Reconsidering the Effects of Education on Political Participation

Cindy D. Kam Var
Carl L. Palmer Ur Davis

The consensus in the olitical participation is that education positively correlates with political participation. osit that education confers participation-enhancing benefits that in and of themselves ca ost of the variation in educational attainment arises between high school completion and decisions to enter postsecondary institutions, we focus our inquiry on estimating the effect of higher education on political participation. Our primary purpose is to test the conventional claim that higher education causes political participation. We utilize propensity-score matching to address the nonrandom assignment process that characterizes the acquisition of higher education. After the propensity-score matching process takes into account preadult experiences and influences in place during the senior year of high school, the effects of higher education per se on participation disappear. Our results thus call for a reconsideration of how scholars understand the positive empirical relationship between higher education and participation: that higher education is a proxy for preadult experiences and influences, not a cause of political participation.*

Scholars of democratic politics have long focused on the individual and institutional correlates of political participation. Much of this work is motivated by a conviction that participatory inequalities are democratically troublesome. Such inequalities allow some individuals to exert disproportionate influence over government, thus violating the democratic norm of equality (Verba 1996). In response, a substantial body of literature has attempted to identify the determinants of political participation, in part to suggest policy recommendations that will alleviate these distortions.

Myriad studies report that education positively correlates with political participation (e.g., Nie, Junn, and Stehlik-Barry 1996; Rosenstone and Hansen [1993] 2003; Verba and Nie 1972; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980). Verba, Schlozman, and Burns observe that, "Educational attainment is, in fact, the single most potent predictor of an adult's political activity" (2003, 13). Typically, this positive relationship is interpreted to mean that education confers participation-enhancing *benefits*, be it through the acquisition of cognitive abilities that enable comprehension of political content, the development of civic skills and civic orientations that foster political action, or through the attainment of socioeconomic status positions that facilitate mobilization into participation. This view that education confers participatory-enhancing benefits resonates with Mann's characterization that education, "beyond all other devices of human origin, is the great equalizer of the conditions of men—the balance wheel of the social machinery" (Mann [1848] 1960, 87).

While the literature argues quite convincingly that education confers benefits which in turn drive participation, we take a different perspective. As most of the variation in educational attainment arises between high school completion and decisions to enter postsecondary institutions, we focus our inquiry on estimating the effect of higher education on political participation. In so doing, we ask for a reconsideration of how scholars should understand the positive empirical relationship between higher education and participation. Specifically, we question the extent to which higher education is a *cause* of political participation as opposed to a *proxy* for other, often unobserved, preadult experiences and predispositions.

Traditional explanations of political participation treat the acquisition of higher education as a stochastic process, but it is not. The likelihood that an individual will pursue higher education is systematically

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determined by a number of factors, including parental characteristics, individual abilities, and predispositions. The same factors that propel individuals into pursuing higher education may also propel them into participating in politics. As such, models that treat education as an exogen[illegible] mis-attributing the participat[illegible]s of preadult experiences and [illegible]gher education.

We test this claim usi[illegible] data from the Political Socializ[illegible] ddition, we replicate our re[illegible]ndependent panel study: the [illegible]f the High School and Beyond Study. We apply a statistical technique known as propensity-score matching to address the nonrandom assignment process that characterizes the decision to pursue higher education. This technique matches respondents who attended college with those who did not by using a propensity score, or predicted likelihood of attending college based upon an individual's preadult experiences and characteristics. The matching process mimics random assignment, thus producing two groups whose levels of participation can then be compared, having essentially controlled for preadult experiences and characteristics.

Our analyses provide clear support for our key claim: that the relationship between higher education and political participation derives not from higher education per se, but from preadult characteristics. Before matching, the college-educated engaged in significantly more political acts than the noncollege educated. After matching, the effects of higher education disappear, with no significant differences in political activity separating the two groups. Once preadult experiences and influences are taken into account in predisposing individuals to seek higher education, the effect of higher education itself is indistinguishable from zero. As such, our analyses call for a reconsideration of how scholars interpret the empirical relationship between higher education and participation: that higher education is not cause, but proxy.

The Conventional View: Education as Cause

The conventional view of education portrays it as a causal agent for participation: education confers participation-enhancing benefits to the individual. In his characterization of the empirical evidence linking education with political behavior, Converse (1972) notes that:

> Whether one is dealing with cognitive matters . . . or . . . motivational matters . . . or questions of actual behavior, such as engagement in any of a variety of political activities from party work to vote turnout itself: *education is everywhere the universal solvent, and the relationship is always in the same direction*. . . . The educated citizen is attentive, knowledgeable, and participatory, and the uneducated citizen is not. (324, emphasis added)

In empirical analyses of political participation, education is often used as an exogenous variable to predict participation in politics.[1] The effects of education are primarily interpreted as human-capital enhancing: education confers participation-enhancing skills such as cognitive ability, civic skills, and civic orientations. Education confers cognitive ability and political information, both of which enable individuals to make sense of politics (Campbell et al. [1960] 1980; Delli Carpini and Keeter 1996; Wolfinger and Rosenstone 1980). Furthermore, education enhances the acquisition of civic skills, "the communications and organizational abilities that allow citizens to use time and money effectively in political life" (Verba, Schlozman, and Brady 1995, 304). Education enhances political competence (Almond and Verba [1963] 1989, 173), efficacy (Campbell et al. [1960] 1980; Wolfinger and Rosenstone 1980), and interest (Wolfinger and Rosenstone 1980), and it fosters a sense of civic duty within citizens (Campbell et al. [1960] 1980). Citizens who are more educated are more able and willing to interact with the political system.

Complementing this human-capital enhancing argument, Nie, Junn, and Stehlik-Barry (1996) posit a second mechanism by which education leads to political participation. They argue that educational attainment acts as a sorting mechanism that allows citizens to obtain higher prestige occupations, higher wealth, and greater involvement in voluntary organizations. These three intervening indicators then place citizens in more or less socially, economically, and politically connected networks, which subsequently facilitate participation. Higher income and job status not only increase the resources available for participation but also place individuals in networks where they are more likely to be mobilized into

[1]This focus on education is not unique to political science; it plays a central role in other disciplines including economics, psychology, and sociology. Smith declares that education "fits into more models and explains more variation than any other single concept" (1995, 238) and notes that it is the most used variable in the General Social Survey.

politics (Nie, Junn, and Stehlik-Barry 1996; Rosenstone and Hansen [1993] 2000). Education operates on political participation through network positioning, but network positioning arises through education's impact on skills and socialization. Although they posit a different pa[illegible] spurs participation, Nie[illegible]Barry (1996) still treat educati[illegible]mply that education changes ir[illegible]pares them for political life.

As Schlozman notes, [illegible]ation have a direct impact on [illegible]more importantly, education h[illegible]ough its consequences for the acquisition of nearly every other participatory factor" (2002, 442). The overwhelming sense from the literature is that education is viewed as a cause of participation by virtue of its human-capital enhancing and social positioning effects.

Our View: Education as Proxy

The argument that higher education confers participatory benefits is persuasive. However, we propose an alternative way of understanding the empirical relationship between education and political participation. Since most of the variation in education occurs between terminal high school degrees and decisions to pursue postsecondary education, we focus on the effects of higher education on political participation. We suggest that higher education should be seen as a proxy for a series of preadult experiences and dispositions. Below, we sketch a theory of how and why these preadult experiences and predispositions might propel both higher education and adult political participation. We note, however, that our primary intention in this article is to test the conventional claim of whether higher education causes participation.

Empirical evidence suggests systematic patterns underlying educational attainment (Pallas 2002; Pascarella and Terenzini 2005). A variety of individual-level characteristics, including values, cognitive abilities, personality, and other preadult experiences are likely to influence an individual's decision to pursue postsecondary education. These factors likely shape not only educational experiences, but also adult participation. As a consequence, the durable empirical relationship between higher education and participation might not reflect higher education conveying participation-enhancing benefits, but rather, higher education serving as a proxy for preadult characteristics.

