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**Schooling and Voter Turnout
Is there an American Exception?**

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SCHOOLING AND VOTER TURNOUT
Is there an American Exception?

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Abstract
One of the most consistent findings in studies of electoral behaviour is that individuals with higher education have a greater propensity to vote. The nature of this relationship is much debated, with US studies generally finding evidence of a causal relationship, while European studies generally reporting no causal effect. To assess whether the US is an exception we rely on an international dataset incorporating 38 countries, the ISSP (International Social Survey Programme) from 1985 to 2010. Both instrumental variable and multi-level modelling approaches reveal that the US is an outlier regarding the relationship between education and voter turnout. Moreover country-specific institutional and economic factors do not explain the heterogeneity in the relationship of interest. Alternatively, we show that disenfranchisement laws in the U.S. mediates the effect of education on voter turnout, such that the education gradient in voting is greater in U.S. States with the harshest disenfranchisement legislature. As such, the observed relationship between education and voting is partly driven by the effect of education on crime.

Keywords: Voter turnout, Education, Disenfranchisement laws

JEL: D72, I20, K42

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1. Introduction

There is a broad consensus in the political and economic literature that education is positively correlated with a range of civic and pro-social behaviours (Putnam, 2000). More specifically, a host of studies find that education is positively associated with voter turnout[1] (Campbell,Converse, Stokes and Miller, 1960; Verba, Schlozman, and Brady 1995; Rosenstone and Hansen 1993). The tenets of this relationship was formalised in the modernization hypothesis, with Lipset (1959, p79) stating that "education presumably broadens men's outlooks, enables them to understand the need for norms of tolerance, restrains them from adhering to extremist and monistic doctrines and increases their capacity to make rational electoral choices"[2]. The relationship between education and civic behaviour is usually advocated as a positive return to education. Indeed, "[this relationship] provides some of the most important justifications for government intervention in the market for education" (Dee, 2004).

However, the relationship between education and political behaviour may be spurious, and driven by unobserved characteristics correlated both with educational attainment and voter turnout. U.S. studies (Sondheimer and Green, 2010; Dee, 2004; Milligan, Moreti, and Oreopoulos, 2004) generally support the modernisation hypothesis and continue to identify a positive relationship between education and turnout. On the

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[1] Indeed the relationship between education and turnout, which was first identified in the 1920's (Merrian and Gosnell, 1924), transcends election type e.g. transnational, national or regional elections, and form of data collection e.g. self reported or administrative data.

[2] At the macro level, proponents of the modernist theory argue that education is a contributor to democratisation (Huntington, 1991), yet the empirical evidence is still controversial. Studies by Barro (1999) and Glaeser, Ponzetto, and Shleifer (2007) using cross country panel evidence were dismissed by Acemoglu,Johnson, Robinson, and Yared (2005) as not robust to specification changes. However, new evidence by Bobba and Coviello (2007) and Castello-Climent (2008) using a GMM estimator report positive and statistically significant effects of education on democracy.

contrary, most analyses using Western European data find that the impact of education on voter turnout disappears when correcting for endogeneity bias (Siedler, 2010, Pelkonen, 2010; Borgonovi, d'Hombres, and Hoskins, 2010)[3]. Whether this difference represents genuine differences in the relationship between education and voting in the U.S. and Europe, or are due to data or methodology differences, is the first question of interest in this study[4].

To address this question, we use comparable data from 38 countries by pooling 25 waves of the International Social Science Program (1985-2009) data. We adopt an Instrumental Variable (IV) strategy to deal with the potential endogeneity of education using changes in the minimum school leaving age (SLA) as an instrument. We find that the causal relationship is only present in samples which include the U.S., thus suggesting that the U.S. is an exception. . A multi-level analysis also confirms that the U.S. is an outlier.

Secondly, we assess the potential drivers of this country heterogeneity. We find that the random slope coefficient is a function of regime duration, time since last election, whether voting is compulsory and to some extent ease of voters' registration. However, none of these factors explain the American exception.

Our third contribution is to assess the role of the judicial system in the U.S. in mediating the relationship between education and voter turnout. The U.S. incarcerates a larger fraction of its population than most countries and most States to some extent

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[3] Similarly, Devereux and Hart (2010) note that U.S. studies relying on changes in school leaving age to identify causal effect of education tend to report large positive financial returns to education, while European-based studies estimate, at most, low financial returns.
[4] Other cross country comparisons include Milligan et al. (2004) who provide results for the U.S., Canada and the U.K., however they use separate dataset for each country, and only identify a positive effect in the U.S. In addition, Borgonovi et al., (2010) uses the European Social Survey and thus is not informative on whether the relationship differs between Europe and the U.S.

disenfranchise felons and ex-felons. As more educated individuals are less likely to be convicted, and thus lose their voting rights, we investigate whether the relationship between education and voting observed in the U.S. is driven by disenfranchisement laws. Using State-level data on voter turnout for the period 1988-2010, we report that indeed the education gradient on voting is greater in States with the harshest disenfranchisement laws. Thus, disenfranchisement laws rather than the modernisation theory, may explain the positive relationship between education and voting. This has important consequences on the computation of the social returns to education.

The remainder of the paper proceeds as follows: Section 2 describes related literature on the potential mediators of the relationship between education and turnout and discusses the U.S. and European studies which attempt to test the causal hypothesis. Section 3 and 4 present the data and methodology respectively. Section 5 presents the results of the various analyses. Finally, section 6 concludes.

2. Related Literature

Multiple causal mechanisms have been proposed to account for the relationship between education and political behaviour. Education improves general knowledge and cognitive skills which may facilitate a better understanding of the political system and voting process including registration (Luskin, 1990; Neuman, 1996; Hauser, 2000, Milligan et al., 2004). It may result in an increased interest in politics and sense of civic duty through improved civic skills and an understanding of political history (Verba, Schlozman, and Brady, 1995; Wolfinger and Rosenstone, 1980; Rosenstone and Hansen, 1993). Greater education can also advance one's social position through better job opportunities and increased earnings leading to better political connections and a greater

stake in election outcomes (Nie, Junn, and Stehlik-Barry 1996; Campbell, 2009). Education may also have a negative impact on voting since it increases the opportunity costs associated with political activity (Verba et al., 1985). Indeed, these costs could lead to non-linearity in the relationship between education and voting turnout (Campante and Chor, 2011).

However, the relationship between education and voting may not be causal. An individual may possess higher education and a greater propensity to vote due to intrinsic characteristics that are not directly observable or easily measureable. For example, personality traits are associated both with educational attainment (Heckman, Stixrud, Urzua, 2006) and voting behaviour (Denny and Doyle, 2008; Mondak and Halperin, 2008). Specifically, individuals with low time preferences may be more likely to stay in school (Mischel, Shoda, and Rodriquez, 1989; Castillo, Ferraro, Jordan, Petrie, 2011) and have a greater propensity of vote (Fowler and Kam, 2006). Similarly, a relationship has been identified between IQ and voter turnout (Deary, Batty and Kale, 2008; Denny and Doyle, 2008), while IQ has long been identified as a predictor of educational attainment. In addition, a number of studies have identified an intergenerational transmission of political values and attitudes between parents and children (Beck and Jennings, 1982; Sears and Carolyn, 1999; McIntosh, Hart, and Youniss, 2007), such that parental voting behaviour, parental values, and early childhood experiences (Sondheimer and Green, 2010) may influence both educational attainment and voter turnout. Siedler (2011), for example, identifies an effect of parental unemployment on the child's subsequent extreme right sympathies. In addition, a new body of research on genopolitics, finds that individual biological differences may interact with the environment to influence political

behaviour such as turnout (Fowler, Baker, and Dawes, 2008; Fowler and Dawes, 2008; Dawes and Fowler, 2009).

