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Who Benefits Most From College? Evidence for Negative Selection in Heterogeneous Economic Returns to Higher Education

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Abstract

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WHO BENEFITS MOST FROM COLLEGE?

EVIDENCE FOR NEGATIVE SELECTION IN HETEROGENEOUS ECONOMIC RETURNS TO HIGHER EDUCATION*

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WHO BENEFITS MOST FROM COLLEGE?

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ABSTRACT

We consider how the economic return to a college education varies across members in the U.S. population. Based on principles of comparative advantage, positive selection is commonly presumed, i.e., that individuals who are most likely to select into college benefit most from college. Net of observed economic and non-economic factors influencing college attendance, we conjecture that individuals who are *least* likely to obtain a college education benefit most from college. We call this theory the *negative selection hypothesis.* To adjudicate between the two hypotheses, we study the effects of completing college on earnings by propensity score strata using longitudinal data from three sources representing three different cohorts: the National Longitudinal Study of Youth 1979, the National Longitudinal Study Class of 1972, and the Wisconsin Longitudinal Study. For both men and women, for every observed life course stage, and for all three data sources, we find evidence for negative selection. We discuss patterns across propensity score strata by gender, cohort, and life course. Results from auxiliary analyses demonstrate differential selection mechanisms and counterfactual expectations offering potential explanations for negative selection and suggest that empirical support in the past literature for positive selection may result from model specifications with more limited variables.

Keywords: college education; economic returns; causal effects; selection bias; heterogeneity;

WHO BENEFITS MOST FROM COLLEGE?

EVIDENCE FOR NEGATIVE SELECTION IN HETEROGENEOUS ECONOMIC RETURNS TO HIGHER EDUCATION

Educational expansion is one of the most apparent, enduring, and consequential features of a modern society. Considering the significant educational expansion, particularly at the post-secondary level, in the United States over the twentieth century, Fisher and Hout (2006) conclude that "the division between the less- and more-educated grew and emerged as a powerful determiner of life chances and lifestyles" (p. 247). In personal earnings, for example, college graduates earned on average roughly $55,000 in 2005 compared to less than $30,000 for high school degree holders (U.S. Census Bureau 2007); this differential represents a significant increase in the earnings advantage associated with a college degree over the last several decades (Fisher and Hout 2006; Mare 1995). Decades of sociological research have confirmed Americans' strong belief in the role education plays in socioeconomic attainment and mobility (e.g., Blau and Duncan 1967; Hout 1988). Given the central role of education in contemporary societies, questions about access to and the impact of education have long occupied the attention of scholars in sociology and economics. More concretely, scholars have asked (1) what family and individual attributes are associated with the attainment of higher education? and (2) what are the causal effects of higher education on socioeconomic outcomes?

In the rational-behavioral model for the choice of attaining higher education common to the economics literature, the questions posed above are intrinsically intertwined: individuals make decisions about whether or not to pursue higher education on the basis of cost-benefit analyses, doing so only if higher education enables them to earn more lifetime earnings, at least in expectation (Becker 1964; Card 1995, 2001; Heckman and Honore 1990; Manski 1990; Mincer 1974; Roy 1951; Willis and Rosen 1979). In other words, except for situations of borrowing constraints, imperfect information, or some degree of uncertainty (Altonji 1993), individuals self-select into college versus non-college on the basis of expected economic returns, such that persons attain college educations only if the economic returns outweigh the costs. While this utility maximization paradigm can in principle accommodate non-economic factors, scholars partial to this approach seldom pay much attention to such factors in determining higher education.[1] The exclusive focus on economic determinants of college education leads to the hypothesis that those individuals who are most likely to attend college, as predicted by observed attributes, are also most likely to benefit from college (Carneiro, Hansen, and Heckman 2003; Carneiro, Heckman, and Vytlacil 2001, 2007; Carneiro and Lee 2005; Cunha, Heckman, and Navarro 2005; Heckman, Urzua, and Vytlacil 2006; Willis and Rosen 1979). We call this thesis the *positive selection hypothesis*.

In the sociological literature, the two research questions are normally separated. This separation has been justified by the recognition that education both introduces independent variation that allows individuals to be evaluated on meritocratic principles and transmits the influences of family background on socioeconomic attainment (Blau and Duncan 1967; Hout and DiPrete 2006). This recognition has resulted in an extremely rich sociological literature on the determinants of college education (Biblarz and Raftery 1999; Boudon 1974; Bourdieu 1977; Bowles and Gintis 1976; Coleman 1988; Collins 1971; DiMaggio 1982; Downey et al. 1999; Guo and VanWey 1999; Hauser, Tsai, and Sewell 1983; Jencks et al. 1972; Lareau 1987; Lucas 2001; MacLeod 1989; Mare 1980; Mare 2006; McLanahan and Sandefur 1994; Phillips 1999; Sewell, Haller, and Ohlendorf 1970; Willis 1981). A key theme that emerges from this literature is that many non-economic factors predict college attainment, as college-going behavior is governed not only by rational choice, but by cultural and social norms and circumstances (Coleman 1988). For some persons in socially advantaged positions, college is a culturally expected outcome and thus less exclusively and intentionally linked to economic gain than it is for people in less advantaged groups, for whom college education is a novelty that may well demand economic justification (Beattle 2002; Boudon 1974; Smith and Powell 1990). Thus, we hypothesize differential selection mechanisms influence college attainment by social background.

[1] There are noteworthy exceptions to this statement. For instance, in recent work, Heckman (2007) emphasizes the importance of socioemotional skills, such as motivation, sociability, self-esteem, and health, for educational attainment.

We further hypothesize there are differential counterfactual expectations of future earnings paths by social background. Low-skilled, less-educated workers increasingly face limited labor market prospects (Farley 1996). Given these currents, the counterfactual earnings path for college-educated workers from disadvantaged backgrounds had they not attended college is particularly bleak, yielding an acutely significant benefit to obtaining a college degree among these workers. In addition, the robust finding of a weaker association between social origins and destinations among the more educated than among the less-educated in the literature on social stratification (e.g., Hout 1988) further suggests the likely presence of differential counterfactual expectations. In sum, once we partial out observed covariates that help predict college education, we conjecture that differential selection mechanisms and counterfactual expectations are such that individuals who are *least* likely to obtain a college education benefit most from college. We call this conjecture the *negative selection hypothesis.*

To adjudicate between the positive selection and negative selection hypotheses, we conducted an empirical study analyzing data from three large U.S. longitudinal surveys: (1) the National Longitudinal Survey of Youth 1979 (NLSY79), a nationally representative sample of young men and women who were 14-22 years old when they were first surveyed in 1979; (2) the National Longitudinal Study of the class of 1972 (NLS72), a nationally representative sample of high school seniors in 1972; and (3) the Wisconsin Longitudinal Study (WLS57), a random sample of Wisconsin high school graduates in 1957. Use of these three data sources enables us to curb relative strengths and weaknesses across the datasets, including differences due to the quality of control variables available, and to establish robustness for our results. It also allows us to examine possible differences in results across cohorts over the life course. As individuals invest in higher education with the expectation of obtaining economic benefits over the lifetime (Mincer 1974), it is important to consider variation in returns to higher education over the life course.

Estimating causal effects with observational data is inherently difficult (Morgan and Winship 2007). The difficulty lies in the researcher's inability to observe true counterfactual outcomes. For our substantive problem, we observe neither the earnings that college graduates would have received had they not completed college nor the earnings non-college graduates would have received had they completed college. To address this methodological conundrum, we use a three-step approach. First, we invoke an ignorability assumption that, after we control for a rich set of observed covariates, there are no additional confounders between persons who do and do not complete college. Under the ignorability assumption, we summarize in estimated propensity scores systematic differences in covariates between persons who do and do not complete college (Rosenbaum and Rubin 1983, 1984). Second, we estimate causal effects of college completion on earnings by propensity score strata and examine patterns of effects using a hierarchical linear model (Xie and Wu 2005). This key step allows us to find either a positive or a negative pattern between the effects of a college education and the likelihood of obtaining a college education. Third, we revisit and challenge the ignorability assumption and conduct a series of auxiliary analyses that aids our interpretation of the results. We examine additional covariates that yield insight into the heterogeneous selection mechanisms among college goers. In a sensitivity analysis, we purposely omit several important covariates and explore the consequences of violating the ignorability assumption by omitting them.

We estimate results separately by sex. While the educational gap between men and women is narrowing, economic returns to education are still larger for men than for women (U.S. Census Bureau 2007). Moreover, historically, women's attachment to the labor force has been more irregular than men's, due primarily to competing family responsibilities (Bianchi 1995). For simplicity, we limit our focus to the earnings gap between individuals who complete college and those who complete only high school. While it is clearly a simplification to treat education as a dichotomous treatment, this allows us to easily borrow the methodological literature on causal inference. In future studies, we will measure higher education more precisely by amount, quality, and major. For now, we compare college-credentialed workers with non-college-credentialed workers, as there is a well-documented difference between the two groups in the labor market (Grubb 1993; Mare 1981).

SELECTION BIAS REVISITED:
A TYPOLOGY FOR ESTIMATING THE EFFECTS OF EDUCATION ON EARNINGS

While it is a well-established empirical regularity that, on average, college-educated workers earn higher wages than less-educated workers, social scientists have long recognized that the observed association between education and earnings may differ from the true causal effect of education (see Card 2001 for a recent survey of the literature). If observed and unobserved factors are correlated both with selection into higher education and with earnings, estimates of the return to education based on comparisons of earnings between persons with and without higher education will be biased (Blundell, Dearden, and Sianesi 2005). This bias is called "selection bias." In the following, we define different forms of selection bias and discuss their theoretical implications.

Two Sources of Selection Bias

As is well known in the causal inference literature, there are actually two types of selection bias in observational data (Morgan and Winship 2007). The first type is due to heterogeneity in preexisting conditions or attributes that are associated with both the treatment condition and the outcome. The second is due to heterogeneity in treatment effects. In the case of economic returns to higher education, the first type refers to individual attributes such as mental ability and work habits that may be positively associated with the likelihood of attaining both higher education and earnings. The second type refers to systematic differences between those who do and those who do not attain a college education in reaping economic returns. While simplified for analytical purposes as binary conditions in our study, college education surely means different treatments for different people. Returns to higher education should vary at the individual level, as it is implausible to assume that the impact of a college education is identical across different members in a society (Card 1999). Given inevitable variability in returns to college, particular segments of the population are likely to have greater or lesser average gains than the population average. In this research, we aggregate individuals according to their estimated propensity to complete college based upon observed attributes. We ask whether persons who are more likely to attain college educations based on observed attributes receive higher or lower returns to college education relative to persons less likely to attain college educations. This approach allows us to explore the potential association between the two sources of population heterogeneity.

To illustrate the first source of bias, let us begin by assuming homogeneous effects of a college education. Let subscript i represent the i^{th} person ($i = 1, \ldots N$) in the population. The following regression function (equation 1) decomposes the observed logged earnings into the sum of three additive parts (in addition to a constant term): the treatment effect of college education, a linear combination of covariates, and the residual:

$$y_i = \alpha + \delta d_i + \boldsymbol{\beta}'\boldsymbol{X}_i + U_i, \qquad (1)$$

where y is the natural logarithm of earnings, d is a dummy variable representing whether or not the respondent completes college (d = 1 if yes; = 0 otherwise), $\boldsymbol{X}$ is a vector of earnings determinants that may also influence the probability of completing college, and U is the residual unexplained by the baseline model. The parameters $\boldsymbol{\beta}$ are regression coefficients measuring the change in log earnings associated with a unit change in the earnings determinants $\boldsymbol{X}$, which typically include several measures of family socioeconomic status, geographic residence, academic achievement, and, in many studies, some measure of mental ability. The exponential transformation of the regression coefficient δ represents the multiplicative increase in earnings associated with the receipt of a college degree, *ceteris paribus*, or holding all other factors constant. Note that in equation (1) δ is assumed to be an unknown constant parameter, invariant across all members in the population. Such a model has been widely used in sociology (e.g., McCall 2000; Zhou and Logan 1989).

If homogeneity is true, the main threat to causal inference is that a least squares regression of y on d, even controlling for $\boldsymbol{X}$, is subject to the first source of selection bias discussed above (Griliches 1977). Selection bias in this case may result from a non-zero correlation between U and d. Under the homogeneity assumption, the conventional wisdom is that OLS estimates of the economic return to schooling are thought to be upwardly biased (Griliches 1977; Hauser and Daymount 1977), as such factors as mental ability and work ethics are assumed to affect both education and earnings positively. The actual direction of the bias, however, has not been settled empirically. For instance, Ashenfelter and Krueger (1994) contend that OLS estimates of the effect of education on earnings are downwardly biased because such estimates are often below IV estimates of returns to schooling.

If we relax the unrealistic homogeneity assumption, whether OLS estimates are biased upward or downward may not have a clear, either-or answer: the OLS estimate is essentially a weighted average of heterogeneous effects, some of which are necessarily higher, while others are lower, than the population average (Angrist and Kruger 1999; Morgan and Winship 2007; Xie, Raudenbush, and Perez 2007). Under this more realistic conceptualization that there is underlying heterogeneity in the returns to education, individuals differ not only in background attributes but also in the economic benefits they reap from a college education (Angrist and Krueger 1999; Card 1999, 2001; Heckman and Robb 1985; Heckman, Urzua, and Vytlacil 2006). When the effect of a college education varies in the population, the return to schooling is a random variable and there is a distribution of effects (Heckman, Urzua, and Vytlacil 2006).

