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The impact of education on political ideology: Evidence from European compulsory education reforms

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Abstract

Previous research documents a correlation between education and political ideology, usually indicating a positive relationship between education and left-wing political views. In this paper, I examine to what extent this association is causal. I merge political ideology data from 25 waves of Eurobarometer surveys with information on 18 educational reforms in 11 European countries. I then instrument for educational attainment with a regression discontinuity design that estimates the increase in education due to compulsory educational reforms. Notably, it appears that omitted variables bias is important here. I find a significant causal effect of education moving individuals to the right when properly addressing the endogeneity whereas there is a significant association between education and left-wing political ideology when treating education as exogenous. I find that on average, among the individuals compelled into additional education from these specific reforms, an

additional year of education moves individuals to the right of the political continuum by about 5–6%. However, I also find no evidence of a causal effect on political ideology for a subgroup of countries.

Keywords

Education, Political ideology, Europe, Compulsory education reforms

1. Introduction

Researchers have long been interested in the effect of education on political ideology. Most previous evidence suggests that education is associated with a more liberal ideology ([Dunn, 2011](#); [Weakliem, 2002](#)). Moreover, it is often stated or implied that education *causes* individuals to move to the political left (for example, [Dunn, 2011](#); [Weakliem, 2002](#); [Weil, 1985](#)). However, to the author's knowledge, previous studies on political ideology have not dealt with the endogeneity of education. Specifically, previous cross-sectional research may suffer from omitted variables bias because there are likely unobserved factors that affect political ideology and lead individuals to attain higher levels of education. For example, innate personality characteristics or the political ideology of one's parents are unobserved and can affect both educational attainment and one's own political ideology. Perhaps more worrisome is that it is unclear what the direction of the bias in previous studies would be. There are plausible stories as to how these unobserved factors could push one either to the political right or to the left while simultaneously affecting educational attainment. Thus, previous studies are limited in their ability to inform us on what would happen to political ideology in an alternative world where individuals are compelled into more education.

In contrast, this paper contributes to the literature by estimating the causal effect of education on political ideology as measured by self-identified left-right placement. The analysis overcomes the endogeneity of education through leveraging changes in compulsory education laws in 20th century Europe. The key feature of these law changes is that individuals were forced into additional education rather than self-selecting into more education. This exogenous shift in educational attainment can then be utilized to identify the effect of education on the outcome of interest. This instrumental variables (IV) approach has now been firmly established in the literature and has been utilized to explain the impact of education on a wide variety of outcomes.

Recent research suggests that the left-right scale as a construct is problematic. Specifically, [Bauer, Barbera, Ackermann, and Venetz \(2015\)](#) argue that the left-right scale is a vague concept and empirical relationships will be biased as a result. In particular, the authors provide evidence that education is endogenous to self-placement on the left-right political scale in Germany. An omitted variable (the meaning that individuals attach to the concepts of political “left” and “right”) is correlated with education and self-placement on the left-right scale. In the typical analysis of the association between

education and left-right identity, we would not know “whether education affects ideology directly or merely through the different associations it triggers within respondents” ([Bauer et al., 2015](#)). This is the benefit of the identification strategy regression employed in the present paper. Rather than simply using the reported level of education, I estimate educational attainment utilizing a regression discontinuity design (RDD) that exploits the timing of compulsory educational reforms. One group of individuals was born just prior to a reform and another group of individuals was born just after a reform. After controlling for cohort trends, unobserved characteristics such as conceptual associations should be on average the same across these two groups. Thus, I am plausibly able to overcome the omitted variables problem detailed in [Bauer et al. \(2015\)](#).

Supporting the concerns of [Bauer et al. \(2015\)](#), I do find that the observed association between education and left-right political identity from a standard OLS regression is quite different from the causal relationship estimated utilizing the fuzzy RDD. There is a small but highly statistically significant association between education and left-wing identity in the observational regressions that treat education as exogenous. However, once leveraging the educational reforms as instruments for endogenous educational attainment, I find that education significantly moves individuals to the right in their political identities. Among individuals who were affected by the educational reforms (the compliers), an additional year of education moves an individual to the right by approximately 0.2–0.4 points on a 10 point scale (4%–8% relative to sample mean) depending on the specification, with most estimates falling near 0.25 (5% relative to sample mean). I primarily utilize parametric specifications of a fuzzy RDD and find that the results are robust to linear and quadratic controls and to the sample bandwidth chosen. However, I also use nonparametric local linear regressions to confirm the results.

There are several theses from the political science and sociology literatures regarding the effect of educational attainment on political identity. As explained by [Dunn \(2011\)](#), these theories include “the self-interest thesis, the developmental thesis and the socialization thesis.” The self-interest thesis reasons that those who attain higher levels of education are likely to earn higher incomes, which attracts them to a right-wing identity out of self-interest. As explained by [Dunn \(2011\)](#), this thesis can explain the traditional European association of higher education leading to a right-wing identification. [Powdthavee and Oswald \(2014\)](#) provide an analytical framework in which a rational voter trades off a desire for low taxes with a desire for a public good. They show that an increase in income can theoretically affect left-right political affiliation for such a utility maximizing agent. [Powdthavee and Oswald \(2014\)](#) then provide evidence that an exogenous shock to income through winning the lottery leads to significant movement to the right in political affiliation in the UK, supporting the idea that individuals act out of self-interest when voting.

In contrast, the developmental thesis predicts that more education will lead to more of a left-wing political identity. In this thesis, education expands one's perspective and stimulates cognitive growth, leading to a more liberal ideology ([Phelan, Link, Stueve, & Moore, 1995](#)). Finally, the socialization thesis maintains that political attitudes are transmitted to students through social learning processes. Students learn about society's attitudes through the behaviors that are modeled and reinforced in the educational system. A leading specific example of the socialization thesis is [Weakliem's \(2002\)](#) core values thesis. In this theory, education serves to increase the commitment to society's core values and institutions. Thus, more education would tend to move individuals to the right in predominantly conservative societies and to the left in predominantly liberal societies.

Concerning the aforementioned theories, the primary finding in this paper that increased educational attainment tends to move individuals to the right of the political spectrum could be consistent with either the self-interest thesis or the socialization thesis and is inconsistent with the developmental thesis. Closer examination of individual countries reveals most support for the self-interest thesis. Specifically, education tends to have a stronger causal effect on political ideology in countries where previous research has shown the reforms to be effective in increasing earnings and no causal effect in countries where research has shown the reforms to not affect earnings.

Aside from the contribution to the literature on the relationship between educational attainment and political attitudes, this paper adds to the growing literature concerning non-pecuniary effects of education. Most often, researchers use changes in compulsory education laws as a source of exogenous variation to identify the effects of interest. Much of this compulsory education literature has focused on labor market outcomes such as income and employment ([Aakvik, Salvanes, & Vaage, 2010](#); [Acemoglu and Angrist, 2001](#); [Angrist & Krueger, 1991](#); [Brunello, Fort, & Weber, 2009](#); [Meghir and Palme, 2005](#); [Oreopoulos, 2006a, b](#)). More recently, non-pecuniary effects of education are receiving more attention ([Oreopoulos & Salvanes, 2011](#)). For example, increases in education have been shown to reduce crime ([Hjalmarsson, Holmlund, & Lindquist, 2015](#); [Lochner and Moretti, 2004](#); [Machin, Marie, & Vujic, 2011](#)), reduce teenage births ([Black, Devereux, & Salvanes, 2008](#)), improve health outcomes¹ ([Gathmann, Jürges, & Reinhold, 2015](#); [Lleras-Muney, 2005](#); [van Kippersluis, O'Donnell, & van Doorslaer, 2011](#)), and increase pro-environmental behavior ([Meyer, 2015](#)).

In the political realm, [Milligan, Moretti, and Oreopoulos \(2004\)](#) and [Dee \(2004\)](#) find that increases in education cause political involvement and voting to increase. However, there are also mixed results in this area. [Siedler \(2010\)](#) finds little evidence of a causal effect of education on political behavior in Germany. [Borgonovi, d'Hombres, and Hoskins \(2010\)](#) pool reforms from 15 European countries and find that education increases political information acquisition but has no effect on voter turnout. [Di](#)

[Pietro and Delprato \(2009\)](#) examine the effects of a compulsory education reform in Italy on a number of civic outcomes. The authors find a positive and significant effect on interest in politics, a negative and significant effect on the stated justifiability of not returning lost money to an owner and insignificant effects on the justifiability of not paying a ticket on a public transport vehicle and of not leaving one's name for the owner of a car one accidentally scraped, and an insignificant effect on the stated importance of the problem of tax evasion. Most recently, [Persson, Lindgren, and Oskarsson \(2016\)](#), leverage exogenous variation in school entry age in Greece, Norway, Slovenia, and Sweden to estimate the casual effect of the ninth year of education; the authors find no significant effect on political participation, democratic values, or political knowledge.

More generally, this study contributes to the literature that analyzes the determinants of voting intentions and support for politicians. Most of this work concentrates on how economic conditions affect voting intentions—often termed economic voting. For example, [Veiga and Veiga \(2004\)](#) find that economic conditions are important in determining support for right-wing and left-wing governments in Portugal. [Sakurai and Menezes-Filho \(2008\)](#) and [Cassette, Farvague, and Hericourt \(2013\)](#) examine how local public spending affect municipal elections in Brazil and France, respectively. Yet, the approach in the present paper differs because it concentrates on a key individual-level determinant of political identity rather than national or local factors. The present results do suggest, however, that voting behavior could be influenced by public policies that change the level of required education. For example, any public policies that reduce the number of high school dropouts could affect political ideology. The results suggest that the effects would be largest in cases where the extra education is effective in increasing earnings of the compliers.

