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Journal of Elections, Public Opinion & Parties

ISSN: 1745-7289 (Print) 1745-7297 (Online) Journal homepage: https://www.tandfonline.com/loi/fbep20

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To cite this article: Mikael Persson (2013) Is the Effect of Education on Voter Turnout Absolute or Relative? A Multi-level Analysis of 37 Countries, Journal of Elections, Public Opinion & Parties, 23:2, 111-133, DOI: 10.1080/17457289.2012.747530

To link to this article: https://doi.org/10.1080/17457289.2012.747530

Journal of Elections, Public Opinion and Parties, 2013
Vol. 23, No. 2, 111–133, http://dx.doi.org/10.1080/17457289.2012.747530

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Is the Effect of Education on Voter Turnout Absolute or Relative? A Multi-level Analysis of 37 Countries

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ABSTRACT *While it is well established that education is positively correlated with voter turnout at the individual level, the increased educational levels in most western countries have not caused increased voter turnout at the aggregate level. The relative education model suggests one explanation: education is only a proxy for social status and has no direct causal effect. The individual-level effect of education is conditional on the level of education in the environment. Whereas previous research on the relationship between relative education and voter turnout has largely focused on the U.S. case, this article uses comparative survey data on voter turnout to test the relative education model. It combines data from the CSES and ESS covering about 275,000 individuals in 173 country-years in 37 countries. The analysis applies a definition of relative education operationalized as each individual's education rank position in relation to the level of education of those born in the same five-year cohort in the same country. The results show that relative education has a much larger effect on voter turnout than absolute education. Moreover, relative education has a stronger effect when aggregate turnout is low.*

The Problem

The relationship between education and voter turnout is a major puzzle in political behavior research. On the one hand it is well established that education is positively correlated with voter turnout at the individual level. On the other hand, the increased educational levels in most western countries have not caused an aggregate increase in voter turnout (e.g., Brody, 1978). How can this puzzle be solved?

The relative education model put forward by Nie, Junn and Stehlik-Barry (1996) (NJS hereafter) proposes an explanation: education has no direct causal effect on voter turnout; it works only as a proxy for social status. The individual-level effect of education is thus conditional on the level of education in the environment. This model stands in sharp contrast to the conventional view, sometimes referred to as the absolute education model, according to which education increases civic skills

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and cognitive capabilities, factors that in turn increase political participation irrespective of the level of education in the environment.

According to the relative education model, or the sorting model as it is sometimes referred to, education functions as a sorting mechanism that influences individuals' social network positions, which in turn affect voter turnout. If this model is correct, educational inflation is the explanation for why aggregate increases in education have not resulted in higher voter turnout. As the level of education increases, more education for each individual is needed to retain the same social status position.

Previous research on the relationship between relative education and voter turnout has mostly employed single country study designs.[1] Following NJS, a number of studies have re-examined the relative education model (Campbell, 2009; Helliwell & Putnam, 2007; Persson, 2011; Tenn, 2005). NJS claim that their model is universally applicable, yet do not provide empirical evidence that their model resolves the paradox between education and voter turnout in other countries than the United States. The present article tests the wider generalizability of the model and goes beyond previous research by using comparative survey data. It combines data from the Comparative Study of Electoral Systems and European Social Survey covering about 275,000 individuals over 173 country-years in 37 countries, and applies a definition of relative education defined as each individual's education relative to the level of education of those born in the same five-year cohort in the same country at each point in time. The results show that relative education has a much larger effect on voter turnout than absolute education.

The article brings the following contributions to the field. First, it provides a test of the relative education model on voter turnout using country comparative data. Hence, it constitutes a crucial test for the generalizability of the model. Second, it presents a refined modeling strategy for relative education relying on country-comparative intra-birth cohort measures. Third, it brings an important substantive message to voter turnout scholars since it shows that the effect of education is relative rather than absolute. The conclusion suggests that most studies on voter turnout misinterpret the effect of education.

The outline of the article is as follows. The next section presents the theory and literature review. Thereafter, data and techniques of analyses are discussed. Results are subsequently presented, and the article concludes with a discussion on the implications of the results.

Theory

How can the puzzling relationship between education and voter turnout be explained? Whether education has any direct causal effects on any form of political participation is a highly contested issue (Berinsky & Lenz, 2011; Burden, 2009; Campbell, 2009; Dee, 2004; Henderson & Chatfield, 2011; Highton, 2009; Hillygus, 2005; Kam & Palmer, 2008, 2011; Mayer, 2011; Milligan et al., 2004; Sondheimer & Green, 2010; Nie & Hillygus, 2001; Niemi & Junn, 1998; Persson, 2012; Persson & Oscarsson, 2010; Tenn, 2005, 2007). According to the conventional view, education is a

direct *cause* of political participation. This view, sometimes referred to as the absolute education model, states that education positively affects voter turnout directly since it supposedly increases skills relevant to understanding politics, increases political interest, and increases the sense of civic duty and concern for the importance of elections (e.g., Lewis-Beck et al., 2008; Verba et al., 1995; Wolfinger & Rosenstone, 1980). The fact that education and turnout are strongly correlated in most countries has been considered to provide support for this view.

However, the conventional view has failed to explain anomalies such as the puzzling relationship between education and political participation. In his seminal work on voter turnout, Franklin estimates the average effect of higher education on turnout to be about 15% (2004: 17). At the same time, he acknowledges the puzzling fact that most countries used to have higher levels of turnout when they had fewer highly-educated citizens and expresses doubts about the true causal effect of education. However, few attempts have been made by researchers dealing with comparative voter turnout to solve this paradox.