Three main approaches have been proposed to studying heterogeneous treatment effects. First, the switching regression model in economics, allowing for two different education returns between those who complete college and those who do not, capitalizes on the expected difference in treatment effects as the mechanism of selection to treatment (Heckman 1978; Roy 1951; Willis and Rosen 1979). This approach is highly parametric and depends on strong theory. Second, the presence of heterogeneous treatment effects changes the interpretation of the IV estimator to a local average treatment effect (LATE) that pertains only to units induced by the instrument (Angrist, Imbens, and Rubin 1996; Angrist and Krueger 1999; Heckman, Urza, and Vytlacil 2006; Imbens and Angrist 1994; Morgan and Winship 2007). This approach is also called "principal stratification," where members of the population are divided into strata based on their responsiveness to instruments (Frangakis and Rubin 2002). A limit form of the IV approach (i.e., with a continuous IV variable) is the marginal treatment effect (MTE), which focuses on the treatment effect for units at the margin of treatment, i.e. for whom the benefit of treatment equals that of non-treatment (Bjorklund and Moffitt 1987; Heckman, Urzua, and Vytlacil 2006). This approach is limited by the difficulty in finding a meaningful IV that affects treatment assignment directly but affects the outcome only indirectly through treatment; a weak IV may give rise to imprecise estimates (Bound, Jaeger, and Baker 1995).

Third, following a long tradition in the literature on returns to education that attempts to incorporate theoretically relevant covariates, such as measures of ability (Griliches and Mason 1972), a simple and straightforward approach is to use rich covariates and then assume ignorability, at least provisionally. This approach allows researchers to directly find empirical patterns of treatment effect heterogeneity as a function of observed covariates. A common approach is to examine the interaction between education and specific covariates that also influence the probability of attaining a college education on wages, such as race or gender (Barrow and Rouse 2005; Cain 1986; Welch 1973) or parents' education or occupation (Altonji and Dunn 1996; Hauser 1973; Olneck 1979). For the question of comparing returns to college between those who complete college and those who do not, however, the most meaningful interaction is between college education and the propensity of completing college. As Heckman, Urzua, and Vytlacil (2006) recently remarked, the propensity score plays "the central role in both selection and IV models" (p.392). Examining estimated effects by propensity scores allows us to directly observe trends in the heterogeneous causal effects by propensity score (Xie and Wu 2005) and/or aggregate heterogeneous effects to various population-level mean treatment effects, such as the average treatment effect (ATE), average treatment effect on the treated (TT), and treatment effect on the untreated (TUT) (e.g., Brand and Halaby 2006).

The empirical patterns approach hinges on the ignorability assumption, the assumption that college attainment is independent of potential outcomes within values of relevant observed covariates. This assumption is also called "unconfoundedness," or "selection on observables." While this assumption can never be verified and indeed should not be taken to be true in practice for observational data, its plausibility depends on the availability of observed covariates that measure differences between college graduates and non-college graduates. While researchers cannot plausibly claim to have controlled for all variables that may affect earnings, researchers can more plausibly claim to have controlled for all the pre-college covariates that may affect *both* the probability of completing college and earnings. Only covariates that affect both the treatment assignment and the outcome may potentially confound the observed relationship between treatment and outcome (e.g. Rubin 1997). In the case of the effects of education on earnings, it is reasonable to suspect that models that do not control for mental ability, for instance, do not satisfy ignorability. Still, measurement of a host of theoretically meaningful confounders renders ignorability tentatively more plausible, but not necessarily true. Results for causal inference under the ignorability assumption should thus be interpreted provisionally and cautiously, as we do later in this paper.

The choice among the three approaches in applied research is not clear-cut, as all three require unverifiable assumptions: the parametric assumption in the case of the switching regression model, the exclusion assumption in the case of the IV estimator, and the ignorability assumption in the case of the empirical patterns approach. We adopt the empirical patterns approach. While we do not think that the ignorability assumption is true, we appreciate that analyses under the ignorability assumption are the most that the data can tell us without additional unverifiable assumptions. Utilizing our strategy, we focus on differences across groups identified by propensities to complete college and adjudicate between two potential patterns in the heterogeneous effects of college completion on earnings: positive selection (persons most likely to benefit from college are most likely to attend) and negative selection (persons most likely to benefit from college are least likely to attend).

Positive Selection versus Negative Selection

Human capital theory in economics has proven an influential explanation for educational acquisition (Becker 1964; Mincer 1974). The core idea of traditional human capital theory is that a gradation in earnings by level of education reflects returns to individuals' rational investment in education. If we let λ represent the present-value of the lifetime economic return of college education, and c the cost of college education, attending college produces a net gain if $\lambda > c$, with the benefit thus defined as $\pi = \lambda - c$. The association between the returns to college and the decision to attend college is at the core of the more recent economics literature that links heterogeneity in returns to education to heterogeneous schooling behavior. Premised on principles of self-selection and comparative advantage, the thesis is that the most "college worthy" individuals, in the sense of having the highest returns to college, are the most likely to select into college (Averett and Burton 1996; Carneiro, Hansen, and Heckman 2003; Carneiro, Heckman, and Vytlacil 2001, 2007; Roy 1951; Willis and Rosen 1979). A central conjecture from this literature is that there should be positive selection because college education is assumed to be most attractive to those individuals who would benefit most from it.

The positive selection thesis is widely, albeit not universally, accepted in economics; nevertheless, it is more a theoretical argument than a proposition that can readily be subject to empirical tests. Empirical research in economics on choice has heavily depended on the revealed preference framework (e.g., Manski and Wise 1983; Train 2003). Applied to the research question on the returns to college education, the framework essentially states that the researcher can infer that $\lambda > c$ (i.e., positive selection), at least in expectation, if a person is observed to complete college education, and $\lambda \leq c$ otherwise. This research strategy was successfully implemented by Willis and Rosen (1979) in their classic study, which applied Roy's (1951) model to the college education question, with the difference in expected utility between college education and high school education determining the likelihood of college education. The Willis and Rosen approach has two weaknesses: (1) it is highly parametric (i.e., tri-variate normal distribution among error terms) and (2) it assumes homogeneity within each of the two regimes while allowing for regime-specific education coefficients. More recently, capitalizing on instrumental variables and non-linearity between propensity scores as predicted by instrumental variables and logged earnings, Carneiro, Heckman, and Vytlacil (2007) use the MTE approach and conclude that, due to self-selection, persons for whom the returns are the greatest are those who are most likely to attend college.

Population heterogeneity has troubled quantitative sociology for some time (Xie 2007). In the literature on college education, sociologists have recognized the heterogeneity in returns to college. Raftery and Hout (1993), for example, state that it "seems likely that the perceived benefit of education varies among individuals" as a function of individual attributes (p. 57). Like economists, sociologists infer that the choice of attending college can result from a cost-benefit analysis (Boudon 1974; Breen and Goldthorpe 1997; Raftery and Hout 1993). However, sociologists emphasize that the costs and benefits are not purely economic. For instance, in terms of costs, sociologists have considered both the financial burden on the family (Bouden 1974; Raftery and Hout 1993) and the difficulty of deviating from the norms expected from one's own socioeconomic background (Boudon 1974).

Moreover, in contrast to the strictly economic cost-benefit model of college attendance, ample sociological research indicates that the decision to attend college is influenced by a multiplicity of actors and factors at various institutional levels (Mare 2006). Beginning with the Blau-Duncan model, sociologists have recognized the significant role of family background, such as parents' education and occupation (Blau and Duncan 1967), family structure (Biblarz and Raftery 1999; McLanahan, and Sandefur 1994) and sibship size (Downey et al. 1999; Guo and VanWey 1999; Phillips 1999), on educational attainment.[2] Building from the Blau-Duncan model, the "Wisconsin model" of status attainment elaborated how family background affects educational attainment: parents' and significant others' encouragement influences educational aspirations that exert an effect on educational attainment, independent of family socioeconomic status and measured ability (Hauser, Tsai, and Sewell 1983; Sewell and Hauser 1975; Sewell and Shah 1967; Sewell, Haller, and Portes 1983; Sewell, Haller, and Ohlendorf 1970). Coleman (1988) also offered insight into why family factors influence children's attainment via the concept of social capital, or the resource of social relationships consisting of expectations, information channels, and social norms. Encouragement, expectations, and norms differ by family background, generating differential mechanisms of selection into college.

[2] Economists have likewise recognized the important role of family background (e.g. Ashenfelter and Rouse 1998), and ability (e.g. Cameron and Heckman 2001) in educational attainment. Heckman's (2007) work on socioemotional skills is also consistent with the main conclusions of this area of sociological research.

In addition, sociological work has developed a neo-Marxist conflict perspective to educational attainment that helps explain differences in educational attainment by social background. For instance, cultural capital scholars also stress the importance of family background on educational attainment, but emphasize the general cultural background, knowledge, disposition and skills that are passed on from parents to their children, and moreover argue that schools systematically reward the cultural capital of the advantaged classes and devalue that of the lower classes (Bourdieu 1977; DiMaggio 1982; Lareau 1987; Willis 1981). Social reproduction theorists further elaborate this theme: primary and secondary schools in fact train advantaged students to take up their positions at the top of the socioeconomic order, including pursuing post-secondary schooling, while conditioning the poor to accept their lower status in the class structure (Bowles and Gintis 1976; Collins 1971; MacLeod 1989). Indeed, many scholars have argued that the more differentiation that is built into the educational system, via stratification within primary and secondary schools, the more differentiation by family background that comes out (Gamoran and Mare 1989; Lucas 1999; Lucas 2001). In fact, some scholars have argued class barriers will persist so long as some advantaged students have not saturated a given educational threshold (Rafterey and Hout 1993),[3] albeit sometimes in different forms than schooling alone (Lucas 2001). In sum, sociologists, both status attainment and conflict scholars, have produced a substantial body of research indicating the consequential role of non-economic factors in affecting college enrollment, suggesting that high social background individuals are likely to go to college even in the absence of a rational economic cost-benefit analysis, whereas low social background individuals must overcome considerable odds to attend college.

Research in social stratification also provides a compelling theoretical and empirical basis for postulating differential expectations by social background for the effect of education on earnings. This research has shown that while there is little direct relation between origin and destination for college graduates, the relationship is strong among workers without college degrees (DiPrete and Grusky 1990; Hout 1988; Mare 1981; Raftery and Hout 1993; Shavit & Blossfeld 1993; Smith and Cheung 1986;

[3] The theory of maximally maintained inequality never fit the U.S. well (Hout, Rafterey, and Bell 1993; Hout and DiPrete 2006; Lucas 2001; Mare 1981).

Yamaguchi 1983). We depict this empirical pattern in Diagram 1. As the two unparallel lines illustrate, the dependency of social destination on social origin is less for persons with more education relative to persons with less education. If we change the perspective and examine returns to schooling, i.e. the difference in destination between college-educated and less-educated workers, as a function of social origin, this interaction pattern yields a smaller difference between college and non-college educated individuals from high social origin (δ_2) than for individuals from low social origin (δ_1). In other words, this body of sociological research has suggested heterogeneity in the return to a college education such that those individuals with relatively disadvantaged social backgrounds, or those with the lowest probability of completing college, have poor counterfactual positions and thus benefit the *most*, rather than the least, from completing college – that is, we should expect negative selection. This pattern results from the particularly poor labor market prospects for workers will low levels of education combined with low levels of other forms of capital, human, social, or cultural. Reflecting differential selection mechanisms, economic expectations between college and non-college goers with low propensity are greater than those between college and non-college goers with high propensity.

We now write out the behavioral model for college education as follows. Let d^* represent the potential likelihood that the i^{th} person completes college, and d_i the observed outcome ($d_i = 1$ if yes; = 0 otherwise). It is customary to relate the two through a threshold measurement model:

$$d_i = 1 \text{ if } d_i^* > 0; \qquad (2)$$

$$d_i = 0 \text{ otherwise.}$$

We further specify that the utility function is determined by a component due to observed covariates ($\boldsymbol{X}_i$), an unobserved self-selection term (π_i), and a residual (ε_i):

$$d_i^* = f(\boldsymbol{X}_i, \pi_i) + \varepsilon_i, \qquad (3)$$

where ε, is assumed to be independent of $\boldsymbol{X}$ and π. Note that π is endogenous, representing the unobserved component of the benefit of attending college. In $\boldsymbol{X}$ we can include various observed background characteristics associated with the probability of college attainment, although $\boldsymbol{X}$ can also include predictors of earnings and thus the systematic part of the expected gain of college education. The likelihood of completing college is high when $f(\boldsymbol{X}_i, \pi_i)$ is large. Writing out the model of equations (1)-(3) makes it easier to appreciate the key difference between the economic and non-economic factors influencing college attainment. In the traditional Roy-type college behavior model, conditional on X, what essentially drives the decision is the unobserved economic gain associated with college education: π, the self-selection component. Under a normalization of comparing two alternative choices, this means that a person would attend college only if he/she expected a positive economic benefit from acquiring a college education; this is the Willis-Rosen model in economics (Willis and Rosen 1979). In most sociological literature considering non-economic factors, familial, personal, and institutional characteristics dominate, so that the decision rule is determined primarily by observed covariates $\boldsymbol{X}$, with the self-selection component (π) given the secondary role or sometimes ignored.

