Are there civic returns to education?

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Abstract

The hypothesized effects of educational attainment on adult civic engagement and attitudes provide some of the most important justifications for government intervention in the market for education. In this study, I present evidence on whether these externalities exist. I assess and implement two strategies for identifying the effects of educational attainment. One is based on the availability of junior and community colleges; the other, on changes in teen exposure to child labor laws. The results suggest that educational attainment has large and statistically significant effects on subsequent voter participation and support for free speech. I also find that additional schooling appears to increase the quality of civic knowledge as measured by the frequency of newspaper readership.
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"... since the achievement of American Independence, the universal and ever-repeated argument in favor of Free Schools has been, that the general intelligence which they are capable of diffusing, and which can be imparted by no other human instrumentality, is indispensable to a republican form of government." Mann (1957)

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1. Introduction

Economists typically justify the government's extensive and varied involvement in the market for education by appealing to distributional concerns and several types of market failures. The most frequently discussed types of market failure involve the positive externalities that might be associated with schooling. For example, some have argued that education generates external social benefits by reducing the prevalence of crime and by promoting knowledge spillovers and technology diffusion in the workplace.[1] However, the externality that is arguably featured most prominently in discussions about education involves civic behaviors and attitudes. Specifically, it is widely believed that education is an essential component of a stable democratic society because it encourages citizens to participate in democratic processes and prepares them to do so in an informed and intelligent manner. The putative existence of such civic returns to education motivated the proliferation of common schools in the early 19th century and early educational reformers like Horace Mann and continues to provide one of the most important justifications for the many public policies and institutions that promote access to all levels of education.

An extensive, empirical literature in political science has documented a strong correlation between educational attainment and various civic behaviors. In particular, this literature has demonstrated that higher levels of schooling are associated with substantive increases in voter turnout. Political scientists generally interpret this literature as providing strong support for the view that education is effective at promoting the quantity and quality of civic participation. However, these correlations could actually be quite misleading since both schooling and civic outcomes are simultaneously influenced by a wide variety of inherently unobservable traits specific to individuals and the families and communities in which they were reared. For example, individuals who grew up in cohesive families and communities that stressed civic responsibility may also be more likely to remain in school. The plausible existence of such unobservables implies that conventionally estimated correlations may spuriously overstate the true civic returns to education.[2]

This study attempts to construct less ambiguous empirical evidence on this policy-relevant issue by identifying the causal effects of additional schooling on civic behaviors and knowledge. The research designs adopted here essentially parallel the extensive, empirical literature on the labor-market returns to schooling (e.g., Angrist

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[1] See Wolfe and Haveman (2001) for a discussion of the nonmarket and social benefits possibly associated with education. Poterba (1996) and Taylor (1999) discuss the case for governmental intervention in the market for education and conclude that there is surprisingly little empirical evidence to indicate whether or not hypothesized, positive externalities exist. However, several recent empirical studies have assessed the effects of schooling on knowledge spillovers (e.g., Moretti, in press; Acemoglu and Angrist, 2000) and on criminal behavior (Lochner and Moretti, 2001; Witte, 1997).

[2] An additional concern is that the existence of measurement error in self-reported schooling could lead correlations to *understate* the true effects of schooling (Angrist and Krueger, 1999; Card, 1999). The direction of omitted variable biases could also be negative. For example, the high-ability individuals who continue their schooling may have higher opportunity costs and may think that voting is largely an expressive act that is extremely unlikely to actually influence policy.

and Krueger, 1999; Card, 1999). More specifically, these inferences rely critically on instrumental variables that generated possibly exogenous variation in individual levels of schooling but that should otherwise be unrelated to adult civic outcomes.[3] First, using data from the High School and Beyond (HS&B) longitudinal study, I estimate the effects of college entrance on adult voter and volunteer participation by relying on the geographic proximity and density of junior and community colleges as a teen. Then, using data from the 1972–2000 General Social Surveys (GSS), I estimate the effects of years of schooling on adult voter participation, on group memberships and on attitudes towards free speech by relying on changes in teen exposure to child labor laws (Acemoglu and Angrist, 2000). Using the GSS data, I also estimate the effects of additional schooling on the frequency of newspaper readership, an outcome that is closely related to measures of civic awareness. The results of these evaluations suggest that additional schooling, both at the secondary and post-secondary levels, had large and statistically significant effects on voter participation. I also find that the additional secondary schooling significantly increased the frequency of newspaper readership as well as the amount of support for allowing most forms of possibly controversial free speech.

2. Education and civic engagement

One of the fundamental mechanisms by which education has long been thought to generate civic externalities involves improvements in the quality of civic participation and awareness. Specifically, it is widely alleged that increases in education generate broad social benefits by allowing citizens to make more informed evaluations of the complex, social, political and technological issues that might be embedded in campaign literature, legislative initiatives and ballot referenda. However, the contemporary literature among political scientists has also put a particular stress on the positive effects that schooling may have on the likelihood of civic participation, in particular, voter turnout (e.g., Wolfinger and Rosenstone, 1980). Education could promote civic participation through at least two broad channels. First, schooling may reduce the effective costs of certain forms of civic participation. In particular, this is thought to occur because increased cognitive ability makes it easier to process complex political information, to make decisions and to circumvent the various bureaucratic and technological impediments to civic participation.[4] Second, education may increase the perceived benefits of civic engagement by promoting "democratic enlightenment" or, stated differently, by shaping individual preferences for civic activity. Similarly, it is often alleged that education plays an important public role

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[3] I discuss a variety of ad hoc empirical evidence that is consistent with the maintained assumptions regarding instrument validity.

[4] However, there are other indirect mechanisms by which education may currently lower the effective costs of voting. For example, since 13 states currently prohibit ex-felons from voting, education may also reduce the effective costs of voting through its effects on criminal activity. Similarly, to the extent that education increases the likelihood of having a driver's license, the recent expansion of "motor-voter" policies may have added to the effects of education on voter turnout.

by directly inculcating students with other fundamental democratic and pluralistic values (e.g., support for free speech, for the separation of church and state, etc.).[5] However, it is also possible that additional schooling shapes civic preferences indirectly through altering the composition of peer groups and shared social norms.