To handle the anomaly of the relationship between education and political participation in the United States, researchers have started to question the conventional view and instead argue that there is no direct causal effect of education on voter turnout. Hence, education could be a *proxy* for other factors (cf. Kam & Palmer, 2008, Nie et al., 1996). NJS draw on a theory generally attributed to Hirsch (1976) according to which education is a positional good with a value that depends on how many others possess it. According to the relative education model, education has no value per se but rather serves as a proxy for social position. It is the social status position gained by education that increases turnout, not what the educational experience actually does with individuals. According to the relative education model, the same amount of education has a stronger effect in a low education context than in a high education context. In a low education context, less education at the individual level is needed in order to gain a high social status position. Hence the impact of education on political participation is relative rather than absolute.[2]

According to NJS, social network position is the causal mechanism connecting relative education and political participation. People who are close to the center of important social networks are more likely to vote than people in the periphery, irrespective of level of absolute education. Franklin's analysis of the costs and benefits of voting supports the idea that having a favorable social network position increases voter turnout: "People in social networks would also incur costs of nonvoting because other members of their group care whether they vote or not … So, the benefits of voting and the costs of nonvoting are higher for socially connected people" (Franklin, 2004: 51). NJS also contend that education is important since it strongly influences a person's social network status, but it is not important in itself. This causal mechanism is, however, not possible to test with the dataset used in the present article; see Persson (forthcoming) for an investigation of the causal mechanism. Instead we focus on the observable predictions of the model, i.e., that persons with a high rank in the education hierarchy are more likely to vote than persons with a low rank, irrespective of their absolute levels of education.

Previous research on relative education and voter turnout has disagreed on how to model relative education. NJS compare each respondent's education level with the mean national levels among individuals aged 25–50 when the respondent was 25, and find support for the relative education model across a wide range of political participation indicators. However, their measure of relative education has been criticized for not being able to separate the effect of the educational environment from the effect of age and year of birth (Tenn, 2005).

Helliwell and Putnam (2007) use geographically narrower measures in order to better capture the local context. They compare each respondent's education to "all other living adults, both older and younger" within the same geographical region (Helliwell & Putnam, 2007: 3). Hence, compared to NJS, Helliwell and Putnam's definition of relative education is wide regarding age but narrow as to geography. Helliwell and Putnam find no support for the relative education model, but it should be noted that their focus is only on social capital and that they do not analyze voter turnout. The problem with their definition is that it makes it impossible to control for state-level variations, such as registration requirements, opportunities for early voting, etc., since the relative education measure is correlated with geographic region.

To overcome these problems, Tenn (2005) uses intra-birth cohort measures of the educational environment in his study of the relative education model and voter turnout in the United States. Tenn employs a measure of relative education according to which each individual's education is compared with the mean level of education of those born in the same year in the entire United States. Tenn's results give strong support for the relative education model over the absolute education model. The problem with Tenn's measure, however, is that it does not consider regional differences in educational levels. Campbell (2009) argues that there are strong geographical variations in educational levels, and since social status is shaped in relation to one's closest surroundings, any measure that does not take the local geographical context into account will be biased. Thus, he defines the educational environment narrowly in terms of both age and place. Campbell (2009) finds support for the relative education model on competitive forms of political participation, including "electoral activities." In a similar vein, Persson (2011) uses mean educational levels in Swedish municipalities and finds support for the relative education model on voting and activities related to political parties.

The modeling strategy applied in this article follows Campbell (2009) in the sense that it uses a relative education measure that varies according to both age and place. As for age, relatively small cohorts are used (five years). As for place, the analysis exploits the cross-national variation in educational levels at different times. Following Campbell it would be preferable to define the educational environment at a geographical level that is as low as possible. But since comparable subnational measures of aggregate educational levels do not exist, and we do not know the exact geographical location of each respondent within countries, it is not possible to construct such a measure with the data used here.

One further issue that previous research on the relative education model has not dealt with is whether the effect of relative education is different in different contexts. According to the so-called "law of dispersion" formulated by Herbert Tingsten in his seminal work *Political Behavior* (1937; see also Rosenstone & Hansen, 1993) the level of equality in political participation is higher when the level of voter turnout is higher. Consequently, political inequality will increase as voter turnout decreases. If this theory holds it would suggest that differences in turnout between citizens with different levels of relative education should be larger when aggregate turnout is lower and that the differences should be smaller when aggregate turnout is higher. The analysis in this article will add to our previous knowledge on the relative education model by testing this idea.

Data

When modeling relative education, it is crucial to use data with enough variation in the educational levels in the environment. With data from only one election and one place, absolute and relative education would strongly correlate. To get necessary variation in the contextual levels of education, this study combines individual-level data from the Comparative Study of Electoral Systems (modules 1, 2 and 3) and the European Social Survey (rounds 1 to 5).[3] Following previous studies on the relative education model, only adult citizens are included in the analyses (25–70 years old), since earlier in life education has not yet had a chance to have its full effects. Including all respondents would result in involving a lot of respondents who have not yet finished their educations.

The 37 countries and the 173 country-years are presented in Table 1. In all these countries, the mean levels of education have increased during the last 60 years, while the development of voter turnout is slightly negative in most countries. Hence, in all countries we see a weak or negative relationship between education and voter turnout at the aggregate level.