The issue of the possible endogeneity of education in voting models first came to prominence with the publication of two papers by Dee (2004) and Milligan et al. (2004), which identify the causal effect of education on political participation using an instrumental variable (IV) approach.

Milligan et al. (2004), in a comparison of the US and the U.K., uses changes in compulsory schooling and child labour laws across regions and time, to identify a strong causal effect of education on voter turnout in the US. Graduating from high school increases the probability of voting by 20 to 30 percentage points in the U.S., while no significant effect of age left school is found in the U.K. The authors provide some evidence that the difference between the two countries may originate from the electoral registration process being less burdensome in the U.K. Similarly, Dee (2004) uses state-wide variation in child labour laws and distance to college as instruments for educational attainment, and finds that college attendance increases turnout by 4 to 7 percentage points. Additionally, both papers highlight that more educated individuals are more politically informed.

More recent studies for the U.S. have reported mixed conclusions regarding the causality of the relationship. Using the rise in education induced by the Vietnam draft, Berinsky and Lenz (2011) do not find a significant effect. Hill (2010), using quarter of birth as an instrument, finds that the relationship between education and turnout is much smaller than previous estimated[5]. Kam and Palmer (2008) also find that voting does not

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[5] However, the validly of using quarter of birth as an instrument for education has been previously called into question (Bound, Jaeger, and Baker, 1995).

differ by college attendance, once controlling for non-random selection into education using data on pre-adult experiences and parental influences in a propensity score matching estimate. Henderson and Chatfield (2009) test the validity of Kam and Palmer's findings (2008) using an alternative form of matching called genetic matching. They find that the results of Kam and Palmer are highly sensitive to the variables used in the matching process and while genetic matching improves the balancing properties, they conclude that the casual effect of education and turnout remains elusive. Tenn (2007) exploits the panel structure of the US Current Population Survey to conduct a difference-in-difference analysis restricted to those still in education. It finds that conditional on being a student, an additional year of education has little impact on voter turnout, but this is only identified for a population of students. Finally, Sondheimer and Green (2010) use data from two randomised controlled trials (Perry Preschool and STAR programmes) and one quasi-experimental programme (I Have a Dream (IHAD) programme), which exogenously changed the high school graduation rates of the treated individuals, and find that after pooling the three experimental results, education has a statistically significant effect on voting[6]. While these papers cast doubts on the original findings of Milligan et al., (2004) and Dee (2004), their identification strategies are not always as clean as the one used in these two papers. Moreover, the randomized controlled trial results, which have the cleanest identification strategy, confirm the causality of the educational gap in voting in the U.S.

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[6] It should be noted that when each RCT is analysed separately, the education effect is not significant mostly due to small sample size. Similarly, Friedman, Kremer, Miguel, Thornton. (2011) assess the effect of a randomised control trial which aimed to increase access to secondary education in Kenya, and report that while political knowledge was increased, this did not lead to more pro-democratic attitudes nor greater voting intentions.

European studies, in the line of Dee (2004) or Milligan et al. (2004), which exploit schooling reforms to identify education effects on voter turnout have been conducted for Germany (Siedler, 2010), Italy (Di Pietro and Delprato, 2009), Spain (Touya, 2006), and Norway (Pelkonen, 2010). Overall, they find that the significant relationship identified in the OLS models does not remain in the IV models. However, it is unclear whether the difference between studies is due to "true" discrepancies in the relationship between the U.S. and Europe, or to data and methodological disparities.

There have also been a number of comparative studies examining the relationship between education and political participation (see for example, Nevitte, Blais, Gidengil and Nadeau, 2009; Campante and Chor, 2011). However, Borgonovi et al. (2010) is the only comparative study to date that uses an instrumental variable. Using the European Social Survey and compulsory schooling laws within each country as instruments, they report that education has an effect on political informational acquisition, yet no effect on voter turnout in the IV models. However, since this study omits the U.S. it is not informative to answer our first question of interest.

There is also a large literature examining the determinants of voter turnout across countries. These studies focus on institutional differences and contextual factors across countries and find that factors such as the ease of voter registration, features of the electoral system such as compulsory voting laws, unicameralism, electoral disproportionality, and the number and strength of parties, as well as the socioeconomic environment, play a role in explaining cross country differences in voter turnout (see Blais, 2006 for a review).

3. Data

The main source of data is 25 waves of the International Social Survey Programme (ISSP) conducted between 1985 and 2009 in a varying set of countries. The ISSP began in 1985 and is a cross-national collaboration of countries conducting identical annual surveys, often embedded in other national surveys, on areas of importance for social science research[7]. We use ISSP surveys conducted between 1985 and 2009 which include information on the voting behaviour of 404,480 respondents in 45 countries.

The voting data is a self-reported question based on whether the respondent voted at the last general/presidential election[8]. It is coded as one for individuals reporting the party of their vote, do not disclose the party of their vote, or could not remember which party they voted for, and zero for those reporting not to have voted[9]. This variable is asked in all ISSP waves with the exception of years 1994, 1995, 1998, 2000 and 2001, but it is not available for all countries in all years. As such, our sample contains voting data for 45 countries and 294 country/year, see Table A2 for details.

As specified above, the identification strategy is based on reforms of the compulsory school leaving age (SLA). This identification strategy has been used extensively to evaluate the causal effect of education on various outcomes including voting behaviour (e.g. Milligan et al. 2004; Borgonovi et al. 2010). We used various

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[7] Further details of data collection, sampling, and response rates can be found on the ISSP website (http://www.issp.org).

[8] See Table A1 for a sample of the question wording.

[9] 0.6% of individuals have missing information on this variable and are dropped from the analysis. Thus, there is little selection into answering this question. Of course, individuals may not report their true voting behaviour. Indeed our computed self-reported measure of voting is almost always larger than the official turnout rate. Most of the literature reports no effect of education on mis-reporting of voting behaviour, yet Anderson and Silver (1986) report that in a small extract of the 1980 National Election Study, mis-reporting from non-voters increases with education in the U.S. However, Milligan et al (2004) reject that the education gradient is driven by differences in misreporting by education level.

sources to obtain information on the school leaving age reforms in the countries of interest and identify schooling age legislation for 38 countries representing 86% of the individuals with valid voting information (Table A3 details the sources of educational reforms). There is a large amount of variation in SLA between countries, however to be credible our estimates require variation within country over time. Indeed, almost all countries for which we could consistently identify the school leaving age have experienced at least one reform affecting a cohort observed in our dataset. For countries with regional variation in SLA we use the individual's current location of residence to allocate SLA when possible[10] or the years in which the majority of the country was affected by a change in SLA. This means that there is potentially some measurement error in SLA which would weaken our instruments, yet it would not lead to any biases in the results.

We further restrict the sample to individuals aged 25 to 70. The age restrictions are imposed to select individuals who have completed education and are not too old, since survival in older ages is correlated with education (Lleras-Muney, 2005)[11]. Education is self-reported and is based on responses to different education questions asked in each ISSP country. The education data was then standardised by ISSP to represent the number of years of full time education the individual received. We exclude individuals with more than 25 years of education and those still in education[12]. As such,

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[10] For the US, only information at the level of the census region is available. We compute the population weighted average SLA for each region by cohort, and allocate individuals to their region of current residency.
[11] Including individual up to age 100 do not substantially affect our results but 2SLS results become less precise.
[12] Just over 5% of the sample reports five years of less of education, excluding these individuals does not substantially affect our results but 2SLS results become less precise.

our final sample contains 253,490 observations, in 38 countries, from 250 country/years[13].

