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College Quality and the Wages of Young Men

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Abstract

Using the rich data from the National Longitudinal Survey of Youth, we show that several dimensions of college quality have substantial positive impacts on young men's wages. This finding is robust to a wide array of alternative specifications. Controlling for ability reveals that sorting of more able persons into better colleges accounts for only a modest portion of the unconditional quality effect. We find that young black men reap larger gains to quality than do young white men. Our results also indicate that attending a college with a racially diverse student body increases the later earnings of both white and black men.

JEL Codes: I2, J3.
Key Words: returns to education, human capital, wages.

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Each year, thousands of students (and their parents) invest their time, energy, and money deciding where to go to college. With the cost of four years at an elite private college or university now approaching $100,000, this decision represents a major financial commitment for most families. For all the importance of this choice, little is known about the decision process itself or about how much it matters to the subsequent earnings of students.

In this paper, we explore the impact of choices about the quality of college to attend on the subsequent earnings of young men.[1] There is a large literature on the returns to higher education measured in terms of years of schooling completed or degree attained, but less is known about the effect of college quality. At the same time, the relationship between educational quality and job performance is receiving increased public attention, and although much of that attention focuses on high schools, the ability of colleges to adequately prepare students for work is also a concern. Other current policy debates also touch on quality issues. Changes in student loan availability or costs will affect the decisions of students to attend public or private colleges and the quality of the education they buy. Changes in affirmative action or other "diversity" programs at colleges and universities would affect the quality of college education available to members of various groups.

Deciding what college to attend involves an array of factors ranging from the family's financial situation, to the student's and his siblings' intellectual abilities, to the perceived benefits of attending the various colleges. An economist looking at the consequences of these decisions can hardly ignore the nonrandom nature of college selection and the potential for selection bias

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[1] In Daniel, Black, and Smith [DBS] (1997), we look at the relationship between college quality and the wages of young women.

this may create.[2] While ability bias is of particular concern, much of the small existing literature on college quality fails to control effectively for ability, background characteristics, labor market experiences, or earlier education. For instance, Weisbrod and Karpoff (1968), who use data from a single large employer, have only crude measures of college quality and the intellectual abilities of students, and no controls for family background. Reed and Miller (1970), using a 1967 Supplement to the Current Population Survey, have limited controls for individual abilities, as well as limited information on labor market experience. Similarly, Morgan and Duncan (1979), using the 1974 Panel Study of Income Dynamics, have only limited controls for intellectual abilities.

Wales (1973), Solmon (1975), Solmon and Wachtel (1975), and Wachtel (1976) use the National Bureau of Economic Research–Thorndike Sample of World War II veterans who volunteered for pilot, navigator, and bombardier training programs. While the NBER-Thorndike data include outstanding controls for individual ability, there is limited information on labor market experience, and the sample was drawn from a highly unrepresentative population. Also using a highly unrepresentative sample, albeit one rich in family and individual endowment measures, Behrman *et al.* (1995) estimate college quality effects among female twins born in Minnesota between 1936 and 1955.

In contrast to these earlier studies, we use unusually rich data from the National Longitudinal Survey of Youth (NLSY) to estimate quality effects from detailed wage regressions that control for previous labor market experiences, family and other background characteristics, and characteristics of respondents' high schools. The data also include a very

[2] See Becker and Tomes (1976) for a model of family resource allocation relevant to college quality choice.

good general ability measure. scores on the Armed Services Vocational Aptitude Battery. As such, our data most resemble those of James *et al.* (1989), Sweetman (1994), Loury and Garman (1995), and Bremer, *et al.* (1996)[3] who use the NLS High School Class of 1972 data (NLSHS72) to examine the impact of college quality on wages. The NLSHS72 data also include information on the intellectual abilities of students. and the student's family background and high school, but the NLSY contains much better controls for the labor market experiences of workers. In addition, our data cover the most recent cohorts of young men currently available. This may be important because the pattern of results from research on high school quality suggests that the link between high school inputs and outcomes has weakened over time (Betts, 1996), while Behrman, *et al.* (1996) suggest that returns to college quality have been increasing.

College quality may affect wages directly, but it may also act indirectly through labor force participation, the likelihood of attending graduate or professional school, the student's marital status and attributes of his spouse, the industry in which he later works, or the region in which he later lives. If college quality affects any of these outcomes, then wage equations that control for them will produce biased estimates. Ideally, we would like to estimate a structural model that accounts for the interrelationship among these factors. Our goal for this paper, however, is more modest: we seek to establish the reduced-form relationship between the quality of college education and male earnings. We find the empirical relationship to be robust across a wide variety of specifications.

We begin by reporting estimates from regressing wages on a constant and on individual college characteristics, with and without controls for ability. These results form a baseline against

[3] Bremer, *et al.* (1996) also use the High School and Beyond data.

which to compare our subsequent results. These estimates are unbiased only if all factors correlated with wages are orthogonal to college quality. While including ability controls generally lowers the return to college quality, the effects of college quality remain substantively and statistically significant. Following that, we estimate a series of standard wage regressions and report the coefficients on individual college characteristics. We continue to find a positive relationship between wages and many measures of college quality. These estimates are the correct measures of the return to college quality if college quality does not affect any of the individual characteristics thatappear in the wage regressions.

While the various measures of college quality are often highly significant when we enter them individually in a wage equation, when we enter them collectively, most are insignificant, and some have the "incorrect" sign. As this suggests, the various measures of college quality are highly correlated, a situation also noted by Solmon (1975), Morgan and Duncan (1979), and James *et al.* (1989). We exploit this collinearity and combine the various measures to construct a college quality index. We then substitute our quality index for the individual characteristics and explore the robustness of our basic finding that respondents who attend higher quality colleges have higher wages. We conclude with an empirical assessment of the plausibility of our quality effect estimates.

Our major findings are as follows. First, we find that attending a higher quality college increases the wages of young men. This is true for a wide array of quality measures. This result is consistent with earlier work; we show that it is a robust relationship that endures even with detailed controls for individual ability, labor market experience, family background, high school quality, and industry of employment. Our point estimates of the effect of quality are consistent with the returns found for other forms of human capital, and are consistent with our estimate of the price of quality.

Second, the returns to college quality are substantially higher for black men than white men. This is consistent with the findings of Loury and Garman (1995) from the NLS Class of 1972. Furthermore, we find that white men attending schools with a higher fraction of black students have higher wages. This is a robust relationship that exists even with controls for college quality, ability, family background, high school quality, college major, and industry of employment. There is weaker evidence that, above a certain level, black men attending colleges with higher fractions of black students have lower wages. Taken as a whole, our results suggest that attending colleges with relatively diverse student bodies increases the earnings of both black and white men.

DATA

Our data come from three sources. Our primary source is the National Longitudinal Survey of Youth (NLSY), a panel data set based on annual surveys of a sample of men and women who were 14–21 years old on January 1, 1979. Respondents were first interviewed in 1979 and have been re-interviewed each year since then. Of the five subsamples that comprise the NLSY, we use only the representative cross-section and the minority oversamples. After deleting observations with missing or inconsistent data, we are left with about 3,100 men.[4]

The NLSY provides unusually detailed information about respondents' employment histories, including detailed information about wages, experience, and tenure. In addition to the

[4] Comparisons of means for white men revealed no evidence of bias from eliminating observations with missing or invalid data. Among black men, there is some evidence that removing observations with missing values tended to select respondents with stable employment.

usual demographic and other controls, it provides the identity of the colleges that respondents attended. Thus, for each respondent who attended college, we were able to attach his college's characteristics.[5] Our sources for college characteristics are the Department of Education's Integrated Postsecondary Education Data System (IPEDS) for 1990 and the *US News and World Report's* Directory of Colleges and Universities (1991). The former source provides most of the information about the colleges and their faculties; the latter provides most of the summary information about colleges' students. We only included information for four-year colleges; roughly one-half of the people in our sample with some type of postsecondary education attended a four-year college. The variables are described more fully in the Appendix.

Whenever necessary, we redefine the raw college characteristics so that larger values correspond to obvious notions of quality. For example, we transform the acceptance rate reported in the raw data into a rejection rate, under the assumption that more selective colleges are of higher quality, but leave college size untransformed. Large size might be a disamenity, but it is not at all obvious that it should be treated as a potentially negative indicator of quality. To the extent that small size limits specialization, larger schools can provide higher quality education both because they can reap efficiency gains from specialization and because they can offer more courses and so more closely match a given student's optimal curriculum. Diseconomies of scale presumably emerge at some point for the usual reasons.[6]

[5] We attached 17 college characteristics: nominal tuition, spending per student, enrollment, the student/faculty ratio, percent of faculty with a Ph.D., the acceptance rate, 1st-year retention rate, graduation rate, percent of students who are black, percent of students who are women, mean, 25th and 75th percentile SAT scores of first-year students (ACT scores are converted to SAT equivalents), percent of first-year students who were in the top 10 and top 25 percent of their high school classes, and an indicator of whether the college is public or private.

