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Social Science Research 35 (2006) 749–770
Social Science RESEARCH
www.elsevier.com/locate/ssresearch

Regression and matching estimates of the effects of elite college attendance on educational and career achievement ☆

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Available online 19 August 2005

Abstract

This paper adopts a potential outcome approach to identify and estimate the average treatment effect of attending an elite college on educational and career achievement. A central purpose is to compare the estimates yielded by regression and matching methods of adjusting for the endogeneity of elite college attendance. The analysis follows a high school graduation and college entry cohort across four decades of labor force participation, and estimates elite college effects on educational attainment, occupational socioeconomic status at early-, mid-, and late-career, and wages at mid- and late-career. The findings suggest that attending an elite college yields an advantage with respect to educational achievement and occupational status; results for wages are mixed. One prominent pattern is that the returns to attending an elite college for those who did attend are small by comparison to those that would have been achieved by otherwise equivalent students who attended non-elite institutions.

Keywords: Higher education; Elite colleges; Causality; Propensity score matching; Careers

☆ An earlier version of this paper was presented at the 2003 Annual Meeting of the American Sociological Association, Atlanta. We are grateful to Guido Imbens for helpful technical assistance and encouragement.

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doi:10.1016/j.ssresearch.2005.06.006

1. Introduction

The 1990s witnessed a wave of studies aimed at estimating the earnings premium associated with attending an elite private college. This rush of interest in elite college effects followed an earlier generation of research that had largely concluded that attending a highly selective college does yield a significant economic payoff (Morgan and Duncan, 1979; Solomon and Watchel, 1975; Wales, 1973; Weisbrod and Karpof, 1968). Yet earlier studies had several weaknesses (Brewer and Ehrenberg, 1996; Loury and Garman, 1995), chief among them the failure to control adequately for systematic selection into elite colleges on the basis of observed and unobserved pre-college individual and family determinants of expected future earnings. Inadequate control for selection implies that estimates of elite college effects are subject to endogeneity bias.

Efforts to more rigorously control selection bias led to the latest round of research, but with mixed results. While some studies find evidence of a significant economic return to attending an elite institution (Behrman et al., 1996; Bowen and Bok, 1998; Brewer et al., 1999; Kingston and Smart, 1990; Loury and Garman, 1995), others find virtually no support for such a conclusion (Black and Smith, 2003; Brewer and Ehrenberg, 1996; Dale and Krueger, 2002). Hence, the jury is still out on whether attending an elite college has a causal effect on labor market success.

The research reported here enters the debate surrounding elite college effects on two fronts. First, we agree that selection bias is the main threat to causal inference from observational data, and hence the central issue in gauging elite college effects. It is well understood that students are systematically sorted across institutions on the basis of individual and family characteristics known to be associated with superior labor market outcomes, so that pre-college characteristics must be effectively controlled to obtain meaningful estimates of elite college effects. Because estimators vary in their strengths and weaknesses with respect to bias control, contrasting the results yielded by alternative methods of adjusting for the endogeneity of elite college attendance is a central theme of this study. To this end, our analysis compares the estimates of elite college effects obtained with regression adjustment to the estimates obtained with matching methods. In contrast to most previous research, we adopt a potential outcome approach to causal inference, thereby enriching the parameter space for assessing elite college effects on career outcomes.

Second, this study expands the empirical range of results pertaining to elite college effects. Our analysis follows a high school graduation and college entry cohort across nearly four decades of labor force participation and estimates elite college effects for multiple outcomes situated at different points along the career trajectory. Because no previous study has followed a cohort of college attendees over such a lengthy period, relatively little is known about long-term effects of elite college attendance and how they may differ from early career effects. Just as elite college effects may vary across cohorts, so too effects may vary over time for a given cohort (Brewer et al., 1999). One advantage of such a lengthy time frame for estimating elite college effects is that our study speaks to a different historical period than past research. The cohort followed here attended college circa 1960, whereas most studies based on probability samples have examined graduation cohorts from the 1970s (Brewer et al., 1999; Dale and Krueger, 2002; Loury and Garman, 1995) and 1980s (Black and Smith, 2003; Brewer et al., 1999). Finally, our analysis achieves a more complete representation of key dimensions of the career than do previous studies: we simultaneously estimate elite college effects on educational attainment, occupational status at early-, mid-, and late-career, and wages at mid- and late-career.

2. Recent research on college quality

Recent analyses of the effect of attending an elite college need to be understood from two key vantage points. First, there is the question of what is meant by "elite." On this score, there has been a remarkable level of consensus with respect to both conceptualization and measurement. Most researchers define an elite college as one that practices a high degree of selectivity at admissions, and most use the mean SAT I scores of incoming freshman as the indicator of admissions selectivity.[1] Usually mean SAT score is used alone and entered as a linear score in a regression model (e.g., Bowen and Bok, 1998; Dale and Krueger, 2002; Loury and Garman, 1995), but occasionally it is augmented by indicators of institutional quality, like faculty salaries (Black and Smith, 2003) or tuition (Dale and Krueger, 2002). A popular alternative to mean SAT scores has been authoritative classifications of college selectivity, like Barron's Profiles of American Colleges (Brewer and Ehrenberg, 1996; Brewer et al., 1999; Kingston and Smart, 1990). Barron's classification is itself based on selectivity measures, including SAT scores, grade point average, class rank required for admission, and overall admissions acceptance rate.

A second vantage point for understanding recent studies considers the method used to control for selection bias. The most common method has been regression adjustment on observable pre-college characteristics of students and families. Like earlier studies using the same method, but now with probability samples and improved measures of pre-college exogenous variables, recent studies that have relied on covariate adjustment typically report a statistically significant elite college effect on earnings (Bowen and Bok, 1998; Kingston and Smart, 1990; Morgan and Duncan, 1979; Solomon and Watchel, 1975).

Most studies that use covariate adjustment have assumed that bias is removed by correcting for linear selection on observables, but a couple analyses have employed in addition model-based corrections for selection on *unobservables*. Brewer et al. (1999) are the primary example of such an approach, but they find little evidence that estimates of elite college effects on earnings are changed by correcting for selection on unobservables. Behrman et al. (1996) capitalize on twins data to control unobservable common family factors. Both Brewer et al. and Behrman et al. find positive elite college effects on earnings even after controlling for selection on unobservables.

