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The Effect of Education on Voter Turnout

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Numerous studies demonstrate that the highly educated are more likely to vote. The literature has not determined, however, why this is the case. The panel structure of the Current Population Survey allows us to exploit exogenous life cycle differences across otherwise similar cohorts, so that the marginal effect of education can be isolated from unobserved correlated factors. We find that an additional year of schooling has very little impact on voter turnout.

1 Introduction

Analyzing the determinants of voter turnout has been the topic of an expansive literature. An empirical regularity found in numerous studies is that the highly educated are more likely to vote (e.g., Wolfinger and Rosenstone 1980; Nie, Junn, and Stehlik-Barry 1996). Despite being well established, the literature has not resolved a critical question regarding this finding. Does education actually raise voter turnout or is it spuriously correlated with voting?

Two recent studies have started to address this fundamental question. Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) rely on instrumental variables to separate the causal effect of education from unobserved correlated factors. They employ as instruments child labor and compulsory schooling laws and the availability of 2-year colleges in the geographic area where a given individual went to high school. Both sets of authors find that controlling for selection bias does not alter the conclusion of previous research. That is, education has a significant positive impact on voter turnout in the United States.[1]

The key identification assumption in these papers is that the employed instruments explain education choice but are not correlated with omitted factors that influence voter turnout. Although Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) undertake robustness checks that suggest their instruments are plausibly valid, it is quite possible they are not. Since the instruments employed vary only by age, year, and geographic

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[1] In contrast, Milligan, Moretti, and Oreopoulos (2004) find that education has little impact on voter turnout in the United Kingdom.

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location, it is impossible to include a full set of interactions between these variables and still identify the effect of education. The estimates of Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) would be biased if their instruments were correlated with omitted variables.

Given this potential problem, it is important to test whether the findings of Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) are robust to the use of an alternative identification strategy. We isolate the causal effect of education by exploiting the panel structure of the Current Population Survey (CPS). Individuals who will acquire a given level of education in the following year are compared to those who are 1 year older and currently have that particular level of education. The identification assumption relied upon is that these two cohorts are "similarly selected," by which we mean they have similar unobserved characteristics. By comparing the voting rates of these two groups, we difference out the impact of unobserved characteristics and thereby isolate the causal effect of education.

This methodology is used to test two explanations of why education is associated with higher voter turnout. According to the "civic education" theory, as an individual's educational development progresses, the ensuing increase in civic skills and knowledge leads to greater political involvement. By looking at marginal changes in education, we explore whether an individual is more likely to vote as he becomes better educated. An alternative explanation is that the relationship between education and voter turnout is spurious, resulting from selection bias in education choice. A finding that a marginal change in education does not affect the likelihood of voting would lend support to that explanation.

Our analysis shows that an additional year of education has very little influence on voter turnout. This finding is consistent with the selection bias explanation that unobserved factors influence both voter turnout and education choice. However, an alternative explanation for why education has little impact on voter turnout focuses on the short-run effect of educational attainment. Whereas Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) consider the impact of education for the general population, our methodology limits the analysis to those still acquiring their education. It is possible that the effect of education is delayed, not materializing until after an individual has finished school. To consider this possibility, we investigate whether the cross-sectional correlation between education and voter turnout exists for the young. Indeed, education appears to have a significant positive impact on voter turnout when selection bias is ignored. This suggests that a significant portion of the cross-sectional relationship between education and voter turnout is spurious, resulting from the endogeneity of education choice.

The layout of the paper is as follows. Section 2 reviews the literature. Section 3 provides an example that illustrates how we identify the causal effect of education. The model specification is presented in Section 4. Section 5 describes the data set employed. Results are provided in Section 6. Section 7 concludes.

2 Literature Review

The relationship between education and political participation has traditionally been viewed in terms of absolute educational attainment.[2] An individual's own education has a direct, positive impact on his political participation (Converse 1972). A widely held explanation for this effect is known as the civic education theory. Simply put, education

[2]Alternatively, Nie, Junn, and Stehlik-Barry (1996) argue that relative rather than absolute education determines political participation.

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develops the civic skills and knowledge that facilitate political engagement (Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995). As an individual becomes better educated, and therefore better prepared, he is more likely to be politically active.

An alternative explanation is that the relationship between education and voter turnout is spurious. Instead of a causal effect, selection bias in education choice explains why the highly educated are more likely to vote.[3] Suppose, for example, that an individual's family background jointly influences his propensity to vote and his educational attainment decision. Standard models that ignore selection bias would produce biased results since the estimated effect captures not only the direct impact of education but also correlated factors such as one's family background.

Two recent papers try to isolate education's causal effect using instrumental variables. Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) employ as instruments child labor and compulsory schooling laws and the availability of 2-year colleges in the geographic area where a given individual went to high school. These are potentially useful instruments since they influence the decision of whether to drop out of high school or attend college. However, they vary only by age, year, and geographic location. It is impossible to include a full set of interactions between these variables and still identify the effect of education. This is a critical shortcoming since a large body of research shows these factors are important determinants of voting that must be robustly controlled for (e.g., Leighley and Nagler 1992a; Miller 1992; Knack 1993, 1995; Miller and Shanks 1996; Oliver 1996). The estimated effect of education would be biased if the instruments employed were correlated with omitted variables. This potential problem highlights the need for exploring alternative estimation strategies. In the following section, we show how to isolate the causal effect of education by exploiting exogenous life cycle variation.

3 Identification

This section presents a simple example that provides the intuition for why selection in education choice leads to biased results. We then show how to isolate the causal effect of education from unobserved correlated factors. Our empirical strategy identifies the effect of education by comparing individuals who will have a given age and education in the following year to counterparts with the same characteristics in the current year. The key identification assumption is that these two groups have similar unobserved characteristics. Since they differ by being 1 year apart in their life cycle, this is equivalent to assuming no generational differences across those born in adjacent years.

To simplify the example, suppose the data is composed of four groups of individuals (Table 1).[4] We observe each individual i's current age a_{it}, education e_{it}, and voter turnout $v_{it} \in 0, 1$. For people in groups 3 and 4, we also know their age and education in the following year. This additional information is available due to the panel structure of the CPS (see Section 5). We constructed this example so that group 3's age and education in the following year are identical to group 1's current age and education. The same relationship holds for groups 4 and 2.