Figure 1 reports a polynomial fit between de-trended education and voting. To account for the differences in level and trends in educational attainment across countries and time, we use the residuals from a regression of educational years on a quadratic in birth cohort interacted with country, as well as country dummies. Similarly, to account for potential variations in voting behaviour over time and between countries, the voting measure in this graph is the residual from a regression of self-reported voting on a country specific linear trend (in survey years). The graph is truncated to observations with education residuals in the -8, +8 range; i.e. individuals who have plus or minus eight years of education compared to their country specific cohort average. The graph display a mostly linear relationship between education and voting, where each year of additional education, compared to the country specific cohort, increases voting by 2.5 percentage points.

The ISSP contains the following covariates which are standard in the voting literature: age (Highton and Wolfinger, 2001; Franklin, Lyons and Marsh, 2004), gender (Pattie and Johnson, 2001), marital status (Denny and Doyle, 2008), number of individuals in the household, trade union membership (Gray and Caul, 2000), urbanicity (Lipset, 1987), and whether the respondent regularly practices religion (Gerber, Gruber and Hungerman, 2008). Table 1 displays descriptive statistics for the variables of interest.

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[13] The countries include Argentina, Australia, Austria, Belgium (Flanders), Canada, Chile, China, Croatia, Cyprus, Czech Republic, Denmark, Dominican Republic, Finland, France, Germany, Hungary, Ireland, Israel, Italy, Japan, South Korea, Mexico, Netherlands, New Zealand, Norway, Philippines, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, Taiwan, Turkey, U.K., U.S., Uruguay, Venezuela.

For each potential covariate, the largest share of the variation comes from within country variation.

Institutional data

For the second part of our analysis, we supplement our data with country specific institutional and contextual factors that may mediate the relationship between education and turnout. Political freedom is measured using two Freedom House democracy indicators including the degree of civil liberties and political rights on a scale of 1-7 where higher values indicate less political freedom (Freedom House, 2011)[14]. Regime durability, which represents the number of years since the most recent regime change, and the openness of the political institutions, measured on a scale of 0-10, where higher scores indicate greater openness, are extracted from the Polity IV project data (Marshall and Jaggers, 2009). Economic freedom is measured using the Fraser Institute's summary index of five economic indicators including size of government; legal structure and security of property rights; access to sound money; freedom to trade internationally; and regulation of credit, labour, and business whereby higher scores indicate greater freedom (Gwartney, Lawson and Hall, 2011). We also include an indicator of whether the country is subject to compulsory voting, the time since the last general election, and the proportion of the voting age population who is registered to vote. This information was extracted from the International Institute for Democracy and Electoral Assistance 'Voter Turnout' Dataset and the latter will be used as a proxy for easiness of voter's registration. Finally, we include economic variables, GDP growth, inflation and unemployment rate

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[14] In our sample, these distributions are truncated to scales from 1 to 4 and 1 to 5 respectively, as there are no countries in our sample with higher Freedom House scores. Similarly, political openness only ranks from 7 to 10 in the data.

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measured at the time of the election[15]. These variables were extracted from the World

Bank database (World Development Indicators, 2011).

4. Methodology

First, the impact of education on voter turnout is estimated using the following

model:

$$V_{ijt} = S_{ijt}\beta + X_{ijt}\alpha + B_i\varphi + C_j\gamma + Y_t\chi + C_jB_i\lambda + \varepsilon_{ijt} \qquad (1)$$

where the subscript *ijt* indicates an individual *i* in country *j* at period *t*. V_{ijt} is a binary

variable indicating whether the respondent voted in the last election. Voter turnout is a

function of education, S_{ijt}, and X_{ijt} which contains the additional individual characteristics

mentioned above. B_i is a linear or quadratic trend in birth cohort. C_j and Y_t represent a set

of regional and year of survey dummies respectively. In our preferred specifications, we

also include an interaction between birth cohort and country (C_jB_i) in order to account for

country-specific trends in education, but in some specification we set $\lambda = 0$.

To facilitate direct comparisons with the two-stage least square (2SLS) model,

this equation is estimated as a linear probability model[16]. Since observations from a given

country/year are unlikely to be independent, standard errors are adjusted for clustering at

the country/year level.

As discussed above, β may be biased if some unobservable characteristics

correlated with education and voting are not included in the model. As such, we estimate

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[15] For elections taking place before 30th June, the economic data refers to year *t-1*, for elections taking place after 1st July, we use year *t* data.
[16] Estimating the model with probit, accounting or not for the endogeneity of education, leads to similar conclusions.

the model using a two stage least square strategy, where S is replaced by $\hat{S}$, the prediction from the following first stage equation, where the exclusion variable is the minimum school leaving age implemented in country j for the birth cohort of individual i (Z_{ij})

$$S_{ijt} = Z_{ij}\beta_1 + X_{ijt}\alpha_1 + B_i\varphi_1 + C_j\gamma_1 + Y_t\chi_1 + C_jB_i\lambda_1 + \mu_{ijt} \tag{2}$$

As such, the identification comes from variation over time in the minimum school leaving age within country. It is important to note that we include country specific trends to capture secular increase in educational attainment, such that the instrument captures deviation from the country specific educational trends.

In the second part of the analysis, and to capture the potential heterogeneity in the effect of education on voting, we estimate multi-level models which accounts for the between country variance in the effect of education. First, we modelled a random intercept model, by including a country specific random estimator (ς_j)[17]:

$$V_{ijt} = S_{ijt}\beta + X_{ijt}\alpha + B_i\varphi_1 + C_j\gamma_1 + Y_t\chi_1 + \varsigma_j + \varepsilon'_{ijt} \tag{3}$$

Finally, voter turnout is estimated using a random coefficient specification. We thus assume that the relationship between education and voting is specific to each country j.

$$V_{ijt} = S_{ijt}\beta_j + B_i\varphi_1 + C_j\gamma_1 + Y_t\chi_1 + Y_t\chi + \varepsilon''_{ijt} \tag{4}$$

The country specific estimates of the intercept and slope in (4) are then used to test whether the relationship between education and voting is affected by institutional factors. Hence we estimate the following:

$$\hat{\beta}_j = W_{jt}\delta + \mu_{jt} \tag{5}$$

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[17] For computational ease, the country specific trends are dropped

Where W_{jt} is a vector of institutional and economic factors discussed above. Standard errors from this model are obtained by bootstrap of equation (4) and (5) with 500 replications.

5.   Results

A – Results of OLS and 2SLS

The OLS estimates are presented for different specifications in the top panel of Table 2. Following the results from Figure 1, the effect of education on voting is assumed to be linear[18]. We control for region fixed effects (within countries) so as to capture potential heterogeneity in turnout within countries[19]. The first specification imposes a common linear birth cohort effect for all countries; it assumes that the effect of a birth cohort on voting is identical for all the participating countries. It shows that one year of education increases the voting probability by just over two percentage points.

The bottom panel reports the 2SLS estimates for each specification[20]. The school leaving age (SLA) is highly significant in the first stage, with each year of compulsory schooling adding 0.16 years of education on average. The F-test on this parameter is 26 which is well above the critical values for weak instruments (Stock and Yogo, 2005). The 2SLS estimate of education on voting increases to 5 percentage points and is precisely estimated.

