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Abstract

The US experienced two dramatic changes in the structure of education in a fifty year period. The first was a large expansion of educational attainment; the second, an increase in test score gaps between college bound and non-college bound students. We study the impact of these two trends on the composition of school groups by observed ability and the importance of these composition effects for wages. Our main finding is that there is a growing gap between the abilities of high school and college-educated workers that accounts for one-half of the college wage premium for recent cohorts and for the entire rise of the college wage premium for the 1910-1960 birth cohorts.

JEL-Code: I200, J240.

Keywords: education, ability, skill premium.

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1 Introduction

The twentieth century witnessed an extraordinary and well-documented expansion of education in the United States (Goldin and Katz 2008). Figure 1a illustrates this trend. For the birth cohorts born every ten years between 1910 and 1960, it displays the fraction of white men in four exhaustive and mutually exclusive education categories: high school dropouts (<HS), high school graduates (HS), those with some college but not a four-year degree (SC), and college graduates with at least a four-year degree (C+). Of the men born in 1910, only one-third finished high school. By the 1960 cohort, high school graduation had become nearly universal and the median man attended at least some college.[1]

At the same time that high school completion and college enrollment were expanding, there was also a systematic and less well-known change in who pursued higher education. The general trend was for education to become more meritocratic, with ability and preparation becoming better predictors of educational attainment. In this paper we build on earlier work by Taubman and Wales (1972) and provide systematic evidence of this trend by comparing the standardized test scores for those who stop their education with a high school degree (the HS group) and those who continue to college (the SC and C+ groups). Figure 1b plots the average percentile rank of these two groups against the birth cohort; as is explained in Section 2, each pair of data points represents the results of a separate study. The trend is striking. For the very earliest cohorts, students who did not continue on to college scored only ten percentage points lower than students who did. By the 1940s cohorts, that gap had grown to nearly thirty percentage points.

Our main idea is that these two trends have combined to change the composition of cognitive abilities by educational attainment for different cohorts. For example, it is unlikely that the ability of high school dropouts is the same for the 1910 and 1960 cohorts, given that more than half of the 1910 cohort dropped out but less than ten percent of the 1960 cohort did. Likewise, the ability of college graduates is likely to have changed given the large expansion of college enrollment and the changes in how college students are selected.

Our primary motivation for studying compositional effects is to understand their importance for the evolution of wage patterns over the course of the twentieth century. To be concrete, we focus on two well-known features of the college wage premium. First, the college wage premium rose by 15 percentage points between the 1910 and 1960 cohorts.[2]

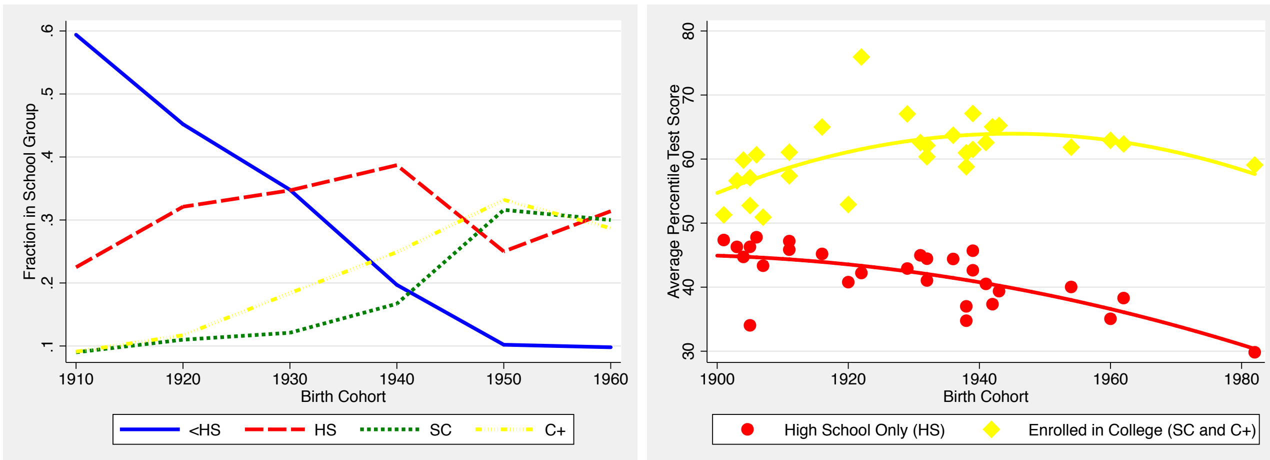
[1]These data are derived from the 1950–2000 population censuses. We focus on cohorts born at ten year intervals to match with the ten year intervals between censuses. Each data point represents average schooling at age 40 for the relevant cohort. For more details on the construction of the data in figure 1, see Appendix A.1 and the Online Appendix.

[2]Katz and Murphy (1992), Bound and Johnson (1992), Autor, Katz, and Krueger (1998), and Goldin

Figure 1: Changes in US Education in the Twentieth Century

(a) The Expansion of Education

(b) Changes in Test Scores by Attainment



Second, the current college wage premium is 50 percentage points, which is difficult to reconcile with the low college completion rate in human capital models.[3] We establish in this paper that changes in the composition of student abilities by educational attainment between the 1910 and 1960 cohorts can quantitatively explain the entire rise in the college wage premium while simultaneously making it easier to reconcile the current college wage premium with human capital theory.

To fix ideas, we think of the average log-wages of workers with a particular educational attainment as being a function of the price of skills specific to that education group and the quantity of those skills the average worker provides. The quantity is in turn determined by workers' cognitive abilities and the human capital they acquire over the course of their lives. Much of the previous literature seeking to explain the college wage premium holds the quantity of skills fixed and focuses on reasons why skill prices may have changed – for example, due to skill-biased technological change. We allow for either component of wages to change. The primary challenge we face is that while mean wages are observed directly, the other terms – skill prices, human capital, and ability – are not. Our approach to this problem is to use the information provided by standardized test scores. We treat test scores as observed, noisy proxies for cognitive ability. We use them to disentangle the

and Katz (2008) propose skill-biased technological change as an explanation for the rising skill premium. Bound and Johnson (1992) and the survey of Levy and Murnane (1992) propose other explanations including international trade or migration.

[3]See for example Heckman, Lochner, and Todd (2006) and Heckman, Lochner, and Todd (2008), who also propose an alternative extension to reconcile the model with the data.

role of cognitive ability from the other two factors. Our methodology does not allow us to separate skill prices from human capital.

We begin by writing down a simple model of school choice with heterogeneous ability that formalizes the challenge we face. We show that the quantitative impact of compositional effects on wages are controlled by two parameters. The first governs how strongly sorted the different school groups are by ability; more sorting means larger gaps in mean ability between school groups. The second parameter governs the mapping from ability to wages; a higher value for this parameter means that mean ability gaps have larger implication for wages. We take this model to the data in two steps.

First, we calibrate the model to the NLSY79 (Bureau of Labor Statistics; US Department of Labor 2002). The NLSY79 is a representative sample of cohorts born around 1960 that includes information on their wages, education, and test scores. We construct two key moments from this data set: the relationship between wages and test scores, and the degree of educational sorting by test scores. We begin by following the previous economic literature and consider the special case where test scores measure cognitive ability exactly (Heckman, Lochner, and Taber 1998, Garriga and Keightley 2007). In this case our two empirical moments identify the two key parameters of the model and we can provide some simple results. However, we also draw on evidence from the psychometric literature to establish that test scores are likely a noisy measure of cognitive ability. We show how to bound the plausible amount of noise in test scores and recalibrate our model. We find that differences in mean ability between college and high school graduates likely account for half of the observed college wage premium.

Then we calibrate the model to fit the historical changes in schooling and test scores from figure 1. Our main result is that the mean ability of college relative to high school graduates rose by 14 percentage points, enough to explain almost all of the college wage premium between the 1910 and 1960 cohorts. We provide decompositions to show that the expansion of education and the increase in sorting each explain about half of the total result. Finally, we provide a number of robustness checks on the calibration exercise and for the key empirical moments that identify our model.

Our paper is most closely related to two existing literatures. First, our empirical work on changes in the relationship between test scores and educational attainment over time builds on prior work by Finch (1946) and particularly Taubman and Wales (1972). The latter paper documented the spread in test scores between those who start and do not start college. This finding seems to have been largely forgotten, likely because it was published at a time when the college wage premium was declining, obscuring any possible link between test scores and wage patterns. In addition to returning attention to this important finding, we greatly expand the number of data points and the documentation of these trends.

Second, our paper is related to a literature that decomposes observed changes in educational wage differences into the underlying changes in skill prices and skill quantities. The fundamental challenge this literature faces is that neither skill prices nor skill quantities are directly observed. The literature has addressed this problem in a variety of ways.