[6] One concern that can be raised about many of the statistics is that colleges know prospective students treat them as quality measures. *The Wall Street Journal* (1995) reports that many colleges misreport these statistics for

ESTIMATION RESULTS FOR COLLEGE QUALITY

In this section we discuss our model. We estimate standard Mincer wage equations of the form

$$(1) \qquad \ln W_i = c + Q_i\beta_q + S_i\beta_S + A_i\alpha + X_i\beta_X + Z_{Hi}\beta_H + Z_{Pi}\beta_P + Z_{HSi}\beta_{HS} + \varepsilon_i ,$$

where c is a constant, Q is a vector of college characteristics (often a single characteristic), S is years of schooling, A is our set of ability controls, X is a standard set of wage regressors, and the Z_J are vectors of background characteristics: Z_H, Z_P, and Z_{HS} describe the early home environment, the individual's parents, and the individual's high school, respectively.

The ε_i captures the effect of all omitted influences. Of course, if our measures of college quality are correlated with ε_i, the corresponding estimate of the return to college quality would be biased. This "selection bias" poses a considerable problem in estimating the returns to college quality. In response to this problem, we could specify a college quality selection equation and perform a "selection correction" such as outlined in Garen (1984). (In essence, Garen extends Heckman's (1976) selection correction procedure to the case of a continuous variable.) As Goldberger (1983) and others have noted, however, experience with such parametric selection corrections have lead researchers to be suspicious of the distributional assumptions underlying such approaches.[7]

Intuitively, the selection problem is nothing more than a missing variables problem. The error term, ε_i, comprises variables that the econometrician does not observe, and some of those unobserved factors may be correlated with the regressors. Rather than try to model the

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marketing purposes, but it is unlikely that misreporting produces a negative relationship between reported and actual values.

distribution of the error term ε_i, our approach exploits the richness of the NLSY data and simply adds more controls to attempt to capture any components of ε_i that are correlated with our measures of college quality. Recently, Heckman, Ichimura, Smith, and Todd (1996) used a particularly rich data set to examine the impact of the Job Training Partnership Act (JTPA) training on earnings. Using experimental estimates of the impact of training as a benchmark, they find that richer data on variables related to selection into training greatly reduces, but does not eliminate, selection bias.

Thus, we begin our analysis with an extremely parsimonious specification of equation (1). We then introduce additional controls. To the extent that the coefficients on our measures of college quality are not sensitive to the inclusion of additional controls, this represents evidence that we have captured the returns to college quality.[8]

A major advantage of the NLSY data is the existence of controls for the student's academic abilities. These ability controls are based on the Armed Services Vocational Aptitude Battery (ASVAB). O'Neill (1990), Blackburn and Neumark (1992), and Neal and Johnson (1996) have used these ASVAB scores to control for otherwise unobserved differences in ability. Following Heckman (1995), we use the first two principal components of respondents' age-

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[7] Bremer, et al. (1996) use a parametric selection correction approach to estimate a model wherein students choose whether or not to attend an "elite" college. They find that correcting for selection in this way has little effect on their estimates.

[8] This strategy is not without risk. For instance, suppose we found that the returns to college quality are sensitive to the inclusion of industry controls. It is plausible that industry wage differentials reflect unobserved differences in the quality of workers. To the extent that these differences are due to innate abilities, the inclusion of industry controls is appropriate, and any reduction in the magnitude of the returns to college quality would be evidence of selection on these innate abilities. It is also plausible, however, that college quality, rather than innate abilities, generates differences in industry. This is precisely the finding of DBS (1995b) for women.

adjusted scores on the 10 exams that comprise the ASVAB, and the squares of these components. (We describe the ASVAB and the age adjustment procedure in the Appendix A.[9])

The dependent variable in each regression is the natural log of the real wage in 1987. Estimated coefficients are from OLS regressions on the set of working respondents not enrolled in school.[10] Using Koenker's (1981) robust version of Breusch and Pagan's (1979) test, we easily reject the null hypothesis of homoskedastic errors for many of our specifications. Therefore, we estimate covariance matrices using White's (1980) heteroskedasticity-consistent estimator. Given our strong presumption that college quality should only enhance human capital, there is a case for performing one-tail significance tests. We report the more conservative two-tail significance tests because, although it is quite unlikely that higher quality colleges provide less human capital, it is possible that quality affects wages in other ways. For example, it is conceivable that college quality affects social or leisure capital or home production in ways that lower wages.

College Characteristics in Wage Regressions

In Table 1, we **separately** enter each of 12 different college quality measures into five different regressions: in column (1) we report a parsimonious specification that only includes the single college characteristic (Q), an indicator variable for having any post-secondary schooling, and a constant; in column (2) we add controls for ability (A); in column (3) , we

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[9] We have repeated much of our analysis using only the first principal component of the unadjusted ASVAB scores, and using (unadjusted) Armed Forces Qualifying Test (AFQT) scores in place of our age-adjusted principal components. In both cases we obtain results very similar to those reported. The AFQT is a weighted average of scores on four of the ten ASVAB tests.

[10] As reported in DBS (1995b), we have repeated some of the analysis for women, estimating wages and participation simultaneously using the model described by Heckman (1976, 1979). Doing so has little effect on the quality coefficients or their (full-information) standard errors.

include quartics in four variables (age, tenure, pre-college graduation labor market experience, and post-graduation experience), controls for race, geographic region, urban residence, any postsecondary school completed, years of school completed, years of school completed after high school, having a 4-year degree, and the ability controls described earlier; in column (4) we add our coarse set of controls for college major; and in column (5) we add controls for industry and union status. In each equation, if the respondent did not attend college, the college quality measure was set to zero. For those who attended college, if the college characteristic was missing for a respondent, it was set to zero and a dummy variable was included indicating that the college characteristic was missing.

For measures of college quality, we use tuition, spending per student, the faculty-to-student ratio, the size of the student body, the rejection rate, the first-year retention rate, the graduation rate, the fraction of the student body in the top 10 percent of their high school class, the average SAT score, the 25th percentile of the SAT score, the 75th percentile of the SAT score, and the fraction of faculty with Ph.D.'s. All of these measures of college quality are widely available in the various college guides we examined.

In the parsimonious specification, all measures of college quality were statistically significant at the five percent level. Of course, given the limited number of controls, these estimates may be biased. One obvious problem with this specification is our failure to control for individual ability. To the extent that higher quality colleges and higher ability students are matched in the college selection process, our measures of college quality may be proxying for ability. In column (2), we include ability controls based on the ASVAB scores. Except for the rejection rate, whose coefficient remains virtually unchanged, the magnitudes of the coefficients for our college quality measures all fall when ability is included, usually by about one-third. This is consistent with matching of students to colleges based on ability.

In column (3) we report estimates of the returns to our college quality measures using the richer set of controls already described. We separated pre- and post-graduation experience because we were concerned that college quality might be correlated with forfeiting occupation- or industry-specific human capital acquired from pre-graduation work.[11]

With inclusion of these basic controls, the coefficients on tuition and size of student body are no longer significant at even the 10 percent level, and the 75th percentile of the SAT is significant only at the 10 percent level. Generally, the magnitude of the coefficients on the college quality measures fall with the inclusion of these basic controls, which is again consistent with matching in the college market between higher quality colleges and persons with high-wage characteristics. Yet, 9 of the 12 measures remain significant at the 5 percent level, and 10 of the 12 remain significant at the 10 percent level.

In column (4) we add controls for college major. The coefficients on the college quality measures are virtually unchanged. Thus, the relationship between college quality and wages is not generated by differences in the distribution of college majors across schools. Similarly, in column (5) we add controls for industry of employment and union status. The coefficient on the size of the student body becomes significant at the 10 percent level, and the coefficient on the 75th percentile of the SAT becomes significant at the 5 percent level. The significance of the other college quality measure coefficients is unchanged. Importantly, these specifications contain several variables (e.g., industry, tenure, experience, location) that are determined after the worker leaves college. To the extent that college quality contributes to workers being employed in higher paying industries, having longer spells of tenure and more experience, and locating in high-wage areas, the estimates reported in columns (3) through (5) of the returns to college quality understate the true returns.

Regardless of specification, spending per student, faculty-student ratio, the rejection rate, the first-year retention rate, the graduation rate, the fraction of students in the top 10 percent of their high school class, the average SAT score, the 25th percentile of SAT score, and the fraction of faculty with Ph.D.'s are always significant at the 5 percent level. Perhaps with the exception of the 25th percentile of the SAT score measure, these are commonly reported measures of college quality, and they have a substantial impact on wages.[12] For instance, using the specification in column (3), a man attending a college with spending per student at the 75th percentile in our sample has 2 percent higher wages than a man with the same characteristics at the 50th percentile of spending per student.[13]

College Quality Index in Wage Regressions

Confronted with a large number of possible measures of college quality, we faced a choice about how to measure college quality overall . We could enter all of the measures together (see Appendix B) but the resulting coefficients are difficult to interpret. Moreover, the college quality measures are highly positively correlated with one another, as has been noted in earlier

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[11] Treating pre-graduation and post-graduation experience separately is justified by F-tests at better than a 1 percent level of significance but has little effect on our estimates.