In the second column, we relax the assumption that the cohort trends are identical between countries and instead allow for country specific linear cohort trends. The OLS

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[18] The results are not sensitive to the exclusion of outliers; i.e. observations with education residuals outside the -8, +8 range. Results excluding these observations are available on request.
[19] Results including country fixed effects are slightly larger than those presented and more precisely estimated.
[20] The models are estimated using the ivreg2 module in Stata written by Baum et al. (2007).

estimates are identical to those obtained when imposing common cohort effects. However, controlling for country specific trends makes it more credible that the SLA dummy captures the impact of the reform on the educational attainment of the affected cohorts, rather than just the secular trend in educational attainment; as such, this is our preferred specification. Indeed, the point estimate in the first stage is reduced by about 50%, such that compulsory education is associated with an increase in education of 0.075 years only, yet it is precisely estimated and the F-tests reaches 43. We thus reject that the instrument is weak. The 2SLS estimate is larger than in the previous specification and reaches 5.9 percentage points, yet it is only marginally significant (at 10% level).

In columns 3 and 4 we assess the sensitivity of our results to imposing a linear trend and instead use a quadratic trend in cohort of birth. The OLS estimates remain almost identical to those obtained with a linear trend. The 2SLS model imposing a common quadratic trend between all countries lead to results that are 10% larger than those obtained with a linear trend. Finally, imposing a country specific quadratic trend generates much larger estimates of the effect of education on voting (7%), but the instrument becomes weak as most of the variation in education is absorbed by the quadratic trend. Overall, these results are in line with the U.S. based evidence of a significant causal effect of education on voting of between 5-8%.

To test the validity of our instrument, we also run a falsification check where the dates of changes in school leaving age are put forward by 5 years[21]. If the instrument is still significant in such a falsification exercise, it would indicate that changes in SLA are mostly capturing the general improvement in educational attainment over time. Using our favoured specification, this falsification exercise leads to an insignificant effect of the

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[21] Results from these estimations are available on request from the authors.

"reforms" in the first stage, and an F-test of 2. This confirms that the 2SLS estimates are capturing the true causal impact of education on voting for the individuals affected by the change in school leaving age reforms (i.e. compliers as defined by Angrist, Imbens and Rubin, 1996) and as such should be interpreted as a Local Average Treatment Effect (LATE). These 2SLS estimates are thus not to be directly compared to the OLS estimates, which represent the average effect of a year of education on the probability of voting. By definition, individuals affected by SLA reforms have a low preference for education and, since the effect of education on voting is likely to be decreasing in education, one should expect the LATE to be larger than the Average Treatment Effects. As such, the estimates represent the upper bound of the effect of education on voting.

As highlighted above, the literature on the causal effect of education on voting is mixed. While most studies identify a positive effect from studies originating from the U.S., few studies identify any significant effect using European data. In Table 3, we thus present robustness checks for our preferred specification e.g. county specify linear trend, using different samples. In the first column, we include only European countries. The OLS estimates is only half of that estimated in the full sample[22]. In addition, the first stage of the 2SLS is weak and the 2SLS point estimate is very imprecisely estimated. Our results are thus consistent with the lack of evidence that compulsory changes in school leaving age lead to improved civic behaviours in Europe. Column 2 and 3 split the sample by high and low GDP per capita countries (>/< \$20,000)[23]. In the poorer

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[22] Note that Borgonovi et al. (2010) also report an OLS effect of just above one percentage points in their cross country analysis of European data.

[23] Supporting modernisation theory, Powell (1982) and Fornos, Power and Garand (2004) report that economic development is associated with higher turnout. However, the relationship between GDP per capita and voting is non-linear and is greater for relatively less economically developed countries (Blais and Dobrzynska, 1998). The impact of changes in economic conditions, however, is mixed with most

countries, the OLS estimate is just below 1 percentage point and the 2SLS is not significant, even though the first stage is very precisely estimated. The results for the sample of rich countries are similar to those obtained for the full sample, with the 2SLS estimate being even larger, reaching 10 percentage points. Finally, to test whether the US is an exception, we simply run the regression excluding the American observations[24]. The OLS estimates of the effect of one year of education on the probability of voting are below 1 percentage points and the 2SLS estimates are not significant. Thus, our results supports that there is an American exception in the effect of education on voting.

B] Multi-level modelling

We now further investigate whether the U.S. is an exception by relaxing the assumption that the relationship between education and voting is homogenous between countries and estimate a multi-level model. The OLS estimate of such a model is reported in Table 4 in columns (0) and (1) with and without sample weights respectively[25]. We then split the error term between a country specific component and an individual component i.e. we estimate a random effect (2). Since the model includes country dummies and country specific trends in the individual level specification, this already captures country level heterogeneity in the intercept and the likelihood ratio test indicates that the assumption of a common intercept between countries cannot be rejected.

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studies reporting that changes in economic conditions are unrelated to turnout (Kostadinova, 2003; Blais and Dobrzynska, 1998).

[24] Using the U.S. sample only, the OLS estimate reaches 4.6 percentage points and is highly significant. However, due to the small sample size the IV estimates are imprecise.

[25] The weighted sample did not converge for the remaining multi-level model. Based on the evidence of the OLS model, the estimates of education in the un-weighted samples may thus be under-estimates.

Finally in column (3) we report the results of a random component model. There is a significant amount of variation in the parameters between countries, such that the likelihood test rejects that the variance on the estimated education slope is 0. Indeed, 95% of the country specific slopes are in the interval [-0.0082, 0.0268][26]. Note that the covariance between the slope and intercept is negative, indicating that in countries with a high average turnout, the effect of education on turnout is lower than in countries with a low average turnout. The estimates from a random slope model are better interpreted in a scatter plot of the estimated country specific intercept and education effects (Figure 2). This clearly displays the heterogeneity in the relationship between education and voting between countries. Again, it is clear that the U.S. is an outlier in this relationship. In the U.S., one year of education increases voting by 4 percentage points, twice as much as the nearest second largest estimate (Czech Republic), and indeed most of the country estimates are clustered around zero. Thus, we have confirmed the disparate results from the rest of the literature: a significant effect of education on voting in the U.S. but not in other countries.

C] Multi-level modelling: Impact of country specific institutional factors

The previous section has demonstrated that the effect of education on voting is causal, yet this relationship is driven by the U.S. We now test whether country-specific institutional characteristics may explain the relationship between education and voting. We focus on the following institutional variables; compulsory voting, time elapsed since the last election, durability of the political system, democracy and political freedom

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[26] The interval in which 95% of the slope coefficients lie is given by: $0.0093 +/- 1.96 * \sqrt{0.00008}$

indicators, and the fraction of the voting age population that is registered to vote. We also include economic variables as the relationship between education and turnout may be moderated by the economic conditions in the country. For example, Charles and Stephens (2011) finds that increases in wages and employment, at the regional level in the U.S., is associated with lower turnout in gubernatorial elections, yet has no effect on Presidential elections. The economic variables included in the analysis are: GDP growth, inflation, unemployment rate as well as an economic freedom indicator, all measured in the election year.

First, we add these regressors to the OLS and IV models presented in the previous section. While jointly significant, the inclusion of these two sets of controls (institution and economics factors) does not reduce the effect of education on voting[27]. Indeed the OLS is marginally larger than the one reported in the base model (0.022, s.e.(0.002), while the IV estimate is larger and more precisely estimated (0.094, s.e. (0.015)[28]. Hence the education effect on voting is not driven by institutional or economic factors.