[3]The appendix of Tenn (2005) provides a formal treatment of this issue. See also Hillygus (2005) and the references cited therein.

[4]In our empirical analysis, the data sample contains a wider range of age and education levels. In addition, the model controls for whether each individual is currently a student.

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Table 1 Idenfification example

	Age, current year	*Education, current year*	*Age, next year*	*Education, next year*
Group 1	19	12	20	Unknown
Group 2	19	13	20	Unknown
Group 3	18	12	19	12
Group 4	18	12	19	13

One could estimate the following linear probability model if selection bias in education choice were not a concern.

$$v_{it} = \alpha_0 + \alpha_a a_{it} + \alpha_e e_{it} + \varepsilon_{it}, \tag{1}$$

where $\Pr(\varepsilon_{it} = 1 - \alpha_0 - \alpha_a a_{it} - \alpha_e e_{it} | a_{it}, e_{it}) = \alpha_0 + \alpha_a a_{it} + \alpha_e e_{it}$ and $\Pr(\varepsilon_{it} = -\alpha_0 - \alpha_a a_{it} - \alpha_e e_{it} | a_{it}, e_{it}) = 1 - \alpha_0 - \alpha_a a_{it} - \alpha_e e_{it}$.
Researchers generally control for additional characteristics X_{it} that influence voter turnout. Further, since v_{it} is a binary variable, a probit or logit framework is usually employed. Nonetheless, it is useful to consider this specification since it provides a particularly simple setting to analyze how the model parameters are identified.

Ordinary least squares estimation of equation (1) yields $\hat{\alpha}_e = \bar{v}^2 - \bar{v}^1$, where $\bar{v}^k$denotes the average voter turnout of group k. That is, the voter turnout difference between 19-year-olds with 12 years of education and 19-year-olds with 13 years of education is entirely attributed to the single year difference in educational attainment between the two groups. Of course, this might not be the case. For example, family background variables such as the education of one's parents might affect whether someone votes. Data limitations frequently prevent such variables from being controlled for, however. This is problematic since parental education (and other family background characteristics) is (are) correlated with an individual's educational attainment decision. The variable controlling for an individual's own education captures the effect of omitted correlated factors, leading to biased estimates of the direct impact of education.

Suppose each group k has unobserved characteristics that have influence δ_k on their propensity to vote. These factors are controlled for by including a set of group fixed effects.

$$v_{it} = \sum_{k=1}^{4} \delta_k 1_{\text{group}_i = k} + \alpha_a a_{it} + \alpha_e e_{it} + \varepsilon_{it}. \tag{2}$$

If equation (2) is the correct model, but one estimates equation (1), the probability limit of $\hat{\alpha}_e$ equals $\alpha_e + \delta_2 - \delta_1$. A consistent estimate of α_e is obtained only when groups 1 and 2 are similarly selected (i.e., $\delta_1 = \delta_2$).

It is not possible to control for selection bias by estimating equation (2); parameters α_a and α_e are not identified since a_{it} and e_{it} are perfectly collinear with the set of group fixed effects. An additional assumption is required to identify the impact of education when selection bias is robustly controlled for in this manner. We assume that individuals with a given age and education in the current year have identical unobservable characteristics as those with the same age and education in the following year. This is a reasonable assumption since generational differences between those born in adjacent years are unlikely to be significant.

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This assumption imposes two parameter restrictions: $\delta_1 = \delta_3$ and $\delta_2 = \delta_4$. All of the model parameters in equation (2) are identified once these restrictions are imposed. It is straightforward to show that ordinary least squares estimation of the regression model is equivalent to solving the following two equations: $\hat{\alpha}_a = \bar{v}^1 - \bar{v}^3$ and $\hat{\alpha}_a + \hat{\alpha}_e = \bar{v}^2 - \bar{v}^4$. Since groups 1 and 3 are similarly selected, voter turnout differences between them are due to the 1-year age difference. Similarly, groups 2 and 4 differ by 1 year of age and 1 year of education.

Our estimation approach requires a data sample composed of relatively young individuals, since they are primarily the ones still in school. The marginal effect of education for the young may differ from the impact of education for the general population. Since the methodology employed cannot estimate the effect of education for those who have already completed school, we are unable to directly test whether this is the case. However, we can compare the results of estimating equations (1) and (2) using the same data sample. In the absence of selection bias, both models provide consistent results, whereas only equation (2) does so when there is selection bias in education choice. Our empirical analysis shows that the two sets of results are very different, with the effect of education largely disappearing once we control for selection bias. This result lends indirect support to the contention that selection bias is responsible for the positive effect of education on voter turnout found in prior research based on nationally representative samples.

4 Methodology

The previous section presented a simple example that shows how we isolate the causal effect of education from unobserved correlated factors. The following model uses the same methodology, but with two changes: (1) we control for additional characteristics X_{it} that influence voter turnout and (2) a logit specification is employed rather than the linear probability model.

Our approach exploits the panel structure of the CPS. Half the data sample can potentially be matched to the following year's survey.[5] For such individuals, let $g_i = (a_{i,t+1}, e_{i,t+1}, s_{i,t+1})$ denote person i's age, education, and student status in the following year. For all other individuals, define $g_i = (a_{it}, e_{it}, s_{it})$, which is the value of these variables in the current year. Given additional characteristics X_{it}, we estimate the following logit specification.

$$\Pr(v_{it} = 1 | X_{it}, g_i, a_{it}, e_{it}, s_{it}) = \Lambda(X_{it}\beta + \sum_g \delta_g 1_{g_i = g} + \alpha_a a_{it} + \alpha_e e_{it} + \alpha_s s_{it}). \quad (3)$$

The control variables included in X_{it} are characteristics that previous research has shown to be important determinants of voting—race, gender, marital status, a dummy variable for having moved in the last 12 months, and a set of dummy variables for state of residence.

The second term in equation (3) is a set of dummy variables for each value of g, which captures the effect of selection bias in education choice. By defining g_i in two different ways, depending on whether an individual's educational attainment in the following year can be observed, we impose the identification assumption discussed in Section 3: those who will have a given age and education in the following year have identical unobserved characteristics as those with the same age and education in the current year.

[5]The other half of the data sample can potentially be matched to the previous year's survey.

