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How does education affect adolescents' political development? ☆

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ABSTRACT

This paper employs a between-grades regression discontinuity design to estimate the causal effect of education on political knowledge, intention to participate and democratic values. Using data on attitudes and knowledge among about 30,000 students from Greece, Norway, Slovenia and Sweden, we employ a fuzzy regression discontinuity design in which we exploit the exogenous variation related to school entry age. By comparing students who are born around the New Year cut-off point we estimate the causal effect of the ninth year of schooling. Results show that an additional year of schooling has no detectable effect on political knowledge, democratic values or political participation.

1. Introduction

Years of education is a standard control variable in political science research that is regularly found to be positively correlated with outcomes such as political knowledge and participation. Indeed, one of the most well established findings in the political behavior literature is that citizens with higher education generally have greater political knowledge, higher levels of political participation and a stronger commitment to democratic core values (e.g. Nie, Junn, & Stehlik-Barry, 1996; Schlozman, Verba & Brady, 2012; Verba, Schlozman, & Brady, 1995; Wolfinger & Rosenstone, 1980). However, few studies have been able to actually estimate the causal impact of an additional year of schooling. Even fewer studies have been able to trace the psychological developmental process that students are going through as they progress through the educational system.

While most previous studies have employed research designs and methods providing little opportunity for considering the causal effect of education, a number of recent studies have used more refined methodological approaches (see Persson 2015 for an overview). These studies have primarily focused on political participation have employed techniques such as instrumental variable approaches (e.g., Berinsky & Lenz, 2011), field experiments (e.g., Green, Aronow, Bergan, Greene, Paris, & Weinberger., 2011; Sondheimer & Green, 2010), and matching analyses on panel data (e.g., Kam & Palmer, 2008; Persson, 2014a; Tenn, 2007). This paper adds another approach to the estimation of causal effects of education to this field, i.e. a between-grades regression discontinuity design that is explicitly aimed at estimating the causal effect of *an additional year* of education. We use survey data on attitudes and knowledge among about 30,000 students from Greece, Norway, Slovenia and Sweden, collected in the eighth and ninth grades. In these countries, children start school at an age determined by the calendar year in which they were born. By comparing students who were born around

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the New Year cut-off point, i.e. whose age difference is negligible while their educational attainment differs by one year, we estimate the causal effect of the ninth year of schooling. The regression discontinuity design, exploiting the exogenous variation related to school entry age, is an identification strategy that has not previously been used to estimate effects of education in this area. However, the approach has been used in educational research to test the impact of education on outcomes such as student achievement (Cahan & Davis, 1987; Kyriakides & Luyten, 2009; Luyten, 2006; Luyten & Veldkamp, 2011).

With the research design employed in this paper we can test whether one additional year of education (the ninth year of schooling) affects civic knowledge, intentions to participate in politics and democratic values. Within the political science literature on education effects, three models dominate the discussion: the absolute education model, the relative education model and the pre-adult socialization model. The absolute education model claims that education has a direct causal effect on civic outcomes such as tolerance, democratic norm adherence and political participation, attentiveness, and knowledge. According to this model, the causal influence of education on civic outcomes is mediated by civic skills and cognitive ability (cf. Condon, 2015). Nie et al. (1996) refer to this mechanism as a 'cognitive pathway', i.e. what individuals learn in school has positive effects on their cognitive ability with downstream effects on a wide array of political attitudes and behaviors (cf. Jackson, 1995; Lewis-Beck et al., 2008). If this model is correct we should expect to see positive effects of an additional year of education on the outcomes under study. Indeed, if they actually learn something in social science education during the ninth year, it would be reasonable to expect that it at least affect political knowledge.

According to the second model that is interchangeably referred to as the relative education model, or the sorting model, education has a more indirect effect on civic outcomes. The argument states that educational attainment will positively influence individuals' social and economic status—i.e. occupational prominence, wealth, income, and membership in voluntary associations. These intervening factors will in turn determine social and political network centrality. Thus, individuals with higher relative education will be more closely connected and exposed to networks that boost participation, encourage commitment to democratic values and expose individuals to information that increases knowledge. Hence, the education effect runs through a 'positional pathway' in which social status functions as the causal mechanism (Campbell, 2009; Nie et al., 1996; Tenn, 2005; Persson, 2014b). Since we estimate the impact of education while students are still in school we cannot rule out the possibility that education has effects via a 'positional pathway' after students have left school. While we cannot strictly test any causal mechanism, any significant results are likely driven be cognitive and psychological process at play while students make progress through the educational institutions.

Although the absolute and the relative models differ with respect to the specific mechanism linking education and civic outcomes they are still united by the belief that the relationship between the two constructs is causal in nature. The pre-adult socialization model offers a revisionist account of this belief. This model suggests that the relationship between education and civic outcomes—be that via the cognitive or the positional pathway—is confounded by pre-adult factors influencing both educational choices and political traits such as participation, knowledge and democratic values. More exactly, according to this approach education is a proxy for factors such as family socio-economic status, the political socialization in the home environment and individual differences in cognitive ability and personality traits, and *not* a cause of civic outcomes (cf. Jennings and Niemi, 1974; Kam & Palmer, 2008; Langton & Jennings, 1968; Luskin, 1990). Since such factors are often not included in studies of the returns to education, the estimated effects of education on civic outcomes are most likely upwardly biased in the bulk of previous research. To overcome this problem a number of recent studies have employed research designs and statistical techniques appropriate for testing whether the education-civic outcome link is causal or not. Using changes in compulsory schooling laws across different regions and different periods in time as an instrument for educational attainment Milligan, Moretti, and Oreopoulos (2004) find a strong positive effect on voter turnout in the US but not in the UK. In the same fashion, Dee (2004) employs the variation in compulsory schooling laws across US states to estimate the effect of education on turnout. Moreover, he also uses the distance to colleges as an instrument for education. Both instruments yield the same result: education is positively and significantly related to voter turnout. Berinsky and Lenz (2011) instead use the Vietnam-era draft to instrument for college education and find no evidence that education positively influences voter turnout.