As a research strategy, we invoke the ignorability assumption and thus assume away the unobserved self-selection component (π) as a first step of data analyses. Rather, we estimate the following mis-specified, reduced-form propensity score model, specifying a linear function for the influences of $\boldsymbol{X}_i$ on d_i^*:

$$d_i^* = \boldsymbol{\lambda}'\boldsymbol{X}_i + \nu_i, \quad (4).$$

We question how the misspecification influences our estimation of equation (4) for estimating propensity score-specific casual effects of college education on earnings. To properly interpret the results under the ignorability assumption, we need to consider $f(\boldsymbol{X}_i, \pi_i)$ in equation (3) carefully. The large body of sociological literature on college education suggests an interactive pattern at work: the decision to go to college among children from high-status families is dictated less by rational choice than that among children from low-status families (Boudon 1974). Given this interaction pattern, it seems reasonable that at the higher end of the estimated propensity of completing college (i.e., high values of $\boldsymbol{\lambda}'\boldsymbol{X}$), the influence of self-selection (i.e., π) is small. At the lower end of the estimated propensity of completing college (i.e., low values of $\boldsymbol{\lambda}'\boldsymbol{X}$), the influence of self-selection (i.e. π) is larger. When a person who is not expected to go to college given the observed characteristics actually does go to college, there are strong unobserved factors involved, one of which may well be the economic incentive. In other words, individuals' multiple logics generate selection processes that may vary across the ***X*** distribution (DiPrete and Engelhardt 2004). This line of reasoning leads us to formulate our *negative selection hypothesis*.[4]

There is already some direct empirical support for negative selection in the economics literature. With data from the National Longitudinal Survey of Youth 1979, Heckman, Tobias, and Vytlacil (2001) report results that suggest negative selection; they find that a randomly chosen person might expect to receive a 9 percent increase in wages due to college education, while those actually selecting into college receive about a 4 percent increase. Moreover, studies that have examined compulsory schooling laws, differences in the accessibility of schools, and similar features as instrumental variables for completed education have found that economic returns are as large as or larger than OLS estimates (see Card 2001 for a review). For example, Card (1995) uses college proximity as an instrument for schooling for young men and finds that the IV estimator is about 30 percent above the OLS estimator. He argues that accessibility matters more for individuals on the margin of continuing their education, such that college proximity has a larger effect for children of less-educated parents.[5] As we note above, if treatment effects are heterogeneous, IV estimates should be interpreted as LATE estimates, average effects that pertain only to the units whose treatment assignment status is affected by the inducement of the instrument. Card's (1995) estimates of high returns to education pertain to a sub-population of individuals with a low propensity to attain college education, and thus also suggest the presence of negative selection.

If the theory of negative selection is correct, how do we reconcile it with some previous studies that have found evidence for positive selection? We think that the answer may lie in the differences in covariates employed across studies. Empirical support for positive selection is sometimes based on models with a limited set of covariates, omitting key variables such as ability and high school academic

[4] A pattern of negative selection, in which some individuals exhibit an interest in loss aversion or preserving their gains, is what Kahneman and Tversky (1979) and Diprete and Engelhardt (2004) call "prospect theory."

[5] Instrumental variables estimates might also exceed OLS estimates if the instruments are correlated with ability (Carneiro and Heckman 2002).

performance as well as social-psychological variables such as parents' and teachers' encouragement. Missing these important confounders in empirical studies may well introduce a distortion to the observed pattern of selection from negative to positive. We explore this possibility later in the paper.

STATISTICAL MODELS

To fix ideas, we adopt the potential outcome approach to causal inference. The potential outcome approach has early roots in experimental designs (Neyman 1923) and economic theory (Roy 1951), has been extended and formalized for observational studies in statistics (e.g., Holland 1986; Rosenbaum and Rubin 1983, 1984; Rubin 1974), in economics (e.g., Heckman 2005; Manski 1995), and in sociology (e.g., Sobel 2000; Winship and Morgan 1999; Morgan and Winship 2007). The approach makes explicit the issues that concern the identification and estimation of causal effects. Let y be logged earnings and let d be a variable scored $d = 1$ for an individual who completes college and $d = 0$ for an individual who does not complete college. Then y_i^d for $d = \{0, 1\}$ are the potential values of the outcome variable for unit i, with y_i^1 the value if i completes college and y_i^0 the value if i does not complete college.

We ask for individual i, what the person's earnings would be if he or she received the treatment, i.e. completed college, compared to not receiving the treatment, or not completing college. The individual-level effect of college graduation on log earnings is defined as the following:

$$\delta_i = y_i^1 - y_i^0. \tag{5}$$

Only one of the two earnings values, i.e. y_i^1 or y_i^0, is actually observed, depending upon whether or not unit i actually completes college (Holland 1986). Causal inference is impossible at the individual level and thus always requires statistical analysis at the group level on the basis of some homogeneity assumption.

While we can compute mean earnings for workers with and without a college education, i.e. $E(y^1 \mid d = 1)$ and $E(y^0 \mid d = 0)$, we never know $E(y^1 \mid d = 0)$, the average earnings of non-college-educated workers had they attended college, or $E(y^0 \mid d = 1)$, the average earnings of college-educated workers had they not attended college. We can, however, assume ignorability or selection on observables:

$$E(y^0 \mid \boldsymbol{X}, d = 1) = E(y^0 \mid \boldsymbol{X}, d = 0) \tag{6a}$$

and

$$E(y^1 \mid \boldsymbol{X}, d = 0) = E(y^1 \mid \boldsymbol{X}, d = 1). \tag{6b}$$

Equation (6a) states that we assume that the average earnings of college-educated workers had they not completed college are the same as the average earnings of non-college-educated workers, conditional on the observed covariates; likewise, Equation (6b) states that we assume that the average earnings of non-college-educated workers had they completed college are the same as the average earnings of college-educated workers, conditional on the observed covariates. It is well known that higher education is a valuable social resource that is unevenly distributed across social groups defined by social background (Shavit, Arum, and Gamoran 2006); as we discuss above, sociological research informs us of a wide array of factors that influence educational attainment. If our set of covariates fails to include all such factors that influence both the probability of receiving a college education and earnings, the ignorability assumption is violated, and selection bias is likely present.[6]

Models for Heterogeneous Treatment Effects

When treatment effects are heterogeneous, there can be two types of selection bias, as we discuss above: pre-treatment heterogeneity bias and treatment-effect heterogeneity bias. Both types of biases may threaten the validity of causal inference with observational data. Estimators such as the fixed-effects estimator and the difference-in-differences estimator attempt to eliminate pre-treatment heterogeneity bias, but not treatment effect heterogeneity bias (Angrist and Krueger 1999).

If we allow the coefficient of treatment in equation (1) to be heterogeneous, i.e., δ_i, we can at least theoretically separate out the two types of heterogeneous components and equation (1) becomes the following:[7]

[6] Again, only covariates that meet both the conditions (affecting both the treatment assignment and the outcome) may potentially confound the observed relationship between treatment and outcome.

[7] Note that α in equation (1) can been viewed as heterogeneous, i.e., α_i, as it cannot be separated from the error term U_i. We write out the heterogeneous intercept explicitly in equation (7).

$$y_i = \alpha_i + \delta_i d_i + \boldsymbol{\beta}'\boldsymbol{X}_i + U_i, \tag{7}$$

In this model, α_i represents pre-treatment heterogeneity while δ_i represents treatment-effect heterogeneity. If there is pre-treatment heterogeneity bias, correlation $\rho(\alpha, d) \neq 0$. If there is treatment-effect heterogeneity bias, correlation $\rho(\delta, d) \neq 0$ (Heckman, Urzua, and Vytlaci 2006; Winship and Morgan 1999). The individual-level heterogeneity model is not identifiable, as α_i and δ_i cannot be separated from U_i without further constraints. Invoking the ignorability assumption, it is assumed that, conditional on $\boldsymbol{X}$, there is no unobserved confounder that causes either pre-treatment heterogeneity bias or treatment-effect heterogeneity bias. Implemented to equation (7), this assumption is tantamount to

$$\alpha \perp\!\!\!\perp d \mid \boldsymbol{X} \tag{8a}$$

$$\delta \perp\!\!\!\perp d \mid \boldsymbol{X} \tag{8b}$$

While equation (8) seems simple, making use of conditioning on $\boldsymbol{X}$, which is typically multi-dimensional, proves difficult due to the "curse of dimensionality"; we often cannot find both treated and untreated units with identical values on $\boldsymbol{X}$ if $\boldsymbol{X}$ is of a high dimension. However, the important work of Rosenbaum and Rubin (1983, 1984) shows that it is sufficient to condition on the propensity score as a function of $\boldsymbol{X}$. The propensity score is defined as the probability of assignment to the treatment group, i.e. college completion, given a set of observed covariates:

$$P = p(d_i = 1 \mid \boldsymbol{X}) \tag{9}$$

We can then reduce the conditioning dimension of $\boldsymbol{X}$ in equation (8), simplifying it to:

$$\alpha \perp\!\!\!\perp d \mid P \tag{10a}$$

$$\delta \perp\!\!\!\perp d \mid P. \tag{10b}$$

In this study, we evaluate heterogeneity in treatment effects by decomposing terms of heterogeneity in equation (7) into a nonparametric function of the propensity score and then utilize a hierarchical model to reveal a pattern of returns.[8] The economics and sociology literatures inform us of a

[8] Propensity score strata were used by Rosenbaum and Rubin (1984), although they did not look for the variation of treatment effects as a function of the propensity score.

variety of family and personal attributes that we use to predict college attendance; thus, we can divide a population into subpopulations with similar predicted propensity scores to complete education based on these observed attributes. Our analysis strategy assesses whether or not population heterogeneity in the propensity to complete college education is associated with heterogeneity in returns to college education; specifically, we ask if the estimated effect of college is positively or negatively associated with the estimated propensity to complete college. Our analyses proceed in four steps: (1) we estimate binary logistic regressions predicting the probability of completing college and derive propensity scores for each individual in the sample; (2) we group respondents into strata of estimated propensity scores to balance the distributions of the covariates between college graduates and non-college graduates ($p<.001$); (3) we estimate the treatment effects specific to balanced propensity score strata; and (4) we pool the results, allowing for heterogeneous treatment effects, and examine the trend in the variation of effects through a hierarchical linear model (Xie and Wu 2005; Xie, Raudenbusch, and Perez 2007).

DATA, MEASURES, AND DESCRIPTIVE STATISTICS

Data Description

To examine heterogeneous treatment effects of education on earnings, we utilize three large longitudinal datasets representing three different cohorts of Americans, each containing extensive information about respondents' social backgrounds, ability, and schooling experiences: (1) National Longitudinal Study of Youth 1979 (NLSY79)[9]; (2) National Longitudinal Study of the Class of 1972 (NLS72)[10]; and (3) Wisconsin Longitudinal Study (WLS57)[11]. All three samples are cohort-based.

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[9] The NLSY79 is sponsored by the Bureau of Labor Statistics of the U.S. Department of Labor. The survey is conducted under contract with the Center for Human Resource Research at the Ohio State University and the National Opinion Research Center at the University of Chicago. Additional funding is provided by the National Institute of Child Health and Human Development and the National Institute on Drug Abuse.

[10] The National Education Longitudinal Studies program consists of three major studies: the National Longitudinal Study of the High School Class of 1972 (NLS72), High School and Beyond (HS&B), and the National Education Longitudinal Study of 1988 (NELS:88). We considered examining these surveys as well. Unfortunately, at each respective last survey date, HS&B and NELS:88 respondents were too early in their careers for a life course analysis of labor market experiences.

[11] Since 1991, the WLS has been supported principally by the National Institute on Aging (AG-9775 and AG-21079), with additional support from the Vilas Estate Trust, the National Science Foundation, the Spencer

Single-cohort longitudinal surveys are advantageous as the potential confounding effect of cohort with experience is effectively controlled.

First, we examine data from the NLSY79. The NLSY79 is a nationally representative sample of 12,686 respondents who were 14-22 years old when they were first surveyed in 1979, the "late baby boom cohort." The NLSY79 consists of three sub-samples: (1) a cross-sectional sample of 6,111 respondents designed to be representative of non-institutionalized civilian 1979 youth; (2) a sample of 5,295 respondents designed to over-sample civilian Hispanic, black and economically disadvantaged 1979 youth; and (3) a sample of 1,280 respondents who were enlisted in the military as of 1978. These individuals were interviewed annually through 1994 and are currently interviewed on a biennial basis. The NLSY79 has been used extensively for the study of economic returns to education. We restrict our sample to respondents who were 14-17 years old at the baseline survey in 1979 (n = 5,581), who had not graduated from high school at the time the Armed Services Vocational Aptitude Battery (ASVAB) tests were administered (n = 3,885), who had completed at least the 12^{th} grade as of 1990 (n = 3,034), and who did not have any missing data on the set of covariates used in our analysis (n = 2,474). We set these sample restrictions to examine a single cohort with little age variation, to ensure that all measures we use are pre-college, and to compare college graduates to those respondents who completed at least a high school education. We evaluate the effects of completing college on earnings for respondents age 25-28 (in 1990), 29-32 (in 1994), 33-36 (in 1998), and 37-40 (in 2002), i.e. from early to mid-career earnings years.

As a result of sample restrictions on the basis of age, our sample size for the NLSY79 is fairly small. To overcome this limitation, and also to represent a different cohort, we examine a second national dataset, the NLS72. Following the "early baby boom" cohort, the NLS72 first surveyed a national probability sample of 22,652 high school seniors in the spring of 1972, and resurveyed the respondents in 1973, 1974, 1976, and 1979, and a subsample in 1986. The sample design for the NLS72 is a stratified,

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Foundation, and the Graduate School of the University of Wisconsin-Madison. A public use file of data from the Wisconsin Longitudinal Study is available from the Data and Program Library Service, University of Wisconsin – Madison, 1180 Observatory Drive, Madison, Wisconsin 53706 and at http://dpls.dacc.wisc.edu/WLS/wlsarch.htm.

two-stage probability sample of students from all schools containing twelfth graders during the 1971-72 school year, public and private, in the 50 states and the District of Columbia. Schools in low-income areas and schools with a high proportion of minority students were sampled at twice the rate used for the remaining schools. The data have been widely recognized as an excellent data source for studies of higher education (e.g., Manski and Wise 1983). By the fifth follow-up, sample members average 32 years of age and had been out of high school for fourteen years. We restrict our sample to respondents who were re-interviewed at the fifth follow-up (n = 14,489),[12] who had completed at least the 12th grade as of 1986 (n = 14,469), and who did not have any missing data on the set of covariates used in our analysis (n = 9,032).