A number of studies specify models of wage determination that motivate regressing wages or skill premiums on cohort education as a proxy for cohort quality (Juhn, Kim, and Vella 2005, Kaymak 2009, Carneiro and Lee 2011). Juhn, Murphy, and Pierce (1993) and Acemoglu (2002) use differences in wage growth between cohorts to eliminate cohort and age effects, thus identifying skill price changes. A final set of papers draws on models of human capital accumulation to disentangle skill prices from skill quantities. Laitner (2000) formulates a model that qualitatively generates predictions for relative wages and wage inequality consistent with post-war U.S. data. However, he does not attempt to quantify the implications of the model. Bowlus and Robinson (2012) estimate time series of skill prices for four school groups using the flat spot method developed by Heckman, Lochner, and Taber (1998).[4]

In spite of the small number of studies, the approaches and findings are quite diverse. While a number of studies find that the expansion of education led to a modest reduction in the college wage premium (Juhn, Kim, and Vella 2005, Carneiro and Lee 2011), other studies infer a sizeable increase (Kaymak 2009, Bowlus and Robinson 2012). We interpret the diversity of the findings as an indication that additional data may be needed to solve the identfication problem associated with decomposing wages into skill prices and quantities. This motivates our paper and its main departure from the literature.

We present new data measuring the cognitive abilities of cohorts born between 1901 and 1982. We document a widening test score disparity between college educated versus high school educated workers and quantify the implications for long-run educational wage premiums in a transparent model. This approach conveys two benefits relative to the literature:

1. Our data directly measure how at least one aspect of cohort quality changes over time. They suggest potentially important changes in the composition of education groups over time.

2. The new data cover a long period (the 1901–1982 birth cohorts) in a consistent way.

[4] Also related is Carneiro and Lee (2009) who estimate the effect of a counterfactual expansion of college enrollment among students born around 1960 using a local instrumental variable approach.

The longer coverage is important because the data indicate that the largest changes in the test score gap occurred before the 1930 cohort, with the rate of change slowing over time.

Our approach is designed primarily to quantify the importance of changing test score gaps as a proxy for changing cognitive ability gaps. It follows that we do not quantify abilities that are uncorrelated with test scores or are unobserved altogether. We refer interested readers to an existing literature especially on changes in the price and quantity of unobserved abilities. That literature has not reached a consensus on whether these changes contribute to the rise in the college wage premium in an important way (Chay and Lee 2000, Taber 2001, Deschenes 2006). It is possible that such changes may accentuate or partly undo our conclusion about changes in cognitive abilities.

The rest of the paper is organized as follows. Section 2 briefly gives details on the rising test score gap between high school graduates and college-goers. Section 3 introduces our model of school choice. Section 4 calibrates the model to the NLSY79 and derives cross-sectional results. Section 5 calibrates the model to the time series data and derives further results. Section 6 provides robustness checks and the final section concludes.

2 The Changing Relationship Between Test Scores and College Attendance

The first contribution of this paper is to provide extensive documentation on the divergence of test scores between high school graduates who continued to college and those who did not. Our main source of data is two dozen studies conducted by psychologists and educational researchers around the country. In this section we provide a brief overview of the content of these studies and how we combined them with results from the more recent, nationally representative samples such as the NLSY79 to generate Figure 1b. A longer description of our procedures, along with references, detailed metadata on the different studies, and a number of robustness checks, is available in an online appendix.

Our starting point was to collect every study we could find with data on the test scores of high school graduates who do and do not continue to college. We focused particularly on studies that predate the availability of large, nationally representative datasets such as the NLSY. The first such studies were conducted shortly after World War I and tested students who were born just after the turn of the century.[5] We have collected more than two dozen

[5] The U.S. Armed Forces made heavy use of group intelligence tests in assigning recruits to positions

such studies. The studies vary in terms of size, geographic scope, test instrument, and so on, but it is useful to describe a typical study, which comes in two parts. First, the researcher would arrange for a large number of high schools in a metropolitan area or a state (sometimes all such high schools) to administer an aptitude or achievement test to high school seniors. Second, the researcher would collect information on the college-going behavior of the students, either by asking them their plans as high school seniors, or by re-surveying the students, their parents, or schools a year or two later after their graduation. We are interested in cross-tabulations of test scores and college-going behavior.

Since many of these studies are quite old the original raw data do not exist. Instead, we rely on the reported summary statistics and tabulations from published articles, mimeographs, books, and dissertations. One commonly reported table gives the number of students with scores in various ranges that did and did not continue to college. Following Taubman and Wales (1972), we convert score levels to percentiles, and then compute the average percentile rank of those who do and do not continue to college from these discretized distributions. This measure can be computed from most of the studies. We then add to these data by computing the same figure for recent, nationally representative samples, including the NLSY79.

The resulting data are plotted in Figure 1b. The trend is striking. For cohorts born around the turn of the 20th century there was a very small test score gap between those who continued to college and those who did not, on the order of 10 percentage points. The earliest studies expressed consistent surprise at how many low-scoring students continued to college and how many high-scoring students did not. The gap between the two groups grew steadily from the 1900 to the 1940 cohort, at which point it plateaued at nearly 30 percentage points.[6] Contemporary sources pointed to two reasons why the gap was growing. First, it became increasingly common for universities to administer tests to applicants as an admissions tool. Second, high schools administered tests to their students with an aim towards vocational guidance. Since tests were often interpreted as measures of academic ability, students who scored well were encouraged to continue their education while those who did not were pushed towards vocational tracks.

We document in the Online Appendix the robustness of this basic finding. We show there that a similar pattern emerges if we use alternative metrics to measure how strongly

during the War. Their use in this context increased awareness and interest among the public and researchers, and provided an opening for their broader acceptance and adoption. Hence, the first studies were conducted immediately after the War (Cremin 1961).

[6] Our finding is closely related to that of Hoxby (2009), who documents a complementary trend of increasing sorting of students by test scores among colleges.

sorted the college goers and non-goers are. We provide references and evidence that the tests used in early years appear to have been of quality similar to those from more recent years, as measured by inter-test correlations or the usefulness of tests for predicting subsequent college grades. Finally, although there are methodological differences between studies, such as when they followed up with students or where the survey was conducted, we find similar trends if we restrict our attention to studies that were similar along multiple dimensions. We conclude that the testing movement influenced who chose to stop their education with high school and who pursued college. In the next section we introduce a model to allow us to analyze the importance of such a shift for wage patterns.

3 A Model of School Choice

Our first goal is to specify a parsimonious model of school choice that formalizes the intuition from the introduction. We show that the quantitative magnitude of our results depends on two key parameters. The model guides our subsequent empirical work.

The basic environment is a discrete time overlapping generations model. Each year a cohort of unit measure is born. Individuals are indexed by their year of birth τ as well as their age v, with the current period given by $\tau + v - 1$. Individuals live for a fixed T periods.

3.1 Endowments

Each person is endowed with a variety of idiosyncratic, time-invariant traits that affect their wages and schooling.[7] We assume that these traits are captured by a two-dimensional endowment (a, p). a represents ability. Ability is useful for both work and school, because it makes it easier to learn and process new information or perform new tasks. p represents the taste for schooling. It is a preference parameter that captures the relative disutility that a person derives from spending time in school instead of working. The two traits are assumed to be independent without loss of generality. We assume that abilities are drawn from a time-invariant standard normal distribution. Given the assumptions that we make below, both the mean and the standard deviation of this distribution can be normalized in this way. An individual's tastes for schooling are also drawn from a normal distribution with mean 0 and a standard error $\sigma_{p,\tau}$. Given that these are the only two endowments in

[7] These traits may be malleable earlier in life. We focus on school choices made from young adulthood onward. Many of the relevant traits appear to difficult to change by age 16.

the model, we can denote by $q = (a, p, \tau)$ the type of an agent, their endowment and their birth cohort.

3.2 Preferences

Let $c(q, v)$ denote the consumption of a person of type q at age v, and let $\beta > 0$ be the common discount factor. Then lifetime utility is given by:

$$\sum_{v=1}^{T} \beta^v \log[c(q, v)] - \exp[-(p + a)]\chi(s, \tau). \tag{1}$$

Workers value consumption in the standard way. They also place a direct utility value on their time spent in school, which is determined by the interaction between a worker-specific component $(p+a)$ and a cohort and school-specific component $\chi(s, \tau)$. The former term captures how enjoyable (p) and easy (a) a particular individual finds schooling to be. The functional form $-\exp[-(p + a)]$ assumes that school is distasteful, but less so for more cognitively able students or those with higher taste for schooling. The latter term captures how desirable school type s and its associated career paths are for cohort τ. It varies by cohort to capture changes in school and work, such as the amount of studying required to succeed in college or the career paths open to those with a particular educational attainment. We restrict χ to be positive and increasing in s. In this case, the preferences show complementarity between school and cognitive ability or taste for school. This complementarity is essential for our results. We could adopt alternative functional forms that preserve complementarity and our results would obtain; we have chosen this functional form as the simplest.

3.3 Budget Constraint

School type s takes $T(s)$ years to complete. While in school, students forego the labor market. After graduation, workers receive earnings $w(s, q, v)$ that depend on their school attainment, age, and ability. Their budget constraint requires them to finance lifetime consumption through lifetime earnings,

$$\sum_{v=1}^{T} \frac{c(q, v)}{R^v} = \sum_{v=T(s)+1}^{T} \frac{w(s, q, v)}{R^v}, \tag{2}$$

where R is the exogenous interest rate.

