EDUCATION AND ANTI-IMMIGRATION ATTITUDES: EVIDENCE FROM COMPULSORY SCHOOLING REFORMS ACROSS WESTERN EUROPE

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Low levels of education are a powerful predictor of anti-immigration sentiments. However, there is little consensus on the interpretation of this important correlation: is it causal or does it result from powerful selection biases? We provide a first step toward answering this question by exploiting compulsory schooling reforms in five Western European countries with politically-influential anti-immigration movements. We find that compelling high school drop outs to stay on for at least an additional year decreases anti-immigration attitudes later in life. Instrumental variable estimates imply that each additional year of secondary schooling reduces opposition to immigration, and the belief that immigration erodes a country's quality of life, by around ten percentage points. This study is the first to our knowledge to document that education causally reduces anti-immigration attitudes, and suggests that increasing education can breed tolerance for immigration. We discuss the implications for future research on the determinants of anti-immigration sentiment.

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1 Introduction

Amidst the uncertainty ushered in by Brexit, the Trump victory, and the electoral strength of far-right candidates in France or the Netherlands, one fact stands out as undisputed: a citizen's level of education is one of the best predictors of support for populist far-right candidates (Becker and Fetzer 2016; Cottrell, Herron and Westwood 2016).[1] To explain this pattern, researchers and pundits have emphasized the mediating role of anti-immigration attitudes. Because the less educated are more hostile to immigration, they are also more likely to embrace platforms that link immigrants to criminality, stagnating wages, higher taxes, or to the decline of "native culture" (Becker, Fetzer and Novy 2016; Hainmueller and Hopkins 2014; Mudde 2012).[2] Such assertions beg the question of the nature of the relationship between education and anti-immigrant sentiment. Is this relationship causal? In other words, does education decrease anti-immigration attitudes?

Two non-competing set of theories argue that education's effect on anti-immigration attitudes is indeed causal. One focuses on ethnocentrism and argues that schooling, because it "explicitly promote(s) tolerance, improve(s) knowledge of and appreciation for foreign cultures, and create(s) cosmopolitan social networks," generates "more pro-immigrant sentiment among more educated individuals" (Hainmueller and Hopkins 2014:79). In this "education-as-character-shaping" literature, the experience of education directly translates into attitudinal differences: "education changes outlook," providing "one of the few known social brakes against intolerance and other antidemocratic sentiments" (Napier and Jost 2008:614).

A second line of argument highlights the emergence in advanced democracies of a new structural cleavage pitting those adversely affected by deindustrialization, automation, and globalization—

[1] See also: "How education level is the biggest predictor of support for Geert Wilders," *The Financial Times*, March 1st 2017. Available at ft.com/dutchvoting. "French election results: Macron's victory in charts," *The Financial Times*, May 9th 2017. Available at ft.com/content/62d782d6-31a7-11e7-9555-23ef563ecf9a.

[2] Examples from mainstream media are numerous; e.g. *The Nation*, "Fear of Diversity Made People More Likely to Vote Trump," March 17th 2017, thenation.com/article/fear-of-diversity-made-people-more-likely-to-vote-trump.

both materially and symbolically—against those benefiting from these structural changes (Kitschelt 1997; Kriesi et al. 2012). Individuals without qualifications fall on the losing side of this cleavage. Consequently, the activation of physical and cultural boundaries through economic protectionism and ethnocentric moral worldviews may boost one's self-worth and social status (Peugny 2009; Quillian 1995; Shayo 2009). In this "education-as-cleavage" literature, education not only matters because of what happens while one is getting an education but also because of everything else that happens after exiting the educational system: the successful completion of additional degrees translates into different life expectations and experiences (e.g. lower unemployment risks, higher wages, cosmopolitan social networks etc.) that make one less likely to hold anti-immigration preferences.

For both lines of work, the robust negative correlation between education and anti-immigration sentiment is believed to be (at least partly) causal. However, "we should be careful before inferring causation" (Hainmueller and Hiscox 2010). The correlation between education and anti-immigration sentiment could also reflect selection biases, as individuals dropping out of formal education are likely to differ substantially in terms of upbringing and opportunities from those who do not drop out (see Kam and Palmer 2008).

In this research letter, we leverage quasi-experimental variation in education in Western Europe to examine the relationship between education and immigration attitudes. Specifically, we use a regression discontinuity (RD) design to exploit six major compulsory schooling reforms that differentially affected cohorts in Denmark, France, Great Britain, the Netherlands, and Sweden. The reforms, on average, increased the average student's secondary schooling by 0.3 years, without affecting tertiary education. Our design enables us to credibly identify the effect of compulsory education reforms and an additional grade of late high school on anti-immigration attitudes in countries currently facing significant anti-immigration pressures.

Pooling across countries, we find that inducing individuals otherwise likely to have dropped out to remain in secondary schooling significantly decreases hostility to immigrants and immigration.