[1] An exception is Behrman et al. (1996), who eschew SAT scores altogether in favor of indicators of "college quality," such as expenditures per student, salaries for full professors, and students per faculty member.

Although selection on observables is the most common assumption of past research, not all studies rely on regression adjustment to control this source of bias. Two recent studies have used matching methods instead of regression. Black and Smith (2003) use propensity score matching to estimate elite college effects on earnings. Their evidence suggests that the assumption of a linear functional form does impact regression estimates of the effect of attending an elite college on earnings. Their results show that while OLS estimates yield statistically significant effects on earnings, there is a smaller, statistically insignificant impact using matching to control for selection. Dale and Krueger (2002), who also used a matching method to deal with selection, find that students who attended more selective colleges do not earn more than students who were accepted and rejected by comparable colleges, but attended less selective colleges.

In summary, recent studies of the effect of elite college attendance have yielded mixed findings that roughly align with the different methods used to adjust for selection bias. Regression adjustment tends to be associated with positive elite college effects on earnings, while matching has so far yielded null results. But only one study has applied both methods to the same data, as is our plan. Nor has any past study estimated elite college effects on occupational achievement, and only rarely have analysts looked at educational attainment itself (Eide et al., 1998).

3. Analytical approach

3.1. Identifying treatment effects

We parameterize the "effect" of attending an elite college by formally locating it within the potential outcome, counterfactual approach to causal inference pioneered by Rubin (Rosenbaum and Rubin, 1983; Rubin, 1974) and developed by others (e.g., Heckman, 1997).[2] Within this framework there is more than one parameter associated with the effect of attending an elite college. It pays to carefully develop the potential outcome parametrization, if only because it is becoming more popular in sociological applications. After identifying the key parameters, we then discuss regression and matching as alternative methods of estimation.

The hallmark of the potential outcome approach is the formal recognition that each observational unit may be conceptualized as having a value of the dependent variable for each value of the causal variable. Let y be a career outcome, say earnings, and let d be a variable scored $d = 1$ for attending an elite college and $d = 0$ for attending a non-elite college. Then y_{id}, for $d = 0,1$, are the potential values of the outcome variable for unit i, with y_{i1} the value if i attends an elite college and y_{i0} the value if i attends a non-elite college. Only one of these values is actually observed, depending on the type of college unit i actually attends. For units that attend an elite college, y_1 is observed, but not y_0, the value that would have been observed if i had attended

[2] Imbens (2004) gives a thorough review of technical developments in the potential outcome framework.

a non-elite college. Similarly, for those who attend a non-elite college, y_0 is observed but not y_1. For any arbitrary unit i, the observed value of the outcome variable may be written as

$$y_i = y_{i0}(1 - d_i) + y_{i1}d_i, \tag{1}$$

where, adopting the potential-outcome nomenclature, $d_i = 1$ indicates the unit was assigned to "treatment" (i.e., attended elite college) and $d_i = 0$ indicates assignment to control (i.e., attended a non-elite college). For unit i the causal, or treatment, effect of attending an elite college is then $\tau_i = (y_{i1} - y_{i0})$. This causal effect is unobservable because one of the quantities needed to calculate it is necessarily missing.

Yet there are parameters of the population distribution of causal effects that are of substantive interest and can be identified. One is the average treatment effect (ATE) for the whole population, which can be written as:

$$\tau = E(y_{i1} - y_{i0}), \tag{2}$$

$$\tau = E(\tau_i), \tag{3}$$

where expectations are over all units regardless of treatment status. The average treatment effect is the expected effect of attending an elite college on a randomly drawn unit from the population. This parameter is a weighted average of two other treatment parameters, one for each subpopulation

$$\tau = \tau_1\pi_{d=1} + \tau_0\pi_{d=0}, \tag{4}$$

where $\pi_{d=j}$, $j = 0, 1$ are the proportions of the population attending elite and non-elite institutions, respectively

$$\tau_1 = E(y_{i1} - y_{i0}|d = 1) = E(y_{i1}|d_i = 1) - [E(y_{i0}|d_i = 1)] \tag{5}$$

is the causal effect of attending an elite college for those who did attend such an institution (ATT, "the effect of treatment on the treated"), and

$$\tau_0 = E(y_{i1} - y_{i0}|d = 0) = [E(y_{i1}|d_i = 0)] - E(y_{i0}|d_i = 0) \tag{6}$$

is the effect of attending an elite college for those who actually attended a non-elite college (ATC). The parameter τ_1 indicates whether those who attended an elite college have higher or lower average outcomes than they would have had if they instead had attended a non-elite college. Similarly, τ_0 indicates whether those who attended an non-elite college would have had higher (i.e., $\tau_0 > 0$) or lower average outcomes if they instead had attended an elite college. Note that τ_1 need not equal τ_0, and the terms in brackets in Eqs. (5) and (6) are unobservable.

The potential outcome approach makes explicit the issues that attend the identification and estimation of causal effects. These issues are clarified by considering the relationship between the treatment parameters and the observable quantities. The only observable (albeit by sampling) quantities in the expressions above are $E(y_{i1}|d_i = 1)$ and $E(y_{i0}|d_i = 0)$, the conditional means of the outcome variable for the populations of units who attended elite and non-elite colleges, respectively. These population means are easily estimated by their corresponding sample means, and their difference is estimated by the difference in sample means. But the difference between these population means does not suffice to identify any of the parameters of interest. To see this, express the observable means in terms of parameters and unobservables. Summing Eqs. (5) and (6) and re-arranging yields

$$E(y_{i1}|d_i = 1) - E(y_{i0}|d_i = 0) = \tau_1 + \{[E(y_{i0}|d_i = 1)] - E(y_{i0}|d_i = 0)\}, \tag{7}$$

which shows that τ_1 is not identified without an additional assumption about the term in brackets.[3] This term is the difference between the mean earnings of those who attended non-elite schools and the mean earnings that those who attended an elite college would have achieved if they had attended a non-elite college instead. If we assume that the mean outcomes observed for those who attend non-elite colleges represent what those who attended elite colleges would have earned if they had attended non-elite colleges, then

$$[E(y_{i0}|d_i = 1)] = E(y_{i0}|d_i = 0) = E(y_0), \tag{8}$$

so that y_0 is mean independent of attendance status d, the parameter τ_1 is identified, and the simple difference between sample mean outcomes $(\bar{y}_1 - \bar{y}_0)$ is an unbiased estimator of it.

