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Does education improve citizenship? Evidence from the United States and the United Kingdom

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Abstract

Many studies document an association between schooling and civic participation, but none credibly investigate causal links. We explore the effect of extra schooling induced through compulsory schooling laws on the likelihood of becoming politically involved in the United States and the United Kingdom. We find that educational attainment is related to several measures of political interest and involvement in both countries. We find a strong and robust relationship between education and voting for the United States, but not for the United Kingdom. Our US results approach the UK findings when conditioning on registration, possibly indicating that registration rules present a barrier to participation.

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1. Introduction

The Commonwealth requires the education of the people as the safeguard of order and liberty—Inscription above the entrance to the Boston Public Library.

A large body of research in the last 30 years shows that schooling has a significant private return in terms of increased earnings. Yet, it is possible that education creates

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other benefits to society that are not reflected in the earnings of the educated. One potentially important example of such positive externalities of education is enhanced political behavior. Economists, educators and politicians commonly argue that one of the benefits of education is that a more educated electorate enhances the quality of democracy. If this is true, then education has social benefits over and above the private return, and Pigouvian subsidies for education may produce more efficient education acquisition decisions.[1]

Interestingly, the argument that education generates positive externalities through its effects on political behavior is not raised only by those who support a larger role for the government.[2] The same argument resonates with noted advocates of a limited role for government, such as Adam Smith and Milton Friedman.[3] For example, Friedman (1962) argues that

> "A stable and democratic society is impossible without a minimum degree of literacy and knowledge on the part of most citizens and without widespread acceptance of some common set of values. Education can contribute to both. In consequence, the gain from education of a child accrues not only to the child or to his parents but also to other members of the society. [...] Most of us would probably conclude that the gains are sufficiently important to justify some government subsidy."

Why might education affect political behavior? The benefit of education may accrue either through the enhanced quality of participation by a given subset of citizens, or through broader participation among the citizenry. The first channel is important if education equips citizens with the cognitive skills they need to be effective participants in a representative democracy. In this case, education increases citizens' ability to select able leaders, understand the issues upon which they will vote, act as a check on the potential excesses of the government, and recognize corruption in leaders.

The second channel is important if education improves citizens' interest and knowledge of political issues, their involvement in the political process and, ultimately, the effectiveness of their political participation. Economists commonly argue that education provides important social benefits through enhanced civic participation.

[1] At a late stage in the preparation of our manuscript, we became aware of a similar paper to ours, Dee (2003). Although the data sources and some outcome variables are different, the question addressed by the paper is similar to ours.

[2] For example, the Center on Education Policy, a liberal think thank that promotes public schools, argues that "The survival of a representative democracy like the United States ultimately depends on having a large group of well-educated citizens". Schools prepare students to be good citizens in three ways: "(1) teach students about the role of government in the United States; (2) uphold civic values by teaching students to be good citizens; (3) equip students with the civic skills they need to be effective participants in a representative democracy".

[3] Smith (1776) emphasizes the benefits of increased cognitive capacity among the "common people", claiming that "They are more disposed to examine, and more capable of seeing through, the interested complaints of faction and sedition, and they are, upon that account, less apt to be misled into any wanton or unnecessary opposition to the measures of government."

Hanushek (2002), among many others, makes this argument in his survey of public education.[4]

In our paper, we focus primarily on this second channel. Although establishing the link between schooling and the quality of political choices would be potentially more interesting, such a topic is hard to investigate empirically. We can think of no way to measure objectively the quality of decisions made by the electorate. We empirically test whether schooling improves civic participation in the United States and the United Kingdom, as measured by the probability of voting.[5] We also test whether more educated voters have better information on candidates and campaigns.[6] Finally, we test whether education increases other measures of political participation such as the probability of attending political or community meetings, working on community issues, and more in general, being politically active.

To account for unobserved characteristics of individuals that may affect both schooling and political participation, we use an instrumental variable strategy. We measure the effects of schooling through changes in compulsory school laws across different regions at different times. The approach identifies the effect of schooling on citizenship from extending duration in school for would-be-dropouts.[7]

We find a strong effect of education on voting in the United States. More than half of the effect appears to be accounted for by differences in voting registration across education groups. Results from the United Kingdom, where persons are legally responsible and actively assisted to register, show little effect of education on voting. We also find strong and persistent effects of education on civic behavior in both the United States and the United Kingdom. Better educated adults are more likely to follow election campaigns in the media, discuss politics with others, associate with a political group, and work on community issues.

Misreporting is well known to be prevalent in voting turnout data. One concern is that our finding could simply reflect a higher probability of overreporting voting among educated individuals. Using information on the validation of voting status of respondents based on official voting records, we directly examine whether misreporting by survey

[4] There are several theoretical models which suggest a link between education and civic participation. Verba and Nie (1972) argue that individuals with higher socioeconomic status may have higher cognitive skills, benefit from the higher effectiveness of their participation, possess more knowledge about the issues, or be influenced by peer effects from other high socioeconomic status individuals. It is also possible that skills acquired from additional schooling may help an individual overcome the bureaucratic inconveniences and difficulties in registering to vote (Wolfinger and Rosenstone, 1980). Feddersen and Pesendorfer (1996) develop a political economy model in which low-education voters prefer to abstain so that the votes of better-informed voters will carry more weight. In their model, the nonvoting of the low educated is a result of their relative lack of education—providing more education to them will only increase voting if the education level of the rest of society stands still. This approach contrasts with the emphasis in Verba and Nie (1972) and Wolfinger and Rosenstone (1980) on absolute levels of education.

[5] Our focus on the United States and the United Kingdom derives from two reasons. First, both countries offer adequate microdata surveys to study the questions we ask. Second, our instrumental variable strategy requires clear, identifiable, and binding changes in compulsory schooling laws.

[6] This evidence speaks, at least indirectly, to the issue of quality of political choice.

[7] Improved citizenship was an important motivation for the passage of compulsory schooling legislation in the 19th century. Reformers saw education as a means for improving the intelligence and leadership capacity of the electorate, among other things (Kotin and Aikman, 1980).

respondents affects our conclusions on the relationship between voting and education. We conclude that misreporting is not systematically correlated with education and therefore does not affect our estimates.

Overall, our results for the United States lend support to the argument that education generates positive externalities in the form of enhanced political behavior. Our findings indicate that education benefits a representative democracy both by increasing the quantity of citizens' involvement in the electoral process (increased probability of voting) as well as the quality of their involvement (increased information on candidates and political parties).

Below, we begin by giving some background on registration and voting in the United States and the United Kingdom and describing the data sources we employ. Sections 4 and 5 provide the empirical results for voting and for other civic outcomes, respectively. We conclude the paper with a discussion of the implications of our results.

2. Voting and registration

A vast body of empirical research in political science has studied civic participation. Verba and Nie (1972) provide some of the first microempirical evidence of a strong link between socioeconomic status (SES) and political participation. Wolfinger and Rosenstone (1980) break down SES into separate income and education effects and find the influence of education to be stronger than income.[8] Powell (1985) suggests that the SES-participation link is stronger in the United States than in other industrialized countries, a finding appearing again in Blais (2000) and Wattenberg (2002). The empirical association between education and turnout is very well established.

An important weakness of the existing evidence lies in the treatment of causality. If any unobserved factor drives both voting behavior and the acquisition of education, then making causal inferences from the existing evidence would not be justified. For example, some parents might encourage their children to participate in civic activities. If these same parents also instill in their children a taste for education, then the empirical association of education and turnout would not be causal. Lacking a strategy to address the potential endogeneity of schooling, the evidence available in the existing literature offers little firm evidence on the causal nature of the relationship.[9]

2.1. Registration

In order to understand the institutional context in which voting decisions are made, we provide some detail on voting and registration for each of the two countries we study.

[8] Teixeira (1987), Leighley and Nagler (1992), Verba et al. (1995), and Weisberg and Box-Steffensmeier (1999) empirically demonstrate the persistence of these effects through the 1980s and 1990s. Helliwell and Putnam (1999) study the effect of education on various measures of social engagement, finding that individual education has a much stronger affect than aggregate measures of education.

[9] One exception is Brady et al. (1995), in which the authors examine the potential endogeneity of political interest using religious engagement, parents' education, and other variables as instrumental variables. However, it seems likely that these instruments could be related to unobserved heterogeneity in political activity; that they are jointly determined.