In keeping with much of the literature, we assume that workers with different educational attainments provide different labor inputs.[8] We assume that wages are given by

$$\log[w(s,q,v)] = \theta a + z(s,\tau+v-1) + h(s,v).$$

Wages have three determinants in our model. As mentioned before, ability affects wages directly. Since we have assumed that ability is distributed standard normal, θ is an important parameter. It measures the increase in wages that comes from a one standard deviation rise in ability. $z(s,\tau+v-1)$ is the price per unit of type s labor supplied by cohort τ at age v. Finally $h(s,v)$ captures the human capital accumulated by workers of education s at age v through experience or learning-by-doing, which has a systematic effect on wages.

3.4 Characterization of School Choice

Workers choose their school attainment s and a consumption path $c(q,v)$ to maximize preferences (1) subject to their budget constraint (2). We characterize the solution in two steps: first, we find the optimal allocation of consumption over time given school choice; then we find the school choice that maximizes lifetime utility.

Consumption in this model satisfies the standard Euler equation, $c(q,v+1) = \beta R c(q,v)$. If we combine this equation with the budget constraint and then plug into the utility function, we can rewrite lifetime utility as:

$$\theta a \sum_{v=1}^{T} \beta^v + \sum_{v=1}^{T} \beta^v \log \left[\frac{R(\beta R)^{v-1}}{\sum_{u=1}^{T} \beta^{u-1}} \sum_{u=T(s)+1}^{T} \frac{e^{h(s,u)+z(s,\tau+u-1)}}{R^u} \right] - \exp[-(p+a)]\chi(s,\tau). \quad (3)$$

This equation has three additive terms. The first term captures the effect of ability on lifetime utility: higher ability allows for higher lifetime consumption. The second term captures the impact of school attainment on lifetime utility: more schooling means fewer years in the labor market but also changes the skill price and the rate of human capital accumulation. Finally, the last term captures the direct utility effect of schooling.

A key property of our model is that school choices depend only on the sum $p+a$, and not on other individual-specific attributes or on p or a independently.[9] To see this, note that

[8] For example, our setup is consistent with the literature that allows high school and college-educated workers to be imperfect substitutes in aggregate production. However, we do not take a stand on the demand side of the market since doing so is not essential to our model. One channel that we are implicitly ruling out is that the rising skill premium may reflect an increase in the rental price of high ability labor relative to low ability labor (Juhn, Murphy, and Pierce 1993, Murnane, Willett, and Levy 1995).

[9] This is the model property that makes it innocuous to assume that p and a are independent. If they

the first term of our indirect utility function depends on ability but does not interact with school choices, so that it drops out of the individual's optimization problem. The second term does not depend on p or a. So endowments interact with school choice only through the third term, which includes the linear combination $p+a$. Our model includes the common property that ability does not affect school choice through the earnings channel, because it raises both the benefits of schooling (higher future wages) and the opportunity cost (higher foregone wages today) proportionally. Instead, ability, tastes, and school choice interact through preferences in the third term. Given our assumptions on $\chi(s,\tau)$, school attainment in our model is increasing in $p+a$. The individuals who have the highest combination of $p+a$ will choose college; those with middling values will choose high school graduation or some college; and those with the lowest values will choose to drop out of high school.

Since ability is one component of the sum $p+a$, the model generates positive but imperfect sorting by ability into school attainment. Further, since the standard deviation of ability is normalized to 1, the degree of sorting by ability into educational attainment is controlled by a single parameter, $\sigma_{p,\tau}$. As $\sigma_{p,\tau}$ rises, more of the variation in $p+a$ comes from variation in p. In this case, workers are less sorted by ability across school groups and mean ability gaps are smaller. In the limiting case of $\sigma_{p,\tau}=\infty$, educational choices are explained entirely by tastes for schooling. In this case, $E(a|s)=E(a)=0$ for all school groups.

3.5 Implications for Mean Ability and Wages

Since the model allows for positive sorting by ability, it generates composition effects that matter for wages. The average wage of workers from cohort τ with education s at age v is given by:

$$E[\log(w)|s,\tau,v]=\theta E[a|s,\tau]+z(s,\tau+v-1)+h(s,v).$$

In our model, these wages are affected by three terms: by $\theta E[a|s,\tau]$, which we call effective ability; by skill prices, z; and by human capital, h. Our goal is to separate out the role of effective ability in explaining wage patterns from the other two terms. We make no attempt to separate out skill prices from human capital endowments in this paper.

The quantitative magnitude of our results depends on two key model parameters. The first is $\sigma_{p,\tau}$, which determines the strength of sorting by ability into different school groups,

were correlated, we could always re-define a as ability plus the correlated component of tastes, and p as the orthogonal component of taste; given that our model depends only on $p+a$, the results will be the same.

which is reflected in $E[a|s,\tau]$ in the average wage equation. The second is θ, which determines the impact of ability on wages. In general, the smaller is $\sigma_{p,\tau}$ and the larger is θ, the larger is the quantitative role for mean ability in explaining observed wage patterns. Other parameters such as β or R matter little or not at all for our quantitative results. Perfect sorting by $p + a$ is critical for this simplification.

3.6 Model Discussion

Our model admits other interpretations that yield similar results. One useful reinterpretation follows Manski (1989). Students still possess ability a, which makes school easier and raises wages, just as in our baseline interpretation. However, students have no tastes for schooling. If they knew their own ability, they would perfectly sort by ability into school attainment. Imperfect sorting in this model comes from the assumption that students are imperfectly informed about their ability, with p representing signal noise and $p + a$ representing their signal of their own ability. Students with better signals of ability further their education, because they anticipate that schooling will be relatively painless. This reinterpretation generates the same prediction of perfect sorting by $p + a$. Because of this the calibration and results from this alternative model would be *identical* to those derived from our baseline model.

Our model does assume only a single stand-in friction that prevents perfect sorting by ability. An alternative approach taken elsewhere is to model multiple frictions in detail (Cunha, Heckman, and Navarro 2005, Navarro 2008). Doing so would complicate our model and identification. However, the primary impediment is that we lack sufficient historical data to calibrate multiple frictions in detail.

An alternative friction to perfect sorting by ability that is not nested by our setup is borrowing constraints. Borrowing constraints differ from tastes because they are asymmetric: they prevent some high-ability students from furthering their education, but have no effect on low-ability students. By contrast, variation in tastes causes some high-ability students to drop out, but it also causes some low-ability students to attain high levels of education. The literature has not arrived at a consensus about the quantitative importance of borrowing constraints. Cameron and Taber (2004) and Stinebrickner and Stinebrickner (2008) find no evidence of borrowing constraints in the United States for recent cohorts of college attendees. We have little evidence as to whether credit constraints were quantitatively important for earlier cohorts. However, evidence gathered in Herrnstein and Murray (1994) suggests that low-ability students are becoming less likely to attend college over time. This information is consistent with a decline in the dispersion of tastes, but not a model featuring only a relaxation of borrowing constraints over time.

Our analysis focuses on measured ability. We abstract from another driving force, namely changes in the price or relative quantity of unmeasured ability across school groups. Whether such changes account for a large part of the rise in the college wage premium remains controversial (Taber 2001, Chay and Lee 2000). We also assume that the return to measured ability is constant over time. This is conservative in our setup. We already find that the gap in measured abilities between groups have grown over time; if we allowed the wage-return to ability to rise at well this would only serve to accentuate our results. Further, our assumption is consistent with the time series evidence in Bowles, Gintis, and Osborne (2001).

4 Calibration to the NLSY79 and Test Scores

Our model is a parsimonious formalization of the basic challenge. Mean wages are affected by skill prices, human capital, and mean ability, none of which are directly observable. In the model, two key parameters determine how important mean ability is for explaining wage patterns. The first is $\sigma_{p,\tau}$, which determines the size of mean ability gaps between school groups; the second is θ, which determines the impact of ability on wages. Now we turn to the question of how standardized test scores can help us calibrate these parameters and quantify the role of ability for wage patterns.

Our primary data source is the NLSY79. The NLSY79 has two properties that make it ideal for our purposes. First, it is a representative sample of persons born between 1957 and 1964. Second, it includes information about the wages, school choices, and AFQT test scores of individuals in the sample. Most other data sets are deficient along one of these dimensions. For example, information on SAT scores are drawn from a non-representative sample, while common data sets such as the population census do not include information on test scores.

We restrict our attention to white men. We exclude women for the typical reason that only a selected sample of women work. Further, the selection process itself may be changing over time. We also exclude minorities because we eventually want to turn our attention to earlier cohorts, for whom discrimination limited school attainment choices and wages. We include members of the supplemental samples, but use weights to offset the oversampling of low income persons. Since everyone born in the NLSY79 is from a narrow range of cohorts, we group them together and call them jointly the 1960 cohort. In this section we focus on the 1960 cohort and provide some initial cross-sectional results; in the next section we generate time series results.