What underlying processes drive both pursuit of higher education and subsequent adult participation? The literature on educational attainment traces outcomes to a constellation of parental, individual, and contextual factors. As Luster and McAdoo suggest, "outcomes in adulthood, such as educational attainment, often represent a continuation of a chain of events in childhood and adolescence" (1996, 27; see also Rutter 1989). Here, we draw upon the existing literature on educational attainment to propose a theory of how and why experiences and predispositions that predate college attendance can also explain participation later in life.

As Jencks et al. forcefully argue, "[T]he most important determinant of educational attainment is family background" (1972, 159). Accordingly, our theorizing of the factors that might affect both educational attainment and adult participation begins with parental characteristics, such as education, occupation, and income. These are among the primary determinants of educational attainment (e.g., Ashenfelter and Rouse 1998; Baker and Vélez 1996; Coleman et al. 1966; Cameron and Heckman 1999; Entwisle, Alexander, and Olson 2005; Haveman and Wolfe 1995; Kane 2004; Kao and Thompson 2003; Morgan and Kim 2006). Scholars have posited several explanations for this empirical regularity. Some argue that these factors affect short-term financial considerations: better-off families are more capable of shouldering the costs of college (but see Cameron and Heckman (1999, 2001) who argue against this explanation). Others argue that these factors affect the logistics of applying to college: parents with college degrees are more prepared to help their children navigate the logistics of the application process (e.g., Lindholm 2006, but see the discussion in Rouse and Barrow 2006 disputing this claim). The primary focus, however, is on how these factors affect values and orientations, cognitive capacity, and personality; we discuss these topics in more depth, below.

Probably the most important mechanism by which parental characteristics influence educational attainment and subsequent adult participation is through the transmission of *values*: "Either directly by their words and deeds or indirectly through unconscious means, parents transmit to their children basic postures toward life which the children carry with them at least until the development of their own critical faculties" (Jennings and Niemi 1968, 177). Parents can serve as "*models* who serve as a basis for emulation" or as "*definers*, whose expectations establish what behaviors are appropriate (Cohen 1987, 339). Parents can provide examples for

imitation by their offspring, simply by achieving a particular level of education and by engaging in political activity. As norm definers, parents who are well educated can transmit the importance of education to their children (Goldthorpe 1996; Tomlinson-Keasey and Little 1990). Parents create home environments that cultivate aspirations (Teachman 1987); parents have accumulated tion display higher educationa t al. 1972). Parental influence are important, as aspirations t of enrollment (Mare and Ch). At the same time, well-educated parents are also more likely to be active participants in politics (as demonstrated by the enduring empirical linkage between education and political participation) and can thus hand out effects on the related topic of economic define participation as valuable behavior (Beck and Jennings 1982). If parents transmit positive orientations towards both higher education and political participation, then their offspring would manifest both behaviors—not because higher education boosts political participation, but because both values were transmitted by their parents.

The link between education and participation might also arise from individual characteristics developed in preadult years, e.g., cognitive skills (or intelligence) and personality.[2] Cognitive skills derive from both genetic and social sources (Devlin, Daniels, and Roeder 1997). Biological parents can transmit genetic predispositions that affect educational attainment (parents who have high levels of education themselves are likely to pass on the genetic predispositions that will help their offspring also succeed in education; see, e.g., Bowles and Gintis 2002; Nielsen 2006). Parents can also enhance cognitive skills through social means; they can establish home environments that aid in the development of cognitive skills (Alwin and Thornton 1984; Jencks et al. 1972). Academic ability (as measured by high school achievement) predicts higher educational attainment (Baker and Vélez 1996; Entwisle, Alexander, and Olson 2005; Pallas 2000), and academic ability could also predict a willingness and ability to deal with cognitive complexity in political affairs. In addition to intelligence or cognitive skills, attitudes towards cognitive activity may also explain both educational attainment and political participation. Individuals who are predisposed to enjoy cognitive activity are likely to pursue higher education and to display interest in political matters (Cacioppo et al. 1996). Aside from values, intelligence/cognitive skills and attitudes towards cognition might be the causal factors that propel both educational attainment and political participation.

Aside from these cognitive factors, noncognitive traits, too, might drive both educational attainment and adult participation. These noncognitive traits (i.e., personality traits) stem from both genetic inheritance and social learning (on social learning, see Erikson and Goldthorpe 2002; Heckman 2000; on the cultural and genetic inheritance of personality

success, see Bowles and Gintis 2002; for a discussion of both, see Duncan et al. 2005). Individuals who feel personally efficacious, competent, and self-assured are more likely to set higher educational aspirations and pursue higher education. For example, Tomlinson-Keasey and Little find that a personality dimension called "social responsibility" (which taps "a sense of duty and dependability"; 1990, 447) predicts educational attainment. Entwisle, Alexander, and Olson (2005) find that first-grade temperament strongly predicts educational attainment at age 22: children who are evaluated by teachers as being enthusiastic, expressive, cheerful, creative, and not afraid of new situations are significantly more likely to acquire more education. Individuals who elect to enroll in higher education are likely those who are willing to postpone short-term earnings to make a long-term investment in future earnings (Becker [1964] 1993; Becker and Mulligan 1997); these individuals are also likely to be willing to delay gratification as a general matter and thus be willing to accept short-term costs for the long-term benefits that might accrue from participating in politics (Fowler and Kam 2006). The same personality traits that propel individuals to pursue higher education might also propel them to engage in political action (for discussions of personality in politics, see, e.g., Froman 1961; Gergen and Ullman 1977; Mussen and Warren 1970; Renshon 1974, 1975). Hence, we propose that personality traits that manifest during or prior to adolescence (e.g., a sense of duty, efficaciousness, and willingness to delay gratification) might also be the causal factors that propel both educational attainment and political participation.

Although parents are probably the most important agents of socialization into education and

[2]Whether cognitive capacity consists of separate types of intelligence (that might or might not be closely related to each other) or general intelligence (commonly referred to as Spearman's *g*) is a topic of some contention in psychology (and a topic deserving of treatment elsewhere). The literature in political science has displayed some imprecision in distinguishing between intelligence (however it is defined) and cognitive skills, and we display this shortcoming as well.

political life, other agents such as peer groups, schools, and neighborhoods matter as well. Having peer groups with aspirations for higher education increases such aspirations among youth respondents (Hallinan and Williams 1990) Schools can socialize individuals into pursuing additional years of schooling (but see Jencks et al. 1 odest effects of qualitative diff hools on educational attainmer cial-ize students to politically cour-age voting later in life rents almost always play a key eigh-borhoods, schools, and oups (Jencks et al. 1972; Kao and Thompson 2003; Rouse and Barrow 2006). The existing literature points to large effects attributable to parents and relatively modest school effects, and school effects can often be proxies for community-level norms (Jennings and Stoker 2007). These potential effects are indirectly taken into account through the individual-level and parental covariates that we include in our model of college attendance.

The literature on educational attainment points to systematic differences in who goes to college and who does not. We suggest that the same preadult experiences and predispositions (values, intelligence, and/or cognitive skills, and personality traits) that propel individuals to pursue education might also propel them into political participation later in life. In short, college attendance does not necessarily convey participation-enhancing benefits; young adults who elect to pursue higher education are different a priori from those who do not. Indeed, this view resonates with some sociologists and economists who have argued that higher education merely serves a credentialing function, rather than a socializing or human-capital enhancing function.[3] As Jencks et al. note, "Highly educated people differ from uneducated people in many important ways, and most people assume that schools must cause many of these differences. In response, we have argued that people who stay in school and attend college would differ from people who now drop out even if they all had exactly the same amount of school" (1972, 135). This argument also appears in economist Arrow's (1973) depiction of higher education as a screening device that provides employers, who have imperfect information, with a proxy for an individual's productivity. What is noteworthy about Arrow's model is that productivity is exogenous and, indeed, causally prior, to higher education.[4]

We use this perspective to understand the link between higher education and political participation. We are not, however, the first to mention this possibility. Indeed, Wolfinger and Rosenstone foreshadow our view:

> Level of education indicates not only the skills and duties learned in school but characteristics of the individual that are unrelated to school . . . People who have gone to college are more likely to have educated and/or affluent parents. As a result, they are more likely to come from homes where books, newspapers, and magazines were read and where politics was discussed. By virtue of this socialization, those who have been to college have grown up exposed to politics and experienced in dealing with information about it. (1980, 20)

Although Wolfinger and Rosenstone (1980) mention that preadult socialization can influence both educational attainment and post-educational participation, their subsequent reading of the relationship between education and turnout entirely ignores this possibility. Instead, it reflects the predominant view that *education* confers participatory benefits.