2. Data

The analysis in this paper requires information about left-right political ideology and compulsory education laws. 25 waves of Eurobarometer surveys provide the data on left-right political ideology and several recent papers provide the needed information about compulsory education laws. The European Commission regularly conducts population representative public opinion surveys of European Citizens; these are Eurobarometer surveys. I identify 25 waves of Eurobarometer surveys conducted between October 2004 and March 2012 that contain a question about left-right political identification.² Each wave collects data over a span of 1 to 3 months during the months of September-May.³ All waves utilized face-to-face interviews, where interviewers followed uniform protocols. Each of the waves completed interviews with approximately 1000 individuals from each country and no more than one individual was interviewed from each household. In addition to the left-right ideology question, interviewers collect several demographic variables including gender, age, and age when the individual left formal education.⁴ As in [Meyer \(2015\)](#), I assume that educational attainment increases

only until age 25 and exclude those who report an age of 36 or higher when they stopped full-time education.⁵ Using the age when an individual stops education as a proxy of educational attainment is not a perfect measure because of possible gaps in one's education. However, this will only bias the estimated effect of reform on education if there are more people with gaps before or after a reform on average.

Following [Borgonovi et al. \(2010\)](#), [Brunello et al. \(2009\)](#), [Brunello, Fabbri, and Fort \(2013\)](#), [Gathmann et al. \(2015\)](#), and [Meyer \(2015\)](#), I elect to pool multiple educational reforms that occurred in 20th Century Europe. I begin with the group of reforms detailed in [Gathmann et al. \(2015\)](#); these reforms were implemented between 1900 and 1980.⁶ I then add the three remaining country-level reforms from [Brunello et al. \(2009\)](#) that fall within this time period.⁷ This results in a list of 18 reforms from 11 Western European countries, which are summarized in [Table 1](#). All of these reforms have been leveraged as instruments in multiple studies. [Fort \(2006\)](#) documents the details of many of the reforms. As mentioned in [Fort \(2006\)](#), the exact date that each reform began to affect the population is sometimes in dispute. Thus, I follow [Meyer \(2015\)](#) in using implementation dates that are consistent with [Brunello et al. \(2009\)](#) and [Gathmann et al. \(2015\)](#). These reforms increased the minimum education level by 1 to 4 years and affected individuals born between 1917 and 1969. The typical reform among this group moved the minimum leaving age up to the 9th or 10th grade. As previously mentioned, the key attribute of these reforms is that they provide plausibly exogenous variation in educational attainment. Individuals are compelled into the additional education rather than electing the extra education because of unobserved factors.

Table 1. Reforms.

Country/region	Reform date	First cohort potentially affected	Change in min. schooling level	Source
Austria	1962	1947	8–9	BR, F, GA
Belgium	1983	1969	8–12	BR, F
Denmark	1971	1957	7–9	AR, BR, F, GA
England and Wales	1947	1933	8–9	CR, DH, GA, HW, O
England and Wales	1973	1958	9–10	CR, GA, GR, HW
France	1937	1923	7–8	AL, F, GA
France	1959/1967	1953	8–10	AL, BR, F, GA, GR
Greece	1975	1963	6–9	B, F
Ireland	1972	1958	8–9	BR, CH, GA
Italy	1963	1949	5–9	BC, BR, F, GA, DD

Country/region	Reform date	First cohort potentially affected	Change in min. schooling level	Source
Netherlands	1928	1917	6–7	GA, vK
Netherlands	1950	1938	7–9	GA, L
Netherlands	1973	1959	9–10	BR, F, GA, L
Northern Ireland	1957	1943	8–9	GA, F, O
Scotland	1946	1932	8–9	GA
Scotland	1976	1958	9–10	GA
Spain	1970	1957	6–8	BR, F, GA, P
Sweden	1962	1950	8–9	BR, F, HJ, M

Note: AL = [Albouy and Leguén \(2009\)](#), AR = [Arendt \(2005\)](#), BC = [Brandolini and Cippolone \(2002\)](#), BR = [Brunello et al. \(2009\)](#), CH = [Callan and Harmon \(1999\)](#), CR = [Clark and Royer \(2013\)](#), DH = [Devereux and Hart \(2010\)](#), DD = [Di Pietro and Delprato \(2009\)](#), F = [Fort \(2006\)](#), GA = [Gathman et al. \(2015\)](#), GR = [Grenet \(2013\)](#), HJ = [Hjalmarsson et al. \(2015\)](#), HW = [Harmon and Walker \(1995\)](#), L = [Levin and Plug \(1999\)](#), M = [Meghir and Palme \(2005\)](#), O = [Oreopoulos \(2006a\)](#), P = [Pons and Gonzalo \(2002\)](#), vK = [van Kippersluis et al. \(2011\)](#). Adapted from Ecological Economics, Vol. 116 No. 0, Meyer, A., “Does Education Increase Pro-Environmental Behavior? Evidence from Europe”, Table 1, Page 111, Copyright (2015), with permission from Elsevier.

The analysis only includes individuals born close enough to a change in the corresponding education reform. Define the pivot cohort as the first cohort that was potentially affected by a change in compulsory education law j and denote the birth year of the pivot cohort as b_j . Let $k = (b - b_j)$ indicate the number of years between the cohort from year b and the pivot cohort. Clearly, I need to know when an individual was born to make this calculation. Some approximation is required here because only age, not birth year, is reported on the survey. As discussed in [Meyer \(2015\)](#), one can be fairly certain of the birth year from some survey waves. For example, Eurobarometer 68.2 was conducted in the months of November 2007–January 2008. Nearly all of the respondents would have had their birthday in calendar year 2007. It is less clear for other survey waves such as Eurobarometer 75.2 because it was conducted in the months of April–May 2011; there is a higher likelihood that an individual would not have had their birthday in calendar year 2011 at the time of the survey. Therefore, I code birth year as (year-age) for individuals in survey waves conducted between September and December and (year-age-1) for individuals in survey waves conducted between January and May.⁸

To begin, individuals with $k \in [-10, -1]$ comprise the pre-reform sample and individuals with $k \in [1, 10]$ make up the post-reform sample.⁹ In this bandwidth of ± 10 years, there are 109,238 observations from individuals who provide an answer to the question about educational attainment. I then narrow the sample to include only individuals who provide an answer to the left-right political ideology

question, leaving 94,694 observations. The specific wording of the left-right political ideology question, which is identical in all waves of the survey, is as follows, “In political matters people talk of the ‘the left’ and ‘the right’. How would you place your views on this scale?” Interviewers then showed the respondent a card with a 10 point scale, ranging from 1 (left) to 10 (right).

[Table 2](#) shows the distribution of the answer to the left-right identity question for the 94,694 individuals who answer the question. In addition, I present a histogram to visualize the distribution in [Fig. 1](#). The distribution is approximately symmetric with a clear spike corresponding to the answer of “5”. An answer of “5” likely indicates that an individual sees themselves as a centrist, not identifying with either the left or the right. [Table 3](#) summarizes this variable by country; there are only modest differences in the averages across the sample countries. [Table 4](#) provides summary statistics for each of the variables. The average age when leaving full-time education for the sample is approximately 18.5, the sample is approximately 49% male, the average age is about 54 years, and the average response to the left-right ideology question is 5.18. Moreover, a little more than one-half of the individuals in the sample were affected by the educational reforms. The online appendix provides summary statistics by country. The biggest differences across countries are in age, which is determined by the date(s) of each country's reform(s), and in average age of leaving full-time education. For example, the average age of leaving full-time education in some countries such as Spain, Italy, and UK is less than 17 years whereas the average age of leaving education is as high as 21.38 in Denmark and 20.21 in Sweden.

Table 2. Summary of self-reported left-right placement.

Left-right		
placement	N	Percent
Box 1 – left	3889	4.11
Box 2	4174	4.41
Box 3	11,479	12.12
Box 4	12,614	13.32
Box 5	26,231	27.70
Box 6	12,252	12.94
Box 7	11,443	12.08
Box 8	7772	8.21
Box 9	2016	2.13
Box 10 – right	2824	2.98

Left-right		
placement	N	Percent
Total	94,694	100

Note: This summarizes the sample generated by a bandwidth of ± 10 years.

Fig. 1. Political ideology. Note: Histogram displays frequency of respondents' political ideology responses. 1 = far left, 10 = far right.

Table 3. Left-right placement by country.

Country	N	Mean	Std. Dev.
Austria	6263	5.26	1.95
Belgium	7657	5.07	1.89
Denmark	7571	5.29	2.11
Greece	6392	5.40	2.16
France	9535	5.01	1.99
Ireland	6972	5.60	1.82
Italy	5045	5.18	2.43
Netherlands	17,337	5.06	1.97
Spain	5926	4.50	1.94
Sweden	8605	5.32	2.16
United Kingdom	13,391	5.30	1.84

Note: This summarizes the sample generated by a bandwidth of ± 10 years.

Table 4. Summary statistics.