We use as our measure of test score their Armed Forces Qualifying Test (AFQT) score. The AFQT is widely recognized as a cognitive test and AFQT scores are highly correlated with the scores from other aptitude tests. For each person, we construct real hourly wage at age 40, educational attainment, and AFQT score. Students did not take the AFQT at the same age, which affects average scores. We use regressions to remove age effects from AFQT scores in the standard way, then standard normalize the residual. Details are available in the Appendix.

Since test scores play a central role in our analysis, it is important to be precise about how we interpret them. We think of test scores as noisy, scaled proxies for cognitive ability, $\hat{a} = \eta\,(a + \tilde{\varepsilon}_{\hat{a}})$, where η is an unknown scaling factor and $\varepsilon_{\hat{a}}$ is a normal random variable with mean 0 and standard deviation $\sigma_{\hat{a}}$. We standard normalize test scores to remove the scaling factor. Once standard normalized, test scores and the noise term are given by:

$$\hat{a} = \frac{a}{\sqrt{1+\sigma_{\hat{a}}^2}} + \varepsilon_{\hat{a}}$$

$$\varepsilon_{\hat{a}} \sim \mathcal{N}\left(0, \frac{\sigma_{\hat{a}}}{\sqrt{1+\sigma_{\hat{a}}^2}}\right)$$

We now turn to using test scores to quantify the role of ability in wage patterns.

4.1 Results When Test Scores Measure Ability Exactly

Test scores provide us with useful information on the role of ability in school choices and wages. Intuitively, we can use the degree of sorting by test scores into educational attainment as a proxy for the degree of sorting by ability into educational attainment, which helps identify $\sigma_{p,1960}$. Likewise, we can use the effect of test scores on wages as a proxy for the effect of ability on wages, which helps identify θ. To see how this process works, we begin with a special case: $\sigma_{\hat{a}} = 0$. In this special case, test scores measure ability exactly, and our identification and results are straightforward.

We begin by identifying θ. The wage generating process in our model is:

$$\log[w(s, q, v)] = \theta a + z(s, \tau + v - 1) + h(s, v).$$

Generally, we do not have direct information on a. Instead, we have measured test scores. Our empirical counterpart to this regression is to regress wages at age 40 on test scores and

Table 1: Log-Wage Returns to Test Score in the NLSY79

Dependent variable: log-wages	
$\beta_{\hat{a}}$	0.104
	(0.017)
γ_{HS}	0.17
	(0.06)
γ_{SC}	0.35
	(0.06)
γ_{C+}	0.69
	(0.07)
Observations	1942
R^2	0.24

a full set of school dummies:

$$\log(w) = \beta_{\hat{a}}\hat{a} + \sum_s \gamma_s d_s + \varepsilon_w. \tag{4}$$

$\hat{a}$ is the individual's standard normalized test score, and $\beta_{\hat{a}}$ is the coefficient associated with that score. d_s is an indicator variable that takes a value of 1 if the individual has school attainment s. Since we focus on wages at age 40, γ_s captures the joint wage impact of skill prices and human capital; we are unable to separate the two. ε_w is assumed to be a normal random variable that captures factors such as shocks or luck that affect wages but are not associated with test scores, skill prices, or human capital.

Table 1 shows the results of our regression of log-wages on test scores as implemented in the NLSY79. The return to test scores is $\beta_{\hat{a}} = 0.104$. This will be our baseline estimate of the return to test scores for the remainder of the paper; what will change is how we interpret it. In the case where test scores measures ability exactly, the interpretation is straightforward: $\beta_{\hat{a}} = \theta$. A one standard deviation rise in ability (which is the same as test score) raises log-wages by 10.4 percentage points. This is the first key parameter for determining the importance of composition effects.

The second feature of the data that is important for our results is the degree of sorting by ability into educational attainment. Table 2 provides some evidence that school groups are strongly sorted by test scores. Each row of the table corresponds to one of our four school groups. The four columns give the conditional probability of someone with that school level having a test score in each of the four quartiles of the distribution. The vast majority (86%) of high school dropouts are from the first test score quartile, while 76% of

Table 2: Conditional Distribution of Test Scores Given Schooling in the NLSY79

	Test Score Quartile			
School Attainment	1	2	3	4
<HS	86%	12%	2%	0%
HS	42%	34%	19%	5%
SC	18%	32%	31%	19%
C+	1%	11%	29%	59%

high school graduates have below-median test scores. On the other hand, 88% of college graduates have above-median test scores.

In the case where test scores measure ability exactly, these facts imply that school groups are strongly sorted by ability. It is again straightforward to use this information. We can compute $E(\hat{a}|s)$ from the NLSY79. In this special case, $E(a|s) = E(\hat{a}|s)$. When combined with our estimate that $\theta = 0.104$, we can calculate the role of effective ability gaps in explaining wage premiums, which is given by $\theta\left[E(a|s) - E(a|s')\right] = \beta_{\hat{a}}\left[E(\hat{a}|s) - E(\hat{a}|s')\right]$.

We also calibrate our model to the NLSY79 and use it to produce results on the role of effective ability gaps in explaining wage premiums. At this point the exercise is not strictly necessary because, as highlighted above, estimates of $\beta_{\hat{a}}$ and $E(\hat{a}|s)$ are sufficient for these results. However in doing this exercise we hope to build some intuition for the general calibration procedure, which will be necessary for subsequent exercises. We also want to show that in this case the calibrated model produces results very similar to the simpler calculation, which suggests to us that the calibration passes a basic test of reasonableness.

We now outline our calibration procedure. In this and all subsequent calibrations we treat $\chi(s, 1960)$ as a set of free parameters that we vary so that we fit schooling by cohort exactly. The reason is that our quantitative results are sensitive to getting educational attainment right; small deviations in the model-predicted school attainment can generate important differences in the quantitative predictions. We find it more straightforward to fit the attainment exactly. Given that we do so, we use our model as a measurement device to study the implied importance of effective ability. Then there are three remaining parameters that do not drop out of the model: $\sigma_{\hat{a}}$, θ, and $\sigma_{p,1960}$. We have set $\sigma_{\hat{a}} = 0$. Our calibration method chooses $\theta = 0.104$ so that the return to test scores in the model matches the same statistic in the data. Finally, we choose $\sigma_{p,1960}$ so that the model fits the sorting by test scores into educational attainment as closely as possible. Our model is deliberately parsimonious, and yet this approach is quite successful. Figure 2 compares the sorting in the data and the sorting predicted by the model for the best fit of $\sigma_{p,1960} = 0.87$. The model is able to generate sorting quite comparable to the data. The only significant discrepancy is that the model-generated distribution for those with some college has too many people with above-average test scores and too few with below-average test scores. Otherwise the fit between model and data is quite close, which suggests that the model will generate mean test score gaps comparable to the data. We now verify that this is the case.

Figure 2: Model-Predicted and Actual Distribution of Test Scores Given Schooling

Our cross-sectional results measure the role of effective ability gaps in explaining observed school wage premiums. Table 3 shows our results. Each row contains the results from comparing high school graduates to one of the three remaining school groups. Of these, we are particularly interested in the college–high school comparison, since the college wage premium receives so much attention in the literature. Columns 2 and 3 gives the effective ability gaps that we find via direct calculation as well as those generated by the calibrated model; they are nearly identical. We view this fact as a useful check on the model and a way to show that the model does not generate any unusual predictions. Further, the gaps are economically large. To help make this point, we provide in the final column the actual school wage premiums from the US Census.[10] Differences in mean ability account for roughly one-quarter to one-third of the wage premiums, with slightly smaller results for the college wage premium.

[10]We use the US Census for wages to be consistent with the results of Section 5. There we examine the importance of mean ability for earlier cohorts, for which NLSY79 wage data are not available. Details of the wage measurement are available in the Appendix.

Table 3: Results when Test Scores Measure Ability Exactly

School	Effective Ability Gap		Wage Gap
Comparison	Calculation	Model	Data
<HS–HS	-0.08	-0.08	-0.24
SC–HS	0.06	0.07	0.18
C+–HS	0.14	0.15	0.52

These initial results help address the puzzle of Heckman, Lochner, and Todd (2006) and Heckman, Lochner, and Todd (2008). They find that in standard human capital models, the college wage premium for recent cohorts is difficult to reconcile with less than one-third of the recent cohorts graduating from college, unless one incorporates some substantial uncertainty or a large "psychic cost" of attending schooling. Our results help reduce this puzzle modestly by pointing out that some of the apparently high college wage premium at age 40 is actually attributable to the gap in mean ability between college and high school graduates; the true private return to college is smaller than the observed wage gap.

4.2 Results When Test Scores Measure Ability With Noise

Our initial results can be derived without calibrating the model. However, the model enables us to undertake two additional exercises. The first is to consider the case where test scores measure ability with noise. In this case, the mean test score for different school groups is not the same as the mean ability, so we cannot measure mean ability gaps directly. Instead, we use the calibrated model to quantify the role of ability.