While the NLSY79 and the NLS72 offer national representation and excellent data on educational experiences, we are limited in our ability to examine the effects of education on earnings beyond the early 40s. We are not similarly restricted with our third data source: a regional study of the WLS57. The WLS57 represents an additional cohort, the "World War II" cohort, contains a very reliable measure of cognitive ability in high school, and provides us with earnings measures at ages 32, 53, and 64. The WLS57 is a panel study based on a random sample of 10,317 men and women who graduated from Wisconsin high schools in 1957. Data were collected from parents of the graduates in 1964, from the graduates themselves in 1974 (at about age 35), in 1992 (at about age 53), and in 2004 (at about age 65). Past research has shown that for processes of socioeconomic attainment, the patterns found in the WLS57 mirror those found in national probability samples (Sheridan 2001). Thus, the issue of generalizability of results from the WLS57 to the national level may be more of an issue in theory than in practice. We restrict our sample to respondents who did not have any missing data on the set of covariates used in our analysis (n = 7,905).

[12] Unfortunately, only about 70 percent of non-college graduates were re-interviewed in the NLS72 1986 wave of data collection. Our analyses are therefore weighted to adjust for this sample selection of more educated respondents. We use educational attainment in the 4th follow up as our predictor of college completion due to missing data for educational attainment in the 5th follow-up.

Variable Measurement

Table 1 lists the pre-college exogenous variables we use to construct propensity score strata for each of our three data sources. Most of these measures have figured prominently in sociological studies of educational and occupational attainment, and their measurement is straightforward. There are, however, a few differences in the measurement of these variables across the datasets. Parents' income is measured as total net family income in 1979 dollars in the NLSY79, on a 10-point scale in 1972 in the NLS72, and as parents' income in 1957 dollars in the WLS57. "Residence/proximity to college or university" indicates whether a respondent's high school was within 15 miles of a college or university in the WLS57; we use an indicator of whether a respondent lived in an SMSA in 1979 in the NLSY79 and whether a respondent lived in a medium-to-large city or suburb in 1972 in the NLS72. College-track indicates whether or not the student was enrolled in a college-preparatory curriculum in the NLSY79, was enrolled in an academic track in the NLS72, or had completed the requirements for UW-Madison in the WLS57. The measurement of mental ability differs across the data sources. In 1980, 94 percent of the NLSY79 respondents were administered the ASVAB, a battery of ten intelligence tests measuring knowledge and skill in areas such as mathematics and language. We follow the practice of Cawley et al. (1997) and first residualize each of the ASVAB tests on age at the time of the test separately by race and gender. The residuals were standardized to mean zero and variance one. We then construct a scale of the standardized residuals ($\alpha = 0.92$). In the NLS72, we use Educational Testing Service (ETS) normalized test scores (mean of 50, standard deviation of 10) in math and verbal. In the WLS57, we use the 1957 Henmon-Nelson Test of Mental Ability scores.

We use hourly wages as the outcome variable in the logarithm form. In the NLSY79, our outcome is logged hourly wages and salary for the mid-20s through the late 30s to early 40s (in 1990, 1994, 1998, and 2002). In the NLS72, our outcome is logged hourly wages at age 32 (in 1986). In the WLS57, our outcome is logged yearly earnings at age 35 (in 1974), and logged hourly wages at age 53 and 64 (in 1992 and 2004, respectively).[13] As unemployed workers have zero wages, we add a small positive constant, 0.5, before taking the logs.

Descriptive Statistics

Descriptive statistics for all pre-college exogenous variables are shown in Table 1. As we discuss above, college graduates come from families with more advantaged social backgrounds. The descriptive statistics in Table 1 support these patterns: College graduates are more likely to have families with high incomes, highly educated parents, intact families, and few siblings than non-college graduates. In addition, individuals with higher levels of secondary school academic success and higher levels of cognitive ability are more likely to attain post-secondary schooling. Moreover, students who received higher levels of encouragement from teachers and parents to attend college and had friends who planned to attend college are more likely to complete college. Finally, the likelihood of completing college also varies by race and Hispanic origin, with whites and Asians more likely to do so than blacks and Hispanics. These patterns suggest that many non-economic factors figure in the educational attainment of youth.

MAIN ANALYSIS AND FINDINGS

College Returns under the Assumption of Homogeneity

Table 2 provides the estimated effects of college completion on earnings for each dataset, separately by sex, through regression analyses controlling for all the full set of covariates described above under the homogenous effect assumption.[14, 15] For NLSY79 men, college completion yields a highly significant positive effect on logged hourly wages that steadily increases over time, from a 43 percent advantage in men's mid-20s to a 56 percent advantage in their late 30s, consistent with the human capital

[13] In the WLS57, there is not an hourly wage measure in 1974 comparable to those available for 1992 and 2004.
[14] Appendix A provides estimates for the control variables for NLSY79 men and women. To reduce the number of supplementary tables, we show these control variable estimates only for the NLSY79.
[15] Results for the NLS72 are weighted by the probability of inclusion in the fifth follow-up.

model in economics. We find a comparable effect for NLS72 men: NLS72 college-educated men in their early 30s have a 45 percent advantage over non-college-educated men (compared to a 44 percent advantage for NLSY79 college-educated men at a similar age). Given widespread evidence for an increasing return associated with a college degree, it is not surprising that the effect of college completion is smaller in magnitude in the earlier WLS57 cohort. Still, results for WLS57 men indicate significant and steadily increasing returns associated with a college degree over the life course.

Patterns for women are somewhat different. In the NLSY79, results for women indicate a large significant effect of college completion for women in their mid-20s, a smaller and only marginally significant effect in their early 30s, and smaller still and insignificant effects in their mid- to late-30s.[16] NLS72 college-educated women in their early 30s have a roughly 28 percent advantage over non-college-educated women, comparable to NLSY79 women at a similar age, but it is only marginally significant. In contrast, in the WLS57, the effect of a college degree is significant for women at each stage we observe over the life course. The returns to college education increase appreciably over women's life courses in the WLS57 data, paralleling the returns for men and differing from those for women in the other two datasets. One reason may be that we are generally examining earnings returns in post-childbearing years for WLS57 women, but not for NLSY79 or NLS72 women. Moreover, women's labor force participation was much lower for the earliest cohort (57 percent of WLS57 women were employed at age 35 in comparison to 76 percent of NLSY79 women in their mid-30s), and thus more selective with respect to earnings than for the two later cohorts.

Generating Propensity Score Strata

Our next objective is to examine the heterogeneous effects of college completion by propensity score strata. To do so, we first generate propensity score strata within which the covariates are balanced. We begin by deriving estimated propensity scores $P(\boldsymbol{X})$ for each individual. We use binary logistic

[16] In the NLSY79 and NLS72, results pertain to individuals in early career stages and we therefore adjust for an indicator of marriage and the presence of children at age 25. Because results for women in the WLS correspond primarily to the post-child-bearing years for this cohort, we do not adjust for marriage and children in the WLS57.

regressions predicting the odds of completing college using the covariates described in Table 1 for each data source, separately by sex (results for the logistic regressions available from the authors upon request).[17] Table 3 provides the number of cases in each stratum, separately by college attainment, gender, and data source. As expected, the frequency distributions for the treatment and control groups run in opposite directions: In the case of college-educated individuals, the frequency count increases with the propensity scores whereas in the case of non-college-educated individuals the count decreases. There is overlap within each stratum; i.e. for each propensity score stratum there are individuals with $d = 1$ and other individuals with $d = 0$.[18] The balancing hypothesis is satisfied when within each interval of the propensity score the average propensity score and the means of each characteristic do not differ significantly between treated and control units.[19] We restrict the balancing algorithm to the region of common support; "common support" restricts analyses to regions of propensity scores in which both treated and control units are observed.[20]

Heterogeneous College Returns

Figures 1-6 present the main results of our study. We first estimate treatment effects specific to propensity score strata and then detect the pattern of treatment effects by propensity score with a hierarchical linear model (HLM). "Dots" in Figures 1-6 represent point estimates of stratum-specific effects of college completion on logged earnings. The linear plots, corresponding equations, and R^2 coefficients included in the figures are based on the HLMs, level-2 models with estimated treatment effects specific to propensity score strata regressing on propensity stratum rank. All point estimates and

[17] We used the Stata propensity score program designed by Becker and Ichino (2002) to generate balanced propensity score strata.

[18] In the NLSY79 and WLS57, we do not, however, have a sufficient number of non-college graduates in our final stratum for the HLM and therefore collapse our final two strata and adjust for the estimated propensity score in our level-1 analyses.

[19] To demonstrate the balance achieved within each stratum, Appendix B for NLSY79 men provides covariate means by propensity score strata. Again, to reduce the number of appendix tables, we show results only for NLSY79 men. In the NLS72, a small number of covariates were not balanced in two strata and we further adjust for these in our level-1 strata-specific regressions.

[20] That is, we exclude from the analyses cases that do not meet this requirement: 238 men and 74 women did not meet the requirement in the NLSY79; 211 men and 14 women did not meet the requirement in the NLS72; and 16 men and 590 women did not meet the requirement in the WLS57.

associated *t*-values corresponding to Figures 1-6 are provided in Appendix Table C.1 for men and Appendix Table C.2 for women.

Beginning with Figure 1, which depicts results for NLSY79 men's wages at age 25-28 (in 1990), age 29-32 (in 1994), age 33-36 (in 1998), and age 37-40 (in 2002), we find evidence for negative selection throughout the life course. The downward linear lines illustrate the declining trend in treatment effects with propensity stratum rank at every observed time period. For men in their mid-20s, a unit change in stratum rank (i.e., crossing a neighboring propensity score stratum) is associated with a 3 percent reduction in the treatment effect. For men in their early 30s, a unit change in stratum rank is associated with an 11 percent reduction in the treatment effect, such that the predicted effect of college completion on earnings in stratum 1 is over 70 percent, while the predicted effect in stratum 5 is under 30 percent. We also find evidence for negative selection for wages for men in their mid- and late 30s. We observe, as would be expected, that college completion is associated with an increasingly larger return over the life course. However, across the life course, the pattern of negative selection always holds true.[21]

Figure 2 presents results for NLS72 men's wages at age 32 (in 1986). These results are comparable to results for NLSY79 men in their early 30s. The results provide further support for the theory of negative selection for this older cohort of men. A unit change in stratum rank is generally associated with a 3 percent reduction in the treatment effect. The predicted gain associated with completing college decreases from about 55 percent in the first stratum to about 30 percent in the final stratum. As would be expected given the secular trend of the increasing importance of college completion on wages, a comparison of the results from NLS72 and NLSY79 indicates a larger benefit to completing college for the late baby boom cohort than for the early baby boom cohort. Still, results from

[21] Examining Appendix C.1 and C.2, we see that the wage gap between the treatment and control groups is statistically significant within many, but not every, propensity score stratum. Moreover, there are only a few level-2 slope coefficients that are statistically significant, as these coefficients are based on very few data points. The HLM model is used to provide an overall one-degree-of-freedom summary of the direction of the pattern of effect heterogeneity as a function of propensity score. In this sense, the statistical significance of the level-2 slope coefficient of the HLM model is not of primary interest. We observe, however, for example, that the selection pattern in Figure 1 is clearly negative.

both data sources suggest that the predicted benefit of completing college is the greatest among men least likely to complete college and declines with the estimated likelihood of completing college.

Figure 3 depicts results for WLS57 men's earnings at age 35 (in 1974), wages at age 53 (in 1992), and wages at age 64 (in 2004). Again, as would be expected given the increasing trend in the importance of college completion on earnings, we find the estimated returns to be higher for NLSY79 and NLS72 men than for WLS57 men, at least in their early to mid-30s. However, the results from WLS57 are consistent with those from the other two data sources in lending support for negative selection. Summarized in the HLM models, the level-2 β coefficients are very similar across all three cohorts. They reveal that the benefit to completing college is greatest among men least likely to complete college at every observed stage in the life course. As expected, the estimated effect of completing college on earnings generally increases over the life course, but this increasing trend is more pronounced in low-propensity strata than in high-propensity strata. The steepest slope, thus, occurs at age 64.[22] In sum, results for men depicted in Figures 1-3 overwhelmingly support the theory of negative selection.

We conducted the same analyses for women; the parallel results are presented in Figures 4-6. The results for NLSY79 women, depicted in Figure 4, are similar to those for NLSY79 men in that we find evidence for negative selection at each observed stage of the life course. The pattern for mid-20s earnings provides the weakest support for any direction of selection. The downward slope for NLS72 women at age 32, depicted in Figure 5, is comparable to the slope for NLSY79 women in their early 30s. As with men, results for the late baby boom cohort of women (the NLSY79 sample) indicate a larger benefit to completing college than for the early baby boom cohort (the NLS72 sample). Finally, Figure 6 depicts results for WLS57 women at ages 35, 53, and 64, providing further support for negative selection at every observed stage in the life course. For WLS57 women, the downward slope is comparable to the

[22] We postulate that the steep downward slope at age 64 may be at least partly explained by the fact that low propensity less-educated men are more likely to retire than high propensity less-educated men, and thus are excluded from the analysis, while the reverse is true among college-educated men, leading to a complex selection mechanism at this stage in the life course.

slope observed for NLSY79 women in their mid- to upper 30s, albeit steeper.[23] For older, post-childbearing women (at age 53 and 64), the downward effect of completing college across propensity score strata is less steep, bearing greater resemblance to pre-childbearing patterns we observe among the sample of women in the NLSY79.

We thus find support for negative selection for both men and women. Life course patterns by propensity score strata, however, differ markedly between men and women. Among low propensity women, returns to college increase over the life course, resembling that of men. The earnings return for high-propensity women is flatter over the life course. The gender differences in life-course patterns in economic returns to education may reflect the influences of gender roles in the family (Becker 1991; Mincer and Polacheck 1974). Traditional division of labor within families has placed responsibility for domestic work and child care primarily on women, and there is some evidence that women from more advantaged social backgrounds, in particular, may be more likely to assume traditional family roles due to a greater likelihood that their mothers specialized, and that they themselves may be married to men with economic resources sufficient for role-specialization within the family (Hill and Stafford 1974). Indeed, there is indirect evidence to this conjecture in our data. While labor force and fertility patterns are similar between low propensity educated and high propensity women in their 20s, the two groups diverge in their 30s, with high-propensity women more confirming to the patterns predicted by the human capital model – declining employment and increasing childbearing. As a result of intermittent labor force participation during childrearing and associated lower expectations of long-term career goals, high propensity women may receive less return to human capital investments while employed (Bianchi 1995; Budig and England 2001). Whereas the life-cycle patterns of low-propensity educated women resemble more closely those of men, low propensity non-college educated women overall have more children, have them earlier in the life course, and withdraw from the labor force more often.