Another set of studies have used matching techniques to evaluate the relationship between education and political outcomes. After matching on a range of pre-adult experiences and influences Kam and Palmer (2008) find no significant differences in political participation (they use an index of different participatory acts) between college attendees and non-attendees in two US samples. However, using the same data but slightly different matching techniques Henderson and Chatfield (2011) instead report a positive and significant effect of college attendance on voter turnout. In contrast, Persson (2014a) reaches similar results to those of Kam and Palmer (2008) using data from a British cohort study. After matching on a rich set of variables such as childhood cognitive ability and family socioeconomic status, the effect of education on political participation is insignificant.

Finally, a number of studies have used field experiments to account for pre-adult confounders. Sondheimer and Green (2010) exploit three experiments in which educational attainment was randomly assigned through different interventions such as smaller classes, extra mentoring and pre-school activities. They find that treated individuals have a higher probability of graduating from high school, which leads to significant increases in turnout rates. Green et al. (2011) represents the study in which the outcomes under study are most similar to this study, i.e. they focus on civic knowledge and political tolerance. In a study where more than 1000 students were randomly assigned to an enhanced civics curriculum intended to foster a greater understanding of civil liberties and rights they report a positive influence on civic knowledge but no effect on political tolerance. This finding suggests that democratic values might be strongly rooted in early socialization or heritable factors, while civic knowledge is more dependent on learning processes promoted by the schooling environment.

Moreover, studies analyzing natural experiments in the form of school reforms from Norway (Pelkonen, 2012) and Germany (Siedler, 2010) do not find support for a causal link between education and civic outcomes.

In comparison with earlier studies, most of which have focused on observational evidence based on cross-sectional surveys, this shift towards instrumental variable approaches, matching techniques and field experiments has considerably furthered our understanding of the relationship between education and civic outcomes. Still, however, we are far from any conclusive answers to whether education is causally related to civic outcomes and, if so, via which pathways its influence is mediated.

First of all, as is obvious from the brief review above, the findings are conflicting, both across and within studies. Some studies report positive and significant effects of education on political participation and other civic outcomes (Dee, 2004; Green et al., 2011; Henderson & Chatfield, 2011; Milligan et al., 2004; Sondheimer & Green, 2010) while others find no support for the conventional wisdom that educational attainment is causally related to civic outcomes (Berinsky & Lenz, 2011; Kam & Palmer, 2008, 2011; Persson, 2014a).

Second, all three approaches—instrumental variable analysis, matching and field experiments—can and have been criticized on methodological grounds. As for the IV approach the validity of the instruments is crucial. Above all, the instrument should only be related to the outcome via its effects on educational attainment. For instance, it may very well be that differences in compulsory schooling laws across states are related to aggregate state-level differences in norms regarding political engagement. The credibility of estimates based on matching techniques is heavily dependent on the availability and measurability of all relevant possible confounding factors. However, and as acknowledged by Kam and Palmer (2008), even the best data sources the political science field can muster to date, including rich information on parents and children in several panels, are still a far cry from the ideal data needed. Concerning randomized experiments of the type used by Sondheimer and Green (2010) the key identification assumption is the exclusion restriction: the condition that the outcome is a function of changes in educational attainment and that treatment assignment in the form of interventions such as smaller classes, extra mentoring and preschool activities does not influence the outcome directly. However, apart from the potential effects of such interventions that are mediated by increases in educational attainment, it is possible that the experience of being provided with extra resources during the school years can make persons view society and its institutions in a more positive light which, in turn, may increase the willingness to become politically active and knowledgeable. If this is the case the estimated effects of education on political participation will be biased upward.

Third, the studies that do actually find a positive effect of education on different civic outcomes are silent about the causal mechanism in terms of whether the relationship is mediated via the cognitive or the positional pathway. For instance, Sondheimer and Green (2010) discuss possible mechanisms driving their finding that childhood school interventions positively influence adult turnout. They propose three hypotheses stating that education enhances civic skills, increases one's general interest in politics, and expands one's social network. But they are not able to empirically test any of these mechanisms.

Against this backdrop, the current study brings important insights to the debate on education effects by exploiting the discontinuity related to school starting age. The approach builds on a simple idea: whereas the age difference between two students born on each side of the New Year is negligible, their educational attainment typically differs by a whole year. Provided that there is no systematic sorting of births around the cut-off—so that children born in January are not systematically different from those born in December—we can obtain the causal effect of one additional year of schooling by comparing the outcomes of individuals born close to the cut-off. More technically, years of schooling can be considered as good as random at the New Year cut-off, which provides us with a good opportunity to estimate the causal effect of an additional school year. To do so, we use high quality survey data from a major comparative survey on attitudes and knowledge covering about 30,000 students from Greece, Norway, Slovenia and Sweden.

This paper will proceed as follows. We begin by discussing the regression discontinuity framework and issues of identification. In the next section we present the data. The following section reports the results and numerous robustness checks and tests of heterogeneity. Finally, we conclude with a discussion of the main findings.

2. Modeling strategy

Assessing the effect of education on adolescents' political development is complicated for at least two reasons. First, as we discussed in the introduction, education may operate as a proxy for important, but difficult to observe, childhood experiences and predispositions. Second, the close relationship between school grade and age makes it difficult to separate the individual effects of these two variables. In this paper we attempt to overcome these problems by applying a regression-discontinuity (RD) approach (see, e.g., Lee & Lemieux, 2010). More precisely, we use the fact that students in the eighth and ninth grades took identical tests in the survey to estimate the following outcome equation:

$$Y_{ij} = \alpha_j + \beta G_{ij} + f^k(M_{ij}) + \varepsilon_{ij}, \qquad (1)$$

where the subscripts i and j refer to individuals and countries, respectively. Y is the dependent variable of interest, α_j denotes country fixed effects, G is a binary indicator that takes on a value of 1 for students attending the ninth grade, and f^k, finally, represents a kth order polynomial function of the variable month of birth (M). The basic idea behind this set-up is to utilize the fact that although individuals born in December year t and in January year $t+1$ are almost of the same age, the individuals born in December have had one more year of schooling at the time of the survey. Provided that the timing of birth is not subject to systematic sorting around the New Year cut-off we should further expect treatment status (ninth grade attendance) to be *as good as randomly* assigned around the cut-off. Therefore, any differences in outcomes found between children born in December and January could be interpreted as the causal effect of one more year of schooling.

