The economic value of education by race and ethnicity

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Introduction and summary

From 1980 to 2004, real average hourly wages grew from $14.47 to $17.51, an increase of 21 percent.[1] However, this increase in average hourly wages over 25 years masks important differences in wage growth for low- and high-wage workers, as well as differences in growth rates over time. Notably, many people became concerned about rising wage inequality over the 1980s; their concerns were driven in part by declines in real wages at the bottom of the wage distribution.[2] Between 1980 and 1990, wages at the 10th and 50th percentiles fell by 7.3 and 1.7 percentage points, respectively, compared with real wage growth of 6.4 percentage points at the 90th percentile.[3] This increase in inequality and falling real wages in the bottom half of the distribution led many politicians and policymakers to consider several policies aimed at improving earnings among low-wage workers, including increasing the minimum wage and expanding the earned income tax credit. In addition, because higher wages are associated with more years of schooling, many argued in favor of education and training programs to boost wages of the lowest-skilled workers.

One of the best documented relationships in the United States and around the world is that more years of schooling is associated with higher average income and wages. In 2004, high school graduates earned an average of $14.31 per hour compared with $11.12 per hour for high school dropouts—an advantage of nearly 30 percent.[4] As shown in figure 1, because more education is associated with more hours worked per year, the annual earnings advantages of more education are even larger and have increased over the past 25 years. High school graduates outearn high school dropouts on an annual basis by 75 percent in 2003, up from 47 percent in 1979. Similarly, individuals with at least a bachelor's degree earn, on average, 2.3 times the annual earnings of an individual with only a high school diploma or equivalency degree, compared with an earnings advantage of 77 percent in 1979.

Economists measure the economic value of additional schooling ("the return to schooling") as the average percentage increase in mean earnings for an additional year of schooling. Current estimates based on *Current Population Survey* (CPS) data suggest that for each additional year of completed schooling, an individual's earnings increase, on average, by roughly 11 percent.[5] Indeed, the widely held understanding that more education leads to higher wages has compelled many researchers and policymakers to argue in favor of education and training policies to bolster the wages of the lowest-skilled workers and to reduce income inequality (Carneiro and Heckman, 2003; Krueger, 2003).

Much less is known, however, about how the estimated returns to schooling vary across the population. Just as wages may fall at the bottom of the wage distribution while rising at the top of the distribution, estimated returns to schooling may differ across subgroups of the population. Some researchers find that the return to education is higher for more able individuals

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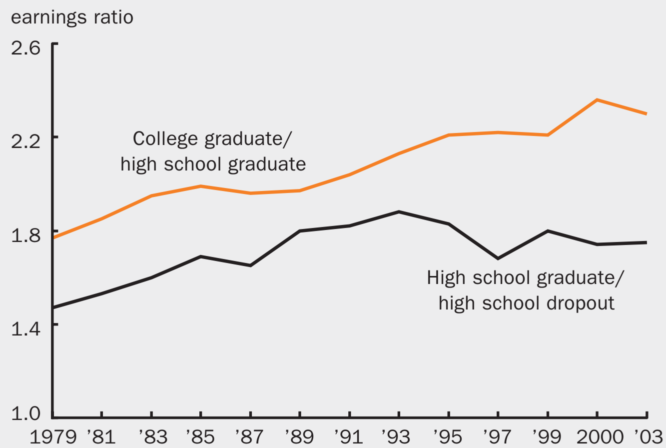
FIGURE 1

Relative annual earnings, by education group



Note: We limit the sample to individuals 25 to 65 years of age who worked at least one week in the previous year.
Source: Authors' calculations based on data from the March *Current Population Surveys* available from Unicon Research Corporation.

(for example, Carneiro, Heckman, and Vytlacil, 2003; Taber, 2001). Others find no consistent evidence that the returns to schooling are higher for individuals that come from more advantaged families (for example, Altonji and Dunn, 1996; Ashenfelter and Rouse, 2000), and in fact, they provide some evidence that the return may be *higher* for more disadvantaged individuals (Ashenfelter and Rouse, 2000). And yet, much social policy hinges on what we believe to be the value of education for individuals. In particular, policies aimed at increasing incomes of the lowest-skilled members of society by increasing their education will not either improve their economic well-being or decrease inequality if their returns to schooling are low.

In this article, we provide further evidence on the variation in returns to schooling in the population by examining whether the benefits vary by race and ethnicity of the individual. We do so by estimating the return to schooling using the U.S. Decennial Census, as well as the *National Longitudinal Surveys of Young Men and Young Women* and the *National Longitudinal Survey of Youth 1979* (NLSY79). We find that the return to schooling is relatively constant across racial and ethnic groups, even controlling for ability and measurement error biases. As a result, policies that increase education among the low-skilled, who are disproportionately African American and Hispanic, have a good possibility of increasing economic well-being and reducing inequality.

Empirical framework

Basic specification

In order to estimate the relationship between schooling and income, we follow Mincer (1974) by regressing the (natural) logarithm of the hourly wage ($\ln w_{ij}$) of individual i of race or ethnicity j on years of completed schooling (S_{ij}), controlling for explanatory variables such as potential experience or age,[6] sex, race, and geographic region of the country (X_{ij}). As such we estimate:

$$1)\qquad \ln w_{ij} = \alpha_j + \beta_j S_{ij} + X_{ij}\gamma_j + \varepsilon_{ij},$$

for which ε_{ij} is an error term. The coefficient on the schooling variable (β_j) is interpreted as the percentage increase in the hourly wage associated with one additional year of schooling and is referred to as the return to schooling.

Related to many of the econometric issues raised in the next section is the question of why we may or may not expect to find differences in the estimated return to schooling by race or ethnicity; that is, why β_j may vary by j. If we begin by assuming that equation 1 represents the true relationship between wages and schooling and that differences in educational attainment occur randomly, then a constant β_j ($\beta_j = \beta$) implies that we should estimate the same return to schooling for any subgroup of the population. However, even if β_j is constant, we may observe different estimates of the return to schooling for different subgroups if the number of years of completed schooling is a poor proxy for human capital due to differences in school quality and if average school quality varies systematically by race.