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4.1 *Short- versus Long-Run Effect of Education*

The model specification differentiates between student status and an individual's educational attainment. The student status variable captures a short-run effect associated with being in an educational environment. For example, an individual who enters college just prior to an election would be only slightly more educated but may be more likely to vote since he is in an environment where people commonly engage in political discourse.[6] One would expect this effect to disappear once an individual leaves school. In contrast, according to the civic education theory, an individual's accumulated schooling has a permanent impact on political participation. Note that since most individuals are students only briefly during their adult lives, there is little connection between the effect of student status and aggregate voting rates. As such, the effect of accumulated schooling, rather than the impact of being a student, has been the primary focus of this literature. Controlling for both educational attainment and student status allows us to separate these two effects and makes our results directly comparable to previous research.

Since educational attainment is one's accumulated years of being a student, it is useful to consider how these two variables are separately identified. For the data sample used in the empirical analysis, which is described in the following section, the majority of variation in student status comes from individuals leaving school. The long-term effect of schooling is captured by the years of education control variable. In the data sample employed, most educational changes come in the middle of an individual's academic life and are therefore not associated with changes in student status. It is for this reason the effects of educational attainment and student status can be separately identified.

4.2 *Measuring Educational Attainment in the CPS*

One shortcoming of the data employed is that starting in 1992 the CPS reports only the highest degree obtained. Individuals with "some college" education have heterogeneous levels of schooling (i.e., 13, 14, or 15 years of school). This poses a problem in our analysis since the methodology employed compares those who will acquire a given level of education in the following year to those who are 1 year older and currently have that level of education. One cannot identify the marginal impact of a single year of education using an education measure that groups multiple years of schooling. We overcome this difficulty, however, by noting that equation (3) is equivalent to the following specification.

$$\begin{aligned}\Pr(v_{it} = 1|X_{it}, g_i, a_{it}, e_{it}, s_{it}) = \Lambda(X_{it}\beta + \sum_g \widetilde{\delta}_g 1_{g_i=g} \\ + \alpha_a(a_{it} - a(g_i)) + \alpha_e(e_{it} - e(g_i)) + \alpha_s(s_{it} - s(g_i))).\end{aligned} \tag{4}$$

Equation (4) is a transformation of equation (3) where the fixed effects that control for selection bias are defined as $\widetilde{\delta}_g = \delta_g + \alpha_a a(g) + \alpha_e e(g) + \alpha_s s(g)$.

We take advantage of the fact that educational attainment is one's accumulated years of being a student, that is, $e_{i,t+1} = e_{it} + s_{it}$. This relationship implies that the term $e_{it} - e(g_i)$ in equation (4) equals $-s_{it}$ when $e(g_i) = e_{i,t+1}$ (and, of course, $e_{it} - e(g_i) = 0$ when $e(g_i) = e_{it}$). Single-year changes in education can therefore be observed even though the CPS reports

[6]The effect of being a high school or college student may differ due to variation in the extent of political discourse in the two environments. Unfortunately, we cannot test whether this is the case since the vast majority of changes in student status in our data sample involve college students (see Section 5 for details on the data sample employed).

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multiple years of schooling as "some college." A key assumption is that individuals who are students as of November remain in school for the rest of the year. We tested the validity of this assumption using CPS data prior to 1992, which reports education by individual grade. The two methods of calculating an individual's change in education give very similar results, which gives us confidence that student status in the earlier year is an accurate measure of an individual's change in years of schooling from one year to the next.

4.3 *Estimation Efficiency*

As stated earlier, we define $g_i = (a_{i,t+1}, e_{i,t+1}, s_{i,t+1})$ for those individuals who can be matched to the following year's survey. Note, however, that additional information is known for such individuals. Specifically, we observe their current age, education, and student status. Therefore, we could treat such individuals as if they could not be matched to the following year's survey and define $g_i = (a_{it}, e_{it}, s_{it})$. Estimates that are more precise are obtained after the estimation procedure is modified so that matched individuals are included twice in the data set.[7] We let $g_i = (a_{it}, e_{it}, s_{it})$ in the first inclusion and define $g_i = (a_{i,t+1}, e_{i,t+1}, s_{i,t+1})$ in the second. Since the two observations for a given person are correlated, we employ robust standard errors that cluster observations for the same individual (Huber 1967).

4.4 *Attrition Bias*

Our empirical methodology relies heavily on the panel structure of the CPS. Even though the panel has a relatively short duration (16 months), some individuals drop out of the survey due to migration and other reasons. Sample attrition is a concern since those who leave the CPS panel may have unobservable differences from those who do not drop out. This potentially violates our identification assumption that those who will have a given age and education in the following year have identical unobserved characteristics as those with the same age and education in the current year. The former group is composed solely of individuals who can be matched to the following year's survey, whereas this is true for only a subset of the latter group.

Our solution to this problem is to compare only those individuals who do not drop out of the CPS panel in the following year. Specifically, we rely upon the following alternative identification assumption. Individuals with a given age and education in the following year have identical unobserved characteristics as those (1) with the same age and education in the current year and (2) that can be matched to the following year's survey. Not only are these two sets of individuals similarly selected due to being 1 year apart in their life cycle but also they are similarly affected by sample attrition since both sets of individuals can be matched to the following year's survey. By comparing these two groups, unobserved characteristics associated with both selection bias in education choice and sample attrition are differenced out, which allows us to isolate the causal effect of education.

This alternative identification assumption is implemented as follows. Let m_{it} be a dummy variable that equals one if an individual can be matched to the following year's survey. Equation (4) is then generalized to allow the unobserved characteristics $\tilde{\delta}_g^m$ of those who can be matched to the following year's survey to differ from the unobserved characteristics $\tilde{\delta}_g^{nm}$ of those who cannot be matched.

[7] It is possible to identify all the model parameters without including matched individuals twice. Doing so makes efficient use of the available data, however, which leads to smaller standard errors.