Interestingly, an economic perspective could also suggest alternative mechanisms by which additional schooling might actually reduce civic engagement. For example, by raising the opportunity cost of an individual's time, increased schooling could reduce the amount of time and attention allocated to civic activity. This could be particularly relevant for volunteering, which, unlike voting, can involve a substantial commitment of time. However, education could also reduce voter participation by promoting an awareness of voting as an essentially expressive act with an infinitesimally small probability of influencing actual policy.[6] Nonetheless, the available empirical evidence seems to provide an emphatic confirmation of the conventional view that education does promote civic engagement. Numerous studies over the last 50 years have demonstrated that higher levels of individual schooling are strongly associated with civic behaviors and knowledge.[7] For example, in a widely repeated interpretation of this empirical evidence, Converse (1972) refers to educational attainment as the "universal solvent" of political participation. Similarly, Putnam (2001) notes that "education is by far the strongest *correlate* that I have discovered of civic engagement in all its forms" (emphasis mine). Also, in their earlier study of voting participation, Wolfinger and Rosenstone (1980) suggest that their core finding is the "transcendent importance of education". However, they also note that an individual's level of schooling could easily proxy for unobserved traits that also influence civic behaviors (pp. 19–20). For example, they suggest that the types of family back-grounds that promote increased schooling may also promote increased socialization into civic activities like voting. Wolfinger and Rosenstone (1980), like other researchers in this field, have attempted to control for the possible bias in the estimated effect of education by introducing a few additional control variables (e.g., income and occupational measures) into multiple regression models. The apparent robustness of the correlations between education and civic outcomes has led most researchers to conclude that education does have a causal effect. For example, in the most recent contribution to this literature, Nie and Hillygus (2001) note that this orthodox view is "largely uncontested".

However, the basic approach of introducing a few additional controls may not convincingly resolve the question of whether the strong correlations between education and civic outcomes actually reflect the true causal effects. In particular, this could occur because so many of the shared determinants of civic behavior and educational attainment are inherently difficult for researchers to measure well. For example, as noted earlier, children who were raised in families or communities that

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[5] The preference-shaping nature of schooling is typically viewed as normatively desirable. However, Lott (1990, 1999) argues that governments use the indoctrination that occurs in pubic schools to support totalitarian regimes and large wealth transfers.

[6] See Mueller (1989) for a discussion of the paradox of voting, models of voting behavior and issues related to the quality of the vote.

[7] See Nie et al. (1996, p. 3) for extensive references to this empirical literature. Educational attainment also appears to be strongly correlated with civic participation in other countries (e.g., Franklin, 1996).

stressed civic responsibility are almost certainly more likely to remain in school longer. This may occur in part because such families and communities are also likely to impart values that encourage schooling. However, it could also occur simply because civic-minded families and communities may do more to insure that their children attend well-funded, high-quality schools. These plausible scenarios imply that the strong association between adult civic outcomes and educational attainment may reflect, to an unknown degree, the confounding influence of unobserved family and community traits. Alternatively, these correlations could also reflect the confounding influence of other, inherently unobservable individual traits like the rate at which future outcomes are valued and the taste for altruism. Certainly, the recent trends in the United States (i.e., increases in educational attainment not matched by increases in voter turnout or political knowledge; Galston, 2001) suggest that the association between education and civic engagement could be specious. And at least two studies in the political science literature provide more formal evidence that such concerns about omitted variable biases may be empirically relevant. Both Luskin (1990) and Cassel and Lo (1997) present evidence that the apparent influence of education on civic outcomes (political literacy and sophistication) may reflect the spurious influence of other individual traits (e.g., intelligence and parents' socioeconomic status). Similarly, Gibson (2001) presents within-twin estimates, which suggest that education actually reduces the probability of volunteering. In the next two sections, I present new empirical evidence on the effects of educational attainment on several civic outcomes. I attempt to identify the causal effects of educational attainment by relying on instrumental variables that generate plausibly exogenous changes in the levels of individual schooling.[8]

3. College attendance and civic participation

3.1. High school and beyond (HS&B)

The data for this section are drawn from High School and Beyond (HS&B), a major longitudinal study conducted by the U.S. Department of Education. This detailed study began with a cohort of high school sophomores in 1980. Follow-up interviews of roughly 12,000 members of the sophomore cohort occurred in 1984 when most respondents were 20 years old and again in 1992 when most respondents were 28 years old.[9] In the 1992 interview, respondents were asked four civic-related questions: whether they were currently registered to vote (mean = 0.67), whether they had voted in a local, state or national election within the past year (mean = 0.36), whether they had voted in the 1988 Presidential election (mean = 0.55) and whether they had volunteered in the last month (mean = 0.37). There is evidence that survey respondents, particularly those with more education,

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[8] A recent study by Milligan et al. (2003) applies a similar methodology to different data sets and finds results similar to those presented here.

[9] See the data appendix in Dee (2003b) for further information on the study and the extract used here.

sometimes overstate their civic participation (e.g., Silver et al., 1986; Bernstein et al., 2002). However, this does not appear to be a problem with respect to the HS&B data.[10] The key measure of educational attainment examined here is college entrance defined as of the 1984 interview (mean = 0.54). This definition of college entrance is based on attendance at a junior college, a community college or a 4-year college or university and explicitly excludes those who only attended a vocational, trade, business or other training school. While this is a somewhat narrow margin of educational attainment, the available evidence indicates that it is also an increasingly important one. The rate of college enrollment among young adults has increased dramatically over the last 20 years with roughly half of this increase being absorbed by junior and community colleges (Kane and Rouse, 1999). And prior studies suggest that modest persistence at 2 and 4-year colleges has beneficial labor-market consequences even when it does not result in a degree (e.g., Kane and Rouse, 1995). The HS&B respondents who had entered college by 1984 did generally remain in college long enough to accumulate a relatively large amount of undergraduate credits.[11] Furthermore, the baseline evidence discussed below demonstrates that this measure of college entrance has a strong partial correlation with the probability of subsequent civic engagement. However, the choice of college entrance as a measure of educational attainment is also dictated by the availability of a plausible instrument, the geographic availability of junior and community colleges as a teen, which appears to have substantively influenced the decision to attend college and to have been otherwise unrelated to civic engagement as an adult.[12]

3.2. Baseline estimates

The validity of the geographic availability of junior and community colleges (hereafter referred to as 2-year colleges) as a basis for identification is a critical issue and is discussed in some detail below. However, before turning to an assessment of the relevant instrumental variables, it is useful to establish an empirical baseline by estimating the effects of college entrance on subsequent civic

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[10] Specifically, these means are generally comparable to those from contemporaneous Current Population Surveys (CPS). And, more important, the education gradients for these data are similar or smaller than those based on CPS and actual voter turnout data (see Dee, 2003b for details). It should also be noted that the potential existence of reporting bias that is increasing in education would imply that schooling actually had some of its intended civic consequences.

[11] Specifically, data from the HS&B transcript study indicate that those who had entered college by 1984 had, on average, about 80 more semester hours of undergraduate credit than those who did not: the equivalent of roughly five full-time college semesters. However, a caveat is appropriate since transcript data are missing or incomplete for roughly 25% of the respondents in this extract.