Before discussing the exact calibration procedure, it is useful to see why allowing for noise in test scores is likely to be important. The main reason is that we use the log-wage return to test scores to help identify θ. In the case where test scores measure ability exactly, then in fact $\theta = \beta_{\hat{a}} = 0.104$. However, if test scores measure ability with noise, then our empirical regression suffers from attenuation bias. The standard result that applies in this case is that $\theta > \beta_{\hat{a}} = 0.104$. Hence, a given gap in mean abilities will lead to a larger effective ability gap, which in turn accounts for more of observed wage premiums. Our goal now is to quantify this analysis: how much noise is there likely to be in test scores, and how much more important is effective ability in accounting for wages?

The primary challenge of implementing a model where test scores are noisy is that we do not have direct evidence of how well test scores measure ability. The obvious reason is that ability itself is not measured; if it were, we would not need to use test scores as a proxy for ability. However, we will establish that we can make inferences that enable us to bound usefully the noise in test scores. We begin by demonstrating how to construct a lower bound on the noise in test scores.

To construct a lower bound on the noise in test scores, we draw on the well-known property that repeatedly administering similar or even identical tests to a group yields positively correlated but not identical results. We construct our lower bound on the noise in test scores by requiring that a given test score not be a better predictor of ability than it is of other subsequent test scores. To quantify this statement, recall that we think of test scores as noisy, scaled proxies for ability. In this context it is natural to think of the noise in tests $\varepsilon_{\hat{a}}$ as being an independent, test-specific draw. Then the correlation between two different test scores for a given individual is $(1+\sigma_{\hat{a}}^2)^{-1}$. We have ample evidence on the magnitude of this correlation. Herrnstein and Murray (1994, Appendix 3) document the correlation between AFQT scores and scores from six other standardized tests taken by some NLSY79 individuals. The correlations range from 0.71 to 0.9, with a median score of 0.81.[11] Cawley, Conneely, Heckman, and Vytlacil (1997) show that the correlation between AFQT scores and the first principal component of the ASVAB scores is 0.83.

Putting these correlations together, we use $(1+\sigma_{\hat{a}}^2)^{-1} = 0.8$. In turn this suggests a lower bound $\sigma_{\hat{a}} \geq 0.5$.[12] If test scores were any more precise as measures of ability, then the correlation between scores from different tests should be higher. We use this lower bound by fixing $\sigma_{\hat{a}} = 0.5$ in the model, then calibrating θ and $\sigma_{p,1960}$ to fit the log-wage return to test scores and the school-test score sorting as well as possible. We are able to hit the former moment exactly. We showed earlier that even with only a single parameter $\sigma_{p,1960}$ we are able to replicate the school-test score sorting closely (Figure 2); that continues to be the case here and throughout the remainder of the paper. We do not show the remaining figures to conserve space.

We also want to establish an upper bound on the plausible noise in test scores. The purpose of this bound is not to argue that the true results are at some midpoint of the lower and upper bounds. Instead, we establish an upper bound to show that a bounding argument in this case is effective in the sense that the range of results is fairly narrow. Given this fact, it is innocuous to use the lower bound as our benchmark, which we do.

[11] A slight complication arises from the fact that Herrnstein and Murray compute correlations between percentile ranks rather than raw scores. We conducted simulations to verify that this has only a minor quantitative effect on the resulting correlation.

[12] A similar approach is taken by Bishop (1989) to estimate the measurement error in the PSID's GIA score. Based on the GIA's KR-20 reliability of 0.652, Bishop's result implies $\sigma_{\hat{a}} = 0.73$, which would imply a larger role for ability than what we find here. In fact, we construct an upper bound for $\sigma_{\hat{a}}$ that is lower than this value below, suggesting that Bishop's results may have counterfactual implications for wages.

Table 4: Cross-Sectional Results when Test Scores Measure Ability With Noise

	Model: Effective Ability Gap			Wage Gap
	$\hat{a} = a$	LB	UB	
$\sigma_{\hat{a}}$	0.00	0.50	0.69	
θ	0.104	0.155	0.228	
<HS–HS	-0.08	-0.14	-0.22	-0.24
SC–HS	0.07	0.11	0.18	0.18
C+–HS	0.15	0.25	0.39	0.52

Further, we will show that the results for the lower bound are already large relative to the wage patterns in the data, which reinforces our decision.

To derive an upper bound, we impose plausible limits on the size of the effects that we find. More noise in test scores implies a larger attenuation bias in the regression of wages on test scores, a larger value for θ, and larger effective ability gaps between school groups. At some point the implied effective ability gaps become implausibly large. One natural benchmark is that an effective ability gap should not be bigger than the corresponding wage premium. If it were, this would imply a negative private return to going to school longer, which would seem inconsistent with simple optimization on the part of the students who achieve that attainment in the data.

Implementing the upper bound requires us to iteratively calibrate the model. We guess a particular value of $\sigma_{\hat{a}}$. We then calibrate the θ and $\sigma_{p,1960}$ to fit the log-wage return to test scores and the school-test score sorting as well as possible. Finally we compute the model's predicted effective ability gaps $\theta(E[a|s] - E[a|s-1])$ and compare them to the corresponding wage premiums in the data. If all effective ability gaps are smaller than the corresponding wage premiums then we guess a larger value for $\sigma_{\hat{a}}$ and restart the process; if any effective ability gap is larger than the corresponding wage premium then we guess a smaller value for $\sigma_{\hat{a}}$ and restart the process. We repeat until we find the $\sigma_{\hat{a}}$ so that one effective ability gap is exactly equal to the corresponding wage premium and all other effective ability gaps are smaller than their corresponding wage premiums.

Table 4 summarizes the results of our bounding exercises. Rows 2 and 3 give the value of the calibrated parameters while rows 4–6 give the results from different school comparisons. Column 2 repeats the results for the case where test scores measure ability exactly, for reference. Column 3 gives the results for the lower bound. We find this lower bound by fixing $\sigma_{\hat{a}} = 0.5$; in this case a modest increase in θ is required for the model to generate a return to test scores of 0.104 as seen in the data. This larger value of θ in turn yields larger effective ability gaps, 57–75% higher than in the case where test scores measure ability exactly. An alternative way to judge the size of effective ability gaps is by comparing them to observed wage premiums, given in column 5. Effective ability gaps account for at least 48% of observed wage premiums, and more than half of the wage premium for high school dropouts and those with some college. These large results go further towards reducing the puzzle that it is hard to reconcile the high college wage premium with a low college completion rate in a human capital model (Heckman, Lochner, and Todd 2006, Heckman, Lochner, and Todd 2008).

Finally, column 4 includes the results at the upper bound. We find that this upper bound binds for the SC–HS comparison at a value of $\sigma_{\hat{a}} = 0.69$. Comparing column 3 to column 4 shows that the lower bound and the upper bound are already fairly similar in terms of the parameterizations and the results. In particular, the results for the upper bound are less than twice those for the lower bound. In the next section we will tighten the upper bound even further so that the difference between the lower and upper bounds is even smaller. We now turn to the time series calibration.

5 Calibration to the Time Series

The first contribution of the model is that it enables us to generate results for the case where test scores measure ability with noise. The second contribution of the model is that it enables us to generate results for the time series. While the NLSY79 provides us with excellent data on schooling, wages, and test scores for the 1960 cohort, no comparable data set exists for earlier cohorts. At the same time, the dramatic expansion of education and the growing test score gap between those who enroll in college and those who do not lead us to believe that composition effects may play a large role in the wage patterns of the twentieth century. In this section we calibrate the model to see if this is the case and, if so, to quantify the magnitude of the effects.

5.1 Calibration

Our time series calibration follows the same basic outline as the cross-sectional calibration. What this means is that we calibrate both a lower bound and upper bound for $\sigma_{\hat{a}}$, and provide the parameters and the results for each case. We now discuss these calibrations in more detail.

Our lower bound is still $\sigma_{\hat{a}} = 0.5$. Given this moment, we use the remaining parameters to fit the model to the data. In particular, we still choose $\chi(s,\tau)$ as a free parameter to fit the expansion of schooling shown in figure 1a. Likewise, we use $\sigma_{p,\tau}$ to fit the estimated quadratic trend in the degree of sorting by test score into educational attainment shown in Figure 1b. The data show that students are becoming more strongly sorted over time. Our model can replicate this observation if the dispersion of tastes is declining over time, so that ability plays a larger role in school choices for later cohorts.[13] Finally, we calibrate θ so that the model-predicted return to schooling matches that of the data, $\beta_{\hat{a}} = 0.104$.

We use the same basic iterative procedure as before to find the upper bound. We guess a particular value of $\sigma_{\hat{a}}$. We then calibrate the remaining parameters to fit the model to the data. Given the full set of parameters, we look at the model's predictions for effective ability gaps. If all effective ability gaps are smaller than their corresponding wage gaps, we start again with a larger $\sigma_{\hat{a}}$; if any effective ability gap is larger than its corresponding wage gap, we start again with a smaller $\sigma_{\hat{a}}$. We expect this upper bound to be closer to the lower bound than in the previous section. The reason is that in the previous section we checked this bound only for the 1960 cohort, whereas now we check it for the 1910–1960 cohorts, which gives more wage premiums that may potentially bind the size of our effective ability gaps.