In a relevant, but largely overlooked chapter in their book, *Generations and Politics,* Jennings and Niemi (1981) raise a similar argument: stratification processes occur early on, in elementary and secondary schools, and individuals who attend higher education are already different from their nonenrolled peers. The evidence that they use to support their conclusion does not settle the issue. Using raw frequencies in the data as evidence that individuals who attend college are qualitatively different from those who did not, Jennings and Niemi note that, "the better educated began to outdistance their former high school classmates quite early in the 1965-1973 period" (1981, 250). However, they do

[3]Some sociologists have focused on the stratification effects of schooling. The key question that we ask is whether higher education really *changes* individuals and thus causes participation. In contrast, the stratification literature is generally agnostic as to whether higher education changes individuals or not; it is more occupied with the systematic consequences of differences in educational attainment—for example, on economic inequality (e.g., Mare and Winship 1988).

[4]Arrow (1973) acknowledges that he puts forth a "dramatic and one-sided presentation" (194). In another classic explication, Thurow (1975) notes that educational attainment can either signal the possession of cognitive skills or some innate ability to acquire job-relevant skills. Spence (2002) proposes a model in which higher education both signals and enhances productivity. Kroch and Sjoblom (1994) present an empirical test of whether higher education in the labor market can be interpreted as human capital enhancing or merely a signal (they find the former). See Bills (2003) for an overview of the different ways that labor economists understand educational attainment. Sociologists and economists who have argued that education serves a credentialing function have largely focused on the relationship between education and dependent variables such as socioeconomic status, occupational prestige, and labor-market returns.

not have a way of systematically ascertaining whether further educational attainment provides a boost to levels of participation or not, nor of parsing out how much of participation can be ascribed to individual-level factors or educational experiences. Hence, while Jennings and Niemi's (198[illegible] line of reasoning, it does not [illegible] nor stringent, a test of the th[illegible] nor has greatly influenced la[illegible]s of the link between particip[illegible]

In another relevant [illegible]ings (1982) use path analysis [illegible]t of youth educational attainm[illegible]ipation. Using this method, they identify a complex web of interrelationships between parental SES, parental civic orientations, youth educational attainment, youth civic orientations, and young adult participation. The Beck and Jennings piece provides insight into the alternative mechanisms that might produce political participation later in life: civic orientations, high school activities, parental participation, and parental SES. However, we believe it is worth revisiting the question of how educational attainment predicts political participation, as Beck and Jennings report a significant direct effect of educational attainment on young adult participation. The significant path coefficient is estimated with the assumption that the only predictor of youth educational attainment is parental SES. While parental SES is clearly important, we go beyond this work by directly incorporating a wider array of factors.

If higher education is conditioned by prior experiences and dispositions, and if it is these prior experiences and dispositions that motivate political participation, then analyses of participation that include the level of education ignore an important set of processes set in motion prior to, and potentially supplanting, the effects of education. Although we have sketched a theory of how and why these preadult experiences and predispositions might propel later participation, we reiterate that our primary intention here is a test of the conventional claim that higher education causes participation.

Testing the Conventional Claim

We begin by analyzing the first two waves of Jennings' and Niemi's Political Socialization Panel Study. This dataset provides an excellent opportunity to trace the effects of education and preadult experiences and predispositions upon participation later in life. The panel enables us to measure contemporaneous constructs rather than recollections of experiences from a prior time period and to analyze a set of characteristics measured before and after pursuit of higher education.

This dataset captures critical moments in individual development that correspond well with our research question. The sample for the Political Socialization Study consists of high school seniors in the Class of 1965, who were randomly selected from a national probability sample of 97 high schools across the country. The first wave of the study, conducted in 1965, consists of in-person interviews with 1,669 high school seniors and in-person interviews with one or both of the student's parents. The 1973 follow-up wave reinterviewed the youth sample, as well as one of the parents.[5] We take advantage of the youth self-reports in both waves of the study. The responses from the 1965 interviews serve as independent variables, and the responses from 1973 comprise our dependent variable: political participation.

Another defining feature of the Political Socialization Study is that it contains interviews with parents. Instead of relying on youths' perceptions of parental characteristics, we have access to parents' contemporaneous self-reports on their resources, political interest, political involvement, etc. This feature of the study enables us to avoid difficulties in disentangling youths' perceptions of their parents from parents' own reports on their beliefs, orientations, and activities (see Beck 1977, 119 for a discussion of this virtue).[6] Hence, we include a rich

[5] The first two waves of the Socialization Study bracket an explosive time in American politics—an era that includes the Civil Rights movement, the Women's movement, and the Vietnam War. We worried that the Vietnam War would distort the processes of educational attainment, because college attendance could be used for military deferment and because the GI Bill financed college for veterans. Rates of college attendance between individuals who experienced military service between the first and second waves of the study were virtually identical. The use of the GI Bill was quite low: only a little over 20% of respondents who performed military service reported it as a source of funding for college. Thus, we believe our results are not significantly affected by this aspect of history. Additionally, we provide a second test on an independent dataset from a different era to address this concern.

[6] For one-third of the 1965 youth sample, both parents were interviewed, for one-third the mother was interviewed, and for one-third the father was interviewed. In the 1965 wave, 107 youth respondents were not paired with any parental interviews; these individuals are excluded from our analysis. Of the original 430 respondents for whom both parents were interviewed, 390 survived until the second wave of the study. To pair these 390 youth respondents with data from exactly one parental interview, we randomly selected data from one of the two parents. After removing observations that failed to survive into the 1973 wave, 1,254 respondents remained.

variety of parental reports, measured in 1965, as independent variables.

The dependent variable measures adult political participation, as reported by youth respondents from the 1973 reinterview. It consists of an unweighted, additive index of the following participating in the 1972 Presidential election campaign meetings/rallies, displaying a campaign bumper sticker, working on a campaign, contacting a public official, participating in a demonstration, and working with others on an issue.[7] Consistent with extant evidence, the distribution of participation is highly skewed: the majority of respondents participate in one or very few acts, while very few individuals participate in most or all acts.[8]

We examine a dichotomous indicator of education: college attendance. We made this choice because, especially for the time period examined, attending college is figuratively a step in individuals' lives (not merely a continuation of high school), one that we can code methodologically as a "step" function using a dichotomous indicator of whether an individual attended college or not. This choice is consistent with much of the literature on educational attainment, which increasingly has focused on transitions across stages of education (a key transition being from high school to college entry) rather than cumulative years of education (e.g., a classic piece by Sewell, Haller, and Portes 1969 and more recent pieces by Cameron and Heckman 2001; Mare 1980; Hout, Raftery, and Bell 1993).[9] Figure 1 displays differences in participation, across individuals who attended college and those who did not.

We see stark differences in the level of participation across individuals who attended college and those who did not. While the shapes of the distributions are roughly similar, at each level of participation, college attendees perform significantly more participatory acts than those who did not attend college. Typical analyses of participation determine the "effect" of education on participation by including it, along with a series of covariates, in a regression model. But given that educational attainment is nonrandomly assigned, can the empirical differences in levels of participation across individuals who did and did not attend college really be interpreted as the "effect" of higher education?