[12] Similarly, I chose to define college entrance as of the 1984 interview when most respondents were 20 years old and not as of later interviews. The estimated effects of college entrance are similar regardless of which interview is used to define it. However, college entrance defined as of later interviews had a plausibly weaker relationship to one of the early measures of college availability (i.e., the distance in miles from the high school to the nearest 2-year college).

Table 1
Estimated marginal effects of college entrance on adult civic behaviors, HS&B

Dependent variable	Single-equation probit				OLS	Sample size
	(1)	(2)	(3)	(4)	(5)	
Registered to vote	0.119	0.119	0.122	0.119	0.111	11,366
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	
Voted in last 12 months	0.092	0.092	0.095	0.093	0.080	11,429
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	
Voted in 1988	0.157	0.156	0.160	0.158	0.142	11,370
Presidential election	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	
Volunteered in last	0.055	0.057	0.056	0.056	0.055	11,484
12 months	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	
School-level controls	no	yes	yes	yes	no	
State/county-level controls	no	no	yes	yes	no	
Census division dummies	no	no	no	yes	no	
School fixed effects	no	no	no	no	yes	

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5) and the base-year composite test score. Standard errors, adjusted for clustering at the school level, are reported in parentheses. All the point estimates are statistically significant at the 1-percent level.

behaviors in specifications that assume the absence of omitted variable biases. Table 1 presents the estimated marginal effects from single-equation probits in which the four measures of civic behavior are the dependent variables. The first specification (column (1)) conditions on 10 variables representing basic demographic information on age, race, ethnicity, gender and religious affiliation, 18 other variables that reflect family income, family composition and parental education as defined during the 1980 interview and a single variable reflecting each respondent's 1980 composite score on reading, mathematics and vocabulary tests.[13] The subsequent models introduce school-level controls (i.e., miles to the nearest 4-year college and urban-icity fixed effects), state and county-level controls based on the location of the base-year school, fixed effects for the Census division of the base-year school and, finally, fixed effects for each of the 961 base-year schools. One of the county-level variables is a well-measured proxy for the civic attitudes of the community in which the respondents grew up: the county-level voter turnout in the 1980 Presidential election. The second county-level variable is a measure of adult educational attainment in the respondent's teen community: the percent of adults aged 25 or older with high school degree. The third county-level control, the population share aged 18–24, may be a relevant determinant of civic engagement and also influence the competitiveness of post-secondary institutions. The two state-level variables reflect influential voter regulations defined as of 1992 (Knack, 1995). One is a binary indicator for whether the state had an active policy of allowing voter registration by mail. The second is the number of years the state had active

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[13] See the appendix in Dee (2003b) for details on these controls. The Huber–White standard errors are adjusted for clustering at the school level.

"motor-voter" regulations in place.[14] The available evidence suggests that a years-based measure is the appropriate variable for identifying the early effects of "motor-voter" policies because state drivers licenses are renewed in cycles as long as 6 years (Knack, 1995).

These models are somewhat unusual in comparison to the prior literature since they condition on detailed individual and community-level socioeconomic variables defined as of each respondent's teen years. Furthermore, HS&B's clustered sampling design also makes it possible to control for the possibly confounding influence of unobserved community traits through the introduction of school fixed effects. The key results from these evaluations, which are presented in Table 1, uniformly suggest that college entrance had positive and statistically significant effects on civic participation. Interestingly, the magnitudes of these estimated marginal effects are also quite robust to the introduction of the additional controls, including school fixed effects. These estimated effects are also quite large, suggesting that college attendance has a sizable influence on subsequent civic participation. For example, these estimates imply that college entrance increased voter registration by approximately 12 percentage points, an increase of nearly 18% in the mean probability of being registered. Similarly, these estimates imply that college entrance increases the mean probability of voting in the last year, voting in the 1988 Presidential election and volunteering by 26%, 28% and 15%, respectively.

However, the central concern with the results in Table 1 is that the strong partial correlations between college entrance and civic behaviors may reflect the confounding influence of unobserved determinants of both schooling and civic engagement. One straightforward way to assess the possible empirical relevance of this concern is to examine the partial correlations between college entrance and measures of civic attitudes and knowledge that preceded attendance in college. I rely on two such measures based on data from the sophomore-year survey. One is a standardized test score on questions related to civics. The other is the student's response to a question about the importance of correcting social and economic inequality (1 = not important, 2 = somewhat important and 3 = very important). Each of these variables is highly predictive of each measure of future civic engagement. For example, a 10% increase in the sophomore-year civics test score is associated with a statistically significant 7% increase in the mean probability of voting within the last year. Similarly, a one-unit increase in the ordered attitudinal measure is associated with a statistically significant 10% increase in the mean probability of voting. In auxiliary regressions where these sophomore-year measures are the dependent variables, the estimated effects of college entrance are positive and statistically significant. However, since the dependent variables in these models *preceded* college entrance, these results cannot plausibly reflect causal effects. Instead, these results suggest the existence of individual-level unobservables that may have a positive covariance with both educational attainment

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[14] "Motor-voter" regulations bundle an application for voter registration with those for driver licenses. All states were required to institute "motor-voter" policies by 1995 as part of the National Voter Registration Act. It should also be noted that North Dakota does not have voter registration. The results reported here are robust to excluding observations from respondents who attended high school in that state.

and adult civic engagement. This stylized evidence underscores the need to rely on instrumental variables in estimating the effects of college attendance on civic outcomes.

3.3. Measures of college availability as instruments

The partial correlations reported in Table 1 are consistent with the prior empirical studies of civic participation. However, a more convincing strategy for assessing whether the estimates in Table 1 reflect the causal effects of attending college is to exploit instrumental variables that generate plausibly exogenous variation in this measure of educational attainment. The fundamental requirements of such instrument are that they actually influence educational attainment and that they are uncorrelated with the unob-served determinants of civic engagement. A recent study of the labor-market returns to schooling by Card (1995) suggests that the geographic availability of colleges may provide valid instruments for schooling.[15] The basic motivation for such instruments is that the proximity of colleges as a teen should substantially reduce the costs of attending college (particularly for students from disadvantaged backgrounds) but should otherwise have no effects on adult outcomes. Rouse (1995) also presents evidence that the availability of 2-year colleges increases educational attainment for those on the margin of attending college (a "democratization" effect) but actually reduces it among those who would have otherwise attended a 4-year college (a "diversion" effect). I also find some support for a modest "diversion" effect (i.e., the proximity of 2-year colleges reducing the probability of completing a bachelor's degree) but rely on the stronger "democratization" effect as a source of identifying information.