The responsibility of registering to vote in the United States rests mainly with the individual. Each state determines its own registration laws, subject to certain limitations imposed at the federal level. At the time our data were collected, most states required registering directly at specific regional offices, during particular hours. Some states required registering more than a month in advance of an election, and some offices were open only during working hours.[10] Many states made it easier to register through the 1970s by allowing mail-in registration and registration while renewing drivers' licenses. In 1993, the National Voter Registration Act (commonly called the 'motor-voter law') was passed federally, mandating mail-in and agency-based registration in all states.

How does registration affect voting? Registration raises the costs of voting, and particularly affects those who find it difficult to deal with bureaucratic hurdles associated with the process. Verba et al. (1995) emphasize the 'resources' or 'civic skills' available to potential voters; concepts analogous to what economists think of as human capital. As well, procrastinators may also be affected by registration, as voters must plan to vote well in advance. If the low educated are less motivated or less able to overcome these barriers, then registration is predicted to adversely affect their voting turnout behavior.

Previous empirical analysis of these reforms suggest a modest effect on voter turnout. Knack (1995) analyzes the 1970s and 1980s variation in registration laws and finds a positive effect on registration, and that about half of the new registrants vote. Martinez and Hill (1999) look at the 1992 and 1996 elections, finding little evidence that the 1993 federal motor-voter law increased turnout. Highton (1997) compares states with high registration barriers to states with low barriers, finding that the effect of the barriers is modest, but hits harder among low-educated voters. Flanigan and Zingale (2002) argue that if registration expansions lead to low-interest citizens becoming registered, little impact may be seen on voter turnout as the newly registered may not turn out to vote.

Unlike the United States, the responsibility to maintain the electoral register in Britain rests with local government officials.[11] Thus, only 5.9% of the British electorate are currently not registered.[12] The process for compiling the register explains this low fraction. Each year, Electoral Registration Officers update the register. A form is sent to every household in a region asking for the householder to indicate the names of all those in the household qualified to be included on the list. If a reply is not received, a reminder is delivered and then a personal visit made to all households who have not returned at least one form. Although electors have the right not to vote, they incur a fine for failing to return a completed form or for giving false information. The penalty was first imposed in 1918, with the current fine for this offence not exceeding £1000 (United Kingdom, 2002).

[10] See, e.g., the discussion of registration in Wolfinger and Rosenstone (1980), Wattenberg (2002), and Patterson (2002).

[11] The responsibility dates back to the passage of the Representation of the People Act of 1918.

[12] This figured is based on verified reports in the 1997 British Election Study.

3. Data

We examine voting behavior and other citizenship outcomes in the United States using the annual National Elections Studies and the November Voting Supplements to the Current Population Survey. We use the British General Election Studies and the Eurobarometer Surveys for our UK analysis. Below, we describe these data sets. We also discuss the issue of measurement error in citizenship variables and describe how we address this issue in the empirical analysis.

3.1. US data sets

The two data sets we employ for the United States complement each other in many ways. Our primary source of data is the complete set of pooled biannual National Election Studies (NES) compiled by Shapiro et al. (2001), spanning the period 1948–2000. These data are the premier source for analysis of voting behavior in the United States and are used regularly for empirical studies by political scientists. It is the largest and most comprehensive data set on political behavior collected continuously for the past 50 years. The survey is collected with telephone and in-person interviews on a random sample of the US population, before and after the election. The data set pulls together demographic information on the respondent with a wide and deep variety of questions about political affiliations, voting behavior, knowledge, and attitudes. Importantly for our instrumental variable strategy, the survey reports the state in which the respondent received his or her education. The sample size for the survey ranges from 662 in 1948 up to 2485 in 1992. We select only those individuals with valid responses to the variables we use. Because some questions weren't asked in all years, the exact number of observations varies across specifications.

The key voting turnout measure we employ is formed from a question in the NES about voting in the November elections. From 1978 onward, the question in the survey was as follows. "In talking to people about the election we often find that a lot of people weren't able to vote because they weren't registered or they were sick or they just didn't have time. How about you, did you vote in the elections this November?" The wording of the question changed only slightly through the time period we study. As well, no differentiation is made among votes for different offices that may be up for election on election day. From this question, we form a binary variable for self-reported voter turnout.

We complement our use of the NES with the 1978–2000 waves of the November Voting Supplement to the Current Population Survey (CPS).[13] The CPS allows us to form variables for being registered and having voted, but no broader citizenship measures. While it reports the current state of residence, we do not observe the state in which the individual grew up. This means that the assignment of school leaving laws must assume that there has been no migration since childhood. We examine this assumption later in the

[13] Earlier years of the CPS November supplement do not report states of residence separately but in regional groups, so cannot be used with our instrumental variable strategy.

paper. The primary advantage of the CPS is its sample size, totalling 946,699 observations with valid responses over the waves we use.

3.2. UK data sets

Two data sets comprise our sources for Great Britain. First, the British Election Studies (BES) collect data for describing and explaining the outcome of general elections. The surveys have been taken immediately after every general election in Britain since 1964, as well as during two nonelection years. We combine the survey years for 1964, 1974, 1979, 1983, 1987, 1992, and 1997.[14]

The combined data set contains information on gender, age, age finished full-time education, and voting behavior. The total sample includes 17,825 adults aged 18 or older, all of whom reached age 14 between 1925 and 1990. The BES is the only study in the United Kingdom to ask a large sample of adults whether they voted or not during the past general election. As with the NES, for most of the survey years, individuals were checked for the accuracy of their response on voting behavior by consulting actual Electoral Register records. Verifying survey records with marked and unmarked Electoral Registers provides a rare opportunity to investigate response bias and, importantly, whether misreporting relates to education attainment or other observable characteristics.

Except for 1997, the BES is not a representative sample of the British population, but instead a sample of those on the electoral register and eligible to vote. The sample is drawn from the register itself. Thus, results from the combined data sets are conditional on being in the register. An analysis using a nationally representative sample is possible using the 1997 BES. For 1997, the sample was drawn from a household address list rather than from the electoral register. Using this survey year only, however, reduces the total sample size to 3390.

Our second source of UK data is the Eurobarometer survey. The Eurobarometers were first assembled in 1970 by the Commission of the European Community and are designed to track opinions and attitudes among European citizens. Each nationally representative survey contains a sample of about 1000 individuals from Britain, and 300 individuals from Northern Ireland. Surveys are carried out more than once a year, from 1973 to 1998. A total of 50 surveys are combined to create a data set with 63,858 individuals who reached age 14 at some point between 1925 and 1990.

The Eurobarometers contain many questions on voting preferences and political activity. Respondents were interviewed and asked, “When you hold a strong opinion, do you ever find yourself persuading your friends, relatives, or fellow workers to share your views?” and, “When you get together with friends, would you say you discuss political matters frequently, occasionally, or never?” Interviewers also asked questions about how often respondents watch news on television or read a newspaper and whether they consider themselves close to any particular party. The Eurobarometers also collect demographic information on age, age finished full time education, and gender.

[14] We omit the 1969 study for lack of a comparable education attainment variable.

3.3. The issue of misreporting

We explore the effect of education on citizenship within the following econometric framework:

$$Y_{it} = \beta' \boldsymbol{X}_{it} + \gamma' \boldsymbol{Q}_{it} + \varepsilon_{it}, \tag{1}$$

where Y_{it} represents an observed citizenship outcome (for example whether the respondent voted in the last elections), $\boldsymbol{X}_{it}$ is a vector of observable characteristics, $\boldsymbol{Q}_{it}$ is a vector of unobservable characteristics, and ε_{it} is the error term. The observed citizenship outcome Y_{it} can be further decomposed as follows:

$$Y_{it} = Y_{it}^{*} + u_{it}. \tag{2}$$

The true value of the citizenship outcome is Y_{it}^{*} and u_{it} is a measurement error term. If the measurement error is correlated with X_{it}, then the estimate of β will be biased. One source of misreporting that could be correlated with schooling is the potential for embarrassment (Bernstein et al., 2001). For example, an individual might wish to hide not having voted from those conducting the survey in order to avoid embarrassment. If misreporting is systematically related to educational attainment, then the estimate of β will be biased, as the estimated β will pick up the propensity to misreport rather than the true effect of education on Y_{it}^{*}.