[12] We were concerned that these college characteristics might be proxying for the SAT scores of respondents and that the latter might be correlated with wages. We found no evidence of bias arising from this source. Including respondents' combined verbal and math SAT score has essentially no effect on the estimated effect of the college aggregate SAT measures.

work by noted by Solmon (1975), Morgan and Duncan (1979), and James *et al.* (1988). A college with high spending per student is likely to also have a high faculty/student ratio and a large fraction of students with high SAT scores.

In addition, it is not obvious that the measures of college quality that we use are true inputs into the production of human capital. For instance, holding fixed the average SAT of students and the fraction of students that graduated in the top 10 percent, would the human capital of Penn students be increased if Penn rejected 10 percent more students? We think not. Rather, we interpret many, if not all, of our measures as noisy signals regarding the true, unobserved quality.

To formalize the problem, let Q be the unidimensional quality of the college. Suppose that across all colleges, $E(Q) = 0$ and $E(Q^2) = 1$. Let Z be a n-vector of noisy signals about the quality of each college, such that for a college with quality Q, the value of the signal is $Z_i = \beta_i Q + u_i$, with $E(Z_i) = 0, E(Z_i^2) = 1_i, E(u_i u_j) = 0 \forall i \neq j,$ and $E(Qu_i) = 0.$ Using the signals of college quality, we construct a measure of college quality by taking a linear combination of the signals. Define $z_i = \beta_i^{-1} Z_i$ (to simplify the notation) and $\hat{Q} = \sum_{i=1}^{n} \gamma_i z_i$, where there is no need for an intercept term as the expected value of Q is normalized to zero. We select the γ's to minimize the expected squared distance between $\hat{Q}$ and Q, or

(2) $$\min_{\gamma_i} E(Q - \hat{Q})^2.$$

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[13] See Table A6 in the Appendix; the percentiles refer to the distribution of college characteristics in our sample, not the distribution of colleges.

The necessary conditions for minimization are

(3) $$1-\sum_{i=1}^{n}\gamma_i-\delta_j\,\gamma_j=0 \quad \forall j\in\{1,2,\ldots,n\},$$

where $\delta_j \equiv \beta_j^{-2}\,\sigma_j$ and $\sigma_j = E(u_j^2)$. It can be shown that

(4) $$\gamma_j=\frac{\delta_j^{-1}\prod_i^n\delta_i}{\prod_i^n\delta_i+\sum_{k=1}^{n}\delta_k^{-1}\left(\prod_i^n\delta_i\right)}.$$

The sum of the γ's are less than one. Thus, a 10 percent increase in all the signals increases the expected college quality by less than 10 percent, which is consistent with the unidimensional signal extraction problem. Moreover, γ_j is decreasing in the variance of the idiosyncratic error u_j, so that signals that more accurately reflect college quality receive more weight in the forecast.

Readers familiar with the psychometrics literature may recognize this model as a transformation of Spearman's (1904) two-factor model; see Harman (1976) for a good discussion of the historical development of the model. To implement the model, we simply specify the signals to be used in the factor analysis.

The desire to produce a quality index for as many colleges as possible guided our choice of which characteristics to include in the factor analysis.[14] Because many colleges fail to report one or more characteristics, we were able to calculate a quality index for a larger number of colleges by using a smaller set of characteristics. (When at least three or four variables were

[14] Factor analysis and principal components analysis produced indices with very high rank order correlation (0.99 for the three variables we use). We experimented with including both the first and second principal components in wage regressions, but the second principal component's coefficient was always small and statistically insignificant (see DBS, 1995a).

included in the sets, it did not matter much which variables we chose to include; all indices produced were highly correlated.) We settled on using as our quality index the first factor of three variables: spending per student, the rejection rate, and average SAT score.[15] The first measure indicates resources available to the college, the second presumably measures selectivity of the school, and the third provides information about the quality of the students.

The factor analysis produced only one positive eigenvalue, indicating that only one common factor was found. The scoring coefficients (normalized weights on each signal) were 0.34 for spending per student, 0.24 for the rejection rate, and 0.43 for the average SAT. (Using this index, the 5 highest ranked colleges in our sample were Stanford, MIT, Yale, Harvard, and Columbia.) In addition to removing some of the measurement error in these signals, the use of the college quality index allows us to focus on a single measure of college quality, which simplifies the interpretation of the estimates and greatly reduces the number of regressions coefficients that we report.

In Table 2, we report results from regressions using the college quality index in place of individual college characteristics. We report results for two quality indicators: the raw quality index and four dummy variables indicating the quality quintile into which each college falls (the lowest quintile is the omitted category). In column (1) we use controls for ability and our basic wage-equation controls (as in column (3) of Table 1), in column (2) we add the coarse college major controls (as in column (4) of Table 1), and in column (3) we include controls for industry

[15] To check further the robustness of the results we report later, we experimented with an alternative quality index constructed by replacing one of our three signals with alternative measures; the resulting indices were quite similar. In an early version of this paper, we also used 6-signal index constructed using principal components. The results were very similar to the ones reported here; see DBS (1995a).

and union status (as in column (5) of Table 1). To see if selection may explain any positive relationship between wages and college quality, in columns (4) through (6) we include several other sets of controls. In column (4) we include "home" controls that ask whether anyone in the student's household regularly subscribed to a magazine, whether any one in the household regularly subscribed to a newspaper, and whether anyone in the household had a library card. In column (5), we add "parent" controls that indicate whether the family was intact when the student was 14, whether the parents were alive, parents' education, and parents' occupation. Finally, in column (6) we include a set of high school controls that indicate, for each individual's high school, the number of books in the library, the salaries of the teachers, and the percentage of disadvantaged students that attended. (See Appendix A, Table A3 for more detail on these controls.)

Men who attend better colleges earn higher wages. This is true regardless of what controls we use.[16] The quality effects are large. According to these estimates, men who attend colleges in the top fifth of the quality distribution earn wages about 13 percent higher than otherwise identical men who attend colleges in the bottom fifth of the quality distribution. Adding background variables has very little effect; neither the point estimates nor the standard errors change materially.[17] Given the stability of the estimates, we think this represents

[16] We also estimated regressions in which we replaced the coarse college major controls with the 81 detailed college major controls. The point estimates of the quality effect are slightly smaller when the more detailed college major controls are used, but overall, across all specifications, the results are very similar.

[17] Home characteristics are jointly significant at 10 percent or better, except when detailed college major and industry controls are present. When major and industry controls are present, and other background controls are absent, whether or not someone in the respondents' household received newspapers and whether anyone had a library card are significant at the 10 percent level. There is stronger evidence for the parental background controls. The parental controls are jointly significant at the 10 percent level or better in all specifications, although except for some of the parental occupation indicators, none of the controls are individually significant in any specification. Respondents' mothers', but not fathers', occupation controls are jointly significant at the 5 percent level or better

reasonably good evidence that the return being measured is truly a return to college quality and not a relic of selection bias.[18]

Overall, our quality index results for men are consistent with those reported earlier for individual college characteristics. We continue to find evidence of a statistically and economically significant benefit to college quality, regardless of whether we control for college major, industry of employment, or a wide range of family background and high school characteristics.

Gender, Race, and the College Quality Index

Some (*e.g.*, Tidball, 1989) have argued that students, especially female students, will learn more when attending schools with a higher proportion of their own gender. Similar arguments have been made for race as well (see, *e.g.*, Worthington, 1991). To examine that claim, in Table 3 we regress the natural logarithm of the wage against the fraction of the student body that is female or the fraction that is black. For both the fraction female and the fraction black regressions, we use the same 6 specifications as we used in Table 2. Because of concern about the endogeneity of the industry of employment, we also report a seventh regression that has all the controls

in all specifications. There is very strong evidence of a relationship between men's high school characteristics and wages. These characteristics are jointly significant at the 5 percent level or better in all specifications. The starting salary of teachers in the individual's high schools is individually significant at 5 percent or better in all specifications, as is total enrollment when major and industry controls are present. Our estimates imply that controlling for college quality, a man who graduated from a high school at the 75th percentile of teachers' starting salaries earns about 3 percent more than a similar man whose high school is at the 25th percentile. While much of the debate about whether primary and secondary schools characteristics affect learning has focused on test scores (see Hanushek (1986) for a review of the literature), evidence using wages is mixed. See Card and Krueger (1992), Betts (1994), Altonji and Dunn (1995) and Heckman, et al. (1996a, 1996b) for recent contributions. As far as we know, Wachtel (1976) is the only other paper that includes both high school and college characteristics, and he also finds that both high school and college characteristics affect wages.