Alternatively, β_j may not be constant. For example, the return to schooling may depend on the level of education. In this case, estimating returns to schooling for subgroups of the population with different levels of education (on average) will generate different estimates of the return to schooling. Further, it is important to keep in mind that differences in educational attainment do not occur randomly in the population but instead arise from individuals' decisions. A simple model of optimal schooling investment as in Becker (1967) and Card (2001) predicts that differences in optimal schooling choice arise from differences in the benefits and/or costs of obtaining additional schooling. As a result, individual differences in costs or expected benefits that vary systematically by subgroup may generate differing returns to schooling estimates for different subgroups. For example, assuming that education does not affect mortality rates, differences in mortality rates by race mean that for any given level of education, African Americans have fewer expected years than whites over which to receive the benefits of an additional year of schooling. If the costs of an additional year of education are the same for all individuals, then an African American who decides to invest in an additional year of education must expect to receive a larger increase in annual income than an otherwise similar white individual making the same decision. Alternatively, the costs may not be the same for all individuals. If some individuals are liquidity constrained, meaning they face higher costs of financing an additional year of education, a liquidity constrained individual will invest in an additional year of education only if she expects to receive a larger increase in annual income than an otherwise similar individual who does not face liquidity constraints. In this case, estimating the returns to school for subgroups of the population that are more likely to be liquidity constrained will also generate higher estimates of the return to schooling. Additionally, as highlighted by Rivkin (1995), differences in the geographic distribution of people by race or ethnicity, combined with differences in returns to schooling across local labor markets, may also generate differences in the returns to investment in schooling by race.[7] For these reasons, it is an empirical question whether the return to schooling is constant across the population.

Econometric issues

Much of the literature on the economic value of education has focused on examining the causal link between education and income, with a much smaller subset examining the extent to which it varies by family background. As mentioned previously, it is conventional in the economics literature to measure the economic value of additional schooling (or the return to schooling) as the average percentage difference in mean earnings for each additional year of schooling. As Mincer (1974) shows, if foregone earnings are the only cost of school attendance, this is the private marginal benefit (or "return") to the investment in a year of schooling.[8] While the economic value of schooling has been well documented, the reason for its existence is more controversial. Specifically, in his seminal work on education, Becker (1967) argues that education provides skills, or human capital, that raise an individual's productivity. If so, then because productivity is reflected in income, more education causes higher wages. It follows then that much of the gap between the rich and the poor arises from a lack of skills among the poor and that education and training should form the cornerstone of policies aimed at reducing income inequality.

Others, such as Spence (1973), argue that education may not *generate* higher incomes; that is, the relationship may not be causal. Instead, it is possible that education and income are positively correlated because individuals with greater "ability" complete more schooling and would likely earn higher wages and salaries, even if they had not received the additional schooling. In this case, the schooling–income connection may mostly reflect the fact that people with greater ability command a premium for their (innate) skills in the labor market. The result is that empirical estimates of the return to schooling are too large. In this view, increasing support for educational programs for the disadvantaged will have little or no effect on those induced to get more education, since schooling cannot change their innate ability.

Therefore, much of the literature estimating returns to schooling is concerned with whether the ordinary least squares (OLS) estimates of β_j in equation 1 reflect the causal effect of education on wages. Specifically, does more schooling cause higher earnings or are more able people more likely both to get more schooling and to earn higher wages, even in the absence of additional schooling? In the latter case, the OLS estimates of β_j will likely be upward biased due to selection on ability.

In order to determine whether more schooling causes higher incomes, the ideal experiment would involve randomly assigning one group of students to, say, complete high school and assigning another group of students to drop out of high school. The assignment to education level would be done irrespective of the student's innate ability, socioeconomic status, race, or ethnicity. Years later we would compare the labor market outcomes of these students. On average, the only difference between the two groups of individuals would be whether they had graduated from high school. Contrasts of the earnings of the two groups would provide an estimate of the causal effect of high school completion on earnings. To estimate whether this causal effect varies by race or ethnicity, one can simply estimate the difference for subgroups of students based on their race or ethnicity.

Since such an experiment cannot be done, researchers have developed two broad approaches to estimate the causal effect of education on labor market outcomes. In the first approach, so-called natural experiments, researchers attempt to locate events that might be expected to alter the schooling decisions of some people but would not be expected to independently alter their income. The basic idea is straightforward. Suppose that we knew of an event that would increase a group's educational attainment. Suppose further we were certain that this event would not have any direct effect on the group's earnings. We would then estimate the effect of education on earnings in two steps. In the first step, we would estimate the effect of the event on the educational attainment of the group. In the next step, we would measure the effect of the same event on the earnings of the group. If we find that the earnings of the group have increased, then we can be sure that education was the cause of the earnings increase, since we were certain the event would have no *direct* effect on earnings. The ratio of the income increase caused by the event to the increase in educational attainment caused by the event is an estimate of the causal effect of education on earnings. This instrumental variables (IV) estimator uses the exogenous event as the instrumental variable. Many studies using IV (for example, Angrist and Krueger, 1991; Kane and Rouse, 1995; Kling, 2001; Card, 1993; Oreopoulos, 2006) find that the instrumental variables estimate of the return to schooling is at least as large as that implied by conventional procedures.[9]

Other researchers have used sibling or twin pairs to construct estimates of the return to schooling. Because sibling and twin pairs share genetic material and were raised in similar household environments, their ability and other unobservable characteristics are much more similar than those of two randomly selected members of the population. As a result, when one relates differences in schooling between siblings to labor market outcomes, one implicitly accounts for these unobserved factors. Although the magnitude of the estimated return to schooling varies because of widely different periods covered, studies using siblings and twins indicate a significant relationship between schooling level and earnings.[10] Further, the more recent and more sophisticated estimates typically do not differ from the simpler cross-sectional estimates of the return to schooling.[11]

Although the IV and sibling/twin estimates discussed earlier find little evidence of upward biased estimates due to ability bias, this result may not hold for all subgroups of the population. For example, if African Americans and Hispanics attend schools of poorer quality on average, then those students who manage to get more schooling (particularly, perhaps, by going to college) may have unusually high ability. This would generate greater selection bias among African Americans and Hispanics, suggesting that the cross-sectional estimate of the return to schooling is more severely upward biased for these populations.