Variable	N	Mean	Std. Dev.	Min	Max
<i>Bandwidth of ± 10 years</i>					
Education age	94,694	18.50	3.84	0	25
Reform	94,694	0.531	0.499	0	1
Male	94,694	0.491	0.500	0	1
Age	94,694	54.22	11.70	25	97
Left identity	94,694	5.18	2.02	1	10
<i>Bandwidth of ± 7 years</i>					
Education age	66,267	18.51	3.81	0	25
Reform	66,267	0.521	0.500	0	1
Male	66,267	0.489	0.500	0	1
Age	66,267	54.43	11.21	28	97
Left right placement	66,267	5.16	2.01	1	10

3. Empirical strategy

The principal goal of this study is to estimate the impact of educational attainment on political ideology, as measured by left-right identity. In the experimental ideal, one would randomly assign individuals to different levels of education and then analyze differences in ideology across groups. However, absent such a randomized controlled trial, individuals will self-select into different levels of education and choose their political identity. Therefore, unobserved factors will likely impact both educational attainment and political ideology. As such, the primary empirical concern is omitted variables bias. I adopt an instrumental variables (IV) strategy to address endogeneity from the omitted variables. Changes in compulsory educational laws have become the instrument of choice for educational attainment in related research. Following this literature, I argue that changes in compulsory education laws are valid as an instrument because these changes exogenously compel individuals into higher educational attainment. That is, changes in these laws affect educational attainment but are uncorrelated with other unobservable factors that affect the outcome of interest—in this case political ideology.

Previous research establishes the positive impact of this set of reforms on average educational attainment ([Brunello et al., 2009](#); [Brunello et al., 2013](#); [Gathmann et al., 2015](#); [Meyer, 2015](#)).

Nevertheless, it is useful to graphically present evidence of a discrete jump in educational attainment for the cohorts first affected by the reform in this particular sample. Recall that k is the interval in

number of years before or after the first affected cohort from each reform. Panels A and B of [Fig. 2](#) plot average education versus k for 10 years pre- and -post-reform. For Panel A, I then regress education on k and k^2 for two subsamples, once for the pre-reform period and once for the post-reform period. Finally, I plot the fitted values from these regressions to show the trends in educational attainment. Panel B of [Fig. 2](#) shows the analogous graph for linear controls. Both panels of [Fig. 2](#) show a discontinuity in average educational attainment in the year of the reform. Individuals born in the cohort immediately prior to a reform ($k = -1$) are predicted to leave formal education 0.37 years earlier than individuals born in a cohort immediately following a reform ($k = 1$) in Panel A for quadratic controls. The size of the discontinuity for linear controls in Panel B is 0.26 years. I also show the analogous graphs using linear controls and bandwidths of 7 (Panel A) and 5 (Panel B) in [Fig. 3](#). From [Fig. 3](#), it appears that a linear functional form fits the data rather well in these narrowed bandwidths.

Fig. 2. Education age vs. interval (k), bandwidth of 10. Note: Average education values represent means of all observations for a given interval (k). Fitted values are from separate regressions of education age on interval and interval squared (Panel A) and education age on interval (Panel B). The interval of 0 corresponds to the pivot cohort. Positive interval values correspond to individuals treated with educational reforms.

Fig. 3. Education age vs. interval (k), narrowed bandwidths. Note: average education values represent means of all observations for a given interval (k). Fitted values are from separate regressions of education age on interval. The interval of 0 corresponds to the pivot cohort. Positive interval values correspond to individuals treated with educational reforms.

Following the approach utilized elsewhere in the literature, I utilize a fuzzy regression discontinuity design (RDD) to identify the effect of compulsory education reforms on political ideology. This leads to the following two-equation model specification for individual i from reform c .

(1)

$$ideology_{ic} = \beta_0 + \beta_1 education_{ic} + f(k_{ic}) + X'_{ic}\beta_2 + ReformFE + SurveyFE + \mu_{ic}$$

and

(2)

$$education_{ic} = \alpha_0 + \alpha_1 R_{ic} + g(k_{ic}) + X'_{ic}\alpha_2 + ReformFE + SurveyFE + \epsilon_{ic}.$$

Here, $f(k_{ic})$ and $g(k_{ic})$ are controls for cohort trends, X'_{ic} is a vector of exogenous controls, R_{ic} is an indicator for being affected by the educational reform, $ReformFE$ is a set of reform fixed effects, and $SurveyFE$ is set of survey wave fixed effects.

Identification of α_1 in the RDD requires that the potentially endogenous relationship between birth cohort and educational attainment is captured by the controls, $g(k_{ic})$. After controlling for these cohort trends, individuals on both sides of the pivot cohort should have the same average educational attainment within some range. A discontinuous jump in educational attainment at the pivot cohort can then be attributed to the reform. I allow the slope of the regression function to change at the pivot cohort by interacting the linear or quadratic controls with the reform indicator, R_{ic} . Therefore, with quadratic controls,¹¹Eq. (2) becomes

(3)

$$\begin{aligned} education_{ic} = & \alpha_0 + \alpha_1 R_{ic} + \gamma_1 k_{ic} + \gamma_2 (R_{ic} * k_{ic}) + \gamma_3 k_{ic}^2 \\ & + \gamma_4 (R_{ic} * k_{ic}^2) + \mathbf{X}'_{ic} \alpha_2 \\ & + ReformFE + SurveyFE + \epsilon_{ic}. \end{aligned}$$

Eq. (1) is then analogously modified to substitute the appropriate linear or quadratic controls for $f(k_{ic})$, resulting in

(4)

$$\begin{aligned} ideology_{ic} = & \beta_0 + \beta_1 education_{ic} + \lambda_1 k_{ic} + \lambda_2 (R_{ic} * k_{ic}) + \lambda_3 k_{ic}^2 \\ & + \lambda_4 (R_{ic} * k_{ic}^2) + \mathbf{X}'_{ic} \beta_2 \\ & + ReformFE + SurveyFE + \mu_{ic} \end{aligned}$$

for the case of quadratic controls.¹² Furthermore, I interact the linear/quadratic controls in Eqs. (3) and (4) with a set of reform dummies so that each reform can take on its own trend.¹³

I initially set the bandwidth at ± 10 years, which produces the maximum sample size with no individual being counted as part of both the pre- and –post-reform groups. This also corresponds to the window chosen by [Gathmann et al. \(2015\)](#). I then decrease the bandwidth to ± 7 years as a specification check. A 7 year bandwidth substantially cuts the sample size but matches the window chosen by [Brunello et al. \(2009\)](#). The tradeoff is that wider bandwidths provide more power but may increase the probability that the included controls do not adequately capture cohort trends. Moreover, I report results for all other possible bandwidths of 2–6 and 8–9 years.

As previously mentioned, I exclude individuals from the pivot cohort because I cannot reliably determine whether or not they were treated with the reform. Therefore, I am effectively utilizing a “donut-hole” RD estimator.¹⁴ Previous studies have shown that this strategy can be advantageous for reasons beyond measurement error. One typical issue in RD estimation is the possibility of manipulation of the forcing variable ([Lee & Lemieux, 2010](#); [McCrary, 2008](#)); the “donut-hole” approach can be useful if one suspects such manipulation. For example, [Barreca, Guldi, Lindo and Waddell](#)

(2011) revisit the conclusions of [Almond, Doyle, Kowalski, and Williams \(2010\)](#) regarding the reduction in 1-year infant mortality as birth weight crosses the 1500 g “very-low-birth-weight (VLBW)” threshold. [Barreca et al. \(2011\)](#) note that the observations occurring at this important threshold are systematically different from those on either side of the threshold so including these threshold observations can bias results. Furthermore, the authors argue that RD estimates should not be sensitive to the observations at the threshold so it can be useful to remove a “donut-hole” of observations ([Barreca et al., 2011](#)). Due to merging 25 different waves of surveys with inconsistent demographic questions, I do not have the ability to test for differences in observable characteristics from individuals in different birth cohorts. In the present context, therefore, one advantage of the donut-hole RD is that it allows for the possibility that individuals born in the pivot cohort are systematically different from the surrounding cohorts.¹⁵

Consistent with previous studies in this literature ([Brunello et al., 2009](#); [Gathmann et al., 2015](#); [Grenet, 2013](#); [Meyer, 2015](#)), I view these as parametric regression specifications. This implicitly assumes that we know the functional form. Many recent RDD papers instead use nonparametric local polynomial regression, which is more appropriate if we are treating the functional form as unknown. Therefore, I also report results using local linear specifications of the fuzzy RDD instead of the parametric global polynomial approach. I utilize a triangular kernel to weight observations and a local quadratic polynomial for bias correction. I select the bandwidth in two ways; I first use the cross-validation procedure from [Ludwig and Miller \(2007\)](#) and then use the [Imbens and Kalyanaraman \(2012\)](#) approach. In each case, I present conventional and bias corrected point estimates along with conventional and robust ([Calonico, Cattaneo, & Titiunik, 2014a](#)) standard errors.¹⁶

4. Results and discussion

4.1. Main results

To begin, columns 1 and 2 of [Table 5](#) present 2SLS results from the bandwidths of ± 10 years from a pivot cohort. Column 1 uses linear controls while column 2 uses quadratic controls. Error terms may possibly be correlated between individuals from the same reform group and birth cohort. As such, I cluster standard errors at the birth year-reform level.¹⁷ I do find a statistically significant local average treatment effect (LATE) of educational attainment on left-right ideology. An additional year of educational attainment leads to an approximate 0.25 point movement to the right on the 10 point scale. Relative to the sample mean of political affiliation, this represents an approximate 5% movement to the right.