Specifically, I rely on two measures of the local availability of 2-year colleges. One is the distance in miles from each respondent's high school to the nearest 2-year college (as reported by a high-school official as part of the HS&B school survey). The second is a count of the number of 2-year colleges within each respondent's county in 1983 (mean = 2.4).[16] These measures of the availability of 2-year colleges are clearly related but they also appear to have had plausibly distinct effects on educational attainment.[17] For example, inferences based on these data suggest that the proximity of base-year high schools to a 2-year college increased college attendance in the late teens and early twenties had no effect on later spells of college attendance and, overall, may have diverted students away from eventually completing a bachelor's degree. In contrast, the number of 2-year colleges within a county appears to have generated more sustained spells of college attendance throughout young adulthood and to have increased the probability of ultimately completing a bachelor's degree.

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[15] See Kling (2001) and Currie and Moretti (2002) for further discussions and applications of this approach.

[16] These counts were created using the 1983–1984 data from the Higher Education General Information Survey (HEGIS). See the appendix in Dee (2003b) for details. Card (1995) and Kling (2001) rely similarly on a binary indicator for any college in county. I also constructed counts of 4-year colleges by county but found that this was highly collinear with the number of 2-year colleges and exclude it from this analysis. So, a caveat about attributing the effects associated with this measure to 2-year, not 4-year, institutions is appropriate.

[17] These measures are not highly collinear and have a relatively low correlation coefficient of − 0.2.

Since the identification strategy implemented here exploits the cross-sectional variation in the availability of 2-year colleges, the key sources of this variation should be noted. While every state has 2-year colleges, their geographic distribution across the United States is somewhat uneven. For example, several states in the West and Southwest (e.g., California, Washington, Texas and Arizona) and in the upper Midwest (e.g., Illinois, Michigan) have relatively extensive systems of public community colleges. The sources of this variation appear to be independent of the outcomes under study here. Specifically, Medsker and Tillery (1971) note that this distribution largely reflects the dramatic growth in new 2-year colleges that occurred in the middle of the last century (i.e., in the decades prior to the HS&B study). They also state that this growth was shaped by the interaction of state-specific enabling legislation and several sources of enrollment pressure (e.g., the G.I. Bill, the baby boom and population migration). However, it should be noted that the states of New York, Pennsylvania, Ohio and Florida also have a large number of 2-year colleges, with a particularly large share of them being older, private junior colleges.

I also considered, but rejected, the idea of using proximity to 4-year colleges as an instrument. Specifically, a central concern with any instrument based on the geographic availability of colleges is that it might be flawed because it is associated with the unobserved determinants of both educational attainment and civic behavior. In particular, the unobserved traits of communities near colleges (e.g., high socioeconomic status) could simultaneously encourage both higher educational attainment and increased civic partic-ipation. Furthermore, the availability of colleges may promote a youth-oriented and politically aware culture that promotes the civic engagement of teens independently of its effects on educational attainment.[18] I assess the empirical relevance of these concerns by providing three types of ad hoc empirical evidence on the validity of these instruments. First, I assess how the effects of these instruments vary across students from advantaged and disadvantaged backgrounds. To the extent that the estimated effects of these instru-ments only reflect variation in the costs of attending college, these effects should be concentrated among students from disadvantaged backgrounds (Card, 1995; Kling, 2001). Second, I examine their effects on different levels of educational attainment and base-year test scores. If the estimated effects of college availability truly reflect the costs of attending college and not the influence of omitted variables, these instruments are likely to have little or no effects on these other measures of educational achievement.[19] And, third, I examine the partial correlations between the instruments and sophomore-year measures of civic attitudes and knowledge that are strongly correlated with future civic participation (i.e., scores on a civics test and attitudes towards correcting inequality). The results of all of these ad hoc specification checks suggest that the proximity to 4-year colleges may be an

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[18] Another potential complication is that college availability may influence adult civic participation by raising the educational attainment of community peers. Fortunately, these sorts of spillover effects (e.g., Lochner and Moretti, 2001) do not appear to be empirically relevant. Specifically, I aggregated the HS&B data to the county-level and evaluated the reduced-form effects of the instruments on civic participation. The results were similar to those based on the individual data, which suggests that the spillover effects are empirically negligible.

[19] However, a signaling model of education suggests that there could be effects on other levels of attainment (e.g., Lang and Kropp, 1986). Also, as noted earlier, these instruments could influence higher levels of attainment through diversion effects (Rouse, 1995).

invalid instrument. In particular, nearness to 4-year colleges is associated with sharp increases in the probability of graduating from high school as well as significant increases in sophomore-year civics knowledge. These results do not constitute a definitive case against this particular measure as an instrument for educational attainment. Nonetheless, all of the models for educational attainment and civic outcomes reported here condition on this measure.[20]

In Table 2, I present the key results of these evaluations with respect to the preferred instrumental variables. The results in the top right panel indicate that both measures of 2-year college availability have plausibly signed and statistically significant effects on college entrance. Specifically, these indicate that a location 100 miles further away from a 2-year college reduces the probability of college entrance by 7.3 percentage points. Similarly, these results suggest that an additional 2-year institution within county is associated with a 0.6 percentage point increase in the probability of entering college.[21] In the bottom panel of Table 2, I provide some ad hoc evidence on the validity of these instruments by estimating their unique effects on students from advantaged and disadvantaged backgrounds. Card (1995) suggests that, if the interpretation of college availability as an independent measure of the costs of attending college is a valid one, the effects of these instruments should be concentrated among students from disadvantaged backgrounds. Following Card (1995), I assess the existence of such response heterogeneity by interacting the availability measures with indicators for high and low parental education.[22] The results indicate that the effects of the availability of 2-year colleges on college entrance are highly concentrated among students with poorly educated parents.

The results presented in Table 2 also indicate that these instruments are plausibly related to other measures of educational achievement. Specifically, these estimates indicate that the availability of 2-year colleges had small and statistically insignificant effects on base-year composite test scores, on the probability of graduating from high school and on the probability of obtaining an associate's degree. These results also suggest that the geographic proximity of 2-year colleges led to relatively small and weakly significant reductions in the probability of obtaining a bachelor's degree: a "diversion" effect that appears to be concentrated among students from disadvantaged backgrounds. However, the results in Table 2 also suggest that, particularly for students with poorly educated parents, the number of 2-year colleges within the county had strong "democratization" effects that increased the probability of entering

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[20] Not surprisingly, models that use this measure as an IV return estimates somewhat larger than those reported here.

[21] These models condition on all the individual, family, school, county and state-level controls and division fixed effects. Specifications that incrementally introduce these controls generate similar results (Dee, 2003b). Since recent studies (Bound et al., 1995; Staiger and Stock, 1997) have illustrated the biases that might be generated by relying on relatively "weak" instruments, I also tested the joint significance of these two variables. The extremely low *p*-values associated with these tests suggest that those concerns are not relevant in this application (Dee, 2003b).

[22] As in Card (1995), low parental education implies that the highest educational attainment of the parents is high school dropout or missing. Students for whom parents' education is missing have lower levels of attainment than the students who report their parents are dropouts.