Second, we regress our estimates of the random component models on these institutional and economic indicators, in order to assess whether these indicators explain the heterogeneity in the effect of education on voting. These results are reported in Table 5. The first column reports the determinants of the random slope coefficient previously estimated. A negative sign indicates a weaker than average effect of education on voting. The results indicate that the characteristics of a country's institutional system have little impact on the relationship of interest, with the exception of compulsory voting which reduces the relationship between education and voting by 1.3 percentage point; i.e. in

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[27] These estimates are available from the authors.
[28] F test on the significance of the change in school leaving age in the first stage is 37.57.

countries with compulsory voting the correlation between education and voting is almost zero. Regime duration has a small impact on the slope of the education coefficient (0.13%), however, time since the last election has a large positive effect, reaching a maximum at 2 years, after which the effect of education on turnout decreases. We also find that political and civil freedom and the openness of the political institutions have no impact on the relationship between education and voting. As we do not have a measure of the ease of voter registration across time for these countries, we approximate it with the fraction of the voting age population registered to vote. Countries where the legislation makes it easy to register should have a larger fraction of their population registered. However, it is also possible that due to poor record keeping, this proxy over-estimates the true fraction of the population that is actually registered[29]. We find a very small negative correlation between the fraction of the population registered to vote and the coefficient on education in the voting regression (0.05%). This indicates that in countries where registration is easier, the probability of voting is less correlated with education. This is in line with the finding of Milligan et al. (2004) when comparing the U.K. and U.S. but yet the effect is rather small. Finally, the economic conditions at the time of election appear to have little impact on the education gradient in voting. GDP growth increases the coefficient on education, but the effect is very small (0.05%) and none of the remaining economic variables are significant.

For completeness we also report, in column 2 of Table 5, the determinants of the random intercept. The random intercept estimates the mean voting rate by country. Few of the covariates are significant; regime duration has a concave effect peaking at 6.5 years. The proportion of the population registered to vote proxy has some validity, as it is

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[29] Some countries also allow non-resident nationals to vote, thus this ratio may be over 100%.

significantly correlated with the mean voting rate. Compulsory voting has, as expected, a very large effect on the voting rate, increasing it by 28 percentage points. Overall, these results suggest that, apart from compulsory voting, the institutional and economic characteristics of the country do not explain the observed relationship between education and voting, and hence do not explain why the education gradient is larger in the U.S.

D] Disenfranchisement law, education and turnout in the US

One remaining potential explanation for the strong relationship between education and voter turnout in the U.S. may be related to disenfranchisement laws. Disenfranchisement laws deny prisoners and felons the right to vote. While several countries deny prisoners the right to vote[30], the U.S. is unique in depriving convicted offenders who have served their sentences of their voting rights (Rottinghaus, 2003)[31]. There is evidence that the U.S. disenfranchisement laws reduce voter turnout at the state level (McLeod, White and Gavin 2003), can influence the outcomes of elections (Uggen and Manza, 2002), and that incarceration not only affects the turnout rate of the felon, but also of their partners (Sugie, 2011).

Disenfranchisement laws may play a role in explaining why the U.S. is an exception regarding the education/voting relationship since the U.S. is an outlier regarding the proportion of individuals that are, or have been, incarcerated. In 2010, the U.S. has a prisoner rate of 743/100,000 while the rate in the U.K., which is the western

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[30] In February 2011, the European Court of Justice ordered European Union countries that still ban prisoners' vote to update their legislation.

[31] "A few countries restrict the vote for a short period after conclusion of the prison term: Finland and New Zealand, for example, restrict the vote for several years after completion of sentence, but only in the case of persons convicted of buying or selling votes or of corrupt practices. Some countries condition disenfranchisement of prisoners on the seriousness of the crime or the length of their sentence. Others, e.g., Germany and France, permit disenfranchisement only when it is imposed by a court order." (Sentencing Project, 1998, p17)

European country with the highest incarceration rate, it reaches only 155/100,000[32]. As lower education is associated with greater criminal activity, and thus greater incarceration rates (Lochner and Moretti, 2004), a relatively high proportion of low educated individuals cannot vote in the U.S., which could explain the higher education gradient in voting.

Since most other countries do not have disenfranchisement laws, we use State level data on voting for the U.S. for the period 1988-2010, and use variations in the disenfranchisement laws over time to identify whether these laws impact on the relationship between education and voting[33]. These legislations are voted by States legislature and the majority of States deny citizens on parole, probation, or even those who have completed their sentence, of their voting rights. It is estimated that 5.3 million Americans have lost their voting rights (Sentencing Project, 2008). We can identify five different disenfranchisement regimes: prisoners allowed to vote, voting rights restored after completion of parole, voting rights restored after completion of probation and parole, voting rights restored after release, voting rights restored on individual cases or permanently denied the vote. In 2010, only two States (Maine and Vermont) allowed prisoners to vote, 13 States disenfranchised prisoners and the remaining 35 disenfranchising felons on parole, probation or permanently. Due to the large discrepancies in incarceration rates and disenfranchisement laws, the fraction of the voting age population disenfranchised varied from 0% to 9%

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[32] Figures are available from the International Centre for Prison Studies

[33] The data on turnout at the State level is available from http://elections.gmu.edu/FAQ.html#Turnout. Previous years could also be investigated however, the fraction of felons in the population increase dramatically from mid-eighties onwards (US department of Justice, 2003). Data on disenfranchisement laws is available from: http://felonvoting.procon.org/view.resource.php?resourceID=286

(Florida) with a national mean of 2.2%[34]. These rates are about twice as large for African Americans.

Table 6 reports the impact of each disenfranchisement regime on state-level turnout, using the voting eligible population turnout rates. Since turnout is higher at presidential elections compared to other high office elections (governor, US senator), our base model, reported in column 1 of Table 6, includes a dummy for presidential elections, as well as the State share of the population that is college educated[35]. As expected, education and presidential elections are positively correlated with turnout, even at this aggregated level. The disenfranchisement laws have the expected negative effect on voting, such that States which do not allow felons to vote experience a 7 percentage point lower turnout. Denying prisoners, individuals on parole or probation the vote, also reduces the State turnout rate by 1 to 4 percentage points. However, the inclusion of the disenfranchisement laws has no effect on the estimated effect of college education on turnout. Adding the average demographic, such as age, gender and race (model 3) or socio-economic characteristics (model 4), such as fraction in employment and in public sector employment, reduces the effect of education yet it remains significant. However, interacting the fraction of college educated population with the disenfranchisement law indicators reveals that the education effect on voting is stronger in States with harsher disenfranchisement laws (model 5) and that the direct effect of education disappears. The interaction effects are jointly statistically significant at the 10% level and the interaction between the share of college educated and permanent disenfranchisement is highly significant. A State moving from banning felons to vote to the most benign system of allowing prisoner to vote would expect an increase in participation of

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[34] Data obtained from http://www.sentencingproject.org/map/map.cfm
[35] State level characteristics are based on CPS means for the election year.

10 percentage points. Harsh disenfranchisement laws could then indeed be partly responsible for the greater effect of education in the U.S. compared to other countries.

6.   Conclusion

The central aim of this study was to investigate the inconsistent findings in the literature regarding the causal impact of education on voter turnout. While studies adopting experimental and quasi-experimental methods often identify a causal relationship using U.S. data, there is almost no evidence of a causal impact when European data is used. Using repeated cross sections over a 20 years period for up to 38 countries, this study proceeded in three steps.

First, we show that an additional year of education increases the probability of voting by ~2% in the OLS model or about 1% when using European data only. Moreover, using educational reforms as instruments, we find that the relationship between education and voting is causal and twice as large. However, our analysis excluding the U.S. does not show a significant effect of education on turnout, suggesting that the observed causal relationship identified in the aggregate analysis is driven by the inclusion of the U.S. Our results including the U.S. also mirrors the estimates of most causal U.S. studies, such as Milligan et al. (2004) and Dee (2004). As such the differences in the effect of education on voting identified in the literature stem from cross national differences in this relationship rather than methodological or data-driven differences.