Table 5. 2SLS regression results.

	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
<i>2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right</i>				
Education age	0.256** (0.120)	0.235** (0.116)	0.249** (0.107)	0.269* (0.157)
Male	0.0466 (0.0460)	0.0533 (0.0445)	0.0549 (0.0459)	0.0491 (0.0578)
Age	0.0332 (0.0273)	0.0336 (0.0267)	0.0526 (0.0321)	0.0582 (0.0408)
Age²	-0.000357 (0.000228)	-0.000361 (0.000225)	-0.000483* (0.000264)	-0.000534 (0.000345)
<i>1st stage. Outcome is education age.</i>				
Reform	0.253*** (0.0487)	0.372*** (0.0841)	0.316*** (0.0603)	0.332*** (0.111)
Male	0.323*** (0.0224)	0.323*** (0.0224)	0.322*** (0.0266)	0.323*** (0.0267)
Age	-0.168*** (0.0248)	-0.176*** (0.0250)	-0.232*** (0.0309)	-0.232*** (0.0310)
Age²	0.00145*** (0.000207)	0.00151*** (0.000209)	0.00195*** (0.000258)	0.00195*** (0.000259)
F-Stat	310.85	216.94	219.13	152.66
(p-value)	<0.0001	<0.0001	<0.0001	<0.0001
F-Stat on excluded instrument	24.02	24.00	28.11	12.77
(p-value)	<0.0001	<0.0001	<0.0001	0.0004
Bandwidth	±10	±10	±7	±7
Cohort trends	Linear	Quadratic	Linear	Quadratic
N	94,694	94,694	66,267	66,267

Robust standard errors in parentheses are clustered at the birth year-reform level.

All regressions include cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

*** $p < 0.01$,

** $p < 0.05$,
 * $p < 0.1$.

As a specification check, I next decrease the bandwidth to ± 7 years in columns 3 and 4 of [Table 5](#). The estimated effects of education on political ideology are rather similar to their counterparts in columns 1 and 2. I also show abbreviated results for bandwidths of 2–6 and 8–9 years in [Table 6](#) for the case of linear cohort trends. As seen in [Table 6](#), an extra year of education leads to a movement to the right of 0.22–0.47 across the various bandwidths, with most of the point estimates concentrated around 0.25.

Table 6. 2SLS regression results, alternative bandwidths.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth	± 9	± 8	± 6	± 5	± 4	± 3	± 2
<i>2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right</i>							
Education age	0.241 ^{**} (0.112)	0.220 ^{**} (0.111)	0.224 ^{**} (0.0967)	0.258 ^{**} (0.120)	0.331 [*] (0.174)	0.282 [*] (0.149)	0.470 ^{**} (0.237)
<i>1st stage. Outcome is education age.</i>							
Reform	0.270 ^{***} (0.0519)	0.293 ^{***} (0.0555)	0.357 ^{***} (0.0662)	0.306 ^{***} (0.0743)	0.257 ^{***} (0.0867)	0.326 ^{***} (0.107)	0.260 [*] (0.158)
F-Stat on excluded instrument	24.54	26.17	33.61	20.86	11.30	13.65	10.80
(p-value)	<0.0001	<0.0001	<0.0001	<0.0001	0.001	0.0003	0.0016
N	85,344	76,056	56,842	47,212	37,558	28,195	18,647

Robust standard errors in parentheses are clustered at the birth year-reform level. All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

*** $p < 0.01$,
 ** $p < 0.05$,
 * $p < 0.1$.

When using 2SLS one may be concerned about the possibility of weak instruments. Therefore, I present first stage regression results in [Tables 5](#) and [6](#). The first stage results from all specifications show that the educational reform does have a statistically significant positive effect. Moreover, the F-stats are all above the rule of thumb value of 10 ([Stock, Wright, & Yogo, 2002](#)). Furthermore, the magnitude of the first stage effect is in line with previous studies. Using many of the same reforms, [Gathmann et al. \(2015\)](#) report a first stage of 0.501 (men)/0.541 women and [Brunello et al. \(2009\)](#) show an average first stage of around 0.3. With a slightly different set of reforms, [Borgonovi et al. \(2010\)](#) report a first stage of 0.508.

All results presented thus far have attempted to deal with the endogeneity of education. However, it is illustrative to examine the results when treating education as exogenous. Thus, [Table 7](#) shows OLS results from 4 representative bandwidths. As seen across the columns of [Table 7](#), there is a statistically significant association between educational attainment and left-leaning ideology when treating education as exogenous. This could suggest that unobservable characteristics tend to move individuals toward a left political ideology as educational attainment increases. Alternatively, it could be that the subgroup of compliers—those who take up treatment from the instrument—is different from the general population. In the context of these compulsory educational reforms, the compliers are likely located in the lower end of the educational attainment distribution. Thus, one should interpret the findings in this paper as representing the effect of education on political identity at the lower end of the educational distribution and for these particular reforms.

Table 7. Exogenous education regression results: OLS.

	(1)	(2)	(2)	(3)
Bandwidth	±10	±7	±5	±2
Education age	-0.0149*** (0.00347)	-0.0163*** (0.00408)	-0.0189*** (0.00468)	-0.0223*** (0.00698)
Male	0.136*** (0.0181)	0.142*** (0.0217)	0.137*** (0.0260)	0.130*** (0.0404)
N	94,694	66,267	47,212	18,647

Robust standard errors in parentheses are clustered at the birth year-reform level. All regressions include a quadratic of age, reform fixed effects, and survey wave fixed effects.

*** $p < 0.01$,

4.2. Ordered probit results

The results presented so far treat the left-right affiliation as a continuous variable and assume a constant marginal effect of education on political affiliation. This is likely a fine approximation but I also address this issue by estimating two ordered probit specifications that use all available observations and treat educational attainment as endogenous. The ordered probit model utilizes [Eqs. \(3\)](#) and [\(4\)](#), but substitutes a latent index in place of the continuous left-hand side variable in [Eq. \(4\)](#).¹⁸ IV ordered probit specification 1 (*IVOP1*) codes the dependent variable into 3 categories (original responses of 1 through 4 = “1”, 5 = “2”, 6 through 10 = “3”). IV ordered probit specification 2 (*IVOP2*) codes the dependent variable into 5 categories (original responses of 1 or 2 = “1”, 3 or 4 = “2”, 5 = “3”, 6 or 7 = “4”, 8 through 10 = “5”). The main drawback of the ordered probit model is that interpretation is more complicated. In interpreting the impact of educational attainment on political identity, there are as

many marginal effects as categories of the dependent variable. *IVOP1* has the simpler interpretation as the categories correspond to “left,” “center,” and “right.” *IVOP2* is more flexible and attempts to group the values of the dependent variable according to the patterns visible in [Fig. 1](#).

[Table 8](#) presents maximum likelihood results from the two ordered probit specifications for linear controls and a bandwidth of ± 7 years. As seen in [Table 8](#), the ordered probit results are consistent with the baseline 2SLS results. The positive signs on the coefficients on education across the columns indicate that, when accounting for the endogeneity of education, additional educational attainment moves individuals to the right of the political scale. However, one must examine the average marginal effects to infer the magnitude of the change. The interpretation of the marginal effects is most straightforward in column 1 from *IVOP1* so I concentrate on those but note that the results in column 2 from *IVOP2* tell a similar story. Results in column 1 suggest that an extra year of educational attainment decreases the probability of falling into the left category (1–4) by approximately 4 percentage points and increases the probability of falling into the right category (6–10) by a similar magnitude. There is a statistically significant decrease in the probability of falling into the center category (5) with more education; however, this effect has near 0 magnitude. Therefore, overall, the results from [Table 8](#) support the baseline analysis in showing that additional educational attainment moves individuals to the right.

Table 8. IV ordered probit regression results.

	(1)	(2)
Variables	MLE (IVOP1)	MLE (IVOP2)
Education age	0.121 ^{***} (0.0442)	0.103 ^{**} (0.0437)
<i>Cutpoints</i>		
Cut1	3.95 ^{**} (1.79)	2.24 (1.91)
Cut2	4.60 ^{***} (1.76)	3.14 [*] (1.87)
Cut3		3.82 ^{**} (1.84)
Cut4		4.58 ^{**} (1.81)

	(1)	(2)
Variables	MLE (IVOP1)	MLE (IVOP2)
<i>Average ME of education</i>		
Left (1–4)	–0.0416*** (0.0140)	
Far Left (1–2)		–0.0184* (0.00989)
Center Left (3–4)		–0.0175*** (0.00445)
Center (5)	–0.00108*** (0.00011)	–0.00106*** (0.00017)
Center Right (6–7)		0.0134*** (0.00301)
Far Right (8–10)		0.0236** (0.0115)
Right (6–10)	0.0427*** (0.0140)	

Robust standard errors in parentheses are clustered at the birth year-reform level.

All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects. N = 66,267. The bandwidth is ± 7 years.

Average ME's of education are calculated using Stata's margins command.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

4.3. Discussion of theories and country-specific results

The developmental thesis predicts that education will move individuals to a more liberal ideology and is hence not supported by the results presented in the preceding sections. In contrast, according to the self-interest thesis, individuals will move to the right out of self-interest when their incomes increase.