In this article, we address the potential for selection bias by including controls for ability directly using test scores and using family relationships by studying siblings.[12] When considering family relationships, we characterize the wage equation as:

$$2)\quad \ln w_{ijk} = \alpha'_j + \beta'_j S_{ijk} + X_{ijk}\gamma'_j + \mu_{jk} + \varepsilon'_{ijk},$$

where $\ln w_{ijk}$ represents the log wage of individual i from racial/ethnic group j and family k, μ_{jk} represents the "family" ability, and the other variables are defined as before. Family ability may represent a number of things, such as genetic endowment with respect to earnings capability or access to resources, that facilitate both educational attainment and labor market earnings. If log wages are linear and separable in this ability, then one can address selection bias by relating the difference in wages between family members (or siblings) to the difference in their education. If there are no further unobserved differences between family members that are correlated with both differences in schooling and differences in earnings, then this "within-sibling" estimator will provide an unbiased estimate of the return to schooling. In our analysis, we can also directly control for an observable measure of ability by including individual test scores in equation 2.

There have been many previous estimates of the return to schooling using this within-sibling estimator to study the mean return to schooling (see, for example, Gorseline, 1932; Chamberlain and Griliches, 1975, 1977). More recently, Altonji and Dunn (1996) and Ashenfelter and Rouse (1998) use this estimator to study how the returns to schooling differ by family background. Other than our own work (Barrow and Rouse, 2005), we are unaware of previous applications of this estimator to studying the return to schooling by race/ethnicity.

Measurement error in reported schooling poses another econometric challenge. As identified by Griliches (1977), because sibling education levels are so highly correlated, within-sibling estimators of the returns to schooling will be biased downward by (classical) measurement error. If the measurement error is classical in nature (meaning that it is uncorrelated with the error term in the wage equation or with the true level of schooling), then an instrumental variables estimator using an independent report of the respondent's schooling as the instrumental variable will generate consistent estimates of β_j.[13]

Again, it is not clear why the measurement error need be constant across the population. The reliability (or "signal-to-noise") ratio is the proportion of the

TABLE 1

Estimated signal-to-noise ratios for schooling levels and within siblings, by race/ethnicity and sex of individual

	Levels	Within-siblings
Women and men		
Overall	0.90	0.78
African American	0.83	0.73
Hispanic	0.89	0.82
Non-African American/non-Hispanic	0.92	0.79
Women		
Overall	0.89	0.79
African American	0.81	0.73
Hispanic	0.88	0.83
Non-African American/non-Hispanic	0.92	0.82
Men		
Overall	0.90	0.76
African American	0.83	0.72
Hispanic	0.90	0.82
Non-African American/non-Hispanic	0.92	0.76

Note: The within-sibling education is the deviation of the individual's schooling level from the mean education of his or her siblings.
Source: Authors' calculations based on data from the *National Longitudinal Survey of Youth 1979*.

observed variance in schooling due to the variance in "true" schooling. If one has two independent estimates of an individual's schooling level, the correlation between the two measures provides an estimate of the reliability ratio.[14] Using the NLSY79 data, we estimate reliability ratios for self-reported schooling, both in levels (that is, for each individual) and for the deviation from sibling means. These estimates are reported in table 1.

Overall, we estimate that 10 percent of the observed variance in schooling levels is due to measurement error. In addition, there is some variation by race/ethnicity. Nearly 20 percent of the observed variance in schooling for African Americans is due to error compared with 11 percent for Hispanics and 8 percent for non-African Americans/non-Hispanics. In contrast, there is not a lot of difference in the estimated reliability ratios by sex. The results in column 2 of table 1 indicate that sibling differences in educational attainment include more noise than individual measures of educational attainment. Overall, 22 percent of the variance in sibling differences in education is due to measurement error, although the proportion due to error is more than one-quarter for African Americans. Based on these estimates, we expect the estimated returns to schooling for African Americans to be more downward biased than those for non-African Americans/non-Hispanics or Hispanics.

Data

U.S. Decennial Census

We begin by using data from the 5 percent samples of the 1980, 1990, and 2000 U.S. Decennial Censuses. The samples included individuals aged 25–65 who were U.S. citizens and born in the U.S., who worked at least one week in the previous year, and who earned at least one-half of the minimum wage.[15] All wages and incomes are adjusted to 2003 dollars using the Personal Consumption Expenditures Price Index, a chain-type price index, from the U.S. Bureau of Economic Analysis. The regression analysis is based on annual earnings.[16]

Because the schooling variable changed in 1990, we calculate the number of completed years of schooling for 1990 and 2000 according to the recoding suggested by Park (1994). In addition, in 1980 and 1990 we identify five racial groups—white, African American (black), Native American, Asian, and other—as well as people who identified themselves as Hispanic, regardless of their race. (Thus, the six racial and ethnic groups are *not* mutually exclusive.) While in the 1980 and 1990 Censuses individuals had to choose one race category, in the 2000 Census individuals could choose multiple races. To make the 2000 Census data as consistent as possible with the previous data, we grouped those who identified themselves as belonging to multiple racial groups into the "other" category.[17] Finally, all estimates using the U.S. Census are weighted by the individual weight assigned by the Census.

National Longitudinal Surveys of Young Men and Young Women

Young men and young women are two of the original cohorts of the *National Longitudinal Surveys* (NLS). Each cohort was chosen to be representative of Americans aged 14 to 24 in the initial survey year, 1966 for young men and 1968 for young women. Both include an oversample of African Americans. We combine the young men and young women cohorts from the 1978 surveys to create a single data set of 7,440 individuals. We restrict our estimation sample to those with hourly pay greater than one-half of the federal minimum wage in 1978 and those who are not self-employed, not enrolled in school, and not in the military. Once we exclude those in 1978 with no hourly pay data and those with no information on highest grade completed, we are left with an estimation sample of 4,802.[18]

The NLS also provides information identifying respondent siblings who are also respondents in the young men or young women cohorts. Based on this information, we were able to identify 567 families with multiple respondents (1,263 respondents) in our estimation sample. If we further restrict the sibling sample to have intelligence quotient (IQ) scores, we are left with 298 families (642 respondents). On average, the sibling sample is somewhat younger than the full estimation sample. Otherwise, the mean characteristics are quite similar.