In the second step, we assess whether the country level heterogeneity in the relationship between education and voting is driven by institutional and economic characteristics. Again, we find that the effect of education on voting is observed almost

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exclusively in the U.S. compared to the other countries in our dataset, yet these differences are not driven by institutional and economic factors. The exceptions are compulsory voting and a proxy for ease of voter registration which tend to reduce the impact of education on voting.

Third, in order to identify an alternative explanation for the U.S. exception, we investigated the role of disenfranchisement laws. Since the U.S. incarcerated population is much larger than in other countries, and its disenfranchisement laws harsher than most, these may be responsible for the differences in the effect of education on voting in the U.S. compared to other countries. By exploiting differences across U.S. States and changes in disenfranchisement laws over time, we find evidence that such legislation may be responsible for the diverging relationship regarding education and voting. To the best of our knowledge, this is the first study to conduct such analysis. While further work taking account of disenfranchisement laws around the world may enrich this finding, such analyses are constrained by the lack of variation, especially across European countries, regarding these laws. Altogether, these findings provide little support that education has a direct causal effect on voting behaviour. As such the modernisation theory is not well supported by our empirical evidence. Arguments regarding the externalities of education in terms of civic engagement should also be revised.

There may be other factors, such as differences in the educational curriculum between countries with regards to civil values, which explain the relationship. If greater emphasis is provided in U.S. schools towards the importance of political and civil involvement than in other countries' schools, this may explain the resulting relationship.

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Figure 1: Relationship between education and voting behaviour
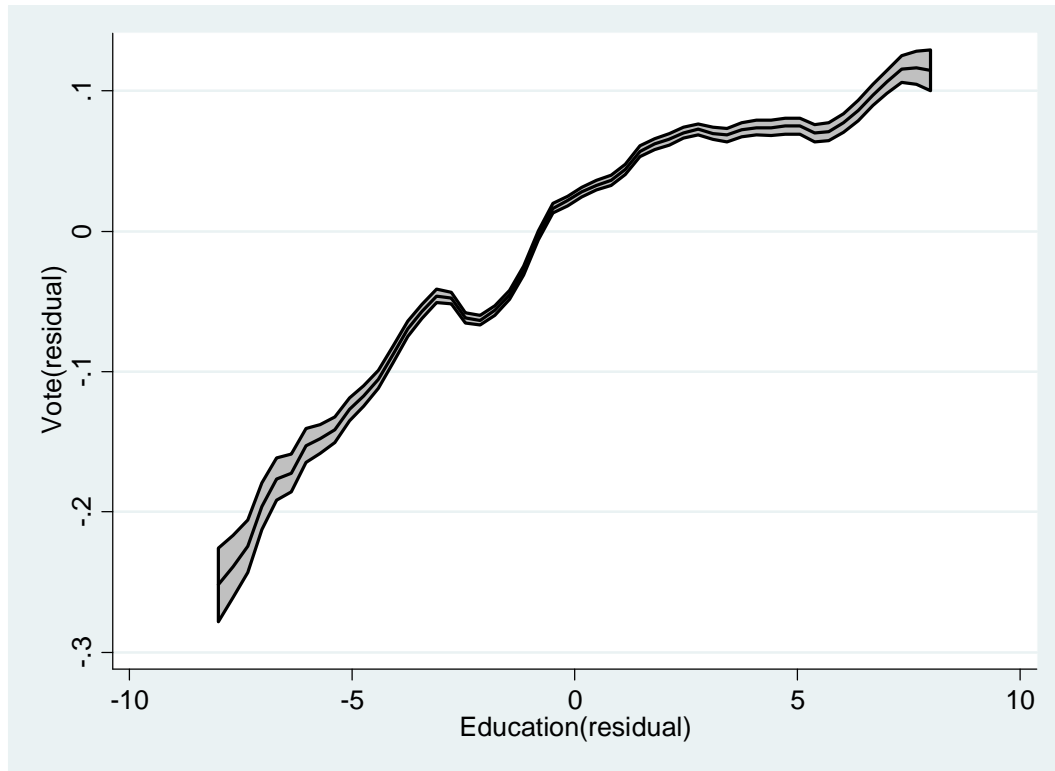
Note: ISPP 1985-2010, all countries with available school leaving age reforms. Observations are re-weighted by population size. Standard errors are displayed by the grey area.
 Education residuals are based on a regression of education on regressing years of education on country specific quadratic trends in birth cohort. Cohort 1 being the oldest cohort satisfying our selection conditions (born in 1914). Residual vote are the residuals of a regression of voter turnout on country specific trends in the survey year. The distribution is truncated at education residuals within -8 and +8.

Figure 2: Estimated intercept and slope effect of education on voting by country
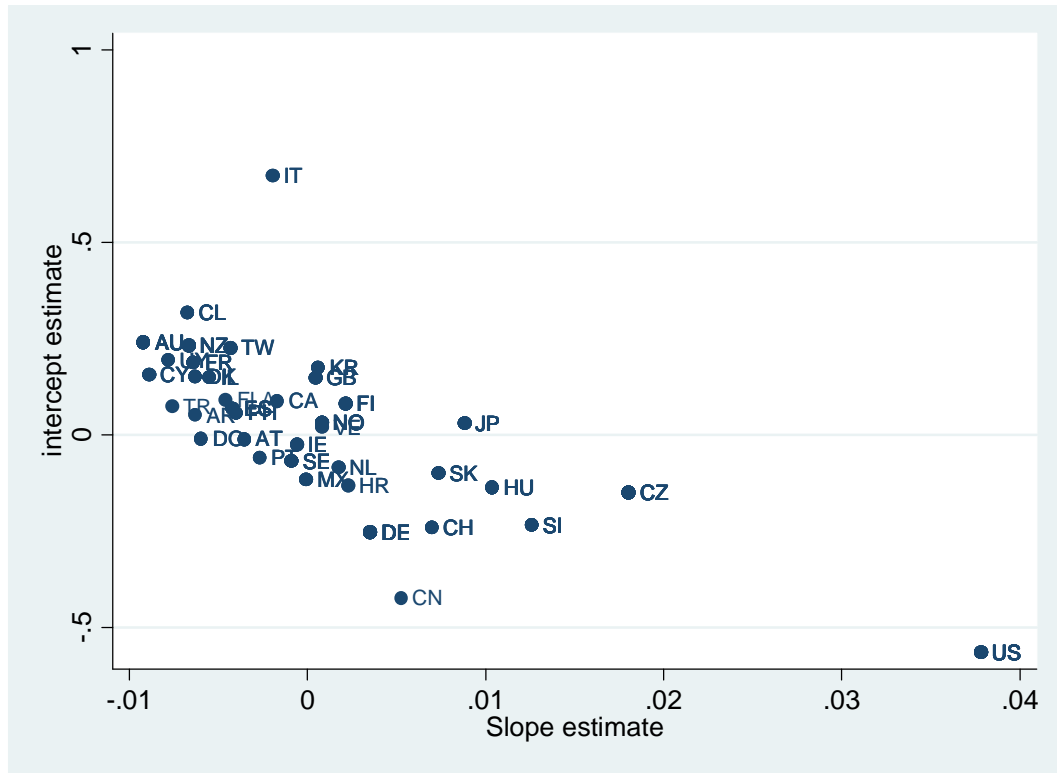
Note: Graph based on the estimates from a random coefficient model (Model (3) Table 5)

Table 1: Descriptive statistics – ISSP 1985-2010 – country panel

Variable		Mean	Std. Dev.
Voter Turnout	overall	0.811	0.391
	between		0.113
	within		0.376
Education years	overall	11.854	3.913
	between		1.832
	within		3.613
Male	overall	0.471	0.499
	between		0.032
	within		0.498
Married	overall	0.673	0.469
	between		0.102
	within		0.461
Nbr individuals In Household	overall	3.198	1.644
	between		0.554
	within		1.550
Union member	overall	0.230	0.421
	between		0.180
	within		0.381
Religious practice	overall	0.682	0.465
	between		0.186
	within		0.437

Pooled sample: unweighted. Reports the overall means and the between country and within country standard deviations. The analysis is based on 253,490 observations in 38 different countries.