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$$\Pr(v_{it}=1|X_{it},g_i,a_{it},e_{it},s_{it},m_{it})=\Lambda(X_{it}\beta+\sum_g(\tilde{\delta}_g^m m_{it}+\tilde{\delta}_g^{nm}(1-m_{it}))1_{g_i=g}$$
$$+\alpha_a(a_{it}-a(g_i))+\alpha_e(e_{it}-e(g_i))+\alpha_s(s_{it}-s(g_i))). \quad (5)$$

This specification accounts for attrition bias in a very general manner since we do not impose any relationship between $\tilde{\delta}_g^m$ and $\tilde{\delta}_g^{nm}$. Note that equation (4) is a special case of equation (5) where those who can be matched to the following year's survey have identical unobserved characteristics as those who cannot be matched (i.e., $\tilde{\delta}_g^m=\tilde{\delta}_g^{nm}, \forall g$). By comparing the results from equations (4) and (5), we test whether sample attrition has a significant effect. As discussed in Section 6, correcting for attrition bias has little impact on the obtained estimate of education's effect on voter turnout.

5 Data

The data source employed is the CPS. Every other November survey contains a supplement regarding voter turnout in that month's presidential or congressional election. The analysis considers the nine major elections over the period 1980–2000 for which individuals can be matched from the current to the following year's survey.[8]

As detailed in the previous section, the empirical methodology requires the use of panel data. Further, since a limited source of variation is used to identify the causal impact of education, a large data set is needed to obtain reasonably precise results. The CPS is the only available data set containing voter turnout information that fulfills these requirements. A drawback of using the CPS is that it lacks some of the control variables that have been employed in the voter turnout literature, such as voter preference and attitudinal characteristics. These variables are available in other data sets, such as the National Election Study, that unfortunately do not meet the necessary criteria.

The data sample is constructed by combining two sets of individuals. The first is a group consisting of U.S. citizens aged 18–24 with at least 11 years of education.[9] To employ the cohort-matching procedure described in Section 4, the data sample is supplemented by an additional set of individuals who will satisfy the same age and education requirements in the following year and who are U.S. citizens of voting age in the current year. It is not possible to include those older than 24 years, as the CPS does not report student status for such people. Further, the minimum grade requirement is imposed since virtually all changes in schooling among those of voting age occur at higher education levels.[10]

The panel structure of the CPS is somewhat unusual in that the survey follows a given residence over a period of time, rather than a given individual or household. The CPS surveys each residence for two sets of four consecutive months that are separated by an 8-month gap. In any given year, those currently in their first sequence of surveys can potentially be matched to the CPS given 12 months later.

[8] Data sampling changes prevent the matching of individuals across 1984–1985 and 1994–1995.

[9] In some survey years, the question of citizenship is only indirectly asked. In an effort to maintain a consistent definition, an individual is considered a citizen unless he specifically answers that he is not.

[10] The inclusion of less-educated individuals would still help identify the effect of the nonschooling variables. Our concern, however, is that the effect of these variables might significantly differ for the less educated. To accommodate this possibility, one could include a full set of interaction terms that allow each model parameter to differ for those with less than 11 years of education. However, since no parameters would be pooled across the two sets of individuals, this is equivalent to estimating the model twice: once for those with less than 11 years of education and once for the rest of the data sample. We report only the latter estimation since our focus is on the marginal effect of education, which cannot be identified for the sample consisting of those with less than 11 years of education due to a lack of voting age individuals still in those grades.

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Data inaccuracies can result in the match of two distinct individuals rather than the same individual in two different periods. For this reason, one must use some criteria for assessing whether a match is valid (Madrian and Lefgren 2000). We require an exact match across fixed characteristics (household ID, household number, line number, state, race, and sex) and plausible differences in age and education. The difference in age must be between zero and two, whereas education is required to be weakly increasing along adjacent education levels. Depending on the year, 3%–5% of matches are invalidated on this basis.

6 Results

For each election year, we estimate the model using two dependent variables. The first is a dummy variable for whether an individual is a registered voter and the second is a dummy variable for having voted in the November election of that year.[11] Since we rely on a very limited source of variation, the effect of education is imprecisely estimated for each individual year. Therefore, our discussion focuses on the average effect of education across multiple elections.[12]

Table 2 reports the marginal effect of education and student status.[13,14] In this baseline specification, we include additional characteristics X_{it} in the model (race, gender, marital status, a dummy variable for having moved in the last 12 months, and a set of dummy variables for state of residence) but do not control for attrition bias. In the sensitivity analysis presented below, we consider alternative specifications that either omit X_{it} or control for sample attrition.

The results presented in Table 2 offer little support for education being a major determinant of voter turnout. Across all elections, the average effect of 1 year of education is only 0.4 percentage points (SE = 0.8). Stronger results are found for the effect of student status. The average impact across all elections is a highly significant 5.2 percentage points (SE = 1.1). Even though an additional year of schooling has little influence on voter turnout, being in an educational environment has a significant effect. Note, however, that this result does not speak to the impact of education on aggregate voting rates since most individuals are students only briefly during their adult lives.

[11] Since the CPS is based on self-reported, rather than validated, voting behavior, misreporting is a potential source of bias in the results presented in this section (Sigelman 1982; Presser and Traugott 1992). Given the limited availability (and small sample size) of validated voter turnout data, there is little one can do to avoid this potential problem.

[12] Alternatively, we could have pooled the data across all years when estimating the model. By estimating a separate model for each year, however, we avoid misspecification problems that arise when invalid parameter restrictions are imposed (Teixeira 1987).

[13] Results for individual elections are reported in the Appendices. The logit parameter estimates are converted to marginal effects using the derivative method of calculation, evaluated at the interyear mean of each dependent variable. The original parameter estimates can be recovered by dividing the voter turnout and voter registration results by 0.235 and 0.244, respectively.

[14] The marginal effects of the nonschooling variables are reported in the Appendices. Since the estimates for the nonschooling variables are not of direct interest to this paper, only a brief summary of the findings is presented. Interyear averages indicate that women and blacks are more likely to vote than men and whites whereas other races are substantially less likely to vote (Leighley and Nagler 1992b). The race effects should be cautiously interpreted, however, since previous research shows that the influence of race significantly depends on whether validated voter data are employed (Abramson and Claggett 1991). Those who marry at a young age have a lower probability of voting (Stoker and Jennings 1995). Lastly, individuals who have recently moved are much less likely to vote (Squire, Wolfinger, and Glass 1987), whereas older individuals are more likely to do so. Each of these results holds for both voter turnout and voter registration.