Previous research establishes that many of these reforms increased earnings for the compliers.

However, other papers find small or no effects on earnings. Appendix [Table A1](#) summarizes previous studies of the compulsory education reforms utilized in this paper and shows that there is strongest prior evidence of significant earnings increases for the UK reforms and some evidence of earnings increases for Italy and Netherlands. Thus, if the self-interest thesis is correct, we should see more movement to the political right in these countries. Likewise, we should not see significant movement

to the right in countries such as France or Sweden where the reforms have not been shown to significantly affect earnings. Hence, instead of pooling together all the reforms and running one analysis, I estimate the model on each of the countries separately. [Table 9](#) shows the LATE of education on political ideology by country. To probe the stability of the results, I present 4 bandwidths for each country. The sample size drastically decreases when examining one country at a time which cuts statistical power, but the point estimates are informative. The second stage point estimates are consistently positive for 3 of the 11 countries (Italy, Netherlands, and UK), consistently negative for 1 country (Spain), and sometimes positive/sometimes negative for 7 of the countries (Austria, Belgium, Denmark, France, Greece, Ireland, and Sweden). However, the standard errors are large for most of these estimates so I typically cannot reject the null of 0 effect at conventional levels. In support of the self-interest thesis, I find some evidence that the Italian reform moved individuals to the right and no evidence that the French reforms affected political ideology. I also find weak evidence that the UK reforms may have moved individuals to the right, but the estimates are never significant at conventional levels. There is some evidence that the Swedish reform moved individuals to the right but the result does not appear robust to the bandwidth selection. As seen in [Table A1](#), [Meghir and Palme \(2005\)](#) find little evidence that this reform increased earnings, with a small positive point estimate. Therefore, overall, the country-specific results are consistent with the predictions of the self-interest thesis.

Table 9. Regression results by country.

Country/region	N	Bandwidth	2nd stage effect of education on political ideology	1st stage effect of reform on education	1st stage F-Stat on excluded instrument
Austria	4403	7	-1.92 (3.84)	-0.105 (0.200)	0.279
Austria	6263	10	-0.647 (0.514)	-0.296*(0.161)	3.39
Austria	5690	9	-0.485 (0.461)	-0.311*(0.170)	3.33
Austria	3774	6	0.0496 (0.183)	-0.00241 (0.219)	0.000122
Belgium	5380	7	0.230 (0.572)	0.212 (0.162)	1.71
Belgium	7657	10	0.0269 (0.338)	0.277** (0.131)	4.50
Belgium	6930	9	0.0328 (0.408)	0.242* (0.139)	3.04
Belgium	4641	6	-0.102 (0.476)	0.266 (0.177)	2.25
Denmark	5093	7	-0.197 (0.564)	0.250 (0.285)	0.771
Denmark	7571	10	0.474 (0.628)	0.248 (0.224)	1.22
Denmark	6718	9	0.244 (0.421)	0.313 (0.242)	1.68

Country/region	N	Bandwidth	2nd stage effect of education on political ideology	1st stage effect of reform on education	1st stage <i>F</i> -Stat on excluded instrument
Denmark	4339	6	−0.137(0.364)	0.414 (0.314)	1.74
France	6748	7	0.0357 (0.227)	0.474 ⁺⁺ (0.189)	6.32
France	9535	10	−0.277 (0.324)	0.297 ⁺⁺ (0.153)	3.78
France	8570	9	−0.104 (0.227)	0.414 ⁺⁺⁺ (0.163)	6.46
France	5746	6	−0.000035 (0.201)	0.585 ⁺⁺⁺ (0.207)	8.03
Greece	4628	7	0.700 (0.652)	0.357 (0.246)	2.12
Greece	6392	10	−0.0531 (0.371)	0.307 (0.201)	2.35
Greece	5833	9	0.205 (0.437)	0.305 (0.213)	2.04
Greece	3965	6	0.855 (1.18)	0.246 (0.270)	0.829
Ireland	4803	7	−0.152 (1.61)	−0.0739 (0.162)	0.208
Ireland	6972	10	−0.358 (0.958)	−0.109 (0.132)	0.687
Ireland	6287	9	−0.0284 (1.02)	−0.0975 (0.140)	0.486
Ireland	4087	6	0.905 (3.21)	−0.0652 (0.182)	0.128
Italy	3532	7	0.339 (0.220)	0.911 ⁺⁺⁺ (0.306)	8.85
Italy	5045	10	0.339 [*] (0.196)	0.836 ⁺⁺⁺ (0.248)	11.37
Italy	4538	9	0.304 [*] (0.178)	0.953 ⁺⁺⁺ (0.263)	13.10
Italy	3071	6	0.358 ⁺⁺ (0.172)	1.31 ⁺⁺⁺ (0.339)	14.80
Netherlands	11,912	7	0.540 (0.413)	0.286 ⁺⁺ (0.142)	4.04
Netherlands	17,337	10	1.58 (1.53)	0.131 (0.115)	1.30
Netherlands	15,562	9	0.994 (0.861)	0.171 (0.122)	1.95
Netherlands	10,207	6	0.615 (0.542)	0.256 [*] (0.157)	2.66
Spain	4067	7	−0.993 (1.30)	0.239 (0.288)	0.691
Spain	5926	10	−0.871 (2.27)	0.101 (0.231)	0.190
Spain	5316	9	−0.381 (25.33)	0.0376 (0.249)	0.0227
Spain	3451	6	−4.93 (38.1)	0.0413 (0.320)	0.0167
Sweden	6192	7	0.316 (0.255)	0.542 ⁺⁺⁺ (0.212)	6.50
Sweden	8605	10	−0.150 (0.181)	0.581 ⁺⁺⁺ (0.173)	11.24
Sweden	7835	9	0.0481 (0.181)	0.588 ⁺⁺⁺ (0.184)	10.18
Sweden	5339	6	0.488 [*] (0.288)	0.601 ⁺⁺⁺ (0.233)	6.68

Country/region	N	Bandwidth	2nd stage effect of education on political ideology	1st stage effect of reform on education	1st stage F-Stat on excluded instrument
UK	9509	7	0.0906 (0.237)	0.355 ^{***} (0.123)	8.37
UK	13,391	10	0.311 (0.256)	0.290 ^{***} (0.0993)	8.54
UK	12,085	9	0.249 (0.257)	0.298 ^{***} (0.106)	7.93
UK	8222	6	0.0414 (0.239)	0.383 ^{***} (0.133)	8.26

Heteroskedastic robust standard errors are in parentheses.

Linear cohort trends, gender, and a quadratic of age are included.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Under the socialization thesis, individuals affected by the educational reforms simply learn more about the core values of their society. To the extent that the educational system explicitly or implicitly taught core values associated with a right-wing status, an additional year of education could push the individuals in that direction. I do not have information about how the explicit curricula may have differed across the different countries but there are ways to group countries by core values. One leading typology relies on classifying the welfare regime. [Esping-Anderson \(1990, 1999\)](#) divides European countries into three welfare regimes: liberal, conservative, and social democratic.¹⁹ Liberal countries include Ireland and the UK. Social democratic countries include Denmark and Sweden. Conservative countries include Austria, France, Greece, Italy, Netherlands, and Spain. Again referring to the country-specific results in [Table 9](#), I find evidence that both supports and conflicts with the socialization thesis. Second stage results are strongest for Italy, where it appears that more education moves individuals to the political right. This is supportive of the socialization thesis because Italy is classified as a conservative country. However, Sweden is classified as a social democratic country so the weak positive and significant result disagrees with the socialization thesis. The other two countries with consistently positive point estimates across the bandwidths, Netherlands and UK, fall respectively into Esping-Anderson's conservative and liberal (neo-liberal, laissez-faire) groupings, which appears consistent with the socialization thesis. However, the results that agree with the socialization thesis under Esping-Anderson's classification only happen in instances where the results also agree with the self-interest thesis.

I use one more metric to gauge the dominant core values of each country. For each country, I take the average of the left-right placement value for individuals who were 21 years or older when stopping full-time education. Recall that the compulsory education reforms primarily affected individuals in the lower end of the education distribution so this measure would reflect the left-right ideology of the

peers who were not affected. Appendix [Table A2](#) summarizes these averages by country and shows that the most left-leaning countries for this sample are Spain, France, and Netherlands. The most right-leaning countries are Austria, Denmark, Ireland, Italy, and Sweden. Belgium, Greece, and UK fall in between the others. Appendix [Table A3](#) shows 2SLS results on these groupings of countries. I find no effect of education on political ideology when limiting the sample to the left-leaning countries in column 1 but the first stage is somewhat weak. Moreover, the effect does seem strongest in the right-leaning countries (0.390 in column 2). This LATE for right-leaning countries is approximately 57% larger than the overall LATE in [Table 5](#) but it is also less precisely estimated with a borderline weak first stage. Column 5 of [Table A3](#) shows a LATE of 0.344 when excluding the left-leaning countries of Spain, France, and Netherlands and column 4 of [Table A3](#) shows a statistically insignificant LATE of 0.173 when excluding the right-leaning countries of Austria, Denmark, Ireland, Italy, and Sweden. Thus, I find no evidence that education moves individuals to the left for left-leaning countries. This suggests that the socialization thesis is not very potent but societal values may either dampen or intensify any self-interested movement to the right.