Table 2: Education and Voting – OLS and 2SLS

Panel A	OLS	OLS	OLS	OLS
Years of education	0.0208	0.0208	0.0208	0.0205
	(0.0025)	(0.0024)	(0.0024)	(0.0024)
R^2	0.160	0.164	0.161	0.165
Panel B	2SLS	2SLS	2SLS	2SLS
First stage: Education				
SLA	0.1622	0.0746	0.1642	0.0345
	(0.0318)	(0.0114)	(0.0332)	(0.0133)
F-test	25.91	42.66	24.44	6.76
Partial R^2	0.0031	0.0004	0.0031	0.0001
Weak instrument				
F-test	220.7	43.95	224.85	5.860
Second stage				
Years of education	0.0500	0.0588	0.0581	0.0710
	(0.0190)	(0.0324)	(0.0152)	(0.0413)
	Linear trend	Country specific linear trend	Quadratic trend	Country specific Quadratic trend

Note: Standard errors are clustered at the country/year level.

Number of observations 253,490

The specification includes dummies for gender, marital status, number of individuals in the household, union member, practicing religion, urbanicity, years of survey and regional dummies. Data is weighted at the country level (using 2010 population).

Table 3: Education and Voting – Robustness checks

	Europe	GDP/capita <$20,000	GDP/capita >$20,000	Excluding USA
A] OLS				
Years education	0.0100	0.0095	0.0262	0.0090
	(0.0008)	(0.0017)	(0.0025)	(0.0006)
R^2	0.088	0.283	0.123	0.200
B] 2SLS				
First stage: Education				
SLA	0.0568	0.0637	0.0796	0.0684
	(0.0400)	(0.0065)	(0.030)	(0.0089)
F-test	2.01	95.67	6.88	58.92
Partial R^2	0.0001	0.0008	0.0002	0.0004
Weak F-test	6.375	26.379	21.361	41.608
Second stage Years education	0.0814	0.0504	0.1049	0.0555
	(0.0675)	(0.0294)	(0.0353)	(0.0311)
Observations	139,734	42,045	211,445	236,519
	Country specific linear trend	Country specific linear trend	Country specific linear trend	Country specific linear trend

Note: Standard errors are clustered at the country/year level.

The specification includes dummies for gender, marital status, number of individuals in the household, union member, practicing religion, urbanicity, years of survey and regional dummies. Data is weighted at the country level (using 2010 population).

European countries are defined as: Austria, Belgium (Flanders), Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Hungary, Italy, Ireland, Latvia, Netherlands, Norway, Poland, Portugal, Slovenia, Slovakia, Spain, Sweden, Switzerland, UK.

GDP/Capita is taken from IMF (2010).

Countries with GDP/Capita less than $20,000 are: Argentina, Chile, China, Croatia, Dominican Republic, Hungary, Mexico, Philippines, Turkey, Uruguay, Venezuela.

Countries with GDP/Capita greater than $20,000 are: Australia, Austria, Belgium (Flanders), Canada, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Italy, Ireland, Israel, Japan, Korea, Netherlands, New Zealand, Norway, Portugal, Slovenia, Slovakia, Spain, Sweden, Switzerland, Taiwan, UK, USA.

Table 4: Education and voting: Multilevel model

	(0) - OLS	(1) - OLS	(2) – Random intercept	(3) – Random slope
Indiv level				
Education	0.0208	0.0097	0.0097	0.0093
	(0.00001)	(0.0002)	(0.0002)	(0.0014)
Country level				
Var (educ)				0.00008
				(0.00002)
Var (cons)			0.0000	0.0305
			(0.0000)	(0.0071)
Cov (educ, cons)				-0.0012
				(.0003)
Var (resid)	0.171	0.1349	0.1349	0.1346
	(0.00002)	(0.0004)	(0.0004)	(0.0004)
Log Lik	-2712847	-105784	-105784	-105692
Population weight	x			

Note:

Number of observations 253,490, number of countries 38

The specification includes dummies for gender, marital status, number of individuals in the household, union member, practicing religion, urbanicity, years of survey, country dummies and a country specific linear trend in birth year.

Table 5: OLS – Political variables and estimated random slope and intercept

VARIABLES	Random slope	Random intercept
Regime duration	-0.0013	0.0498
	(0.0006)	(0.0157)
Regime duration square	0.0001	-0.0037
	(0.0000)	(0.0010)
Openness of the political institutions (7)	-0.0061	0.1855
	(0.0049)	(0.1019)
Openness of the political institutions (8)	0.0020	-0.1724
	(0.0072)	(0.1680)
Openness of the political institutions (9)	-0.0082	0.0942
	(0.0073)	(0.1692)
Openness of the political institutions (10)	0.0014	-0.2221
	(0.0073)	(0.1670)
Freedom House Political right 2	-0.0018	-0.0307
	(0.0024)	(0.0723)
Freedom House Political right 3	-0.0039	-0.0446
	(0.0047)	(0.1054)
Freedom House Political right 4	-0.0099	0.1099
	(0.0050)	(0.0895)
Freedom House Civil liberties 2	0.0039	-0.0031
	(0.0016)	(0.0485)
Freedom House Civil liberties 3	-0.0026	0.0478
	(0.0030)	(0.0859)
Freedom House Civil liberties5	-0.0061	0.1758
	(0.0158)	(0.3170)
Compulsory voting	-0.0134	0.2788
	(0.0015)	(0.0386)
Time since last election	0.0145	-0.1179
	(0.0072)	(0.1738)
Time since last election square	-0.0036	0.0099
	(0.0025)	(0.0605)
% of voting age population registered	-0.0005	0.0099
	(0.0001)	(0.0018)
GDP growth in election year	0.0005	-0.0039
	(0.0002)	(0.0061)
Inflation in election year	0.0001	-0.0026
	(0.0002)	(0.0039)
Unemployment in election year	0.0000	-0.0047
	(0.0002)	(0.0040)
Economic freedom score	-0.0017	0.0347
	(0.0011)	(0.0280)
Constant	0.0513	-0.9326
	(0.0143)	(0.3061)
Observations	244	244
R-squared	0.5810	0.5639

Note: The estimated slopes and intercept are predicted from Model (2) in Table 4. Standard errors are obtained by bootstrap of Model (2) and these regressions. Bootstrap is conducted with 200 replications.

Table 6: Disenfranchisement law, education and voting in the US – State fixed effects using Voting eligible population turnout 1988-2010

	(1)	(2)	(3)	(4)	(5)
College educated	27.422	27.294	17.381	22.109	-1.052
	(4.142)	(4.156)	(4.599)	(4.846)	(9.754)
Presidential election	16.219	16.248	16.482	16.535	16.467
	(0.573)	(0.579)	(0.608)	(0.616)	(0.609)
Dis 1: cannot vote in jail		-1.042	-2.045	-2.015	-7.145
		(2.308)	(2.016)	(1.605)	(7.141)
Dis 2: cannot vote in jail or probation		-4.542	-6.126	-5.771	-10.900
		(2.292)	(2.002)	(1.939)	(6.768)
Dis 3: cannot vote in jail, parole or probation		-3.753	-4.55	-3.996	-14.292
		(2.317)	(2.171)	(1.898)	(6.862)
Dis 4: felons cannot vote		-6.821	-7.216	-7.371	-21.195
		(2.566)	(2.552)	(2.191)	(7.192)
Dis 1 *college					10.439
					(12.564)
Dis 2 *college					10.251
					(11.751)
Dis 3 *college					20.838
					(11.919)
Dis 4 *college					31.344
					(12.402)
					2.28
$F_{(4,50)}$ (p)					P=0.08
Age, gender, race			X	X	X
Self-employed, public				X	
Constant	29.731	33.475	80.961	70.747	80.219
	(1.996)	(2.779)	(13.316)	(13.495)	(13.659)
Observations	612	612	612	612	612
Number states	51	51	51	51	51
R-squared	0.60	0.62	0.39	0.40	0.41

Note: The omitted disenfranchisement regime category is allowing prisoners to vote.
Robust standard errors, clustered at state level
$F_{(4,50)}$ test of joint significance of interaction terms.