Table 2
Estimated marginal effects of college availability on base-year variables and educational attainment, HS&B

Independent variable	Base-year variables			Educational attainment			
	Civics test	Inequality attitude	Composite test	High school graduate	College entrant	Associate's degree	Bachelor's degree
Model (1)							
Miles to a 2-year college (÷100)	− 0.250 (0.417)	− 0.021 (0.029)	0.83 (2.0)	− 0.003 (0.012)	− 0.073[‡] (0.025)	0.018 (0.020)	0.032* (0.019)
Number of 2-year colleges in county	0.005 (0.023)	0.002 (0.002)	− 0.07 (0.10)	0.0004 (0.001)	0.006[‡] (0.002)	0.002 (0.002)	0.003[†] (0.0019)
Model (2)							
Low parental education × miles to a 2-year college (÷100)	− 0.046 (0.438)	− 0.045 (0.039)	1.09 (1.9)	0.004 (0.014)	− 0.133[‡] (0.035)	0.015 (0.031)	0.049* (0.025)
High parental education × miles to a 2-year college (÷100)	− 0.421 (0.562)	− 0.001 (0.041)	0.61 (2.5)	− 0.011 (0.014)	− 0.028 (0.034)	0.021 (0.023)	0.022 (0.024)
Low parental education × number of 2-year colleges in county	− 0.012 (0.032)	0.001 (0.003)	0.06 (0.11)	0.0004 (0.001)	0.008[‡] (0.002)	0.002 (0.002)	0.006[‡] (0.002)
High parental education × number of 2-year colleges in county	0.016 (0.027)	0.003 (0.002)	− 0.17 (0.13)	0.0004 (0.001)	0.005[†] (0.002)	0.002 (0.002)	0.002 (0.002)
Sample size	9751	10,336	11,489	11,475	11,489	11,343	11,343

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5), school-level controls (3), state/county-level controls (5) and Census division dummies (8). The first three models were estimated by OLS; the remaining results are based on single-equation probits and include base-year composite test scores as a control. Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10% level.
[†] Statistically significant at the 5% level.
[‡] Statistically significant at the 1% level.

college as well as the probability of obtaining a bachelor's degree.[23] But the more general and important result from these comparative models is that the effects associated with the availability of 2-year colleges are highly concentrated on the margin of attending college. This evidence is consistent with the maintained assumption that these measures reflect plausibly exogenous variation in the costs of college entrance and not other unobserved traits of these communities. In particular, because these instruments have such large and narrowly focused effects on this single margin

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[23] As noted earlier, these results for attaining a bachelor's degree are consistent since the proximity of a single institution may promote diversion while the availability of several could facilitate the ultimate progression to a bachelor's degree.

of educational attainment, it suggests that they do not proxy for the unobserved determinants of future civic engagement.

Table 2 contains additional evidence on the validity of these exclusion restrictions based on two sophomore-year variables that reflect each student's latent civic engagement and knowledge: a standardized test score on questions related to civics and an attitudinal question about the importance of correcting social and economic inequality (1 = not important, 2 = somewhat important and 3 = very important). These variables, which are highly predictive of future civic participation as an adult, provide a potentially plausible basis for evaluating the validity of the instruments. Specifically, if the measures of college availability have an association with the unobserved determinants of future civic engagement, we would expect them to be correlated with these observed measures as well. However, the regression results in Table 2 uniformly indicate that the availability of 2-year colleges, both generally and for students with poorly educated parents, has a small and statistically insignificant association with sophomore-year civics knowledge and with attitudes towards inequality.

3.4. Results

The results summarized in Table 2 are consistent with the maintained assumption that the geographic availability of 2-year colleges provides a potentially valid source of identification. The availability of 2-year colleges is associated with a significant increase in college attendance but smaller and statistically insignificant changes in base-year test scores and in other measures of educational attainment. These increases are plausibly concentrated among students with poorly educated parents. And these measures are unrelated to sophomore-year indicators of civic attitudes (e.g., civics knowledge and attitudes towards inequality). In Table 3, I present the key results from bivariate probits in which the adult civic behaviors are the dependent variable of interest and college entrance is an endogenous regressor (Wooldridge, 2002).[24] The excluded instruments are miles to the nearest 2-year college and the number of 2-year colleges within county. These results of these models suggest that college entrance has relatively small (but imprecisely estimated) effects on the probability of volunteering. However, the estimated effects of college entrance on each of the three measures of voter participation are uniformly large and positive. Specifically, these estimates indicate that college entrance increases voter participation by roughly 17 to 22 percentage points.

These results are clearly consistent with the conventional claims that educational attainment is a critical determinant of civic engagement. In fact, with respect to voter

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[24] The estimates from 2SLS models are similarly signed and statistically significant estimates but are substantially larger than the marginal effects and average treatment effects (ATE) based on these bivariate probits, particularly in models saturated with the additional controls. This raises the critical issue of whether identification in the bivariate probits relies on possibly unjustified assumptions about functional form, instead of the exclusion restrictions. The available evidence suggests that this is not the case. Specifically, I found that bivariate probits that do not include the instrumental variables lead to smaller and statistically insignificant effects. Altonji et al. (2002, appendix) discuss similar issues in the context of the literature on the effects of Catholic schooling.

Table 3
Estimated effects of college entrance on adult civic behaviors, bivariate probit, HS&B

Dependent variable	$\hat{\beta}$	Marginal effect	ATE	Sample size
Registered to vote	0.607[†] (0.263)	0.218	0.215	11,366
Voted in last 12 months	0.482[†] (0.196)	0.176	0.174	11,429
Voted in 1988 Presidential election	0.453* (0.256)	0.178	0.171	11,370
Volunteered in last 12 months	− 0.124 (0.213)	− 0.047	− 0.045	11,484

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5), base-year composite test score, school-level controls (3), state/county-level controls (5) and Census division dummies (8). The two instrumental variables are miles to a 2-year college and the number of 2-year colleges in county. Standard errors, adjusted for clustering at the school level, are reported in parentheses.

  * Statistically significant at the 10% level.
  [†] Statistically significant at the 5% level.

registration and having voted in the last 12 months, these estimated effects are noticeably larger than those based on partial correlations (Table 1). However, the sampling variation associated with these estimates clearly suggests that these differences should not be overemphasized. In particular, the hypothesis that the error terms for college entrance and each measure of civic participation are uncorrelated cannot be rejected in any of the models presented in Table 3. Nonetheless, it is also worth noting at least three reasons that the true effects of educational attainment on voter participation might exceed the estimates based on partial correlations (Table 1). First, as frequently noted in the literature on wages and schooling, this could reflect an attenuation bias driven by measurement error in reported schooling. Second, these estimates could indicate that the civic returns associated with college entrance are particularly large for the nonrandom subset of individuals whose post-secondary attainments were influenced by the instruments (e.g., those from disadvantaged backgrounds, Imbens and Angrist, 1994).[25] And, third, a downward bias in conventional estimates could also reflect the influence of unobserved ability on both schooling decisions and time allocated to civic endeavors.