In particular, each additional year of late high school reduces strong opposition to immigration and the belief that immigration makes the country a worse place to live by 10 and 14 percentage points respectively. While lacking statistical power, our country-by-country analyses reinforce these patterns and suggest that education induces greater tolerance for immigrants more strongly in France and the Netherlands.

These findings, which are the first to our knowledge to document strong evidence of a causal relationship between education and immigration attitudes, indicate that the widely-cited attitudinal differences between high school drop outs and the rest of the population cannot be solely attributed to selection bias. The paper concludes by discussing the implications for future research.

2 Research design

Following the end of World War II, countries across Western Europe passed laws raising the legal age at which a child is permitted to leave compulsory education to join the labor market. Individuals belonging to the cohort subject to the higher leaving age are, in effect, randomly assigned a strong incentive to remain in school for an additional year. Compulsory schooling laws are widely used in labor economics to estimate the returns to schooling (e.g. Acemoglu and Angrist 2000; Grenet 2013; Oreopoulos 2006).[3] We follow these approaches to identify the effect of education on anti-immigration attitudes, using an RD design to compare individuals from cohorts just young enough to be affected by the reform to individuals from cohorts just too old to have been affected.

[3]These laws have also been used to examine turnout in the U.S. (Milligan, Moretti and Oreopoulos 2004) and vote choice in Great Britain (Marshall 2016*b*).

Table 1: Compulsory education reforms

Country	Date of reform passing	Year reform came into effect	Change in minimum school leaving age	Change in years of compulsory education	Year of birth of first affected cohort
Denmark*	1975	1972	14 to 16	7 to 9	1957
France	1959	1967	14 to 16	8 to 10	1953
Great Britain	1944	1947	14 to 15	9 to 10	1933
Great Britain†	1962	1972	15 to 16	10 to 11	1957
Netherlands	1975	1974	15 to 16	9 to 10	1959
Sweden	1962	1965	14 to 15	8 to 9	1951

Notes: *We code the first affected reform in Denmark as those born in 1956, as there appear to have been anticipatory effects. †The second reform in Great Britain was first passed in 1962, but was not implemented until a 1972 statutory instrument.

2.1 Data

We focus on six major compulsory schooling reforms across five countries; namely, one reform in Denmark, France, the Netherlands and Norway, and two reforms in Great Britain.[4] Table 1 lists the six reforms, highlighting the date and nature of the reform, as well as the cohorts affected. Detailed information on each reform is provided in Appendix A.1. Importantly for our question at hand, all five countries in our sample now have politically-influential anti-immigration movements.

Our empirical strategy requires a large sample of respondents from cohorts born just late enough and not quite late enough to be affected by the reform. Accordingly, we pool seven waves of the European Social Survey (ESS) collected between 2002 and 2012.[5] A key feature of the ESS is its battery of six items on immigration attitudes repeated in each wave of the survey. Since the reforms generally occurred in the 1960s and 1970s, these surveys capture downstream attitudes

[4]We first identified 12 countries that passed reforms in the post-war period. We dropped the seven countries—Austria, Finland, Germany, Greece, Ireland, Italy, and Spain—where the reform did not significantly increase education levels in our sample. The British reforms did not affect Northern Ireland.

[5]ESS uses strict random probability methods to construct samples to be nationally representative of residents aged 15 and above. All surveys are conducted in-person, and sample around 2,000 people per country-round.

Table 2: Item Wording

1	To what extent do you think [country] should allow people of the same race or ethnic group as most [country] people to come and live here ?	Allow Many / Some / Few / None
2	How about people of a different race or ethnic group from most [country] people?	Allow Many / Some / Few / None
3	How about people from the poorer countries outside Europe?	Allow Many / Some / Few / None
4	Is it generally bad or good for [country]'s economy that people come to live here from other countries?	0/10 scale: Bad (0) ... Good (10)
5	Is [country]'s cultural life generally undermined or enriched by people coming to live here from other countries?	0/10 scale: Undermined (0) ... Enriched (10)
6	Is [country] made a worse or a better place to live by people coming to live here from other countries?	0/10 scale: Worse (0) ... Better (10)

once respondents are middle-aged.

2.2 Outcome of interest: anti-immigration attitudes

To measure anti-immigration attitudes, we use the six survey items listed in table 2. The first three items reflect preferences for the number of different types of immigrant allowed into the country (from many to none). The second set of three items reflect beliefs about the consequences of immigration on the economy, national culture and quality of life. Unfortunately, simply examining the raw responses encounters two important issues.

The first issue is the difficulty of ensuring comparability when data are pooled across countries and waves. Our solution is to re-code survey responses as binary variables anchored around substantively unambiguous reference points. We transform the first three variables into binary variables equal to 1 when respondents chose the end-of-scale "allow none" answer. However, social desirability bias might induce respondents to avoid such an uncompromising answer, inducing respondents to select "few" immigrants instead of "none." Consequently, we also define indicators for respondents answering “few” or “none.” We examine the effects of schooling reforms on both outcomes. Due to its unambiguous reference point, we prefer the former. To similarly improve comparability, we re-code items 4-6 as dummies indicating whether an individual has chosen a non-neutral anti-immigration response category, namely 0-4. Not only is 5 the cut-off value separating pro- from anti-immigration responses, it is also the value at which respondents bunch, as inferred from the heaping of responses at 5.