However, in order to be able to interpret the coefficient β in Eq. 1 as the causal effect of an additional year of education two problems must be addressed. First, the polynomial of birth month must be sufficiently flexible to ensure that no age effects are picked up in β, so that the grade effect is really identified from the December-January comparison. To ensure that this is the case we will thoroughly examine the robustness of our results with respect to the choice of the polynomial function f^k.

Second, we need to take into account that not all individuals start school the year they are supposed to, i.e., some start a bit early and some a bit late. More technically speaking, since the probability of attending the ninth grade does not go from 0 to 1 at the December-January cut-off we have a so-called fuzzy regression discontinuity design. The problem here is that because there are reasons to suspect that both the early and the late school starters differ from other individuals in their birth cohorts a direct comparison of individuals in the eighth and ninth grades may provide biased results. Following the current standard in the literature we deal with this problem through a two-stage instrumental variable approach in which the December-January cut-off will be used to instrument G in Eq. 1 (see, e.g., Lee & Lemieux, 2010). More formally, we estimate the following first stage equation:

$$G_{ij} = \delta_j + yC_{ij} + g^k(M_{ij}) + \mu_{ij}, \tag{2}$$

where g^k is a polynomial function of order k, and C is a binary indicator taking on the value of 1 for all respondents born before 1st of January 1995. In the second stage, we then replace G in Eq. 1 with the linear predictions from Eq. 2. We expect the variable C to meet the assumptions of a valid instrument. That is, the variable should be highly correlated with ninth grade attendance, whereas it should be uncorrelated with the error term (ε) in Eq. 1. Empirical evidence supporting both these assumptions will be presented in the next section.[1]

[1] It should be noted that the fact that the dependent variable G in Eq. 2 is binary rather than continuous does not cause any additional difficulties here. The two-stage least square estimator is known to be consistent also when the endogenous variable of interest is binary (Wooldridge, 2002:622). However, if we are willing to invoke additional modeling assumptions it is possible to improve statistical efficiency by estimating a treatment effect model of the type suggested by Heckman (1978). We do this as a robustness check.

3. Data

We use data from the 1999 Civic Education Study (CivEd) and the 2009 International Civic and Citizenship Education Survey (ICCS). The surveys were carried out in 31 (CivEd) and 38 (ICCS) countries respectively, and administered by the International Association for the Evaluation of Educational Achievement (IEA). Both waves were primarily targeting students in the eighth grade. However, in four countries—Greece, Norway, Slovenia and Sweden—students in both the eighth and ninth grades were surveyed in the 2009 ICCS study. Furthermore, the Swedish sample from the 1999 CivEd study also included both eighth and ninth graders. Since the research design we use requires information from two adjacent grades with information on month of birth for the students our empirical analysis will be based on these five studies. We will primarily focus on the four countries available in the 2009 ICCS data and use the Swedish sample from the 1999 CivEd survey to corroborate our findings.[2] To our knowledge these are the only studies including the necessary information—adjacent grades, information on month of birth and information on civic outcomes—that exist and can be used for this kind of analysis.

In Greece, Norway and Slovenia children start school the year that they turn six years old, while in Sweden they start the year they turn seven. All the four countries in the study have a common curriculum without tracking up to the ninth grade. We should thus expect to see no drastic differences in the curriculums of the eighth and ninth graders within each country.

As mentioned in the introduction it should be emphasized that we do not measure the effects of education in general, but rather the effects of the ninth grade. Moreover, we could expect that in addition to increased generic skills and cognitive capabilities, it is the social science studies in the ninth grade that should matter for civic outcomes. Therefore it is of interest how much social science studies the curriculums in the different countries do offer. In Greece, students have 90 h of social studies in grade eight and 113 h in grade 9. In Norway students have 249 h spread over grades 8 to 10 (about 83 h each year). In Slovenia students have 118 h of social science in grade eight and 96 h in grade 9. And in Sweden, students have 885 h spread over the first 9 years (about 98 h per year).[3]

The ICCS and CivEd studies focused on civic knowledge, citizen participation and democracy as well as teaching related to these issues. The surveys aimed to achieve national representative samples. Sampling was made using a stratified two-stage probability design in which schools were first sampled and then classes were sampled within schools (see Schulz, Ainley, & Fraillon, 2011 for additional information about the sampling procedure). Students from the eighth and ninth grades came from the same schools. The samples include about 6000 students in each country.

[2] Data is publicly available at http://rms.iea-dpc.org/.

[3] For additional details on the curriculums see: http://eacea.ec.europa.eu/education/eurydice/documents/facts_and_figures/taught_time_EN.pdf.

To measure civic knowledge we use a summary index of knowledge questions.[4] Unfortunately, IEA has not publicly released the questionnaire items. The summary scale consists of standardized national Rasch scores with a mean of 150 and SD of 10 in each country (Brese, Jung, Mirazchiyski, Schulz, & Zuehlke, 2011). Hence, the measure is suitable for comparison within countries (between grades) but not between countries.[5] According to the ICCS technical report the knowledge test included 80 items intended to measure "civic and citizenship knowledge, analysis, and reasoning" (Schulz et al., 2011, p. 17). The questions are of a general character and are designed with the intention to capture generic cognitive capabilities that should reasonably be expected to be influenced by effective social science education.

As for intended political participation we use an index consisting of three questions regarding whether the respondent thinks that, as an adult, s/he will: (a) vote in elections; (b) join a political party; and (c) stand as a candidate. The response alternatives were coded on a four-point scale running from "I will certainly do this" (1) to "I will certainly not do this" (4). The resulting additive index was rescaled to vary between 0 (low degree of intended participation) and 100 (high degree of intended participation). We focus on these forms of participation since they cover core activities in representative democracy and the questions were administered in all five surveys.[6] We acknowledge that there might be skepticism against measuring intended political participation rather than actual participation since we cannot say for sure to what degree intentions will turn into behavior. However, studies comparing intended, reported and validated voting show that the different measures correlate strongly (Achen & Blais, 2014; Granberg & Holmberg, 1990).