It is also informative to examine the first stage estimates of the effect of an educational reform on years of education by country. As seen in [Table 9](#), several of the countries display little evidence of reforms increasing educational attainment. Three such countries (Austria, Ireland, and Spain) produce negative second stage point estimates for most of the bandwidths. Previous studies also find the reforms in Spain ([Pons and Gonzalo, 2002](#)) and Ireland ([Callan and Harmon, 1999](#)) to be rather ineffective in increasing average educational attainment, and hence weak instruments. Across the countries, I find first stage effects that are mostly in line with previous studies. I find a first stage effect for the UK reforms (0.290–0.383) in [Table 9](#) that is consistent with previous studies from [Table A1](#). The first stage effect is comparatively high for France for shorter bandwidths, but comparable to previous studies for the bandwidth of 10. I estimate a first stage effect for Italy that is considerably higher than the effect found in [Brandolini and Cippolone \(2002\)](#) but lower than the first stage effect from [Di Pietro and Delprato \(2009\)](#). Likewise, the first stage effect in [Table 9](#) for Netherlands falls between those found by [Levin and Plug \(1999\)](#) and [van Kippersluis et al. \(2011\)](#). For Denmark, the first stage in [Table 9](#) is similar to that found by [Arendt \(2005\)](#). Finally, I estimate a first stage effect for Sweden that is somewhat higher than that found by [Meghir and Palme \(2005\)](#). However, [Meghir and Palme \(2005\)](#) have a more refined regional educational reform measure which could explain some of the difference.²⁰ Supporting this notion, the first stage effects from [Hjalmarsson et al. \(2015\)](#) when not including municipality fixed effects are similar to what I find in [Table 9](#) but similar to [Meghir and Palme \(2005\)](#) when including the municipality effects.

4.4. Robustness checks

Here, I conduct a series of robustness checks. In each case, I set the bandwidth at ± 7 years and utilize linear cohort controls.²¹ First, I exclude the countries that have the weakest or negative first stage effects of the reforms on educational attainment (Austria, Ireland, and Spain). As seen in column 1 of [Table 10](#), the second stage estimated LATE increases only slightly and becomes more precise. In Column 2, I restrict the sample further to countries that have the strongest evidence of the reforms increasing educational attainment (France, Italy, Sweden, and UK). The second stage estimated LATE decreases to approximately 0.19 (3.7% of the sample mean) but remains significant at the 5% level. Next, recall that I exclude individuals from the pivot cohorts in the baseline analysis. Column 3 of [Table 10](#) shows results when I include individuals from the pivot cohorts. As would be expected, the magnitude of the first stage decreases somewhat here because some individuals who were not actually treated with the reform are being coded as though they were treated. Nonetheless, the impact of education on political affiliation remains significant at the 10% level. In column 4 of [Table 10](#), rather than allowing each reform to take on its own trend, I estimate the model with a common trend. Again, there is only a slight change to the point estimates and the second stage estimate remains significant at the 10% level.

Table 10. 2SLS regression results, robustness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Selected countries	Selected countries	Include pivot cohort	Common Trend	Exclude Survey FE	Cluster SE at Reform Level	Include Regional Germany Reforms	Exclude Left-right = 5
<i>2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right</i>								
Education age	0.262*** (0.0978)	0.191** (0.0936)	0.190* (0.114)	0.296* (0.159)	0.248** (0.107)	0.249** (0.116)	0.238** (0.109)	0.312** (0.141)
<i>1st stage. Outcome is education age.</i>								
Reform	0.390*** (0.0685)	0.508*** (0.0918)	0.249*** (0.0548)	0.304*** (0.0777)	0.315*** (0.0594)	0.316*** (0.0657)	0.292*** (0.0566)	0.352*** (0.0727)
F-Stat on excluded instrument	33.44	30.64	20.66	15.30	28.13	21.15	26.66	24.23
(p-value)	<0.0001	<0.0001	<0.0001	0.0001	<0.0001	0.0003	<0.0001	<0.0001
N	52,994	25,981	70,685	66,267	66,267	66,267	71,419	47,784

Robust standard errors in parentheses are clustered at the birth year-reform level (except column 6). The bandwidth is ± 7 years.

All regressions include gender controls, a quadratic of age, reform fixed effects, and survey wave fixed effects (except (5)).

All except column 4 include linear cohort trends that can vary at the reform level.

Column 1 includes Belgium, Denmark, France, Greece, Italy, Netherlands, Sweden, and UK. Column 2 includes France, Italy, Sweden, and UK.

Column 4 includes a common cohort trend that does not vary at the reform level.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Next in column 5, I exclude the survey wave fixed effect. In column 6, I cluster standard errors at the reform level instead of the baseline birth year-reform level. In column 7, I include the regional reforms in Germany that are utilized by [Brunello et al. \(2009\)](#), assuming that individuals were educated in the same region in which they lived at the time of the survey. Finally in column 8, I exclude individuals who report a left-right political affiliation of 5 (centrist) in case they are fundamentally different in some way. As seen in columns 5–8 of [Table 10](#), the magnitude of the estimated LATE changes only modestly for these robustness checks and the estimates remain significant at the 5% level.

Another issue is that there are a relatively large number of individuals, approximately 13% of the sample, who refuse to answer the political affiliation question. This could be an indication that the individual is not politically engaged. Recall that these individuals are dropped from the main analysis. Therefore, I alternatively include these individuals in the sample and create a dichotomous variable for refusal to answer the political affiliation question. I replace “ideology” with this dichotomous refusal variable (1 = refuse, 0 = answer) in [Eq. 4](#) and estimate the model with 2SLS. I do not find any statistical evidence that education affects the probability of refusing to answer this question.²² Likewise, one may wonder if education significantly affects the probability responding with a “5” to the political ideology question given the large spike seen in [Fig. 1](#). Using the same samples from [Table 5](#), I replace “ideology” with a dichotomous dependent variable (1 = ideology of 5, 0 = other) and estimate the model with 2SLS. I find no evidence that education affects the probability of identifying as a political centrist.²³

As I am pooling reforms from many different countries, one may wonder whether it is one particular country that is driving the results. To investigate this issue I omit one country at a time from the sample and re-estimate the model. Appendix [Table A4](#) shows that it is not any one country that is responsible for the estimated effect. The estimated first stage effect of the reform on education always stays between 0.284 and 0.346. Also, although the precision of the estimated LATE changes depending on which country is omitted, the scale is always between 0.206 and 0.306 and always significant at conventional levels.

Finally, I report a series of falsification tests. In appendix [Table A5](#), I show RD estimates for six different false reform dates. In each case, I utilize the maximum bandwidth for the falsified reform dates that does not overlap with the true reform. For example, in column 1 of [Table A5](#), I move each educational reform back in time by 4 years relative to the true reform dates. In column 2 of [Table A5](#), I move each reform forward by 4 years. I then repeat this process for ± 7 years and ± 10 years relative to the true reform dates.²⁴ As seen in [Table A5](#), I find no evidence that these false reform dates significantly affect educational attainment; the first stage point estimates on “reform” are near 0 and have large standard errors. Furthermore, the second stage effect of education on political affiliation is never statistically significant while the point estimates are sometimes negative and sometimes positive. [Table A5](#) presents another set of falsification tests. In columns 1 and 2, I draw a random number from the uniform distribution over the interval of [1917, 1969] for each reform and assign this random year as the first affected birth cohort.²⁵ In contrast, in columns 3 and 4, I use the true reform dates but randomly assign a false level of educational attainment to each individual.²⁶ Once again, there is no evidence that these falsified reforms affect educational attainment or political ideology in columns 1 and 2. Likewise, there are no statistically significant findings for the falsified educational attainment in columns 3 and 4.

4.5. Local linear fuzzy RD results

[Table 11](#) reports the nonparametric local linear fuzzy RD results. Columns 1 and 2 of [Table 11](#) show results for two different optimal bandwidth choices. Column 3 shows results when I manually set the bandwidth to approximately 50% of the optimal bandwidth (± 5 years). Then, columns 4–5 report results for bandwidths of 7 and 10 to compare with the main parametric results in [Table 5](#). The conventional and bias-corrected local linear results are statistically significant and similar in magnitude to the main parametric results in [Section 4.1](#) when using the optimal bandwidths and conventional inference.²⁷ Results for the manual bandwidths in columns 3–5 are statistically significant at conventional levels only in some cases and statistical significance does not hold up when using the robust inference suggested by [Calonico et al. \(2014a\)](#). However, overall, the local linear results agree with the magnitude and sign of the parametric results of [Section 4.1](#).

Table 11. Local linear fuzzy RD estimates.

	(1)	(2)	(3)	(4)	(5)
<i>Structural estimates (2nd stage). Outcome is left-right affiliation</i>					
Conventional	0.303* (0.156)	0.298* (0.156)	0.353 (0.250)	0.285* (0.165)	0.320* (0.165)
Bias-corrected	0.306**	0.509***	0.516**	0.431***	0.261

	(1)	(2)	(3)	(4)	(5)
	(0.156)	(0.156)	(0.250)	(0.165)	(0.165)
Robust	0.306	0.509	0.516	0.431	0.261
	(0.277)	(0.379)	(0.554)	(0.313)	(0.285)
1st stage. Outcome is education age.					
Reform	0.262***	0.278***	0.265***	0.291***	0.236***
	(0.0658)	(0.0699)	(0.102)	(0.0781)	(0.0615)
N	76,056	76,056	37,558	56,842	85,344
BW type	CV	IK	Manual	Manual	Manual
BW local poly	9.0	8.14	5.0	7.0	10.0
BW bias	9.0	6.68	5.0	7.0	10.0

Local linear regressions used for point estimators. Local quadratic regressions used for bias correction.