The underlying test scores used to construct the IQ score were only collected for individuals who had completed nine years of schooling as of the initial survey year, resulting in a nonrandom sample of respondents for whom we have an IQ score. In particular, respondents with reported IQ scores are less likely to be African American or live in the South and have higher average wages and more years of schooling. Within the sibling sample, respondents with IQ scores are also more likely to be male.

National Longitudinal Survey of Youth 1979

The *National Longitudinal Survey of Youth 1979* is a survey of youth aged 14 to 21 as of December 31, 1978, which includes a nationally representative sample of civilian noninstitutionalized youths; an oversample of civilian African American youths, Hispanic youths, and economically disadvantaged youths who are non-African American/non-Hispanic; and a small military sample of youths aged 17 to 21 years.[19] We use the 1993 survey of the NLSY79 in our analysis here and limit our sample to those with hourly pay greater than one-half of the federal minimum wage in 1993 and less than $300 per hour, as well as those who are not self-employed, enrolled in school, or currently enlisted in the military.

An advantage of the NLSY79 is that in 1980 most survey participants were administered the ASVAB (Armed Services Vocational Aptitude Battery), a basic skills test, from which it is possible to construct an Armed Forces Qualification Test (AFQT) score. While researchers disagree about whether AFQT scores mostly reflect "innate intelligence" or also reflect skills acquired in school, most would agree that they reflect *some* information about the skills that individuals possess at the time of the test.[20] We use the AFQT score as a measure of "observed" ability.[21]

As in the case of the *National Longitudinal Surveys of Young Men and Young Women*, many of the NLSY79 respondents have siblings who are also included in the survey. In 1979, 5,914 of the civilian respondents lived in a household with at least one other sibling (U.S. Bureau of Labor Statistics, 2003). And in 1993, respondents were asked about their educational attainment as well as the educational attainment of up to 13 of their siblings—whether or not these other siblings were respondents in the original NLSY79.[22] As a result, we can obtain own-reported and sibling-reported measures of a respondent's education level for those with siblings in the original NLSY79 sample who also participated in the 1993 wave.

Once we additionally exclude those with no wage information and no information on education, our "full sample" includes 6,119 men and women between the ages of 28 and 36. Our sibling sample contains information on 2,419 individuals from 1,062 households (for an average of 2.3 observations per household).

Results

Results using the U.S. Decennial Census

Using data from the U.S. Decennial Censuses allows us to get very precise estimates of the relationship between education and earnings by race/ethnicity for a representative sample of the working-aged population. In addition, we look at how the relationships changed between 1979 and 1999 when there were large increases in both inequality and the returns to schooling. The primary drawback with the census data is that we cannot examine the potential for ability bias or measurement error problems.

We present estimates of the returns to schooling for men and women by six race/ethnicity categories in figure 2, panel A.[23] In 1979, an additional year is associated with a 7.3 percent increase in annual income for African Americans and an 8.5 percent increase in annual income for whites. Between 1979 and 1989, the estimated return to schooling increased dramatically for all races—especially African Americans—but remained in a relatively tight range from 10.7 percent for the other category to 12.3 percent for Asians and Pacific Islanders. Between 1989 and 1999, there was a much smaller increase in the estimated returns to education on average, but an increase in the range of estimates (10.2 percent for the other category to 13.6 percent for Asians and Pacific Islanders). As shown in the contrast between panel B (for women) and panel C (for men) of figure 2, this increase in the variation in the returns to schooling by race/ethnicity is particularly true for men.[24]

Based on estimates of the returns to schooling using the census data, we would conclude that the estimated return to schooling for African Americans is roughly the same as that for whites and that the return for Hispanics is somewhat lower. While previous selection corrected and measurement error corrected estimates suggest OLS generates an estimate of the return to schooling that is roughly "right" overall (because the selection and measurement error biases balance one another),[25] we do not know if this rule of thumb holds by race/ethnicity.

Results using the National Longitudinal Surveys of Young Men and Young Women

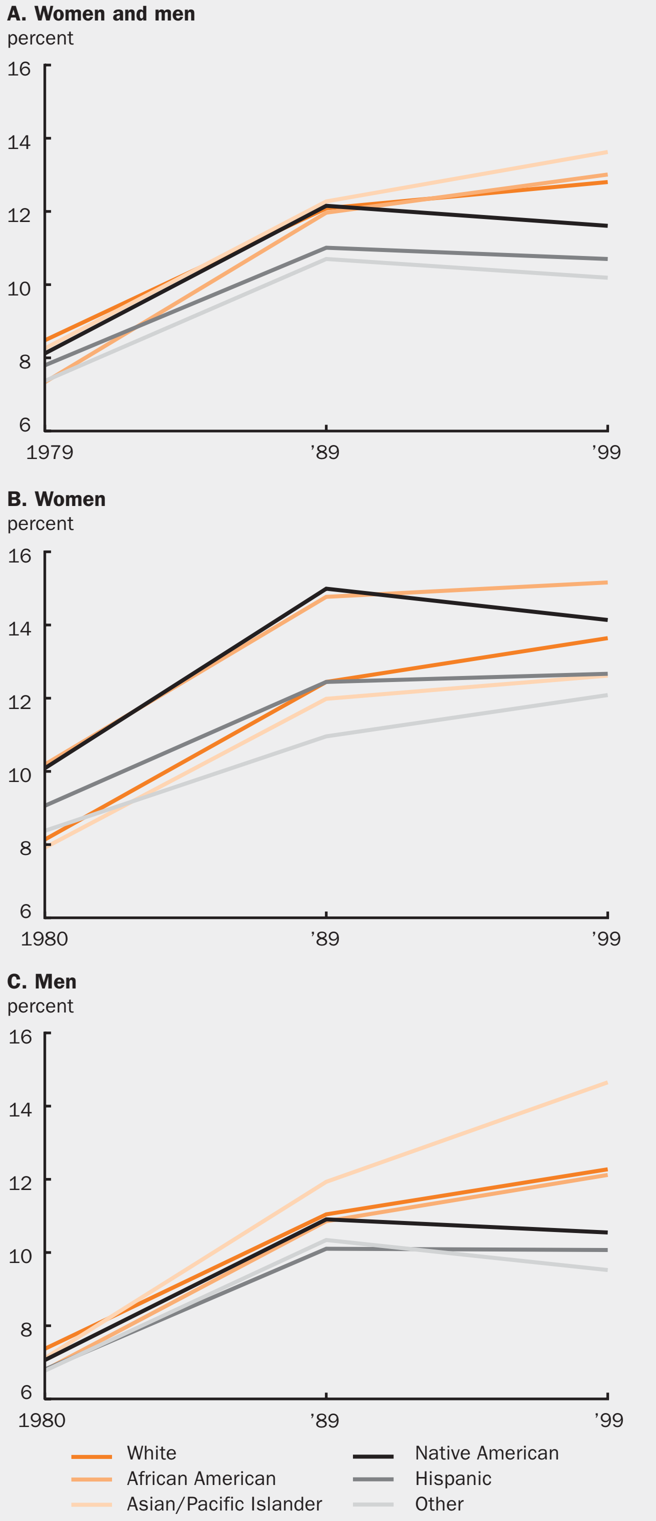
Using the NLS young men and young women cohorts, we can similarly estimate returns to education overall and separately for whites and African Americans. While we cannot generate estimates for any other racial or ethnic group, we can use the NLS measure of ability and information on siblings to get some idea about the role of ability bias.[26] Later, when we turn to the NLSY79 cohort we will additionally be able to address measurement error issues. The main drawbacks of the NLS samples are the smaller sample sizes and the fact that the cohorts are relatively young at the time of the surveys.

