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The Impact of College Quality on Wages: Are There Differences Among Demographic Groups?

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Abstract

There has been a rising interest in understanding better the impact of college choices on wages that has been motivated by concerns about increasing wage inequalities, about increasing costs of elite colleges and about the perceived increasing roles of highly educated individuals in maintaining international competitiveness. Previous studies on the impact of college quality on wages generally have been subject to two limitations: (1) They have not considered the differential impact for different demographic groups identified by race and gender. (2) They have not considered the choice dimensions with regard to time in college and college quality (with two recent exceptions). This study addresses both of these limitations. Estimates are obtained of the impact of time in college and of college quality choices on wages for four demographic groups based on an explicit dynamic framework. These estimates suggest that treating time in college and college quality as choices within a dynamic framework affects fairly substantially the estimated effects. Based on these results, most previous studies may have overestimated the impact of both college quality and time in college because of the failure to deal with such choices and, in a lesser but still substantial number of cases, because of the failure to control for the quality dimensions of pre-college education. The estimates also suggest that there are important demographic differences. The estimated wage benefits from higher college quality and more time in college tend to be highest for nonwhite males and next for nonwhite females, then white females and least for white males. For some members of all groups there appear to be some incentives for increasing college quality (by paying somewhat higher tuitions or whatever), more so for those who are longer in college because of interactions between college quality and time in college. But these incentives differ strongly across groups, being much higher for nonwhite males and much lower for white males, with the females in between. For nonwhites (again stronger for males than females) there also appear to be incentives to increase time in college. For whites, in contrast, the estimated net gains do not appear to create such incentives at least at a 4% real discount rate -- a result that contrasts with common interpretations of semilog wage relations. The results for nonwhites raise important questions regarding why there is unrealized potential for reaping net benefits from more time in college or attending higher quality colleges. A number of possible answers to this question -- poor information about the impact of college, imperfect capital markets for financing college, poor information about college quality differentials, discriminatory admissions -- may have important policy implications on both equity and efficiency grounds.

College education is perceived to have important impact on wages, perhaps with differential effects of differential types of colleges. Nevertheless, questions frequently are posed such as: How important is the quality of one's college peers for learning of the type that is rewarded in the labor market? How important are college resources per student? To what extent are positive associations between measures of college quality and subsequent labor market outcomes the effects of college per se versus the effects of pre-college education and ability? Are there incentives for individuals to improve their education by attending higher quality colleges, attending college longer, or both? Are there significant differences in these effects for females versus males? For whites versus nonwhites?

Social scientists long have attempted to estimate what some of these effects are in order to better inform choices of prospective students and their families, college administrators, and policymakers and to judge the fairness of the distribution of educational opportunities among various demographic groups. Interest in such estimates has increased for at least four reasons, the first two of which are not entirely consistent. First, individuals with a college education in the United States have been receiving increased wages relative to secondary school graduates (e.g., Murphy and Welch 1989, Bound and Johnson 1992, Katz and Murphy 1992, Levy and Murnane 1992, Mincer 199 1). This perceived increased premium for college has important implications for the returns to investing in college education and for the distribution of income. Second, the relative price of college has increased relatively rapidly in comparison with the cost of living since the early 1980s which has raised concern in some quarters about whether college education -- particularly at elite colleges -- is a worthwhile investment (e.g., McPherson and Schapiro 1991, Brewer, Eide and Ehrenberg 1996). Good estimates of the wage impact of colleges with differentiation by their characteristics are an important component of a good evaluation of the benefits of different types of colleges. Third, the perception has increased that higher levels of education are likely to be ever more important for individuals, demographic groups and whole societies to adapt to more rapidly changing technologies and markets in an increasingly integrated and competitive world economy (e.g., Goldberger and Manski 1995, Herrnstein and Murray 1994, Preston and Campbell 1993). Fourth, controversy has increased about the appropriate means, if any, for addressing educational and labor markets outcomes that differ by gender or by race. Affirmative actions programs that have been in effect for the last quarter century are increasingly under legal and political challenges. Better knowledge of the impacts of different types of colleges on different demographic groups would inform these debates.

As a result of the increased interest in such issues there have been an increasing number of efforts to estimate the wage impact of going to college and a smaller, but growing number of efforts, to estimate the wage impact of college quality. Many of these studies motivate their estimates by appealing to standard human capital investment models (e.g., Becker 1967, Ben-Porath 1967) with wages determined by three main inputs: the human capital endowed to individuals, the time spent by the individual in human capital investment such as in college, and other inputs that are purchased by the individual, provided by the family or publicly allocated such as the quality of college. However, for addressing the above questions there are two serious

limitations in most of the literature.

First, most studies of the impact of college quality on wages focus exclusively on one demographic group (usually white males, though there are exceptions such as Datcher Loury and Garman 1995) or treat demographic differences only by including additive controls (dummy variables) despite other evidence that educational and labor market options may differ substantially for females versus males and for whites versus nonwhites. Therefore relatively little is known about the possible differential impact of college quality choices on outcomes of different demographic groups.

Second, most empirical studies of the wage impact of college characteristics ignore the choice aspects of college education -- that individuals or their families choose how much time is spent in college and the quality of college -- by treating the time spent in college and the quality of the college as predetermined in a statistical sense (e.g., Daniel, Black and Smith 1995, Datcher Loury and Garman 1995, Fox 1993, Griffin and Alexander 1978, James and Alsalam 1993, James, Alsalam, Conary and To 1989, Kingston and Smart 1990, Morgan and Duncan 1979, Reed and Miller 1970, Solomon 1973, 1975, Solomon and Wachtel 1975, Wachtel 1975, Wales 1973, Weisbrod and Karpoff 1968, Wise 1975). This treatment ignores the possibility that college attendance and type of colleges attended may be in response to unmeasured (in the data used for the analysis) individual endowments such as abilities and motivation. If individuals with high endowments are more likely to attend college and more likely to attend better colleges, then estimates of the effects of college attendance and of college qualities on wages can be misleading. We are aware of only two (more-or-less concurrent) studies that attempt to deal with the college attendance and college characteristics choices in estimating the impact of college on wages.

(1) Behrman, Rosenzweig and Taubman (1996, hereafter BRT) assess the impact of college quality on white women's earnings and the influence of family and individual endowments on college choice using data from a survey of identical and non-identical twins born in Minnesota. The identical twins data permit control for genetic endowments in estimates of the impact of college attendance and college characteristics on wages. The fraternal twins data permit investigation of whether individual endowments affect time spent in college and the characteristics of college attended. The estimates reject models that ignore school choice. The statistically-preferred estimates suggest that Ph.D.-granting, private universities with well-paid senior faculty produce students who have significantly higher earnings later in life. Both the quantity of schooling and the quality of schooling resources are allocated to higher-endowed individuals, which exacerbates pre-existing inequality in human capital and biases conventional estimates of college quality effects.

(2) Brewer and Ehrenberg (1996, hereafter BE) and Brewer, Eide and Ehrenberg (1996, hereafter BEE) examine the impact of choices regarding the selectivity of colleges attended on wages for white and nonwhite females and males combined who attended

four-year colleges in the National Longitudinal Study of the High School Class of 1972 (NLSHS72) and High School and Beyond (HSB) Samples. They first estimate multinomial logits for attendance at "top," "middle," or "bottom" private or public colleges (based on Barron's classification of colleges primarily on the selectivity of the admissions process) that depend on individual and family background characteristics and "net college costs" (estimated on the basis of individual and family background characteristics including residence).[1] They then use these logit estimates to control for selectivity regarding type of college attended in estimates of wage relations as functions of individual and family characteristics for subsamples for each college category. In their estimates they generally control for gender and race with additive dummy variables. They interpret there results to imply that, even after controlling for such selectivity, "there is strong evidence of significant economic return to attending an elite private institution, and some evidence that this premium has increased over time" (BEE, abstract).