While misreporting is a general problem for any empirical estimates based on survey data, misreporting is well known to be prevalent in voting turnout data. Although some degree of misreporting is likely to plague many surveys, there are very few examples of data sets where some form of exogenous data validation is available. One important feature of our data is the validation of voting status of respondents using official voting records. With the information on validated voting, we can provide a direct analysis of misreporting. Specifically, in Section 4 we directly test whether misreporting by survey respondents affects our conclusions on the relationship between voting and education. We conclude that misreporting is not systematically correlated with education, and therefore our results on voting are not affected.

A second type of misreporting may be a problem for our broader indicators of civic participation. Some of these outcomes are not connected with a specific action (voted/not voted) but describe subjective opinions of the respondent. For these outcomes, the absence of a preexisting opinion may be another source of measurement error. Bertrand and Mullainathan (2001) explain that individuals may need to expend mental resources in order to form an opinion. Those who have not previously thought about an issue may therefore truthfully report a 'wrong' opinion; an unconsidered opinion that might change upon deeper reflection. Again, if this type of 'soft opinion' measurement error is correlated with education, our estimate of the impact of education on citizenship outcomes would be biased.

For the citizenship outcomes we study, we separate the results into the two categories of *actions* and *attitudes*. We contend that soft opinion bias is less likely to arise in response to questions about past actions than to questions about attitudes. This holds if fewer mental resources need to be expended in the recollection of past actions than in the formation of abstract opinions. To the extent that the soft opinion bias influences our estimates, we take greater caution in the interpretation of the attitudes results.

4. The effect of education on voting

We now turn to the empirical evidence. We begin by looking at differences in the average probability of voting by educational attainment, and subsequently extend the analysis to control for observable and unobservable heterogeneity across education groups. In general, we find that in the United States, more educated citizens appear to be more likely to vote, while this is not true in the United Kingdom. Furthermore, we show that the difference in voting probability across education groups that we uncover in the United States is unlikely to be due to differential misreporting of voting status. Much of the estimated effect of education on voting appears due to registration differences. We find that when we condition on being registered to vote, the remaining effect of schooling on voting in the United States drops to less than a third of the estimated effect based on the whole sample.

Of course, the effect on citizenship may be through income if education increases lifetime earnings. Any differences we uncover across educational attainment groups could be attributed to the higher income that resulted from more education, rather than to some direct component of education. Our approach does not have the power to test among competing mechanisms that potentially explain *how* education affects civic behavior. Instead, we focus on quantifying the magnitude and confirming the existence of the relationship, rather than identifying the exact mechanism.

4.1. Unconditional means

Table 1 analyzes differences in the self-reported probability of voting across education groups. The first column in the top panel indicates that, in the United States, individuals with more schooling are more likely to report having voted in the last election. While only 52% of US high school dropouts report voting, this percentage increases to 67% for high school graduates, 74% for individuals with some college and 84% for college graduates. These results are consistent with previous findings in the political science literature. Interestingly, when we include only individuals who are registered to vote (Column 2), the differences in voting rates across groups significantly decline. For example, the difference in the probability of voting between high school drop outs and high school graduates is 15 percentage points in the full sample, but drops to 5 percentage points in the sample of registered voters. Similarly, the difference in the probability of voting between high school drop outs and college graduates is 32 percentage points in the full sample, but only 10 percentage points in the sample of registered voters.

The bottom panel in Table 1 shows similar conditional means for the United Kingdom. The comparison between UK and US data is complicated by the fact that our UK data report the age when the respondent finished school. This variable for educational achievement has the advantage, however, that it can be matched closely with changes to the minimum school leaving age.

Table 1
Probability of voting and misreporting by education level

	Self-reported probability of voting		Validated probability of voting		Validated probability of registering	Validated probability of misreporting	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full sample	Conditioning on registered to vote	Full sample	Conditioning on registered to vote		Full sample	Conditioning on registered to vote
United States							
Less than high school	0.52	0.85	0.49	0.67	0.67	0.19	0.22
High school	0.67	0.90	0.58	0.74	0.74	0.18	0.20
Some college	0.74	0.93	0.64	0.77	0.80	0.20	0.20
College	0.84	0.95	0.74	0.82	0.87	0.14	0.15
Britain							
Finished school at age 14 (or less)	0.85	0.88	0.69	0.76	0.97	0.17	0.13
Finished age 15	0.79	0.85	0.63	0.74	0.94	0.18	0.13
Finished age 16	0.77	0.83	0.62	0.73	0.94	0.13	0.12
Finished age 17	0.79	0.87	0.63	0.72	0.94	0.17	0.16
Finished age 18 (or more)	0.84	0.88	0.65	0.78	0.91	0.18	0.12

United States data are from the combined National Election Studies for survey respondents. British data are from the combined 1963–1997 British Election Surveys, for citizens aged 18 and over.

There is little discrepancy in voting patterns between the population sample (using only the 1997 BES) and the registered sample (using the combined BES). The fraction voting is somewhat U-shaped by education level. Eighty-five percent of the British electorate who finished school at age 14 reports voting in the last general election survey. The proportion reporting they voted falls slightly below 80% for those who finished school between ages 15 and 17, and the proportion of British that finished school past age 17 increases to 84%. Conditioning on registration does not significantly change the fraction of the population voting. For registered voters who finished school at age 14 or earlier, the voting rate is 88%. The corresponding figures for those who finished school at 15, 16, 17, and 18 or more are 85%, 83%, 87% and 88%, respectively.

4.1.1. Misreporting of voting behavior

The self-reported probabilities of voting in Column 1 are higher than official turnout rates in recent elections.[15] A natural explanation is misreporting. Some respondents may be reluctant to admit that they did not vote. If the probability of misreporting is random across individuals, it will reduce the precision of our estimates, but it will not bias our estimates. On the other hand, it is possible that more educated individuals are more likely

[15] While turnout rates are currently low, they used to be significantly higher in the 1950s and 1960s. For example, the turnout rate in the 1960 presidential election was 63%, while the turn out rate in the 1996 presidential election was only 49%.

to feel the stigma of not having voted and therefore are more likely to overreport voting. In this case, the strong relationship between schooling and voting documented in Column 1 could simply reflect differences across education groups in the probability of misreporting.

One strength on the NES is that, for a selected number of years, the voting status of respondents was validated using official voting records.[16] Voting and registration records were checked in the jurisdiction in which the respondent was living when the survey was conducted. For those who were registered outside the current jurisdiction of residence, attempts to contact the proper jurisdiction by phone were made. With the vote validation variables, we can test whether more educated individuals are more likely to overreport voting participation.

Columns 3 and 4 of Table 1 show the validated probability of reporting, using the subsample of years in which responses were validated. The same positive gradient of voting with education appears in the validated data for the United States as in Columns 1 and 2 for all voters. Column 5 displays the validated probability of registering. The gradient of registration with education is quite strong, with a difference of 20 percentage points between the first and fourth education categories.

To examine misreporting more directly, we show in Columns 6 and 7 the probability of misreporting by education group. We create a misreporting dummy, which is equal to 1 if the respondent reports having voted and official records indicated that she did not vote, or if the respondent reports not having voted and official records indicated that she did vote. The great majority of misreporting cases are those for which respondents report having voted and official records indicate that they actually did not vote. Column 6 shows that, if anything, more educated individuals are slightly less likely to misreport. The probability of misreporting is between 18% and 20% for high school dropouts, high school graduates, and individuals with some college. For college graduates, the misreporting rate drops to 14%. A similar finding emerges from Column 7, where we show the probability of misreporting for registered voters.[17]

Our finding stands in contrast to some established results from political science. In particular, Silver et al. (1986), Leighley and Nagler (1992) and Bernstein et al. (2001) find an upward gradient for misreporting with SES indicators, including education. The explanation for the contrasting finding is the definition of misreporting. These three papers select only those who are validated nonvoters and classify misreporting as falsely reporting having voted. Instead, we take the full sample of nonvoters and voters, and classify all untrue reports as misreports.[18] Ours is the correct measure if the question of interest is whether the regression coefficient on education will be biased by measurement error. We include in our regressions the whole sample of respondents—not just the

[16] Specifically, vote validation studies were conducted in 1964, 1976, 1978, 1980, 1984, 1986, 1988, and 1990.

[17] We also investigated the pattern of misreporting over time. The gradient of misreporting with education is flat for each of the decades from 1960 to 1990. For example, misreporting among those with less than high school and for those with college was 16% in the 1970s. In the 1990s, the misreporting rate for those two education groups was again very similar, at 13%. The level of misreporting across the decades was comparable, with the exception of the 1980s, when it was higher.