5.2 Results for Lower Bound

We begin by presenting the results for the lower bound in detail; we take these results to be our benchmark findings and show the comparison to the upper bound in the next section. Table 5 shows the full set of calibrated parameters for the lower bound. The value for θ is the same as in the cross-sectional calibration. The main new point to note is that the calibrated dispersion of tastes declined substantially between the 1910 and 1960 cohorts, indicating that ability played a much greater role in determining who continued to college for the 1960 cohort. Workers in this model sort perfectly by $p + a$ and the variance of a is set at 1 throughout. Then for the 1910 cohort variance in ability accounted for just 32% of the variance in $p + a$, while for the 1960 cohort it accounted for 72%.

These changes in sorting, along with the expansion of education, imply large changes in the mean ability of the four school groups. Figure 3 shows the model-implied evolution of the distribution of ability conditional on schooling. Figure 3a illustrates the degree of sorting found for the 1960 cohort in the NLSY79. There are clear differences in the mean of the ability distribution between each of the four school groups, and almost no overlap

[13] An alternative interpretation is that students are imprecisely informed about their own ability, but that they are becoming more precisely informed over time; see section 2.6 for further discussion.

Table 5: Calibrated Parameters for the Lower Bound

Parameter	Role	Value
$\sigma_{\hat{a}}$	Noise in Test Scores	0.50
θ	Effect of Ability on Wages	0.155
$\sigma_{p,1960}$	Dispersion of Preferences	0.62
$\sigma_{p,1950}$	Dispersion of Preferences	0.80
$\sigma_{p,1940}$	Dispersion of Preferences	1.12
$\sigma_{p,1930}$	Dispersion of Preferences	1.10
$\sigma_{p,1920}$	Dispersion of Preferences	1.28
$\sigma_{p,1910}$	Dispersion of Preferences	1.44

between the distributions for high school dropouts and college graduates.

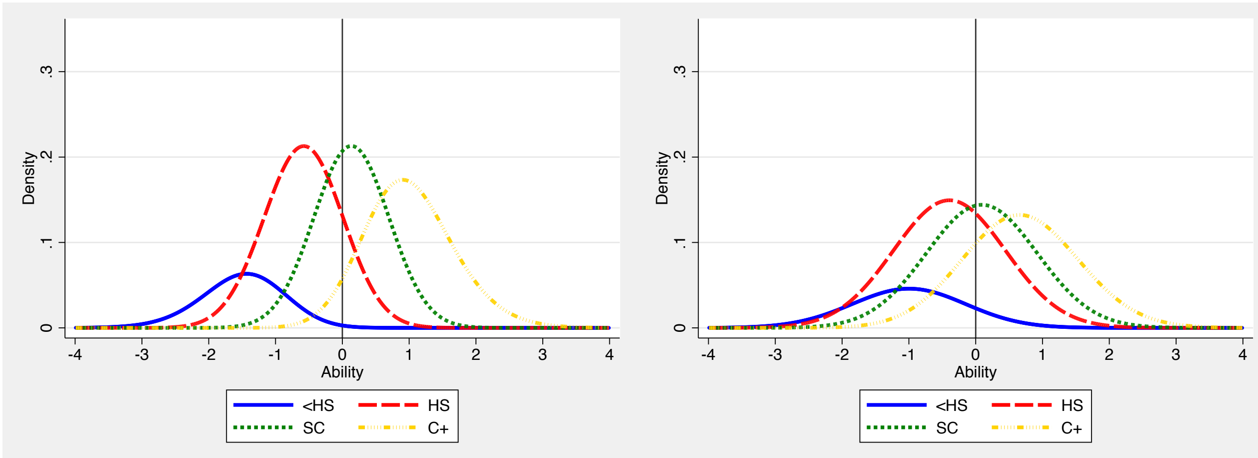
Figure 3b illustrates a particular counterfactual: it shows the distribution of ability conditional on schooling that would have applied if we held the 1960 cohort's attainment fixed, but gave them the dispersion of tastes that we calibrated for the 1910 cohort. By comparing figures 3a and 3b we can see the effect of the increase in sorting isolated from the effect of the expansion of education. The distributions in figure 3b have very small mean differences, particularly for those who at least graduate high school. Further, the distributions overlap substantially.

Finally, figure 3c illustrates the model-implied distributions for the 1910 cohort. By comparing figures 3b and 3c we can see the effect of the expansion of education. The mean of each distribution is shifted left in figure 3b by the rise in schooling. To see why this happens, consider the distribution for high school graduates. Over time, attainment rises. In the model, this happens because high school graduates with relatively high levels of $p+a$ in later cohorts start attempting college. At the same time, some people with relatively low $p + a$ in later cohorts will complete high school instead of dropping out. Both of these effects act to reduce the average ability of high school graduates.

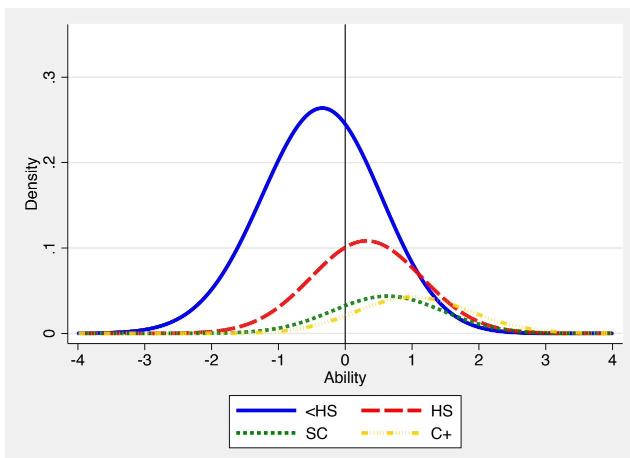
Comparison of figures 3a and 3c shows the combined effect of the expansion of education and the change in sorting. The leftward shift in ability for high school dropouts is particularly pronounced because both effects move in the same direction, toward a decline in ability. On the other hand there is hardly any change in the peak of the distribution for college graduates, as the expansion of college is in large part offset by the change in sorting. Intuitively, it is possible to expand college enrollment without lowering the mean ability of college graduates if stronger sorting by test scores makes it possible to identify high-ability students who in earlier cohorts did not attend college. This point will be central to our

Figure 3: The Distribution of Ability Conditional on Schooling

(a) 1960 Cohort (b) Counterfactual: 1960 Cohort with 1910 Sorting



(c) 1910 Cohort



subsequent results.

We have two main sets of time series results. First, we examine how changes in effective ability between cohorts have affected wage growth. These results are presented in table 6. Our measured time series for wages and our model-implied time series for mean ability by school group are smooth, so we focus only on the total change between the first and last cohorts. The second column gives the model-predicted change in effective ability between the 1910 and 1960 cohorts for each of the four school groups. In the fourth column we give the measured wage growth conditional on schooling, taken from census data. Given the observed wage growth and the model-implied change in mean ability, we back out the implied growth in $h + z$ in the third column. This column measures the growth in skill

Table 6: Changes in Mean Ability and Wages, 1910–1960 Cohorts

	Model-Predicted Change		Data Change
	Effective Ability	$h+z$	Wage
<HS	-0.17	0.40	0.22
HS	-0.14	0.42	0.29
SC	-0.08	0.38	0.30
C+	0.00	0.44	0.43

Table 7: Changes in Mean Ability Gaps and Wage Premiums, 1910–1960 Cohorts

	Model-Predicted Change		Data Change
	Effective Ability Gap	$h+z$ Gap	Wage Premium
<HS–HS	-0.03	-0.03	-0.06
SC–HS	0.06	-0.04	0.02
C+–HS	0.14	0.01	0.15

prices and human capital, which is also the wage growth that would have been observed if mean ability had remained constant. Changing ability had the largest effect for high school dropouts: the 17 percentage point decline in effective ability caused observed log wage growth to be roughly one-half of the growth in $h + z$. The effect for high school graduates was smaller and for those with some college smaller still. For college graduates we find effective ability remained constant. Hence our model can generate a wage slowdown that affects the less educated groups more.

For our second set of time series results we examine how changes in effective ability gaps have affected wage premiums between the 1910 and 1960 cohorts. These results are presented in table 7. The second column gives the model-predicted change in effective ability gaps as compared to high school graduates for each of the three remaining school groups. Given the observed wage premium growth in the fourth column, we again back out the implied growth in $h+z$ gaps in the third column. Changing ability had the largest effect for college graduates, relative to high school graduates. In fact, we find that almost the entire rise in the college wage premium can be attributed to the fact that effective ability for college graduates remained roughly constant while for high school graduates it declined between the 1910 and 1960 cohorts. Likewise, we find that half the change in the high school dropout-high school graduate premium can be attributed to changes in mean ability for the two groups.