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Table 2 Marginal effects on voter turnout and voter registration, selection bias model

	Education			*Student*		
	Estimate	*SE*		*Estimate*	*SE*	
Voter turnout						
Total	0.004	0.008		0.052	0.011	***
Presidential elections	0.005	0.011		0.056	0.015	***
Congressional elections	0.001	0.012		0.046	0.017	***
Difference	0.004	0.016		0.010	0.022	
1980–1988	−0.012	0.011		0.032	0.016	**
1990–2000	0.016	0.012		0.068	0.016	***
Difference	0.028	0.016	*	0.036	0.022	
Voter registration						
Total	0.024	0.008	***	0.068	0.011	***
Presidential elections	0.011	0.011		0.056	0.016	***
Congressional elections	0.041	0.011	***	0.082	0.016	***
Difference	−0.030	0.016	*	−0.027	0.022	
1980–1988	0.024	0.011	**	0.067	0.016	***
1990–2000	0.025	0.011	**	0.068	0.016	***
Difference	0.001	0.016		0.002	0.022	

Note. Marginal effects are evaluated at the interyear mean of voter turnout (voter registration), which is 0.38 (0.58). Age, education, and student status are controlled for in the manner described in Section 4. In addition, race, gender, marital status, a dummy variable for having moved in the last 12 months, and a set of dummy variables for state of residence are also included as control variables. Significance levels correspond to *0.1, **0.05, and ***0.01.

Table 2 also presents estimates for when voter registration is the dependent variable. Across all elections, the effect of 1 year of education is larger than before, although still relatively small, with a point estimate of 2.4 percentage points (SE = 0.8). In addition, we find that student status has a positive impact on voter registration. The average impact across all elections of being a student is 6.8 percentage points (SE = 1.1).

We explore whether the effects of education and student status differ in presidential and congressional elections, along with whether these effects changed between the 1980s and the 1990s. Table 2 reports evidence of two differences, although they should not be overemphasized given their marginal statistical significance. First, there is some evidence that the impact of education on voter turnout is increasing over time. The difference between the 1990s and the 1980s is 2.8 percentage points (SE = 1.6), which is statistically significant at the 10% level. In addition, when voter registration is the dependent variable, the effect of education is bigger in congressional elections than in presidential elections. With a point estimate of only 1.1 percentage points (SE = 1.1), one cannot reject the hypothesis that the effect in presidential elections is zero while the effect in congressional elections is a much larger 4.1 percentage points (SE = 1.1). The difference between the effect of education in presidential and congressional elections is 3.0 percentage points (SE = 1.6), which is statistically significant at the 10% level.

The CPS lacks control variables that have been employed in previous research, such as voter preference and attitudinal characteristics. This is not a problem, however, since our empirical methodology explicitly accounts for unobserved characteristics. We identify the causal effect of education by comparing those who will have a given age and

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Table 3 Sensitivity analysis, selection bias model

	(i)			*(ii)*			*(iii)*		
	Estimate	*SE*		*Estimate*	*SE*		*Estimate*	*SE*	
Voter turnout									
Education	0.004	0.008		0.006	0.008		−0.003	0.007	
Student	0.052	0.011	***	0.063	0.011	***	0.055	0.014	***
Control for									
Demographics		Yes			No			Yes	
Attrition bias		No			No			Yes	
Voter registration									
Education	0.024	0.008	***	0.027	0.008	***	0.018	0.007	**
Student	0.068	0.011	***	0.082	0.011	***	0.075	0.014	***
Control for									
Demographics		Yes			No			Yes	
Attrition bias		No			No			Yes	

Note. Marginal effects are evaluated at the interyear mean of voter turnout (voter registration), which is 0.38 (0.58). Age, education, and student status are controlled for in the manner described in Section 4. Specifications (i) and (iii) include additional control variables for race, gender, marital status, a dummy variable for having moved in the last 12 months, and a set of dummy variables for state of residence. Specification (iii) includes additional variables that control for attrition bias, as described in Section 4. Significance levels correspond to **0.05 and ***0.01.

education in the following year to those with the same age and education in the current year. If these two groups are identical apart for differences in age and education as we assume, it is unnecessary to include additional control variables in the model specification since their impact is differenced out when making comparisons between the two groups. The only reason to include additional control variables is to increase the explanatory power of the model and thereby obtain smaller standard errors. We demonstrate this by estimating the model without the additional control variables included in X_{it}. The results from doing so are reported in specification (ii) of Table 3. To facilitate comparison, specification (i) presents counterpart results from the baseline model reported in Table 2. As expected, we obtained very similar findings after omitting X_{it} from the model.

As a second robustness check, we explore whether sample attrition has a significant impact on our findings. Specification (iii) of Table 3 reports the results from reestimating the model after controlling for attrition bias in the manner described in Section 4. In this specification, the unobserved characteristics of those who can be matched to the following year's survey are allowed to differ from the unobserved characteristics of those who cannot be matched. The results are similar to specification (i), which does not control for sample attrition. As such, we conclude that attrition bias has little impact on the obtained estimate of education's effect on voter turnout.

To summarize, our results show that being a student has a significant positive impact on both voter turnout and voter registration. However, an additional year of education does not have a statistically significant impact on voter turnout and has only a small positive effect on voter registration. Since we do not find an individual is substantially more likely to vote after gaining an additional year of education, our results cast doubt on the civic education theory. A potential shortcoming in our analysis, however, is that we do not

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Table 4 Marginal effects on voter turnout and voter registration, non–selection bias model

	College			*Student*		
	Estimate	*SE*		*Estimate*	*SE*	
Voter turnout						
Total	0.278	0.007	***	0.095	0.004	***
Presidential elections	0.337	0.009	***	0.116	0.005	***
Congressional elections	0.206	0.010	***	0.070	0.007	***
Difference	0.132	0.013	***	0.046	0.008	***
1980–1988	0.262	0.009	***	0.076	0.006	***
1990–2000	0.290	0.010	***	0.111	0.006	***
Difference	0.028	0.013	**	0.035	0.008	***
Voter registration						
Total	0.283	0.007	***	0.119	0.004	***
Presidential Elections	0.355	0.011	***	0.134	0.006	***
Congressional Elections	0.197	0.010	***	0.101	0.006	***
Difference	0.158	0.015	***	0.033	0.009	***
1980–1988	0.266	0.010	***	0.112	0.006	***
1990–2000	0.297	0.011	***	0.125	0.006	***
Difference	0.030	0.015	**	0.012	0.009	

Note. Marginal effects are evaluated at the interyear mean of voter turnout (voter registration), which is 0.38 (0.58). The model includes control variables for age, education, student status, race, gender, marital status, a dummy variable for having moved in the last 12 months, and a set of dummy variables for state of residence. Significance levels correspond to **0.05 and ***0.01.

estimate the effect of education for a representative sample of the general population. The data set only contains individuals who are quite young, and the effect of education is identified from those individuals who are in the "institutionalized" state of being in school. Further, the marginal impact of recently obtained education is estimated. The effect of education might not materialize immediately after an individual has obtained such schooling.