In this paper we use the NLSHS72 to investigate the impact of both college attendance and college characteristics on wages of individuals aged 30-32, by which age for most individuals initial job search/matching has been completed. We allow for differences in all of the relations for females versus males and, within gender groups, between whites and nonwhites in contrast to both BRT (who consider only white females) and BE/BEE (who assume that all gender and race effects are additive in their wage relations). We explicitly treat whether one attends college and, conditional on going to college, the characteristics of the college attended as choices in contrast to all the studies cited above except BRT and BE/BEE. We use a national sample as do BE/BEE (in fact we use one of the samples that they use), but not BRT. We consider the full sample of high school seniors in addition to the selected subsample of those who attended four-year that BE/BEE analyze. We use measures of college quality that relate both to the quality of the student body and to college instructional resources per student (most studies use one or the other, e.g., BRT focus on the former, BE/BEE only the latter). Finally we consider whether there are nonlinearities not only among groups of colleges (as BE/BEE consider, but not BRT) but also different marginal effects within such groups (in contrast to BE/BEE).

In Section 1 we summarize alternative approaches to treating college characteristics as reflecting behavioral choices and indicate the assumptions in the alternatives that we estimate in this study. In Section 2 we discuss the data that we use, including the representations of school quality. In Section 3 we present our preferred estimates for females and males, with disaggregation between whites and nonwhites, in the NLSHS72. We find significant effects of college quality and time in college, with interactions between the two and with substantial differences among the four demographic groups that we consider (with the greatest gains from improving college quality and increasing time in college for nonwhite males and the least for white males). In Section 4 we consider alternative estimates that suggest that the procedures used in previous studies may cause substantial biases in estimates of the impact of college quality on

1 As we discuss in Section 1, this treatment of selectivity seems to depend on functional form, which is problematic.

wages. Section 5 presents our conclusions.

Section 1. Modeling College Choices

Interpretation of empirical associations in behavioral data always is conditional on some underlying model (though the model is not always explicit). In this section we discuss first our preferred choice for modeling and estimating the impact of college quality choices on wages. To our knowledge this approach has not been used before in studies of the impact of college quality choices on wages. Then we turn to some alternatives that have been used in the literature and which we explore in alternative estimates. We estimate below the various relations discussed in this section for females and males and, within gender groups, for whites and nonwhites separately. Because these demographic characteristics are determined exogenously rather than as choices, the use of these subsamples does not add any complications for our modeling.

Preferred approach instrumental variables estimates of endogenous human capital investment choices: We assume that the choices of time and quality of college and labor market experience are outcomes of the constrained maximization of an objective function (utility function) subject to budget, market, and production function constraints given pre-college educational attainment and ability and motivation endowments within a dynamic framework. A critical production function in this process is the one that relates wages to human capital investments such as time in college, college characteristics ("quality"), pre-college educational attainment, work experience, endowments related to ability and motivation, and labor market luck.[2]

$$(1) \qquad W_i = W(S_i, Q_i, X_i, T_i, E_i, e_i)$$

where W_i = hourly wage rate;
S_i = time in college;
Q_i = quality of college;
T_i = pre-college education;
X_i = work experience;
E_i = endowments; and
e_i = random wage shock, inclusive of measurement error in wages.

In our estimates below we specify the wage production function as semilog both because wages seem to be distributed lognormally and because that is the form used in most of the previous

2 Wages are observed only for those who elect to participate in the wage labor force and who are employed so that they receive wages. A priori this would seem to be a selected sample, and there is a considerable and well-known literature on the possibility of labor force participation selectivity. Our initial explorations, however, did not reveal evidence that labor force participation selectivity affects our estimates significantly or substantially (BE/BEE report similar results) so, for simplicity, we ignore the possibility of labor force participation selectivity in this paper.

literature.[3] However we interact time in college with quality of college because a *priori* from a human capital perspective it would seem that attending college of zero quality or attending zero years of college of positive quality both would have no impact on subsequent wages (e.g., Behrman and Birdsall 1983).[4]

Pre-college educational attainment reflects earlier maximization within a dynamic framework given predetermined variables:

(2) $T_i = T(P_i, A_i, E_i u_i)$,

where P_i = relevant actual and expected prices;[5]
A_i = relevant individual or household assets;[6] and
u_i = random shocks in pre-college human capital investments, including random measurement errors in the indicator of pre-college human capital investments.

The maximization process at the time of the termination of secondary schooling leads to dynamic decision rules for investing in Si, Qi and Xi also as a function of predetermined variables (perhaps working through in part through pre-college education, college choices and work experience choices -- but that do not enter directly into the wage production function except for endowments):'

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3 Often semilog wage function estimates are rationalized by appeal to Mincer (1974). However the Mincer formulation leads to a semilog equilibrium relation between wages and schooling under strong assumptions about the underlying wage production function and the underlying human capital investment process (i.e., that human capital investments are independent of endowments) and the interpretation of wage production function estimates as if they are Mincerian equilibrium relations may be quite misleading (see Section 3 for an illustration).

4 A more general formulation would be to use a higher order polynomial approximation. Because of the high multicollinearity among the instrumented versions of such terms (on the order of .96-.99), the estimates have considerable imprecision and are very difficult to interpret.

5 These expected prices include those for post-secondary education because within a dynamic investment process if pre-college human capital is complementary to post-secondary human capital investments as usually is presumed, such prices affect time and other resources devoted to pre-college education.

6 These may include financial assets (or perhaps income) if capital markets are imperfect for human capital investments as generally is assumed to be the case, as well as the predetermined educational stock of household members (particularly parents).

7 During the investment process there may be some realizations of prices different from those that are expected at the initiation of the investment because, for example, of technological or policy changes that alter the market returns to education.. If so, these price "shocks" also will affect the final investments.

(3) $Q_i = Q(P_i, A_i, E_i, V_i)$,
$S_i = S(P_i, A_i, E_i, W_i)$, and
$X_i = X(P_i, A_i, E_i, X_i)$,

where V_i, W_i, X_i = random shocks in respective dynamic decision rules for human capital investments, including measurement errors in the human capital indicators.

Conditional on the assumption that the relations l-3 are specified correctly, the predetermined variables (e.g., P_i and A_i in relations 2 and 3) (a) determine S_i, Q_i, T_i, and X_i and (b) are independent of the compound disturbance term in the wage relation (including E_i). Under these assumptions, such estimates are consistent -- not contaminated either by unobserved components of endowments or by random measurement error.

Alternative 1. Direct estimation of a wage production function with ordinary least squares (OLS): The dominant alternative in the previous literature is to estimate a relation similar to relation 1 with OLS. If the wage function is correctly specified, all of the right-side variables are observed without error, all of the observed right-side variables are independent of the disturbance term, and the disturbance term is distributed normally and iid, then the estimates obtained are unbiased.