However, a fourth possibility with very different implications is that the size of these estimates reflects undiagnosed violations of the maintained exclusion restrictions. One indication that this is not so is that the results from Table 3 are quite similar across models that incrementally introduce the school, county and state-level controls. However, another way to assess this concern is to use as instruments the interaction of low parental education and the measures of 2-year college availability. Specifically, in such models, the interaction of high parental education and the measures of 2-year college availability can then be included as controls in the outcome equations (e.g., Card, 1995). This approach to identification can provide effective controls for the possible, indirect effects on civic outcomes associated with

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  [25] The results from single-equation probits (i.e., as in Table 1) that allow the effect of college entrance to vary by parental education suggest that the effects of college entrance on voter registration and turnout are larger among those with poorly educated parents.

college availability to the extent that these effects are constant across students with different family backgrounds. The results based on this specification are similar to those reported in Table 3 (Dee, 2003b). The robustness of these results suggests that the basic identifying assumptions are accurate. I also assessed this issue by relying alternatively on miles to a 2-year college and number of 2-year colleges in county as the sole instrument and including the other variable as a control. This approach leads to similarly large and positive point estimates in models for voter registration and turnout. However, in most cases, the reduction in identifying assumptions makes these estimates statistically imprecise.

4. Secondary schooling and civic outcomes

4.1. General social surveys (GSS)

The evidence from the HS&B data has at least two critical shortcomings. One is that it only identifies the civic returns to education at the post-secondary level. And the second is that the available data provide no measures of the degree of civic awareness or of other fundamental civic values. The data from the General Social Surveys (GSS) provide an opportunity to address both of these concerns. The GSS is a nationwide survey, conducted every 1 to 2 years, on a broad range of attitudes and behavior.[26] My extract is based on the pooled 1972–2000 surveys and consists of the respondents who lived in the US at age 16 and were 14 years old between 1914 and 1978. In each survey, these respondents were asked about their educational attainment and whether they voted in the last Presidential election. On average, 73% of the GSS respondents claimed to have voted in the most recent Presidential election.[27]

In most, but not all, survey years, GSS respondents were also asked about how often they read the newspaper, about their group memberships (e.g., fraternal and community-service groups, political clubs, school-service and youth groups, church-service groups, etc.) and about their attitudes towards free speech for particular groups. The GSS respondents report an average of 1.8 group memberships. The frequency of newspaper readership is based on five possible responses (never, less than once a week, once a week, a few times a week and every day) coded here as varying from 0 to 4 (mean = 3.2). This measure of newspaper readership is meant to indicate whether voters stay informed about current affairs. There are inarguably better ways of measuring the degree of civic awareness. For example, in 1987, the GSS respondents were asked to identify their congressional representative. Interestingly, only 37% of respondents were able to answer this question correctly. Unfortunately, since this question was only asked in 1987, there are relatively few observations ($n = 1555$) and a plausible identification strategy cannot be implemented. However, the data from

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[26] See the appendix in Dee (2003b) for details on the GSS and construction of this extract.

[27] There does not appear to be a propensity for better-educated GSS respondents to overstate their voter turnout. In particular, the education gradients based on CPS data and on county-level data reflecting actual voter turnout are similar or larger than those reported in Table 4 (Dee, 2003b).

1987 do indicate that the frequency of newspaper readership is strongly associated with being able to identify your congressman. Specifically, conditional on all the covariates discussed below, a one-unit increase in the measure of newspaper readership is associated with a 10 percentage-point increase in the probability of answering correctly (i.e., a 27% increase in the mean). This suggests that the frequency of newspaper readership is a reasonable proxy for the degree of civic awareness. The measures of attitudes towards free speech are based on separate survey questions that allowed respondents to indicate whether they would allow particular types of people to speak in their community. These types include someone against churches and religion (an anti-religionist), an admitted Communist, an admitted homosexual, someone who advocates outlawing elections and letting the military run the country (a militarist) and someone who believes blacks are inferior (a racist). Support for allowing free speech ranges from 59% for the militarist to 73% for the homosexual (see Dee, 2003b).

4.2. Baseline estimates

In Table 4, I present baseline OLS estimates of how years of completed schooling influences these measures of civic engagement and attitudes. The sparsest specification only includes as controls basic demographic information (9 variables) and fixed effects for survey year (as many as 22 variables), year of birth (64 variables) and Census division of residence at age 16 (8 variables). The second specification adds three control variables that reflect the quality of public schools and the degree of civic engagement in each respondent's teen community. The two school quality measures are the pupil–teacher ratios and relative teacher salaries in public schools at age 14 in the Census division of residence at age 16.[28] The third variable is the voter turnout in the Presidential election that occurred between the ages of 13 and 16 in the Census division of residence at age 16. The third specification introduces variables based on survey responses that reflect a variety of family and community-specific traits. These include family income at age 16 (five variables), family structure at age 16 (five variables), parental education (four variables) and the urbanicity of residence at age 16 (six variables). In the final model, I control for all the unobserved determinants that might be specific to a particular Census division in a particular year (e.g., weather, close political races, etc.) by including approximately 200 fixed effects for each unique Census-division and survey-year combination. The standard errors are adjusted for unspecified heteroscedasticity specific to the Census division of resident at age 16.[29] The OLS results in Table 4 uniformly indicate that schooling is strongly and positively correlated

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[28] Card and Krueger (1992) present evidence that these measures influenced average years of schooling. I converted average teacher salaries to a relative measure by exploiting data on wages paid to road workers on Federal projects (Card and Krueger, 1992) and data on wages for production workers in manufacturing. See the appendix in Dee (2003b) for information on the construction of these variables.

[29] This conservative approach may be appropriate since the pre/post nature of the instrumental variable and serial correlation in the dependent variables could lead to overstated precision (Bertrand et al., 2002). As a practical matter, this only appears to increase the 2SLS standard errors slightly. However, this approach is also a conservative one because the existence of only nine Census divisions implies there are only 8 degrees of freedom in the critical value of the t-statistics.