By combining the ESS and CSES, we get variation in both time (1996–2010) and geographic context (37 countries). To ensure that the electoral behavior of the respondents is performed without governmental coercion, the analysis is restricted to countries that are considered free according to the Freedom House Index.[4,5]

As individual-level control variables, only a small set of controls for which items are equivalent in the ESS and CSES are used. More specifically, the following control variables – which previous research repeatedly has shown to be correlated with voting (e.g., Franklin, 2004; Lewis-Beck et al., 2008; Verba et al., 1995) – are included in the analysis: age, age squared, gender[6] and marital status.[7] This article does not explore the effects of these variables; the purpose of including them is only to control for their influence on the main independent variables.[8] In addition, controls for five contextual-level factors that are likely to affect voter turnout are included in the analysis: compulsory voting,[9] democratic system,[10] electoral system,[11] type of ballot[12] and registration requirements.[13]

Table 1. Countries and years included in the analysis

Country	Year of survey
Austria	2008
Belgium	1999, 2002, 2003 2004, 2006, 2008, 2010
Brazil	2002, 2006
Bulgaria	2001, 2006, 2008, 2010
Canada	1997, 2004
Croatia	2007, 2008
Cyprus	2006, 2008
Czech Republic	1996, 2002, 2004, 2006, 2008, 2010
Denmark	1998, 2001, 2002, 2004, 2006, 2008, 2010
Estonia	2004, 2006, 2008, 2010
Finland	2002, 2003, 2004, 2006, 2007, 2008, 2010
France	2002, 2004, 2006, 2007, 2008, 2010
Germany	1998, 2002, 2004, 2005, 2006, 2008, 2009, 2010
Greece	2002, 2004, 2008
Hungary	1998, 2002, 2004, 2006, 2008, 2010
Iceland	1999, 2003, 2004, 2007, 2009
Ireland	2002, 2004, 2006, 2007, 2008
Israel	1996, 2002, 2003, 2006, 2006, 2008, 2010
Italy	2002, 2004, 2006,
Japan	2004, 2007
Latvia	2006, 2008
Lithuania	1997
Mexico	1997, 2000, 2003, 2006, 2009
Netherlands	1998, 2002, 2004, 2006, 2008, 2010
New Zealand	1996, 2002, 2008
Norway	1997, 2001, 2002, 2004, 2005, 2006, 2008, 2010
Peru	2006
Poland	1997, 2001, 2002, 2004, 2006, 2007, 2008, 2010
Portugal	2002, 2004, 2005, 2006, 2008, 2009, 2010
Romania	1996, 2004 2006, 2008
Slovenia	1996, 2002, 2004, 2006, 2008, 2010
Spain	1996, 2000, 2002, 2004, 2006, 2008, 2010
Sweden	1998, 2002, 2004, 2006, 2008, 2010
Switzerland	1999, 2002, 2003, 2004, 2006, 2007, 2008, 2010
Taiwan	1996, 2001, 2004
United Kingdom	1997, 2002, 2004, 2005, 2006, 2008, 2010
United States	1996, 2004

Indeed there are additional factors that could affect turnout at both the individual level (such as sense of civic duty, political knowledge, etc.) and election context level (such as the competitiveness of the elections, the number of parties, the campaign activity, etc.) (cf. Franklin, 2004). As for the individual-level variables, equivalent measures are unfortunately not available in the merged dataset. Concerning the context-level variables, there should be no reason to expect additional factors to affect the estimates for the absolute and relative education coefficients, and hence the context-level controls are restricted to these five factors. In addition, the multilevel structure of the models includes random intercepts that will account for the fact that the turnout levels vary between countries and country-years. However, there is still of course a theoretic possibility that omitted variables at the country or country-year level bias the results. To handle this problem, a model with country-year fixed effects that remove all second- and third-level variance will be presented as a robustness check.

As for the dependent variable, it measures reported voting in the most recent election and the variable is coded as a dichotomy (0 = not voted, 1 = voted). The item construction follows different national standards in the CSES.[14] In the ESS, the item construction for the voter turnout question is "Some people don't vote nowadays for one reason or another. Did you vote in the last [country] national election in [month/year]?" In addition, while the CSES is carried out in conjunction with elections, the ESS might be carried out when considerable time has passed since the last election. Moreover, the turnout item measures reported voting, and it is well known that some respondents over-report voting (cf. Granberg & Holmberg, 1991; Persson & Solevid, forthcoming). However, recent research has shown that while different item constructions of the turnout question can produce different predicted levels of turnout, different item constructions correlate strongly. Most importantly, the same independent variables have been found to show significant effects using different items measuring intended, reported and validated voting (Achen & Blais, 2010). While these shortcomings regarding the measurement of the dependent variable should be acknowledged, they are irreparable at this stage. Despite these problems, the dataset constitutes the best available opportunity to study individual-level variation in turnout in a large number of countries.

As for the education variables, data on both individual-level educational attainment and aggregate levels of education is used. Regarding the individual-level variables, items on reported education in CSES and ESS are used. Most previous studies on relative education have used data on *years* of education at the individual level. However, since the length of specific educations differ between countries (and over time), it is not possible to construct a valid measure of years of education drawing on the combined CSES and ESS data. Thus, four categories of educational attainment that exist in most countries are used: (1) incomplete primary, (2) primary education completed, (3) secondary education completed, and (4) higher education completed. While it would have been preferable to further distinguish between different levels of education, country-specific differences in how education is organized and measured make it impossible.

Data on aggregate levels of education comes from the Barro & Lee Educational Attainment Dataset.[15] The data reports the share of citizens with different educational levels within five-year age cohorts in each country during every five-year period from 1950 to 2010. To construct the relative education measure, data on the share of citizens with (1) incomplete primary, (2) primary education completed, (3) secondary education completed, and (4) higher education completed within each cohort in each country at each point in time is used in combination with the individual education measures.

Modeling Strategy

Previous studies of effects of relative education have differed in their modeling strategies. NJS and Helliwell and Putnam (2007) estimate regression models that include both years of education at the individual level and aggregate mean levels of years of education. They interpret a negative effect of the aggregate measure as support for the relative education model. Campbell (2009) and Persson (2011) treat relative education as the interaction between years of education at the individual level and the level of education in the environment. This is a more feasible strategy since the theoretical model suggests an interactive relationship; as the level of education in the environment increases, the individual-level effect of education decreases. However, none of these strategies are feasible in the present article. The reason is the lack of a valid measure of years of education that is equivalent in the 37 countries. In order to model relative education using this data, we need a measure that relies on the four categories of education.