Regarding democratic values, we use an index constructed of four items. These items measured students' opinions on the following statements: (a) "everyone should always have the right to express their opinions freely"; (b) "people should always be free to criticize the government publicly"; (c) "all citizens should have the right to elect their leaders freely"; and d) "people should be able to protest if they believe a law is unfair". The response alternatives ran from "strongly agree" (1) to "strongly disagree" (4) and the resulting additive index was rescaled to vary between 0 (maximum disagreement) and 100 (maximum agreement).[7]

The Swedish data from CivEd 1999 provides identical items of intended political participation but lacks comparable measures of democratic values. The 1999 data also include knowledge questions covering both knowledge of content and interpretation. A number of these questions have been publicly released (the items are presented in Schulz and Sibberns (2004), p. 237–241). The items ask about, for example, the purpose of laws in society, what constitutes a political right, what discrimination is, who should govern in a democratic system, and the major purpose of the United Nations. They also include questions related to interpreting political messages and election campaign material. In addition to an index based on maximum likelihood estimation, similar to the knowledge measure found in ICCS 2009, the 1999 data also includes a variable measuring the number of correct answers on the 36 knowledge questions. The correlation between the mle-index and the raw index is 0.92 and Cronbach alpha for the raw summary scale is 0.91. We present results from analyses using both of these knowledge measures as a robustness check.

To check the validity of our research design and test the heterogeneity of the treatment effect we use a number of family background characteristics; more specifically we use dummy indicators for parental education, parental occupation and number of books at home. Parental education is defined as high (1) for respondents who have at least one parent with post-secondary education and low (0) for those who have parents with lower education. Parental occupation is defined as high (1) for respondents with at least one parent with a white color occupation (ISCO codes beginning with 1, 2 or 3) and low (0) for the rest. Number of books at home is defined as high (1) for respondents with more than one hundred books at home (about 48% of the students in the pooled sample) and low (0) for those with fewer books at home. Number of books at home is included since it correlates strongly with socio-economic status characteristics such as parents' education (Evans, Kelley, & Treiman, 2010). Furthermore, the indicator has the advantage that it yields lower levels of non-response among adolescents (Beaton, Mullis, Martin, Gonzalez, Kelly, & Smith, 1996).

The datasets also include information on birth-month, birth-year and which grade the students' were currently attending. For the ICCS sample we use the 1994–1995 New Year as our cut-off since children born in 1995 were attending grade eight while children born in 1994 were placed in grade nine at the time of survey.[8] The majority of the surveys were administered in March 2009, which implies that what we actually measure is the causal effect of the end of the eighth grade (from March until the summer break) and the beginning of the ninth grade (from the summer break to March): in other words, one additional year of schooling.

4. Results

The presentation of the results will proceed as follows. First, we look at analyses related to the validity of the key assumption underlying the regression discontinuity design. Second, we graphically present our main results. Third, we present results from 2SLS instrumental variable regression models with different levels of flexibility regarding how to control for age. Fourth, we present a number of robustness checks showing that our results are not sensitive to specific

[4] The variable nwlciv in the ICCS dataset.

[5] To handle this problem all our models will include country fixed effects.

[6] Cronbach alpha for this index in the ICCS sample is .53.

[7] Cronbach alpha for this index in the ICCS sample is .62.

[8] Since the children in Sweden start school one year later than in the other three countries, the relevant cut-off in Sweden for the ICCS sample is the 1993–1994 New Year. Analogously we use the 1983–1984 New Year as cut-off in the Swedish CivEd sample from 1999.

Table 1
Distribution of birth years among, pooled sample (percent).

Year of birth	Grade 8	Grade 9
1992	0.0	0.1
1993	0.2	3.4
1994	3.7	94.3
1995	94.6	2.2
1996	1.6	0.0

Note: In Sweden students start school one year later than in the other countries. Hence, one year has been added to the Swedish birth years to reflect the number of students who start school the “correct” year.

Table 2
Distribution of birth months (percent).

January	8.2
February	7.6
March	8.7
April	9.0
May	8.8
June	8.4
July	8.9
August	8.5
September	8.1
October	8.2
November	7.5
December	8.2

model specifications and choices of bandwidth around the cut-off. Fifth, we look at the heterogeneity of the results by analyzing whether the estimates differ across students with different family background characteristics.

As mentioned earlier we do not have a traditional regression discontinuity design with a sharp cut-off forcing students on each side of the break to attend different grades. The reason is that some students enter one year early while others start one year later. Moreover, grade retention results in additional students attending the “wrong” grade. But despite these threats to the design, being born on either side of the cut-off strongly influences grade placement which provides sufficient leverage for a fuzzy regression discontinuity design. We begin by looking at the shares of students attending grade eight and nine who started school at the normal school age. Table 1 shows that in the pooled sample over 94% were born in the ‘correct’ year. Tables A1–A5 in the Online Appendix present the corresponding distributions by country/year. Norway has the largest share of students entering school the correct year (about 98%) while Greece has the smallest share (about 92%). Hence, birth year is a strong predictor of grade placement. This claim is substantiated by the results from the first stage regressions presented in Tables 4 to 6 below.

Next, we examine the plausibility of the key assumption underlying the regression discontinuity design, i.e., the absence of strategic sorting around the threshold. In this case, it would for example be problematic if parents with certain background characteristics tended to time their birth of children to specific periods during the year. For instance, more highly educated parents may try to avoid having children towards the end of the year and instead try to time the births of their children to the beginning of the year. Such patterns could cause bias in our analyses rendering the research design inapplicable (cf. Fredriksson and Öckert 2014).