Addressing Nonrandom Assignment Through Matching

We say the answer is "No." The nonrandom process that characterizes educational attainment makes simple comparisons across individuals who attend college and who do not elect to attend college problematic, especially if the processes that lead to educational attainment also lead to political participation. This problem of inference is attributable to the lack of randomization of assignment to the "treatment" of attending college: a classic nonrandom assignment problem.

Typical regression analyses assume that independent variables are "fixed in repeated sampling." Another way to conceptualize this assumption is that the levels on independent variables are randomly assigned; observations hold particular levels of X's, and variation in other covariates or other unobserved processes would not alter these levels of these X's. Educational attainment, however, is not a random process. Regression analyses that ignore nonrandom assignment, where the process underlying assignment is correlated with the outcome of interest, face the threat of biased and inconsistent statistical estimates (Achen 1986). The traditional manner of dealing with nonrandom assignment issues is through instrumental variable estimation; the weakness of such a technique is that credible instruments, that is, exogenous variables that are used to identify the system of equations and that predict assignment but not the outcome of interest, are difficult to find (Achen 1986; Bartels 1991).[10]

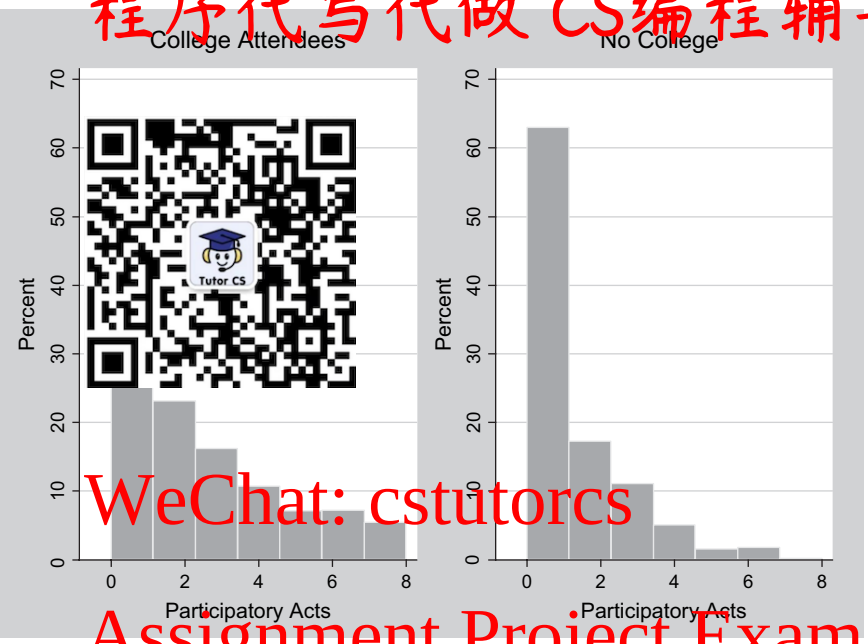
[7]The scale mean is 2.29 acts (of a total of 8), with a standard deviation of 3.58, and Cronbach's $\alpha = 0.70$.

[8]In constructing this index, the 18 cases that were missing on more than two of the acts were dropped.

[9]Our sample omits high school dropouts. This limits our ability to generalize our results to this group. For the civilian noninstitutional population in grades 10–12, the dropout rate in 1967 (the earliest year where comparable data were available) was 5.2%. By 2002, this rate had fallen to 3.3%. Hence, even though we cannot generalize to this group of individuals, it is a rather small group. (Bureau of the Census: http://www.census.gov/population/socdemo/school/tabA-4.xls). To further justify how our designation of the "treatment," an analysis of the cumulative National Election Studies reveals that the modal categories in educational attainment among all cohorts born after 1942 were high school degree (or its equivalency) and some college.

[10]Some economists have used instrumental variables regression to estimate the returns to education on earnings, occupational status, and (to a limited extent) political engagement. To support the identification restriction, these instruments must be related to college enrollment but unrelated to the ultimate dependent variable. For example, Milligan, Moretti, and Oreopoulos (2004) and Dee (2004) use geographic proximity to postsecondary institutions and state-level compulsory education laws as instruments for college enrollment. Capturing believable instruments that satisfy the identification requirements of instrumental variables analysis is often an insurmountable obstacle.

FIGURE 1 **Participatory Acts, By College Attendance, 1965–73 Waves of the Political Socialization Study**



Recently, researchers in the biometric and social sciences have gained purchase on questions of causality by using matching (e.g., Morgan and Harding 2006; Winship and Morgan 1999). Matching is surprisingly underutilized in political science, despite its potential utility in overcoming nonrandom assignment issues in both experimental and observational data.[11] Matching allows a researcher to estimate causal effects as defined by the Rubin causal model (Holland 1986; Rubin 1973, 1974). Rubin initially developed this causal model to identify the effect of some treatment, T, in the absence of randomized experimental data. Consider two possible dependent variables: $Y_{\sim T}$ and Y_T. The former, $Y_{\sim T}$, refers to a case's response, in the absence of the treatment. The latter, Y_T, refers to a case's response, in the presence of a treatment. The effect of some treatment, T, would ideally be estimated by the quantity $Y_T - Y_{\sim T}$. However, for a given case, using observational data, typically only one of the two responses is observable. That is, the counterfactual is never observed; for a given case, the treatment is either administered and Y_T observed, or the treatment is withheld and $Y_{\sim T}$ is observed.

[11]Rubin's (1973) early discussion of this technique focused on its utility in observational studies, while one of the most prominent recent political science example has focused on matching techniques in experimental studies where complete randomization was not achieved (see (Imai 2005); but also see the reply by Gerber and Green (2005)). For additional political science examples, see Barabas (2004); Diamond (2006); Sekhon (2004); Simmons and Hopkins (2005).

Given that the counterfactual is not generally available for observational data, one solution is to attempt to estimate an average treatment effect: $E(Y_T - Y_{\sim T}) = E(Y_T) - E(Y_{\sim T})$. As Holland notes, examining the average treatment effect "replaces the impossible-to-observe causal effect of *[T]* on a specific unit with the possible-to-estimate *average* causal effect of *[T]* over a population of units" (1986, 947). The key notion is that the expected value of the response across two sets of essentially equivalent groups can be compared.

Identifying "essentially equivalent" groups is the key challenge for matching. In some applications, treated and untreated cases can be paired using exact matching on a given set of covariates; that is, treated and untreated cases might be paired based on categorical variables such as sex. However, in datasets with numerous covariates, plus with continuous or multivalued covariates, exact matching can be impossible to achieve (Ho et al. 2007). Rosenbaum and Rubin (1983, 1985) show that the information from numerous covariates can be summarized by a scalar (a propensity score), and matched groups can be constructed on the basis of the propensity score. This is the technique we have elected to use, given the many variables that are available to us in our dataset. Through propensity-score matching, treated respondents (for the purposes of this study, those who attend college) are paired with "essentially equivalent" control respondents, and the average treatment effect

consists of the mean difference in participation across the matched groups (Ho et al. 2007; Rosenbaum and Rubin 1983, 1985; Rubin 1973). As with any statistical methodology, propensity-score matching imposes assumptions. One key assumption is that observables can account for the selection into treatment; any remaining bias due to treatment is not systematic in the assignment process, and therefore can be determined through comparing the group with the matched nontreated and Wahba note:

> . . . conditioning on observables can take assignment to treatment to have been random and that, in particular, unobservables play no role in the treatment assignment; comparing two individuals with the same observable characteristics, one of whom was treated and one of whom was not, is . . . like comparing those two individuals in a randomized experiment" (2002, 153).