Table 2, panel A provides various estimates of the return to schooling (× 100) overall and separately for African Americans and whites using the NLS young men and young women cohorts. Each cell represents estimates from a separate regression and each column represents estimates from a different specification. All estimates are based on a regression of the natural logarithm of hourly pay on years of completed education, a third-order polynomial in age, an indicator for whether the individual is female, an indicator for whether the individual lives in the South (and an indicator for when the South/non-South region information is missing), and a constant. We weight observations using the 1978 sampling weights. Estimates for the overall sample (row 1) include indicators for whether the individual's race is African American or other.[27]

Using the entire sample, we estimate that an extra year of education increases hourly pay by almost 6 percent. The separate estimates by race are quite similar. Although the estimate for African Americans is somewhat higher than for whites, the

FIGURE 2

Returns to education over time



Sources: Authors' calculations based on data from the 1980, 1990, and 2000 U.S. Decennial Censuses.

TABLE 2

Estimates of the returns to schooling using the National Longitudinal Surveys of Young Men and Young Women

	1	2	3	4
A. NLS young men and young women cohorts				
Overall	5.69	5.11	4.18	
	(0.23)	(0.30)	(0.35)	
African American	6.24	6.03	5.32	
	(0.41)	(0.78)	(0.85)	
White	5.51	5.02	4.11	
	(0.27)	(0.33)	(0.38)	
Drop observations with missing IQ score	No	Yes	Yes	
Include IQ score	No	No	Yes	
B. Siblings from the NLS young men and young women cohorts				
Overall	5.49	4.63	3.65	3.14
	(0.47)	(0.85)	(1.32)	(1.38)
African American	7.05	6.28	5.73	5.54
	(0.73)	(1.23)	(3.34)	(3.43)
White	5.09	3.80	3.27	2.76
	(0.59)	(1.09)	(1.46)	(1.54)
Drop observations with missing IQ score	No	No	Yes	Yes
Include IQ score	No	No	No	Yes
Sibling fixed effect	No	Yes	Yes	Yes

Notes: Estimates are given in percent. Standard errors are in parentheses. Estimates are based on regressions of the natural logarithm of hourly pay in 1978 on years of completed schooling, a third-order polynomial in age, an indicator for sex being female, an indicator for living in the South, an indicator for the South/non-South region information being missing, and a constant. Observations are weighted using the sampling weights for 1978 provided by the *National Longitudinal Surveys* (NLS). The overall estimates also include indicators for race being African American and race being other. Estimates in column 3 of panel A and column 4 of panel B include the NLS measure of intelligence quotient (IQ). Estimates in columns 2, 3, and 4 of panel B include a family fixed effect. See the text for further details.
Source: Authors' calculations based on data from the *National Longitudinal Surveys of Young Men and Young Women*.

difference is not statistically significant. The estimates shown in column 1 of table 2, panel A do not control for the potential selection on ability problem discussed previously. Because IQ scores are missing for a nonrandom subset of the sample, the estimates in column 2 are based on the sample of individuals who have an NLS measure of IQ but do not include the IQ score in the regression. In column 3 we control for ability by including the IQ score in the regression. The column 2 estimates of the returns to education are somewhat smaller at 5 percent, but once again we find no strong evidence that the returns to education differ between African Americans and whites. Controlling for the IQ score in column 3, we see some evidence that indeed those who get more education are more able because the estimated returns to education decline by almost 1 percentage point relative to the column 2 estimates.[28] Note, however, there is a slightly larger decline in the estimates between columns 2 and 3 for whites. This larger decrease may be indicative of more selection on ability for whites or the IQ score may be a noisier measure of ability for African Americans, such that the column 3 estimates do not fully account for ability bias for this subgroup.

In table 2, panel B we turn to the NLS young men and young women sibling sample in order to allow for sibling fixed effects as well. The results in column 1 are based on the sample of siblings and are quite similar to the estimates based on the entire sample. The return to schooling estimated from the sibling sample is about 5.5 percent overall; the estimate for African Americans is higher at roughly 7 percent, although once again the difference is not statistically significant. When we allow for a sibling fixed effect in the column 2 estimates, the estimates decline by 10 percent for African Americans and 25 percent for whites. In column 3 we further restrict the sibling sample to those with reported IQ scores but do not directly control for IQ score in the regression. Again, we see that the sample with reported IQ scores is not a random subset of our estimation sample. Returns to schooling estimates for African Americans and whites are again slightly lower than for the full sibling sample. Finally, in column 4 we reestimate returns to schooling while controlling for ability with the IQ score and allowing for a sibling fixed effect. The estimated return to schooling for African Americans is little changed by including IQ score once we have already allowed for a sibling fixed effect. The estimate of the return to schooling for whites declines by 16 percent when controlling for a direct measure of ability.