These assumptions would seem to be violated often in several important respects:

(1) Endowments generally are not measured or are measured imperfectly[8], so they are included (at least in part) in a compound disturbance term in addition to e_i. As a result, the observed human capital investments variables (S_i, Q_i, X_i) are correlated with the compound disturbance term if they indeed are determined in part by E_i as in the dynamic decision rules in relation 2. Therefore their coefficient estimates are biased because they include the effects of the correlated E_i, in the compound disturbance term in addition to the effects of S_i, Q_i and X_i.

(2) Most previous studies of the impact of college quality have not had observations on nor a way to control for all pre-college educational inputs, such as the qualities of secondary and primary schools and learning at home. Therefore, the estimated

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But this possibility has not been explored in the previous literature and is not considered further in the present study.

[8] In some cases test scores that measure achievement rather than innate abilities are used to represent endowments, but achievement reflects numerous choices. In some other cases endowments are represented by IQ and related test scores which purport to measure part of endowments, though there is controversy about the extent to which in fact they measure achievement in part. If whatever indicator that is used for endowments does not represent fully the endowments, the disturbance term may be correlated with the other observed variables so that biases result.

coefficients of time spent in college and of college quality are biased to the extent that these previous educational inputs are not controlled in the estimates but are correlated with time in college and college quality -- as seems likely.

(3) The measures of time in college and which college was attended used in most previous studies are self-reported and are likely to be subject to error. If such errors are random, they are likely to bias the estimated coefficients towards zero (though this is certain only in the one right-side variable case).

Alternative 2. Reduced-form estimates for wages with selectivity control for college choices: Rather than estimating the structural relation 1 directly for wages, the expressions in relations 2 and 3 for the human capital investment choice variables can be substituted into relation 1 to obtain a reduced-form wage relation:

(4) $W_i = W(P_i, A_i, E_i, z_i)$,

where z_i = a compound disturbance term that depends on e_i from relation 1, u_i from relation 2 and v_i, w_i and x_i from relation 3.

Conditional on the same assumptions about the observed right-side variables being predetermined as in necessary for them to be good instruments for our preferred alternative, estimating relation 4 yields unbiased estimates of the coefficients of the observed variables. These estimates have at least two advantages over those in our preferred alternative, while still being consistent with the recognition that choices are involved in human capital investments: (I) They do not require the assumption that certain predetermined variables (e.g., some of the assets) do not enter directly into the wage production function in relation 1 but only indirectly through affecting human capital investments in relations 2 and 3 and (ii) they do not require the assumption that all the choice inputs on the right side of relation 1 are observed. If the objective were only to predict wages, therefore, this alternative would seem to dominate our preferred alternative.

While such estimates may be very informative about the impact of the predetermined variables on wages and useful for predications of wages, however, direct estimation of relation 4 does not provide any insight into the impact of college choices on wages. BE/BEE therefore use college quality indicators that permit categorization of college quality into a relatively small number of categories and estimate relation 4 separately for each of these categories for sample members who elected to attend each category of four-year colleges so that the reduced-form wage relations can differ across four-year college categories. In such estimates they attempt to control for selection into each college category using multinomial logits that are identified with a "net college cost" measure that is individual-specific, reflecting both the probability that that individual will receive financial aid and location-related tuition fees for attendance at state colleges and universities.

This procedure is problematic on several grounds. First, relation 4 should include all relevant predetermined variables, including the "net costs" of attending college variable that BE/BEE exclude from relation 4 in order to attempt to identify the selection process. In fact their procedure is equivalent to including the college quality variables (and thus their identifying determinants) in interaction with each of the observed variables in relation 4 (because the coefficients of such variables are allowed to vary across college categories). Thus their selectivity control for choice of college quality is identified only by functional form, which is not very satisfactory. Second the restriction of the sample to those who attend four-year colleges means that the sample is selected because the subset of high school seniors who attend college is not randomly selected. Third, even among those who attend four-year colleges, there is a range of years attended and the choice of how many years to attend college is likely to be correlated with the quality choice. If so, this sample restriction results in estimates of relation 4 that confound the impact of the time in college with that of college quality.

While the second and third problems could be dealt with by alternative estimation strategies,[9] the first problem is fundamental and not easily resolvable. Nevertheless, for the purpose of comparison, we have attempted some estimates similar to those of BE/BEE using a small number of categories of college quality and selectivity terms from multinomial logits for the choices among these categories that are identified (under similar assumptions to those that BE/BEE make) by exclusion restrictions from relation 4 of some important components of costs. We also explore for our various alternatives the impact on the estimates of using the full sample versus the sample for those who attended four-year colleges.[10]

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[9] In principle, for example, the categories in the multinomial logit could include all outcomes for high school seniors (including no post-secondary education) and the categories could be defined for combinations of years of college and college quality (e.g., one category could be four years at an elite college) so that the time in college and college quality are not confounded. In practice, this might be difficult to estimate because of the large number of categories. We explore the addition of the former choice (i.e., no post-secondary education) in estimates that are summarized in Section 4 below.

10 In the introduction we also discuss in some detail the BRT study in which the wage production function in relation 1 and the human capital investment decision rules in relation 3 are estimated directly with control for genetic endowments using twins data. In essence in their procedure the difference in wages and in human capital investments for identical twins are used to obtain unbiased estimates of relation 1 (controlling both for unobserved endowments and for all determinants of pre-college educational investments) and the incorporation of the differences in human capital investments for fraternal twins permits estimation of the response to endowments in relation 3. This procedure assumes that there are no human capital choice inputs in the wage production function that are not observed and not identical between members of a twinship -- a weaker assumption than in production function estimates using individual data. Their procedure also has the advantage over all of the alternatives discussed in the text of generating estimates of the human capital response to unobserved individual endowments. One disadvantage of their procedure is that endowments are assumed to enter linearly in the functions estimated (though this is consistent with the usually estimated semi-log wage functions). A second disadvantage is that special data are required for such estimates. For the latter reason we are not able to present estimates using this procedure for comparison in this paper.

Section 2. Data

Our basic data source, as noted in the introduction, is the National Longitudinal Study of the High School Class of 1972 (NLSHS72). Though the NLSHS72 is a rich data source, an important aspect of our study is that we link our basic NLSHS72 data with other data sets in order to obtain better representations of variables related to local labor market conditions, college quality, and state and local college options than used in much of the previous literature. We select the NLSHS72 as our basic data set because: (1) The NLSHS72, through its Post-secondary Education Transcript Survey (PETS), has more accurate information about college enrollments than do most data sets because most data sets depend on respondent self reports. More accurate information on schooling is important because in recent years there has been increasing awareness of possible estimation problems using socioeconomic survey data not only because of random measurement errors (e.g., Ashenfelter and Krueger 1994, Behrman, Rosenzweig and Taubman 1994, Card 1993), but also because of systematic discrepancies with individuals with lower schooling levels and poorer family backgrounds more likely to overreport their schooling (Grubb 1990, 1993). For the OLS estimates, random measurement errors regarding the quantity of post-secondary schooling would cause problems. For all of the estimates systematic errors regarding the quantity or the quality of post-secondary schools attended would be problematic. (2) The NLSHS72, in contrast to many data sets, includes the names of colleges attended, which are essential for linking individual experiences to school quality indicators. (3) The NLSHS72 includes identification of state of residence when a high school senior, which permits linking the data to local labor market conditions and to state college tuition rates to provide information on relevant prices (Pi) for instruments for our second estimation alternative and for identifying selectivity in our third estimation alternative, again a feature not shared by a number of data sets. (4) The NLSHS72 is well-known and has been subject to a number of previous investigations regarding the determinants of college (as well as the impact of that schooling) in which high school achievement has been treated as predetermined (e.g., BEE/BE 1996, Datcher Loury and Garman 1995, Grubb 1990, 1993, Manski and Wise 1983, Rouse 1993, 1994, Velez 1985). (5) The last round of the NLSHS72 in 1986 contains information on the labor force experience of sample members when they are 14 years out of secondary school (most are 31-32 years old) by which age most sample members have passed through initial job search/matching experiences.