In summary of figures 1-6, we have presented robust evidence for a strong selection mechanism at work: When individuals with a low latent propensity of completing college, i.e. those individuals from

[23] The trend toward delayed childbearing has been increasing in recent cohorts (Bianchi 1995).

the most disadvantaged social origins, actually complete college, they benefit the most from college in economic rewards. This finding holds for men and for women, at every observed stage in the life course, and for three different cohorts (across three different data sources).

AUXILIARY ANALYSES

Given the extensive evidence we have presented for negative selection, we now consider the question of causal mechanisms. It is plausible, indeed likely, that multiple mechanisms are accountable for this negative selection pattern. Towards this goal, we utilize several additional variables to determine whether low propensity college attendees are more economically driven than high propensity college attendees, for whom college attendance is a cultural expectation. In Figure 7, we examine a ratio of the importance of monetary factors to non-monetary factors in selecting a career across propensity score strata for college-educated men in the NLS72. It is indeed the case that men in low propensity score strata are more likely to state that monetary factors are more important than non-monetary factors than men in high propensity strata.[24] In Table 4, we explore this idea further examining stratum-specific college majors for college-educated men in the WLS57. While low propensity students are more likely to concentrate in business and education, majors that would yield immediate economic return, high propensity students are more likely to major in the sciences and humanities, subjects that require strong academic interests and are less likely to be motivated by immediate economic rewards.

We further test this idea with a measure of the value of college among high school seniors in the WLS57.[25] In Figure 8, we examine average values of college by propensity score strata and education among WLS57 men. We find a large differential between college graduates and non-college graduates in low propensity strata, but the gap gradually decreases across propensity score strata to almost no

[24] While all women state that non-monetary factors are more important than monetary ones, low propensity women are more likely to state that monetary factors are important, yielding a negative slope across propensity score strata comparable to that of men (results available upon request).
[25] The variable "value of college" was created by J. Michael Armer for his dissertation “Community and School Environments and College Plans of Public High School Seniors,” University of Wisconsin, 1964. See WLS Memo 129 for variable construction details.

difference among those in the highest propensity stratum. The atypically high value placed upon college by disadvantaged youth who actually completed college stands in contrast to the uniformly high value (i.e., undifferentiated by actual college completion status) placed upon college among advantaged youth.

Further, we return to the issue of differential counterfactual expectations by propensity score strata and educational status. Empirical patterns across our three data sources generally are consistent with our hypothesized interaction effect depicted in Diagram 1. That is, as we would expect given prior sociological research and current labor market trends, workers with disadvantaged social origins and low ability who do not go to college have particularly poor labor market prospects. Thus, we note that the pattern of negative selection emerges not because low propensity college goers earn more than high propensity college goers; they do not. Rather, the pattern emerges because low propensity non-college goers earn so little. Taken together, the evidence from our auxiliary analyses suggests that college graduates from disadvantaged social backgrounds are more inclined to view college as a means for economic mobility than students from more advantaged social backgrounds.

Our robust results supporting negative selection beg a question: Why have some prior studies found evidence for positive selection? We suspect that one reason lies in the choice, or availability, of covariates in the analyses. Some studies that have found evidence for positive selection are limited to a small set of covariates, missing key variables like mental ability [e.g., Carneiro, Heckman, and Vytlacil (2001) using data from the PSID]. If the set of covariates fail to include key factors influencing the probability of attending college and future earnings, estimates may be biased. This bias may be greater in high propensity strata than in low propensity strata, leading to inflated evidence for positive selection. To test this possibility, we now pretend that we do not have access to the full set of covariates and restrict covariates in the WLS57 to a set comparable to that used in Carneiro, Heckman, and Vytlacil (2001);[26, 27]

[26] We use the WLS57 for this analysis because of the rich set of covariates at our disposal. We restrict our analysis to age 35 men's earnings as the prior study to which we are referring did not examine effects for men in their 50s and 60s, or for women.

[27] Carneiro, Heckman, and Vytlacil (2001) do not accept the ignorability assumption, and therefore their approach differs from ours. Thus, another possible reason for the difference in results may be difference in approach. We do not explore this issue here.

that is, we omit ability and academics, social-psychological variables, and religion from our models. As expected, we find selection bias with the omission of these covariates. For example, we observe a roughly 30 percent difference in mental ability between college and non-college men when we omit ability from the model. Under the assumption of homogeneity, we find an average 28 percent increase in earnings ($p < 0.000$) as a result of completing college for WLS57 men at age 35 (Table 2). Of course, omitting a key selection variable that affects college completion and earnings positively (such as mental ability) yields a substantially larger estimated effect of college on earnings.

What would happen to our earlier results indicating negative selection had the ignorability assumption been violated by only controlling for this more limited set of covariates? To answer this question, Figure 9 shows results for WLS57 men at age 35 omitting the aforementioned variables. We observe that when we restrict models to a limited set of covariates there is (weak) evidence for positive selection. Note that this figure should be compared to Figure 3, as the analysis is the same for the same sample, the difference lying in the specification of covariates for the propensity model. Thus, the omission of these variables results not only in an overall bias (as in the case when treatment effects are assumed to be homogeneous), but also in changing the direction of association between propensity of treatment and treatment effects. With a full set of covariates, we observe negative selection. When we trim covariates to a more limited set, we observe positive selection.

SUMMARY AND DISCUSSION

Heterogeneity in response to a common treatment is a norm rather than an exception. Individuals differ not only in background attributes but also in how they respond to a particular treatment. An important task of sociological research is to summarize systematic patterns in population variability, a longstanding demographic tradition that Xie (2007) attributes to Otis Dudley Duncan. We consider population heterogeneity in returns to schooling, examining the effects of completing college by propensity score strata in a hierarchical linear model. We ask whether patterns of population heterogeneity reflect positive or negative selection, i.e. whether economic benefits of college are greater among persons most or least likely to complete college. We find overwhelming evidence in favor of negative selection: Individuals who are most likely to benefit from a college education are the *least* likely to obtain one. We find evidence for negative selection for both men and for women, for many observed stages over the life course, and for three different cohorts. We note some interesting differences between men and women patterned by propensity score strata across the life course and by cohort, reflecting differences across strata in women's labor market intermittency during childbearing years as well as the significant overall changes that have occurred in women's labor force participation over the periods we study. We plan to further analyze women's labor market participation and fertility patterns in greater depth in future research.

We have used the propensity score in identifying heterogeneous treatment effects. We realize that focusing on heterogeneity in treatment effects by observed covariates is limited, as we overlook heterogeneity due to unobserved variables. However, we have shown several benefits in focusing on observable heterogeneity in treatment effects. Although treatment effect heterogeneity is potentially observable, as we have shown; it is seldom studied in empirical sociological research. With a focus on observable heterogeneity, we uncover an important finding indicating that the most disadvantaged individuals with respect to observed social background, achievement, and measured ability are the most likely to benefit from a college education. By examining additional observed covariates, we found evidence for differential selection mechanisms by social background. Individuals with disadvantaged social backgrounds who attend college may wittingly utilize the educational system as a means for economic mobility, a rational utilization considering their counterfactual positions, while those with advantaged social backgrounds, for whom college is a cultural norm, may be less purposively driven by economic rationale. Moreover, we present evidence suggesting that empirical support for positive selection in prior research may be a product of missing certain key variables. As a result, we demonstrate that the plausibility of the ignorability assumption depends on the richness of the observed covariates.

The increasing demand for educated, skilled workers alongside the decreasing demand for less-educated, less-skilled workers results in an earnings differential between educated and less-educated workers; we find that this differential is especially large among individuals with a low propensity of completing college. Thus, a central component to the relatively large economic return experienced by low propensity college-educated workers is that their social position, coming from disadvantaged socioeconomic origins coupled with little education, is marked by substantial disadvantage. In the absence of a college degree, low propensity men and women have limited human, cultural, and social capital and hence particularly limited labor market prospects; in contrast, individuals with more advantaged social backgrounds, in the absence of a college degree, may still rely on their advantaged background and ability. Still, we remind the reader that a higher probability of attaining a college degree is still the most important causal mechanism for realizing the advantage associated with high socioeconomic origins, a key finding of the classic Blau and Duncan (1967) study.

The widespread belief in the socioeconomic return to higher education has prompted policy efforts that support the increased educational opportunities for all Americans. However, in the presence of heterogeneous treatment effects, there is no simple summary statement that can be invoked as to the benefit of completing college, either for those already receiving higher education or for those who are likely to benefit from such policies. The average benefit depends on the composition at any given time of the group of students who complete college. While many if not most policy makers at least implicitly assume homogeneity in the return to schooling, potential heterogeneity in returns to schooling has been receiving more attention as many countries are experiencing rapid expansion in college enrollment, leading to a questioning of the relative costs and benefits of higher education for those who were not previously receiving it. If there is positive selection, discounting general equilibrium effects, the expansion of higher education to a larger population would lower the average rate of return among those who receive it. In contrast, if there is negative selection, and again we ignore general equilibrium effects, the expansion of higher education results in *higher* average gains. Consistent with the negative selection hypothesis, the expansion of the American educational system has been accompanied by an overall increase, not decrease, in returns to education and promotion of openness in society (Hout 2000; Mare 1995). While this increase has been commonly explained as resulting from the increasing reliance on technology in the post-industrial economy (Levy 1998), our work provides an alternative explanation that the increase in the average return may have been due to the negative selection pattern of returns to college education. To those who doubt the utility of educational recruitment efforts, we provide evidence suggesting that a college education may be particularly advantageous among groups targeted by such efforts.

While our findings afford cause for optimism that continued educational expansion could lead to a reduction in economic inequality, there is nevertheless reason for continued concern. First, a trend toward increasing educational attainment across all groups of students is by no means guaranteed. Indeed, there is evidence to suggest widening gaps in college enrollment by class and race (Kane 2004). Factors such as decreasing public support for higher education (Hout 2000) and increasing use of grade retention and high stakes testing (Hauser 2004) may further contribute to slowing educational expansion among disadvantaged groups. Second, even in the event of increasing educational attainment among students with disadvantaged family backgrounds, there is the possibility that future countercurrents modify the pattern of negative selection. High propensity college graduates may effectively maintain inequality by attaining even higher levels of education, leading to an increased association between origins and destinations among college-educated workers (Lucas 2001; Raftery and Hout 1993). Moreover, if the market for less-skilled workers continues to deteriorate, less-educated workers, of both low *and* high propensities alike, may well face declining labor market opportunities in the future. Finally, if there are differential selection mechanisms across propensity score strata such that low propensity students who have completed college are fundamentally different, in unobserved attributes, than low propensity students who do not complete college, the benefit may not equally accrue as the composition of those who complete college shifts. Future research should continue to assess these important research and policy questions.

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Table 1. Descriptive Statistics of Pre-College Exogenous Covariates

Variables	NLSY79 Means				NLS72 Means				WLS57 Means			
	Men (n=1265)		Women (n=1209)		Men (n=4356)		Women (n=4676)		Men (n=3690)		Women (4215)	
	Non-College Grad.	College Grad.	Non-College Grad.	College Grad.	Non-College Grad.	College Grad.	Non-College Grad.	College Grad.	Non-College Grad.	College Grad.	Non-College Grad.	College Grad.
Race												
Black	0.18	0.07	0.15	0.07	0.06	0.04	0.09	0.07	---	---	---	---
Hispanic	0.07	0.03	0.07	0.03	0.04	0.01	0.04	0.01	---	---	---	---
Social backgrounc												
Parents' income*	17870	26538	18174	25991	5.6	7.14	5.28	6.83	5605	8123	5622	9262
Mother's educatior	11.26	13.32	11.18	13.37	12.48	13.53	12.35	13.60	10.15	11.56	9.94	12.02
Father's educatior	11.23	14.39	11.16	14.14	12.68	14.41	12.67	14.41	9.10	11.37	9.21	11.79
Intact family (0-1)	0.72	0.83	0.67	0.85	---	---	---	---	0.90	0.92	0.90	0.92
Number of siblings	3.29	2.34	3.40	2.45	2.05	1.96	2.18	2.01	3.45	2.61	3.51	2.40
Rural residence (0-1)	0.25	0.19	0.24	0.21	0.27	0.15	0.24	0.15	0.22	0.12	0.20	0.16
Urban res. / prox. to coll.*	0.77	0.78	0.75	0.80	0.73	0.72	0.72	0.71	0.42	0.50	0.50	0.53
Jewish (0-1)	0.00	0.03	0.00	0.04	0.01	0.06	0.01	0.06	0.00	0.02	0.00	0.03
Ability and academics												
Class rank	---	---	---	---	42.20	68.87	54.60	77.43	35.76	65.49	53.78	79.51
Mental ability (IQ)*	-0.09	0.69	-0.04	0.64	---	---	---	---	97.03	111.75	98.67	112.00
Mental ability (math)	---	---	---	---	50.35	59.62	48.20	57.08	---	---	---	---
Mental ability (verbal)	---	---	---	---	49.38	57.10	50.09	57.99	---	---	---	---
College track (0-1)*	0.23	0.59	0.23	0.49	0.35	0.81	0.30	0.82	0.54	0.91	0.46	0.89
Social-psychological												
Teachers' encouragemen	---	---	---	---	0.59	0.80	0.61	0.83	0.35	0.75	0.36	0.77
Parents' encouragemen	---	---	---	---	0.44	0.92	0.36	0.92	0.47	0.91	0.39	0.90
Friends' plans	0.42	0.79	0.48	0.81	0.47	0.88	0.50	0.89	0.22	0.66	0.30	0.76
Weighted Sample Proportion	0.76	0.24	0.77	0.23	0.70	0.30	0.74	0.26	0.69	0.31	0.82	0.18

Notes: Parents' income is measured as total net family income in 1979 dollars in the NLSY79, on a 10 point scale in 1972 in the NLS72, and in 1957 dollars in the WLS57. "Urban res. / proximity to coll." indicates whether a respondent lived in an SMSA in the NLSY79, whether a respondent lived in a medium to large city or suburb in the NLS72, and whether a respondent's high school was within 15 miles of a college or university in the WLS57. Ability is measured with a scale of standardized residuals of the ASVAB in the NLSY79, with normalized ETS math and verbal scores in the NLS72, and with the Henmon-Nelson IQ test in the WLS57. College-track indicates whether the student was enrolled in a college-preparatory curriculum in the NLSY79, whether the student was in an academic track in the NLS72, and whether the student completed the requirements for UW-Madison in the WLS57.