Thus, a modeling strategy similar to the one applied by Tenn (2005) is used. A measure of each respondent's rank position in the education hierarchy within each respondent's five-year cohort in each country at the time of each survey is calculated.[16] Each person is assigned a value on the relative education variable that corresponds to the midpoint of the percentile range of the person's educational level. To illustrate how the measure is calculated, take a person for whom 20% of the people in his/her cohort have no primary education, 30% have completed primary education and 20% have completed secondary education. If this person has completed secondary education, he/she is assigned the value 60 on the percentile-ranking variable. If the respondent has only completed primary education, he/she is assigned the value 35, etc. Hence, it reflects the share of people in the same birth cohort in the same country with higher, lower and similar levels of education. This measure is of course not perfect since we only know the distributions of the four main educational categories. However, it is reasonable to assume that it roughly proxies each person's relative education position. And even a rough measure allows us to evaluate whether what matters for turnout is having achieved a specific level of education or relative position in the education hierarchy.

An important advantage of this measure is that it is not perfectly correlated with birth year or age (as is NJS's relative education measure) or geographical area (as is Helliwell and Putnam's measure). We can thus control for both age and factors

at the country level without having to worry about multicollinearity. This modeling strategy produces more valid estimates than the strategies employed by NJS as well as Helliwell and Putnam since they cannot separate their relative education measures from confounding factors related to age and place. Our measure comes closer to capturing the true effect of relative education separated from these confounding factors.

The fact that individuals are clustered within different countries and country-years with different levels of voter turnout violates one of the general assumptions of regression analysis – that the residuals are uncorrelated with each other. Employing a modeling strategy that does not take the clustered structure of the data into account would likely produce inaccurate standard errors (e.g., Gelman & Hill, 2007; Goldstein, 1995). Thus, a multi-level regression model is employed in which the nested three-level structure – individuals (i), within country-years (j), within countries (k) – is explicitly modeled. Logistic multi-level regression is used as the estimator since the dependent variable is dichotomous.[17]

Results

We begin by estimating a random intercept-only model to focus on the variation at the country and country-year level (Model 1 in Table 2). The variances at both these levels are statistically significant, and multi-level modeling is thus needed to take the nested structure of the data into account. Model 2 estimates the effects of the absolute education measures together with the control variables. As regards the controls, they all run in the expected directions. Age has a positive effect, yet the quadratic effect suggests that the probability of voting decreases in old age. Men vote more than women, as do married compared to non-married (albeit this effect does not reach statistical significance). As for the contextual-level control variables, only the difference between compulsory voting with sanctions and non-compulsory voting reaches statistical significance. However, the primary purpose of this model is to see whether it shows the expected significant relationship between absolute education and voter turnout. The results clearly show that education has significant effects when modeled using the conventional absolute education approach.[18]

Having established that there is a significant relationship between education and voter turnout when education is modeled in the conventional absolute way, we now move forward to analyze the relative impact of education. Model 3 in Table 2 adds nine dummy variables indicating each respondent's relative education rank position (with the lowest decile rank position as reference category), in addition to the variables included in Model 2. Including both the relative and absolute education variables is a hard test for the relative education model. If the absolute education measures show strong significant estimates while the relative education variables are insignificant, we can conclude that what matters in relation to turnout is which specific level of education a person has and not the position in the education hierarchy. Yet if the relative education measures are significant while the absolute education measures are not, we can conclude that what matters is not the level of education as such, but rather the position in the education hierarchy.

Table 2. Effects of absolute and relative education on voter turnout (multi-level logistic models)

	(1)	(2)	(3)
Individual level controls			
Age		0.084*** (0.003)	0.089*** (0.003)
Age^2		-0.001*** (0.000)	-0.001*** (0.000)
Civil status		0.018 (0.011)	0.018 (0.011)
Gender		0.027** (0.011)	0.027** (0.011)
Contextual level controls			
Democratic system		0.085 (0.254)	0.161 (0.253)
Voting system		-0.100 (0.250)	-0.099 (0.249)
Compulsory voting, weak sanctions/without sanctions		-0.620 (0.558)	-0.674 (0.555)
Non-compulsory voting		-1.485*** (0.486)	-1.482*** (0.484)
Registration requirements		-0.291 (.693)	-0.079 (0.689)
Type of ballot		0.163 (0.279)	0.152 (0.278)
Absolute education (reference category =not completed primary)			
Primary education completed		0.163*** (0.022)	-0.154*** (0.037)
Secondary education completed		0.612*** (0.022)	-0.033 (0.060)
Higher education completed		1.177*** (0.024)	0.237*** (0.083)
Relative education rank position (reference category = 0–10)			
Rank position 11–20			0.193*** (0.033)
Rank position 21–30			0.309*** (0.038)
Rank position 31–40			0.370*** (0.043)
Rank position 41–50			0.515*** (0.047)
Rank position 51–60			0.568*** (0.053)
Rank position 61–70			0.650*** (0.058)
Rank position 71–80			0.701*** (0.066)

Rank position 81–90			0.754*** (0.069)
Rank position 91–100			0.959*** (0.079)
Constant	1.843*** (0.109)	-0.133 (0.549)	-0.191 (0.546)
Standard deviation of intercept at country level	0.651*** (0.084)	0.554*** (0.076)	0.571*** (0.077)
Standard deviation of intercept at country-year level	0.379*** (0.024)	0.439*** (0.028)	0.433*** (0.027)
Number of countries	37	37	37
Number of country-years	173	173	173
Number of individuals	275439	275439	275439
Log likelihood	-118465.	-113802.9	-113721.8