The NLSHS72 is a Department of Education survey of 22,652 high school seniors during the 1971-72 school year. The Department recovered further information on 13,274 of these individuals through its PETS. The initial interview was conducted in the Spring of 1972, with follow-up surveys in 1973, 1974, 1976, 1979, and 1986. In the Appendix, we provide a detailed description of NLSHS72 and the other data sets that we use. Pertinent sample statistics are presented in Table 1 for those for whom wages are available, separately for females and males with further disaggregations between whites and nonwhites (we do not further disaggregate the

nonwhites because of the small sample sizes[11]). The differences between the means for females and males and, within genders, between white and nonwhites, generally are significantly different at the 5% level with the exceptions that are indicated in the notes to the table. We now describe important characteristics of the variables used in our analysis.

Labor market outcomes in 1986: Mean hourly wages are $14.28 for males and $11.15 for females, with this difference significant. Within gender categories, however, the means for whites versus nonwhite are not significantly different even at the 10% level. Mean work experience is significantly greater for males than for females and, within genders, for whites than for nonwhites.

College choices: The mean time in college for males is 3.1 years, which is significantly greater than the mean time in college for females of 3.0 years. For both females and males, mean time in college is significantly greater for whites than for nonwhites. Further there are the same patterns between males and females and, within gender groups, between whites and nonwhites, among means for the proportions who attended four-year colleges -- higher for males than for females and, within gender groups, for whites than for nonwhites.

As noted in the introduction, we use two indicators of college quality:

(l)Median SAT score for students attending the college during the early 1970s that an individual selects to represent the quality of the student body and correlated college characteristics. We use data provided to us by Datcher Loury and Garman and link it to respondents' actual college choice.

(2) Instructional expenditures per student in 1975-6 to represent the resources per student. We use data from the federal government's Higher Education General Information Survey (HEGIS) that we link to respondents' actual college choices.[12]

Median SAT scores at college attended average significantly higher for males than for females and, within genders, for whites than for nonwhites. Instructional expenditures per full-time equivalent students average significantly more for males than for females (at the 10% level but not at the 5% level), but, within genders, do not differ between whites and nonwhites even at the 10% level.

We note here that if we treat the human capital investment choices on the right side of the wage production function in relation 1 as endogenous, there is sufficiently high correlation

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11 Among individuals with positive wages, 12.5% of females and 6.7% of males are classified as black and 3.4% of females and 4.6% of males are classified as Hispanic.

12 Gilmore (1990) reports significant positive correlations among various HEGIS indicators of college quality for 1985-6.

among them that we can not identify with any precision the separate contributions of the two college quality indicators (though together they are significantly nonzero at high levels). Therefore we have constructed an index in which these two indicators (each normalized by its standard deviation) are weighted equally that we use in our preferred estimates that are presented in Section 3. In alternative estimates that are discussed in Section 4 we present alternative estimates in which only one or the other of the two quality indicators is included.

Precollege education: NLSHS72 provides test scores that we can use as an indicator of individual pre-college education. As part of the Base Year survey in 1972, each student was asked to complete a 69-minute Test Book measuring verbal and nonverbal ability. The Test Book consisted of six tests, which we list here and describe in more detail in the Appendix: Mathematics, Reading, Vocabulary, Letter Groups, Picture Number, and Mosaic Comparisons. We use the sum of the first three of these tests as achievement measures in our estimates.[13] The means of these test scores are significantly higher for males than for females and, within gender groups, for whites than for nonwhites.

Family assets: Our basic indicators of family assets are family income,[14] mother's and father's schooling, number of siblings, birth order, and whether mother was not working when individual was of preschool age.

Higher income presumably represents greater general resources, which are diluted if there are more siblings and for children higher in the birth order (at least if parents have preferences that favor lower birth order children[15] or if higher birth order births are more likely to be unplanned). For family income the means are somewhat higher for males than for females and, within gender groups, substantially higher for whites than for nonwhites. This pattern in income is reinforced by reversed patterns in number of siblings and in birth order.

Higher parental schooling and mothers not working (and therefore providing more childcare?) during the individual respondent's preschool years (conditional on income) presumably mean higher resources directed towards education. The means for parental schooling do not differ significantly between males and females, but do differ significantly and fairly substantially between whites and nonwhites for both gender groups. The means for mother not

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13Manski and Wise (1983) use the first four components to predict an individual's SAT score, but we use only the first three components since the fourth appears quite related to the second and third.

14The family income variable is an estimate by the sample members when they were seniors in high school and probably is contaminated by considerable measurement error (quite possibly with a systematic tendency to over-report low income and under-report high income due to community norms regarding the prevalence of middle-income families in the U.S.).

15Behrman and Taubman (1986) report some evidence that parental preferences favor lower birth orders in the U.S. and Behrman (1988) reports similar results for India. Steelman and Powell (1989), however, claim the opposite for the U.S.

working when the individual was a preschooler are slightly higher for females than for males, and much higher for whites than for nonwhites within gender groups.

Prices: NLSHS72 provides data on the distances to the nearest two-year and the nearest four-year. We also employ a measure of tuition for state residents at public four-year institutions averaged over the 1972-1976 period. There are no significant differences by gender for these price indicators, but tuitions are significantly higher where whites reside than where nonwhites reside and four-year colleges are significantly closer where nonwhite females reside than where white females reside. Thus these components of prices, if anything, favor nonwhites.

We use data from the Current Population Survey (CPS) to develop measures of local labor market conditions (as explained more fully in the Appendix), with "local" defined by Census regions. We represent the high school opportunity wage by the median local wage for 16 year olds, which we posit may affect time allocation during high school years and thus high school achievement, but with only an indirect effect through high school achievement on college choices, We represent the opportunity cost of time for 18-21 year olds by the median wage for individuals 18-21 years of age who have completed high school but not gone on to college.[16] These prices are gender and race specific. They are significantly higher on average for males than for females and, within gender groups, for whites than for nonwhites (except there is no significant difference between the means in the median wages for college age white and nonwhite females).

Section 3. Preferred Estimates

Table 2 presents the predicted hourly wage rates based on our preferred estimates[17] for different combinations of years in college (two and four) and of college quality (bottom 25th percentile -- B25, median -- M50, top 25th percentile -- T25, and top 10th percentile -- TlO) for females and males with white-nonwhite breakdowns within each gender group. These simulations indicate increasing wage rates for increases in college quality holding constant the time in college and for time in college holding constant the college quality, as anticipated. However *prima facie* they may seem to suggest that there are not very large effects of college

16 We also have explored a representation of the expected return to investment in higher education by the discounted value of the wage differential between persons who have four years of college and persons who stopped their formal education with completion of high school as calculated from the 1972-1976 CPS with life-cycle patterns inferred from the cross-section patterns across age cohorts observed during the time period that the college decisions were being made. But we have not found evidence that this measure has significant effects so we have not included it in the estimates reported in this study.