In short, propensity-score matching allows us to analyze observational data through the lens of an experimental design. A common estimand, once assignment to treatment has been conditioned on observables, is the *Average Treatment Effect for the Treated*: $\tau\ (T=1) = E[(Y_T - Y_{\sim T})|T=1]$. The treatment effect for the treated provides the estimate of how much change was experienced by those who actually received the treatment. This is what we will estimate.[12]

The propensity score is an estimate of each observation's likelihood of having received the treatment. We generate the propensity score by first estimating a logistic regression where the dependent variable is the treatment (attending college). We predict college attendance, as ascertained in the 1973 reinterview, using a series of covariates theorized to be substantively related to the decision to attend college.[13] Each of these covariates is measured in the 1965 interviews with youth and parents, temporally prior to the dependent variable (college attendance). The panel data are particularly useful in this regard, as we are able to predict a subsequent behavior (going to college) using measures collected *ex ante* that predate the behavior of interest (rather than relying on *post hoc* recollections of high school experiences that could themselves be contaminated by going to college). Further, the dataset provides additional leverage in generating the propensity score, because it contains parental characteristics collected through direct interviews with parents.

The propensity score for each individual i is the predicted probability of attending college, based on this logistic regression.[14] The propensity scores are then used to pair treated observations with similarly ranked control observations. Each respondent who received the treatment (i.e., a respondent who went to college, in our research question) is matched with a set of untreated respondents (i.e., respondents who did not attend college) that have similar propensity scores. This technique is called nearest-neighbor matching.[15] Constructing these sets of matched observations allows us, in essence, to consider assignment to the

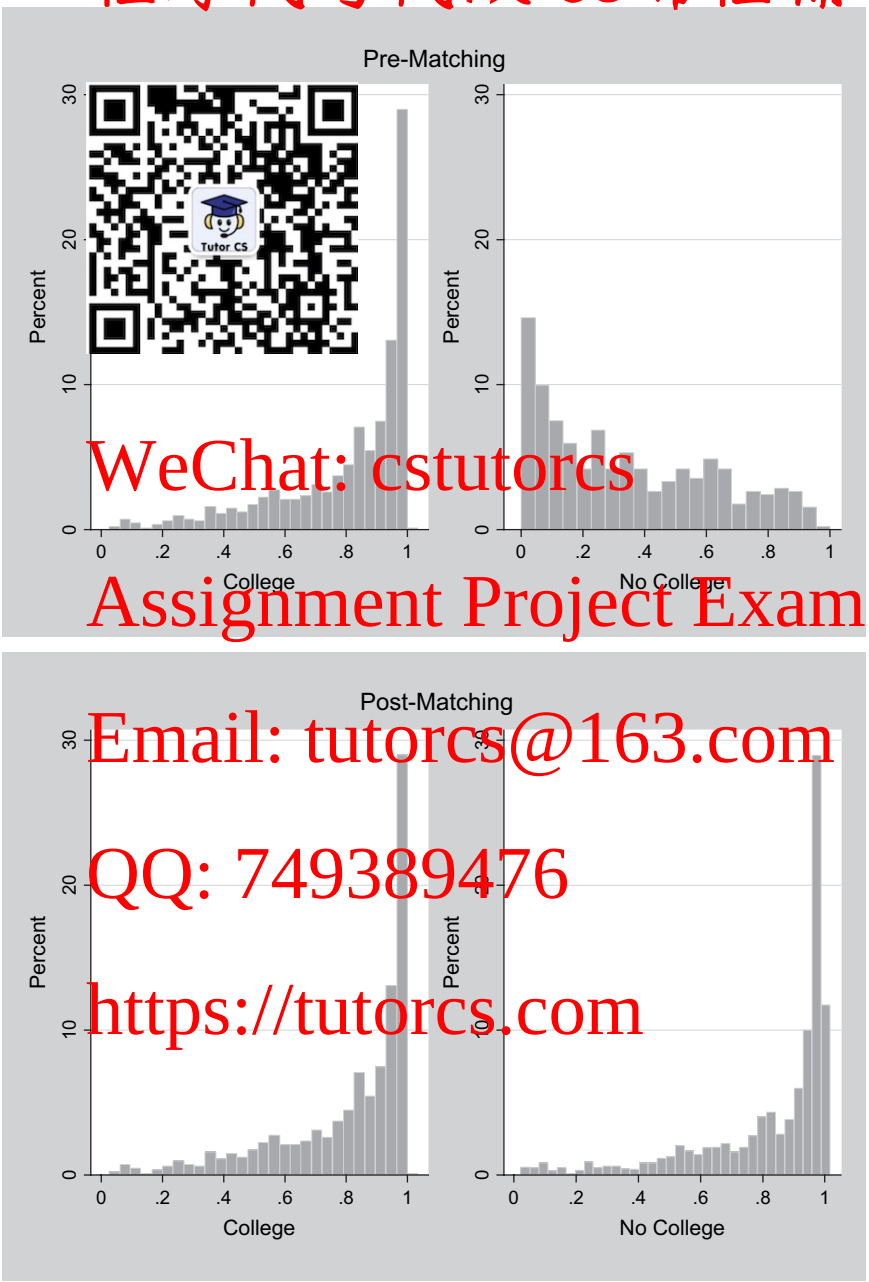
[12]See Note 30 for other estimands.

[13]The online appendix at http://journalofpolitics.org provides a complete list of the propensity score covariates. All categorical covariates are entered into the propensity score as a series of dummy variables (Sekhon 2004). The underlying process for selecting covariates for the propensity score regression is the same as for any other model specification: all covariates that would be included in the logistic regression predicting the treatment, even if only weakly predictive, should be included in the propensity score equation (Ho et al. 2007). Balance on relevant covariates must also be taken into consideration in generating the propensity score model. To produce the final propensity score model we report here, we identified the model that produced maximal balance on relevant covariates. See Note 17 for a discussion of balancing. Propensity score specification and balancing go hand in hand. See Diamond and Sekhon (2005) for an innovative procedure that uses a genetic algorithm to identify the propensity score model that maximizes balance.

[14]A second assumption in matching is that the propensity score model is correctly specified. This assumption of correct specification is standard fare in nearly any analysis (e.g., OLS, probit, logit models nearly all assume correct specification), and, as such, is not unique to propensity-score matching. If the propensity score model is misspecified, estimates of the treatment effect could be biased. In their Monte Carlo experiments, however, Morgan and Harding (2006) find that ignoring nonrandom assignment (estimating a typical OLS regression model) yields severely biased estimates of the treatment effect; the bias is considerably less pronounced in the presence of even a misspecified propensity score model. To address this issue, we conduct and report on sensitivity tests in Note 20.

[15]Nearest-neighbor matching allows the researcher to specify some integer value m of control cases to pair with each treated case. These control cases are those *closest* to the treated case on the propensity score (Dehejia and Wahba 2002) or on its linear index (Diamond and Sekhon 2005). A modification of nearest neighbor matching imposes a caliper, which constrains the distance allowed for the nearest neighbor procedure. Caliper matching limits the distance between a given treatment case and eligible control matches. When we specified a caliper of 0.25 standard deviations, as seems standard in the literature (Rosenbaum and Rubin 1985), our results were unchanged. Another method, aside from propensity-score matching, is to match on the Mahalanobis metric, which essentially is the distance between column vectors of the X's (multiplied by the inverse of the sample covariance matrix; Diamond and Sekhon 2005; Rosenbaum and Rubin 1985).

FIGURE 2 **Comparison of Propensity Scores, Pre- and Post-Matching, 1965–73 Waves of the Political Socialization Study**



treatment condition as "randomized" once the observations are matched along the propensity score. The matching process essentially mimics random assignment to treatment and control groups. Using these matched treatment and control groups, we can then determine the degree to which there are preadult experiences and influences that, in occurring prior to college attendance, affect adult participation above and beyond, or instead of, the posited effects of education. Figure 2 illustrates the results of matching.[16]

The top half of Figure 2 provides the histograms of estimates for the treatment and control group propensity scores, prior to matching. Here, the two groups are extremely different; those who have gone

[16]The propensity-score generation, matching, and balancing were implemented using R-code developed by Sekhon (2007).