Table 4
OLS and 2SLS estimates of the effect of highest grade completed on civic behaviors and attitudes, 1972–2000 GSS

Dependent variable	(1)		(2)		(3)		(4)		Sample size
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	
Voted in last Presidential election	0.043‡ (0.002)	0.037* (0.019)	0.038‡ (0.002)	0.064 (0.036)	0.038‡ (0.002)	0.069* (0.036)	0.038‡ (0.002)	0.068* (0.035)	32,111
Newspaper readership	0.112‡ (0.013)	0.203‡ (0.021)	0.104‡ (0.014)	0.127* (0.066)	0.105‡ (0.014)	0.112* (0.056)	0.104‡ (0.014)	0.113* (0.056)	21,805
Group memberships	0.239‡ (0.009)	0.138† (0.047)	0.221‡ (0.011)	0.144 (0.108)	0.222‡ (0.011)	0.157 (0.141)	0.222‡ (0.011)	0.164 (0.184)	16,361
Allow anti-religionist to speak	0.036‡ (0.001)	0.092‡ (0.032)	0.030‡ (0.001)	0.137‡ (0.028)	0.030‡ (0.001)	0.126‡ (0.024)	0.029‡ (0.001)	0.125‡ (0.021)	22,449
Allow communist to speak	0.043‡ (0.002)	0.060† (0.026)	0.036‡ (0.002)	0.085‡ (0.019)	0.036‡ (0.002)	0.084‡ (0.020)	0.036‡ (0.002)	0.080‡ (0.021)	22,111
Allow homosexual to speak	0.035‡ (0.002)	0.092‡ (0.018)	0.029‡ (0.001)	0.138‡ (0.026)	0.029‡ (0.001)	0.126‡ (0.021)	0.029‡ (0.001)	0.123‡ (0.028)	20,678
Allow militarist to speak	0.037‡ (0.002)	0.005 (0.027)	0.031‡ (0.002)	0.054† (0.023)	0.030‡ (0.002)	0.053* (0.025)	0.030‡ (0.002)	0.036 (0.031)	18,514
Allow racist to speak	0.027‡ (0.002)	0.040 (0.031)	0.023‡ (0.002)	0.022 (0.032)	0.022‡ (0.002)	0.020 (0.031)	0.022‡ (0.002)	− 0.002 (0.032)	18,488
Teen-division/ cohort controls	no		yes		yes		yes		
Family/ community controls	no		no		yes		yes		
Current-division-by-survey-year dummies	no		no		no		yes		

All models include age, age squared and binary indicators for gender (1), race (2) and religious preference (4) and fixed effects for survey year, year of birth and Census division of residence at age 16. Standard errors, adjusted for clustering at the division level, are reported in parentheses.
  * Statistically significant at the 10% level.
  † Statistically significant at the 5% level.
  ‡ Statistically significant at the 1% level.

with all of these measures of civic engagement and attitudes. For example, these estimates suggest that an additional year of schooling increases voter participation by 3.8 percentage points, an increase of approximately 5%. These results also imply that another year of schooling significantly increases the index of newspaper readership (by 0.104, an increase of 3%) and the number of group memberships (by 0.222, an increase of 12%). Another year of schooling also appears to increase support for free speech by a statistically significant 2.2 to 3.6 percentage points, depending on who is doing the speaking. Interestingly, these estimated effects are generally quite robust to dramatic increases in the set of controls for observed traits.

4.3. Restrictive child labor laws as an instrument

The OLS estimates in Table 4 suggest that additional years of schooling led to significant increases in the quality and quantity of civic engagement and in the support for free speech. I attempt to assess whether these estimates reflect a causal relationship by exploiting the exogenous variation in years of schooling generated by teen exposure to restrictive child labor laws. Recent studies by Acemoglu and Angrist (2000) and Lleras-Muney (2002) provide evidence that the variation in child labor laws influenced the amount of schooling at the secondary level. The coding of the data on child labor laws used here is discussed in detail in Acemoglu and Angrist (2000). Essentially, for each state and year from 1914 to 1978, they identified the minimum amount of schooling required before a child could enter the workforce (the variable CL). This variable is equal to the greater of the years of schooling a state required before granting a work permit and the difference between the age at which children could work and the age at which they had to enter school. These laws are represented here by a dummy variable equal to one for CL greater than or equal to 9.[30] These state-year laws could not be matched directly to GSS respondents because the available data only identifies which of nine Census divisions they resided in at age 16. Therefore, I calculated division-by-year means of these state-year dummies using state-year population estimates as weights. I then matched each GSS respondent to these fractional variables representing restrictive child-labor laws that were in effect at age 14 in their reported division of residence at age 16. This unusual division-by-year aggregation undoubtedly introduces some measurement error into the instruments. However, it is not clear that the implied measurement error is any less than that in other studies, which rely on matches by state of birth, not state of teen residence. Furthermore, an auxiliary regression indicates that this division-year measure of restrictive child labor laws captures a substantial proportion of within-state variation in these laws. Specifically, I regressed a state-year indicator for a CL of 9 or higher on the division-year measure for a CL of 9 or higher, state fixed effects and year fixed effects. The coefficient on the division-year measure was 0.72 with a *t*-statistic of 26.

In Table 5, I present the estimated effects of restrictive child-labor laws on different levels of educational attainment based on specifications that include the full set of controls (i.e., as in Model (4) in Table 4). The results indicate that restrictive child-labor laws increased years of schooling by a statistically significant 0.53 years. This point estimate is somewhat larger than those reported by Acemoglu and Angrist (2000). Specifically, they found (Table 4, p. 30) that a CL of 9 or higher increased years of schooling by 0.4 years among 40–49-year-old white males from the 1950–1990 Censuses. However, these differences are small relative to the sampling variation. Furthermore, these modest differences also appear to reflect the unique composition of the sample analyzed by Acemoglu and Angrist (2000). The first-stage effects of CL9 are smaller (and more imprecise) when the GSS sample is similarly limited to prime-age, white males.

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[30] In some models based on PUMS data, Acemoglu and Angrist (2000) find that CL of 7 and 8 had smaller but statistically significant effects on years of schooling. Estimates based on the GSS also suggest that CL of 7 and 8 had positive but smaller effects. However, since the GSS has relatively few observations, these effects are always estimated imprecisely and do not provide a plausible source of identifying information.

Table 5
OLS estimates of the effects of restrictive child-labor laws on measures of educational attainment, 1972–2000 GSS

Dependent variable	$\hat{\beta}$
Highest grade completed	0.53[‡] (0.14)
Completed 9th grade or higher	0.12[‡] (0.02)
Completed 10th grade or higher	0.10[‡] (0.01)
Completed 11th grade or higher	0.10[‡] (0.02)
Completed 12th grade or higher	0.10[‡] (0.01)
High school graduate	0.08[‡] (0.02)
Completed at least 1 year of college	− 0.04 (0.03)
Associate's degree	− 0.03 (0.02)
Bachelor's degree	− 0.03 (0.02)

The sample size is 32,111. All models include age, age squared, binary indicators for gender (1), race (2), religious preference (4), family income at age 16 (5), parental education (4) and family composition at age 16 (5), urbanicity of residence at age 16 (6), pupil–teacher ratio at age 14, relative teacher salaries at age 14, voter turnout as a teen and fixed effects for year of birth, Census division of residence at age 16 and current division-of-residence by survey-year dummies. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

[‡] Statistically significant at the 1% level.