To summarize, our results suggest that changes in ability have slowed observed wage

Table 8: Results for Lower and Upper Bounds of Test Score Noise

	Model: Effective Ability			Data: Wages
	$\hat{a} = a$	LB	UB	
$\sigma_{\hat{a}}$	0.00	0.50	0.66	
θ	0.104	0.155	0.216	
C+–HS	0.15	0.25	0.37	0.52
ΔHS	-0.08	-0.14	-0.21	0.29
ΔC+–HS	0.08	0.14	0.20	0.15

growth for most school groups as mean ability has declined. Further, our most important result is that the entire rise in the college wage premium can be explained by changes in the relative ability of college and high school graduates. Our results use a different methodology but arrive at a similar conclusion as Bowlus and Robinson (2012), who find that 72% of the rise in the college wage premium between the years 1980 and 1995 can be attributed to changes in the quantity of labor services provided by college relative to high school graduates. We conclude this section by noting that these results are actually the lower bound of what is plausible, which we take as our benchmark. We now turn to showing the entire range of possible results.

5.3 Range of Results

We use the iterative procedure outlined in the previous section to find the upper bound on $\sigma_{\hat{a}}$, calibrate the remaining parameters, and derive the model predictions. Rather than present all possible results, we focus on a few select results, presented in table 8. These results are for the college wage premium for the 1960 cohort; the growth in wages for high school graduates between the 1910 and 1960 cohorts; and the change in the college wage premium between the 1910 and 1960 cohorts, presented in rows 5–7. The fifth column shows the data on wages, while columns 2–4 show the model-implied role for effective ability given different values of $\sigma_{\hat{a}}$. The main message from these rows is that the upper bound is roughly 50 percent larger than the lower bound in terms of θ and in terms of each of the three key wage statistics. This means that it accounts for roughly 50 percent more of the college wage premium, the slowdown in observed high school wage growth, and the rise in the college wage premium. The upper bound is overall quite close to the lower bound, suggesting that our bounding argument restricts the range of potential results successfully. We now decompose the driving forces that explain our results.

Table 9: Decomposition: Role of Changes in Sorting

	Model: Effective Ability				Data: Wages
	Baseline		Constant Sorting		
	LB	UB	LB	UB	
$\sigma_{\hat{a}}$	0.50	0.66	0.50	0.54	
θ	0.155	0.216	0.155	0.165	
C+–HS	0.25	0.37	0.25	0.27	0.52
ΔHS	-0.14	-0.21	-0.16	-0.18	0.29
ΔC+–HS	0.14	0.20	0.08	0.09	0.15

5.4 Decomposing the Role of Changes in Sorting and the Expansion of Education

Our next experiment seeks to decompose the relative role of the increase in sorting and the expansion of education in driving our results. To do so, we fix $\sigma_{p,\tau} = \sigma_{p,1960}$ for all cohorts so that sorting is held fixed. Other than holding $\sigma_{p,\tau}$ fixed, the details of the calibration are as in the baseline experiment. We continue to calibrate θ and $\sigma_{p,1960}$ to the $\beta_{\hat{a}}$ and test score–school sorting from the NLSY79. We again provide results for both the lower and the upper bound. The lower bound is still given by $\sigma_{\hat{a}} = 0.50$, but we have to recalibrate the upper bound since constant sorting changes the effective ability gaps for earlier cohorts.

The results are presented in table 9 in the same format as table 8. We present again the results for the baseline model as well as those for the model for constant sorting. We note two key findings. First, the model with constant sorting generates smaller time series results. The quantitative reduction is modest for the change in wage levels and stronger for the change in the college wage premium; for the latter, our results are roughly one-half of those in the baseline model. This finding indicates that half of the model's predictions for the time series of the college wage premium stems from changes in sorting and half from the expansion of education; each is important.

The second main finding of this table is that the upper bound collapses to lie almost exactly at the lower bound for the case with constant sorting. This happens because the constant sorting experiment assumes more sorting and larger effective ability gaps in earlier cohorts than does the baseline experiment. Because of this the model with constant sorting hits its upper bound for much smaller values of $\sigma_{\hat{a}}$. The range of plausible results in this case is extremely narrow.

Table 10: Robustness: Lower Log-Wage Returns to Test Scores

	Model: Effective Ability				Data: Wages
	Baseline		$\beta_{\hat{a}} = 0.07$		
	LB	UB	LB	UB	
$\sigma_{\hat{a}}$	0.50	0.66	0.50	0.78	
θ	0.155	0.216	0.104	0.205	
C+–HS	0.25	0.37	0.17	0.37	0.52
ΔHS	-0.14	-0.21	-0.09	-0.21	0.29
ΔC+–HS	0.14	0.20	0.09	0.20	0.15

6 Robustness

In the previous section we established our three key results. At the lower bound of the range, our model accounts for about half of the college wage premium for the 1960 cohort as well as the entire rise of the college wage premium between the 1910 and 1960 cohorts. It also predicts a slowdown in wages conditional on schooling that has a stronger effect on less educated groups. We now perform robustness analysis. We focus on two experiments. First, we provide results for the case where $\beta_{\hat{a}} < 0.104$. This is the key moment for our calibration so we find it worthwhile to consider smaller values. Second, we explore the results from an alternative model where schooling and cognitive ability have a complementary effect on wages. We also briefly consider the possible importance of changes in the ability distribution over time.

6.1 Lower Log-Wage Return to Test Scores

The key moment for our calibration is the log-wage return to test scores in the NLSY79, which we measure as $\beta_{\hat{a}} = 0.104$. Our estimate is similar to other estimates in the literature that use the NLSY79 (see for example Mulligan (1999) table 6, or Altonji and Pierret (2001) table I). However, estimates based on other data sources differ. Bowles, Gintis, and Osborne (2001) collect 24 studies using different data sources. The mean return across studies was 7%, with substantial dispersion. In this section we examine the robustness of our results to using $\beta_{\hat{a}} = 0.07$ as an input to our calibration.

Our calibration strategy is the same as the baseline case, except that for each possible $\sigma_{\hat{a}}$ we calibrate θ to replicate $\beta_{\hat{a}} = 0.07$. Table 10 gives the results in the same format as the previous two tables. The main finding is that the results for the lower bound are about one-third smaller than in the baseline case, while the results for the upper bound are the same. Note, however, that even for the lower bound of the robustness check $\beta_{\hat{a}} = 0.07$, we still account for nearly two-thirds of the rise in the college wage premium and nearly one-third of the current college wage premium.

Our results do not change at the upper bound because of our bounding methodology. We choose the upper bound so that ability gaps are as large as plausible. To do so, the model chooses a calibrated value of θ similar to the upper bound in the baseline. It rationalizes the low measured return to IQ as being the result of higher levels of calibrated noise in test scores and a larger degree of attenuation bias in the regression. Hence, the essentially unchanged results at the upper bound are driven by our requirement that the upper bound be defined as the point where ability gaps are as large as plausible.

6.2 Ability-School Complementarity in Wages

Empirically, high test score students tend to go to school longer (see for example figure 2). In our baseline model we use complementarity between cognitive ability and school in the utility function to match this fact. A common alternative in the literature is instead to use complementarity that comes through wages. In this case, more able workers go to school longer because their wage payoff to doing so is higher, not because they find it less distasteful. We show in this subsection that the exact form of the complementarity is not important for our key results.

To introduce ability-school complementarity we change the period log-wage function to:

$$\log[w(s, q, v)] = \theta_s a + z(s, \tau + v - 1) + h(s, v).$$

Complementarity requires that θ_s be weakly increasing in s. We find it useful to focus on the alternative interpretation of the model discussed in Section 3.6. In this interpretation a is still a worker's cognitive ability but p represents noise in the worker's signal about that ability and $p + a$ is the signal. This interpretation offers the convenient feature that workers sort perfectly by $p + a$, as in the baseline model. Hence, the basic mechanics of this wage complementarity model will be the same as the mechanics of the baseline model, which allows us to focus on whether there are any important quantitative differences in their implications.

To explore wage complementarity we split the NLSY sample in two groups: we pool <HS and HS, and then SC and C+. We estimate the log-wage return to test scores separately for each, finding $\gamma_{HS} = 0.076$ and $\gamma_C = 0.120$, as opposed to $\gamma = 0.104$ when the data are pooled. We then recalibrate the model as above, but require the model to match the

Table 11: Robustness: Ability-School Complementarity in Wages

	Model: Effective Ability				Data: Wages
	Baseline		Complementarity in Wages		
	LB	UB	LB	UB	
$\sigma_{\hat{a}}$	0.50	0.66	0.50	0.54	
θ_{HS}	0.155	0.216	0.117	0.184	
θ_C	0.155	0.216	0.178	0.266	
C+–HS	0.25	0.37	0.25	0.40	0.52
∆HS	-0.14	-0.21	-0.11	-0.18	0.29
∆C+–HS	0.14	0.20	0.10	0.17	0.15

separate estimated return to test scores for each of the two groups.