It is possible that the civic education theory is correct, but education influences voter turnout only later in life. If this were true, however, one would not expect to observe a positive correlation between education and voter turnout among the young. To explore this issue, the data employed in the above analysis are used to estimate a standard model of voter turnout that does not control for selection bias. As seen in Table 4, education has a large positive impact on voter turnout when we do not control for selection bias.[15] Across all elections, the average effect of a college degree (relative to a high school education) is a substantial 27.8 percentage points (SE = 0.7). We also find that education has a significant impact on voter registration, with the marginal effect of a college degree being 28.3 percentage points (SE = 0.7). After dividing these estimates by four to make them comparable to the results reported earlier in Tables 2 and 3, which measure the impact of an additional year of school, the effect of education is several times larger when selection bias is ignored. The only exception is the effect of education on voter registration during congressional elections, where the estimate is only slightly larger when selection bias is ignored. Similar results are found for the impact of student status, with substantially larger effects in the model that does not control for selection bias.

[15]Results for individual elections are reported in the Appendices.

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Our earlier finding that education has little causal impact, once selection bias is controlled for, is therefore not an artifact of using a data set comprising individuals from a narrow segment of the population. Further, the evidence does not indicate that the effect of education is entirely delayed, since the non–selection bias estimates show education's impact is quite large even for the young. Rather, our findings suggest that a significant portion of the cross-sectional relationship between education and voter turnout is spurious, resulting from selection bias in education choice.

7 Conclusion

We test the central implication of the civic education theory. As an individual obtains additional schooling, and thereby develops skills and knowledge that facilitate political engagement, is he more likely to vote? Our empirical methodology is well suited to addressing this question since it measures the marginal impact of an additional year of education. We compare those who will acquire a given level of education in the following year to those who are 1 year older and currently have that particular level of education. The effect of unobserved characteristics for these similarly selected cohorts is differenced out, isolating the causal effect of education.

Our results show that being a student has a significant positive effect on both voter turnout and voter registration. However, an additional year of education has little impact on voter turnout and has only a small (but statistically significant) effect on voter registration. Since being in an educational environment, rather than an individual's accumulated years of schooling, is the primary effect of education, our results are evidence against the civic education theory. Moreover, they suggest that education has little long-run effect since most individuals are students for only a brief period in their adult lives.

As a robustness check, we explore a variant of the theory in which the impact of education is delayed, not materializing until after an individual has completed school. If this were the case, however, one should not find a positive correlation between education and voter turnout among the young. In contrast, we find that both years of education and student status appear to have a much larger impact on voter turnout when selection bias is ignored. This evidence lends support to the selection bias theory of why the highly educated are more likely to vote. Unobserved factors, such as family background characteristics, influence both education and voter turnout, which generates a positive correlation between the two variables.

We recognize two limitations of our analysis. First, it does not estimate the marginal effect of education for the general population. Rather, we estimate the short-run impact of education for those currently in the process of obtaining their schooling. It is certainly possible that the effect of education does not fully materialize until later in life. Second, our analysis does not measure the marginal impact of education across all levels of schooling. We are unable to estimate the effect of education for less than grade 11 due to a lack of voting age individuals still in those grades.

Addressing these issues is an important area for future research. Only after the effect of education is estimated for different segments of the population and for different margins of educational attainment will the literature be able to state conclusively whether education leads to higher voter turnout.

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Appendix A Variable means

	1980	*1982*	*1984*	*1986*	*1988*	*1990*	*1992*	*1994*	*1996*	*1998*	*2000*
Voter	0.49	0.31	0.50	0.27	0.45	0.26	0.54	0.26	0.41	0.22	0.44
Registered voter	0.60	0.53	0.63	0.52	0.60	0.52	0.66	0.54	0.62	0.52	0.61
Female	0.53	0.52	0.52	0.52	0.53	0.52	0.51	0.51	0.51	0.51	0.51
Black	0.11	0.12	0.13	0.13	0.13	0.14	0.14	0.15	0.14	0.13	0.15
Hispanic	0.04	0.04	0.05	0.06	0.06	0.06	0.06	0.08	0.09	0.10	0.10
Other nonwhite	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.03	0.03	0.03
Married	0.29	0.27	0.25	0.24	0.22	0.19	0.18	0.18	0.16	0.14	0.15
Recently moved	0.30					0.31	0.31	0.33	0.32	0.31	0.32
Education	12.65	12.67	12.69	12.70	12.74	12.74	12.73	12.71	12.67	12.67	12.70
Age	21.02	21.11	21.15	21.11	21.08	21.03	21.12	21.12	20.99	20.92	20.96
Student	0.25	0.27	0.32	0.34	0.38	0.40	0.41	0.42	0.44	0.46	0.45
Number observed	16,866	14,639	13,572	12,303	11,083	10,845	10,542	9451	8029	7848	7667

Note. Data sample restricted to U.S. citizens who are, in the current or following year, aged 18–24 with at least 11 years of education and who are at least 18 years old in the current year. Recently moved is a dummy variable for having changed residence in the last 12 months and is not available for the 1982–1988 surveys. Education is measured as 12 = high school, 13 = some college, 16 = college degree, and 17 = advanced degree. Educational attainment less than high school is coded by numeric grade level. Age is measured in whole years. All other variables are binary.