Table 2 presents the share of students by birth month. If births were spread equally over the months of the year, 8.3% of the students would be born in each month. It is evident that a larger proportion of students are born between March and August, but, more importantly, there is no large difference in the number of children born before and after the New Year cut-off. Table A6 in the Online Appendix presents corresponding information by country; while there is some variation between the countries, no country shows any substantial overweight in the share of students born in January compared to December. [9]

A more direct test of the validity of the assumption that the instrument is uncorrelated with the error term in Eq. 1 involves regressing the instrument on a number of important family background characteristics, i.e. parental education, parental occupation and books at home. Table 3 presents results from regression models testing the impact of these factors separately (models 1–3) and jointly (model 4). Given that the oldest students in the sample are about 2 years older than the youngest students, the older students do, on average, have older parents. Older parents are likely to have achieved, on average, a slightly higher educational level and to have better occupations. Naturally, we do not want the cut-off variable to account for that and thus we need to control for age to pick up such influence. That is, what we are interested in is whether there are any systematic differences in students' family background characteristics between those born on either side of the cut-off after controlling for the impact of age. Hence, there is a need to flexibly control for age. To control for age effects we include first and second order polynomials for birth months interacted with the cut-off variable.[10] The models also include country fixed effects. Since the cut-off indicator is coded so that 1 equals children born in 1994 and 0 equals those born in 1995 a negative coefficient means that children of parents with more books at home, higher occupational status and longer education are more likely to be born at the beginning of 1995 than at the end of 1994. Since the coefficients and the standard errors are very small we have multiplied them by 1000 to facilitate interpretation. Both the p-values for joint significance reported in the third row from the bottom of the table and the individual coefficient estimates point in the same direction. When controlling for month-of-birth effects there are no systematic differences between students on either side of the cut-off regarding books at home, parents' occupation or parents' education. Tables A7–A11 in the

[9] We have excluded 37 individuals from the pooled sample who attended the eighth or ninth grades 2 years too early or 2 years too late in order to not get a too wide bandwidth window.

[10] Including the first and second order polynomial for birth month interacted with the cut-off corresponds to our main specification of the effect of ninth grade attendance on political outcomes presented in the next section. The models reported in Table 3 are restricted to pupils born in 1994 and 1995 (1993 and 1994 in Sweden).

Table 3
Balancing of covariates.

Main independent variables	1	2	3	4
Number of books at home	0.003			0.001
	(0.010)			(0.011)
Mother's occupation		0.012		0.011
		(0.011)		(0.012)
Father's occupation		−0.003		−0.004
		(0.012)		(0.013)
Mother's education			0.011	0.009
			(0.013)	(0.014)
Father's education			0.007	0.008
			(0.014)	(0.014)
Constant	0.449*	0.448*	0.453*	0.452*
	(0.018)	(0.018)	(0.018)	(0.018)
Month of birth controls:				
Second order	YES	YES	YES	YES
Interacted with cut-off	YES	YES	YES	YES
Country dummies	YES	YES	YES	YES
p-value of *F*-test	.750	.546	.433	.710
Number of individuals	23,984	22,844	22,537	21,674

Note: OLS regression with robust clustered standard errors at the school class level (954 clusters). * $p < 0.05$.

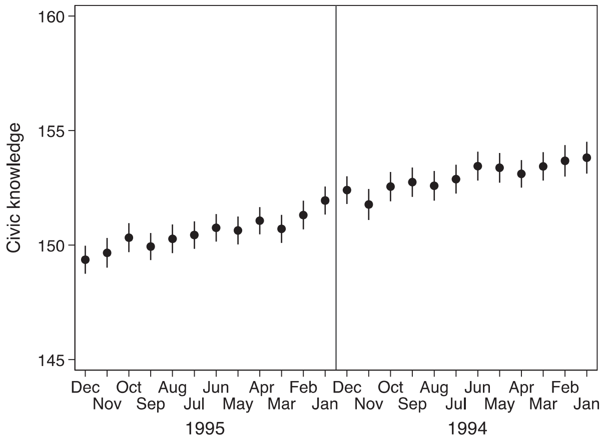

Fig. 1. Civic knowledge by birth month.

Online Appendix present equivalent estimates for each country/year. The main pattern is, once again, a lack of systematic differences in family background characteristics between students born on either side of the cut-off.[11]

Next we move on to the graphical presentation of the main results. Figs. 1–3 show mean levels and 95% confidence intervals (+/− 1.96 standard errors) of civic knowledge (1), intentions of political participation (2) and democratic values (3) for students in the pooled sample born in 1994 and 1995 by month of birth. We make no distinction between which grades they attend and show only the average by birth month. The further to the right on the *x*-axis, the older are the students. Looking at Fig. 1 it is evident that civic knowledge increases monotonically with age. The difference in the mean knowledge level between those born in January 1994 and December 1995 is about 10 units. More important for our purposes is the lack of a jump in civic knowledge around the cut-off (the vertical line). Thus, the message from this graph seems pretty clear. There is an average difference in civic knowledge between those attending the eighth and ninth grades. However, this increase in knowledge does not appear to be attributable to the extra year of schooling but is instead fully accounted for by age differences.

The graphs in Figs. 2 and 3 also show the absence of positive effects of an extra year of schooling. There are no evident discrete shifts in levels of intended political participation (Fig. 2) or democratic values (Fig. 3) when comparing those born early in 1995 and those born late in 1994. However, unlike the case for civic knowledge, there is no support for a positive age effect on these outcomes. Nevertheless, as pointed out in the methods section, simple comparisons by birth month such as the ones presented in Figs. 1 to 3 may lead to biased results if late and early school starters are systematically different from their peers. Moreover, one violation to the major exclusion restriction could be that being the oldest in a cohort can by itself influence education performance and development differently than being the youngest in a cohort. In other words, those who are born early in the year may perform better since they are more mature relative to their younger cohort peers. However, we do not see any strong evidence of such a tendency in the graphical illustrations.

The two-stage least squares IV estimates are presented in Tables 4–6. A concern in all regression discontinuity analyses is which bandwidth to choose, i.e. how far from the cut-off observations should be included. Since observations far from the cut-off in our data are likely to include some outliers (students attending the eighth or ninth grades 2 years too early or 2 years too late), which will

[11] The only exception to this pattern is the positive significant effect of father's occupation in the Swedish sample from 2009. However, when applying Bonferroni correction for multiple hypothesis testing none of the estimates in Tables A7–A10 reach conventional levels of significance.

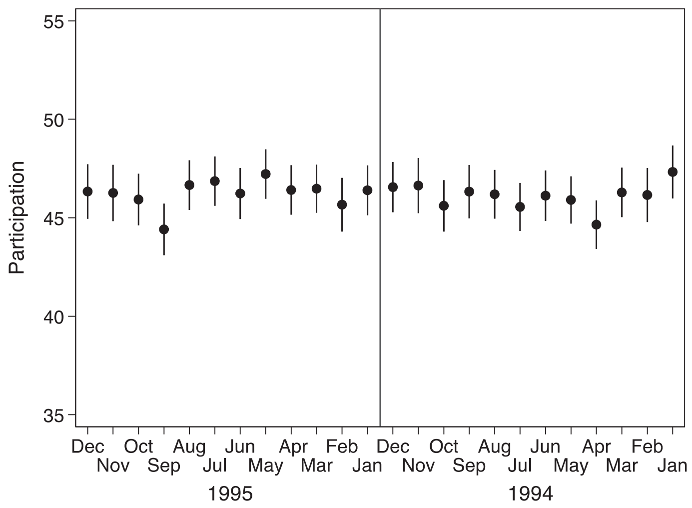

Fig. 2. Intention to participate in politics by birth month.