Table 11 shows our results in the same format as for the previous robustness check. Overall, a model with complementarity that comes through wages produces results quite similar to the baseline model, but slightly smaller in the time series. The main reason for the smaller results can be understood by comparing the importance of cognitive ability for wages across school types and models, $\theta_{HS} < \theta < \theta_C$, with θ referring to the shared importance of ability for wages in the baseline model. The alternative model implies that cognitive ability is more important for wages for college students but less important for high school students. This in turn leads to smaller results since across all of our calibrations the model finds a much larger decline in mean cognitive ability for high school dropouts and high school graduates than for those who attend college; see for example table 6. In fact, many of our calibrations imply almost no change in the mean ability of college students between the 1910 and 1960 cohorts. Hence, the main impact of allowing for complementarity through wages is that it amplifies the relatively small changes in mean ability for students who attend college and compresses the relatively large changes in mean ability for students who do not, with a net result of modestly lower results. Still, we emphasize that even our lower bound would imply that changes in mean ability between high school and college graduates account for two-thirds of the rise in the college wage premium. We conclude that the exact way in which we introduce school-cognitive ability complementarity is not central to our conclusions.

6.3 The Flynn Effect

Our results so far have assumed that the distribution of abilities is time-invariant. There is, however, substantial evidence of a sustained rise in test scores throughout our time period, a phenomenon known as the Flynn effect (Flynn 1984, Flynn 2009). There is disagreement in the psychometric literature as to whether the Flynn effect represents real gains in skills, improvements in test-taking skills, or some other possibility (see Flynn (2009)). Here we explore the implications for our measurements if the Flynn Effect captures actual rises in ability.

Although it is still somewhat controversial, the evidence now seems to suggest that the rise in ability is a mean shift that affects all parts of the distribution more or less equally. In this case, our approach is simple. Flynn (2009) documents that average test scores on the WISC, a broad-based IQ exam, rose 1.2 standard deviations between 1947 and 2002, which corresponds roughly to our cohorts. He also conjectures (based on incomplete evidence) that test scores on the Raven's Progressive Matrix Exam, a test of spatial recognition, rose 1.83 standard deviations over the same years. We measure the implications in our model if these two changes represent real gains in ability.

The Flynn effect has modest implications for our work. An increase in the entire distribution of ability changes the mean ability of workers $E(a|s)$ by a constant amount, but does not affect the mean ability gaps $E(a|s) - E(a|s')$. In particular, a rise in ability of 1.2–1.83 standard deviations implies a rise in effective ability $\theta E(a|s)$ by 19–28 percentage points, if we use the benchmark $\theta = 0.155$. Any of these results implies that the mean ability of all school groups actually rose between the 1910 and 1960 cohorts, so that our model does not help explain the wage slowdown. Otherwise, the Flynn Effect has no important implications for our results about wage premiums because it affected different school groups equally.

7 Conclusion

Between the 1910 and 1960 cohorts the college wage premium widened substantially. Today the college wage premium is sufficiently large that it may be difficult to reconcile with a model of individual human capital investment. Most papers have tried to understand these movements as the result of changes in skill prices, roughly the wage per unit of labor. We break with this literature by asking instead whether changes in the units of labor per worker may be responsible. Large changes in the school attainment of workers and the degree of sorting by test scores into educational attainment suggest that the mean ability of workers with different education levels may have changed. The main purpose of this paper is to quantify these compositional effects and their impact on wages.

Our results suggest that much of the most important wage questions can be attributed to changes in the mean ability of students by school attainment. Our benchmark results can explain all of the rise in the college wage premium as well as half of the currently high college wage premium. Additionally, our model can help explain some of the wage slowdown as the result of declining mean ability conditional on schooling. Our robustness checks indicate that roughly half of our time series results come from the expansion of education and half from the increase in sorting. Our results would still be economically significant even if test scores measured ability exactly or if we used more conservative moments for our calibration.

We have relied on a simplified model with one dimension of ability and one generic friction to sorting, the tastes of workers. Future work could make progress by developing a more detailed model of ability or the frictions that act to prevent stronger sorting by ability, and by finding more historical data on these forces for empirical use.

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Table 12: Summary statistics: Census data

	Census Year					
	1950	1960	1970	1980	1990	2000
Number of Observations	17,503	74,744	72,873	379,087	539,145	562,262
Fraction <HS	59%	45%	35%	20%	10%	10%
Fraction HS	23%	32%	35%	39%	25%	31%
Fraction SC	9%	11%	12%	17%	32%	30%
Fraction C+	9%	12%	18%	25%	33%	29%
$w_{<HS}$	9.6	13.0	15.3	12.4	12.7	12.0
w_{HS}	11.4	15.7	18.3	16.4	16.7	15.2
w_{SC}	13.4	18.0	21.1	18.6	19.3	18.1
w_{C+}	16.5	22.7	28.5	25.1	26.5	25.4
College wage premium	0.37	0.37	0.44	0.42	0.47	0.52

A Appendix

A.1 Census Data

Samples. The census is taken every ten years; we have data from many birth cohorts spanning multiple censuses. We focus our attention on the cohorts that are exactly 40 years of age in the 1950–2000 censuses, or the cohorts born every ten years from 1910–1960. Focusing on one age eliminates any problems associated with comparability of educational data and wages at different ages. We use the public-use micro data files available from Ruggles and Sobeck (2007). We use 1% samples for 1950–1970 and 5% samples thereafter. In 1950, only sample line individuals report wages and hours worked. This reduces the effective sample size to only one quarter of the 1960 sample. Table 12 shows descriptive statistics for each census year.

Educational attainment. Our measure of educational attainment is derived from the variables EDUCREC and HIGRADE (both detailed). For the 1990 and 2000 censuses we use the variable EDUCREC, which records the information on degrees obtained. We include those with GEDs among high school graduates and those with 2 year degrees among those with some college. For earlier censuses we have only the variable HIGRADE, which records the number of years of education a person has obtained. We classify persons into four school groups as follows: we call those with fewer than 12 years of schooling high school dropouts; those with exactly 12 years of schooling high school graduates; those with 13–15 years of schooling have some college; and those with at least 16 years of schooling, college graduates.

Cohorts that respond to both the HIGRADE and EDUCREC questions (in two different censuses) typically have different measured attainment for the two questions, even if they are old enough that significant changes in actual school attainment are unlikely. Goldin and Katz (2008) use Current Population Survey data to produce a more detailed concordance between EDUCREC and HIGRADE questions that they use between 1980 and 1990. We use the raw responses as discussed above, for two reasons. First, the concordance is likely to vary by year as the structure of education changes, especially within each of our four discrete categories. Second, our focus here is on the large-scale movements, such as the near-universality of high school graduation and the increase in college attendance. Since most of those identified as dropouts in the 1910 census report less than 11 years of schooling, we are confident that they did not achieve a high school degree, let alone start college. By contrast the 1960 cohort answered directly about degree completion, raising our confidence in these estimates. We therefore believe that these major trends are real and are not likely the artifact of changing data collection.

Wages. We calculate hourly wages as the ratio of wage and salary income (INCWAGE) to annual hours worked. Annual work hours are the product of weeks per year times hours per week. For consistency, we use intervalled weeks and hours for all years. Where available we use usual hours per week. Wages are computed only for persons who report working "for wages" (CLASSWKR) and who work between 520 and 5110 hours per year. All dollar figures are converted into year 2000 prices using the Bureau of Labor Statistics' consumer price index (CPI) for all wage earners (all items, U.S. city average).

A.2 NLSY79 Data

Sample. The sample includes white males. We drop individuals with insufficient information to determine their schooling. We also drop individuals who completed schooling past the age of 34 or who did not participate in the ASVAB aptitude tests. Observations are weighted.

Schooling. We divide persons into four school groups (less than high school, high school, some college, and college or more) according to the highest degree attained. Persons who attended 2-year colleges only are assigned the "some college" category. The last year in school is defined as the start of the first three year spell without school enrollment.

Wages. We calculate hourly wages as the ratio of labor income to annual hours worked. Labor income includes wages, salaries, bonuses, and two-thirds of business income. We delete wage observations prior to the last year of school enrollment or with hours worked outside the range $[520, 5110]$. We also delete wage observations outside the range $[0.02, 100]$ times the median wage. Wages are deflated by the CPI.

We remove from the wage data variation that is due to demographic characteristics not captured by our model. This is done by regressing log wages on schooling, experience, and region of residence. Separate regressions are estimated for each year and schooling group. The adjusted wage removes the effects of years of schooling (within school groups) and region.

For consistency with the Census data, we focus on wages earned at age 40. Since not all persons are interviewed at age 40, we interpolate these wages using data for ages 39 to 41.

AFQT. The measure of standardized test score in the NLSY79 is the 1980 Armed Forces Qualification Test (AFQT) percentile rank (variable R1682). The AFQT aggregates various ASVAB aptitude test scores into a scalar measure. The tests cover numerical operations, word knowledge, paragraph comprehension, and arithmetic reasoning (see (NLS User Services 1992) for details). We remove age effects by regressing AFQT scores on the age at which the test was administered.