By an analogous argument, the parameter τ_0 can be identified and estimated. Hence, if the observed mean earnings of elite attenders are representative of the average earnings non-elite attenders would have achieved had they attended an elite college, then

$$[E(y_{i1}|d_i = 0)] = E(y_{i1}|d_i = 1) = E(y_1) \tag{9}$$

so y_1 is mean independent of d and

$$E(y_{i1}|d_i = 1) - E(y_{i0}|d_i = 0) = \tau_0, \tag{10}$$

which is identical to τ_1 when y_0 is mean independent of d. Hence, if the potential outcome pair (y_1, y_0) is mean independent of d, then $\tau_1 = \tau_0$ as seen from Eqs. (5) and (6). Since the average treatment effect τ is a weighted average of τ_1 and τ_0, all three parameters are equal when potential outcomes (y_1, y_0) are mean independent of attendance status d. In this case, the average treatment effect $\tau = E(y_1 - y_0)$ can be estimated by the simple difference between sample means $(\bar{y}_1 - \bar{y}_0)$.

The idea that y_1 and y_0 are independent of d is very strong: it is tantamount to assuming that type of college attended is randomized across students, in the sense that both those who did and did not attend an elite college could be expected to be equal on pre-college causes of career outcomes. We know, however, that students who attend elite and non-elite colleges differ with respect to numerous characteristics that both determine potential career outcomes (regardless of type of college attended) and determine the type of college attended. This implies that (y_1, y_0) are correlated with d, in which case none of the treatment parameters are identified without additional assumptions.

[3] The last term on the right is the bias that results when the sample quantity $E(y_{i1}|d_i = 1) - E(y_{i0}|d_i = 0)$ is used as an estimator of τ_1.

A standard assumption used to achieve identification is that potential outcomes are mean-independent of d after conditioning on observable exogenous covariates, say X, that capture pre-treatment characteristics of units that determine selection into treatment and control status, into elite and non-elite colleges.[4] We then have

$$E(y_{i1}|d,X) = E(y_{i1}|X); \quad \text{and} \quad E(y_{i0}|d,X) = E(y_{i0}|X) \tag{11}$$

a restriction known as the unconfoundedness or "selection on observables" assumption. The restriction of Eq. (11) implies that all three *conditional* effects are equal (Wooldridge, 2002, p. 608),

$$\tau_{|x} = \tau_{1|x} = \tau_{0|x}, \tag{12}$$

although the unconditional effects of primary interest will in general not be. In particular, the average treatment effect controlling for X would be the expectation of $\tau_{|x}$ over the distribution of X across the whole population of units; the average effect of the treatment on the treated (i.e., τ_1) would be the expectation of $\tau_{1|x}$ across the distribution of *X among units for whom d = 1*; and the average effect of the treatment on the controls (i.e., τ_0) would be the expectation of $\tau_{0|x}$ over the distribution of X *among units for whom d = 0*.[5] The key assumption is that conditioning on a sufficiently exhaustive set of pre-college characteristics randomizes college attended.[6] This is analogous to the assumption typically invoked to achieve consistent estimation in standard regression analyses.

3.2. Estimating treatment effects

In observational studies, matching and regression are the two basic methods for adjusting estimates of causal effects for confounding due to observed covariates that may be correlated with the causal variable and that determine the outcome variable. The goal of both methods is to construct comparisons of treated and control units that are balanced in the sense that the sample distribution of covariates determining selection into treatment is the same in the treated and control groups. This section describes how these methods are used to construct estimators of treatment effect parameters.

3.2.1. Regression

With assumptions no stronger than ordinarily invoked by regression analyses, the effect parameters can be estimated by ordinary least squares. Consider first the case in which the career outcome is linear in covariates X, unconfoundedness holds, and treatment effects are constant across all units. Then the parameters are all equal, $\tau = \tau_1 = \tau_0$, and estimable from the regression (Wooldridge, 2002)

[4] All frameworks for causal inference make a similar assumption at some point.

[5] If the unconfoundedness assumption holds and if $0 < Pr(d = 1|X) < 1$ for all X, then treatment is said to be "strongly ignorable."

[6] τ_1 can be estimated under the weaker assumption that y_0 alone is independent of d.

$$y_i = \alpha + \tau d_i + \sum_k^p \beta_k x_{ik} + u_i, \tag{13}$$

where τ is the average treatment effect, $x_k(k = 1 \ldots p)$ is a set of pre-college covariates that are uncorrelated with u_i. Eq. (13) is the typical model used in past analyses of the effect of elite college attendance.[7]

Regression may also be used to estimate the treatment parameters when the constant treatment effect assumption is relaxed, which implies that the mean treatment effect on the treated is different than the mean treatment effect on the controls, i.e., $\tau_1 \neq \tau_0$. In this case Wooldridge (2002) suggests fitting

$$y_i = \alpha + \tau d_i + \sum_k^p \beta_k x_k + \sum_k^p \gamma_k d_i (x_{ik} - \mu_k) + u_i, \tag{14}$$

where the population means μ_k of the covariates would be proxied by the sample means. This model allows the average effect of the treatment to depend on the pre-college covariates and yields $\hat{\tau}$, the coefficient of d, as a consistent estimator of the average treatment effect τ. Wooldridge (2002, p. 613) suggests that the average treatment effect on the treated, τ_1, may be estimated by

$$\hat{\tau}_1 = \hat{\tau} + \left(\sum_i^n d_i\right)^{-1} \sum_i^n \sum_k d_i \hat{\beta}_k (x_{ik} - \bar{x}_k), \tag{15}$$

where the right-hand side is the estimate $\hat{\tau}$ of ATE adjusted for the conditional treatment effect among the treated averaged over the sample distribution of the covariates among those *who attended elite colleges*. Similarly, the average treatment effect among those attending non-elite colleges, τ_0, would be estimated by

$$\hat{\tau}_0 = \hat{\tau} + \left(\sum_i^n (1 - d_i)\right)^{-1} \sum_i^n \sum_k (d_i - 1) \hat{\beta}_k (x_{ik} - \bar{x}_k), \tag{16}$$

where now the estimate $\hat{\tau}$ of the ATE is adjusted for the conditional treatment effect among the treated averaged over the sample distribution of covariates among those *who attended non-elite colleges*. Standard errors for both $\hat{\tau}_1$ and $\hat{\tau}_0$ may be obtained by bootstrapping methods.

