denotes significance at the 5% level; and * denotes significance at the 10% level.

Figure 3: The Relationship Between the Item-Anchored and NLSY79 Scales



Notes: Each panel plots the mean, along with the middle 50% and 90% range, of the item-anchored scores in each ventile of the NLSY79-given score distribution. The given-anchored lines likewise plot the mean of the given-anchored scores (predicted values from regression/probit models relating NLSY79-given scores to the outcome) in each ventile of the NLSY79-given score distribution.

Figure 4: Percentile Differences Between the Item-Anchored and NLSY79 Scales



Notes: Each panel plots for math and reading a kernel density estimate of the distribution of the absolute value of the percentile differences between the item-anchored and NLSY79-given scores. In detail, if p is the percentile of a student in the NLSY79-given score distribution and q is her percentile in the item-anchored distribution, the panels plot the density of $|p-q|$.

Table 3: Bootstrapped Standard Errors for the Item-Anchored Achievement Gaps

White/Black		naive	bootstrapped
math	hs	0.03	0.05
reading	hs	0.03	0.05
math	college	0.03	0.03
reading	college	0.04	0.05
math	log labor	0.03	0.04
reading	log labor	0.04	0.05
math	log wages	0.03	0.05
reading	log wages	0.03	0.07
math	log wages (median anchor)	0.03	0.04
reading	log wages (median anchor)	0.03	0.05
Male/Female			
math	hs	0.02	0.03
reading	hs	0.02	0.03
math	college	0.02	0.03
reading	college	0.03	0.04
math	log labor	0.02	0.03
reading	log labor	0.02	0.04
math	log wages	0.02	0.04
reading	log wages	0.02	0.04
math	log wages (median anchor)	0.02	0.03
reading	log wages (median anchor)	0.02	0.03
High/Low			
math	hs	0.03	0.05
reading	hs	0.03	0.05
math	college	0.03	0.05
reading	college	0.04	0.06
math	log labor	0.03	0.05
reading	log labor	0.03	0.05
math	log wages	0.03	0.05
reading	log wages	0.03	0.06
math	log wages (median anchor)	0.03	0.05
reading	log wages (median anchor)	0.03	0.05

Notes: The naive standard errors are calculated without accounting for the sampling variation in the shrinkage adjustment factor $1/\hat{\gamma}^{(1)}$ or in the item-anchored scales. The bootstrap standard errors are based on a normal approximation using 250 bootstrapped estimates where the instruments use $\hat{A}^{(2)}$ sorted into 20 (rather than 100) equinumerous bins.

Table 4: Item-Anchored School Completion Gaps

White/Black	given (z)	given-anchored (z)	item-anchored (z)	predicted	actual	item R
math, college	0.98	0.98	0.81	0.20	0.13	0.87
	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	.
reading, college	1.05	1.22	1.28	0.25	0.13	0.74
	(0.02)	(0.04)	(0.04)	(0.01)	(0.01)	.
math, hs	0.98	1.21	1.21	0.15	0.06	0.81
	(0.03)	(0.03)	(0.03)	(0.00)	(0.01)	.
reading, hs	1.05	1.40	1.22	0.17	0.06	0.86
	(0.02)	(0.03)	(0.03)	(0.00)	(0.01)	.
Male/Female						
math, college	0.18	0.23	0.13	0.03	-0.00	0.87
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	.
reading, college	-0.11	-0.10	0.01	0.00	-0.00	0.74
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	.
math, hs	0.18	0.12	0.05	0.01	-0.04	0.81
	(0.02)	(0.02)	(0.02)	(0.00)	(0.01)	.
reading, hs	-0.11	-0.16	-0.09	-0.01	-0.04	0.86
	(0.02)	(0.02)	(0.02)	(0.00)	(0.01)	.
High/Low Income						
math, college	0.99	1.07	0.94	0.23	0.29	0.87
	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	.
reading, college	0.90	1.16	1.20	0.23	0.29	0.74
	(0.03)	(0.03)	(0.04)	(0.01)	(0.01)	.
math, hs	0.99	1.09	1.10	0.14	0.19	0.81
	(0.03)	(0.03)	(0.04)	(0.00)	(0.01)	.
reading, hs	0.90	1.13	1.03	0.14	0.19	0.86
	(0.03)	(0.04)	(0.03)	(0.00)	(0.01)	.