TABLE 1 Participation, Across Individuals Who Did and Did Not Attend College

	Pre-Matching	Post-Matching
Mean Participation, College Attendees (standard error)	2.79 (0.07)	2.79 (0.07)
Mean Participation, No Co… (standard error)	1.43 (0.07)	2.77 (0.04)
Comparison of the two gr…		
Difference in Means (s.e.	1.37 (0.10)	0.023 (0.47)
t-statistic	13.04	0.05
p-value	0.00	0.96
N	1,254	3,250

Source: 1965–1973 Waves of t… udy.
Note: Post-Matching N consists of 803 treated observations plus three nearest neighbors: 803 + 2,447 matched controls = 3,250.

to college manifest much higher propensity scores than those who do not. The mass of the distribution is skewed to the right for individuals who attended college, and the mass of the distribution is skewed to the left for those who did not attend college. The matching procedure takes each individual who attended college and matches the respondent with three individuals who did not attend college, but who have similar propensity scores. Matching produces the histograms of estimates in the lower panel of Figure 2, where the distributions for the paired propensity scores now nearly entirely overlap. The matched pairs of treatment and control observations are virtually identical in their propensity to attend college.

The interpretation of differences between matched treatment and control observations depends upon our ability to show that the matched pairs of treatment and control observations are exchangeable once we have conditioned on a long series of relevant covariates. This post-matching validation is called *balancing*. A successful match requires us to determine whether the matched pairs of treatment and control observations are distributionally similar on a series of substantively relevant covariates.[17] We checked for balance on a series of covariates that we believed would be substantively important to our research question and achieved balance on youth respondents' plans to continue with school, parental reports of head of household income, parental participation, and parental political knowledge.

Youth who are more attentive to the political process (by reading magazines about politics, engaging in political discussion with their family, possessing more political knowledge) are more likely to attend college. Frequent participation in extracurricular activities such as neighborhood clubs and service organizations also increases the likelihood of attending college. Youth with parents who are more engaged in the political process (through news media use and political participation), and who are more active within the community (in church, professional, or civic organizations) are more likely to attend college. Youth whose parents are employed, earn higher income, and are more educated are also more likely to attend college. The fit of our propensity score is very good; the full model correctly predicts 81% of observations, an improvement of 17% above the base model.

Matched Groups and Levels of Participation

After matching, the outcome of interest can be compared across the matched treated and control groups.[18] We focus on the difference in mean levels

[17]Balance tests typically examine the univariate distributions of single variables across the matched treatment and control groups as well as multivariate distributions across a collection of variables, comparing the matched treatment and control groups. Univariate tests are t-tests of difference of means between the matched treatment and control groups and difference of variance-tests. Where applicable, we have also used Kolgomorov-Smirnov tests of distributional similarity (these cannot be used on dichotomous indicators).

[18]Indeed, one of the advantages of matching, assuming that matched control and treated groups are well balanced, is that a simple comparison of matched treated and control groups can be used; the analyst need not rely on parametric assumptions (e.g., of linearity) commonly employed in regression analysis.

of participation across individuals who did and did not attend college. Prior to running the matching procedure on the data, we conducted a simple difference of means test to determine the difference in level of participation among individuals who attended college con[illegible] did not.

The first column of [illegible] estimated differences betwe[illegible]ntrol groups on the number [illegible]acts. Prior to matching, we se[illegible]ween treatment and control g[illegible] attended college averaged [illegible]rticipatory acts compared with individuals who did not attend college, who averaged approximately 1.43 participatory acts. The difference of means is statistically significant at $p < 0.001$.[19]

As Figure 2 illustrated, the matching procedure pairs respondents who attended college with respondents who had a similar propensity to attend college but did not attend. In this way, matching enables us to mimic an experimental design. If the treatment (attending college) serves a human-capital enhancing function that provides those who attend with the resources and skills necessary for participating in politics, then adult participation levels across matched treatment and control groups will differ: even though the individuals were similar (based on their 1965 covariates), participation levels as assessed in 1973 will diverge. If the treatment does not confer participation-enhancing benefits to college attendees, then no difference in participation rates will be observable across the groups.

After matching, we observe a dramatic decline in the difference in the number of political acts across groups. The treatment group maintains an average number of political acts of 2.79, since all respondents in the treatment group are retained for analysis. The matched control group now averages 2.77 participatory acts. The difference between the treatment and control group plummets substantially, from a gap of 1.37 acts to only 0.02 acts, and the treatment and control groups are statistically indistinguishable from one another in participation. Our results suggest that education is to a substantial degree a *proxy* for preadult experiences and orientations that subsequently propel individuals to participate in politics.[20]

[19]Observed differences on the aggregated scale cannot be attributed to just one or two specific acts. College attendees have significantly higher participation rates than nonenrollees on each and every act in the scale (all differences in proportions tests are significant at $p < 0.000001$).

[20]To determine whether our results were sensitive to our specification of the propensity score model and the subsequent matching procedure, we repeated the matching process 81 times, each time dropping a variable from the generation of the propensity score, generating matched groups, and recalculating the treatment effect. Across the 81 specifications, nearly all of them suggest that the levels of participation across the matched treatment and control groups are statistically indistinguishable from each other. After matching, the treatment effect in 80 of the 81 specifications is statistically indistinguishable from 0. The average of the treatment effects, calculated across the 81 specifications is 0.08, with a standard deviation of 0.17. A statistically significant treatment effect appears only in one of the 81 specifications, which is far lower than what we might have even expected "by chance."

We also worried that our results could be disproportionately driven by a small set of observations. To test whether this was the case, we first dropped cases at extreme values on the propensity score (the 5% of cases with the lowest scores and the 5% of cases with the highest scores). We reran the matching analysis and obtained substantively and statistically similar results. The treatment effect was .088, with a standard error of 0.42 and a p-value of 0.83.

We use a "greedy" matching algorithm, which allows control observations to be used multiple times as matches for the treated observations. Matching with replacement is generally viewed as more desirable than matching without replacement because it produces "matches of higher quality than matching without replacement" (Abadie and Imbens 2006, 240) and because it reduces bias (Dehejia and Wahba 2002). In their review of matching estimators, Morgan and Harding (2006) advise nearest-neighbor caliper matching with replacement. As a second robustness check against whether our results were disproportionately driven by a small set of observations, we reestimated our analyses, omitting control cases that were used over 20 times. This ensures that our matched control group is not disproportionately driven by a handful of cases that were repeatedly matched with treated observations. Reestimating our results produced substantively and statistically similar results. The High School and Beyond Study (which we analyze, below) has a much bigger sample size (and a significantly larger control to treatment ratio). In that study, we have a much lower rate of repeatedly using observations, and the results are substantively similar to those for the Political Socialization Study analysis.

We also reestimated our models with a caliper of 0.25 standard deviations (Rosenbaum and Rubin 1985) to limit how "far away" on the propensity score matches can be. Imposing a caliper did not change the substantive or statistical results.

Our results do not depend on the number of neighbors we used. We chose to use three nearest neighbors, based on Sekhon (2004). Using two or four nearest neighbors yields similar results.

Another concern is that our results might be driven disproportionately by one act: voting. We reestimated the treatment effect, with a participation scale that excluded voting. No statistically distinguishable effect attributable to college attendance appeared (the treatment effect was -0.11, with a standard error of 0.42 and a p-value of 0.79).

We applied each of these robustness checks to analysis of the third wave of the Political Socialization Study and to analysis of the High School and Beyond Study (both of which we discuss below), and they also produced substantively and statistically similar results.

TABLE 2 **Participation Later in Life, Across Individuals Who Did and Did Not Attend College**

	Pre-Matching	Post-Matching
Mean Participation, College Attendees (standard error)	2.94 (0.08)	2.94 (0.08)
Mean Participation, No College (standard error)	1.83 (0.09)	2.78 (0.04)
Comparison of the two gr		
Difference in Means (s.e.	1.11 (0.12)	0.15 (0.48)
t-statistic	9.22	0.32
p-value	0.00	0.74
N	1,051	3,104

Source: 1965–82 Waves of the [illegible]y.