3.2.2. Matching

Matching estimates may be derived by comparing mean levels of career outcomes across units that differ in type of college attended, yet share a similar configuration of the pretreatment covariates which determine selection into elite colleges. Groups of units that share a similar configuration of covariates have roughly the same *propensity* to attend an elite college, though only the "treatment" group actually did attend.

[7] Under the constant effect assumption, unconfoundedness means that d_i and u_i are independent conditional on the exogenous covariates.

All the various methods for matching involve the construction of counterfactual expectations of the dependent variable.[8] Such counterfactual means are generated by imputing values of $y_{i0|d=1}$ for units that attended elite schools and imputing values of $y_{i1|d=0}$ for units that attended non-elite schools. These unobserved quantities are estimated by averaging over the observed values of y for units that are similar on the covariates, but attended the opposite type of college. Hence, for each unit there are two potential outcomes, say y_{i0} and y_{i1}, which may be estimated by

$$\hat{y}_{i0} = \begin{cases} y_{i0} & \text{if } d_i = 0, \\ \overline{y}_{i0(m)} & \text{if } d_i = 1, \end{cases}$$

and

$$\hat{y}_{i1} = \begin{cases} \overline{y}_{i0(m)} & \text{if } d_i = 0, \\ y_{i1} & \text{if } d_i = 1, \end{cases}$$

where $\overline{y}_{id(m)}$ is the mean of the outcome variable for the matched units. Then the matching estimator of, say, the average treatment effect τ has the form

$$\hat{\tau}_m = (n)^{-1} \sum_{i=1}^{n} (\hat{y}_{i1} - \hat{y}_{i0}).$$

The estimators τ_{m0} and τ_{m1} of τ_0 and τ_1 are defined analogously, but the summations are over only units for whom $d = 1$ and $d = 0$, respectively. The matching criteria and estimators used here are described fully by Abadie et al. (2004) and implemented in a Stata routine.

3.3. Summary

Matching methods have become popular as a complement to regression in applied econometric research (e.g., Angrist and Krueger, 1998; Dehejia and Wahba, 2002, 1999; Heckman, 1997; Heckman et al., 1998) because they have two advantages for estimating treatment effects. First, unlike regression adjustment, matching estimators are not model-based and hence do not depend on arbitrary functional form assumptions. Assuming linearity to account for selection bias is problematic, given that there is rarely theoretical justification for a specific functional form (Black and Smith, 2003; Heckman et al., 1998). In contrast, matched control units serve as observation-specific counterfactuals for each treated unit, thereby avoiding bias due to misspecification of the functional form. Although matching assumes selection on observables, it does not assume *linear* selection as does covariate adjustment through regression. This is the main advantage of matching.

Second, when validly implemented, matching insures that comparisons between treated and control units occur for values of the conditioning covariates that

[8] For example, estimating τ involves constructing estimates of $E(y_0|d_i = 1, x)$, i.e., the mean earnings of elite students if they had attended non-elite colleges, and estimates of $E(y_1|d_i = 0, x)$, the mean earnings of non-elite students if they had attended elite institutions.

represent areas of so-called common support, that is, regions of covariate values where both treatment and control units have positive frequency in the sample. Common support is not achieved when, for units of a given type (i.e., treatment or control), there are no units of the opposite type with a comparable configuration of covariates. Linear regression estimates are not typically restricted to areas of common support. On the contrary, in the absence of explicit restrictions imposed by the researcher, regression uses observations outside the area of common support if they exist in the sample (Black and Smith, 2003; Tobias, 2003).

4. Description of data and measurement

The data are from the Wisconsin Longitudinal Study (WLS), a panel study based on a random sample of 10,317 men and women who graduated from Wisconsin high schools in 1957. Data on pre-college aspects of family and social background, cognitive ability, and high school academic experience and achievement pertain to circa 1957, when the respondents were about 18 years old. The outcome measures are from surveys conducted in 1964, 1975, and 1992. The measurement of all the pre-college exogenous variables and the outcome variables is standard and described in Table 1.

Table 1
Descriptions of pre-college exogenous and career outcome variables

Variables	Descriptions
Ability and academics	
Class rank	High school grades percentile rank 0–99
Mental ability	Henmon-Nelson 11th grade IQ scores
College track	Dummy variable (met UW-Madison requirements = 1)
Math	Semesters of high school math
Private/public high school	Dummy variable (private = 1)
Family background	
Parent's income	Truncated at $99,800; started log
Father's (head's) SEI	1957 Occupation; Duncan 1970 SEI 0-96;
Mother's education	Years of schooling completed
Catholic	Dummy variable (Catholic = 1)
Jewish	Dummy variable (Jewish = 1)
Intact family	Dummy variable (living with both parents = 1)
Number of siblings	Number of siblings
Rural/urban residence	Dummy variable (rural = 1)
Career outcomes	
Graduated college	Dummy variable (bachelor's = 1)
Advanced degree	Dummy variable (masters or PhD = 1)
Occupational status, first job	Duncan 1970 SEI 0-96
Occupational status 1974	Duncan 1970 SEI 0-96
Wage 1974	Hourly wage (started log)
Occupational status 1992	Duncan 1970 SEI 0-96
Wage 1992	Hourly wage (started log)

The exogenous measures constitute a fairly comprehensive representation of variables that have figured prominently in sociological and economic studies of college attendance and college selectivity. The outcome variables tap major dimensions of educational, occupational, and economic achievement that have appeared in the stratification literature over the past three decades.

As discussed earlier, past studies typically have used SAT I scores of incoming freshman or authoritative national rankings to operationalize the concept of college selectivity. Both methods generally yield highly comparable lists of elite colleges. Because the potential outcome approach requires a binary classification of colleges, and because we wished to avoid imposing our own arbitrary elite cutpoint to a quantitative measure of college selectivity, we defined "elite" using categorical national rankings supplied by Barron's Profiles of American Colleges 1969 College Admissions Selector.[9] Barron's Profiles, the only authoritative ranking available for an earlier historical period, has the advantage of over time comparability because of its use as an indicator of college selectivity by several contemporary studies (Behrman et al., 1996; Brewer et al., 1999).[10] Because Barron's ranking is for 1969 rather than 1959, there is some risk that the "treatment status" of schools attended by the WLS cohort have been misclassified. Since it is reasonable to expect a measure of stability in elite rankings, we deem this risk to be small. Still, such misclassification would have the effect of biasing downward toward zero the estimates of elite college effects.