In general we conclude from these surveys that the returns to schooling for young African Americans and whites in the 1970s are roughly equal, even after controlling for ability bias. However, using these surveys, we cannot correct for classical measurement error bias.

Results using the National Longitudinal Survey of Youth 1979

Estimates of the return to schooling using the *National Longitudinal Survey of Youth 1979* are presented in table 3 for our entire sample and in table 4 for women and men separately. Each table has the following layout. Each cell represents the estimated return to schooling (× 100) from a separate regression. The basic specification is an OLS regression of the natural logarithm of hourly pay on years of completed education, a third-order polynomial in age, an indicator for whether the individual is female, indicators for four geographic regions[29] (and an indicator for when region information is missing), and a constant. These regressions are unweighted, although results are similar if we generate estimates using sampling weights to weight each observation. Further, as with the analysis using the older NLS surveys, we do not cluster the standard errors on the household such that the standard errors, especially those within siblings, are understated.[30]

Each column represents a different specification. The specifications in columns 1 and 2 use the full sample; those in columns 3–8 are restricted to the sibling sample. The estimates in the odd-numbered columns do not include the AFQT score, while those in the even-numbered columns do. The estimates in columns 5–8 control for a sibling fixed effect, and those in columns 7 and 8 use the average of the sibling reports of the respondent's education as an instrumental variable in an IV analysis. Thus, the estimates in columns 1 and 3 represent the cross-sectional estimates and those in columns 2 and 4 address selection bias by controlling for the AFQT score. The estimates in column 5 control for selection by including a sibling fixed effect; those in column 6 control for both a sibling fixed effect *and* the AFQT score; and the estimates in columns 7 and 8 are similar to those in columns 5 and 6 but also correct for measurement error.

In table 3 we estimate an overall cross-sectional return to schooling of about 9 percent. The estimate is highest among African Americans (11.4 percent) and lowest among Hispanics (7.6 percent). Only the estimated return for Hispanics is statistically different from that for the other two groups. In general, controlling for selection by including an AFQT score decreases the cross-sectional estimate of the return to schooling by about 3 percentage points (that is, comparing columns 2 and 1 or columns 4 and 3). However, while controlling for the AFQT score makes the biggest difference for the estimated returns to schooling (that is, it decreases the coefficient by the most) for African Americans and Hispanics in the full sample (columns 1 and 2), it makes the biggest difference for non-African Americans/non-Hispanics in the sibling sample (columns 3 and 4). Overall, based on the cross-sectional estimates, we find little difference in the return to schooling by race/ethnicity.

Estimates that account for a sibling fixed effect are presented in columns 5–8. As also found with the older NLS surveys, a comparison of the estimates in columns 3 and 5 (or columns 4 and 6) suggests that controlling for a sibling fixed effect makes a bigger difference for non-African Americans/non-Hispanics than for Hispanics and especially for African Americans. In fact, the within-sibling estimate of the return to schooling for African Americans is at most 1 percentage point lower than the corresponding cross-sectional estimate. The fact that controlling for siblings makes a smaller difference for African Americans and Hispanics than non-African Americans/non-Hispanics may reflect less selection bias in the cross-sectional returns to schooling. Or, it may suggest that controlling for a sibling fixed effect is less effective for some populations than others. Why might this occur? While we limit our sample to siblings (excluding other household relationships, such as spouses, parents, foster siblings, stepsiblings, and adopted siblings), we cannot distinguish between "full siblings" and "half siblings." If African Americans and Hispanics are more likely to live with half siblings than those who are non-African American/non-Hispanic, then the family fixed effect may not be a good proxy for unobserved family ability.[31]

TABLE 3

Estimates of returns to schooling using National Longitudinal Survey of Youth 1979

	Full sample		Sibling sample					
	Cross-section				Within siblings			
	OLS 1	OLS 2	OLS 3	OLS 4	OLS 5	OLS 6	IV 7	IV 8
Overall	9.29	6.29	9.36	6.16	7.60	5.21	9.16	6.74
	(0.24)	(0.30)	(0.40)	(0.48)	(0.69)	(0.76)	(0.88)	(1.02)
African American	11.43	7.15	9.97	7.14	9.84	6.60	12.66	9.87
	(0.49)	(0.58)	(0.73)	(0.89)	(1.23)	(1.38)	(1.71)	(2.12)
Hispanic	7.63	4.22	9.18	5.92	7.21	5.32	8.52	6.45
	(0.55)	(0.67)	(1.05)	(1.21)	(1.60)	(1.76)	(1.91)	(2.20)
Non-African American/ non-Hispanic	9.43	6.69	9.18	5.64	6.30	4.44	7.54	5.65
	(0.33)	(0.40)	(0.54)	(0.66)	(1.00)	(1.07)	(1.24)	(1.38)
Include AFQT score	No	Yes	No	Yes	No	Yes	No	Yes
Sibling fixed effect	No	No	No	No	Yes	Yes	Yes	Yes

Notes: Estimates are given in percent. Standard errors are in parentheses. OLS means ordinary least squares. IV means instrumental variables. Estimates of the return to schooling (× 100) based on regressions of the natural logarithm of hourly pay in 1993 on years of completed schooling, a third-order polynomial in age, indicator for regions, an indicator for region information being missing, and a constant. The overall category's estimates reported in first row of each panel also include indicators for race being African American and ethnicity being Hispanic. Estimates in columns 2, 4, 6, and 8 include the Armed Forces Qualification Test (AFQT) score. The average of the sibling reports of the respondent's education are used as the instrumental variable in columns 7 and 8. All estimates are unweighted. See the text for further details.
Source: Authors' calculations based on data from the *National Longitudinal Survey of Youth 1979*.