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Appendix B Marginal effects on voter turnout, selection bias model

	Estimate	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*
	1980		1982		1986		1988			
Intercept	−0.258	0.045	−0.467	0.077	−0.580	0.127	−0.437	0.090		
Education	−0.007	0.021	−0.003	0.021	−0.032	0.023	−0.004	0.022		
Age	0.016	0.010	0.009	0.011	0.033	0.015	0.000	0.014		
Student	0.021	0.028	0.011	0.028	0.053	0.034	0.042	0.034		
Female	0.012	0.009	−0.020	0.010	0.002	0.011	0.021	0.011		
Black	−0.101	0.016	0.064	0.017	0.069	0.018	0.011	0.018		
Hispanic	−0.167	0.024	0.015	0.026	−0.009	0.029	−0.060	0.028		
Other nonwhite	−0.154	0.037	−0.063	0.042	−0.129	0.043	−0.174	0.043		
Married	0.007	0.011	−0.080	0.012	−0.066	0.014	−0.051	0.014		
Recently moved	−0.121	0.010								
Number observed	20,092		17,446		14,360		13,019			
	1990		1992		1996		1998		2000	
Intercept	−0.455	0.118	−0.326	0.091	−0.437	0.118	−0.512	0.131	−0.362	0.100
Education	0.002	0.024	0.075	0.023	−0.011	0.025	0.039	0.030	−0.026	0.027
Age	0.010	0.016	−0.004	0.015	0.011	0.017	0.008	0.021	0.053	0.019
Student	0.045	0.035	0.099	0.031	0.064	0.034	0.076	0.039	0.055	0.035
Female	0.009	0.012	0.029	0.011	0.053	0.013	0.026	0.015	0.024	0.013
Black	0.023	0.020	−0.015	0.019	0.057	0.022	0.068	0.026	0.022	0.021
Hispanic	−0.076	0.029	−0.085	0.025	−0.102	0.026	−0.033	0.031	−0.090	0.025
Other nonwhite	−0.087	0.041	−0.203	0.033	−0.147	0.037	−0.146	0.043	−0.171	0.036
Married	−0.060	0.018	−0.019	0.016	−0.004	0.019	−0.029	0.023	0.018	0.020
Recently moved	−0.176	0.015	−0.135	0.012	−0.119	0.014	−0.171	0.018	−0.150	0.014
Number observed	12,781		12,554		9664		9352		9181	

Note. Marginal effects are evaluated at the interyear mean of voter turnout, which is 0.38. Data sampling changes prevent the estimation of the model for 1984 and 1994. A set of dummy variables for state of residence are also included as control variables.

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Appendix C Marginal effects on voter registration, selection bias model

	Estimate	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*
	1980		1982		1986		1988			
Intercept	−0.203	0.044	−0.246	0.055	−0.336	0.086	−0.205	0.072		
Education	−0.006	0.023	0.039	0.021	0.050	0.022	0.011	0.024		
Age	0.021	0.010	0.030	0.011	0.020	0.013	0.013	0.014		
Student	0.048	0.031	0.084	0.027	0.088	0.033	0.046	0.036		
Female	0.009	0.009	−0.007	0.009	0.004	0.010	0.025	0.011		
Black	−0.052	0.016	0.049	0.017	0.075	0.017	0.056	0.018		
Hispanic	−0.127	0.024	0.020	0.024	−0.036	0.027	−0.060	0.027		
Other nonwhite	−0.163	0.039	−0.113	0.041	−0.102	0.039	−0.182	0.043		
Married	−0.022	0.011	−0.087	0.012	−0.086	0.013	−0.054	0.015		
Recently moved	−0.108	0.010								
Number observed	20,010		17,279		14,277		12,996			
	1990		1992		1996		1998		2000	
Intercept	−0.159	0.078	−0.226	0.079	−0.367	0.097	−0.442	0.097	−0.305	0.088
Education	0.028	0.023	0.042	0.024	0.017	0.026	0.047	0.025	−0.009	0.027
Age	−0.001	0.014	0.025	0.015	0.016	0.016	0.052	0.017	0.063	0.018
Student	0.061	0.033	0.074	0.034	0.068	0.035	0.096	0.036	0.042	0.037
Female	−0.003	0.011	0.011	0.012	0.039	0.013	0.035	0.013	0.032	0.013
Black	0.043	0.018	0.030	0.019	0.049	0.022	0.022	0.023	0.025	0.022
Hispanic	−0.060	0.025	−0.053	0.025	−0.046	0.025	−0.020	0.025	−0.105	0.024
Other nonwhite	−0.096	0.036	−0.160	0.033	−0.140	0.036	−0.235	0.035	−0.180	0.035
Married	−0.052	0.016	−0.038	0.016	0.003	0.018	−0.042	0.020	0.029	0.021
Recently moved	−0.157	0.013	−0.088	0.013	−0.104	0.014	−0.130	0.015	−0.093	0.015
Number observed	12,582		12,551		9707		9319		9096	

Note. Marginal effects are evaluated at the interyear mean of voter registration, which is 0.58. Data sampling changes prevent the estimation of the model for 1984 and 1994. A set of dummy variables for state of residence are also included as control variables.

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Appendix D Marginal effects on voter turnout, non–selection bias model