Appendix

Table A1: Sample of SSP Voter Turnout Question Wording - ISSP 2009

Country	Turnout Question
Argentina	Do you remember for whom did you vote in the 2007 presidential elections?
Austria	Which party did you vote for in the last legislative election on 28th September 2008?
Australia	Did you vote in the Federal Election held on October 9, 2004?
Switzerland	For which party did you vote at the last federal elections in October 2007?
Chile	Are you registered to vote? (If yes) Did you vote on the last municipal elections of October 2008?
China	Have you participated in the latest Deputy to the People's Congress election/ election of village committees?
Cyprus	Have you voted in the last parliamentary elections?
Czech Republic	Let us go back to the last elections to the Chamber of Deputies that were held on 2 and 3 July, 2006. Did you take part in the elections?
Denmark	The last general election took place in September 2009. Did you vote in that election?
Germany	Did you vote in the previous election the 13 November 2007, or were there some reason that did that you didn't have opportunity to or desire to vote?
Estonia	Did you vote in the last general elections?
Finland	Did you vote in the last parliamentary elections in 2007?
France	Did you vote in last first round of the 2007 French Presidential Election?
Croatia	IS: Did you vote in last general election?
Israel	Did you vote last election?
Japan	Did you vote in the Upper House election in July 2007?
South Korea	Did you vote in the South Korean legislative election (April-9, 2008)?
Latvia	Did you vote in the 9th Parliament elections on the 7th of October 2006?
Norway	Did you vote in the last national election?
New Zealand	At the 2008 General Election, who did you vote for?
Poland	Did you vote in the elections to the Seym and Senate on the 21 X 2007?
Portugal	Did you vote in the last elections? (Assembly of the Republic on 20th of February of 2005)
Russia	Did you vote in the last election of the State Duma in December 2003?
Sweden	Did you vote in the latest general elections?
Slovenia	Did you vote in last general election?
Slovakia	In June 2006, elections to National Council of the Slovak Republic were held. Did you vote?
Turkey	Were you able to cast your vote in the last election on 22 July 2007?
Ukraine	Did you vote in early parliamentary elections on September 30, 2007?
USA	In 2008, you remember that Obama ran for President on the Democratic ticket against McCain for the Republicans. Do you remember for sure whether or not you voted in that election?
South Africa	For which party did you vote for in the last election, which was held in 2004?

Table A2: Sample selection

	Observations	Country/years	Country
ISSP 1985-2009			
All waves including voting information	404,480	294	45
Self reported voting	402,031	294	45
School leaving age information	346,665	259	38
Less than 25 years of schooling & not in school	308,938	250	38
Gender defined & Age [25-70]	253,490	250	38

Note: sample based on pooled ISSP 1985-2009

Table A3 Educational Reforms for cohorts born between 1932 and 1982

	Date	Effect	First affected cohort	Source
Argentina	1995	12 to 14	1983	Heran et al. (2001)
Australia**				Varies by state
Austria	1962	14 to 15	1947	Brunello et al. (2009)
Belgium	1983	14 to 18	1969	Brunello et al. (2009)
Canada**				Oreopoulos (2005)
Chile	1966	12 to 14	1954	Joaquin, J. (1990)
China	1986	12 to 15	1974	http://unesdoc.unesco.org/images/0012/001218/1
Taiwan	1968	6 to 15		Tsai, L.A. (2008)
Cyprus	1971	14 to 15	1957	Unesco, International yearbook of education 196
Czech	1948	14 to 15	1934	Unesco International yearbook of education 194
Republic	1953	15 to 14	1940	Educational Audiovisual and Culture Executive
	1960	14 to 15	1946	
	1984	15 to 16	1969	
Denmark	1958	11 to 14	1947	Brunello et al. (2009)
	1971	14 to 16	1957	Garrouste, C. (2010)
Dom. Rep.	1985	12 to 16	1973	http://education.stateuniversity.com/pages/388/D
Finland**	1972-77	13 to 16	1961-1966	Brunello et al. (2009)
France	1967	14 to 16	1953	Brunello et al. (2009)
Germany**	1947-69	14 to 15	1934-1955	Pischke and Watcher (2005)
	1990	16 to 15	1975	Borgonovi et al. (2010)
Hungary	1961	14 to 16	1947	Borgonovi et al. (2010)
Ireland	1972	14 to 15	1958	Brunello et al. (2009)
Israel	1968	13 to 15	1955	http://www.mfa.gov.il/MFA/History/Modern%2
	1974	15 to 16	1959	%20Israel
	1979	16 to 18	1963	
Italy	1964	12 to 14	1952	Brunello et al. (2009)
	1998	14 to 16	1984	
Japan	1947	12 to 15	1939	Okano, J. & Tsuchiya, M. (1999).
South Korea	1949*	10 to 12	1937	Gwang-Jo, K. (2001)
	1997	12 to 15	1985	http://www.inca.org.uk/pdf/comparative.pdf
Mexico	1992	12 to 15	1980	Anderson,J.B. & Gerber, J. (2008)
Netherlands	1949	13 to 14	1936	Garrouste (2010)
	1969	14 to 16	1955	
New Zealand	1944	14 to 15	1932	Unesco: compulsory education in New Zealand
	1993	15 to 16	1978	http://www.inca.org.uk/pdf/comparative.pdf
Norway*	1960-1972	14 to 16	1953	Black et al. (2005)
Philippines	1953	0 to 12	1946	http://hrd.apec.org/index.php/Education_in_the_
				http://www.chanrobles.com/republicacts/republi
Portugal	1973	12 to 14	1951	Fort (2006)
Slovakia	1948	14 to 15	1934	Unesco International yearbook of education 194
	1953	15 to 14	1940	Educational Audiovisual and Culture Executive
	1960	14 to 15	1946	

	1984	15 to 16	1969	
Spain	1970	12 to 14	1957	Brunello et al. (2009)
	1990	14 to 16		Garrouste (2010).
Sweden*	1962*	14/15 to 15/16	1951	Brunello et al. (2009) Garrouste (2010).
Switzerland*				Varies by Canton
Turkey	(1961) 1998	10 to 13	1988	http://siteresources.worldbank.org/INTTURKEY (reform implemented for 5th grader in 1998)
UK**	1947	14 to 15	1933	Harmon and Walker (1995)
	1973	15 to 16	1958	
US**				Oreopoulos (2005)
Uruguay	1973	12 to 15	1961	Masena, A. & Salaverria, J. (2003).
Venezuela	1958	N/A to 12		Hanson, M. E. (1986).
	1980	12 to 15	1968	Patrinos, H. and Sakellariou, C. (2005)
Ex-Yugoslavia	1950	14 to 15	1936	Unesco international yearbook 1950

Note: * regional variations in the implementation of the reform, no regional information available use year when median population is treated

** regional variations in the implementation of the reform, use current region of residence to define SLA

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