17 As noted in the table, because of high multicollinearity and small sample sizes, our instrumental variable estimates for nonwhites do not appear very stable, so we present OLS estimates in this table for this group. Based on the comparisons for all females and all males in Section 4, OLS estimates tend to overstate the marginal effects of improving college quality and of increasing time in college by about 13% on the average for females and by about twice as much on average for males.

quality, though perhaps larger effects of time in college. For females with four years of college, for example, the increments in hourly wages from changing from the bottom quartile to the median in terms of quality is $0.07, from changing from the median to the top quartile $0.23, and from the top quartile to the top decile $0.23 (and the estimates are only about half as large for two years of college). For males with four years of college the estimates for similar changes are $0.10, $0.28, and $0.29 -- a little larger than for females, but of the same order of magnitude (and, as for females, about half as large for two years of college). Increasing time in college from two to four years at median quality levels increases predicted hourly wages for females by $0.66 and for males by $0.81 -- both of which are about 7% of the wage rates for an individual with two years of college of median quality. And increasing time at top decile colleges from two to four years increases hourly wage rates for both females and males by about 9%.

To understand further the implications of these estimates it is useful to consider the options open to a senior in high school. These options include going to college of some quality level for a while and then entering the labor market and working thereafter, reaping any increased earnings that accrue because of the college choice. Table 3 illuminates dimensions of the choice regarding the quality of college, based on the predictions in Table 2, for individuals who attend college for two years and for four years. Table 3 gives the present discounted value (PDV) at the time the individual completes secondary school of labor earning gains over the next 45 years (generally until s/he is 62 or 63) of upgrading the choice of college quality from the bottom 25 percentile to the median, from the median to the top 25 percentile, and from the top 25 percentile to the top 10 percentile. The first three columns consider such quality upgrades for two years of college; the last three columns consider them for four years of college. In all cases it is assumed that the indicated time in college (either two or four years) is completed first and then earnings differentials are gained for the equivalent of full-time work (2000 hours per year) over the remainder of the 45 year horizon. A real discount rate of 4% is used for these calculations.[18] As is noted in the table, with a discount rate of 8%, the gains from the college quality upgrades are 57% as large as in the table for two years of college and 53% as large as in the table for four years of college.

The calculations summarized in Table 3 suggest that there is a considerable range of payoffs in terms of lifetime earnings gains possible from individuals choosing to upgrade college quality conditional on years of college -- with variation depending on what the quality upgrade is, what the number of years of college are, and gender and race. Noteworthy features of this table include:

First because of the assumed interaction between time in college and quality of college in the wage production function, for all demographic groups considered the gains for the same change in quality percentiles are larger (about twice as large) with four years rather than two years of college.

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18 The previous literatures on college and occupational choice include a range for discount rates from 3 to 15 percent (e.g., Boskin 1974, Willis and Rosen 1979).

Second, for all demographic groups for a given number of years of college the gains are much larger for changes higher in the quality range than for equal percentile changes lower in the quality range. The gain from changing from the median to the top 25th percentile, for example, for each group is about three times as large as the gain from going from the bottom 25th percentile to the median, The gain from changing from the top 25th percentile to the top 10th percentile, likewise, is about the same as the gain from changing from the median to the top 25th percentile.

Third, for all demographic groups increasing quality further in the high quality range with college equal to two years yields higher gains than increasing quality by an equal percentile with college equal to four years (i.e., the effect in the second point outweighs the effect in the first point).

Fourth, for every quality change considered in the table the gains are largest for nonwhite males and smallest for white males, with those for nonwhite females tending to be second largest and those for white females tending to be third largest.[19] One aspect of this pattern is that there is a big white-nonwhite gap for males, but a much smaller (and in some cases possibly reversed) pattern for females. Another aspect is that the gains are substantially larger for females than for males among whites, but larger for males than for females among nonwhites.

Fifth, while some of the estimated gains are fairly small (at one extreme, $738 for white males changing from the bottom 25th percentile to the median with two years of college), for every demographic group the largest gains are fairly large -- in the $5,000 to $15,000 range.[20] Thus, under our assumptions, there would seem to be some incentives at least for those planning on attending median and higher quality colleges for four years to consider paying higher tuitions if necessary to attend higher quality colleges (or to bribing admissions officers if the admissions decision is the bottleneck). For example, taken at face value these numbers suggest that a risk neutral nonwhite male who should be willing to pay about $4,000 more per year in tuition (or an initial lump sum of over $15,000) to attend for four years a college with quality at the top 25 percentile instead of at the median or at the top 10 percentile instead of at the top 25 percentile of quality. Such calculations raise the question, why did not more nonwhite males avail themselves of the possibility of attending higher quality colleges? The answer to this question might include one or more of the following: imperfect information that led to an underappreciation of the returns to college quality (with information more imperfect for

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19 As noted above with regard to Table 2, the use of OLS estimates may bias upwards the estimates for nonwhites. But if the biases are of the magnitude suggested by the comparisons in Section 4, this statement still holds.

20 Though with the qualification again about the probable overstatement for nonwhites because of the use of OLS.

nonwhites than for whites), less access to capital markets and less possibility for self-financing (or family financing) education for nonwhites than for whites, more discrimination against nonwhites than whites in admissions at higher quality institutions, and higher discount rates on the average for nonwhites than for nonwhites. We are not able to identify the relative importance of these (and perhaps other) alternatives.

The simulations in Table 3, thus, suggest that the private earnings gains from improved college quality (holding time in college constant) vary considerably across quality levels, years of college and demographic groups -- but there are some possibilities of fairly considerable gains,

But what about the effects of increasing years of college, holding quality constant. The first four columns of Table 4 give the PDV of gains in wages for years 5-45 from increasing college from two to four years, holding quality constant at the same four levels, for all females and all males. Estimates are presented for real discount rates of 2% and 4%. Some noteworthy features of these simulations include:

First, the gains that are presented in the first four columns increase substantially -- and at an increasing rate -- as quality increases, For instance, for total females with a 4% (2%) discount rate the PDV of these gains are $21,700 ($32,000) for quality equal to the bottom 25 percentile, but $32,300 ($47,700) at the top 10 percentile. Generally the gains at the top 10 percentile are about one and a half as large as those at the bottom 25 percentile. This is another aspect of the interaction between time in college and college quality mentioned above.

Second, the gains in the first four columns are larger for males than for females at all quality levels -- from 14 to 38% larger. There does not seem to be much of a tendency for the gains for males relative to females to be systematically associated with quality levels for either whites or nonwhites.

Third, even though the PDV of earnings gains over years 5-45 are fairly considerable, they do not mean that the estimates imply that there are strong incentives for members of all demographic groups at all college quality levels to increase time at college from two to four years. To the contrary, the estimated gains over years 5-45 are greater for whites than the opportunity costs of not earning in years three and four (the last four columns in the table[21] only for the top 25 and 10 percentiles of quality if the real discount rate is 2% and, in addition for white females, at the top 10 percentile of quality if the real discount rate is 4%. For nonwhites, in contrast, the PDV of gains in the first four columns are greater than the opportunity costs in the last four columns with the single exception of nonwhite females at the bottom 25 percentile if the discount rate is 4%. In part this racial difference reflects that the nonwhite estimates, as noted, are based on OLS and

21 In addition there may be other costs of the third and fourth year of college, such as tuition and fees.

therefore probably upward biased. But even if the nonwhite estimates are discounted by the amount of the apparent bias based on the comparisons between our preferred estimates and OLS estimates that are discussed in Section 4, the gains net of opportunity costs for increasing time in college would be greater for nonwhite than for whites. Once again as for the discussion of gains from increasing quality for given time in college, there are several possible reasons why there might be this racial differential -- differences between whites and nonwhites in capital market access and possibilities of self- and family-financing, admissions processes, and discount rates -- and once again we are not able to identify the relative importance of these alternatives.