Table 4
Effect of the 9th grade (IV-estimates) on political knowledge, pooled sample.

	1	2	3	4
9th grade	0.607	0.609	0.550	0.734
	(0.412)	(0.412)	(0.456)	(0.469)
Age in months (first order)	0.180*	-0.015	0.174*	1.139
	(0.025)	(0.455)	(0.029)	(0.878)
Age in months (second order)		0.001		-0.003
		(0.001)		(0.003)
Age variables interacted with cut-off			YES	YES
Country fixed effects	YES	YES	YES	YES
Constant	151.768*	151.739*	151.758*	151.894*
	(0.363)	(0.362)	(0.361)	(0.377)
F-value from first stage regression	48,709	46,152	47,523	39,863
R^2	0.025	0.025	0.024	0.025
Number of individuals	24,023	24,023	24,023	24,023

Note: Robust clustered standard errors at the class level (954 clusters), individual level weights are applied. Sample restricted to those born in 1994/1995 * $p < 0.05$.

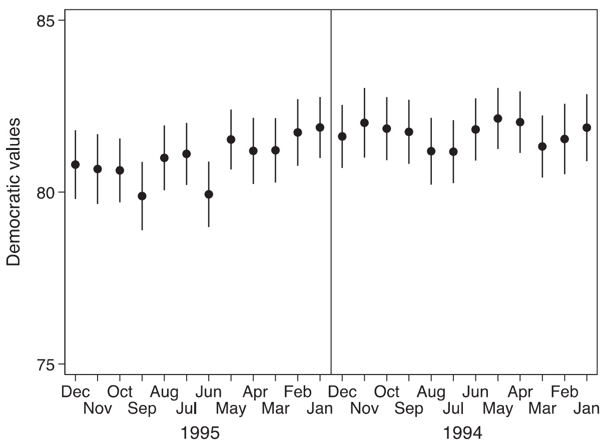

Fig. 3. Democratic values by birth month.

reduce the precision in the causal estimates, we will present our main analyses with a window encompassing individuals born one year before and one year after the cut-off (these observations account for more than 97.5% of all observations). However, as a robustness check we will also show the effect of the ninth grade for each possible bandwidth size from the smallest window close around the cut-off to the largest window.

Another important challenge is to separate the effect of the ninth grade from the effect of age. In order to parse out the grade effect we present four different model specifications. In model 1 in each table we include only a linear control for age in months. In the second model we also enter a second order polynomial control for age. Finally, in the last two models we allow a more flexible control for age by interacting the linear (model 3) and polynomial (model 4) age variables with the cut-off indicator. In essence this means that we allow the age effect to differ between those born 1994 and 1995. To facilitate interpretation of the constant we have centered the birth month (age) variables so that 0 corresponds to the first month before the cut-off and −1 the first month after the cut-off. Since the Swedish students start school one year later, 12 indicates the first month before the cut-off

Table 5
Effect of the 9th grade (IV-estimates) on intended political participation, pooled sample.

	1	2	3	4
9th grade	0.438	0.440	0.347	0.286
	(0.761)	(0.761)	(0.778)	(0.896)
Age in months (first order)	−0.045	−0.173	−0.055	1.738
	(0.050)	(0.691)	(0.058)	(1.725)
Age in months (second order)		0.000		−0.005
		(0.002)		(0.005)
Age variables interacted with cut-off			YES	YES
Country fixed effects	YES	YES	YES	YES
Constant	45.449*	45.430*	45.434*	45.657*
	(0.558)	(0.571)	(0.561)	(0.602)
F-value from first stage regression	50,341	49,123	50,555	42,196
R^2	0.001	0.001	0.001	0.001
Number of individuals	23,083	23,083	23,083	23,083

Note: Robust clustered standard errors at the class level (954 clusters), individual level weights are applied. Sample restricted to those born in 1994/1995 * $p < 0.05$.

for the Swedish students and 11 the first month after the cut-off.[12]

In the third row from the bottom of Tables 4 to 6 we report *F*-values corresponding to the coefficient estimates for the effect of the cut-off indicator on the probability of attending the ninth grade from the first stage of the IV-regression. As should be expected the very large *F*-values support the notion that the cut-off dummy is a very strong instrument for grade placement. The coefficients from the first stage range between 0.89 and 0.90, while the 95% confidence intervals range from 0.88 to 0.91.

Looking next at the coefficient estimates of the impact of education on civic knowledge in model 1 in Table 4, we find that the results mirror the descriptive evidence illustrated in the graph in Fig 1.[13] The linear effect of birth month is significant while the influence of having an additional year of education is substantially small and insignificant. Hence, there seems to be some evidence of an age effect; as students grow older they increase their knowledge—presumably merely by the fact that time goes by and they process more information. The effect of the ninth grade remains statistically as well as substantially insignificant when successively including a more flexible control for age in models 2 to 4. In all model specifications the effect of attending the ninth grade on civic knowledge is small in magnitude and imprecisely estimated. Tables 5 and 6 present the results using intended participation and democratic values as outcomes. Once again, we find no support for the assumption that one year of additional education increases the students' intended political participation or levels of democratic values. Irrespective of the specific control function for age, the effects of the ninth grade of schooling are insignificant and small in size. Moreover, we find no evidence of any robust effects of age on either democratic values or political participation.