For analytical purposes, the sample was restricted in a number of ways. Because most of the women in this 1957 cohort were out of the labor force for the bulk of the 35-years covered by this study, only males who attended college ($n = 2358$) are included in the analysis.[11] To control for extraneous differences that might be associated with type of college attended, the sample was further restricted to respondents

[9] Colleges in the top two categories of Barron's *Profiles*, "Most Competitive" and "Highly Competitive," are considered elite for our purposes. The highest frequency of "elite" respondents (19%) in the WLS attended Lawrence University, located in Wisconsin, followed by Northwestern University (10%) in Illinois. Several students attended liberal arts colleges such as Dartmouth College, Carleton College, and Wellesley College, while several students attended large research universities such as Cornell, Duke, and the University of Chicago. One reviewer thought that the University of Wisconsin-Madison ought to be among the elite schools even though it was not so classified by Barron's *Profiles*. Consultation with UW-Madison faculty and administrators unanimously confirmed that UW-Madison circa 1960 was definitely not "elite" by the selectivity criteria applied here. We note that other prestigious public research universities that might qualify as elite by contemporary criteria, such as UC-Berkeley, University of Michigan, University of North Carolina, Chapel Hill, are also excluded from the elite category.

[10] While most of the colleges in our "elite" category are of the small liberal arts type, respondents themselves are about evenly divided between research universities (53%) and liberal arts colleges (47%). One reviewer suggested that the distinction between elite and non-elite might be confounded with the distinction between research university and liberal arts college, with the latter distinction actually driving findings that we might erroneously construe as elite college effects on career outcomes. It turns out that the association between the two classifications is in the hypothesized direction, but it is very small ($\chi^2 = 2.46$) and statistically non-significant ($p > .11$).

[11] Graduation from college is not required for inclusion in the sample; on the contrary, graduation is potentially one of the effects of elite college attendance and intervenes between college attendance and later career outcomes. Confining attention to college attendance achieves comparability with most other studies (Black and Smith, 2003; Brewer et al., 1999; Brewer and Ehrenberg, 1996; Dale and Krueger, 2002).

who attended college within two years of high school graduation. While those who attended elite colleges generally did so soon after high school graduation, respondents who attended non-elite colleges did so throughout the period from 1957 to the 1980s. Restricting the analysis to those who attended college within two years of high school graduation insures that both groups are facing historically comparable educational and labor market opportunities at similar points in their age trajectory. With these restrictions, the final sample ($n = 1733$) consists of 1659 men who attended non-elite colleges and 74 men who attended elite colleges.[12]

The WLS has several advantages for estimating elite college effects. First, there is the exceptional quality of the pre-college exogenous variables that characterize social and family background, cognitive ability, and high school academic experience, and whose omission is a central source of selection bias. The quality of both regression and matching estimators ultimately hinges on the reliable measurement of such pre-college determinants of elite college attendance. Second, restricting the analysis to a high school graduation cohort that comes from one state and that attended college at about the same time improves efficiency by controlling extraneous variation on numerous unmeasured causes of career achievement. Although this restriction may reduce generalizability, the quality of the estimators of treatment effects is more important given the aims of this study.[13]

5. Analysis and findings

5.1. Pre-college determinants of elite college attendance

To fix ideas and gauge the extent to which selection bias is a threat in this sample, Table 2 gives the results of a logistic regression of elite college attendance on the pre-college exogenous variables. The likelihood ratio chi-square for the equation is highly significant ($\chi^2 = 141$; $p < .000$), which suggests that the exogenous variables do represent a powerful set of forces shaping elite college attendance. In terms of explanatory power, the most important determinants of elite college attendance in this sample of college-bound students of the late 1950s are cognitive ability, achievement in high school, and family socioeconomic status, especially mother's schooling.[14] These patterns are consistent with past studies of educational attainment. Although none of the other coefficients are statistically significant, their coefficients

[12] Some early regression studies used the Wisconsin Longitudinal Study to examine the effect of college quality on 1974 career outcomes, with some mixed results depending upon the definition of elite used, the choice of input variables, and the outcomes examined (Alwin, 1974; Sewell et al., 1975). These studies did not impose the sample restrictions adopted here, did not adopt a potential outcome approach, and used standard regression methods for estimation.

[13] Limitations on generalizability may be more of an issue in theory than in practice. Past research has shown repeatedly that for outcomes linked to social background and to attainment, the patterns found in the WLS mirror those found in national probability samples (Sheridan, 2001).

[14] A reviewer suggested including occupational aspirations among the exogenous variables. Doing so yielded a coefficient with a t ratio of 0.25.

Table 2
Logistic regression of elite college attendance on exogenous variables

Exogenous variables	Coefficient	*t*
Ability and aademics		
Class rank	0.041	3.39***
Mental ability	0.040	3.42***
College track	−0.577	0.88
Math	0.178	1.60
Private/public high school	0.299	0.75
Family background		
Parent's income	0.342	1.36
Father's (head's) SEI	0.014	1.92*
Mother's education	0.201	3.80***
Catholic	−0.544	1.53
Jewish	−0.221	0.27
Intact family	−0.265	1.13
Number of siblings	−0.071	0.87
Rural/urban residence	−0.104	0.34
LR χ^2	140.88	

Note. $n = 1733$, with 74 elites and 1659 non-elites.
* $p < .05$.
** $p < .01$.
*** $p < .001$ (two-tailed tests).

are in the expected direction. Being from a Jewish, Catholic, or farm family reduces the odds of attending an elite college, while going to private school increases the odds. The analysis that follows includes as regression controls and matching covariates all of the variables in this model of elite college attendance.

5.2. Regression estimates of treatment effects

We begin with the regression estimates that have been a staple of past research. Table 3 gives the estimates for all the outcome variables. Model 1 establishes a benchmark by giving the coefficient from a regression of each outcome variable on a dummy variable for elite college attendance. In every instance these differences in means show that those who attended an elite college enjoy a statistically significant advantage at each career stage. Given the way cognitive ability, high school academic achievement, and family background affect college selection in this sample, some level of educational and career advantage for those attending an elite college would be expected even if an elite college provided no special benefits. Still, the associations represented by these estimates are impressive. Those who attended an elite college were more likely to receive a bachelor's degree and more likely to receive an advanced degree. They also had an advantage with respect to occupational socioeconomic status at early, middle and late career, although the advantage declines over time. In contrast, those who attended an elite college enjoyed a wage advantage that grew considerably from middle to late career.