Delayed Effects: Analyzing Participation Later in Life

Thus far, we have shown that the effects of higher education on participation when respondents are in their mid-20s, after preadult experiences are controlled through propensity-score matching, are indistinguishable from zero. One possible counterargument against these results is that the effects of education, if indeed there are any, need time to take root. Perhaps if we were to analyze the effects of education on participation later in life, we might find that differences by educational experiences would emerge. We can test this argument by analyzing political participation as reported in the third wave of the Political Socialization Study, which took place in 1982, when respondents were in their mid-30s.[21]

We used the same propensity score model to generate matched treatment and control groups, but now we analyze differences in participation, based on responses from the 1982 interview.[22] Our results appear in Table 2. Prior to matching, and consistent with our analysis of the previous wave, respondents who did not attend college participated in significantly fewer political acts than their counterparts who did attend college (an average of 1.83 acts compared with an average of 2.94 acts, respectively, with difference of means significant at $p < 0.001$). After applying the matching procedure and ensuring balance on a set of relevant covariates,[23] we find that the treated and control groups are separated by only 0.15, with standard error of 0.48; the null hypothesis that the difference of means between the two groups is 0 cannot be rejected, with p~0.74. In short, selection processes that affect educational attainment determine participation in the short-term eight years following high school. Further, these selection processes continue to influence participation up to 17 years thereafter.

Past the Vietnam Generation: An Independent Replication

Up to this point, we have utilized a dataset that uniquely positions us to capture family and individual characteristics in the preadult years. The tradeoff to this dataset is that our results might reflect life patterns for a specific cohort: the Class of 1965, and thus pose limited generalizability to cohorts coming of age in other eras. To be sure, college education has become a much less exclusive experience since the Class of 1965 came of age. In 1970, 25.7% of 18–24 year olds were enrolled in degree-granting universities; by 2003, that proportion increased to 38.7% (Fox, Connolly, and Snyder 2005, 52). Systematic processes still determine educational enrollment, sorting into institutions, and completion rates (Pascarella and Terenzini 2005). As the pool of individuals who attend college becomes more heterogeneous and thus less self-selected (Karen 1991), the key nonrandom assignment indicator may shift from

[21]About 84% of respondents who completed the 1973 wave were reinterviewed in 1982 (yielding a 68% retention rate between 1965 and 1982). The number of observations for our purposes is 1,051, of which 675 reported having attended college, as of the 1973 interview.

[22]The dependent variable is an additive scale identical to that used in the 1965–73 analyses, consisting of participation in eight acts: voting (in either 1976 or 1980), attending campaign meetings/rallies, displaying a campaign button/bumper sticker, working on a campaign, donating to a campaign, contacting a public official, participating in a demonstration, and working with others to solve a local issue. Most of these questions ask respondents to reveal whether they have participated in any of these acts since 1973, whereas typical surveys ask for only a one or two-year retrospection. As a consequence of this longer time period, the scale mean is a bit higher than might ordinarily be expected. The average respondent participated in 2.54 acts, with a standard deviation of 1.96, and Cronbach's $\alpha = 0.70$.

[23]Covariates used for the propensity score model appear in the online appendix. We obtain balance on youth respondents' plans for the next year, parental voting behavior, parental attempts at political persuasion, and household income.

TABLE 3 **Participation in a Different Cohort, Across Individuals Who Did and Did Not Complete College**

	Pre-Matching	Post-Matching
Mean Participation, College Completed (standard error)	1.61 (0.03)	1.61 (0.03)
Mean Participation, Not Completed (standard error)	1.24 (0.01)	1.59 (0.03)
Comparison of the two gro		
Difference in Means (s.e.)	0.37 (0.03)	0.026 (0.06)
t-statistic	11.10	0.42
p-value	0.00	0.67
N	9,711	15,558

Source: 1980–86 Waves of the udy.

mere college attendance t hich is still a selective outcome (Baker and Vélez 1996), or the type of institution (e.g., two-year vs. four-year college) into which the individual matriculates, which is also a selective outcome (Karen 1991).[24] Both alternatives would still be characterized as nonrandom assignment processes with comparable implications for ensuing interpretations of the link between education and political participation.

We address this issue of generalizability by analyzing data from the High School and Beyond Study (HSB). The HSB dataset is designed and administered by the National Center for Education Statistics in the Department of Education. This panel study contains interviews with high school seniors in 1980, plus a follow-up set of interviews in 1986.[25] The sample was drawn from a stratified sample of 36 seniors from each of 1,100 secondary schools selected for participation in the study (U.S. Department of Education 1987). One drawback of the HSB is that it contains parental interviews with only a subset of the sample. In order to utilize the full sample, we confine our analysis to responses reported by the high school students themselves. This data limitation precludes us from crafting as complete a picture of the various familial, social, and economic factors that predict educational attainment, but the dataset contains several obvious advantages. We test the same propositions that we advanced above, on an entirely different cohort, with data collected through an entirely different survey house, using a dataset that is about five times larger than that of the Political Socialization Study.

The first wave of the HSB consists of interviews with 28,240 high school seniors in 1980. Follow-up interviews with a sizable subsample of the original base sample occurred on a biennial basis until 1986, and a rich array of political participation questions were administered—but only in the 1986 wave. Hence, we restrict our analysis to data from the initial interviews in 1980 and the follow-up interviews in 1986, yielding 9,711 observations.

Consistent with our reasoning that the selection processes into college may have shifted over time, our treatment variable using the HSB sample consists of an indicator of whether or not the respondent attained a four-year college degree; in the HSB sample, only 18.4% of respondents completed a four-year degree or higher.[26] The dependent variable consists of an additive scale of six acts: voting in the 1984 or 1986 elections, attending political meetings, performing campaign work, making campaign donations, talking with public officials about an issue, and holding political office. The average number of acts among those who did not earn a four-year college degree was 1.24, compared with an average of 1.61, and the means are significantly different at $p < 0.0001$.[27] We apply the matching procedure and ensure balance on

[24]But see Kane (2004) and Haveman and Smeeding (2006) for discussions of the widening inequalities (by income and race) in college attendance; they argue that although educational attainment has risen generally, these gains have disproportionately been experienced by youth from the highest income categories.

[25]High school sophomores were also interviewed, but few political variables appeared on the questionnaires.

[26]Constructing the treatment variable this way, and using a dataset with a much larger sample size, improves several statistical aspects of the estimation. One downside of the Political Socialization Study is limited sample size (in particular, a smaller control group relative to the treatment group). The nearest-neighbor matching technique with replacement caused several respondents to be used multiple times (see Note 20). Although we tested the sensitivity of our results by limiting the number of times respondents could be used, and by omitting from analysis respondents at the extremes on the propensity score, this second dataset, given the absolute and relative size of the control group, provides a better arena for identifying matched respondents for the treated cases.

[27]The difference is more pronounced when we apply a less stringent treatment variable: attainment of two- or three-year vocational degree or a four-year college degree. 7.3% of respondents obtained a two- or three-year vocational degree. Individuals who did not acquire a two, three, or four-year college degree participated in significantly fewer political acts, on average, than those who did acquire a two, three, or four-year college degree. Analyses using this treatment variable were not substantially different.

a set of relevant covariates.[28] The results appear in Table 3. We find that the treated and control groups are separated by only 0.03 SDs, with a standard error of 0.06. The null hypothesis that the difference of means between the two groups is 0 cannot be rejected, with p~0.67. T[illegible] for independent replication of our results [illegible] set in a different time period. [illegible]ences and dispositions are take[illegible] edu-cation appears to confer [illegible]ncing benefits.

Really? Does This Mean Higher Education Has No Effect?