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results show that when taking the relative education measures into account, the coefficients for the absolute education variables change considerably. The significant coefficient for secondary education turns insignificant, and the size of the coefficient for higher education is considerably reduced. In contrast, the relative education measures have strong and significant effects. Since substantive interpretations are hard to draw from logistic coefficients, Table 3 presents the marginal effects of absolute and relative education derived from the fixed part of Models 2 and 3 respectively (while all other variables are held at their mean values). The marginal effects are also presented graphically in Figure 1. The estimates show that absolute education has limited importance in relation to voter turnout. Completed higher education has a modest marginal effect of 0.027. The effect of being among the 10% with highest education has an effect about three times as large (0.092). The effect of relative education is increasingly higher within every decile. To conclude, when modeling education as a relative position defined as the rank within a cohort within a country, and taking variation in both time and between countries into account, relative education has far greater explanatory value than absolute education. What matters in relation to turnout does not seem to be the specific level of education, but rather the relative rank position in the education hierarchy. Moreover, the decreasing log likelihood values across the models in Table 2 shows that the goodness of fit increases when including the relative education variables.

Table 3. Marginal effects of absolute and relative education on voter turnout

	Estimates derived from Model 2	Estimates derived from Model 3
Absolute education		
Primary education completed	0.019*** (0.003)	-0.019*** (0.005)
Secondary education completed	0.070*** (0.006)	-0.004 (0.008)
Higher education completed	0.112*** (0.009)	0.027*** (0.011)
Relative education rank position		
Rank position 11–20		0.022*** (0.005)
Rank position 21–30		0.034*** (0.007)
Rank position 31–40		0.040*** (0.009)
Rank position 41–50		0.053*** (0.004)
Rank position 51–60		0.060*** (0.005)
Rank position 61–70		0.065*** (0.005)
Rank position 71–80		0.067*** (0.006)
Rank position 81–90		0.073*** (0.007)
Rank position 91–100		0.092*** (0.007)

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

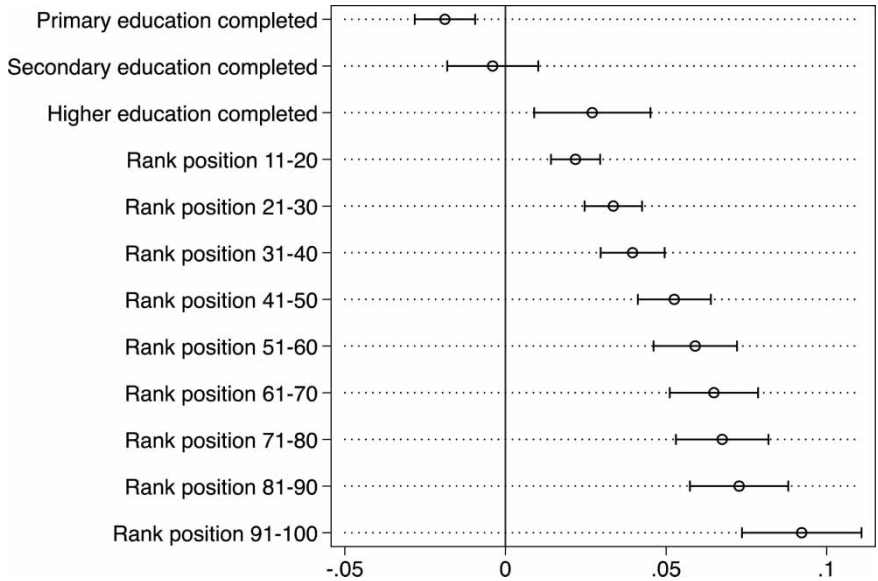

Figure 1. Marginal effects of absolute and relative education on voter turnout.

While the results presented in Tables 2 and 3 indicate support for the relative education model over the absolute education model, there are several reasons to be cautious before drawing decisive conclusions. This is especially the case when using this kind of data, compiled from different surveys in different countries and years. The results from the data presented in Table 3 are not weighted with respect to any potential form of bias. This section continues by testing for four potential forms of bias and presents robustness checks that show that the general pattern of the results hold after correction for potential biases. The four potential forms of bias are (a) bias due to sampling error within each survey, (b) bias as a consequence of the overrepresentation of respondents from some countries, (c) bias as a result of overrepresentation of respondents from some country-years, and (d) bias as a result of omitted variables at the country-year level.

To correct for the first three potential biases, three forms of weights will be applied in three separate models. We begin with internal sampling error within each survey. In some of the surveys, particular populations within countries are oversampled and the distribution of socio-demographic characteristics might not closely resemble the characteristics of the populations. Weights are provided with some, but far from all, of the surveys to correct for such bias.[19] Model 4 in Table 4 presents estimates from a model with the survey weights applied.[20] When these weights are applied, the coefficients for the absolute education variables all turn insignificant. However, the relative education measures remain statistically significant.[21]