[18] Using only nonvoters, our data show a similar pattern to the results reported in Silver et al. (1986). These results are available from the authors on request.

nonvoters—so we care about correlations between misreporting and education in the whole sample of respondents, not just in the subsample of nonvoters.

Similar findings can be seen for the United Kingdom in the bottom half of the table. The probability of misreporting does not appear to be systematically correlated with schooling achievement. The fraction misreporting ranges from 13% to 18% across education categories for the full (1997) sample. The actual fraction of the British electorate that vote is distributed about the same across education groups as the self-reported fraction, ranging between 62% for those finishing school at age 16 and 69% for those finishing school at age 14 or less.

From the above analysis, we conclude that, although misreporting is not uncommon in our sample, it is unlikely to introduce any significant upward bias in our estimates of the effect of schooling on voting participation. If validated information on voting were available for all the elections, we would use the validated information instead of the self-reported data. However, validated voting is available only for a limited number of years. For this reason, throughout the paper we use self-reported voting as our preferred dependent variable, although our results remain similar if validated voting is used instead.[19]

4.2. Evidence from the United States

In the Section 4.1, we showed that more educated individuals are more likely to vote in the United States. However, this documented correlation between schooling and voting might not be causal. There are many individual characteristics that affect both schooling achievement and political participation, possibly creating spurious correlation. In the next two sections, we turn to a more formal analysis of the relationship between education and voting, and we try to account for observable and unobservable individual characteristics that may be correlated with schooling and voting.

4.2.1. National Election Studies results

Table 2 shows OLS regressions based on NES data. The independent variable of primary interest is a dummy equal to 1 if the respondent has a high school education or more. The mean of this high school graduation rate in the full sample is 0.705. The first column indicates that after conditioning on year effects and a fourth-order polynomial in age, the difference in the probability of voting between high school drop outs and individuals with 12 or more years of schooling is 21 percentage points.[20] When we include race and gender (Column 2), this difference increases to 28.6 percentage points. When we also control for state of birth effects (Column 3) and linearly for the year of birth of the respondent, the coefficient is 0.256.[21]

[19] We also tried regressions using misreporting as the dependent variable, finding little evidence that education predicts misreporting.

[20] Using age dummies instead of the quartic gives very similar results.

[21] For all regressions in the paper when we control for the year of birth, we use a linear term rather than year of birth dummies. In the CPS, both the OLS and the IV results are robust to the inclusion of a set of year of birth dummy variables. However, in the NES, the small sample sizes weaken the power of the instruments in the presence of a set of year of birth dummies. To maintain comparability, we control linearly for year of birth effects across all the data sets we use.

Table 2
OLS estimates of the effect of education attainment on the probability of voting in the United States

	Full sample				Conditioning on registered to vote			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High school	0.217 (0.005)**	0.286 (0.005)**	0.256 (0.006)**	0.256 (0.006)**	0.080 (0.017)**	0.100 (0.008)**	0.091 (0.008)**	0.091 (0.008)**
Black		− 0.054 (0.008)**	− 0.014 (0.008)*	− 0.014 (0.008)*		− 0.053 (0.008)**	− 0.027 (0.008)*	− 0.027 (0.008)*
Female		− 0.042 (0.005)**	− 0.041 (0.005)**	− 0.041 (0.005)**		− 0.028 (0.005)**	− 0.029 (0.005)**	− 0.029 (0.005)**
Year effects, quartic in age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of birth effects	No	No	Yes	Yes	No	No	Yes	Yes
Year of birth	No	No	No	Yes	No	No	No	Yes
Observations	30,026	30,026	30,026	30,026	7387	7387	7387	7387

Huber-White standard errors are shown with clustering by state and year of birth.
* Significant coefficient at 10% level.
** Significant coefficient at 1% level.

Columns 5 to 8 report estimates from similar models obtained by including only individuals who are registered to vote. Consistent with our findings in Table 1, conditioning on registration significantly reduces the coefficient for high school graduation. The most robust specification in Column 8 suggests that the difference in the probability of voting between high school dropouts and individuals with 12 or more years of schooling is 9 percentage points, or about a third of the corresponding coefficient in Column 4. These results are consistent with existing evidence that finds the effect of education on turnout diminishes among the registered (e.g., Highton, 1997).

Note that the information on registration is missing in some years, so that the sample used in Columns 1 to 4 is different from the sample used in Columns 5 to 8. To make sure that the documented difference in results is not driven by differences in the sample, we reestimate the models in Columns 1 to 4 using only the years when information on registration is available. We find results that are very similar to the ones reported in Columns 1 to 4.[22]

It is possible that our OLS estimates are biased by unobserved characteristics that are associated with schooling and outcomes. One potential solution to this problem is to find a set of instrumental variables that are related to voting only through their impact on schooling. We use mandatory schooling laws as instruments.[23] States changed their mandatory schooling laws at different times, generating variation across cohorts and

[22] For example, the coefficient on high school graduation for the model in Column 4 estimated using only the years when information on registration is available is 0.28 (0.13).

[23] This type of instrument has been used previously by Acemoglu and Angrist (2000) to study the social return to education, Lochner and Moretti (2001) to study crime, Lleras-Muney (2002a) to study adult mortality, Oreopoulos (2003) to study well-being, and in the study of labor market outcomes by Angrist and Kruger (1991), Harmon and Walker (1995), and Meghir and Palme (2003).

jurisdictions in exposure to the laws. If this variation leads to higher educational attainment, but is unrelated to citizenship outcomes, then mandatory schooling laws are valid instruments.

Years of compulsory attendance are defined as the maximum between (i) the minimum number of years that a child is required to stay in school and (ii) the difference between the earliest age that he is required to be in school and the latest age he is required to enroll. Child labor laws are defined as the earliest grade in which children are allowed to leave school to enter the labor market. In the years relevant for our sample, 1914–1990, states changed compulsory attendance levels and child labor laws several times, and not always upward.[24] We assign compulsory attendance laws and child labor laws to individuals based on state of residence at age 14 and the year when the individual was 14 years old.[25]

The effect of compulsory schooling laws and child labor laws on schooling is well documented (see, e.g., Acemoglu and Angrist (2000), Lochner and Moretti (2001) and Lleras-Muney (2002b)). Increases in compulsory schooling and in child labor laws have been shown to affect educational attainment, controlling for state and year of birth. Our first stage estimates are consistent with findings in the existing literature. The top panel in Table 3 quantifies the effect of compulsory attendance laws and child labor laws on educational achievement in the NES. For compulsory attendance laws, we create four indicator variables, depending on whether years of compulsory attendance are 8 or less, 9, 10, and 11 or 12. For child labor laws, we create four indicator variables, depending on whether the minimum number of years of school before work is permitted is 6 or less, 7, 8, and 9 or more. All models include controls for age, election year, state of birth, and year of birth.[26] The *F*-statistics for the exclusion of the set of instruments are reported beneath the coefficients.

Identification of the estimates comes from *changes* over time in the number of years of compulsory education or child labor laws in any given state. The identifying assumption is that conditional on state of birth, cohort of birth and election year, the timing of the changes in compulsory attendance laws within each state is orthogonal to characteristics of individuals that affect voting, like family background or tastes. Columns 1 to 3 indicate that, in general, the more stringent the compulsory attendance legislation or the child labor law legislation, the higher is the probability of high school graduation. For example, individuals who were 14 in states and years requiring 11 or more years of compulsory attendance, are 7.5 percentage points more likely to have at least high school compared

[24] The most dramatic examples of downward changes are South Carolina and Mississippi, who repealed their compulsory attendance statutes following the forced integration of schools in order to avoid requiring white children to attend racially mixed schools. Within the following decade, South Carolina reenacted a compulsory attendance statute, although it was weakened by provisions making the statute a mere enabling act which could be utilized at local option (Kotin and Aikman, 1980). See Lochner and Moretti (2001) for more details on changes in compulsory schooling laws.

[25] The data sources for compulsory attendance laws are given in Appendix B of Acemoglu and Angrist (2000). We use the same cut off points as Acemoglu and Angrist (2000) and Lochner and Moretti (2001) did. We experimented with a matching based on the year the individual is age 16 or 17, and found qualitatively similar results.

[26] More precisely, we include a dummy if the voter is female, a dummy if the respondent is black, a linear trend in year of birth, a four-term polynomial in age, dummies for each election year, and dummies for each state of birth. The standard errors we use allow for clustering at the state of birth and year of birth level.