Finally, the overall measurement error corrected (instrumental variables) estimate of the within-sibling return to schooling increases to 9.16 percent in column 7 relative to an estimate of 7.6 percent in column 5, suggesting an attenuation bias of approximately 20 percent. Further, we find that correcting for measurement error has the greatest effect on the estimated returns to schooling for African Americans, as expected based on the reliability ratios in table 1. Generally, while we continue to estimate a larger point estimate for African Americans than for Hispanics and non-African Americans/non-Hispanics, the differences across race/ethnicity are not statistically significant.[32]

While we find that returns to schooling, overall, do not appear to vary much by race or ethnicity, in table 4 we examine whether this pattern also holds for men and women. The panels in table 4 have the same structure as table 3. We continue to estimate no significant differences in the returns to schooling across African Americans, Hispanics, and non-African Americans/non-Hispanics for both men and women. While this result may partially obtain because of smaller sample sizes (resulting in less precise estimates), we find no evidence that the economic value of an additional year of schooling is lower for African Americans or Hispanics than for non-African Americans/non-Hispanics.

Conclusion

Alarmed by the increasing wage and income inequality in the United States, many researchers and policymakers who are concerned that low-income individuals are losing ground have turned to policies aimed at increasing educational attainment. And because African Americans and Hispanics are disproportionately represented in the low-income population, they are also disproportionately the focus of such policies. Yet, we know little about the magnitude of the economic benefit from the increased education for these groups. Using data from the U.S. Census and the *National Longitudinal Surveys*, we find little evidence of differences in the return to schooling across racial and ethnic groups, even with attempts to control for ability and measurement error biases. While we find point estimates that are relatively similar across racial and ethnic groups, we also partly conclude this because of relatively large standard errors in some specifications due to small sample sizes. That said, we find no evidence that returns to schooling are lower for African Americans and Hispanics than for non-African Americans/non-Hispanics. As a result, policies that increase education among the low-skilled, who are disproportionately African American and Hispanic, have a good possibility of increasing economic well-being and reducing inequality.

TABLE 4

Estimates of returns to schooling using National Longitudinal Survey of Youth 1979, by sex

	Full sample		Sibling sample					
	Cross-section				Within siblings			
	OLS 1	OLS 2	OLS 3	OLS 4	OLS 5	OLS 6	IV 7	IV 8
A. NLSY79 cohort, women								
Overall	10.45	7.37	9.79	6.01	9.87	6.62	10.83	7.37
	(0.35)	(0.42)	(0.80)	(0.92)	(1.31)	(1.44)	(1.59)	(1.83)
African American	11.45	7.54	12.08	7.37	10.63	5.39	13.93	8.26
	(0.67)	(0.78)	(1.35)	(1.56)	(2.42)	(2.59)	(3.38)	(4.06)
Hispanic	9.48	5.52	11.88	7.17	9.04	7.36	9.71	8.27
	(0.74)	(0.91)	(2.04)	(2.38)	(6.04)	(3.65)	(3.54)	(4.51)
Non-African American/ non-Hispanic	10.37	7.90	8.10	5.35	9.13	6.61	9.54	6.75
	(0.50)	(0.59)	(1.14)	(1.31)	(1.95)	(2.13)	(2.23)	(2.52)
Include AFQT score	No	Yes	No	Yes	No	Yes	No	Yes
Sibling fixed effect	No	No	No	No	Yes	Yes	Yes	Yes
B. NLSY79 cohort, men								
Overall	8.23	5.28	8.01	5.15	5.69	3.32	8.16	5.91
	(0.34)	(0.42)	(0.70)	(0.85)	(1.26)	(1.39)	(1.66)	(1.98)
African American	9.43	6.66	8.66	7.60	8.05	4.88	8.20	4.89
	(0.72)	(0.87)	(1.31)	(1.61)	(2.10)	(2.47)	(2.99)	(3.92)
Hispanic	6.05	2.90	6.31	3.73	2.92	0.52	8.61	6.97
	(0.79)	(0.96)	(1.94)	(2.20)	(3.55)	(3.96)	(4.27)	(5.04)
Non-African American/ non-Hispanic	8.66	5.67	8.22	4.35	5.06	3.30	7.79	6.23
	(0.44)	(0.55)	(0.93)	(1.13)	(1.71)	(1.85)	(2.24)	(2.56)
Include AFQT score	No	Yes	No	Yes	No	Yes	No	Yes
Sibling fixed effect	No	No	No	No	Yes	Yes	Yes	Yes

Notes: Estimates are given in percent. Standard errors are in parentheses. OLS means ordinary least squares. IV means instrumental variables. Estimates of the return to schooling (× 100) based on regressions of the natural logarithm of hourly pay in 1993 on years of completed schooling, a third-order polynomial in age, indicator for regions, an indicator for region information being missing, and a constant. The overall category's estimates reported in first row of each panel also include indicators for race being African American and ethnicity being Hispanic. Estimates in columns 2, 4, 6, and 8 include the Armed Forces Qualification Test (AFQT) score. The average of the sibling reports of the respondent's education are used as the instrumental variable in columns 7 and 8. All estimates are unweighted. See the text for further details.
Source: Authors' calculations based on data from the *National Longitudinal Survey of Youth 1979*.

We find some evidence that measurement error and selection bias may differ by race/ethnicity. For example, self-reported levels of schooling are noisier for African Americans than for other groups. And, we find less evidence of ability bias among African Americans and Hispanics than among those who are non-African American/non-Hispanic. The finding of less ability bias among African Americans and Hispanics may arise because there is indeed less selection among these groups. Or, estimators that attempt to address self-selection may be less effective for some subgroups. More generally, our analysis suggests further research is needed to better understand the nature of measurement error and ability bias across subgroups in order to fully understand potential heterogeneity in the return to schooling across the population.

NOTES

[1]This is based on authors' calculations using 2004 *Current Population Survey* (CPS), Outgoing Rotations Group data available from Unicon Research Corporation. We limit the sample to individuals aged 25 to 65 and drop observations with wages less than one-half of the minimum wage or above the 99th percentile of the wage distribution.

[2]In fact, until 1999 real wages at the 10th percentile of the distribution were below their level in 1980, and the gap between wages at the 90th and 10th percentiles continued to grow over the entire period.

[3]These numbers are based on authors' calculations using 2004 CPS, Outgoing Rotations Group data available from Unicon Research Corporation. We limit the sample to individuals aged 25 to 65 and drop observations with wages less than one-half of the minimum wage or above the 99th percentile of the wage distribution.