Fourth, the calculation at the start of this section that wages tend to be about 7% higher, holding quality constant, for individuals with four rather than two years of college -- while similar to calculations often made from data such as in Table 2 -- is misleading, particularly for the total estimates and for whites because it ignores the opportunity cost of time if relation 1 is a wage production function. Of course semilog wage relations similar in appearance to relation 1 often are interpreted to mean that the rate of return to schooling is the coefficient of the time in school following Mincer (1974). But the Mincerian derivation of this relation is an equilibrium condition in which an individual is indifferent among various schooling levels and is not consistent with the model that we present in Section 1 because it assumes that schooling is independent of any endowments that directly affect wages. As our results in Table 3 illustrate, interpreting the coefficient of schooling in such a relation as the rate of return to private time spent in school can be very misleading.

Section 4. Alternative Estimates

We emphasize in the introduction and in Section 2 how our assumptions differ in a number of respects from those in previous studies in the literature or may depend on particular assumptions that we make. We now explore how sensitive our results are to a number of alternative assumptions. Table 5 presents at the top the increases in hourly wage rates for changes in quality given years of college (again, first held at two years and then head at four years) and the changes in post college wage rates for increases in years of college from two to four for alternative college quality levels. The alternatives are presented as percentages of the preferred estimates in order to facilitate comparisons with the preferred estimates.[22]

OLS estimates: Almost all of the previous literature has used OLS, with two exceptions that are discussed in the introduction. OLS may lead to omitted variable biases (probably upward

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22 In addition to the estimates that are summarized in this table, we explored the possibility of selectivity control for choices among categories defined by college quality (the second alternative discussed in Section 1). The estimates we obtained are not robust, but change substantially (sometimes to implausible values) with small changes in the specification. We believe that this lack of robustness reflects the identification only by nonlinearity problem that we discuss in Section 1.

due to correlations of human capital investments choices with unobserved ability and motivation endowments) and measurement error biases (probably downward if the measurement errors are random in the right-side variables), as are discussed in Section 2. The OLS estimates summarized in Table 5 suggest that the omitted variable bias due to the failure to control for unobserved endowments probably dominates. They are systematically as high or higher than our preferred estimates, averaging about 13% higher for females and about 27% higher for males. Thus, conditional on our preferred model, the OLS procedures lead to upward biases in the returns to both college quality and college quantity.

No control for pre-college education: Many of the studies in the literature, as noted in Section 1, attempt to evaluate the impact of college characteristics on wages without control for pre-college education. If characteristics of pre-college education affect wages and are correlated with college choices as would follow from longer-run optimizing behavior and seems plausible on the basis of casual observations (i.e., students from better quality secondary schools appear more likely to attend better quality colleges), this exclusion is likely to lead to overestimates of the impact of time in college and of college quality. The estimates summarized in Table 5 for a model identical to our preferred model except that the indicator of pre-college education is dropped do indeed suggest such an upward bias. For total females, this bias is about 10% -- both for the returns to college quality and to the time in college. For total males, this bias is much larger -- generally in the range of 70 to 90% -- again both for the returns to college quality and to the time in college. The gender differences suggest that quality dimensions of pre-college and college education are more correlated for males than for females, so estimates for the former are more biased if there is no control for pre-college education. Again, therefore, conditional on our preferred model, the failure to control for pre-college education (as in many studies) causes upward biases that may be considerable in estimates of the impact of both time in college and college quality.

Sample limited to those who attend four-year colleges: Some studies, such as BE/BEE, limit their empirical analysis to those who attend college or to those who attend colleges of a certain type. It would seem that the use of such a sample might change the estimated impact of time in college and of college quality both due to the selected nature of the sample and due to reduced sample variance. In Table 5 we summarize some estimates that are identical to our preferred estimates except the sample is limited to those who attended four-year colleges instead of including all high school seniors. The use of this selected sample changes the estimates considerably and differentially by gender, resulting in lower estimated effects for females and larger ones for males. Thus, such a selection rule is not innocuous because it leads to much different estimates than does the use of the full sample.

Individual components of college auality: As noted in Section 2, because of the high collinearity between our two college quality measures we are not able to identify with much precision their respective individual contributions. Therefore in our preferred estimates we use a composite quality measure weighing equally median SAT and instructional expenditures per full time equivalent (each normalized by its respective standard deviation). Table 5 summarizes what

happens to the estimates if the two components each is used individually in what otherwise is our preferred specification. The estimates suggest that clearly the two components of our composite quality index are representing different aspects of college quality that are not perfectly correlated. The use of instructional expenditures leads to higher effects of changing college quality and lower effects of changing time in college than the composite index, and vice versa (except at very low quality levels) for median SATs. These results reinforce the desirability of finding ways to treat these and other aspects of colleges as reflecting choices and permit identifying better the individual contributions of each to wages.

Section 5. Conclusions

There has been a rising interest in understanding better the impact of college choices on wages that has been motivated by concerns about increasing wage inequalities, about increasing costs of elite colleges and about the perceived increasing roles of highly educated individuals in maintaining international competitiveness. There have been a number of previous studies on the impact of college quality on wages. But they have been subject to two general limitations. First, they have not considered the differential impact for different demographic groups identified by race and gender. Second, with two recent exceptions, they have not considered the choice dimensions with regard to time in college and college quality.

We address both of these limitations. We obtain estimates of the impact of time in college and of college quality choices on wages for four demographic groups based on an explicit dynamic framework. Our estimates suggest that treating time in college and college quality as choices within a dynamic framework affects fairly substantially the estimated effects. Based on our results, most previous studies may have overestimated the impact of both college quality and time in college because of the failure to deal with such choices and, in a lesser but still significant number of cases, because of the failure to control for the quality dimensions of pre-college education. Our estimates also suggest that there are important demographic differences. The estimated wage benefits from higher college quality and more time in college tend to be highest for nonwhite males and next for nonwhite females, then white females and least for white males, For all groups there appear to be incentives for increasing college quality by paying somewhat higher tuitions or whatever, more so for those who are longer in college because of interactions between college quality and time in college. But these incentives differ strongly across groups, being much higher for nonwhite males and much lower for white males, with the females in between. For nonwhites (again stronger for males than females) there also appear to be incentives to increase time in college. For whites, in contrast, the estimated net gains do not appear to create such incentives at least at a 4% real discount rate -- a result that contrasts with a common interpretation of semilog wage relations based on implicit assumptions that are not consistent with our modeling framework (and we believe not plausible). The results for nonwhites raise important questions regarding why there is unrealized potential for reaping net benefits from more time in college or attending higher quality colleges. A number of possible answers to this question (e.g., poor information about the impact of college, imperfect capital markets for financing college, poor information about college quality differentials, discriminatory

admissions) may have important policy implications on both equity and efficiency grounds.