Table 3
Estimates of the effect of education attainment on the probability of voting in the United States

	Full sample			Conditioning on registered to vote		
	(1)	(2)	(3)	(4)	(5)	(6)
First stage: dependent variable is high school graduation						
Compulsory schooling = 9	0.046		0.031	0.044		0.031
	(0.010)**		(0.011)**	(0.017)**		(0.019)**
Compulsory schooling = 10	0.020		0.007	0.041		0.035
	(0.014)		(0.015)	(0.026)		(0.026)
Compulsory schooling = 11 or 12	0.075		0.051	0.065		0.049
	(0.013)**		(0.015)**	(0.021)**		(0.023)**
Child labor = 7		0.034	0.014		0.039	0.025
		(0.012)**	(0.013)		(0.020)*	(0.022)
Child labor = 8		0.067	0.042		0.059	0.04
		(0.012)**	(0.014)**		(0.021)**	(0.024)**
Child labor = 9		0.085	0.052		0.064	0.038
		(0.014)**	(0.016)**		(0.026)**	(0.028)
F-statistic for exclusion of instruments	13.83	13.96	9.57	3.35	2.72	2.18
p-value	0.000	0.000	0.000	0.018	0.043	0.042
Second stage						
High school	0.296	0.305	0.288	0.179	0.281	0.188
	(0.138)*	(0.143)*	(0.120)*	(0.254)*	(0.249)	(0.207)
Black	− 0.008	− 0.070	− 0.009	− 0.009	− 0.008	− 0.009
	(0.021)	(0.022)**	(0.0190)	(0.031)	(0.034)	(0.032)
Female	− 0.041	− 0.042	− 0.041	− 0.038	− 0.034	− 0.033
	(0.005)**	(0.005)**	(0.005)**	(0.015)**	(0.015)**	(0.014)**
Observations	30,026	30,026	30,026	7387	7387	7387
Dependent variable mean	0.681	0.681	0.681	0.900	0.900	0.900

All regressions include individual survey year and state of birth fixed effects, as well as the year of birth and a quartic in age. Huber-White standard errors are shown with clustering by state and year of birth.

* Significant coefficient at the 5% level.

** Significant coefficient at the 1% level.

with individuals who were 14 in states/years requiring 8 years or less (the excluded case). The relationship between high school graduation and compulsory schooling is not perfectly monotonic, possibly because there are few individuals in the state years where compulsory schooling is equal to 10. Individuals who were 14 in states and years requiring 9 or more years of schooling before work are 8.5 percentage points more likely to have at least high school compared with individuals who were 14 in states/years requiring 6 years or less (the excluded case).[27] The instruments easily pass the *F*-tests for the full sample.

Are compulsory schooling laws valid instruments? We start to address this question by asking whether increases in compulsory schooling ages are associated with changes in political attitudes that may affect voter turnout. If increases in mandatory schooling correspond with increases in political participation, IV estimates might be too large. Similarly, changes in schooling laws may be correlated with civil rights changes that made

[27] Similar nonmonotonicities are found by Lleras-Muney (2002a).

registration easier. However, we do not believe this to be a serious problem. In contrast to most studies using state policy changes as an instrument, simultaneous changes in compulsory schooling laws and changes in political attitudes are not necessarily problematic for the instrument in this study because we examine voting behavior among individuals many years after they were subject to schooling laws. Recall that we assign compulsory attendance based on the year an individual is age 14, and our sample only includes individuals ages 20 and older. For the instrument to be invalid, changes in state political attitudes that take place when an individual is aged 14 must directly affect her voting behavior years later. In general, this does not appear to be a likely scenario.

Another important concern with using compulsory attendance laws as an instrument is that the cost of adopting more stringent versions of the laws may be lower for states that expect faster increases in high school graduation rates. It is, therefore, possible that changes in compulsory attendance laws simply reflect underlying state-specific trends in graduation rates. This issue has been extensively examined by previous research, which has shown that changes in compulsory schooling laws do not appear to be simply picking up underlying trends in education. Stricter compulsory attendance laws appear to raise education, not vice versa (see Lochner and Moretti (2001); Lleras-Muney (2002b)).

The bottom of Table 3 reports instrumental variable estimates of the effect of high school graduation on voting. Column 1 uses only compulsory schooling laws as instruments, Column 2 uses only child labor laws, and Column 3 uses both. Irrespective of the instruments used, the IV estimates are very similar to OLS estimates. The IV coefficient on high school graduation is between 0.288 and 0.305, statistically indistinguishable from the corresponding OLS coefficient. All our models report standard errors adjusted for clustering on state of birth and year at 14. If we use a more conservative stance and adjust standard errors for clustering on state of birth only, the standard errors are larger.[28]

Columns 4 to 6 report estimates for the sample of registered voters. Unfortunately, registration information was not recorded for many of the NES surveys, and the sample is therefore significantly smaller. IV estimates appear to be generally lower than the corresponding estimates for the full sample, but the large standard errors make it hard to draw firm conclusions. As well, the F-statistics are smaller, suggesting a weak first stage. In the Section 4.2.3, we show more precise results based on the larger sample available in the CPS. With the CPS, we can confirm that IV estimates for registered voters is much smaller than with the full sample. Finally, when we reestimate the models in Columns 1 to 3 using only the years when information on registration is available, we find results similar to the ones reported in Columns 1 to 3.[29]

[28] For the first three columns, the state of birth clustered standard errors for High School in the second stage are 0.180, 0.186, and 0.161 respectively. These suggest the coefficients are only marginally significant. In the CPS which we use below, results are still strongly significant with the more conservative assumption.

[29] We also reestimated our models separately for 1948–1974 and 1975–2000. OLS estimates are generally similar in the two periods: for the base model, they are 0.222 (0.008) in the earlier period, and 0.285 (0.008) in the later period. Unfortunately, IV estimates are not very well identified. In the earlier period, there simply is not enough variation to identify the first stage. The first stage F-statistic when we use compulsory schooling as instrument is 1.000, with a p-value of 0.39. We get similar results when we use both compulsory schooling and child labor laws as instruments. For the later period, the first stage is better identified. The F-statistic is 10.21, with a p-value of 0.000. The second stage coefficient on high school attainment is 0.212 (0.164).

4.2.2. Sensitivity checks

To assess whether there are state of residence-specific shocks that are driving our results, we estimate models that include state of residence effects and models that include state of residence times year effects. OLS and IV estimates are shown in Table 4. OLS estimates are slightly larger than the baseline. IV estimates are larger than the corresponding estimates which do not include these state of residence controls, and less precisely estimated.

Another concern in the interpretation of the IV estimates is the possibility of omitted variable bias. As discussed above, changes in compulsory schooling laws may coincide with changes in the state political environment. In Table 4, we report results for models that include controls for two variables that try to proxy for the political environment at the time of the law change. Specifically, we condition on the percent democratic vote in the presidential election and the voter turnout in state of birth when the respondent was 14. These results appear in the fourth row of the table. The point estimates appear to be slightly lower and less precisely estimated, but not significantly different from the corresponding estimates which do not include these additional controls.

In the last row of Table 4, we report results with a selected sample of whites only. Lleras-Muney (2002b) finds that there is no effect of compulsory schooling laws on the educational attainment of blacks, but her results focus on the first part of the century. Using more years, Lochner and Moretti (2001) show that there is a significant effect of CSL on blacks' education. Our results show weaker, less precise effects when we exclude nonwhites.

Finally, as a specification check on our first stage, we tested whether compulsory schooling laws affect the probability of college graduation. In theory, by increasing the probability of high school graduation, compulsory schooling laws could affect indirectly

Table 4
Robustness checks

	(1)	(2)	(3)	(4)
	OLS	Compulsory schooling IV	Child labor IV	Both laws IV
Baseline results	0.217 (0.005)***	0.296 (0.138)**	0.305 (0.143)**	0.288 (0.120)**
Control for state of residence	0.260 (0.007)***	0.430 (0.156)***	0.365 (0.156)***	0.349 (0.130)***
Control for state of residence times year	0.255 (0.007)***	0.411 (0.194)**	0.137 (0.164)	0.221 (0.144)
Include childhood political environment controls	0.257 (0.007)***	0.211 (0.145)	0.243 (0.142)*	0.217 (0.123)*
Include whites only	0.256 (0.006)***	0.208 (0.156)	0.196 (0.182)	0.194 (0.141)

Each cell reports the coefficient on the High School dummy. All regressions include individual survey year and state of birth fixed effects, as well as the year of birth and a quartic in age. Huber-White standard errors are shown with clustering by state and year of birth.