Notes: The first column shows gaps calculated using the NLSY79-given scales in sd units. The second column shows gaps calculated using given-anchored scales in sd units, and the third uses the item-anchored scales in sd units. The fourth column shows the predicted school completion gaps using the item-anchored scales, while the fifth column shows the actual gaps. The sixth column shows the estimated reliabilities of the item-anchored scales following the method outlined in Section 6. Estimates based on probit anchoring models that include age indicators in addition to item indicators. Instruments use the outcome scale divided into 100 equinumerous bins. Standard errors treat the estimated reliabilities as known with certainty. Please see Table 3 for bootstrapped alternatives.

Table 5: Item-Anchored Log Labor Earnings Gaps

White/Black	given (z)	given-anchored (z)	item-anchored (z)	predicted	actual	item R
math, wage	0.98	1.13	1.21	0.25	0.24	0.75
	(0.03)	(0.03)	(0.04)	(0.01)	(0.02)	.
reading, wage	1.05	1.42	1.20	0.23	0.24	0.87
	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	.
math, pdv	0.98	1.13	1.49	0.46	0.45	0.69
	(0.03)	(0.03)	(0.04)	(0.01)	(0.03)	.
reading, pdv	1.05	1.42	1.34	0.41	0.45	0.75
	(0.02)	(0.03)	(0.04)	(0.01)	(0.03)	.
Male/Female						
math, wage	0.18	0.20	0.24	0.05	0.22	0.75
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	.
reading, wage	-0.11	-0.14	-0.10	-0.01	0.22	0.87
	(0.02)	(0.02)	(0.02)	(0.00)	(0.01)	.
math, pdv	0.18	0.20	0.25	0.07	0.47	0.69
	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	.
reading, pdv	-0.11	-0.14	-0.08	-0.03	0.47	0.75
	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	.
High/Low Income						
math, wage	0.99	1.14	1.19	0.24	0.46	0.75
	(0.03)	(0.03)	(0.04)	(0.01)	(0.02)	.
reading, wage	0.90	1.23	1.09	0.20	0.46	0.87
	(0.03)	(0.04)	(0.03)	(0.01)	(0.02)	.
math, pdv	0.99	1.14	1.18	0.36	0.82	0.69
	(0.03)	(0.03)	(0.04)	(0.01)	(0.03)	.
reading, pdv	0.90	1.23	1.06	0.32	0.82	0.75
	(0.03)	(0.04)	(0.04)	(0.01)	(0.03)	.

Notes: The first column shows gaps calculated using the NLSY79-given scales in sd units. The second column shows gaps calculated using given-anchored scales in sd units, and the third uses the item-anchored scales in sd units. The fourth column shows the predicted ln(wage_30) and ln(pdv_labor) gaps using the item-anchored scales, while the fifth column shows the actual gaps. The sixth column shows the estimated reliabilities of the item-anchored scales following the method outlined in Section 6. Estimates based on regression anchoring models that include age indicators in addition to item indicators. Item-anchored scales constructed using white men only. Instruments use the outcome scale divided into 100 equinumerous bins. Standard errors treat the estimated reliabilities as known with certainty. Please see Table 3 for bootstrapped alternatives.