Table 4. Robustness checks

	(4) Survey weights model	(5) Country level weights model	(6) Country-year level weights model	(7) Country-year fixed effects model
Individual level controls				
Age	0.092*** (0.007)	0.089*** (0.002)	0.087*** (0.002)	0.089*** (0.005)
Age^2	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Civil status	-0.074 (0.060)	0.003 (0.007)	0.015*** (0.006)	0.022 (0.043)
Gender	0.052** (0.023)	0.012* (0.007)	0.015*** (0.005)	0.027 (0.019)
Contextual level controls				
Democratic system	0.564*** (0.130)	-0.187* (0.101)	0.413*** (0.062)	-
Voting system	-0.125 (0.132)	-0.314*** (0.105)	0.161** (0.066)	-
Compulsory voting, weak sanctions/without sanctions	-0.671** (0.317)	-0.854*** (0.179)	-0.121 (0.154)	-
Non-compulsory voting	-1.490*** (0.230)	-1.577*** (0.168)	-1.099*** (0.126)	-
Registration requirements	-0.083 (0.289)	-0.565** (0.265)	0.406* (0.210)	-
Type of ballot	0.232* (0.118)	0.136 (0.137)	0.081 (0.069)	-
Absolute education (reference category =no completed primary)				
Primary education completed	-0.103 (0.159)	-0.239*** (0.022)	-0.158*** (0.018)	-0.154** (0.065)
Secondary education completed	-0.037 (0.257)	-0.405*** (0.035)	-0.051* (0.030)	-0.025 (0.102)
Higher education completed	0.399 (0.332)	-0.391*** (0.050)	0.235*** (0.041)	0.251* (0.144)
Relative education rank position (reference category = 0–10)				
Rank position 11–20	0.398*** (0.132)	0.278*** (0.021)	0.185*** (0.017)	0.192*** (0.058)
Rank position 21–30	0.489*** (0.173)	0.481*** (0.023)	0.319*** (0.019)	0.307*** (0.078)
Rank position 31–40	0.440** (0.194)	0.547*** (0.026)	0.363*** (0.021)	0.368*** (0.085)
Rank position 41–50	0.434** (0.188)	0.694*** (0.029)	0.496*** (0.024)	0.513*** (0.080)
Rank position 51–60	0.598*** (0.220)	0.768*** (0.031)	0.557*** (0.026)	0.564*** (0.090)
Rank position 61–70	0.710*** (0.260)	0.903*** (0.034)	0.632*** (0.029)	0.644*** (0.098)

Rank position 71–80	0.865*** (0.290)	1.085*** (0.038)	0.699*** (0.033)	0.693*** (0.109)
Rank position 81–90	0.637** (0.287)	1.219*** (0.041)	0.732*** (0.034)	0.745*** (0.120)
Rank position 91–100	0.856*** (0.321)	1.453*** (0.048)	0.943*** (0.039)	0.947*** (0.139)
Constant	-0.633* (0.362)	0.568*** (0.186)	-0.905*** (0.155)	0.198 (0.134)
Standard deviation of intercept at country level	-	0.534*** (0.029)	-	-
Standard deviation of intercept at country-year level	-	-	0.708*** (0.018)	-
Country-year fixed effects	NO	NO	NO	YES
Number of countries	37	37	37	37
Number of country-years	173	173	173	173
Number of individuals	275439	275439	275439	275439
Log likelihood	-124479.1	-286130.0	-456755.0	-113294.03

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The second potential form of bias stems from the fact that the 37 countries included in the analysis contribute with unequal numbers of respondents to the dataset. This is troublesome if respondents are overrepresented from countries where absolute education has particularly weak or strong associations with voting. The results from Model 5 presented in Table 4 show estimates from a model in which each country is weighted so that it contributes equally to the distribution of respondents.[22] We see that this weighting procedure slightly affects the results. However, and most importantly, the estimates for absolute education show small negative effects while relative education has strong and positive significant effects.

The third potential source of bias is the fact that some countries contribute with a large number of surveys while others only contribute with one or two. Hence, Model 6 of Table 4 is estimated with weights that correct the sample so that each country-year has an equal contribution of respondents to the dataset. Again we find that the general pattern holds when correcting for this potential bias. The absolute education measures show small effects, while the relative education measures show stronger and significant effects. Hence, while it is evident that these three forms of bias affect the estimates to different degrees, they do not alter the general conclusion: the relative education model gains more support than the conventional absolute education model.

As a fourth robustness check, Model 7 includes country-year fixed effects to make sure that omitted variables at the country or country-year level do not bias the results. Including the country-year fixed effects variables increases the model fit, which indicates that a number of factors at the country and country-year level that are omitted from the previous models affect voter turnout.[23] However, they have a negligible effect on the absolute and relative education coefficients. The size and significance of the coefficients for the main variables of interest are essentially the same as in Model 3.

At this point, one final issue remains to be analyzed and that is whether the effect of relative education is different in different contexts. In order to test this a model including a random coefficient for relative education was estimated. Since including the 10 relative education dummy variables are impractical for computational reasons, I use the original relative education rank variable (indicating the relative rank position on a scale from 0 to 10). Hence, Model 8 in Table 5 is a two-level model testing whether relative education has different effects in the 37 different countries by including the random coefficient for relative education (for computational reasons the country-year level is omitted in this model). The results show that the random slope is significant and the effect of relative education indeed varies between the countries (and the log likelihood value is significantly lower in this model compared to a model without the random coefficient for relative education).

Can the varying effect of relative education be explained by differences in the aggregate turnout rates? In order to test this a variable measuring the aggregate level turnout at each country year was included in the next model and interacted with the relative education variable.[24] Results from Model 9 show that this interaction is indeed significant. To better interpret the interaction effect Figure 2 illustrates the