* Significant coefficient at the 10% level.

** Significant coefficient at the 5% level.

*** Significant coefficient at the 1% level.

the probability of college graduation by preserving the option to go to college. However, one would expect that the effects of changes in compulsory schooling laws on the probability of college attainment are smaller than the effect on the probability of high school graduation. Lochner and Moretti (2001) provide a detailed discussion of this issue. They show that increases in the number of years of compulsory attendance raise high school graduation rates but have small effects on college graduation rates. We have run similar regressions for our data sets and found similarly poor relationships between college education and compulsory schooling laws.[30]

4.2.3. Current Population Survey results

We now turn to an alternative data source, the Current Population Survey (CPS). The CPS has the advantage that its sample size is many times larger than the NES, and therefore can in theory produce more precise estimates. The main disadvantage of the CPS is that it does not report the state of residence at age 14 nor the state of birth. Consequently, we need to rely on the current state of residence to assign compulsory schooling laws and child labor laws. Because there is interstate mobility, this introduces error into our assignment of laws to our observations. In the NES, 28% of respondents currently reside in a different state than they did at age 14. To assess the consequences of the erroneous assignment, we tried assigning the laws to respondents in the NES based on their current state of residence. Results were similar. The *F*-statistics on our set of instruments are lower but still strong.[31]

Results based on the CPS are reported in Table 5, and in general confirm those based on NES data. Column 1 shows that OLS and IV estimates for the entire sample are, respectively, 0.29 and 0.34, only slightly larger than the corresponding NES estimates.[32] When we condition on the sample of registered voters, the IV coefficient drops sharply. The standard error here is much smaller, making this inference more precise than was the case for the NES results in Table 3. This suggests that a large part of the effect of education on voting happens through registration.

Differences in registration across education groups may reflect, at least in part, higher barriers to registration or ignorance of the system on the part of less educated citizens. Learning where to register and filling out the relevant forms could be problematic for the less educated. In this case, our results would suggest that lowering barriers to registration may reduce the effect of education on political participation. We should note, however, that empirical estimates of the effects of registration on different education groups by Nagler (1991) and Martinez and Hill (1999) show no evidence that more liberal registration regimes differentially increase registration by the low educated.

[30] Using the three compulsory schooling laws, the coefficients on 9, 10, and 11 or 12 years are -0.015, -0.034, and 0.009, respectively. For the CPS, the coefficients are -0.004, -0.008, and -0.003. These coefficients are very close to 0 compared to the observed coefficients in Tables 3 and 5.

[31] For example, using all 6 instruments in the full sample, we find an *F*-statistic of 7.5 (compared to 9.57 matching on the state at age 14). The estimated 2SLS coefficient on high school is 0.369 (0.133).

[32] We also tried many of the robustness checks with the CPS data, where feasible. Results were comparable to the NES robustness checks. Specifically, including the political environment variables has no effect on the results. Excluding nonwhites leads to a weaker first stage but similar point estimates.

Table 5
Estimates of the effect of education attainment on the Probability of Voting in the United States using the Current Population Survey

	Full sample		Conditioning on registered to vote	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
First stage: dependent variable is high school graduation				
Compulsory schooling = 9		0.020 (0.005)***		0.023 (0.005)***
Compulsory schooling = 10		0.025 (0.006)***		0.029 (0.006)***
Compulsory schooling = 11 or 12		0.051 (0.007)***		0.051 (0.007)***
Child labor = 7		− 0.017 (0.006)***		− 0.019 (− 0.006)***
Child labor = 8		0.000 (0.006)		− 0.005 (− 0.006)
Child labor = 9		0.003 (0.006)		− 0.001 (− 0.006)
F-statistic for exclusion of instruments		17.43		16.97
p-value		0.000		0.000
Second stage				
High school	0.273 (0.002)***	0.435 (0.054)***	0.135 (0.002)***	0.159 (0.041)***
Black	0.018 (0.003)***	0.041 (0.008)***	0.002 (0.002)	0.006 (0.007)
Female	− 0.006 (0.001)***	− 0.008 (0.002)***	− 0.015 (0.001)***	− 0.015 (0.001)***
Observations	948,699	948,699	715,477	715,477
Dependent variable mean	0.618	0.618	0.823	0.823

All regressions include individual survey year and state of birth fixed effects, as well as the year of birth and a quartic in age. Huber-White standard errors are shown with clustering by state and year of birth.

*Significant coefficient at the 10% level.

**Significant coefficient at the 5% level.

*** Significant coefficient at the 1% level.

4.3. Evidence from the UK

The US results seem to indicate that the positive effect of schooling on voting may be driven largely by the effect of schooling on registration. The IV results, in particular, imply that youth obliged to attain additional education are much more likely to register to vote, and somewhat more likely to vote after becoming registered. As discussed in Section 2, the responsibility of registering to vote in the United States, during the period when our data were collected, rests with the individual. In contrast, the responsibility in Britain rests with regional electoral officers who send letters and visit households in order to complete the electoral register. Given the substantial differences in the registration process between the two countries, we may also expect to see differences in our estimates of the effect from schooling on voting.

OLS estimates for Britain in [Column 1 in Table 6] indicate that an extra year of schooling has a small but significant effect on probability of voting. Controlling for

Table 6
OLS and IV estimates of the effect of education attainment on the probability of voting in the United Kingdom

	1997 Registered and nonregistered sample		Full sample conditioned on registered to vote	
	OLS	IV	OLS	IV
First stage: dependent variable is age left full-time education				
Dropout age 15		0.723 (0.330)*		0.512 (0.097)**
Dropout age 16		0.523 (0.357)		0.953 (0.185)**
Dependent variable mean		16.70		15.93
F-test statistic for whether dropout age coefficients are jointly zero		2.69		14.80
Second stage: dependent variable is whether voted in last general election				
Age left FT education	0.013 (0.004)**	0.060 (0.059)	0.010 (0.0014)**	− 0.008 (0.018)
Female	− 0.024 (0.015)	− 0.030 (0.014)*	− 0.008 (0.006)	− 0.009 (0.006)
Observations	3390	3390	17,825	17,825
Dependent variable mean	0.786	0.786	0.850	0.850

All regressions include individual fixed effects for survey year, a linear birth cohort trend, and a quartic in age. Huber-White standard errors are shown with clustering by year of birth.

* Significant coefficient at the 5% level.

** Significant coefficient at the 1% level.

registration in Column 3 of the table does not alter the estimates by much, which is not surprising considering most British are registered and that there exists little association between registration and education (see Table 1).

A comparison with US data is complicated by the fact that the UK data do not allow us to create a dummy for high school graduation. If we assume that in the United States, the difference in the number of years of schooling completed by those with less than high school and those with a high school degree or more is about 4 years, the coefficient for the United Kingdom appears to be six times smaller than the coefficient for the United States.

For the United Kingdom, we also investigate changes in compulsory school laws as possible instruments for education. Fig. 1 illustrates the remarkable influence the raising of the school-leaving age had on education attainment in Britain. A substantial fraction of children in Britain in the early 20th century left school as soon as possible. In 1935, when the school leaving age was 14, more than 60% of 14-year-olds left school. The 1944 Education Act legislated an increase of the minimum school leaving age to 15. After much concerted effort, Britain implemented the raise in 1947. Fig. 1 shows the fraction of 14-year-olds leaving schools at age 14 fell from 51% in 1946 to less than 10% 2 years later. The trend in the fraction of 15-year-olds leaving at 15 or less remains about the same before and after the 1947 change, suggesting those children that would have left school at age 14 before the law change still leave immediately after attaining the new minimum school-leaving age. The minimum age was raised again in 1972, from 15 to 16. While the trend in school attainment beyond the minimum required fell leading up to this time, the effect of this most recent change was nevertheless still significant, lowering the fraction of children at age 15 leaving school from about 30% in 1972 to less than 10% after 1973.