Table 6: Item-Anchored Log Labor Earnings Gaps, Full Sample

White/Black	given (z)	given-anchored (z)	item-anchored (z)	predicted	actual	item R
math, wage	0.98	1.13	1.18	0.28	0.24	0.84
	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	.
reading, wage	1.05	1.42	1.33	0.28	0.24	0.82
	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	.
math, pdv	0.98	1.13	1.22	0.46	0.45	0.86
	(0.03)	(0.03)	(0.03)	(0.01)	(0.03)	.
reading, pdv	1.05	1.42	1.26	0.45	0.45	0.89
	(0.02)	(0.03)	(0.03)	(0.01)	(0.03)	.
Male/Female						
math, wage	0.18	0.20	0.25	0.06	0.22	0.84
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	.
reading, wage	-0.11	-0.14	0.17	0.04	0.22	0.82
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	.
math, pdv	0.18	0.20	0.24	0.09	0.47	0.86
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	.
reading, pdv	-0.11	-0.14	0.16	0.05	0.47	0.89
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	.
High/Low Income						
math, wage	0.99	1.14	1.13	0.26	0.46	0.84
	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	.
reading, wage	0.90	1.23	1.22	0.25	0.46	0.82
	(0.03)	(0.04)	(0.03)	(0.01)	(0.02)	.
math, pdv	0.99	1.14	1.02	0.38	0.82	0.86
	(0.03)	(0.03)	(0.03)	(0.01)	(0.03)	.
reading, pdv	0.90	1.23	1.03	0.36	0.82	0.89
	(0.03)	(0.04)	(0.03)	(0.01)	(0.03)	.

Notes: The first column shows gaps calculated using the NLSY79-given scales in sd units. The second column shows gaps calculated using given-anchored scales in sd units, and the third uses the item-anchored scales in sd units. The fourth column shows the predicted ln(wage_30) and ln(pdv_labor) gaps using the item-anchored scales, while the fifth column shows the actual gaps. The sixth column shows the estimated reliabilities of the item-anchored scales following the method outlined in Section 6. Estimates based on regression anchoring models that include age indicators in addition to item indicators. Item-anchored scales constructed using the full sample. Instruments use the outcome scale divided into 100 equinumerous bins. Standard errors treat the estimated reliabilities as known with certainty. Please see Table 3 for bootstrapped alternatives.

Table 7: Item-Anchored Log Labor Earnings Gaps, Male Sample

White/Black	given (z)	given-anchored (z)	item-anchored (z)	predicted	actual	item R
math, wage	1.04	1.20	1.27	0.25	0.30	0.75
	(0.04)	(0.05)	(0.06)	(0.01)	(0.03)	.
reading, wage	1.06	1.45	1.21	0.23	0.30	0.87
	(0.04)	(0.05)	(0.05)	(0.01)	(0.03)	.
math, pdv	1.04	1.20	1.54	0.47	0.56	0.69
	(0.04)	(0.05)	(0.06)	(0.02)	(0.04)	.
reading, pdv	1.06	1.45	1.39	0.42	0.56	0.75
	(0.04)	(0.05)	(0.06)	(0.02)	(0.04)	.

High/Low Income						
math, wage	0.96	1.12	1.15	0.22	0.47	0.75
	(0.04)	(0.05)	(0.06)	(0.01)	(0.03)	.
reading, wage	0.93	1.26	1.15	0.21	0.47	0.87
	(0.04)	(0.05)	(0.05)	(0.01)	(0.03)	.
math, pdv	0.96	1.12	1.20	0.36	0.85	0.69
	(0.04)	(0.05)	(0.06)	(0.02)	(0.04)	.
reading, pdv	0.93	1.26	1.21	0.37	0.85	0.75
	(0.04)	(0.05)	(0.06)	(0.02)	(0.04)	.

Notes: The first column shows gaps calculated using the NLSY79-given scales in sd units. The second column shows gaps calculated using given-anchored scales in sd units, and the third uses the item-anchored scales in sd units. The fourth column shows the predicted ln(wage_30) gaps using the item-anchored scales, while the fifth column shows the actual gaps. The sixth column shows the estimated reliabilities of the item-anchored scales following the method outlined in Section 6. Estimates based on regression anchoring models that include age indicators in addition to item indicators. Item-anchored scales constructed using white men only. Instruments use the outcome scale divided into 100 equinumerous bins. Standard errors treat the estimated reliabilities as known with certainty.