As mentioned earlier, one drawback of the ICCS survey is that the knowledge measure is not very transparent. However, the Swedish data from the 1999 CivEd study includes a number of publicly released items. In addition, this dataset also includes a variable measuring the number of correct answers as well as a standardized index similar to the one found in ICCS. The Swedish data from 1999 also includes some measures of political participation equivalent to those in the ICCS data. Table A12 in the Online Appendix reports three models with our most flexible specification (first and second order age controls interacted with the cut-off) testing the impact of the ninth year of schooling on intended political participation and political knowledge (mle index and raw score of correct answers). Again we fail to detect any significant effects of the ninth year of schooling.[14]

Next we move on to present a number of robustness checks with alternative model specifications. One concern could be that the results may differ depending on how many months on each side of the cut-off are included in the analysis. To assess whether the results are sensitive to the chosen bandwidth Figs. 4 and 5 show the coefficient estimate for the instrumented measure of the ninth grade for a series of IV-regressions with all possible bandwidth windows (with equal number of months on each side of the cut-off). To the far left on the *x*-axis the regressions only include persons born 2 months before or after the cut-off and for each step further right on the axis one additional month on each side of the cut-off is added to the bandwidth window. The graphs in Fig. 4 only

[12] If Swedes are coded so that the month variables are 0 for the first month before the cut-off (that is, analogously to the pupils from the three other countries), the results still show no significant effects of the ninth grade.

[13] All models in Tables 4–6 include country fixed effects, clustered standard errors at the school class level, and population weights.

[14] One way to further validate the results would be to contrast these null results with substantial effects, using the same method, on variables in another subject area. Unfortunately, the CivEd and ICCS studies do not include any measures of skills in other subject areas that could be used for such validations. However, other studies have successfully used a similar kind of between-grade regression discontinuity approach to detect substantial and significant effects of grade year on test scores in mathematics and language.

Table 6
Effect of the 9th grade (IV-estimates) on democratic values, pooled sample.

	1	2	3	4
9th grade	−0.252	−0.254	−0.063	0.237
	(0.608)	(0.608)	(0.632)	(0.667)
Age in months (first order)	0.087*	0.401	0.107*	−0.378
	(0.039)	(0.576)	(0.044)	(1.385)
Age in months (second order)		−0.001		0.001
		(0.002)		(0.004)
Age variables interacted with cut-off			YES	YES
Country fixed effects	YES	YES	YES	YES
Constant	82.905*	82.952*	82.935*	82.896*
	(0.437)	(0.447)	(0.440)	(0.469)
F-value from first stage regression	49,078	46,177	47,347	39,551
R^2	0.004	0.004	0.004	0.004
Number of individuals	23,409	23,409	23,409	23,409

Note: Robust clustered standard errors at the class level (954 clusters), individual level weights are applied. Sample restricted to those born in 1994/1995 * $p < 0.05$.

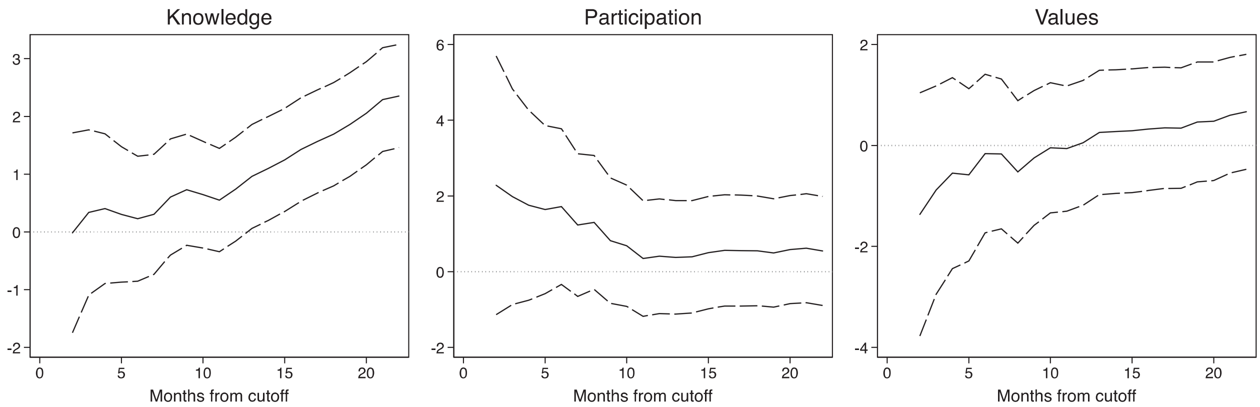

Fig 4. The treatment effect and 95% confidence interval by varying bandwidth (with linear control for age).

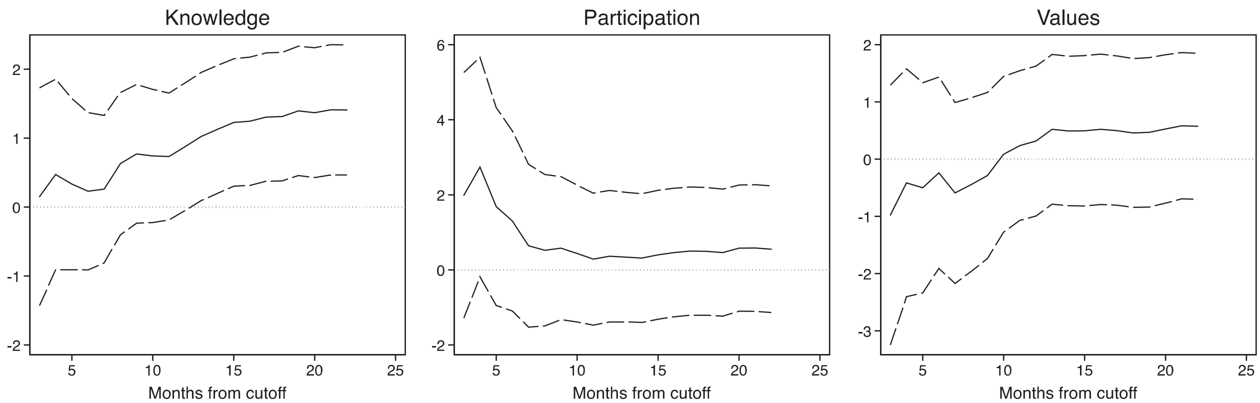

Fig 5. The treatment effect and 95% confidence interval by varying bandwidth (with linear and second order control for age).

include a first order control for age interacted with the cut-off (equivalent to model 3 in Tables 4–6) while the graphs in Fig. 5 also include a second order polynomial control for age interacted with the cut-off (equivalent to model 4 in Tables 4–6).