Table 2. Effects of College Completion on Log Wages under the Assumption of Homogeneity

	Men	Women
NLSY79		
1990 Wages	0.428 ***	0.460 ***
(age 25-28)	(3.96)	(4.09)
1994 Wages	0.439 ***	0.239 †
(age 29-32)	(3.81)	(1.80)
1998 Wages	0.527 ***	0.198
(age 33-36)	(4.51)	(1.42)
2002 Wages	0.563 ***	0.174
(age 37-40)	(4.18)	(1.06)
NLS72		
1986 Wages	0.452 ***	0.280 †
(age 32)	(4.15)	(1.90)
WLS57		
1974 Earnings	0.154 *	0.334 **
(age 35)	(2.08)	(2.83)
1992 Wages	0.428 ***	0.462 ***
(age 53)	(7.92)	(6.95)
2004 Wages	0.597 ***	0.506 ***
(age 64)	(4.00)	(3.93)

Note: Numbers in parentheses are t-ratios.
Treatment effects are conditional upon the set of covariates for each data source described in Table 1. NLSY79 estimates further condition on age at baseline. NLSY79 and NLS72 results for women also condition on an indicator for married with children at age 25. The "small set of covariates" omits religion, ability and academics, and the social-psychological variables. All outcome variables indicate current hourly wages, except for WLS57 1974 earnings, which indicate current yearly earnings. Wages for WLS57 in 2004 are treated as missing if the respondents retired. Full models for the NLSY are shown in Appendix B.

† p < .10 * p <. 05 ** p < .01 *** p < .001 (two-tailed tests)

Table 3. Frequency Counts per Propensity Score Stratum

NLSY79						NLS72						WLS57					
Men			Women			Men			Women			Men			Women		
P-Score	***d*=0**	***d*=1**	**P-Score**	***d*=0**	***d*=1**	**P-Score**	***d*=0**	***d*=1**	**P-Score**	***d*=0**	***d*=1**	**P-Score**	***d*=0**	***d*=1**	**P-Score**	***d*=0**	***d*=1**
[.00, .10)	454	20	[.00, .05)	573	12	[.00, .025)	642	11	[.00, .025)	1231	26	[.00, .05)	931	28	[.00, .025)	908	11
[.10, .20)	135	25	[.05, .10)	181	17	[.025, ..05)	382	18	[.025, .05)	357	29	[.05, .10)	418	33	[.025, .05)	459	16
[.20, .40)	130	43	[.10, .20)	156	28	[.05, .10)	326	39	[..05, .10)	352	30	[.10, .15)	255	25	[.05, .10)	441	38
[.40, .60)	52	65	[.20, .40)	147	47	[.10, .20)	322	91	[.10, .15)	190	41	[.15, .20)	155	45	[.10, .20)	367	67
[.60, 1.00)	27	76	[.40, .60)	37	48	[.20, .30)	207	99	[.15, .20)	134	55	[.20, .40)	386	149	[.20, .40)	391	172
---	---	---	[.60, 1.00)	19	55	[.30, .40)	143	151	[.20, .30)	214	88	[.40, .60)	208	200	[.40, .60)	204	185
---	---	---	---	---	---	[.40, .60)	263	368	[.30, .40)	153	121	[.60, .70)	72	122	[.60, 1.00)	101	265
---	---	---	---	---	---	[.60, .80)	164	523	[.40, .60)	251	375	[.70, .80)	46	173	---	---	---
---	---	---	---	---	---	[.80, 1.00)	46	350	[.60, .80)	161	487	[.80, 1.00)	48	380	---	---	---
---	---	---	---	---	---	---	---	---	[.80, 1.00)	28	339	---	---	---	---	---	---

Table 4. College Majors for College-Educated Men: WLS57

College Major	Percentage in each major by Propensity Score Strata								
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
physical science	0.00	0.06	0.04	0.02	0.03	0.05	0.05	0.04	0.05
math	0.00	0.06	0.04	0.02	0.06	0.09	0.08	0.04	0.05
biological science	0.11	0.03	0.04	0.02	0.09	0.09	0.11	0.07	0.12
engineering	0.04	0.06	0.13	0.12	0.06	0.14	0.13	0.23	0.22
pre-professional	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.02
computer science	0.04	0.00	0.04	0.00	0.01	0.02	0.01	0.01	0.01
business	0.19	0.27	0.17	0.19	0.16	0.15	0.10	0.11	0.10
social science	0.15	0.15	0.25	0.17	0.18	0.19	0.10	0.22	0.21
humanities	0.04	0.03	0.00	0.10	0.13	0.08	0.13	0.11	0.10
art and music	0.11	0.09	0.04	0.07	0.04	0.05	0.05	0.01	0.05
education	0.22	0.18	0.21	0.14	0.15	0.08	0.07	0.06	0.05
communications	0.04	0.03	0.00	0.02	0.06	0.01	0.01	0.04	0.01
agriculture	0.04	0.00	0.00	0.02	0.01	0.01	0.02	0.04	0.01
other	0.04	0.03	0.04	0.10	0.02	0.03	0.03	0.04	0.02
Number	27	33	24	42	145	196	120	171	375

Appendix A. Full Models of the Effects of College Completion on Log Wages under the Assumption of Homogeneity: NLSY79

	Men				Women			
Variables	**1990 Earnings (age 25-28)**	**1994 Earnings (age 29-32)**	**1998 Earnings (age 33-36)**	**2002 Earnings (age 37-40)**	**1990 Earnings (age 25-28)**	**1994 Earnings (age 29-32)**	**1998 Earnings (age 33-36)**	**2002 Earnings (age 37-40)**
College degree	0.428 ***	0.439 ***	0.527 ***	0.562 ***	0.460 ***	0.239 †	0.198	0.174
	(3.96)	(3.81)	(4.51)	(4.18)	(4.09)	(1.80)	(1.42)	(1.06)
Black	-0.376 ***	-0.499 ***	-0.394 ***	-0.682 ***	-0.092	-0.257 *	-0.100	-0.166
	(4.18)	(4.98)	(3.89)	(5.80)	(0.96)	(2.14)	(0.81)	(1.15)
Hispanic	-0.067	-0.100	-0.084	-0.242 †	-0.037	-0.081	0.034	0.131
	(0.57)	(0.80)	(0.67)	(1.69)	(0.33)	(0.60)	(0.24)	(0.77)
Parents' income	0.000 †	0.000	0.000	0.000	0.000 ***	0.000	0.000	0.000
	(1.85)	(1.20)	(0.11)	(1.02)	(3.61)	(0.81)	(1.10)	(0.59)
(Parents' inc.)^2	0.000	0.000	0.000	0.000	0.000 *	0.000	0.000	0.000
	(1.48)	(0.64)	(0.52)	(1.00)	(2.43)	(0.86)	(0.35)	(0.34)
Mother's edu.	0.048	0.038	-0.003	0.022	0.014	-0.031	0.094	0.040
	(0.96)	(0.73)	(0.05)	(0.37)	(0.28)	(0.53)	(1.47)	(0.53)
(Mother's edu.)^2	-0.002	-0.002	0.000	-0.004	-0.022	0.000	-0.006 *	-0.001
	(0.93)	(0.72)	(0.07)	(1.23)	(0.89)	(0.10)	(2.07)	(0.40)
Father's edu.	-0.024 †	-0.013	-0.003	0.007	0.013	0.010	-0.025	-0.031
	(1.82)	(0.90)	(0.23)	(0.41)	(0.92)	(0.58)	(1.44)	(1.55)
Intact family	0.101	0.159 †	0.091	0.133	-0.008	0.049	-0.126	-0.167
	(1.26)	(1.78)	(1.01)	(1.28)	(0.10)	(0.49)	(1.19)	(1.37)
Num. of siblings	-0.008	-0.011	-0.012	-0.038 *	-0.032 *	-0.019	-0.021	-0.012
	(0.50)	(0.64)	(0.73)	(2.04)	(2.03)	(1.02)	(1.07)	(0.55)
Rural res.	0.146	0.125	0.167 †	0.056	0.044	0.108	-0.013	-0.166
	(1.62)	(1.28)	(1.69)	(0.49)	(0.49)	(1.00)	(0.12)	(1.25)
Availability coll.	0.064	-0.020	0.112	0.134	0.040	-0.059	-0.057	0.133
	(0.72)	(0.21)	(1.13)	(1.18)	(0.45)	(0.55)	(0.50)	(1.01)

Appendix A (cont.). Full Models of the Effects of College Completion on Log Wages under the Assumption of Homogeneity: NLSY79

Jewish	0.322	0.181	0.203	0.549	0.522	0.925	0.577	-1.350
	(0.82)	(0.47)	(0.50)	(1.23)	(0.97)	(1.26)	(0.84)	(1.60)
Mental ability	0.148 **	0.240 ***	0.307 ***	0.346 ***	0.200 **	0.385 ***	0.371 ***	0.379 ***
	(2.76)	(4.11)	(5.12)	(5.10)	(3.33)	(5.25)	(4.70)	(4.30)
(Mental ability)^2	-0.031	-0.051	0.003	-0.041	-0.222 ***	-0.124 *	-0.127 †	-0.190 *
	(0.72)	(1.08)	(0.05)	(0.72)	(4.33)	(2.00)	(1.84)	(2.60)
College track	0.049	-0.051	0.042	0.151	0.163 †	0.108	0.083	0.014
	(0.57)	(0.55)	(0.45)	(1.41)	(1.79)	(1.00)	(0.73)	(0.10)
Friends' plans	0.119	0.056	-0.015	-0.020	-0.024	0.004	0.118	0.044
	(1.57)	(0.69)	(0.18)	(0.21)	(0.31)	(0.04)	(1.17)	(0.38)
Age	-0.025	-0.012	-0.007	-0.038	-0.143 **	-0.139 **	-0.032	-0.138 *
	(0.66)	(0.30)	(0.17)	(0.79)	(3.48)	(2.77)	(0.61)	(2.22)
Marr/kids age 25	---	---	---	---	-0.457 ***	-0.293 **	-0.019	0.060
					(5.85)	(3.10)	(0.19)	(0.51)
Sample size	1225	1031	1009	908	1184	1008	976	878

Note: Numbers in parentheses are t-ratios. † p < .10 * p <. 05 ** p < .01 *** p < .001 (two-tailed tests)

Appendix B. Covariate Balance by Propensity Score Strata: NLSY79 Men

	Strata 1		Strata 2		Strata 3		Strata 4		Strata 5	
Variables	**Non-Coll. Grad.**	**Coll. Grad.**	**Non-Coll. Grad.**	**Coll. Grad.**	**Non-Coll. Grad.**	**Coll. Grad.**	**Non-Coll. Grad.**	**Coll. Grad.**	**Non-Coll. Grad.**	**Coll. Grad.**
Black	0.37	0.25	0.21	0.40	0.23	0.23	0.19	0.12	0.15	0.07
Hispanic	0.18	0.30	0.12	0.08	0.12	0.09	0.12	0.09	0.07	0.05
Parents' income	13381	12253	17614	18482	19324	18422	23062	21348	23469	34702
Mother's edu.	10.31	10.05	11.67	12.16	11.98	12.21	12.71	12.54	13.67	14.79
Father's edu.	10.17	9.95	11.79	10.72	12.08	12.53	13.33	13.97	15.11	16.30
Intact family	0.63	0.55	0.63	0.80	0.74	0.67	0.85	0.80	0.85	0.91
Num. of siblings	3.84	4.05	3.04	3.04	2.64	2.47	2.88	2.46	2.04	2.17
Rural res.	0.21	0.30	0.26	0.20	0.21	0.21	0.19	0.12	0.11	0.20
Availability coll.	0.76	0.70	0.80	0.84	0.75	0.77	0.73	0.77	0.81	0.78
Jewish	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.02	0.04	0.08
Mental ability	-0.14	-0.01	0.31	0.48	0.62	0.57	0.79	0.76	0.90	1.05
College track	0.17	0.16	0.32	0.37	0.41	0.52	0.57	0.55	0.83	0.73
Friends' plans	0.35	0.55	0.61	0.52	0.66	0.74	0.90	0.85	0.93	0.93

Appendix C.1. Effects of College Completion on Log Wages by Propensity Score Strata: Men