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Appendix

A. Description of Data and Variables

We combine the NLSHS72 data with information of highest degree attained from the Post-secondary Education Transcript Study (PETS). The PETS survey culled the NLSHS72 first through fourth follow-ups for the names of colleges attended by the 14,700 respondents who reported attending college. Transcripts were requested from all schools respondents reported attending. PETS allows us to calculate educational attainment as of 1984, 12 years after high school graduation. These data improve on the self-reported enrollments provided in NLSHS72. Grubb (1990) shows that individuals on average over-report their post-secondary attainment, particularly those not in academic courses in high school and with lower ability, high school grades and family socioeconomic status. Thus, use of the self-reported data would bias downward the estimated effects of high school and family background on college enrollment. Our measure of attainment is constructed by finding the highest degree earned or level of schooling attended as reported in the PETS survey. The attainment variable is underestimated for some observations; this might be due to transcripts being lost or destroyed, respondents not listing a school they attended, or respondents missing a follow-up survey.

Our family background and achievement variables are drawn directly from the NLSHS72 data file. These family background variables -- including maternal and paternal education and family income -- are presented in the original data source in categorical form. In our estimates we use continuous variables by using the mid-points of the categories.[23] NLSHS72 also provides information on the distance to the closest two-year and the closest four-year college (which is part of the representation of prices).

With state identifiers, we can link individuals in the NLSHS72 data set with information from the Current Population Survey (CPS), and the Bureau of the Census. We use CPS data for 1972-1976 to obtain two measures of labor market conditions for individuals for the same sex, race and region. We use six Census regions (North Central, South Central, Atlantic, mountain, Pacific, New England) with aggregation of contiguous regions in cases in which otherwise the cell sizes would be less than 25. The first variable is the opportunity cost of time while in high school, which we represent by the median wage for 16 year olds. The second variables is the opportunity cost of attending a post-secondary institution, which we represent by the median wage for individuals who have completed high school but not gone on to college and who are 18-21 years of age. We use the medians instead of the means so that our representation is not dominated by outliers in the distribution of wage rates.

PETS contains FICE codes for colleges, which allow us to link our data with HEGIS data to measure institutional quality. We use instructional expenditures per FTE student for the

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[23] We have explored different means of consolidating these data into fewer variables. The results do not differ substantially with different representations.

academic year 1975-76.

B. Description of Test Book

The six tests are described below in sequential order of administration, based on Center for Education Research and Evaluation (198 1, pg. 17- 18). Due to the proprietary nature of the Test Book, a copy of the test is not available to data users. The data file contains scaled scores for the six tests.

a. Vocabulary A brief test using synonym format. The items were selected to avoid academic or collegiate bias and to be an appropriate level of difficulty for the twelfth grade population.

b. Picture Number; A test of associative memory consisting of a series of drawings of familiar objects each paired with a number. The student, after studying the picture number pairs, is asked to recall the number associated with each object.

c. Reading: A test based on short passages (100-200 words) with several related questions concerning a variety of reading skills (analysis, interpretation) but focusing on straightforward comprehension, In combination with the vocabulary test, it provides a means to drive a verbal score which can allow links to the normative data available for the SAT.

d. Letter Groups: A test of inductive reasoning requiring the student to draw general concepts from sets of data or to form and try out hypotheses in a nonverbal context. The items consist of five groups of letters among which four groups share a common characteristic while the fifth group is different. The student indicates which group differs from the others.

e. Mathematics: Quantitative comparisons in which the student indicates which of the two quantities is greater, or asserts their equality of the lack of sufficient data to determine which quantity is greater. This type of item is relatively quickly answered and provides measurement of basic competence in mathematics.

f. Mosaic Comparisons A test which measures perceptual speed and accuracy through items that require that small differences be detected between pairs of otherwise identical mosaics or tile-like patterns. A deliberately speeded test, it has three separately timed sections consisting of increasingly more complex mosaic patterns.

Table 1. Means and Standard Deviations of Basic Variables by Gender and Gender-Race Groups[a]

	Female			Male		
	Total	White	Non White	Total	White	Non White
Labor market outcomes 1986						
wage ($/hour)[f]	11.15	11.11[b]	11.36[b]	14.28	14.36[b]	13.65[b]
	(8.74)	(7.83)	(12.50)	(9.60)	(8.89)	(13.98)
work experience (years)	9.6	9.6	9.3	10.7	10.7	10.2
	(2.7)	(2.6)	(2.9)	(2.3)	(2.2)	(2.6)
College choices						
years of college	3.0	3.0	2.5	3.1	3.2	2.5
	(1.7)	(1.7)	(1.5)	(1.7)	(1.8)	(1.5)
attended four year college	.727	.741	.656	.768	.784	.639
	(.445)	(.438)	(.475)	(.422)	(.411)	(.481)
median SAT for college	912	927	837	932	940	865
	(207)	(202)	(214)	(202)	(201)	(192)
instructional expenitures/ full time equivalent ($)	1382[c]	1375[b]	1404[b]	1443[c]	1446[b]	1419[b]
	(1264)	(1264)	(1266)	(1455)	(1462)	(1391)
Test Scores in 1971/72	162.5	167.3	135.4	164.8	167.8	138.4
	(24.0)	(21.3)	(20.5)	(23.0)	(21.0)	(22.7)
Family assets						
income ($)	8577	9211	5221	9978	10425	6450
	(6557)	(6681)	(4577)	(6293)	(6310)	(4902)
mother's schooling (grades)	12.3[d]	12.6	10.8	12.4[d]	12.7	10.4
	(2.6)	(2.5)	(2.8)	(2.5)	(2.4)	(2.7)
father's schooling (grades)	13.1[d]	13.5	10.7	13.2[d]	13.5	10.4
	(3.6)	(3.5)	(3.3)	(3.6)	(3.5)	(3.1)
number of siblings	2.8	2.6	4.2	2.7	2.6	3.7
	(2.1)	(1.8)	(2.9)	(2.1)	(2.0)	(2.9)
birth order	2.3	2.2	3.1	2.2	2.1	2.7
	(1.5)	(1.3)	(2.6)	(1.5)	(1.4)	(2.0)
mother not working when preschooler	.634	.683	.377	.607	.631	.422
	(.482)	(.466)	(.485)	(.488)	(.483)	(.495)
Prices						
distance to 2-year college (miles)	17.9[d]	18.1[b]	16.5[b]	17.3[d]	17.5[b]	15.6[b]
	(25.2)	25.4)	(24.0)	(26.9)	(27.2)	(24.0)
distance to 4-year college (miles)	18.3[d]	18.7	16.0	18.4[d]	18.5[b]	17.9[b]
	(24.7)	(24.7)	(25.2)	(24.2)	(24.0)	(26.6)
average state tuition ($)	471[d]	482	413	474[d]	482	408
	(176)	(174)	(173)	(181)	(179)	(185)
median wage high school age (16)[e]	7.17	7.23	6.84	8.52	8.51[b]	8.59[b]
	(1.02)	(1.01)	(1.02)	(0.96)	(0.95)	(1.00)
median wage college age (18-21)[e]	7.47	7.47[b]	7.45[b]	9.07	9.20	8.07
	(0.64)	(0.49)	(1.17)	(0.80)	(0.62)	(1.22)
Number of observations	3222	2710	512	3206	2846	360

[a]Based on all individuals in NLSHS72 with positive wages. t-tests indicate significant differences at the 5% level between female and male means and race means (within gender groups) unless otherwise noted.
[b]Means not significantly different by race within gender at 10% level.
[c]Means significantly different by gender at 10% level.
[d]Means not significantly different by gender at 10% level.
[e]For race-gender groups in census region in 1970-71 where resided from CPI.
[f]For comparison with the estimates below from semilog wage functions, wages evaluated at means of ln wage distributions are, respectively, $9.66, $9.74, $9.26, $12.52, $12.70 and $11.19.