[18] As yet another test of robustness, we estimated a Cox regression model. Commonly used in duration analysis, the Cox model only relies on the rank of the dependent variable; it does not assume a distribution for it. Using the Cox model, we estimate that college quality significantly increases wages; see DBS (1995a).

except for the industry controls. For both the fraction female and the fraction black regressions, we repeat the analysis with and without our college quality index.

When we do not use the college quality index, we find, to our surprise, a negative and significant relationship between men's wages and the proportion of the student body that was female at their college: in each of the 7 regressions the coefficient on the fraction of the student body that was female was negative and significant at the 5 percent level.[19] (The reader should keep in mind, however, that the interquartile range of the fraction female is only 48 percent to 57 percent.) A much different story, however, emerges when we include our measure of college quality. In only 3 of the 7 specifications is the coefficient significant and then only at a 10 percent level. The coefficients on fraction female are reduced by roughly one-third with the inclusion of the college quality index, which suggests that the fraction female and college quality are negatively correlated. Indeed, the correlation between the fraction of the student body that is female and our quality index is -0.34. Nevertheless, the point estimates suggest a moderate effect on wages: using the specification in column (6), moving from the 25th percentile to the 75th percentile in the distribution of the fraction of the student body that is female would reduce wages by about 1.8 percent.

The results for the fraction of the student body that is black are even more intriguing. Because we expect the fraction black to have differing effects for black students than for white students, we enter the fraction black students and the interaction of that variable with an

[19] This is broadly consistent with the findings of Sweetman (1994), who found that men, but not women, attending single sex schools.earned significantly higher wages. Using data from the National Longitudinal Study of the High School Class of 1972, Rothstein (1995) finds that women who attend schools with a greater fraction of female undergraduates are significantly less likely to attend graduate school, receive an advance degree, and earn

indicator that the student is black. Thus, the coefficient on fraction black provides the impact of that variable for non-black (primarily white) students, and the sum of the coefficient on fraction black and the interaction term provide the impact for black students. In the regressions in which we do not control for college quality index, we find some evidence that attending schools with a higher fraction of black students increases the earnings of non-black students, but there is no evidence that attending schools with a higher fraction of black students increases the earnings of black students. The evidence is not particularly strong: in three of the specifications the coefficient is insignificant and in two other specifications it is significant only at the 10 percent level.

When we add the college quality index, the coefficient on percentage black students increases in each of the 7 regressions and is significant at the 10 percent level or better in each regression. Thus, once we control for college quality, there is reasonably strong evidence that attending schools with more black students increases the wages of non-black students. There is no evidence of a similar effect, however, for black students. In each of the 7 regressions, we cannot reject the hypothesis that the fraction of students who are black has no impact on the wages of black students.

Recently, Loury and Garman (1995), using the NLS High School Class of 1972 data, report that blacks receive a higher return to attending selective colleges than comparably qualified whites. In the last section of Table 3, we look for this effect with the more recent NLSY data by interacting our quality index with a dummy variable indicating that the individual is black. In each specification, blacks have a higher return to college quality than non-blacks,

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significantly lower wages. These differences, however, cease to be statistically significant when she conditions on and the difference is significant at the 10 percent (two-tailed) level or better in each of the 7 regressions. Moreover, the point estimates indicate that the return to quality for blacks is nearly three times that for non-blacks. Finally, even when we include the interaction term for college quality, we cannot reject the hypothesis that the fraction black has no impact on the earnings of black students in each of the 7 regressions.

The typical black man in our sample attended a college with a very different racial makeup than that of the typical non-black. For non-black men, the 25th percentile school has a student body that is 2 percent black; the 50th percentile is 5 percent; the 75th percentile is 8 percent. For black men, the 25th, 50th, and 75th percentiles are 7, 16, and 83 percent black, respectively. One interpretation of our results, therefore, is that attending overwhelmingly non-black or mostly black schools lowers the wages of both groups. To test this interpretation, we construct four categorical variables from the fraction black variable: less than 5 percent black, between 5 and 7 percent black, between 8 and 17 percent black, and more than 17 percent black.[20] We also interact these categories with a dummy variable indicating that the person is black. In both specifications, we include the college quality index, and in the second specification, we include the index interacted with a dummy indicating the person is black. We report these results in Table 4. For specifications without the interaction terms, we find those attending colleges with between 5 and 7 percent black students earn more than those attending colleges with fewer than 5 percent black students. In addition, men attending colleges with between 8 and 17 percent black students earn more than men attending schools with fewer than 8

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the distribution of majors at the college attended.

[20] These categories were chosen to ensure that each had sufficient numbers of both black and non-black men.

percent black students and more than 17 percent black students. A similar story arises when we allow the interaction terms. We also test to see if it was necessary to include the interaction terms for the percent black; in each of the seven regressions we cannot reject the hypothesis that the coefficients on the interaction terms for the fraction black categories are zero. Thus, the evidence suggests that a diverse student body raises the wages of both non-blacks and blacks, up so a point, and that the returns to diversity are the same across races.[21] In sum, even after controlling for college quality and background characteristics, we find evidence consistent with the view that attending a college with a moderate level of racial diversity among its students raises earnings for both black and non-black men. Controlling for college quality, however, greatly weakens the evidence that men's wages are inversely related to the proportion of female students that the college they attend.

Are the Estimates Plausible?

We have produced estimates of the return to college quality. If we are willing to make some simplifying assumptions we can answer the question, "Do wages increase enough to pay for the added financial investment required to attend a good college?" More importantly, given that the additional assumptions are reasonable, we can use our answer to gauge the believability of our quality effect estimates. If the market for college quality is in equilibrium, then at the margin, the price of quality should just offset its return. We have estimates of the average, rather than the marginal, return. If, as seems likely, most students are inframarginal, then our estimates of the return to quality should exceed our estimated price of quality.

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[21] These results treat educational attainment as given. If attending schools with a greater fraction black improves the educational of blacks, the increase in attainment would offset the earnings penalty. Interestingly, Ehrenberg and Rothstein (1993) report that blacks attending historically black colleges are more likely to graduate.

We compare our direct estimates of the wage effect produced by college quality to estimates of the wage effect that would just offset the additional cost of quality. The direct financial benefit from additional college quality is the present value of the additional earnings produced by the increment in quality. The cost is the present value of the additional tuition a student must pay each year to attend a higher quality college.

Denote the post-graduation wage at time t by $w(t;q)$, where q is college quality. Assume students work forever, can borrow at their discount rate r, and take c years to complete college. Ignore any effort or other non-pecuniary costs or benefits to students of attending a higher quality college, and denote the difference in the post-graduation wage associated with a difference in quality of Δq by $\Delta w(t) = w(t;q_1) - w(t;q_0)$. To simplify matters, we assume $\Delta w(t)$ is constant over time and therefore equal to the increment produced in the first wage received after college, $w(c)[\exp\{b\Delta q\} - 1]$, where b is our log wage regression estimate and $w(c)$ is the average starting salary after graduation from college given in U.S. Bureau of the Census (1994). We denote this constant, persistent wage effect by Δw. To the extent that college quality increases wage growth we underestimate the value of quality; if quality effects diminish over time we overestimate its value. Discounting reduces the importance of either of these sources of bias.

The dollar price of quality is ΔT per year in added tuition. Additional quality is worth buying if

$$(5) \qquad \Delta w \sum_{i=c}^{\infty} (1+r)^{-i} \geq \Delta T \sum_{i=0}^{c-1} (1+r)^{-i},$$

$$\Delta w \geq \Delta T[(1+r)^c - 1] = \overline{\Delta w}.$$

To evaluate this expression, we need an estimate of the price of quality, ΔT. We estimated this price by regressing tuition on our quality index and controls not used in the construction of the quality index that we hope are correlated with amenities or with the costs of providing a college education. These controls are quartics in size, fraction black, and fraction female. We also included the private college indicator and interaction terms between this indicator and all other variables in the regression. We performed the regressions on the set of all four-year colleges for which we had information. We experimented with several functional forms, each differing by the way quality entered the regression. Based on R^2's and inspection of residuals we chose to include higher-order quality terms up to the fourth power. This functional form fits the data quite well.