[4]This is based on authors' calculations using 2004 CPS, Outgoing Rotations Group data available from Unicon Research Corporation. We limit the sample to individuals aged 25 to 65 and drop observations with wages less than one-half of the minimum wage or above the 99th percentile of the wage distribution.

[5]This is based on a regression of the natural logarithm of hourly wages on years of completed schooling, a quadratic in potential experience, controls for sex, race and ethnicity, marital status, and nine U.S. Census geographic divisions (or subregions) using the March 2004 CPS. The regression was weighted using the earnings weight. For details on the U.S. Census regions and divisions, see www.census.gov/geo/www/us_regdiv.pdf.

[6]In the analyses using all the *National Longitudinal Surveys*, we control for age rather than potential work experience because of possible measurement error in education.

[7]See Dahl (2002) for evidence on differences in returns to schooling across states.

[8]For higher education, a more detailed calculation of this return would incorporate the other costs of schooling, including tuition.

[9]Angrist and Krueger (1991) use an individual's quarter of birth as the instrumental variable; Kane and Rouse (1995), Card (1993), and Kling (2001) use proximity to a two- and/or a four-year college as instrumental variables.

[10]See, for example, Ashenfelter and Zimmerman (1997) and Altonji and Dunn (1996) for studies using siblings. See Ashenfelter and Krueger (1994), Behrman, Rosenzweig, and Taubman (1994), and Rouse (1999) for studies using twins.

[11]The measurement error in reported schooling poses an econometric challenge for these models. The reason is that classical measurement error is exacerbated in within-sibling (or within-twin) estimators because sibling education levels are so highly correlated (Griliches, 1977). As a result, much of the more recent literature using this approach has focused on addressing the measurement error bias as well as ability bias.

[12]Other researchers, such as Angrist and Krueger (1991), Kane and Rouse (1995), and Card (1995), address selection bias using instrumental variables estimators. In this strategy one must identify an exogenous event (the instrumental variable) that affects an individual's years of completed schooling but is uncorrelated with the error term in the wage equation. It is very difficult to identify valid instrumental variables, and one usually requires large samples in order to get precise estimates.

[13]In this article, we assume classical measurement error in schooling. Kane, Rouse, and Staiger (1999) provide evidence that measurement error in schooling may not be classical. However, the sample sizes provided in our data are too small to implement their suggested estimator by race/ethnicity.

[14]See Ashenfelter and Krueger (1994) for an excellent discussion of measurement error models.

[15]We constructed an hourly wage rate by adjusting annual wage and salary income by the number of weeks worked in the previous year and the usual number of hours worked each week. We used the minimum wage in effect in the year before the U.S. Census in question because the census income and wages refer to the previous year.

[16]We get greater variation when we estimate the returns to schooling, using hourly wages rather than annual earnings. This is because the relationship between greater schooling and more stable jobs is stronger for African Americans and Hispanics than for those who are non-African American/non-Hispanic. Whether this correlation is explained by access to more stable jobs or changes in labor supply decisions is an empirical question (Ashenfelter and Ham, 1979).

[17]To judge the sensitivity of our results to how we categorized the 1.37 percent of individuals who selected multiple races, we tried alternative categorizations. Specifically, we tried running our regressions for whites, counting anyone who selected white only and any other combination including white as "white" (and similarly for African Americans and Asians). These alternative categorizations did not substantively change our results.

[18]Descriptive statistics on the overall and estimation samples are available upon request from the authors.

[19]Much of the military sample is dropped after 1984, and the supplemental sample of economically disadvantaged youths is dropped after 1990.

[20]For example, Herrnstein and Murray (1994) argue that the AFQT has many of the properties of an IQ test: The scores do not just reflect specific knowledge that has been learned in school; rather, they reflect more general factors of "intelligence." In contrast, Neal and Johnson (1996) argue that AFQT scores increase with years of schooling and therefore are not a good measure of IQ. Others, such as Rodgers and Spriggs (1996), argue that the AFQT is a racially biased test.

[21]In the estimates presented here, we simply control for AFQT scores and do not address the fact that individuals took the AFQT at different ages and had therefore completed differing years of schooling. We have also estimated our models controlling for the individual's education as of 1979 with qualitatively similar results.

[22]Respondents were also asked a few other questions about their siblings (for example, their age and sex).

[23]These returns to schooling were estimated from OLS regressions of the logarithm of annual earnings on years of schooling, indicators for nine U.S. Census geographic divisions (or subregions), a quadratic in potential experience, and, in figure 2, panel A, an indicator for whether the individual was female. The regressions were weighted by the U.S. Census weight.

[24]For an analysis of the change in returns to schooling by race/ethnicity between 1979 and 2000 using the *Current Population Survey*, see Bradbury (2002).

[25]See, for example, Ashenfelter and Rouse (1998).

[26]In addition, because the sample sizes are so small we cannot estimate the returns to schooling for men and women separately.

[27]These specifications and those using the NLSY79 do not account for possible correlations across individuals within the same household. As a result, the standard errors are likely understated, although allowing for such intra-household correlations makes little difference in the cross-sectional specifications.

[28]Estimates that include IQ score but include an indicator for IQ score is missing are quite similar to the estimates shown in column 2 of table 2, panel A. The coefficient estimates (standard error in parentheses) are 5.06 (0.25) for the overall sample, 5.93 (0.44) for African Americans, and 4.86 (0.30) for whites.

[29]The four regions are the Northeast, Midwest, South, and West as defined by the U.S. Census.

[30]In the cross-sectional specifications, those using clustered standard errors are very similar to those that do not allow for such intra-household correlations; the standard errors for the within-sibling specifications presented in columns 5 and 6 in table 3 are understated by approximately 50 percent.

[31]As evidence consistent with this explanation, the within-sibling coefficients in column 6 (that control for AFQT scores) decrease the most relative to column 5 (that do not control for AFQT scores) for African Americans. This pattern of results would be expected if the AFQT score controls for other aspects of ability that are not captured by the sibling fixed effect.

[32]In results not presented here, we have also estimated these specifications using annual earnings rather than hourly wages. While the estimated returns to schooling are a little higher, there are no differences by race or ethnicity.

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