	Estimate	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*
	1980		1982		1984		1986		1988			
Intercept	−0.617	0.052	−0.907	0.060	−0.685	0.058	−1.058	0.068	−0.733	0.067		
Female	0.011	0.008	−0.017	0.009	0.038	0.009	0.004	0.010	0.021	0.010		
Black	−0.092	0.014	0.071	0.016	0.071	0.015	0.074	0.017	0.017	0.016		
Hispanic	−0.156	0.022	0.007	0.024	−0.047	0.022	−0.026	0.027	−0.066	0.025		
Other nonwhite	−0.155	0.034	−0.065	0.038	−0.168	0.034	−0.091	0.040	−0.173	0.039		
Married	−0.002	0.010	−0.073	0.011	−0.033	0.011	−0.059	0.013	−0.045	0.013		
Recently moved	−0.117	0.009										
High school	0.110	0.016	0.078	0.020	0.109	0.018	0.106	0.024	0.100	0.021		
Some college	0.175	0.010	0.128	0.011	0.158	0.012	0.109	0.014	0.162	0.013		
College graduate	0.198	0.019	0.079	0.018	0.144	0.020	0.028	0.019	0.129	0.021		
Advanced degree	0.038	0.045	−0.060	0.039	−0.107	0.041	−0.008	0.046	0.016	0.046		
Age	0.020	0.002	0.030	0.003	0.022	0.003	0.034	0.003	0.023	0.003		
Student	0.061	0.011	0.033	0.012	0.111	0.012	0.074	0.014	0.100	0.014		
Number observed	16,605		14,196		13,326		11,901		10,807			
	1990		1992		1994		1996		1998		2000	
Intercept	−0.803	0.075	−0.586	0.069	−0.743	0.082	−0.311	0.077	−0.928	0.093	−0.575	0.079
Female	0.007	0.011	0.026	0.010	0.020	0.012	0.048	0.012	0.021	0.014	0.021	0.012
Black	0.023	0.018	−0.021	0.017	−0.001	0.021	0.047	0.020	0.060	0.024	0.016	0.019
Hispanic	−0.072	0.027	−0.089	0.023	−0.041	0.028	−0.100	0.024	−0.038	0.028	−0.089	0.023
Other nonwhite	−0.079	0.037	−0.203	0.029	−0.136	0.038	−0.128	0.032	−0.138	0.040	−0.162	0.033
Married	−0.053	0.016	−0.019	0.014	0.036	0.017	−0.003	0.017	−0.021	0.021	0.023	0.018
Recently moved	−0.184	0.013	−0.127	0.011	−0.180	0.014	−0.117	0.013	−0.160	0.016	−0.145	0.013
High school	0.095	0.027	0.129	0.022	0.108	0.028	0.098	0.023	0.050	0.029	0.075	0.023
Some college	0.125	0.015	0.163	0.012	0.111	0.015	0.149	0.014	0.126	0.018	0.125	0.015
College graduate	0.076	0.021	0.225	0.022	0.167	0.021	0.189	0.022	0.079	0.025	0.206	0.024
Advanced degree	−0.035	0.045	0.039	0.085	0.119	0.095	−0.007	0.114	−0.040	0.107	0.109	0.108
Age	0.025	0.003	0.019	0.003	0.023	0.004	0.004	0.004	0.030	0.004	0.018	0.004
Student	0.047	0.015	0.151	0.013	0.099	0.015	0.128	0.014	0.097	0.017	0.144	0.014
Number observed	10,485		10,307		9143		7814		7600		7547	

Note. Marginal effects are evaluated at the interyear mean of voter turnout, which is 0.38. A set of dummy variables for state of residence are also included as control variables. The education results correspond to the difference between the given education level and the next highest level. The omitted category corresponds to those with less than a high school degree.

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Appendix E Marginal effects on voter registration, non–selection bias model

	Estimate	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*	*Estimate*	*SE*
	1980		1982		1984		1986		1988			
Intercept	−0.657	0.055	−1.066	0.059	−0.688	0.061	−1.197	0.065	−0.764	0.070		
Female	0.010	0.009	−0.012	0.009	0.022	0.010	0.005	0.010	0.022	0.010		
Black	−0.051	0.015	0.049	0.016	0.118	0.017	0.074	0.016	0.054	0.017		
Hispanic	−0.130	0.022	0.016	0.023	−0.027	0.023	−0.030	0.025	−0.055	0.026		
Other nonwhite	−0.161	0.036	−0.120	0.037	−0.191	0.035	−0.077	0.038	−0.191	0.040		
Married	−0.032	0.010	−0.086	0.011	−0.052	0.012	−0.086	0.012	−0.052	0.014		
Recently moved	−0.107	0.009										
High school	0.136	0.016	0.124	0.018	0.114	0.018	0.133	0.021	0.127	0.021		
Some college	0.190	0.011	0.128	0.011	0.169	0.013	0.129	0.013	0.175	0.014		
College graduate	0.196	0.023	0.065	0.020	0.166	0.024	0.001	0.020	0.113	0.024		
Advanced degree	0.085	0.058	−0.005	0.045	−0.093	0.052	−0.008	0.050	0.065	0.059		
Age	0.026	0.002	0.043	0.003	0.028	0.003	0.049	0.003	0.031	0.003		
Student	0.101	0.013	0.110	0.013	0.126	0.014	0.104	0.014	0.120	0.015		
Number observed	16,531		14,058		13,286		11,816		10,787			
	1990		1992		1994		1996		1998		2000	
Intercept	−0.822	0.071	−0.563	0.075	−0.817	0.075	−0.323	0.080	−1.146	0.083	−0.672	0.083
Female	−0.008	0.011	0.013	0.011	0.017	0.011	0.049	0.012	0.038	0.012	0.027	0.013
Black	0.043	0.017	0.022	0.018	−0.015	0.018	0.049	0.020	0.011	0.021	0.024	0.021
Hispanic	−0.063	0.024	−0.054	0.024	−0.094	0.024	−0.104	0.025	−0.024	0.024	−0.096	0.023
Other nonwhite	−0.086	0.034	−0.159	0.031	−0.184	0.034	−0.132	0.034	−0.213	0.034	−0.162	0.033
Married	−0.051	0.015	−0.041	0.015	−0.017	0.016	−0.003	0.018	−0.042	0.018	0.028	0.019
Recently moved	−0.166	0.012	−0.083	0.012	−0.136	0.012	−0.121	0.013	−0.126	0.014	−0.088	0.014
High school	0.106	0.022	0.163	0.021	0.151	0.022	0.102	0.023	0.133	0.023	0.103	0.023
Some college	0.171	0.014	0.176	0.013	0.151	0.013	0.155	0.015	0.148	0.015	0.142	0.015
College graduate	0.069	0.023	0.231	0.027	0.072	0.022	0.196	0.023	0.047	0.025	0.220	0.030
Advanced degree	0.037	0.052	0.025	0.102	−0.035	0.104	−0.007	0.118	0.091	0.129	0.046	0.137
Age	0.036	0.003	0.021	0.003	0.031	0.003	0.004	0.004	0.047	0.004	0.026	0.004
Student	0.082	0.015	0.176	0.014	0.130	0.014	0.133	0.015	0.079	0.015	0.147	0.016
Number observed	10,311		10,307		9218		7850		7559		7464	

Note. Marginal effects are evaluated at the interyear mean of voter registration, which is 0.58. A set of dummy variables for state of residence are also included as control variables. The education results correspond to the difference between the given education level and the next highest level. The omitted category corresponds to those with less than a high school degree.

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