Table 2. Predicted Hourly Wage Rates for Alternative Combinations of Time in College and College Quality Using Preferred Estimates[a]

	College = 2 Years				College =4 Years			
	B25	M50	T25	T10	B25	M50	T25	T10
Females - total	9.33	9.37	9.48	9.58	9.94	10.03	10.26	10.49
- white	9.31	9.35	9.46	9.57	9.96	10.03	10.27	10.52
- nonwhite	8.65	8.69	8.84	8.98	9.47	9.58	9.90	10.23
Males - total	$11.94	$11.98	$12.12	$12.25	$12.69	$12.79	$13.07	$13.36
- white	11.74	11.76	11.82	11.89	12.11	12.16	12.29	12.43
- nonwhite	11.19	11.27	11.46	11.66	12.32	12.47	12.91	13.35

[a]These hourly wage rates are in 1986 dollars. Quality is compared at the bottom 25th percentile (B25), the median (M50), the top 25th percentile (T25), and the top 10th percentile (T10). The predictions are based on mean sample experience for each demographic group for work experience and end-of-secondary school scores. The preferred estimates are described at the start of Section 1, with the right-side variables treated as endogenous using instrumental variable estimates and with the composite college quality index discussed in Section 2. The first-stage estimates are in Appendix Table A1 and the preferred estimates are in Appendix Table A2, both of which are available from Lori Kletzer upon request. For nonwhites OLS estimates are given because the instrumental variable estimates are not very stable apparently due to multicollinearity and small sample sizes. (See Section 4 for a comparison of the preferred and OLS estimates for the whole sample.)

Table 3. Present Discounted Values at Age 18 of Improved College Quality Given Years of College, based on Preferred Estimates[a]

	College = 2 Years			College = 4 Years		
	B25 to M50	M50 to T25	T25 to T10	B25 to M50	M50 to T25	T25 to T10
Females - total	$1566	$4308	$3916	$3197	$8170	$8170
- white	1566	4308	4308	2842	8525	8880
- nonwhite	1566	5874	5482	3907	11366	11722
Males - total	1566	5482	5090	3552	9946	10301
- white	738	2350	2741	1776	4618	4973
- nonwhite	3133	7740	7832	5328	15629	15629

[a]Based on wages summarized in Table 2, a time horizon of 45 years when a high school senior, and a real discount rate of 4%. The first three columns assume that the individual goes to college for two years, and then reaps any wage change due to different college quality for years 3 through 45. The last three columns assume that the individual goes to college for four years and then reaps any wage change due to different college quality for years 5 through 45. Individuals are assumed to work 2000 hours per year after college through year 45. Under these assumptions the present discounted value (PDV) at age 18 or $1 per year more in each of years 3-45 is $19.58 and that for years 5-45 is $17.76. Under the same assumptions but with a real discount rate of 8%, these PDVs respectively are $11.14 and $9.49 -- which implies that with a real discount rate of 8% the items in the first three columns would have to be multiplied by 0.57 (= $11.14/$19.58) and those in the last three columns would have to be multiplied by 0.53 (= $9.49/$17.76). All values are in 1986 dollars.

Table 4. Present Discounted Values in 1000s of 1986 Dollars at Age 18 of Wage Differences in Years 5-45 and 3-4 from Increasing College from 2 to 4 Years with Quality Constant[a]

		PDV of Wage Difference in Years 5-45 in $1000s				Foregone Wages in Years 3-4 in $1000s			
		B25	M50	T25	T10	B25	M50	T25	T10
Females - total	4%	21.7	23.4	27.7	32.3	32.5	32.7	33.1	33.4
	2%	32.0	34.6	40.9	47.7	34.8	35.0	35.4	35.8
- white	4%	22.7	23.8	28.8	33.7	32.5	32.6	33.0	33.4
	2%	32.0	34.6	40.9	47.7	34.7	34.9	35.3	35.7
- nonwhite	4%	29.1	31.6	37.7	44.4	30.2	30.3	30.8	31.3
	2%	43.0	46.6	55.5	65.5	32.3	32.4	33.0	33.5
Males - total	4%	24.8	28.8	33.7	39.4	41.6	41.8	42.3	42.7
	2%	39.3	42.4	49.8	63.4	44.6	44.7	45.2	45.7
- white	4%	26.6	28.8	33.7	39.4	40.9	41.0	41.2	41.5
	2%	38.3	42.4	49.8	58.1	43.8	43.9	44.2	44.4
- nonwhite	4%	40.1	42.6	51.5	60.0	39.0	39.3	40.0	40.7
	2%	59.2	62.9	75.5	88.6	41.8	42.1	42.8	43.5

[a]See notes to Table 3. The alternative rows are for real discount rates of 4% and 2% respectively. The PDV at age 18 of $1 per year more in each of years 5-45 is $26.20 at a 2% real discount rate.

Table 5. Comparison of Hourly Wage Predictions from Preferred Estimates with Those from Alternatives[a]

	College = 2 Years			College = 4 Years			College increased from 2 to 4 Years			
	B25 to M50	M50 to T25	T25 to T10	B25 to M50	M50 to T25	T25 to T10	B25	M50	T25	T10
Preferred Estimates										
Females	$.04	$.11	$.10	$.09	$.23	$.23	$.61	$.66	$.78	$.91
Males	.04	.14	.13	.10	.28	.29	.75	.81	.95	1.21
OLS Estimates										
Females	100	118	120	100	117	117	113	112	113	113
Males	175	114	131	130	136	127	129	127	132	120
No Control for Pre-College Education										
Females	100	109	120	111	113	113	110	111	112	111
Males	225	171	177	190	193	186	183	181	186	172
Sample Limited to Those Attending 4-Year College										
Females	50	37	40	44	35	39	38	38	37	37
Males	150	121	131	130	136	128	129	128	132	120

	College = 2 Years			College = 4 Years			College increased from 2 to 4 Years			
	B25 to M50	M50 to T25	T25 to T10	B25 to M50	M50 to T25	T25 to T10	B25	M50	T25	T10
Quality=Median SAT										
Females	150%	73%	80%	144%	74%	74%	123%	124%	117%	110%
Males	200	79	69	178	79	69	124	125	119	106
Quality=Instruct. Exp./FTE										
Females	275	118	200	267	130	187	54	70	78	92
Males	350	121	192	290	136	179	53	68	80	85

[a]The preferred estimates are those that are summarized in Tables 1-3, with endogenous treatment of human capital investments as choices through instrumental variable methods. The first two rows give the implication of the preferred estimates for changes in hourly wage rates for the indicated changes in college quality (holding constant time in college at two and four years, respectively) and changes in time in college from two to four years (holding college quality constant at alternative percentiles). The alternatives all are identical to the preferred estimates except for the one indicated change. The alternatives are summarized by the change they imply relative to that implied by the preferred estimates in percentage terms.