Triangular kernel used in all cases. BW=bandwidth.

CV=optimal BW selected by cross-validation procedure ([Ludwig & Miller, 2007](#))

IK=optimal BW selected according to method of [Imbens and Kalyanaraman \(2012\)](#).

All estimates are generated using the rdrobust package in Stata. Conventional 1st stage estimates are reported.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

5. Conclusion

In this article, I use data from 11 European countries to identify the effect of increasing educational attainment on political ideology as measured by self-placement on a left-right scale. Following a technique now well established in the literature, I instrument for educational attainment with 18 compulsory education laws that increased the minimum school leaving age in these 11 countries during 20th century Europe. This provides an advantage over previous work that has examined the link between education and political ideology as I am able to produce the first causal estimates of the education moving some individuals along the political spectrum. Furthermore, these results also add to the growing literature concerning the non-pecuniary effects of education. Overall, I find robust evidence that an extra year of education moves the average reform complier to the right of the political continuum by approximately 5%.

Previous work provides conflicting theoretical predictions concerning the effect of educational attainment on political ideology. The self-interest thesis predicts that individuals will move to the right with more education whereas the developmental thesis predicts that individuals will move to the left with more education. The socialization thesis, on the other hand, predicts that the effect of

educational attainment will depend upon the values of the society. In the socialization theory, education will tend to move individuals to the right in more right-leaning societies and to the left in more left-leaning societies. In the present study, when pooling all of the reforms, I find that education moves individuals to the right on average. An analysis of each country independently shows that the strongest results occur where previous research has documented a positive return to earnings from the educational reforms. Education appears to have no causal effect on left-right political affiliation in countries where previous research has shown reforms to be ineffective in increasing earnings. This suggests that the likely causal mechanism is that education increases earnings, which then moves individuals to the right; this is consistent with the self-interest thesis.

This study adds to the growing evidence that individuals tend to support political ideologies that are aligned with one's own self-interest. [Doherty, Gerber, and Green \(2006\)](#) show that larger lottery winnings leads to less support of redistribution and more support of eliminating the estate tax. Likewise, [Powdthavee and Oswald \(2014\)](#) show that lottery winners increase their probability of voting right by approximately 4 percentage points. [Oswald and Powdthavee \(2010\)](#) empirically show that having a daughter moves one to the political left and having a son moves one to the political right, and theoretically shows why this can be in one's self-interest. In their sample of individuals from the UK, a parent is approximately 2 percentage points more (less) likely to vote left for each daughter (son). Thus, the scale of the LATE herein is about the same size as the estimated effect from [Powdthavee and Oswald \(2014\)](#) and twice the size of that from [Oswald and Powdthavee \(2010\)](#). However, one may expect the effect to be comparatively larger in the present case because the survey is taking place typically many years after the educational reform treatment and it may take some time for political views to evolve whereas [Powdthavee and Oswald \(2014\)](#) and [Oswald and Powdthavee \(2010\)](#) are identifying the more immediate effects on political ideology.

As in any instrumental variables identification strategy, these results apply for the specific instrument utilized and for the specific subgroup of individuals who are affected by the instrument. These compliers in this context would come from the lower end of the schooling distribution. Therefore, we should not use these results to infer what would happen in a very different context, for example the United States, under a different compulsory education reform. Similarly, these results cannot speak to the causal effect of education at the higher end of the educational attainment distribution such as the effect on political identity of treating individuals with another year of college. One would need a different instrument that affects individuals in this range of education to answer this question.

Finally, the results in this paper support the assessment of [Bauer et al. \(2015\)](#) that there can be problems with observational studies linking education to left-right identity. I find a positive association

between educational attainment and left-wing political ideology when failing to account for the endogeneity of education. An analysis that treats education as exogenous will likely suffer from omitted variables bias and the results herein show that the extent of the bias can be significant. Furthermore, there are reasons to believe in this context that multiple omitted variables may play a role and they may work in opposite directions. Thus, I would caution against inferring causality from studies that do not have an identification strategy to overcome this omitted variables problem.

Acknowledgment

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Appendix

Table A1. Reforms from the literature.

Country/region	Study	Reform date	Gender	1st stage	Ln earnings effect
Denmark	Arendt (2005)	1971	Men	0.262	N/A
Denmark	Arendt (2005)	1971	Women	0.166	N/A
England, Wales	Clark and Royer (2013)	1947	All	0.450***	N/A
England, Wales	Devereux and Hart (2010)	1947	All	0.506***	0.021
England, Wales	Devereux and Hart (2010)	1947	Men	0.47***	0.037**
England, Wales	Devereux and Hart (2010)	1947	Women	0.55***	-0.003
England, Wales	Clark and Royer (2013)	1972/73	All	0.353***	N/A
England, Wales	Grenet (2013)	1972/73	Women	0.310***	0.067***
England, Wales	Grenet (2013)	1972/73	Men	0.267***	0.069***
France	Albouy and Lequien (2009)	1937	All	0.11	N/A
France	Albouy and Lequien (2009)	1967	All	0.280***	N/A
France	Grenet (2013)	1967	Women	0.274***	-0.007
France	Grenet (2013)	1967	Men	0.263***	-0.004
Great Britain	Oreopoulos (2006a)	1947	All	0.436***	0.145**
Great Britain	Oreopoulos (2008)^a	1947	All	0.408***	0.066
Ireland	Callan and Harmon (1999)	1972	Men	Not reported ^b	0.10***
Italy	Brandolini and Cippolone (2002)	1963	Women	0.124***	0.207***

Country/region	Study	Reform date	Gender	1st stage	Ln earnings effect
Italy	Di Pietro and Delprato (2009)	1963	Men	1.69*	N/A
Italy	Flabbi (1999)	1963	Men	Not reported	0.053***
Italy	Flabbi (1999)	1963	Women	Not reported	0.030***
N. Ireland	Oreopoulos (2006a)	1957	All	0.391***	0.187**
N. Ireland	Oreopoulos (2008)	1957	All	0.444***	0.18***
Netherlands	van Kippersluis et al. (2011)	1928	Men	0.669***	N/A
Netherlands	Levin and Plug (1999)	1947, 1950, 1975	Men	-0.168, -0.399, -1.927***	0.064**
Spain	Pons and Gonzalo (2002)^c	1970	Men	0.82, 0.72*	-0.015
Sweden	Meghir and Palme (2005)	1962	All	0.298***	0.0142
Sweden	Hjalmarsson et al. (2015)	1962	Men	0.588*** (no municipality FE)	N/A
Sweden	Hjalmarsson et al. (2015)	1962	Women	0.400*** (no municipality FE)	N/A
Sweden	Hjalmarsson et al. (2015)	1962	Men	0.328*** (municipality FE)	N/A
Sweden	Hjalmarsson et al. (2015)	1962	Women	0.199*** (municipality FE)	N/A
UK	Harmon and Walker (1995)	1947, 1972/73	Men	0.54*, 0.11*	0.15**
UK	Oreopoulos (2006a)	1947/1957	All	0.397***	0.149***
UK	Oreopoulos (2008)	1947/1957	All	0.44**	0.105***

a[Oreopoulos \(2008\)](#) is a corrigendum to [Oreopoulos \(2006a\)](#).

b[Callan and Harmon \(1999\)](#) state that the reform is not a valid instrument and they do not use it in their final IV regressions.

c[Pons and Gonzalo \(2002\)](#) state that the reform is not a valid instrument and they do not use it in their final IV regressions.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Table A2. Left-right placement by country for individuals leaving full-time education at 21 years or older.

Country	N	Mean	Std. Dev.
Austria	824	5.38	2.02
Belgium	3346	5.04	1.85
Denmark	4974	5.20	2.10

Country	N	Mean	Std. Dev.
Greece	1808	5.06	2.08
France	1959	4.80	1.98
Ireland	1129	5.45	1.81
Italy	707	5.15	2.56
Netherlands	6566	4.80	1.96
Spain	1032	4.44	2.01
Sweden	3994	5.50	2.15
United Kingdom	2068	5.07	1.87

Note: This summarizes the sample generated by a bandwidth of ± 10 years.

Table A3. 2SLS regression results, countries grouped by left-right placement.

	(1)	(2)	(3)	(4)	(5)
Included countries	Left	Right	Center	Left and center	Right and center
<i>2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right</i>					
Education age	0.0800 (0.153)	0.390* (0.208)	0.294 (0.194)	0.173 (0.120)	0.344** (0.142)
<i>1st stage. Outcome is education age.</i>					
Reform	0.345*** (0.120)	0.296*** (0.102)	0.317*** (0.0771)	0.332*** (0.0727)	0.306*** (0.0657)
F-Stat on excluded instrument	8.27	8.42	16.93	20.86	21.68
(p-value)	0.0051	0.0050	0.0001	<0.0001	<0.0001
N	22,727	24,023	19,517	42,244	43,540

Robust standard errors in parentheses are clustered at the birth year-reform level. The bandwidth is ± 7 years. All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

Left group = France, Netherlands, and Spain. Right group = Austria, Denmark, Ireland, Italy, and Sweden. Center group = Belgium, Greece, and UK.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Table A4. Regression results leaving out 1 country.