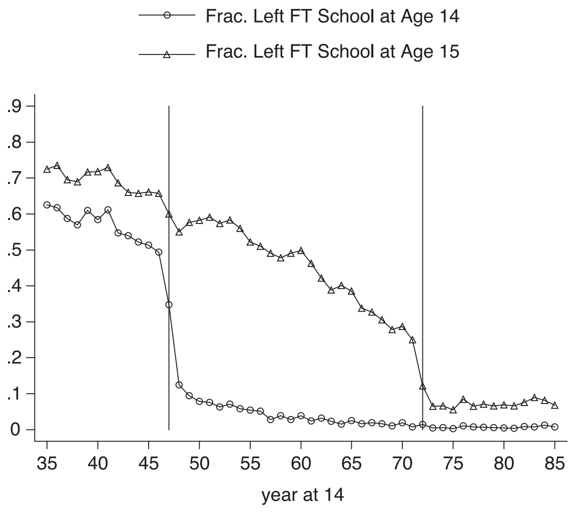

Fig. 1. Fraction left full-time education by year aged 14 and 15 in Great Britain. Notes: The lower line shows the proportion of British-born adults aged 16–65 from the 1973 to 1998 Eurobarometer Surveys who report leaving full-time education at, or before, age 14. The upper line shows the same, but for age 15.

Unlike the US data, where changes to compulsory schooling occurred at different times across different states, each birth cohort in the British Election Surveys faced the same school leaving age. We are therefore restricted in our analysis to examining differences in voting outcomes among cohorts before and after the law changes. We attempt to control for potential underlying time trends by adding linear birth cohort controls and a quartic in age over the 1920–1995 period analysis. We also conduct several robustness checks below that focus on the discontinuity in school attainment from the 1947 and 1973 changes. These checks help verify that the identification for the UK analysis comes from the time period exactly corresponding to the school leaving age changes.

Oreopoulos (2003) describes the history behind the UK school leaving age changes and examines additional validity checks with law changes in Northern Ireland, which occurred at different times than changes in Britain. We also use the Northern Ireland laws to examine the effects of compulsory schooling on other citizenship variables recorded in the Eurobarometers. Fig. 2 shows similar impacts from the Northern Ireland changes as those from the United Kingdom. Lack of political cooperation delayed the change in the school leaving age from 14 to 15 in Northern Ireland by 10 years. Fig. 2 shows the same sharp decline in the fraction of adults self-reporting they left school at age 14, but for 1957 rather than 1947. Northern Ireland also changed the school leaving age to 16 in 1973. With the inclusion of nation and birth cohort fixed effects, Oreopoulos (2003) shows the laws raised education attainment (and subsequent adult earnings, unemployment, health, and subjective well-being measures) for low-educated adults in the sample, but not for more educated, who were unlikely constrained by these laws. He also shows the effects are similar when estimating over shorter periods specifically around the 1947, 1957, or 1973 changes.

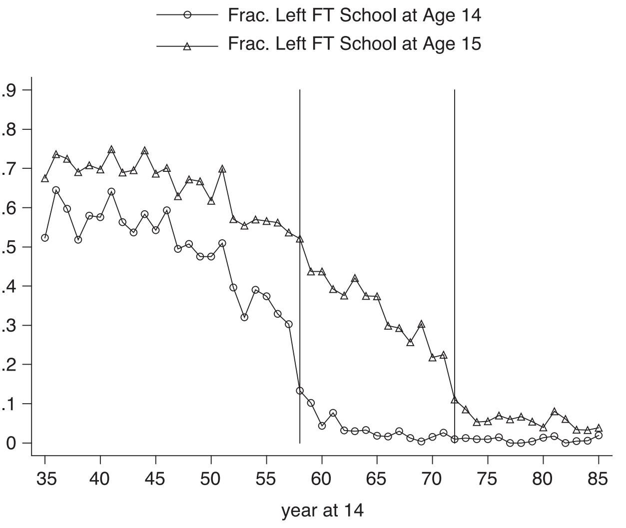

Fig. 2. Fraction left full-time education by year aged 14 and 15 in Northern Ireland. Notes: The lower line shows the proportion of Northern Irish adults aged 16–65 from the 1973 to 1978 Eurobarometer Surveys who report leaving full-time education at, or before, age 14. The upper line shows the same, but for age 15.

The second and fourth columns of Table 6 show the first stage effects of these laws on the age that adults in the BES left full-time education. The regressions include fixed effects for survey year and a quartic in age. We also add a linear birth cohort trend to control for possible cohort-specific changes in voting behavior. The sensitivity checks below show the same analysis, but only for individuals who were age 14 just before or just after the law changes.

The 1947 change in the school-leaving age, from 14 to 15, raised the average age before leaving by 0.512, as indicated in Column 4. Relative to those who faced a drop out age of 14, the coefficient of 0.953 on age 16 indicates British youth facing a minimum school leaving age of 16 attain almost a full year more of school, on the average. We reject the joint hypothesis, without difficulty, that the coefficients on the law changes are zero, as indicated by the *F*-statistic from this test of 14.8. However, the first-stage results using only the 1997 sample including both registered and nonregistered adults in Column 2, are less precise.

Similar to the OLS results, the IV estimates in Table 6 suggest a weak effect of schooling on voting in Britain. The nationally representative 1997 sample is too small to derive precise conclusions from the results, but the estimates from the full sample are also very small and insignificant.

4.3.1. Sensitivity checks

The law changes in the United Kingdom had a remarkably quick influence on education attainment, as indicated by Figs. 1 and 2. The analysis above, however, uses cohorts who were age 14 from 1925 to 1990. A trade-off exists between reducing the number of birth cohorts affected before and after the law changes and the precision of the estimated effects of education attainment on citizenship. In Table 7, the need for birth cohort controls is reduced by restricting the sample to birth cohorts a few years before and after the law changes.

The second column shows the estimated effect of 1 year of education on voting behavior for 14-year-old school leavers in 1946 and 15-year-old school leavers in 1949. The possibility that the 1947 law change explains virtually all the difference in schooling among these two groups seems plausible, given how many people were leaving at age 14 before the change. Yet Column 2 indicates no association between voting and education, after including controls for survey year, gender, and age. We do find a significant increase in the probability of reporting that an individual tries to persuade others to share her views, which is also what we find from the full sample IV results, discussed below.

The other columns show IV estimates of the effect from education on citizenship using different ranges of birth cohorts aged 14 around the law changes. For birth cohorts aged 14, 3 years before or after the 1947 change, we find no effect from education attainment on whether voted. We do find significant affects on the likelihood of trying to persuade friends and relatives for cohorts both around the 1947 change and the 1972 change.

To probe more deeply into the identification, we tried samples with only low-educated respondents and ones with high-educated respondents. We expect that the changes in school leaving laws will have a stronger effect on the outcomes of those who leave school early than on those who go on to higher education. The estimates for the effects of

Table 7
UK analysis with restricted samples

	OLS full sample (1925–1990)	OLS just school leavers	IV full sample (1925–1990)	IV (1944–1950)	IV (1969–1975)
Voted last election (self-reported)	0.010 (0.001)**	−0.004 (0.001)	0.001 (0.018)	−0.003 (0.004)	0.012 (0.038)
Number of grouped observations	17,892	330	17,892	1472	1311
Often try to persuade friends, relatives, coworkers to share views	0.029 (0.001)**	0.105 (0.008)*	0.066 (0.019)**	0.095 (0.063)	0.038 (0.040)
Number of grouped observations	25,298	173	25,298	2155	1542
Discuss political matters with friends at least occasionally	0.041 (0.001)**	0.022 (0.004)**	0.095 (0.026)**	0.013 (0.061)	0.001 (0.141)
Number of grouped observations	24,777	171	24,777	2108	1508

The regression on voting in the second column includes individual survey year and gender fixed effects, and a linear age control. The other regressions on voting include individual fixed effects for survey year and gender, a linear birth cohort trend, and a quartic in age. Huber-White standard errors are shown with clustering by year of birth. The other regressions include fixed effects for survey year, gender, birth cohort, and region (UK or N. Ireland), and a quartic in age.

* Significant coefficient at the 5% level.

** Significant coefficient at the 1% level.

schooling on voting were measured imprecisely, but generally showed the expected pattern. In other work using the same instruments, Oreopoulos (2003) shows this validation check holds quite well using larger data sets that examine the effects of education on other social-economic variables.

5. The effect of education on citizenship outcomes

The empirical analysis so far has focused on participation in the political system as measured by the probability of voting. In this section, we extend our analysis to other measures of citizenship outcomes. Our motivation is to provide credible causal evidence into other potential socially beneficial externalities that may arise with a more educated population. For example, we look at whether and how voters obtain information about the candidates. One important potential channel through which education may improve citizenship is by raising citizens' ability and interest in obtaining information about candidates and campaigns. Another potential channel through which education may improve citizenship is by increasing citizens' involvement in community issues and their participation in community meetings.