Table 5. Testing random effects of relative education

	(8) Random coefficient model	(9) Cross level interaction model
Individual level controls		
Age	0.087*** (0.003)	0.086*** (0.003)
Age^2	-0.001*** (0.000)	-0.001*** (0.000)
Civil status	-0.085*** (0.010)	-0.026** (0.010)
Gender	0.035*** (0.011)	0.029*** (0.011)
Contextual level controls		
Democratic system	0.200 (0.300)	-0.138 (0.123)
Voting system	-0.061 (0.296)	0.134 (0.119)
Compulsory voting, weak sanctions/ without sanctions	-0.419 (0.645)	0.000 (0.277)
Non-compulsory voting	-1.611*** (0.566)	-0.454* (0.244)
Registration requirements	-0.453 (0.799)	-0.436 (0.321)
Type of ballot	0.193 (0.335)	0.052 (0.132)
Education variables		
Primary education completed	0.036 (0.035)	0.080** (0.035)
Secondary education completed	-0.217*** (0.061)	0.079 (0.061)
Higher education completed	-0.092 (0.085)	0.348*** (0.085)
Relative education rank position (0-10)	0.137*** (0.014)	0.263*** (0.033)
Aggregate turnout		7.832*** (0.225)
Relative education rank position X Aggregate turnout		-0.213*** (0.037)
Standard deviation of random slope: relative education	0.069*** (0.009)	0.057*** (0.007)
Standard deviation of intercept at country level	0.731*** (0.087)	0.277*** (0.038)
Number of countries	37	37
Number of country-years	173	173
Number of individuals	275439	275439
Log likelihood	-113565.02	-113565.02

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

predicted level of voter turnout for individuals with low and high relative education at different levels of aggregate turnout. As could be theoretically expected, the difference between individuals with high and low relative education is largest when turnout is low and the difference is smaller when turnout is high (albeit the difference remains statistically significant also at the highest levels of aggregate turnout).[25] Hence, we can conclude that relative education matters more in low turnout contexts than in high turnout contexts. It should also be noted that the standard deviation of the

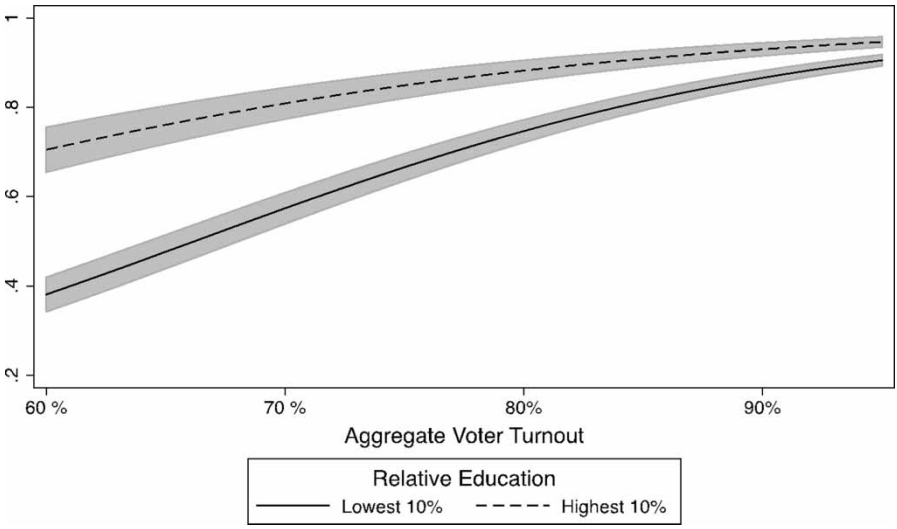

Figure 2. Predicted probability of voting for individuals with lowest and highest relative education across different levels of aggregate voter turnout (estimates derived from Model 9).

random slope for relative education is reduced when relative education is interacted with aggregate turnout. This implies that differences in aggregate turnout account for at least some of the difference in the effect of relative education between different countries.

Conclusion

This article deals with the paradoxical relationship between education and voter turnout. While the relationship is well established at the individual level, it does not seem to hold at the aggregate level. The article is the first to use intra-birth cohort measures of relative education on cross-country survey data to test the relative education model as a possible solution to the paradox. The results confirm that what matters is not so much people's absolute level of education as their rank position in the education hierarchy. The relative education model solves the paradox concerning the relationship between education and turnout; if there is little direct causal effect of education on turnout, we have no reason to expect that increased levels of education at the aggregate level should result in higher levels of voter turnout. One important conclusion from this study is that one must be careful when drawing inferences about causal relationships from cross-sectional data. Education has often been considered to be a direct cause of voter turnout, yet the results presented here suggest that the direct effect is quite marginal.

While this study does not include any data on the alleged causal mechanism, a few words ought to be said about it. According to the relative education model, social

network centrality connects education with political participation. By obtaining higher education, individuals get in contact with other highly-educated people and form social networks. Access to high-status social networks encourages political participation, mainly by increasing the likelihood of getting recruited. In other words, highly-educated people are more likely to be tied to networks that consist of active political participants, and they are therefore more likely to be recruited to participate themselves. Relative education determines who are the people with these important network ties.

If we regard the estimates for relative education as the effect of education via social status and the estimates for absolute education as the "direct" causal effect of education (via the mechanisms civic skills, political interest, etc.), we should reach the conclusion that the relative effect via social status is much stronger than the direct effect. The education effect on voter turnout is not a phenomenon operating solely at the individual level; rather the effect of education depends on how many others have which levels of education. This conclusion adds further evidence to a larger literature suggesting that education has no direct causal effect (e.g., Berinsky & Lenz, 2011; Kam & Palmer, 2007). Policy implications differ greatly depending on whether the absolute or relative education model is correct. While many scholars have attributed a central role to the education system in determining a country's level of voter turnout, this role is likely overestimated if the relative education model is correct.

While the results clearly favor the relative education model, this article is not likely to be the last word in the debate. Instead, we should anticipate further studies drawing on cross-country data. In the future, there will hopefully be better data that covers larger time spans and more countries. A promising way to accomplish this would be to harmonize the many existing election studies around the globe. Clearly, more robust results could also be obtained if advances were made in the way education is measured in different countries, providing more reliable and equivalent measures of years of education. Yet until further evidence is presented, the most reasonable conclusion is that education is first and foremost a proxy for social status and the social network that surrounds higher status individuals, not a variable that has direct absolute effects on voter turnout.

Acknowledgements

I am grateful for helpful comments from Peter Esaiasson, Mark Franklin, Mikael Gilljam, Henrik Oscarsson, and three anonymous reviewers.