Table 5 reports estimates from this admittedly *ad hoc* hedonic price regression; Figure 1 graphs the implied tuition-quality relationship for private colleges. Table 6 uses these price estimates to derive the break-even earnings effect for private colleges under different assumptions about the interest rate and time to complete a college degree. Table 6 also reports the earnings effects implied by our earlier quality estimates for three different increments in college quality: moving from a college at the 25 percentile of the quality distribution to one at the median, moving from the median to the 75th percentile, and moving from the median to the 95th percentile. The earnings effects are those implied for someone earning $24,305, the beginning salary offered Bachelor's degree recipients in Business in 1992 (U.S. Bureau of the

Census, 1994; Table 289).[22] The estimates are consistent with one another: the present value of the increase in wages from attending a college of greater quality is about the same as the present value of the tuition increases associated with the extra quality.[23]

CONCLUSION

College quality increases the wages of young men. Regardless of what controls we use or how we measure quality, there is a positive and significant relationship between wages and college quality. Our results are based on a representative sample of young men that includes detailed information on ability, labor market experience, family background, and high school quality. Because of the extensive controls that we have used and the stability of the parameters we estimated, it is doubtful that selection bias could explain this robust finding. We find that about one-third of the unconditional effect of college quality on earnings arises from the sorting of more able students to higher quality colleges. According to our estimates conditional on ability, men who attend colleges in the top fifth of the quality distribution earn wages about 13 percent higher than otherwise identical men who attend colleges in the bottom fifth of the quality distribution. In addition, we find that the returns to college quality are significantly higher for black men than for non-black men. Our estimates of the quality effect for all men are consistent with our estimate of the price of quality.

These estimates are in striking contrast to the primary and secondary school literature, where the evidence that easily quantifiable school characteristics matter for later productivity is

[22] Evaluation at the average beginning salary for Humanities graduates ($22,941) does not greatly change the estimates.

[23] Of course, to the extent that colleges discount with scholarships and other financial aid, our estimates of the net benefits of college quality are lower bound estimates.

at best mixed. One explanation may be a greater dispersion in resources and other characteristics among colleges compared to primary and secondary schools. It may also be evidence of the lack of competition within the largely public high school system. Public colleges face a more competitive environment than do public high schools, and therefore may deploy resources in ways more beneficial to students, with the result that variation in college characteristics more closely matches variation in quality.

Perhaps our most intriguing results are those suggesting the possibility that moderate levels of racial diversity among a college's students increase later earnings. This may be indirect evidence of the productive benefit of social or cultural capital. Unfortunately, our ability to explore this phenomenon is limited by the large differences in our sample between the racial makeup of colleges attended by blacks and those attended by non-blacks.

Table 1: Wages Regressed on College Characteristics, Separate Equation for Each Characteristic

(heteroskedasticity-consistent standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)
Ability controls	No	Yes	Yes	Yes	Yes
Basic controls	No	No	Yes	Yes	Yes
College major controls	No	No	No	Yes	Yes
Industry controls	No	No	No	No	Yes
College characteristics	(N=3,100)	(N=3,100)	(N=2,834)	(N=2,834)	(N=2,834)
Tuition	.018**	.015**	.032	.033	.039
	(.006)	(.006)	(.055)	(.055)	(.053)
Spending per student (in $1,000)	.011**	.009**	.055**	.059**	.058**
	(.002)	(.002)	(.018)	(.018)	(.017)
Faculty-to-student ratio	3.34**	3.02**	1.92**	1.97**	1.96**
	(.842)	(.847)	(.794)	(.739)	(.708)
Size (in 10,000's)	.039**	.030**	.018	.019	.024*
	(.014)	(.014)	(.013)	(.013)	(.012)
Rejection rate (ratio)	.440**	.438**	.249**	.258**	.257**
	(.123)	(.122)	(.116)	(.108)	(.103)
First year retention rate (ratio)	.770**	.645**	.401**	.401**	.388**
	(.135)	(.136)	(.127)	(.125)	(.121)
Graduation rate (ratio)	.484**	.388**	.222**	.237**	.254**
	(.115)	(.116)	(.110)	(.108)	(.105)
Top 10% of high school class (ratio)	.437**	.359**	.281**	.267**	.260**
	(.120)	(.120)	(.116)	(.116)	(.111)
Average SAT score (100's)	.075**	.063**	.034**	.035**	.036**
	(.013)	(.013)	(.013)	(.012)	(.012)
25th percentile SAT score (100's)	.060**	.052**	.034**	.034**	.034**
	(.017)	(.017)	(.016)	(.016)	(.015)
75th percentile SAT score (100's)	.064**	.056**	.030*	.029*	.031**
	(.017)	(.017)	(.016)	(.016)	(.016)
Faculty with Ph.D.'s (ratio)	.513**	.434**	.289**	.308**	.289**
	(.108)	(.108)	(.099)	(.100)	(.097)

Table 1 (continued): Wages Regressed on College Characteristics, Separate Equation for Each Characteristic

Note: ** indicates significance at the 5% level; * indicates significance at the 10% level. Standard errors are estimated by White's (1980) heteroskedasticity-consistent method. The dependent variable is the natural log of the real wage for the year ending at the 1987 interview. "Ability controls" are the first two principal components of respondents' age-adjusted ASVAB scores and these two variables squared. All regressions include a constant, a variable indicating whether the respondent has any postsecondary schooling, and a variable indicating when the college characteristic was unavailable. Basic controls include a constant, quartics in four variables (age, tenure, pre-college graduation labor market experience, and post-graduation experience), controls for race, geographical region, urban residence, any postsecondary school completed, years of school completed, years of postsecondary school completed, receipt of a Bachelor's degree, and the ability controls described above. "College major controls" are the 24 basic college majors described in Appendix A; "Industry controls" are the 15 industry indicators described in Appendix A; union status is included whenever industry controls are. Missing values were set to zero, and a dummy variable was included to indicate missing values.

Table 2: Wages and College Quality Index

(heteroskedasticity-consistent standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
Ability controls	yes	yes	yes	yes	yes	yes
Basic controls	yes	yes	yes	yes	yes	yes
College major controls	no	yes	yes	yes	yes	yes
Industry controls	no	no	yes	yes	yes	yes
Home controls	no	no	no	yes	yes	yes
Parent controls	no	no	no	no	yes	yes
High school controls	no	no	no	no	no	yes
College quality index	.063**	.067**	.067**	.067**	.063**	.063**
	(.021)	(.021)	(.020)	(.020)	(.020)	(.021)
Quintiles of quality index						
2nd quintile	.005	.007	.016	.016	.014	.005
	(.049)	(.048)	(.045)	(.045)	(.045)	(.045)
3rd quintile	.090*	.100*	.087*	.086*	.082*	.078
	(.052)	(.052)	(.048)	(.048)	(.048)	(.048)
4th quintile	.096*	.104**	.124**	.122**	.119**	.113**
	(.052)	(.051)	(.049)	(.049)	(.049)	(.049)
5th quintile	.124**	.136**	.144**	.141**	.0133**	.131**
	(.058)	(.057)	(.053)	(.053)	(.052)	(.052)

Note: ** indicates significance at the 5% level; * indicates significance at the 10% level. Standard errors are estimated by White's (1980) heteroskedasticity-consistent method. The dependent variable is the natural log of real wage for the year ending at the 1987 interview. "Ability controls" are the first two principal components of respondents' age-adjusted ASVAB scores, and these two variables squared. All regressions include a constant, a variable indicating whether the respondent has any post-secondary schooling, and a variable indicating when the college characteristic was unavailable. Basic controls include a constant, quartics in four variables (age, tenure, pre-college graduation labor market experience, and post-graduation experience), controls for race, geographical region, urban residence, any post-secondary school completed, years of school completed, years of post-secondary school completed, receipt of a Bachelor's degree, and the ability controls described above. "College major controls" are the 24 basic college majors described in Appendix A; "Industry controls" are the 15 industry indicators described in Appendix A; union status is included whenever industry controls are. Missing values were set to zero, and a dummy variable was included to indicate missing values. Home, parent, and high school controls are described in Table A3 in Appendix A.

Table 3: Wages and Gender and Racial Composition of Student Body

(heteroskedasticity-consistent standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ability controls	yes	yes	yes	yes	yes	yes	yes
Basic controls	yes	yes	yes	yes	yes	yes	yes
College major controls	no	yes	yes	yes	yes	yes	yes
Industry controls	no	no	yes	yes	yes	yes	no
Home controls	no	no	no	yes	yes	yes	yes
Parent controls	no	no	no	no	yes	yes	yes
High school controls	no	no	no	no	no	yes	yes
Gender:							
Fraction female	-.317**	-.315**	-.341**	-.331**	-.306**	-.325**	-.300**
	(.123)	(.128)	(.128)	(.127)	(.126)	(.127)	(.129)
Fraction female	-.196	-.189	-.218*	-.207*	-.191	-.213*	-.182
	(.123)	(.124)	(.123)	(.122)	(.121)	(.122)	(.125)
College quality index	.057**	.061**	.060**	.060**	.057**	.056**	.058**
	(.022)	(.021)	(.020)	(.020)	(.020)	(.020)	(.021)
Race:							
Fraction black	.431**	.402*	.305	.304	.357*	.346	.443**
	(.213)	(.210)	(.216)	(.216)	(.215)	(.214)	(.206)
Fraction black interacted	-.533**	-.506**	-.427*	-.431*	-.491**	-.475**	-.556**
with black indicator	(.228)	(.226)	(.231)	(.231)	(.230)	(.230)	(.222)
Fraction black	.498**	.468**	.379*	.376*	.429**	.422**	.514**
	(.218)	(.214)	(.217)	(.218)	(.216)	(.214)	(.209)
Fraction black interacted	-.542**	-.517**	-.440*	-.444*	-.502**	-.487**	-.566**
with black indicator	(.229)	(.227)	(.231)	(.230)	(.229)	(.228)	(.222)
College quality index	.068**	.071**	.070**	.069**	.066**	.066**	.069**
	(.022)	(.021)	(.021)	(.021)	(.020)	(.020)	(.021)
Fraction black	.497**	.468**	.380*	.376*	.431**	.424**	.517**
	(.218)	(.214)	(.217)	(.217)	(.215)	(.214)	(.209)
Fraction black interacted	-.483**	-.454*	-.386*	-.389*	-.444*	-.431*	-.501**
with black indicator	(.233)	(.231)	(.234)	(.234)	(.232)	(.231)	(.226)
College quality index	.050**	.052**	.053**	.053**	.048**	.048**	.048**
	(.024)	(.024)	(.023)	(.023)	(.022)	(.023)	(.024)
College quality index	.099**	.109**	.095*	.095*	.103*	.101*	.117**
interacted with black indicator	(.057)	(.055)	(.056)	(.056)	(.057)	(.056)	(.055)