Table 8: Item-Anchored Log Wage Gaps Using Median Regression

White/Black	given (z)	given-anchored (z)	item-anchored (z)	predicted	actual	item R
math	0.98	1.13	1.48	0.34	0.24	0.64
	(0.03)	(0.03)	(0.04)	(0.01)	(0.02)	.
reading	1.05	1.42	1.27	0.25	0.24	0.78
	(0.02)	(0.03)	(0.04)	(0.01)	(0.02)	.
Male/Female						
math	0.18	0.20	0.31	0.05	0.22	0.64
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	.
reading	-0.11	-0.14	-0.18	-0.05	0.22	0.78
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	.
High/Low Income						
math	0.99	1.14	1.44	0.30	0.46	0.64
	(0.03)	(0.03)	(0.04)	(0.01)	(0.02)	.
reading	0.90	1.23	1.12	0.21	0.46	0.78
	(0.03)	(0.04)	(0.04)	(0.01)	(0.02)	.

Notes: The first column shows gaps calculated using the NLSY79-given scales in sd units. The second column shows gaps calculated using given-anchored scales in sd units, and the third uses the item-anchored scales in sd units. The fourth column shows the predicted ln(wage_30) gaps using the item-anchored scales, while the fifth column shows the actual gaps. The sixth column shows the estimated reliabilities of the item-anchored scales following the method outlined in Section 6. Estimates based on median regression anchoring models as outlined in Section 6.4 that include age indicators in addition to item indicators. Instruments use the outcome scale divided into 100 equinumerous bins. Standard errors treat the estimated reliabilities as known with certainty. Please see Table 3 for bootstrapped alternatives.

Table 9: Reading Puzzle Regressions – Wages at Age 30

	(1) given	(2) given-anchored	(3) item-anchored	(4) given	(5) given-anchored	(6) item-anchored
math	0.17^{***}	0.17^{***}	0.16^{***}	0.10^{***}	0.11^{***}	0.11^{***}
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
reading	0.03	0.03	0.09^{***}	-0.01	-0.01	0.06^{***}
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
education	no	no	no	yes	yes	yes
parental income	no	no	no	yes	yes	yes
white male only	yes	yes	yes	yes	yes	yes
Observations	2,306	2,306	2,217	2,232	2,232	2,142
Adjusted R^2	0.12	0.12	0.16	0.17	0.17	0.20

Notes: Table shows the estimated coefficients on math and reading for regression of the form $\ln(wage) = \alpha + \beta_1 math + \beta_2 read + \gamma X + \varepsilon$, where X denotes education, race, and parental income controls (or not, as indicated). Column labels correspond to the math and reading test scores used (given, given-anchored, or item-anchored). All regressions use test scores in sd units. Standard errors based on 1,000 bootstrap iterations. *** denotes significance at the 1% level; ** denotes significance at the 5% level; and * denotes significance at the 10% level.

Table 10: Reading Puzzle Regressions – Wages at Age 30, Alternative Scales

	(1) ln_w30 item	(2) college item	(3) college given	(4) high school item	(5) high school given
math	0.11^{***}	0.07^{***}	0.10^{***}	0.05^{**}	0.08^{***}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
reading	0.06^{***}	0.03^{*}	-0.02	0.04^{**}	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
education	yes	yes	yes	yes	yes
parental income	yes	yes	yes	yes	yes
white male only	yes	yes	yes	yes	yes
Observations	2,142	2,142	2,232	2,142	2,232
Adjusted R^2	0.20	0.16	0.17	0.16	0.16

Notes: Table shows the estimated coefficients on math and reading for regression of the form $\ln(wage) = \alpha + \beta_1 math + \beta_2 read + \gamma X + \varepsilon$, where X denotes education and parental income controls (or not, as indicated). Column labels correspond to the different anchoring outcomes for the math and reading scales. All regressions use test scores in sd units. Standard errors based on 1,000 bootstrap iterations. *** denotes significance at the 1% level; ** denotes significance at the 5% level; and * denotes significance at the 10% level.