	Level-1 Slopes									Level-2 Slopes
	Strata 1	**Strata 2**	**Strata 3**	**Strata 4**	**Strata 5**	**Strata 6**	**Strata 7**	**Strata 8**	**Strata 9**	
NLSY79										
1990 Earnings	0.494 †	0.190	0.629 **	0.467 *	0.228	---	---	---	---	-0.026
(age 25-28)	(1.68)	(0.67)	(2.77)	(2.37)	(0.90)					(0.38)
1994 Earnings	0.915 **	0.563 †	0.074	0.764 **	0.246	---	---	---	---	-0.114
(age 29-32)	(2.38)	(1.90)	(0.34)	(3.34)	(0.94)					(1.03)
1998 Earnings	0.733 *	0.653 *	0.418 *	0.606 **	0.443 †	---	---	---	---	-0.063
(age 33-36)	(2.19)	(2.01)	(2.08)	(2.82)	(1.82)					(1.84) †
2002 Earnings	0.995 **	-0.150	0.877 **	0.757 *	0.243	---	---	---	---	-0.060
(age 37-40)	(2.73)	(0.45)	(3.24)	(2.51)	(0.91)					(0.35)
NLS72										
1986 Earnings	1.511 **	0.666	-0.394	-0.052	0.679 *	-0.337	0.398 †	1.005 ***	0.671	-0.028
(age 32)	(3.11)	(1.05)	(0.83)	(0.16)	(2.18)	(1.09)	(1.88)	(4.13)	(1.57)	(0.33)
WLS57										
1974 Earnings	0.396	-0.064	-0.173	0.478	0.361 *	0.322 *	-0.283	-0.062	-0.023	-0.034
(age 35)	(1.17)	(0.23)	(0.57)	(1.64)	(2.55)	(2.03)	(1.13)	(0.23)	(0.09)	(0.93)
1992 Earnings	0.482 *	0.407 †	0.346	0.656 **	0.461 ***	0.301 *	0.301 †	0.424 *	0.485 *	-0.006
(age 52)	(2.25)	(1.93)	(1.24)	(3.28)	(4.04)	(2.59)	(1.81)	(2.04)	(2.60)	(0.42)
2004 Earnings	0.446	0.994 †	1.384 *	1.667 **	0.985 **	0.188	0.136	0.336	0.550	-0.092
(age 64)	(0.71)	(1.96)	(2.25)	(3.42)	(3.20)	(0.54)	(0.26)	(0.69)	(1.10)	(1.39)
***WLS57* Small Set of Covariates**										
1974 Earnings	0.358 **	0.089	0.435 **	0.227	0.082	0.312	0.381	---	---	0.006
(age 35)	(2.61)	(0.77)	(3.39)	(1.08)	(0.40)	(1.37)	(1.50)			(0.20)

Note: Numbers in parentheses are t-ratios. † p < .10 * p <. 05 ** p < .01 *** p < .001 (two-tailed tests)

Appendix C.2. Effects of College Completion on Log Wages by Propensity Score Strata: Women

	Level-1 Slopes										Level-2 Slopes
	Strata 1	**Strata 2**	**Strata 3**	**Strata 4**	**Strata 5**	**Strata 6**	**Strata 7**	**Strata 8**	**Strata 9**	**Strata 10**	
NLSY79											
1990 Earnings	0.278	0.134	0.673 **	0.501 *	0.734 **	-0.041	---	---	---	---	0.001
(age 25-28)	(0.63)	(0.40)	(2.74)	(2.54)	(3.04)	(0.09)					(0.01)
1994 Earnings	0.626	0.207	0.023	0.124	0.503	0.151	---	---	---	---	-0.040
(age 29-32)	(1.26)	(0.50)	(0.07)	(0.51)	(1.54)	(0.32)					(0.66)
1998 Earnings	1.101 *	0.396	-0.058	0.136	0.083	0.060	---	---	---	---	-0.170 *
(age 33-36)	(2.30)	(0.92)	(0.18)	(0.50)	(0.22)	(0.10)					(2.24)
2002 Earnings	1.168 †	0.141	-0.031	0.277	0.195	-0.323	---	---	---	---	-0.200 *
(age 37-40)	(1.92)	(0.32)	(0.08)	(0.85)	(0.48)	(0.45)					(2.22)
NLS72											
1986 Earnings	0.669	1.039	0.470	0.742	0.824	-0.169	-0.560	0.510 †	0.568 †	-0.273	-0.055
(age 32)	(0.84)	(1.39)	(0.73)	(1.29)	(1.62)	(0.38)	(1.20)	(1.70)	(1.89)	(0.41)	(0.92)
WLS57											
1974 Earnings	1.746 **	0.575	0.261	0.309	0.295	-0.047	0.383	---	---	---	-0.189 *
(age 35)	(2.72)	(0.77)	(0.62)	(0.94)	(1.36)	(0.20)	(1.14)				(2.24)
1992 Earnings	0.715 †	0.851 *	0.458 *	0.588 **	0.579 ***	0.045	0.619 ***	---	---	---	-0.064
(age 52)	(1.75)	(2.34)	(2.03)	(3.16)	(4.36)	(0.30)	(3.57)				(1.43)
2004 Earnings	0.485	0.953	0.888 †	0.517	0.378	-0.002	0.810 *	---	---	---	-0.052
(age 64)	(0.64)	(1.38)	(1.80)	(1.43)	(1.51)	(0.01)	(2.40)				(0.79)

Note: Numbers in parentheses are t-ratios. † p < .10 * p <. 05 ** p < .01 *** p < .001 (two-tailed tests)

Figure 1. College Completion Treatment Effects on Log Wages by Propensity Score Strata: NLSY79 Men

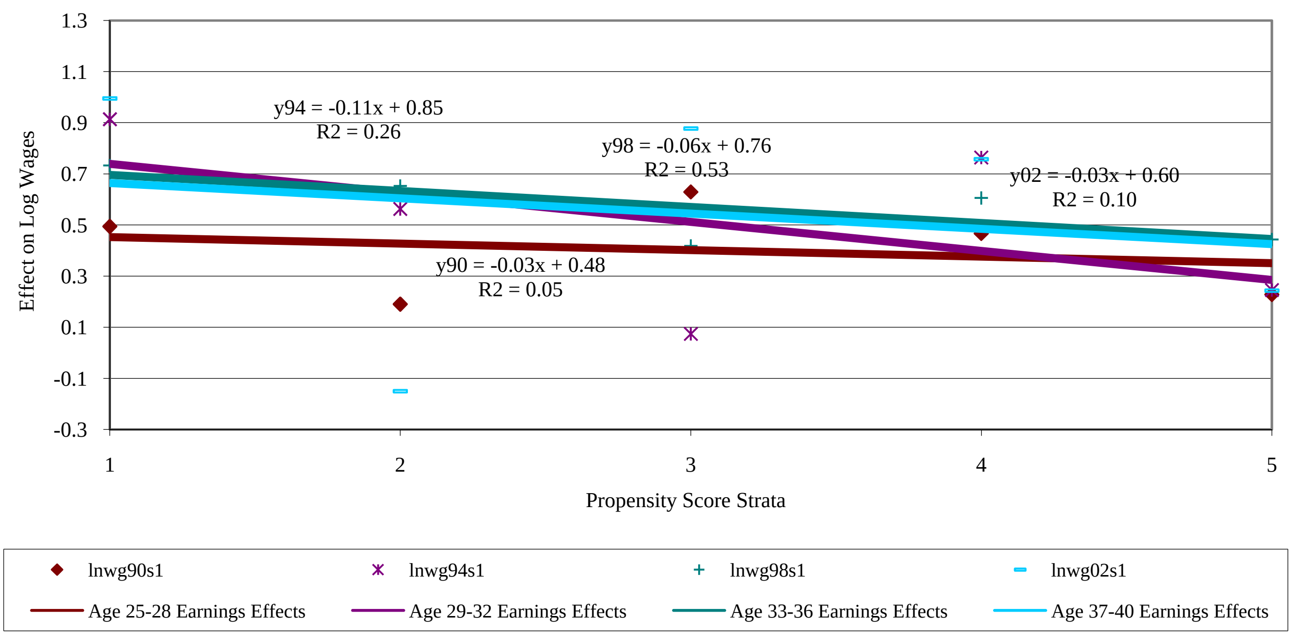

Figure 2. College Completion Treatment Effects on Log Wages by Propensity Score Strata: NLS72 Men

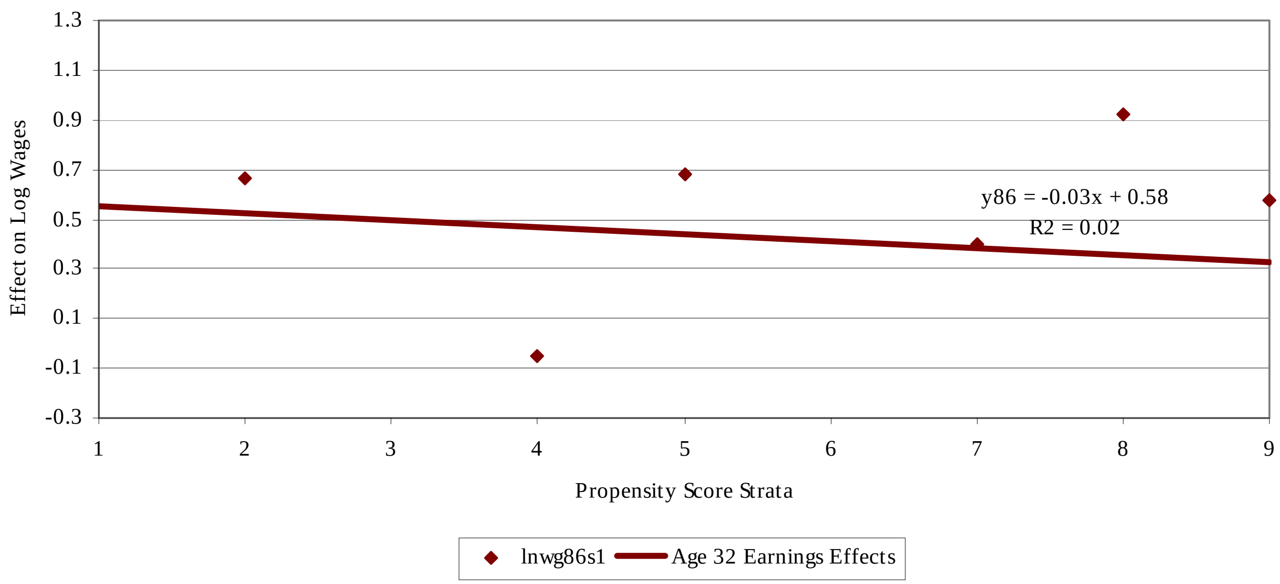

Figure 3. College Completion Treatment Effects on Log Wages by Propensity Score Strata: WLS57 Men

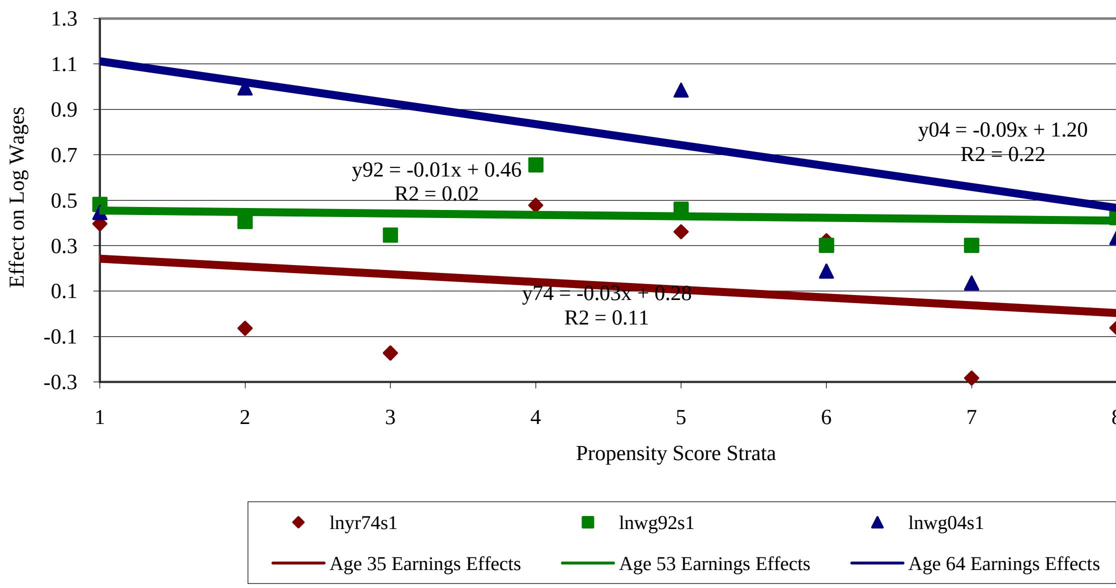

Figure 4. College Completion Treatment Effects on Log Wages by Propensity Score Strata: NLSY79 Women

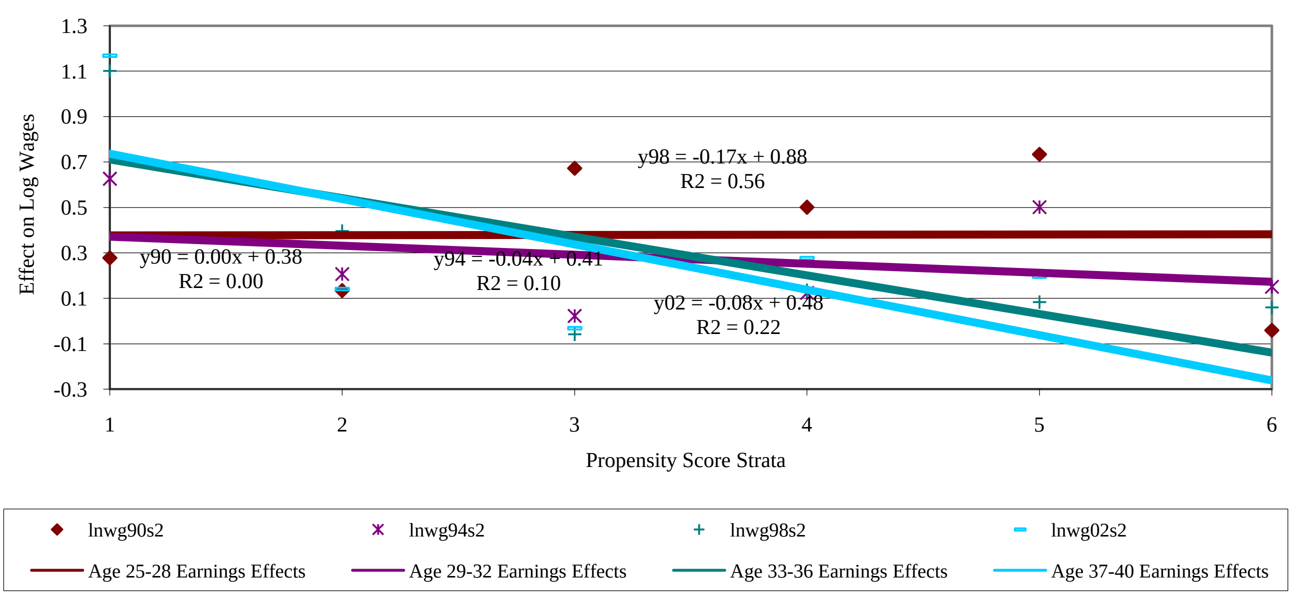

Figure 5. College Completion Treatment Effects on Log Wages by Propensity Score Strata: NLS72 Women