Table 3
Regression estimates of average treatment effects of elite college attendance on attainment across the career

Career outcomes	Model 1	Model 2	Model 3				$n1$	$n0$
	$Y=f(d)$	$Y=f(d,X)$	$Y=f(d,X,\ d*X)$					
	ATE 1	ATE 2	ATE 3	ATT 4	ATC 5	ATC–ATT 6		
Educational attainment								
Bachelor's degree	0.296^{***}	$0.094^{\#}$	$0.176^{\#}$	0.092^{***}	0.180^{**}	$0.088^{\#}$	74	1659
	(5.30)	(1.75)	(1.76)	(3.15)	(2.63)	(1.78)		
Master's/Ph.D.	0.270^{***}	0.122^{*}	0.089	$0.124^{\#}$	0.088	−0.036	74	1659
	(5.02)	(2.28)	(0.90)	(1.88)	(0.72)	(0.34)		
Early career attainment								
Occupational status 1st job	15.08^{***}	4.37	12.07^{*}	$4.13^{\#}$	12.47^{**}	8.34^{*}	65	1542
	(4.96)	(1.47)	(2.09)	(1.91)	(3.01)	(2.02)		
Mid-career attainment								
Occupational status 1974	9.52^{***}	1.84	5.75	1.71	5.9	4.19	70	1634
	(4.02)	(0.79)	(1.26)	(1.11)	(1.55)	(1.25)		
Wage 1974	$.069^{***}$	0.028	0.025	0.028	0.026	−0.002	68	1568
	(3.84)	(1.53)	(0.75)	(1.19)	(0.59)	(0.05)		
Late-career attainment								
Occupational status 1992	7.86^{***}	1.31	5.93	1.12	6.23	$5.11^{\#}$	65	1481
	(3.16)	(0.53)	(1.32)	(0.48)	(1.56)	(1.80)		
Wage 1992	0.399^{***}	0.185^{*}	$0.329^{\#}$	0.179	$0.339^{\#}$	0.16	57	1390
	(4.19)	(1.95)	(1.93)	(1.60)	(1.85)	(1.05)		

Note. Numbers in parentheses are t ratios. The t ratios for ATT, ATC, and (ATC–ATT) are calculated from bootstrap standard errors generated from 1000 replications.
* $p < .05$.
** $p < .01$.
*** $p < .001$ (two-tailed tests).
$^{\#}$ $p < .05$ (one-sided).

Model 2 gives the estimates of the average treatment effects after controlling for the pre-college exogenous variables that govern selection into elite colleges. These estimates, which pertain to τ in Eq. (13), assume that all the treatment parameters are equal and that the effects are constant across the distribution of the covariates. This equation has the form typical of early models of elite college effects. As these figures show, introducing controls for pre-college academic and family background dramatically reduces the magnitude of the coefficients, although some are still statistically significant. In particular, attending an elite college is still associated with a statistically significant difference in degree attainment and late career wages.

Columns 3–5 of Table 3 give the estimates of treatment parameters derived from fitting Eq. (14) to estimate ATE (τ) and using Eqs. (15) and (16) to estimate ATT (τ_1) and ATC (τ_0), respectively. Column 6 gives the magnitude of the difference (ATC–ATT), and the corresponding t ratio is the test statistic for the hypothesis $\tau_1 = \tau_0$. Looking first at the parameter estimates themselves, notice that ATE falls between ATT and ATC but is always very close to ATC. This is not surprising, because over 95% of the sample are non-elites.

Two notable patterns characterize the estimates for model 3. First, for five of seven outcomes the estimated effect of elite college attendance on the controls (ATC) exceeds the estimated effect on the treated (ATT), i.e., on those who actually attended elite colleges. For both groups, the estimated parameters are positive, so attending an elite college yields an expected gain in terms of educational attainment, occupational status, and earnings. Yet for most outcomes, the returns to attending an elite college for those who did attend are small by comparison to those that would have been achieved by otherwise equivalent students who attended non-elite institutions. In other words, the estimated gains from attending an elite college would have been greater for the non-elites had they attended than they were for those who in fact did attend. Since the former conclusion pertains to a counterfactual parameter, *ceteris paribus* is critical. In particular, we need to assume that had non-elites actually attended elite colleges, nothing else would have changed to effect their outcomes or those of the elite students. Second, there is an interesting relationship between the estimates for model 3 and those for the typical regression of model 2. For all outcomes, the standard regression of model 2 yields an estimate of the average treatment effect (ATE) which is very close to the estimate of the average treatment effect on the treated (ATT) from model 3, but which severely underestimates ATC, and hence ATE, as yielded by model 3. This result is not an idiosyncratic property of our regression estimator of ATT. As we shall see, the matching estimates of ATT for all outcomes also closely resemble the estimated average treatment effect yielded by regression model 2.

Beyond these overall patterns are the estimates pertaining to specific outcomes. Consider first the estimates for degree attainment. The effect of elite college attendance is to increase the probability of receiving a bachelor's degree by .092 for those who actually attended and by .188 for observationally equivalent non-elites if they had attended. Both estimates are statistically significant, as is the difference between them.[15] Hence, we reject the hypothesis that the effect of elite college attendance on obtaining a bachelor's degree is the same for treated and controls; rather, the imputed gain from attending an elite college is almost twice as large for those who attended non-elite colleges. This pattern is reversed for the probability of achieving an advanced degree, although the difference between ATT and ATC is very small, not even as large as its standard error. In this case, we would accept the null hypothesis of equality of ATT and ATC, and revert to model 2 for an estimate .122 of the average treatment effect of elite college attendance on the probability of obtaining an advanced degree.