Note: See note to Table 2.

Table 4: Wages and Racial Composition of Student Body

(heteroskedasticity-consistent standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ability controls	yes	yes	yes	yes	yes	yes	yes
Basic controls	yes	yes	yes	yes	yes	yes	yes
College major controls	no	yes	yes	yes	yes	yes	yes
Industry controls	no	no	yes	yes	yes	yes	no
Home controls	no	no	no	yes	yes	yes	yes
Parent controls	no	no	no	no	yes	yes	yes
High school controls	no	no	no	no	no	yes	yes
Fraction black categories							
5% to 7%	.110**	.114**	.108**	.107**	.108**	.101**	.107**
	(.042)	(.042)	(.039)	(.039)	(.040)	(.040)	(.042)
8% to 17%	.151**	.156**	.170**	.169**	.179**	.172**	.158**
	(.045)	(.044)	(.043)	(.043)	(.043)	(.043)	(.044)
greater than 17%	.094	.089	.068	.070	.077	.072	.095*
	(.052)	(.050)	(.048)	(.048)	(.048)	(.049)	(.051)
College quality index	.060**	.064**	.062**	.062**	.059**	.059**	.062**
	(.022)	(.021)	(.020)	(.020)	(.020)	(.021)	(.021)
Fraction black categories nteracted with black indicator							
5% to 7%	.098**	.103**	.102**	.101**	.101**	.093**	.093**
	(.046)	(.045)	(.043)	(.043)	(.043)	(.043)	(.046)
8% to 17%	.165**	.172**	.188**	.187**	.198**	.194**	.179**
	(.051)	(.050)	(.048)	(.048)	(.048)	(.048)	(.050)
greater than 17%	.150**	.130**	.102	.102	.109*	.104	.134**
	(.063)	(.064)	(.065)	(.066)	(.066)	(.068)	(.067)
Interaction terms							
5% to 7% interacted with black indicator	.101	.104	.067	.068	.075	.079	.114
	(.086)	(.086)	(.082)	(.083)	(.084)	(.086)	(.087)
8% to 17% interacted with black indicator	-.050	-.048	-.059	-.061	-.064	-.070	-.063
	(.084)	(.086)	(.086)	(.086)	(.086)	(.086)	(.086)
greater than 17% interacted with black indicator	.026	.067	.073	.072	.072	.074	.066
	(.101)	(.100)	(.100)	(.100)	(.099)	(.100)	(.100)
College quality index	.039	.039	.040*	.040*	.035	.035	.036
	(.024)	(.024)	(.024)	(.023)	(.022)	(.022)	(.024)
College quality index interacted with black indicator	.134**	.153**	.145**	.144**	.151**	.150**	.161**
	(.062)	(.061)	(.062)	(.062)	(.062)	(.061)	(.060)
F-test for black categories are the same across races (p-value)	.58	.48	.60	.62	.57	.51	.39

Note: See note to Table 2.

Table 5: The Price of College Quality

(standard errors in parentheses)

Dependent Variable: Tuition

Regressors:	(1)
Quality index	-4,442**
	(1,196)
Square of quality index	3,903**
	(580.9)
Cube of quality index	-752.0**
	(111.7)
Quartic of quality index	43.4**
	(7.17)
Public	-6,423**
	(1,790)
Public × quality index	2,133
	(2,260)
Public × square of quality index	-1,595
	(1,517)
Public × cube of quality index	25.2
	(421.0)
Public × quartic of quality index	33.3
	(39.8)
N	1,220
R^2 (adjusted)	0.84

Note: The dependent variable is tuition (in-state tuition for public schools). The quality index is normalized so that its minimum value is zero. The regression is estimated using data on the full set of four-year colleges for which we have information. In addition to the variables shown, the regression includes a quartic in size, proportion female, and the fraction black, and interactions between each of these variables and a public school indicator. Adding the quality variables increases the adjusted R^2 by about 0.18. Using only the quality variables, the indicator for public school, and public-quality interactions in the regression results in an adjusted R^2 of about 0.81.

Figure 1: Tuition and College Quality
Relationship for Private Colleges Implied by Estimates in Table 5

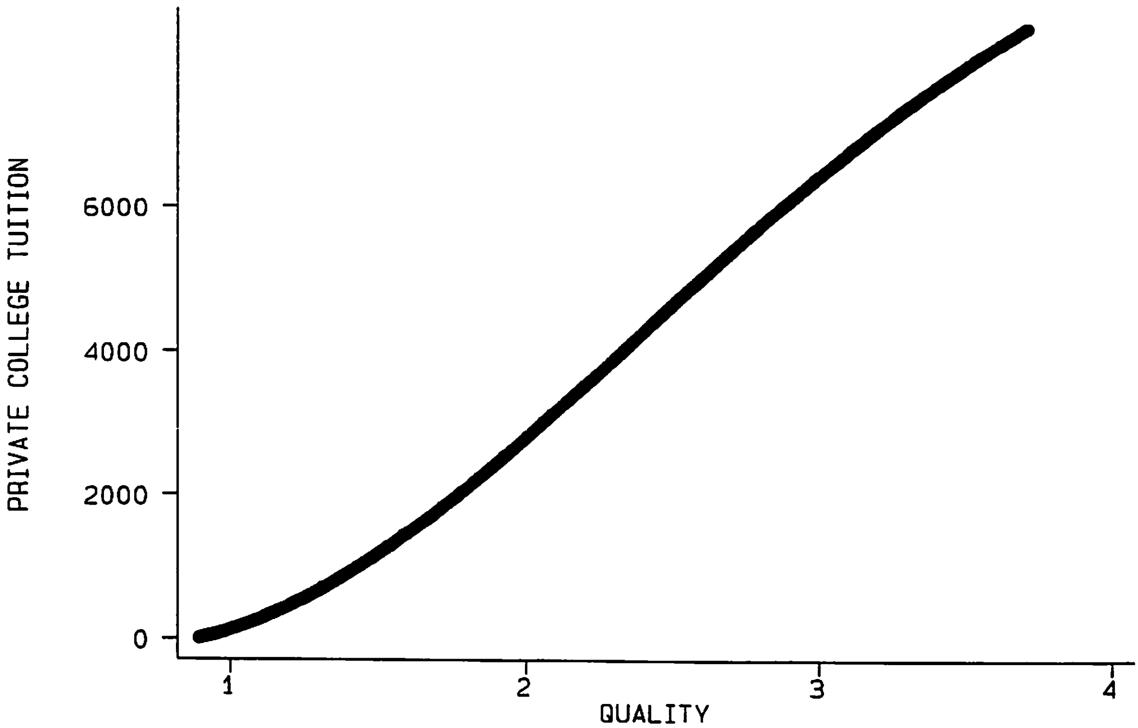

Table 6: Are the Quality Estimates Plausible?

	Break-even	Estimated gains with lower return to quality	Estimated gains with higher return to quality
Graduate in 4 years. r = .1			
25th percentile to median	\$591	\$598	\$636
Median to 75th percentile	\$907	\$796	\$847
Median to 95th percentile	\$2,801	\$2,684	\$2,855
Graduate in 4 years, r = .06			
25th percentile to median	\$334	\$598	\$636
Median to 75th percentile	\$513	\$796	\$847
Median to 95th percentile	\$1,584	\$2,684	\$2,855
Graduate in 5 years, r = .1			
25th percentile to median	\$778	\$598	\$636
Median to 75th percentile	\$1,193	\$796	\$847
Median to 95th percentile	\$3,685	\$2,684	\$2,855
Graduate in 5 years, r = .06			
25th percentile to median	\$441	\$598	\$636
Median to 75th percentile	\$661	\$796	\$847
Median to 95th percentile	\$2,042	\$2,684	\$2,855
Estimated return to quality	---	.063	.067

Note: See Equation (5) of the text. The break even point in the first column is based on the estimated costs of quality at private colleges in Table 5. The quality coefficients are taken from columns (1) and (2) of Table 2. They are evaluated at \$23,529, the average beginning salary offered to a Bachelor's degree recipient in Business in 1990 (U.S. Bureau of the Census, 1994: Table 289). The 25th, 50th, 75th, and 95th percentiles for the normalized quality are 1.13, 1.69, 2.17, and 3.40, respectively.