Omitted country	N	2nd stage effect of education age on political ideology	1st stage effect of reform on education age	1st stage F-Stat on excluded instrument
Austria	61,864	0.206** (0.0961)	0.342*** (0.0630)	29.39

Omitted country	N	2nd stage effect of education age on political ideology	1st stage effect of reform on education age	1st stage <i>F</i> -Stat on excluded instrument
Belgium	60,887	0.251 ⁺⁺ (0.107)	0.327 ⁺⁺⁺ (0.0641)	26.10
Denmark	61,174	0.271 ⁺⁺ (0.108)	0.327 ⁺⁺⁺ (0.0606)	29.04
Greece	61,639	0.206 ⁺ (0.108)	0.313 ⁺⁺⁺ (0.0624)	25.13
France	59,519	0.296 ⁺⁺ (0.125)	0.296 ⁺⁺⁺ (0.0598)	24.44
Ireland	61,464	0.251 ⁺⁺ (0.105)	0.346 ⁺⁺⁺ (0.0630)	30.21
Italy	62,735	0.226 ⁺ (0.123)	0.284 ⁺⁺⁺ (0.0593)	22.92
Netherlands	54,355	0.211 ⁺ (0.111)	0.321 ⁺⁺⁺ (0.0656)	23.93
Spain	62,200	0.306 ⁺⁺⁺ (0.112)	0.324 ⁺⁺⁺ (0.0602)	29.05
Sweden	60,075	0.239 ⁺⁺ (0.119)	0.293 ⁺⁺⁺ (0.0626)	21.94
United Kingdom	56,758	0.282 ⁺⁺ (0.125)	0.307 ⁺⁺⁺ (0.0679)	20.45

Robust standard errors in parentheses are clustered at the birth year-reform level. The bandwidth is ± 7 years. All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Table A5. Falsification tests, reform dates moved backward and forward in time.

	(1)	(2)	(3)	(4)	(5)	(6)
2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right						
Education age	-0.383 (0.571)	-0.303 (4.04)	0.0947 (0.435)	0.726 (0.958)	-0.361 (0.453)	0.546 (0.843)
1st stage. Outcome is education age.						
Reform	-0.0774 (0.0693)	0.0343 (0.0663)	0.0726 (0.0534)	0.0517 (0.0518)	0.0788 (0.0504)	0.0382 (0.0440)
<i>F</i> -Stat on excluded instrument	1.25	0.268	1.85	0.996	2.45	1.00
N	36,129	39,641	59,678	71,224	77,728	101,114
False reform year	-4	4	-7	7	-10	10
Bandwidth	4	4	7	7	10	10

Robust standard errors in parentheses are clustered at the birth year-reform level. The bandwidth is ± 7 years. All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

Table A6. Falsification tests, random reform years and random educational attainment.

	(1)	(2)	(3)	(4)
2nd stage. Outcome is left-right affiliation. 1 = far left, 5 = center, 10 = far right				
Education age	-4.83	7.00	-12.34	-2.82
	(11.62)	(38.20)	(130.44)	(7.10)
1st stage. Outcome is education age				
Reform	0.0292	-0.0123	-0.00634	-0.0231
	(0.0694)	(0.0673)	(0.0669)	(0.0568)
F-Stat on excluded instrument	0.177	0.0332	0.0090	0.165
N	47, 709	67, 911	66,267	94,694
Reform year	Random	Random	Actual	Actual
Education	Actual	Actual	Random	Random
Bandwidth	7	10	7	10

Robust standard errors in parentheses are clustered at the birth year-reform level. The bandwidth is ± 7 years. All regressions include gender controls, a quadratic of age, linear cohort trends that can vary at the reform level, reform fixed effects, and survey wave fixed effects.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

Fig. A1. Density of the forcing variable. Note: Histogram shows the density of observations according to birth cohort. Interval (k) = 0 corresponds to the pivot cohort.

Appendix B. Supplementary materials

Research data for this article

Open Data

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application 1

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¹Some studies find no effects of education on health ([Albouy and Lequien, 2009](#); [Clark and Royer, 2013](#)) or inconclusive effects ([Arendt, 2005](#)). [James \(2015\)](#) finds effects for some health outcomes but not for others and [Brunello et al. \(2013\)](#) find an effect for females but no effect for males.

²The Eurobarometer wave numbers are: 62.1, 62.2, 63.1, 63.2, 64.3, 64.4, 65.1, 66.2, 66.3, 67.1, 67.2, 68.1, 68.2, 69.1, 69.2, 70.1, 71.1, 72.4, 73.1, 74.2, 74.3, 75.1, 75.2, 76.1, and 77.2.

³I do not include the smaller number of waves that take place exclusively in summer months because this would introduce more measurement error in the year of birth.

⁴Further details about the surveys are provided in [Meyer \(2015\)](#). Eurobarometer primary data files are publicly available from GESIS (for example, [European Commission, 2014](#)).

⁵Including individuals in the sample who report an age higher than 35 when stopping education only slightly changes point estimates across all specifications.

⁶[Table 1](#) of [Gathmann et al. \(2015\)](#) list two early reforms that do not apply to this analysis. Denmark's 1903 reform first affected those born in 1890 whereas the earliest birth year in Denmark in the present sample is 1914. Similarly, Belgium's 1919 reform first affected those born in 1910 whereas earliest birth year in Belgium in the present sample is 1912.

⁷I exclude the regional level reforms from Finland because the within-country NUTS codes in some of the Eurobarometer surveys do not map cleanly into the [Brunello et al. \(2009\)](#) regions; definitions of within-country NUTS regions change over time. I also exclude regional level reforms from Germany because I am not able to identify the region of birth. I discuss this further in [Section 4](#).

⁸Consistent with [Meyer \(2015\)](#), I exclude individuals born in the pivot cohort from the sample because I cannot accurately identify whether or not they were treated with the reform.

⁹I also include robustness checks using multiple other bandwidths.

¹⁰Exogenous controls such as age and gender can help reduce the sampling variability in the RD estimates ([Lee and Lemieux, 2010](#)). Other potentially endogenous controls such as income that are simultaneously determined with educational attainment should not be included in the regression design ([Angrist and Pischke, 2009](#)).

¹¹For the linear specification, $\gamma_3 = \gamma_4 = 0$.

¹²For the linear specification, $\lambda_3 = \lambda_4 = 0$.

¹³[Brunello et al. \(2009\)](#) also includes similar interactions.

¹⁴I thank an anonymous referee for making this point.

¹⁵However, as seen in appendix [Fig. A1](#), I do not see any evidence of heaping at the pivot cohort when examining the density of the forcing variable. Likewise, the test suggested by [Cattaneo, Jansson, and Ma \(2016a\)](#) finds no evidence of a discontinuity in the density of the forcing variable at the pivot cohort. This test, implemented on a bandwidth of 7 by the `rddensity` command ([Cattaneo, Jansson, & Ma, 2016b](#)) in Stata reports test statistics (in parentheses) for three approaches: conventional (−0.217), undersmoothed (0.0089), and robust bias-corrected (0.0747). [Barreca, Lindo, and Waddell \(2016\)](#) more deeply explore this issue of bias from heaping in the running variable.

- ¹⁶I utilize the `rdrobust` package in Stata for all nonparametric local linear regressions ([Calonico, Cattaneo, & Titiunik, 2014b](#)).
- ¹⁷[Brunello et al. \(2013\)](#) use a similar clustering strategy.
- ¹⁸I utilize [Roodman's \(2011\)](#) `cmp` program within Stata 14.1 to estimate the parameters of this system of equations.
- ¹⁹As emphasized by [Esping-Anderson \(1999\)](#), “readers should be warned that ‘liberal’ and ‘conservative’ are used here in their classical, European usage. Liberal therefore does not imply leftist, but rather a laissez-faire, neo-liberal view; conservative does not refer to ‘whatever is not liberal’, be it the Christian right or the Republican party, but to the European usage of the term.”
- ²⁰Another difference is that [Meghir and Palme \(2005\)](#) impute years of education from categorical education levels.
- ²¹Results for these robustness checks from other bandwidths are available upon request.
- ²²Using the analogous specifications to columns 1 and 3 of [Table 5](#), the 2nd stage coefficient on education is 0.0198 with a standard error of 0.0206 for a bandwidth of 10 ($n = 109,238$) and 0.0180 with a standard error of 0.0178 for a bandwidth of 7 ($n = 76,326$).
- ²³Using the analogous specifications to columns 1 and 3 of [Table 5](#), the 2nd stage coefficient on education is 0.0016 with a standard error of 0.023 for a bandwidth of 10 ($n = 94,694$) and 0.013 with a standard error of 0.022 for a bandwidth of 7 ($n = 66,267$).
- ²⁴[Grenet \(2013\)](#) conducts similar falsification tests in moving the reforms forward or backward several years.
- ²⁵This follows the fake reform falsification test from [Borgonovi et al. \(2010\)](#). I exclude Netherlands from this falsification test because the randomly selected years would cause the falsified reforms to overlap.
- ²⁶I take the true observations on educational attainment for the entire sample and randomly assign each true observation to a different individual.
- ²⁷One exception is the bias-corrected point estimate in column 2 is substantially larger than the others.