The pattern observed for bachelor's degree is replicated for socioeconomic status of first occupation. Both ATT and ATC are positive and statistically significant, as is the difference between them. The magnitude of the estimates indicate that had

[15] For all outcomes, the standard errors of $\hat{\tau}_1$, $\hat{\tau}_0$, and $(\hat{\tau}_0 - \hat{\tau}_1)$ were obtained by bootstrapping with 1000 replications. Statements about statistical significance assume symmetrical bootstrap distributions of the estimates.

non-elites attended an elite college (everything else equal), their gain in occupational status would have been three times the gain observed for those who did in fact attend. Again, the ATE estimated by model 2 is more like an estimate of ATT; it severely underestimates ATC. The same pattern occurs for occupational status at mid- and late-career, although the magnitudes are smaller and standard errors somewhat larger. Note too there is not much difference between middle and late career; the estimated parameters and their difference (ATT–ATC) are very similar for these outcomes separated by 18 years. At both career stages, the estimated difference (ATC–ATT) favors non-elites and is larger than its standard error; at late career the difference is statistically significant. Overall, elite college effects on socioeconomic status are positive, and stronger for the non-elites than for elites. Indeed, at middle and late career its arguable whether there is much of an effect of attending an elite college on those who did attend; they achieve about what they would have achieved otherwise. In contrast, the evidence of a positive effect for non-elites if they had attended an elite college is statistically stronger.

Most previous research has examined economic outcomes and yielded mixed results. Our regression results for model 3 are also mixed insofar as elite college effects on wages are mainly confined to 1992; the estimated treatment effects for 1974 are small and statistically weak. The 1992 results, however, do follow the familiar pattern: The economic gain for non-elites if they had attended an elite college (ATC) is positive and nearly twice the magnitude of the same effect for those who actually attended elite colleges (ATT). Both estimates are more than 1.5 times their standard errors, and the difference between them is large though not significant. On the whole, the regression estimates from models 2 and 3 suggest that elite college attendance positively affects late- but not mid-career wages. Brewer et al. (1999) also report elite college effects that are stronger at later compared to earlier points in the career.

5.3. Matching results

The regression estimates provide a benchmark against which to assess the two sets of matching estimates given in Table 4. The left panel displays the estimates obtained without any adjustments for the bias that results from the failure of matching to be exact. The right panel gives the bias-adjusted estimates that correct the within-match mean differences in the outcome variables for post-match differences in covariates (Abadie et al., 2004). Both the sets of estimates show, as before, the ATE falling between ATT and ATC, and very close to ATC.

In other respects, too, the matching estimates are in close agreement, both qualitatively and quantitatively, with the regression estimates. Consider the first column of Table 4 which gives the uncorrected estimates of the ATE. These estimates, which suggest a statistically significant effect of attending an elite college on all dimensions of early-, mid- and late-career attainment, are very close to the corresponding regression estimates without controls for selection bias (Table 3, model 1), and quite similar even to those yielded by model 3, which controls for the pre-college covariates, but relaxes the assumption of constant unit effects. The corresponding bias-corrected matching estimates of ATE (Table 4, column 5) are generally smaller than the uncor-

Table 4
Matching estimates of average treatment effects of elite college attendance on attainment across the career

Career outcomes	No bias adjustment				Full bias adjustment				$n1$	$n0$
	ATE 1	ATT 2	ATC 3	ATC–ATT 4	ATE 5	ATT 6	ATC 7	ATC–ATT 8		
Educational attainment										
Bachelor's degree	0.265***	0.101**	0.273***	0.172***	0.234***	0.067#	0.241***	.174*	74	1659
	(4.34)	(2.53)	(4.34)	(3.74)	(3.75)	(1.67)	(3.76)	(2.26)		
Master's/Ph.D.	0.244***	0.172**	0.247***	0.075	0.131#	0.125*	0.132#	0.007	74	1659
	(3.14)	(2.73)	(3.11)	(0.87)	(1.69)	(1.97)	(1.66)	(0.04)		
Early career attainment										
Occupational status 1st job	12.40***	5.36*	12.69***	7.33**	9.61**	3.16	9.88**	6.72	65	1542
	(3.68)	(2.11)	(3.68)	(2.58)	(2.82)	(1.29)	(2.83)	(1.26)		
Mid-career attainment										
Occupational status 1974	8.01**	1.98	8.26***	6.28**	6.65*	0.254	6.93*	6.68	70	1634
	(2.93)	(1.04)	(2.95)	(2.84)	(2.46)	(0.14)	(2.49)	(1.58)		
Wage 1974	0.038	0.027	0.039#	0.012	−0.01	0.02	−0.011	−0.031	68	1568
	(1.62)	(1.27)	(1.61)	(0.55)	(0.39)	(0.94)	(0.44)	(0.54)		
Late-career attainment										
Occupational status 1992	7.77**	2.10	8.02**	5.92*	12.64***	1.45	13.13***	11.68*	65	1481
	(2.52)	(0.98)	(2.54)	(2.42)	(3.79)	(0.68)	(3.84)	(2.48)		
Wage 1992	0.258*	0.141	0.263*	0.122	0.114	0.094	0.114	0.02	57	1390
	(2.14)	(1.35)	(2.14)	(1.00)	(0.92)	(0.90)	(0.09)	(0.08)		

Note. Numbers in parentheses are t ratios. Results are based on matching four non-elite control units per elite treated unit.

* $p < .05$.

** $p < .01$.

*** $p < .001$ (two-tailed tests).

\# $p < .05$ (one-tailed).

rected estimates—1992 wages is an exception—but the pattern of statistical significance is similar, and they too are comparable to the corresponding regression estimates.

We saw with the regression estimates that for most career outcomes the ATC exceeded the ATT, frequently by a statistically significant margin. This same pattern emerges in the matching estimates, both uncorrected and corrected. Both estimates indicate that, as in the regression case, attending an elite college yields an expected gain in educational attainment, occupational status, and earnings. All but one of the estimates (i.e., for 1974 wages) of the difference (ATC–ATT) is positive, thereby indicating that the estimated gains that would have been achieved by non-elites had they attended an elite college are greater than the estimated gains for those who did attend.

The overall congruence between the matching and regression estimates is largely repeated at the level of specific career outcomes. In most instances, the adjusted and unadjusted matching estimates are quantitatively similar to those generated by regression model 3. Row 1 of Table 4 displays the matching estimates of elite college effects on the probability of obtaining a college degree. The estimates of ATE, ATT, and ATC are somewhat larger than those yielded by regression (model 3, Table 3), and the pattern is similar, with ATC exceeding ATT by a statistically significant margin. Together the regression and matching estimates suggest that ATC is 2–4 times ATT. Clearly, the hypothesis of equality can be rejected. Rather, it is estimated that the gain from attending an elite college would have been more than twice as large for non-elites had they attended than it was in fact for elites. This contrasts with the results pertaining to an advanced degree, for which regression and matching yield estimates of ATT and ATC that are statistically indistinguishable. The bias-adjusted point estimates indicate that attending an elite college boosts by .12 the probability of obtaining an advanced degree.