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Appendix A

Detailed Data Description

This appendix describes our data in more detail. Table A1 describes the basic set of regressors included in the log wage regressions. Table A2 reports the results of the principal components analysis used to create the ability controls. Our ability controls were created in two steps. First, we created age-adjusted ASVAB scores by regressing each of the ten ASVAB scores for each individual on age dummy variables and an indicator of whether or not the respondent had completed high school when he took the ASVAB. The residuals from these regressions are the age-adjusted scores. These are the data for the principal components analysis. The first two principal components of the age-adjusted scores are the ability controls used in the wage regressions throughout the paper.

Table A3 describes the family and other background variables included as controls in some of the regressions. Table A4 describes the college characteristics we use as indicators of quality. Table A5 reports descriptive statistics for the college characteristics. Table A6 lists and describes our two sets of college major controls.

APPENDIX B

Regressions with Multiple College Quality Indicators

In Panel A of Table A7, we simultaneously include the 12 measures of college quality that we used in Table 1 in our wage regression. While the coefficient on the Faculty-Student Ratio is always significant at better than the 5 percent level, we cannot reject joint hypotheses that 12 coefficients are zero. Moreover, numerous coefficients are negative, suggesting that increases in one of these measures of college quality may actually lower wages, conditional on all of the others. In contrast, in Panel B of Table A7, we simultaneously include the 3 measures of college quality that we use in the construction of our college quality index. While none of the coefficients are individually significant at the 5 percent level, the coefficients are always jointly significant at better than the 2 percent level. This improved performance is not a function of our picking the three college quality measures strategically: in Panel A, 2 of the 3 measures we use to construct our college quality measure have negative parameter estimates.

APPENDIX C

Some Specification Checks

In this appendix, we examine in greater detail how college quality affects male wages. Specifically, we consider whether college quality is a complement of or substitutes for on-the-job human capital accumulation. For example, higher quality colleges might provide students with a superior ability to accumulate human capital. If so, wage growth should be greater for people who attend higher quality colleges. In the main text, we assume this is not the case. The estimates we report here suggest that this assumption is justified.

All of the estimates we have reported are from single period regressions of the form

(A1) $\ln W_t = \gamma Q + Z\Gamma + X_t\beta + e_t,$

where Z includes all non-time-varying controls except quality (Q), and X_t includes all time-varying controls. In differences, this becomes

(A2) $\Delta \ln W_t = \Delta X_t\beta + \Delta e_t.$

We can write our alternative hypothesis, that college quality affects the ability to accumulate human capital, so that it includes equation (A1) as a special case:

(A3) $\ln W_t = \gamma Q + \delta(tQ) + Z\Gamma + X_t\beta + e_t,$

where t is tenure or post-college experience. For someone employed steadily between periods, in differences this becomes

(A4) $\Delta \ln W_t = \delta Q + \Delta X_t\beta + \Delta e_t.$

Equation (A4) nests equation (A2), so there are two ways to test whether equation (A1) or (A3) is correct. We can estimate equation (A3) directly or we can estimate equation (A4). In either case, our alternative hypothesis suggests we should reject the restriction that $\delta = 0$. If we have specified the functional form correctly and there is no relevant unobserved heterogeneity, then the two tests are equivalent. Equation (A4) can be estimated using fixed effect methods. If the functional form is correct only up to fixed effects, then the two strategies might produce different estimates and equation (A4), but not equation (A3), will produce an unbiased estimate of δ.

Whether we estimate equation (A3) or (A4), we cannot reject the null hypothesis represented by equation (A1). Table A8 reports estimates of equations (A3) (top panel) and (A4) (bottom panel). There is no evidence from either set of regressions that college quality is related to the speed with which people later accumulate human capital. We repeated the analysis with our ability controls present to allow for the possibility that higher ability workers might accumulate human capital at different rates than lower ability workers. Including these controls had no effect on the qualitative results. This is contrary to the findings of Sweetman (1994) and Solmon (1975) who found that college quality was positively related to wage growth.

We have not interacted college quality with any measures of the duration or intensity of students' college experiences, although such interactions are certainly plausible. To test for interactions between college quality and the duration or intensity of the college experience, we interacted our quality measure with years of schooling beyond high school and receipt of a 4-year degree. The results of this exercise appear in Table A9. F-tests fail to reject the simpler specification without quality interaction terms at the 15 percent level. Interestingly, Solmon (1975) found no relationship between the earnings of his World War II veterans and how long they had attended the college conditional on college quality.

Table A1: Regressors for Log Wage Regressions

log wage	Log of average real wage (1982 dollars) on all jobs held during the year 1987 or 1989, as indicated
west, south, northeast	dummy variables indicating region of residence at the 1987 or 1989 interview, as indicated
smsa	dummy variable indicating that respondent lived in an SMSA at the 1987 or 1989 interview, as indicated
union	indicates whether any job held during the year was covered by a collective bargaining agreement
age	respondent's age at the 1987 interview
experience	total months the respondent has been employed since age 16
tenure	total months the respondent has worked for the current employer
AA degree	dummy variable indicating the respondent has a 2-year college degree as of 1987 interview
BA degree	dummy variable indicating the respondent has a 4-year college degree as of 1987 interview (AA and BA are mutually exclusive; BA takes precedence over AA)
highest grade completed	highest grade or year of school the respondent completed as of the 1987 interview
black	dummy variable indicating the respondent is black
hispanic	dummy variable indicating the respondent is hispanic (black & hispanic are mutually exclusive)
AFQT score	Armed Forces Qualification Test, based on Armed Services Vocational Aptitude Battery, administered in 1980.
AFQT (not HS grad)	AFQT interacted with dummy variable indicating respondent had not completed high school when the AFQT was administered

Table A2: Construction of Age-Adjusted Ability Measure

Our ability measures are the first two principal components of ASVAB residuals

Component	Eigenvalue	Difference	Explained Proportion	Cumulative Explained
1	6.23577		0.6236	0.6236
2	1.23656	4.99921	0.1237	0.7472
3	0.58332	0.65324	0.0583	0.8056
4	0.48691	0.09641	0.0487	0.8543
5	0.31585	0.17106	0.0316	0.8858
6	0.29880	0.01784	0.0298	0.9156
7	0.23991	0.05809	0.0240	0.9396
8	0.23062	0.00929	0.0231	0.9627
9	0.19614	0.03448	0.0196	0.9823
10	0.17692	0.01922	0.0177	1.0000

Eigenvectors, 1st and 2nd Principal Components

	1st PC	2nd PC
general science residuals	0.34983	−0.15182
arithmetic reasoning residuals	0.34406	0.04834
word knowledge residuals	0.34782	0.04514
paragraph comprehension residuals	0.32261	0.15402
numerical operations residuals	0.27533	0.47801
coding speed residuals	0.24530	0.53074
auto and shop knowledge residuals	0.27982	−0.45859
mathematics knowledge residuals	0.32972	0.14300
mechanical comprehension residuals	0.32099	−0.32185
electrical information residuals	0.32887	−0.32301

Note: ASVAB scores are adjusted for age by regressing each test score on age dummy variables and a variable indicating whether the respondent had completed high school when the ASVAB was administered. Principal components analysis is performed on the OLS residuals from these regressions. In all wage regressions, "ability controls" are the first two principal components and their squares. See Heckman (1995) on using the first two principal components.