Our analyses of the three waves of the Political Socialization Study and of the two waves of the High School and Beyond senior cohort suggest that the positive relationship between college and political participation derives largely from the preadult socialization experiences that propel individuals to pursue higher education, not from the acquisition of higher education per se. But, the scholarly literature on political participation suggests that political participation arises from individual-level motivations as well as from mobilization by external agents. That is, there is both an internal push and an external pull to participate. Even if educational experiences do not enhance the internal push to participate, perhaps they serve as an external pull by physically aggregating individuals in an easily tapped locale.[29]

We expect this to be the case especially with respect to the late 1960s and early 1970s, where college campuses "were repeatedly the locus, and sometimes the focus, of demonstrations, rallies, and confrontations with officials and fellow citizens" (Jennings and Niemi 1991, 253). Recall that in our first analysis, we focused on a participatory scale comprised of eight acts. To determine whether in this particular time period colleges served as sites for mobilizing individuals in protest behaviors, we repeated our matching analyses using the Political Socialization 1965–73 Panel, estimating a treatment effect for each participatory act separately. We found no significant differences between college attendees and nonattendees across seven of the eight acts. Consistent with the idea that college campuses in the late 1960s and early 1970s served as mobilizing sites for protests, the only significant difference pertained to participating in a demonstration. After matching, individuals who attended college were 18.1% more likely to participate in a demonstration (with a standard error of 0.078), compared with the matched individuals who did not attend college. The difference is statistically distinguishable from 0 at $p < 0.01$. For the first two waves of the Political Socialization Study, then, college attendance has a specific effect limited to political protest. This effect does not replicate in the High School and Beyond analysis, where we found no statistically significant differences attributable to types of acts. The contrast in these results suggests that in extraordinary circumstances, where college campuses served as a focal point for pulling individuals into political action, that higher education did, indeed, spur political participation in the form of protest.

Conclusions

Empirical analyses disentangling the effects of education per se from preadult experiences are difficult to find, perhaps due to a lack of committed interest among political scientists to the study of political socialization and perhaps due to data limitations in studying this phenomenon. Our analysis, however, should have bearing on how scholars of mass behavior *interpret* the empirical linkages between education and participation. By highlighting the nonrandom assignment processes at work in determining who attends college, we show that the empirical linkage between higher education and participation should not be attributed to higher education per se. Instead, the same factors that propel individuals to pursue higher education also appear to propel them to participate in politics. Rather than conferring benefits that in turn cause participation, higher education appears to proxy *other* factors that catalyze political participation. The one exception to this rule revolves around the extraordinary upheaval in college campuses in the late 1960s and 1970s, where we find that

[28]We obtain balance on the vast majority of variables included in the propensity score generation, including: youth respondents' high school grades, gender, father's occupation, mother's occupation, father's education, mother's education, household income, and parental homeownership. This, too, is an advantage of the High School and Beyond dataset. The relatively larger size of the control group enables us to identify matched control respondents that are virtually identical to the treated respondents. We achieve significantly better balance using the HSB dataset compared with the Political Socialization Study, but the substantive results are similar across the two studies.

[29]We are grateful to Laura Stoker for suggesting this line of analysis.

college attendance increased the likelihood of participating in protests.

We set out to test the conventional claim that higher education might confer human capital and social-positioning benefits that in turn boost political participation. Through ou[illegible]sing the three waves of the P[illegible]tudy and two waves of the High[illegible]udy, we have cast doubt on th[illegible] and have instead suggested a[illegible]eting the positive empirical re[illegible]gher education and political [illegible]have pointed to a complex con[illegible]eady in place during adolescence that propels individuals into higher education. In so doing, we have shifted the temporal spotlight from what happens during college to what happens before it, during adolescence and childhood. We have argued that these preadult processes, not higher education per se, account for political participation in the adult years.

Yet what exactly are these factors that propel individuals to acquire education and to participate in political life? We concede that it is beyond the scope of this article to answer this question with precision. Our primary purpose has been to highlight the complexity underlying the relationship between higher education and participation and to encourage a "sober second thought" for scholars of political participation who routinely include education on the right-hand side of their equations and interpret it as an exogenous variable and as a causal agent. Our hope, too, is that this article will in turn motivate additional research that will attempt to adjudicate among conceptual frameworks to pin down the relative importance of the various explanations we have outlined for the empirical linkage between education and participation.

It is worth mentioning that we as political scientists probably project own biases about what we expect to see as a consequence of higher education. Maybe we expect our students to change and to be enlightened during college. Students very well might change during their college years; nothing in our analysis suggests that college students fail to do so. Our analyses show that the level of participation among those who enrolled in college is no different from those who were essentially identical but did not enroll. This group of the essentially identical but nonenrolled is a group that we (as faculty) generally do not see. And perhaps, for whatever reason, we assume that because they are not enrolled in college, exposed to lectures and campus life, that they are not undergoing changes. We note that the campus is not the only place where reinforcement of predispositions to participate can occur. Other environments can fit the bill—such as the workplace, religious institutions, civic organizations, trade unions, etc. In these environments, even individuals who did not attend college can still enhance their civic skills or be pulled into political participation (Rosenstone and Hansen [1993] 2003; Verba, Schlozman, and Brady 1995). While (some) college students might be exposed to reinforcing influences, (some) noncollege youth might also be exposed to reinforcing influences, thus accounting for the absence of a treatment effect.

We also note that there is heterogeneity among both the enrolled the nonenrolled. Hillygus (2006), for example, finds that social science majors are more likely to participate than business school and science majors. Just as we see heterogeneity among college students, we might also expect to find heterogeneity across the essentially identical matched nonenrollees. For example, a matched pair consisting of a mechanic and a mechanical engineering major may both be unlikely to participate in politics. A matched pair consisting of a political science major and a nonenrolled youth who devours political news on the sides may both be highly likely to participate in politics.

Our work also highlights the importance of acknowledging nonrandom assignment in estimating the impact of self-selected experiences. While we have taken this opportunity to examine enrollment and completion of college, other researchers have considered nonrandom assignment processes in examining other aspects of higher education, such as the effect of diversity (see, e.g., Kuklinski's 2006 review of research on the effect of institutional diversity on educational outcomes). Our hope is that this article will not only affect how researchers understand the causal effect of higher education, but also will shape the ways in which researchers view other nonrandomly assigned factors often implicitly assumed to be randomly assigned.

Our substantive results have a clear normative impact for our understanding of the role of education in politics. Education has long been considered a potential cure—the "universal solvent" (Converse 1972, 324) that might alleviate participatory inequalities. However, if the effects of higher education are in fact only minimal, and preadult predispositions and experiences in the home and in primary and secondary schools actually play a role in spurring participation, then those who seek to remedy inequalities in participation must look to these agents of socialization for remedies. Indeed, our analyses have only

estimated the causal effect of higher education on adult political participation.[30] Education might still affect political participation—but in the earlier years—in primary and secondary schools. Alternatively, attention might be focused instead on external agents of mobilization, [illegible] interest groups, and particularly [illegible]ions. These agents may provic[illegible] qual-izing political access by [illegible]ularly those who might not be [illegible] into participating in politics.

Acknow[illegible]

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[30]Moreover, we have estimated the treatment effect for the *treated*, which compares adult political participation across those who went to college and a set of matched control observations who happened not to go to college but were similarly ranked on their propensity to attend. Estimating the treatment effect for the treated makes substantive sense to us, because it answers the question of what, if anything, college did for the respondents who went to college. Further, most policies designed to open access to higher education are directed at (and most effective for) those who are likely to go, but by some chance did not. Estimating the treatment effect for the *untreated* is a separate matter—both substantively and empirically. Substantively, the question shifts from: "What did college do for those who attended?" to the more hypothetical question of: "What would college have done for those who did not attend?" In our overview of existing studies, we found none that explicitly examined the treatment effect for the untreated (although it is easily estimable). Some did estimate the average treatment effect (which averages the treatment effect for the treated and the treatment effect for the untreated), but by and large, the majority of existing work focuses on the treatment effect for the treated. For our purposes, since we wanted to respond to the conventional claim that higher education provides participatory-enhancing benefits to those who attend, we estimated the treatment effect for the treated.

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Cindy D. Kam is associate professor of political science, Vanderbilt University, Nashville, TN 37235. Carl L. Palmer is a Ph.D. candidate in political science, University of California, Davis, Davis, CA 95616.