The matching estimates of the effects of attending an elite college on occupational socioeconomic status are qualitatively similar, but quantitatively much stronger, than the corresponding regression estimates. Regression yielded positive estimates of all treatment parameters and the difference (ATC–ATT), but only for early career attainment were these statistically strong enough to reject the null hypotheses. Both sets of matching estimates of ATC are positive and significant at all career stages (and similar to ATE), and larger than the estimates of ATT, which tend not to be statistically significant. The estimated differences (ATC–ATT), like those generated by regression, always favor non-elites and are statistically large at all career stages. Indeed, none of the bias-adjusted estimates of ATT are statistically significantly different from zero. This parallels the conclusion about ATT arrived at on the basis of the regression estimates. The status returns to attending an elite college for those who did attend are very small by comparison to those that would have been achieved by otherwise equivalent students who attended non-elite institutions.

These findings for educational and status attainment show two broad patterns. First, the unadjusted and adjusted matching estimates have been similar for both dimensions of achievement; and second, the estimates yielded by matching are similar to those yielded by regression. Neither pattern describes the estimates pertaining to the effect of elite college attendance on wages. While the regression estimates are similar to the unadjusted matching estimates for both mid- and late-career, neither are close in magnitude to the much smaller and statistically insignificant bias-adjusted matching estimates shown in the right-hand panel of Table 4. This disparity is sharpest in the late career, where both regression and unadjusted matching suggest a statistically significant ATC and difference (ATC–ATT), while the adjusted matching estimates of these same parameters are statistically small in all instances. Bias adjustment brings about a reduction of more than 50% in the estimates, all of which are still positive but now smaller than their standard errors. The failure of these bias-adjusted estimates to yield evidence of elite college effects on wages is consistent with other studies that have used matching to control selection bias. Note, however, that both the adjusted and unadjusted matching estimates, like the regression estimates, are consistently larger for wages in the late than in the early career, which is what others have reported (Brewer et al., 1999).

6. Discussion and conclusions

This paper has extended in several directions past efforts to estimate elite college effects on career achievement. Compared to other studies, we have examined a broader range of achievement dimensions spread across a much lengthier span of the socioeconomic career. Our analysis has emphasized the issues surrounding selection bias by contrasting regression and matching estimates of the treatment effect parameters entailed by a potential outcome framework for causal inference. The use of alternative estimation strategies, one model based and the other nonparametric, provides a platform from which to assess the robustness of elite college effects to alternative specifications. It is important to remember, however, that both methods assume that bias is induced largely by selection on *observable* pre-college covariates.

As it turns out, the method of estimation is not a major part of our findings, since regression and matching yield rather similar patterns of results, especially for the earliest outcomes.[16] We have found that attending an elite college boosts the probability of graduating from college and of obtaining an advanced degree, and increases the socioeconomic status of first job. With respect to both college graduation and status of first job, the imputed (counterfactual) gains for non-elites (i.e., ATC) had they attended an elite college are significantly greater than those estimated for elites (i.e., ATT) who did attend. This same pattern,

[16] One reviewer wondered what would happen if UW-Madison was treated as elite. Although we would question the logical and empirical basis for doing this, as an exercise it is easy enough. What happens is that the sample of elites is greatly expanded, so that all the standard errors shrink and virtually all the estimates of all the treatment parameters yielded by regression model 3 and bias-adjusted matching are positive and statistically significant. The main difference is that now ATE, ATT, and ATC are approximately equal.

with the magnitude of estimates of ATC (and hence ATE) many times the corresponding estimates of ATT, shows up again in both the regression and matching results for mid- and late-career occupational status. The two sets of estimates together suggest that, overall, the average treatment effect of attending an elite college is about 6 SEI points at mid-career and 6–12 SEI points at late career. The estimates at early-, mid-, and late-career indicate that the return to attending an elite college are 5–12 SEI points greater for those who did not attend than they were for those who did. One pattern that cuts across career outcomes and methods is that the returns to attending an elite college for those who did attend are small by comparison to those that would have been achieved by otherwise equivalent students who attended non-elite institutions.

Our estimates of elite college effects on wages are mixed, and in this respect cannot be said to go much beyond previous studies. Both matching and regression yield very small and statistically very weak estimates of a positive effect of attending an elite college on mid-career wages. The null hypothesis of no effect cannot be rejected with these data. The evidence for a positive effect is much stronger for late-career earnings, but not uniform across the alternative estimation methods. Regression and uncorrected matching estimates are in the range .18–.33 and statistically significant, but the bias-adjusted matching estimates are smaller, about .10, and less than their standard errors. Still, the magnitude of the estimates for late-career earnings are 4–10 times larger than the corresponding estimates for mid-career earnings, which suggests that a larger sample of elites might have yielded statistically detectable elite college effects for the later period.

The small sample of elite respondents is only one factor that constrains the conclusions that can be drawn from these findings. The fundamental historical changes that have occurred in higher education since 1960 mean that findings for a cohort of males who graduated from high school in 1957 may not easily generalize over time to more recent graduates. We know that the market for higher education has shifted in the direction of greater selectivity by institutions in the top categories of the Barron's classification, resulting in student SAT scores becoming more homogeneous within and heterogeneous between institutions. This process would tend to increase elite college effects via the two mechanisms sometimes invoked to account for them (Loury and Garman, 1995). First, if the advantage of attending an elite college is partly a matter of peer effects that come from interacting with more able classmates, then over time the gap between elites and non-elites would increase. If elite college effects are partly due to differences in instructional quality as conditioned by faculty–student interaction, then elite college effects would possibly increase over time as the correlation between the quality of students and the quality of faculty increases with improved sorting of students by ability. Although available evidence suggests that elite college effects have increased since 1960 (Brewer et al., 1999; Hoxby, 1998), such evidence pertains only to earnings and is based exclusively on regression adjustment for selection. Add to this caution the dearth of evidence on the commonly proposed mechanisms by which elite college effects are transmitted to educational and career outcomes (Loury and Garman, 1995), and it is clear that there is much more research to be done on this subject.

Acknowledgments

This research uses data from the Wisconsin Longitudinal Study (WLS) of the University of Wisconsin-Madison. 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 Foundation, and the Graduate School of the University of Wisconsin-Madison. A public use file of WLS data 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.

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