Table A3: Family Background & High School Controls

Home: magazine	"When you were about 14 years old, did you or anyone else living with you get magazines regularly?'
Home: newspaper	"When you were about 14 years old, did you or anyone else living with you get a newspaper regularly?'
Home: library card	"When you were about 14 years old, did you or anyone else living with you have a library card?"
Parents: mom education	Highest grade or year of school completed by respondent's mother.
Parents: mom living	Was the respondent's mother living at the 1979 interview (when respondents were between 14 and 22 years old)?
Parents: mom age	At the 1987 interview.
Parents: dad education	Highest grade or year of school completed by respondent's father.
Parents: dad living	Was the respondent's father living at the 1979 interview?
Parents: dad age	At the 1987 interview.
Parents: living together	Indicator for whether the respondent's mother and father lived in the same household at the 1979 interview.
Parents: mom occupation	Occupation of job held longest by mother or stepmother in 1978, represented by dummy variables for each Census 2-digit occupation.
Parents: dad occupation	Occupation of job held longest by father or stepfather in 1978, represented by dummy variables for each Census 2-digit occupation.
HS: Size	Asked of respondents' high schools: "As of 10/1/79 [or nearest date] what was [your] total enrollment?"
HS: books	Asked of respondents' high schools: "What is the approximate number of catalogued volumes in the school library (enter 0 if your school has no library)." [in 1979]
HS: teacher salary	Asked of respondents' high schools: "What is the first step on an annual salary contract schedule for a beginning certified teacher with a bachelor's degree?' [in 1979]
HS: disadvantaged	Asked of respondents' high schools: "What percentage of the students in [the respondent's high school] are classified as disadvantaged according to ESEA [or other] guidelines?" [in 1979]

Table A4: College Characteristics Definitions

tuition	1990. For public schools, tuition is that for in-state residents.
acceptance rate	Percent of applicants accepted, fall 1990.
spending per student	Educational and general expenditures per full-time equivalent student, 1990.
1st year retention (%)	Average percent of 1987-89 freshmen who enrolled as sophomores.
Graduation rate (%)	Average percent of 1983-85 freshmen who graduated within 5 years.
Faculty/student ratio	Based on full-time equivalent total faculty and students, fall 1990.
Size (# students)	1990.
% of faculty with Ph.D.	Percent of full-time faculty with doctorate or highest terminal degree.
% students black	Fall 1990.
% students female	1990
average SAT	Average SAT scores of fall 1990 freshmen.
25th percentile SAT	25th percentile of SAT scores of fall 1990 freshmen.
75th percentile SAT	75th percentile of SAT scores of fall 1990 freshmen.
HS top 10, HS top 25	Percent of fall 1990 freshmen who graduated in the top 10 or top 25 percent of their high school class.

Whenever a school quality measure appears, missing values are set to zero and a dummy variable indicating whether the quality measure is missing is included.

Table A5: College Characteristics for Men
(unweighted)

	mean	25th percentile	median	75th percentile	N
tuition	$3,600	$1,450	$1,900	$5,060	838
rejection rate (%)	25	14	23	33	829
spending per student	$12,290	$7,280	$9,690	$14,720	821
1st year retention (%)	74	67	75	83	819
graduation rate (%)	44	31	44	55	764
faculty/student ratio	.066	.052	.059	.071	834
size (# students)	13,140	3,890	10,160	18,534	886
private college or university	.31	•	•	•	887
% of faculty with Ph.D.	74	62	77	87	817
% students black	14	3	5	10	797
% students female	52	48	53	57	886
average SAT	921	835	900	1,007	790
25th percentile SAT	824	740	839	1,040	524
75th percentile SAT	1,077	960	1,075	1,170	526
HS top 10	27	12	21	34	574

Table A6: College Major Controls

Agriculture (24)
- agriculture

Architecture (11)
- architecture
- interior design
- misc. architecture

Area Studies (1)
- area studies

Biology (54)
- biochemistry
- biology
- microbiology
- pre-medicine
- zoology
- misc. biology

Business (282)
- accounting
- banking and finance
- business administration
- economics
- international business
- institutional management
- marketing
- misc. business

Communications (45)
- advertising
- communications
- journalism
- radio & TV
- misc. communications

Computer Science (41)
- computer science
- misc. computer science

Education (150)
- education

Engineering (104)
- aerospace engineering
- chemical engineering
- civil engineering
- electrical engineering
- engineering technologies
- general engineering
- industrial engineering
- mechanical engineering
- misc. engineering

Fine Arts (58)
- art
- commercial arts
- drama
- design
- fine arts
- music performing
- music liberal arts
- studio arts
- misc. fine arts

Foreign Languages (14)
- French
- Spanish
- misc. foreign language

Health Professions (159)
- nursing
- misc. health professions

Home Economics (16)
- home economics
- nutrition
- misc. home economics

Interdisciplinary Studies (66)
- general studies
- liberal arts, & other interdisciplinary studies
- misc. other

Law (11)
- law
- pre-law
- misc. law

Letters (25)
- English
- misc. letters

Mathematics (9)
- applied mathematics

mathematics
misc. mathematics

Military (1)
military science

Office Occupations (53)
data processing
secretarial studies

Physical science (31)
chemistry
geology
physics
misc. physical sciences

Psychology (35)
psychology
misc. psychology

Public Service (46)
law enforcement
social work
misc. public service

Social Science (66)
anthropology
history
political science
sociology
misc. social science

Theology (12)
theology

This table lists the two aggregations of college majors used. Respondents for whom there is no college major information have all of the indicator variables set to zero. Regressions that include college major controls also include a dummy variable indicating whether college major information was available for the respondent.

Detailed controls: The detailed measure recognizes 82 distinct college majors, listed above.

Basic controls: The 82 "detailed" college majors were aggregated into 24 areas. These are listed in boldface, with the number of respondents reporting a major in each of these categories given in parentheses.

Table A7: College Characteristics in Wage Regressions

(heteroskedasticity-consistent standard errors in parentheses)

(N = 2,834)	(1)	(2)	(3)	(4)
Family & high school controls	No	No	No	Yes
College major controls	No	Yes	Yes	Yes
Industry controls	No	No	Yes	Yes
Panel A:				
Tuition	-.009	-.010	-.009	-.011
	(.008)	(.008)	(.008)	(.008)
Spending per student (in $1,000)	-.034	-.022	-.021	-.013
	(.037)	(.024)	(.034)	(.034)
Faculty-to-student ratio	2.41**	2.29**	2.24**	2.29**
	(1.13)	(1.01)	(.989)	(.978)
Size (in 10,000's)	-.003	-.007	.003	.004
	(.017)	(.017)	(.016)	(.016)
Rejection rate (ratio)	.117	.140	.122	.129
	(.126)	(.119)	(.115)	(.115)
First-year retention rate (ratio)	.148	.111	.067	.114
	(.171)	(.171)	(.168)	(.171)
Graduation rate (ratio)	.008	.041	.108	.102
	(.170)	(.167)	(.165)	(.163)
Top 10% of high school class (ratio)	.093	.025	.014	-.013
	(.186)	(.183)	(.176)	(.173)
Average SAT score (100's)	-.869	-.503	-.168	-.664
	(2.22)	(2.24)	(2.15)	(2.16)
25th percentile SAT score (100's)	3.19	3.07	1.59	2.39
	(3.91)	(3.72)	(3.69)	(3.69)
75th percentile SAT score (100's)	-1.62	1.83	-.640	-1.00
	(4.04)	(3.86)	(3.77)	(3.69)
Faculty with Ph.D.'s (ratio)	.181	.221*	.144	.115
	(.124)	(.126)	(.125)	(.127)
p-value, test for joint significance of 12 quality measures	.23	.08	.16	.11
Panel B:				
Spending per student (in $1,000)	.030	.034	.032	.037*
	(.022)	(.211)	(.021)	(.020)
Rejection rate (ratio)	.111	.113	.105	.119
	(.124)	(.116)	(.111)	(.112)
Average SAT score (100's)	1.5	1.53	1.75	1.10
	(1.63)	(1.55)	(1.55)	(1.55)
p-value, test for joint significance of 3 quality measures	.02	.01	.01	.01

Note: See note for Table 2.

Table A8: College Quality and Wage Growth

(heteroskedasticity-consistent standard errors in parentheses)

(N = 2,452)	(1)	(2)	(3)	(4)	(5)
Family & high school controls	No	No	No	Yes	---
College major controls	No	Yes	Yes	Yes	---
Industry controls	No	No	Yes	Yes	---
Quality index	.053	.061	.067	.066	---
	(.43)	(.043)	(.041)	(.041)	
Quality × experience	-.099	-.144	-.243	-.317	-.559
(1,000)	(.584)	(.548)	(.521)	(.516)	(.693)
P-value. joint significance of quality and interaction	.09	.05	.03	.06	---
R^2 (adjusted	.29	.30	.35	.36	.05

Note: See note for Table 2.

Table A9: College Quality Interacted with Educational Attainment Variables

(heteroskedasticity-consistent standard errors in parentheses)

(N = 2,834)	(1)	(2)	(3)	(4)
Family & high school controls	No	No	No	Yes
College major controls	No	Yes	Yes	Yes
Industry controls	No	No	Yes	Yes
Quality index	.070	.067	.065	.062
	(.049)	(.050)	(.047)	(.047)
Quality index × years of post-secondary	.010	.010	.013	.013
education	(.017)	(.018)	(.018)	(.018)
Quality index × four-year degree receipt	-.070	-.060	-.071	-.075
	(.053)	(.054)	(.054)	(.053)
p-value, quality index and interactions	.01	.01	.01	.01
p-value, interaction terms only	.41	.53	.40	.36
R^2 (adjusted)	.26	.27	.34	.35

Note: See note for Table 2.