The NES provides information on two sets of citizenship outcomes for the United States. First, respondents were asked questions on actions that they have taken, such as whether they have followed the campaign on newspapers or television, or whether they regularly attend community meetings. Second, the NES collects more subjective data on respondent attitudes about the political system.

The top panel of Table 8 reports OLS and IV estimates of the effect of high school graduation on action outcomes. Because not all the questions were asked in all years, the sample size varies considerably across outcomes. Consequently, IV estimates in some cases are not informative because there are simply too few observations for the first stage to be effective.[33]

Generally, the effect of education on these outcomes is to improve citizenship, when citizenship is measured by the available action outcomes. High school graduates, relative to dropouts, are more likely to be registered, follow campaigns on television or newspapers, follow public affairs, attend political meetings, volunteer for community issues and attend community meetings. In turn, all of these activities are correlated with voting.[34]

The bottom panel of Table 8 focuses on more subjective measures of the respondents' attitudes. More educated individuals are more likely to report that they are interested in elections, they don't mind jury duty, that they are more likely to trust the Federal government and less likely to think that Federal officials are crooked. We also ran regressions with the 'environment' controls used in the regressions in Table 4. The results were quite similar.

[33] We also tried controlling for current state of residence and for the political environment when young for these outcomes. The results changed little.

[34] We ran regressions of each of these outcomes on a dummy variable for voting. We find a strong, significant and positive effect in each case.

Table 8
The effect of education attainment on social and citizenship outcomes in the United States

	Mean	OLS	IV	Number of observations
Self-reported action outcomes				
Registered to vote	0.82	0.187 (0.005)***	0.093 (0.097)	20,328
Follow campaign on TV	0.79	0.087 (0.006)***	0.392 (0.116)***	23,179
Follow campaign on newspapers	0.66	0.268 (0.006)***	0.852 (0.139)***	25,301
Follow public affairs	0.66	0.237 (0.007)***	0.544 (0.126)***	25,500
Attend political meeting	0.07	0.064 (0.003)***	0.132 (0.074)*	20,328
Work on community issues	0.25	0.171 (0.019)***	− 0.036 (0.751)	3855
Attend community meetings	0.30	0.235 (0.049)***	− 1.000 (0.821)	1024
Self-reported subjective outcomes				
Interested in election	0.30	0.166 (0.006)***	0.270 (0.132)**	30,199
Does not mind jury duty	0.59	0.183 (0.022)***	1.510 (1.490)	3821
Trust federal government	0.40	0.050 (0.007)***	0.353 (0.159)	25,136
Trust people	0.50	0.231 (0.010)***	0.330 (0.197)*	12,007
Federal officials are crooked	0.40	− 0.051 (0.008)***	− 0.175 (0.176)	22,304

All regressions include gender, race, individual survey year and state of birth fixed effects, as well as the year of birth and a quartic in age. Huber-White standard errors are shown with clustering by state and year of birth.

* Significant coefficient at the 10% level.

** Significant coefficient at the 5% level.

*** Significant coefficient at the 1% level.

Table 9 uses the combined Eurobarometer surveys to estimate similar effects of schooling on citizenship outcomes for the United Kingdom (with samples from both Britain and Northern Ireland). As with the BES results above, we find no relationship between registration and schooling. Among respondents, 92% say their name is on the electoral list for the next general election. The OLS and IV estimates of the effect of age left full-time education are insignificantly different from zero.

Least-squares estimates indicate a small association with more schooling and greater likelihood of watching news in the media. Four additional years of school, for example raises the probability of watching news every day by about 3 percentage points. The instrumental variable results are not only insignificantly different from 0, but also insignificantly different from the least squares results.

All 50 Eurobarometers ask questions about whether respondents discuss politics, try to persuade people of their views, and consider themselves politically active. We find strong effects of schooling on all these variables. For example, those compelled to take an extra year of school because the minimum school leaving age was raised, are about 7 percentage points more likely to report they try to persuade others to share their views, 6 percentage points more likely to frequently discuss political matters with friends, and 3 percentage points more likely to consider themselves politically active.

These results suggest that education improves participation not only as measured by voter turnout, but also in broader measures. As well, the evidence on education and political information may provide support for models that focus on the lower cost of information acquisition for the more highly educated. However, it may also be the case

Table 9
The effect of education attainment on social and citizenship outcomes in the United Kingdom

	Mean	OLS	IV	Number of observations
Self-reported action outcomes				
Name on electoral list for next general election	0.92 0.001	0.000	− 0.014 0.009	36,490
Follow news everyday from TV, newspaper, or radio	0.90	0.007 (0.001)**	− 0.007 (0.049)	22,935
Seldom or never follow news from TV, newspaper, or radio	0.01	− 0.002 (0.0003)**	0.000 (0.017)	22,935
Often try to persuade friends, relatives, coworkers to share views	0.12	0.007 (0.0007)**	0.011 (0.015)	63,858
Never discuss opinions or persuade others to share views	0.24	− 0.041 (0.001)**	− 0.095 (0.025)**	62,310
Often or from time to time try to persuade friends, relatives, coworkers to share views	0.45	0.029 (0.001)**	0.066 (0.019)**	63,858
Discuss political matters with friends frequently	0.15	0.020 (0.001)**	0.066 (0.018)**	62,527
Discuss political matters with friends at least occasionally	0.67	0.041 (0.001)**	0.095 (0.026)**	62,527
Never discuss political matters with friends	0.33	− 0.041 (0.001)**	− 0.092 (0.025)**	62,527
Self-reported subjective outcomes				
Consider oneself politically active	0.10	0.002 (0.001)**	0.033 (0.014)*	62,310
Consider oneself to be fairly close or very close to one party	0.29	0.016 (0.001)**	0.012 (0.030)	41,721
Satisfied with the way democracy works	0.54	0.013 (0.001)**	0.009 (− 0.020)	44,174
Give people more say in important government decisions	0.50	− 0.003 (0.001)*	− 0.002 (0.022)	48,406

All regressions include gender, individual survey year and region fixed effects, as well as the year of birth and a quartic in age. Huber-White standard errors are shown with clustering by region and year of birth.

* Significant coefficient at the 5% level.

** Significant coefficient at the 1% level.

that voters who know they will not vote do not bother investing in the acquisition of political information. We leave further investigation of the channels through which education affects participation to future research.

6. Discussion

We find a strong and robust relationship between education and voting in the United States, but not in the United Kingdom. When the US sample is restricted only to citizens who are registered, the estimated effect of education on voting drops to less than a third of the effect for the full sample. In addition, our evidence on broader outcomes indicates that education increases citizens' attention to public affairs and to following politics. More educated citizens appear to have more information on candidates and campaigns. We find similar results across both countries. Overall, these results lend support to the notion that education has social externalities through the production of a better polity.

Our results on registration suggest an interesting counterfactual—what would happen if the registration regime in the United States were changed to resemble that of the United Kingdom?[35] The answer depends on which of two distinct cases holds. On one hand, if citizens don't care about the costs of registration or they do not suffer from procrastination, then anyone who plans to vote will register. In this case, registration is a veil. Changes in the registration regime would have no impact on the education gradient of voting.

On the other hand, if costs matter or if procrastination is a concern, then a liberalization of registration could increase voting. To the extent that the registration barrier disproportionately affects the low educated, liberalizations of the registration regime could increase the turnout of the low educated and flatten the education gradient. As the empirical literature on the effects of registration liberalization is mixed, we cannot draw any strong conclusions.

However, holding the existing US registration regime constant, our results have clear implications for citizenship behavior under our main counterfactual of interest. Our estimates suggest that an increase in educational attainment causes an increase in voter turnout in the United States, but not in the United Kingdom. For the United States, the magnitude for a high school graduate on the self-reported probability of voting is on the order of 28.8% to 34.2 percentage points. Given that the high school attainment rate among those 25 and older increased by 36.1 percentage points from 1964 to 2000 (US Census Bureau, 2000), our estimates suggest that the 2000 turnout rate would have been 10.4% to 12.3 percentage points lower if the high school completion rate had not changed from 1964, holding all other factors constant. In other words, we predict that the observed drop in the turnout rate would have been even sharper, if it were not for the large observed increase in high school attainment between 1964 and 2000.

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[35] Patterson (2002) asserts (p. 133) that liberalized registration would be the "single most important step" that could be taken to improve turnout.

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