The quality of the measure of restrictive child-labor laws as an instrument hinges critically on the maintained assumption that these estimates accurately reflect its independent effects on educational attainment. The evidence from prior studies is generally consistent with this view. For example, Lleras-Muney (2002) presents a variety of ad hoc empirical evidence on changes in child-labor laws and concludes that they were not endogenously determined. Furthermore, Goldin (2001) argues that such laws played a relatively minor role in the dramatic "high school movement" from 1910 to 1940, which suggests that these law changes were not part of substantive social changes that might have also influenced civic attitudes. The robustness of the first-stage estimates to the introduction of additional controls also provides some supporting evidence (Dee, 2003b).

The remaining results in Table 5 also provide indirect evidence on the validity of these instruments. More specifically, if these models effectively identify the influence of stricter child-labor laws on educational attainment, we should find that these estimated effects are largely concentrated at the lower end of the distribution of educational attainment (Acemoglu and Angrist, 2000; Lleras-Muney, 2002). However, we should be especially concerned about the existence of undiagnosed specification errors if similarly specified models indicate that these laws had relatively large effects on higher levels of educational attainment. The results in Table 5 indicate that the effects associated with stricter child-labor laws were largely concentrated at the secondary level. More specifically, these estimates indicate that the strictest child-labor laws led to large and statistically significant increases in the probability of completing 9, 10, 11 and 12 years of schooling and the probability of high school graduation. However, the same specifications indicate that these law changes had smaller and statistically insignificant effects on several measures of post-secondary educational

attainment. Another similarly ad hoc way to assess the validity of this identification strategy is to note that the variation in child-labor laws should be particularly relevant for respondents who had relatively disadvantaged backgrounds. I examined this possibility by estimating how the effects of stricter child-labor laws varied across respondents with low and high levels of parental education.[31] The results indicated that the increases in educational attainment associated with more restrictive child-labor laws were larger among those who had more poorly educated parents. Specifically, using the voting sample, the estimated effect of these laws was 0.75 among those with poorly educated parents and 0.36 among the remaining respondents. However, it should be noted that this difference, though suggestively plausible, is not statistically meaningful.

4.4. 2SLS results

The results in Table 5 suggest that restrictive child labor laws provide a valid source of information for identifying the effects of years of schooling on adult civic behaviors. In Table 4, I present 2SLS estimates based on this instrument in models for each measure of civic engagement.[32] The results indicate that schooling has uniformly positive and statistically significant effects on most measures of civic engagement and attitudes. For example, these 2SLS estimates suggest that an additional year of schooling increased voter participation by a weakly significant 6.8 percentage points (*t*-statistic = 1.93). While this estimate is nearly twice as large as the OLS estimate, this difference is not particularly large relative to the sampling variation. More formally, a Hausman test fails to reject the hypothesis that the corresponding OLS estimate is consistent. Interestingly, the effect sizes from the voting models are quite similar to those based on post-secondary attainment and the HS&B data. Specifically, the college entrants in HS&B had roughly 2.5 more years of schooling than nonentrants and the results in Table 3 suggest that this additional schooling increased voter turnout by 16 to 17 percentage points. The 2SLS results in Table 4 suggest that 2.5 years of secondary schooling would also increase voter turnout by roughly 17 percentage points (2.5×0.068).

The 2SLS estimates in Table 4 also suggest that schooling increases the quality of civic engagement and knowledge. More specifically, the 2SLS estimates imply that an additional year of schooling generates a weakly significant increase (*t*-statistic = 2.02) in the frequency of newspaper readership that is roughly equivalent to that implied by the OLS estimate. The estimated effect of schooling on group memberships is also positive but highly imprecise. However, these estimates also imply that that schooling significantly increased support for free speech by anti-religionists, communists and homosexuals. These estimated effects (8.0 to 12.5 percentage points) are several times larger than those implied by the corresponding OLS estimates.[33] In contrast, the estimated

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[31] Low levels of parental education implied that the highest attainment among the parents was less than a high school degree or missing.

[32] I also experimented with specifications that recognized the categorical nature of the dependent variable and the potential endogeneity of schooling (Wooldridge, 2002) and found that they generated similar results.

[33] Hausman tests indicate that the consistency of the OLS estimates can be rejected with regard to free speech for anti-religionists and homosexuals but not with respect to free speech for communists.

effects of schooling on support for speech by militarists and racists are smaller and statistically imprecise. One concern that is suggested by the relative size of some of the 2SLS estimates is that they are due to the biases from "contagion" effects (e.g., Lochner and Moretti, 2001). This refers to the possibility that these 2SLS estimates reflect how restrictive child labor laws influenced both an individual's schooling as well as that of his peers. One simple way to assess the empirical relevance of this issue is to replicate the 2SLS models after aggregating the data to cells defined by the interaction of birth year and division of residence at age 16 ($n = 585$). The 2SLS estimates based on these aggregate data reflect both individual and contagion effects. I found that the estimated effects of schooling were quite similar to those in Table 4, suggesting that any contagion effects were negligible.[34]

5. Conclusions

In this study, I presented an empirical analysis of one of the fundamental relationships that motivates public policies towards education: the effects of schooling on civic participation and attitudes. In particular, I assessed whether increases in educational attainment have causal effects on civic outcomes by exploiting possibly exogenous sources of variation in schooling that should otherwise be unrelated to civic outcomes in adulthood (i.e., the geographic availability of 2-year colleges as a teen and exposure to child labor laws as a teen). The results suggested that educational attainment, both at the post-secondary and the secondary levels, has large and independent effects on most measures of civic engagement and attitudes. The apparent existence of these civic returns implies that much of the long-lived hyperbole about the important role of education in a functioning democracy may be accurate. However, it should be noted that a great deal of the discussion surrounding the role of education in a democracy has also confused the existence of these externalities with the fundamental issues of how the government should intervene in the market for education (e.g., price subsidies, regulation of the private sector, public production). In particular, the existence of large civic returns to education is not necessarily relevant to the difficult question of whether government should be involved in directly producing education (i.e., the "choice of instrument" problem, Poterba, 1996). In fact, there is some suggestive evidence that most private schools are actually more effective than public schools in promoting civic engagement (e.g., Dee, 2003a; Campbell, 2001). Nonetheless, the results presented here clearly underscore the dramatic relevance of schooling to the critical functions of a democratic society and imply that initiatives to promote educational attainment merit the continued and careful scrutiny of researchers and policymakers.

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[34] This is not entirely surprising since over 20% of GSS respondents were not even living in the Census division in which they resided at age 16.

Acknowledgements

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