In order to estimate the treatment effect correctly we need a minimum of 2 months on each side of the cut-off for the models with only a linear control for age and 3 months on each side of the cut-off for the models also including a control for the second order polynomial of age. The results show that the null results for the grade effect are not very sensitive to the chosen bandwidth window. The exception is the results for civic knowledge (shown to the left in Figs. 4 and 5). When expanding the bandwidth to over +/− 12 months around the cut-off, the coefficient estimate for the ninth grade reaches significance and increases in size. However, we should keep in mind that it is harder to separate the effects of age and grade when expanding the bandwidth window and this significant coefficient of grade is likely to be driven by the age differences to a large extent. When adding controls for the second and third order polynomial of age, the coefficient for the ninth grade does not reach significance.

A further issue is whether we find the same trend in all countries or if there are dissimilarities between the four countries hidden in the results from the pooled sample. Figs. A1–A5 in the Online Appendix present separate results from each of the four countries in the pooled sample.

Overall, we do not find any sizeable statistically significant effects of the ninth year of schooling.

Finally we test for heterogeneity in our results by looking more closely at whether the results differ when dividing the sample along a number of background characteristics. Previous research on education effects on civic outcomes have shown that education can have a compensatory effect, i.e. being particularly effective for students from disadvantaged family backgrounds (cf. Campbell, 2008). This goes back to the seminal study of Langton and Jennings (1968) which detected a stronger curriculum effect on low-SES students. Table A13 in the Online Appendix reports the IV estimates of the effect of an additional year of schooling on the three outcomes across gender and dummy indicators for parental education, parental occupation and number of books at home. In these regressions we have used our most flexible control for age (first and second order polynomial interacted with the cutoff indicator). Twenty-two out of the 24 coefficient estimates presented in Table A13 are not significantly different from zero. The exceptions are the significant positive effects on democratic values for students living in homes with a large number of books and students whose parents are highly educated. However, given the large number of tests in Table A13 we should expect one or two significant results by chance alone. Thus, the overwhelming message from the heterogeneity analysis is in line with the results presented earlier. Irrespective of family background and gender of the children, the effects of the ninth grade on political knowledge, intended participation and democratic values are close to zero and far from statistically significant.

5. Conclusion

To summarize, this article brings the following contributions to the research debate on effects of education on political outcomes. It provides a novel approach to testing the causal effects of education using a between grade fuzzy regressions discontinuity design. We are able to simultaneously test the effect of age and distinguish this effect from the education grade effect. Moreover, we test whether the impact of education is conditioned on a number of socio-economic factors. Overall, our results show no evidence that the ninth year of schooling significantly influences intended political participation, knowledge or democratic values. Moreover, this conclusion seems to hold among students from privileged as well as less privileged home environments and in four different countries. Regarding age we find an effect of birth month on knowledge but not on intended participation or democratic values.

The treatment in the study, an additional year of education, might seem as a weak stimuli. But if the education effect runs through a ‘cognitive pathway’ it would be reasonable to expect effects of a single year of education. And indeed, the bulk of correlational studies presenting significant coefficients for “years of education” variables suggest that there is a significant relationship. Our findings rather corroborate the idea that political attitudes are established early in life and subject to little subsequent change during adolescence (cf. Sears and Funk 1999). The answer to why we usually find strong relationships between education and civic outcomes could thus be that education is strongly correlated with (most often) unobserved preadult factors. Another possibility is that the effect of education should be seen in a long term perspective and runs through a positional pathway, i.e. education affects social network positions that in turn affect political participation, knowledge and attitudes. However, we cannot test for such effects since we only measure the short-term consequences while students are still in school. Hence, education might still have long-term effects that we cannot detect with this research design.

While this study has some important advantages over previous studies—it provides a precise estimate of one additional year of education—it also has limitations. The most obvious one is of course that we can only test the effect of the ninth year of schooling. Earlier or later years of schooling might have important effects. Thus, we cannot say whether the ninth year has a particularly strong or weak effect. However, we share the drawback of being restricted to the impact of some specific level of education with other recent studies on education and civic outcomes. To take but a few examples, Kam and Palmer (2008) and Berinsky and Lenz (2011) estimate the influence of college education whereas Sondheimer and Green (2010) focus on high school education.

As far as we know there is no study that systematically tests the causal impact of all (or even several) different levels of education. Hence, we do not know what the full causal effect of education amounts to. Instead we are restricted to a number of studies estimating the local causal effects at specific points in the education distribution. But, given that it is mandatory in most developed countries, why should we care specifically about the ninth year of education? One reason is that there is an obvious need to evaluate whether the education that is provided to adolescents actually has the beneficial effects hypothesized in the literature. This provides a better understanding of the role of education in relation to adolescents’ political development. Moreover, if the ninth year were shown to have an effect on civic outcomes we would have a stronger argument in favor of extending mandatory education in countries where mandatory education is shorter than 9 years. The analyses also provide a better understanding of what we measure with standard variables on education. Years of schooling is a standard control in political behavior research. It is often used with the hypothesis that each year represents a learning process with a linear effect on civic skills and cognitive resources that subsequently translates into higher levels of political knowledge, participation and democratic values. The analysis presented here indicates that an additional year of schooling does not necessarily equate an additional year of learning. Instead of routinely including years of education as a control variable, research would benefit from including measures of education at the cut point that reasonably matters for the model being specified (such as an indicator variable for college).

Future studies would benefit from trying to estimate the causal influence of schooling on civic outcomes at several different stages of education to better understand which stages matter most. If the effect runs through the positional pathway it is reasonable to assume that increases in education that are fundamentally life-changing and form future network positions matter the most (such as completing a college degree) while incremental increases within a stage of education (such as one year of high school education) are less important.

The implications of this study might seem disappointing for policy makers and practitioners hoping to increase students' political knowledge, participation and democratic values by providing education. If education has no direct effect, inequalities in participation, knowledge and values cannot be easily educated away by providing more and better education. If knowledge and values are rooted earlier in life and not subject to much change in adulthood it might be a hard task to change patterns of inequality. If these factors are inherited and individuals tend to raise their children as they were raised themselves inequalities might be reproduced from generation to generation.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2016.03.015.

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