Notes

1. However, a previous comparative analysis using the ESS was presented in a working paper by Campbell (2006). Campbell presents support for relative education model by modeling the interaction between the individual level years of education with the aggregate level of education in each country.
2. Yet other researchers argue that education is a proxy for pre-adult factors that affect both educational choice and political participation in adulthood, most importantly early political socialization in the

home environment (e.g., Achen, 2002; Alwin & Thornton, 1984; Andolina et al., 2003; Beck & Jennings, 1982, 1991; Jennings & Niemi, 1968, 1974; Kam & Palmer, 2008; Langton & Jennings, 1968; McIntosh et al., 2007; Westholm, 1999). Yet others claim that education is a proxy for intelligence (Deary et al., 2008; Luskin, 1990), genetic factors (Alford et al., 2005; Fowler et al., 2008) or personality types (Mondak & Halperin, 2008; Mondak et al., 2010).

3. Information about the surveys and data is available at http://www.europeansocialsurvey.org/ and http://www.cses.org/.
4. Information about the Freedom House Index is available at http://www.freedomhouse.org/.
5. Moreover, Australia and Thailand were excluded since in these two countries 98 and 99% of all respondents in the surveys report that they voted and there is hence no meaningful variation in the dependent variable.
6. The variable is coded 0 for females and 1 for males.
7. The variable is coded 1 for those who are married and 0 for those who are single/divorced/widowed.
8. The most important individual level control variable missing from the analysis is household income. There are several reasons why this item is not included. First, it is measured in different ways in the different countries. Second, the variable includes a large number of non-responses (54,000), so including this variable would substantially decrease the number of respondents included in the analysis. As a robustness check, all models have been re-estimated with income as control variable. In these models, absolute education has an even weaker effect and relative education has a stronger effect than in the models presented in the article. Of course it would be preferable if more controls could be included. However, for variables such as occupation, urban/rural residence and political interest, no equivalent variables are available in all datasets.

   However, two additional individual level variables that might be related to education are included in a majority of the surveys: church attendance and party identification. Yet including these variables reduces the number of country-years by 14, from 173 to 159. All models have been re-estimated with controls for church attendance and party identification. The size and significance of the absolute and relative education coefficients are nearly identical.
9. The variables indicate whether each country has compulsory voting with strong sanctions, with weak/no sanctions, or no compulsory voting. The source of the coding is http://www.idea.int/vt/compulsory_voting.cfm. Compulsory voting with strong sanctions is the reference category in the models.
10. The variable indicates whether the country has parliamentarism or presidentialism. The source of the coding is http://www.bu.edu/sthacker/data.htm. The variable is coded 0 for presidentialism/semi-presidentialism and 1 for parliamentarism.
11. The variable indicates whether the parliamentary system is proportional or majoritarian. The source of coding is http://www.bu.edu/sthacker/data.htm. The variable is coded 0 for majoritarian voting and 1 for proportional voting.
12. The variable indicates whether the type of ballot is open or closed. The source of the coding is Gallego (2010). The variable is coded 0 for open ballot and 1 for closed ballot.
13. The variable indicates whether registration requirements are absent (coded 0), compulsory (coded 0.5), or voluntary (coded 1). The source of the coding is Gallego (2010).
14. Additional information about how the turnout variable is constructed in different countries can be found in the codebooks at http://www.cses.org.
15. Data and documentation are available at http://www.barrolee.com/.
16. The Barro and Lee data only reports levels of education in each cohort in each country every five years. Hence, when calculating the relative education measure, the Barro and Lee data from 1995 is used for the surveys from 1996 to 1999, the data from 2000 is used for the surveys from 2000 to 2004, the data from 2005 is used for the surveys from 2005 to 2009, and the data from 2010 is used for the surveys from 2010.
17. The results are produced by the STATA command XTMELOGIT.
18. However, additional analyses show that there is substantial variation in how much education is related to voter turnout in the 37 countries. In most of the countries, there is a significant difference between the low and high educated. Analyses from separate logit models in each country show that in the

United States, Norway, the Netherlands, Poland and Switzerland, the differences in turnout are very large between the low and high educated, while in countries such as Belgium, Spain and Sweden the differences are much smaller (albeit significant). Yet the high educated do not vote to a higher degree than the low educated in all countries. As a matter of fact, there are no significant differences in the predicted probability of voting between low and high educated individuals in Bulgaria, Croatia, Cyprus, Greece, Iceland, Italy and Romania. Future research is encouraged to explore why education does not seem to have any significant relationship with turnout in these countries.

19. See the ESS and CSES web pages for more information on the weights supplied. For CSES, a combination of the "Polity weight: Sample" and "Polity weight: demographic" is used, and for ESS the "Design weights" is used.
20. Due to the complexity of applying survey weights in multi-level models (see, e.g., http://www.stata.com/bookstore/stata12/pdf/xt_xtmixed.pdf), the weights are applied for illustrative purposes in an ordinary logit model with clustered standard errors at the country level.
21. Marginal effects of the education variables are not shown for the robustness checks due to space constraints. However, the marginal effects tell a similar story as the logit coefficients in Table 4: the absolute education variables show weak effects while the relative education variables show stronger and significant effects.
22. Models 5 and 6 are estimated using the XTLOGIT command in STATA12. The models are two-level models. Model 6 is takes into account the country level and its corresponding weights and Model 7 takes into account the country-year level and its corresponding weights.
23. The estimator is logistic regression with clustered standard errors at the country-year level.
24. Aggregate voter turnout was calculated as the share of voters at each country-year in the dataset and might thus deviate from the official levels of turnout in the elections.
25. Note that two countries with virtually universal reported turnout were omitted from the data.

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