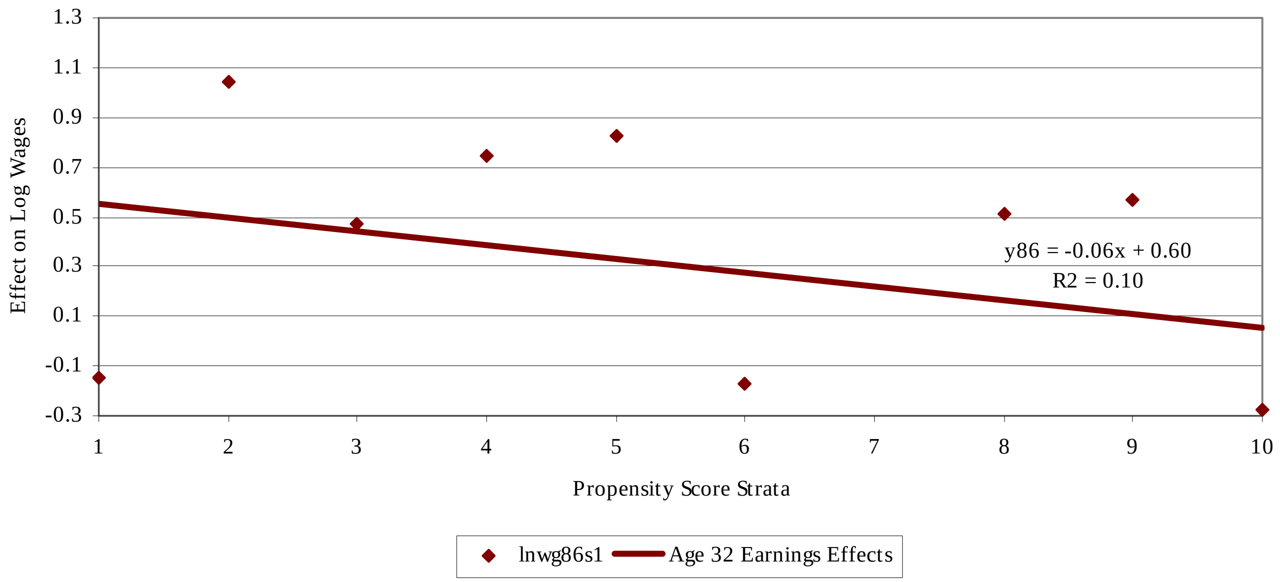

Figure 6. College Completion Treatment Effects on Log Wages by Propensity Score Strata: WLS57 Women

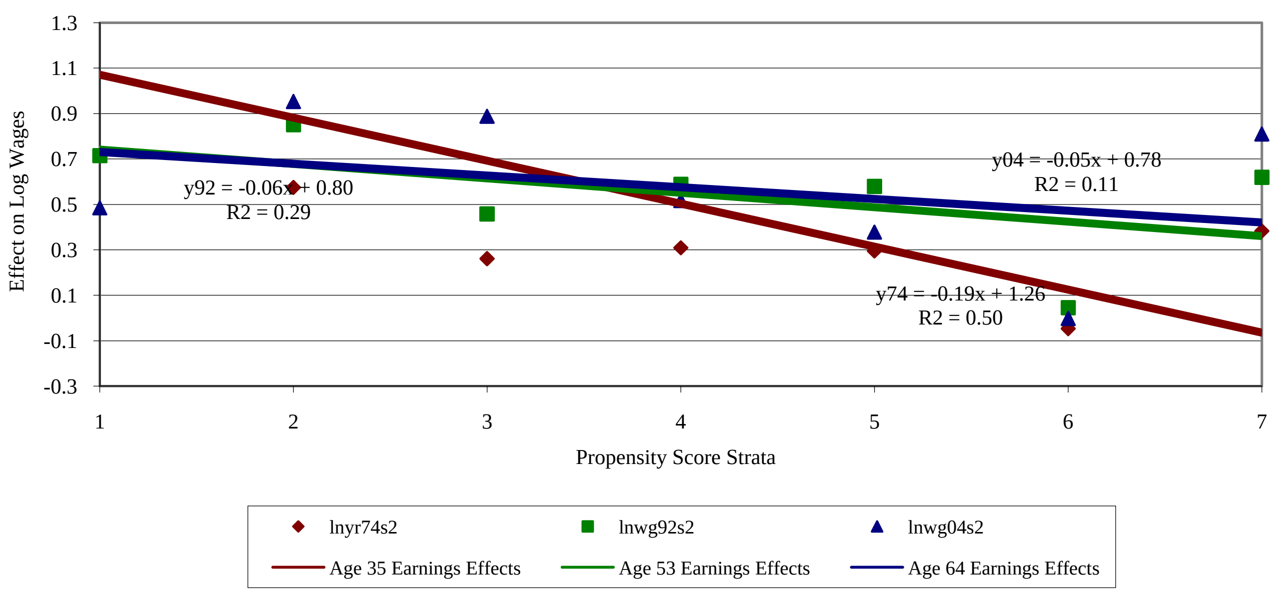

Figure 7. Ratio of Monetary to Non-monetary Importance in Selecting a Career by Propensity Score Strata, College Graduation Treatment and Control Groups: NLS72 College-Educated Men

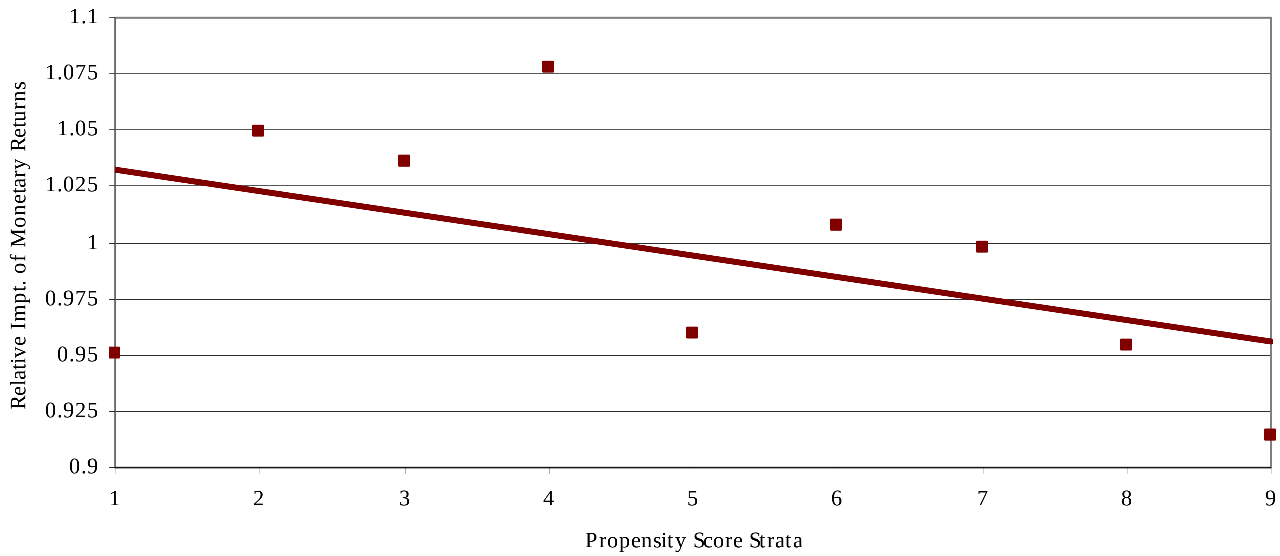

Figure 8. College Graduation Treatment and Control Value of College by Propensity Score Strata: WLS57 Men

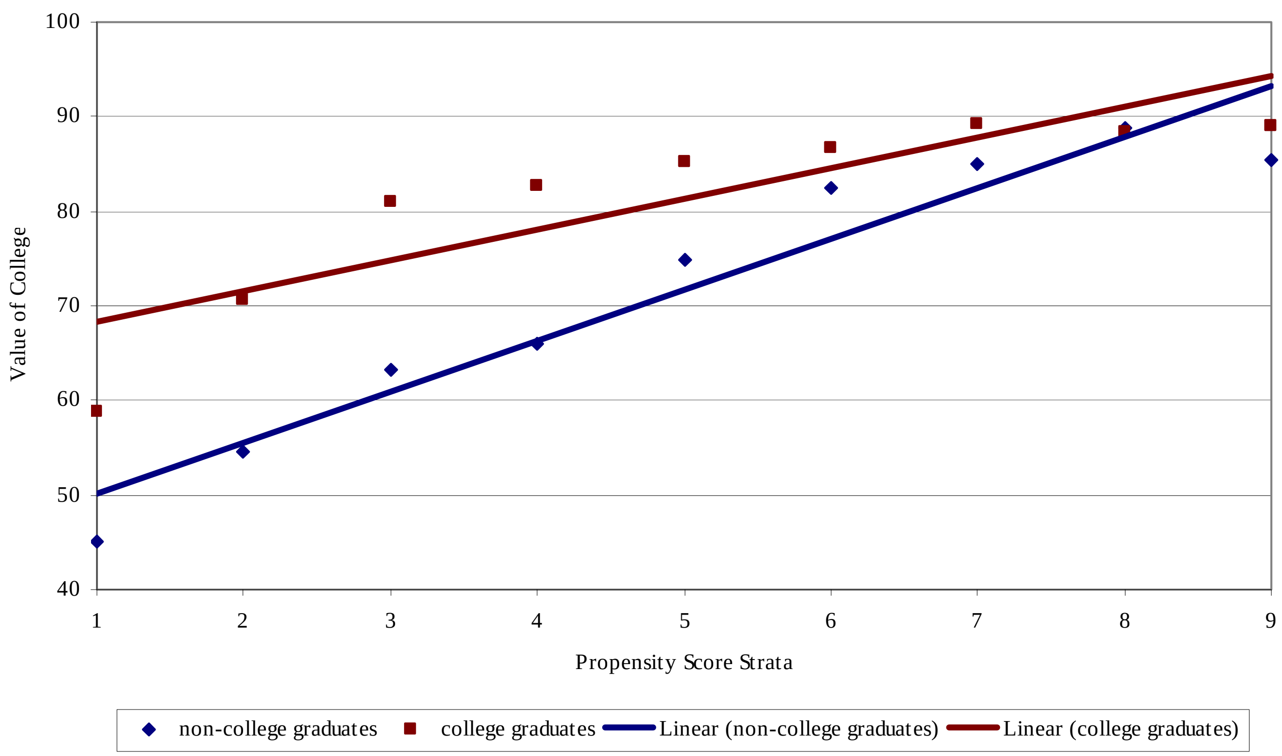

Figure 9. College Completion Treatment Effects on Log Wages by Propensity Score Strata, "Small Set" of Covariates: WLS 57 Men

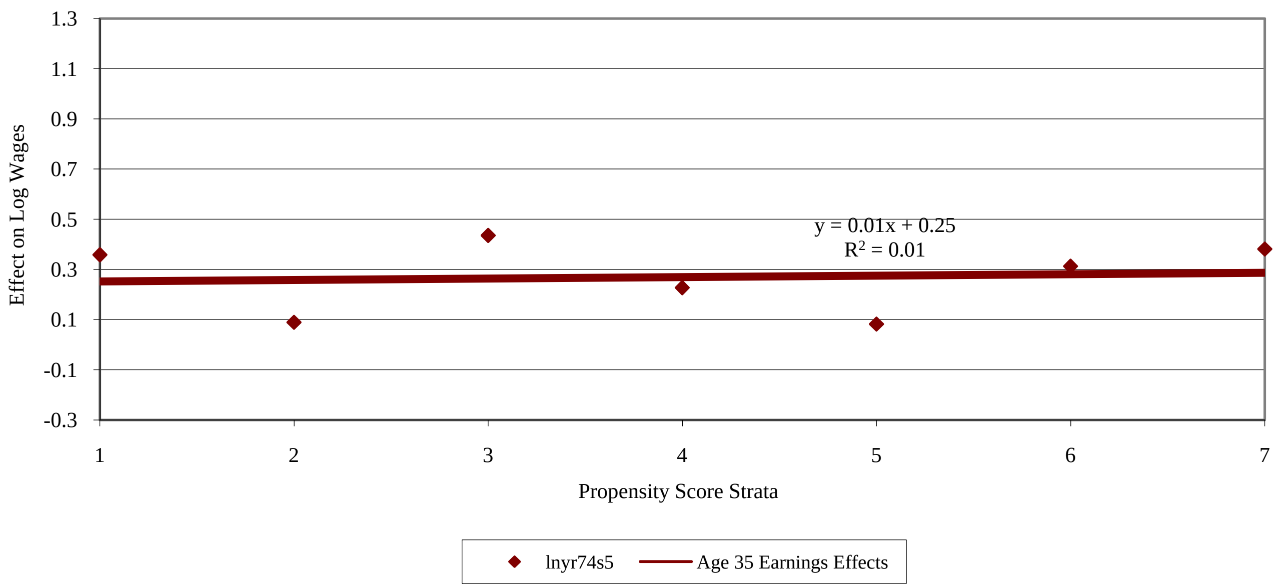

Diagram 1. Hypothetical Model: Origins, Education, and Destinations