A second issue afflicting items 1-3 is item substitutability. The stereotypical immigrant is both poor and an ethnic outsider. Respondents with the same level of opposition to immigration might express this opposition differently, depending on whether they perceive immigrants through a economic or ethnic lens. In addition, respondents may be unwilling to express multiple anti-immigrant sentiments. Examining these items separately would thus risk under-estimating attitudinal differences across cohorts. We consequently combine items 1-3 as a dummy variable indicating whether a respondent has chosen the extreme “allow none” response category on *any one* of the three items.[6] We repeat this procedure including individuals who answered “few” or “none.”

Our final outcome variable combines the six binary variables that we derive from the ESS to produce an additive scale of anti-immigration sentiment designed to capture an individual’s latent *predisposition* to toward immigration. By taking the average across these outcomes, we follow Ansolabehere, Rodden and Snyder (2008:215), who argue that using “a large number of survey items on the same broadly defined issue area (...) eliminates a large amount of measurement error.” More information on this scale—which has a high inter-item reliability coefficient of 0.77—is available in Appendix A.2.

[6] In practice, the variation driving our results reflects the items asking about poor and non-majority immigrants; see Appendix A.2.

2.3 Identification strategy

To identify the effects of compulsory education reform eligibility, we use a RD design to compare cohorts just young enough to be affected by the reforms to cohorts just too old to be affected. We thus define our treatment—being affected by a compulsory education reform—for respondent i from cohort b in country c as:

$$reform_{bc} = \begin{cases} 0 & \text{if } birth\ \ year_{bc} - birth\ \ year\ \ first\ \ affected_{bc} > 0 \\ 1 & \text{if } birth\ \ year_{bc} - birth\ \ year\ \ first\ \ affected_{bc} \leq 0 \end{cases} \tag{1}$$

where $birth\ \ year\ \ first\ \ affected_{bc}$ is the birth year of the first cohort affected by a reform. We then identify the local average treatment effect of our compulsory education reforms, among cohorts just young enough to be affected by the reforms, by estimating the following regression equation:

$$y_{ibc} = \beta reform_{bc} + f(x_{bc}) + \varepsilon_{ibc}, \tag{2}$$

where y_{ibc} is an individual's anti-immigration attitudes, and f is a function of our running variable $x_{bc} \equiv birth\ \ year_{bc} - birth\ \ year\ \ first\ \ affected_{bc}$. Specifically, f implements a local linear regression using a triangular kernel and the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). Intuitively, this entails flexibly controlling for trends across (around ten) cohorts separately either side of the discontinuity, while assigning higher weights to the cohorts immediately around the discontinuity. We run the analysis on the pooled data to increases the design's power. To address concerns regarding heterogeneous treatment effects, we also report results by country.

This design enables us to estimate the causal effect of raising the school leaving age among the first students to be affected, provided that potential outcomes are continuous through the cohort eligibility threshold (e.g. Angrist and Pischke 2008). Our identification strategy thus relies on affected cohorts being comparable to unaffected cohorts around the reform. We validate this assumption in Appendix A.3. First, figure A1 and the associated density tests unsurprisingly indicate that individuals born around the discontinuity did not manipulate their birth year to sort into receiving the reform. Second, A3 shows that cohorts just young enough to be affected by the reform are similar to those just too old to be affected according to 13 predetermined covariates. Third, section A.1 describes the context of the reforms, highlighting that the compulsory education reforms are not confounded by other changes differentially affecting those subject to the reform.

Many students would have stayed in school absent a reform. Accordingly, another key quantity of interest is the effect of schooling itself. To identify the local average treatment effect of an additional year of late secondary schooling on those that only completed additional schooling because of a reform, we estimate a "fuzzy" RD by using the reforms to instrument for the number of completed years of schooling using an analogous local linear regression:[7]

$$y_{ibc} = \beta years\ \ of\ \ completed\ \ schooling_{ibc} + f(x_{bc}) + \varepsilon_{ibc}, \tag{3}$$

where $years\ \ of\ \ completed\ \ schooling_{ibc}$ is instrumented using the following first stage regression:

$$years\ \ of\ \ completed\ \ schooling_{ibc} = \gamma reform_{bc} + f(x_{bc}) + \varepsilon_{ibc}. \tag{4}$$

Identification requires two additional assumptions: the reforms do not decrease education levels for any student (monotonicity), and only affect anti-immigration attitudes through their effect on completing additional years of schooling (exclusion restriction). These assumptions are particularly plausible in this context, as students are unlikely to respond by completing less education and it is hard to see how the reforms could influence downstream attitudes without working through the additional time spent in school (see Marshall 2016*b*). We nevertheless discuss possible violations

[7]We use years of schooling—rather than a dummy for completing high school—because Marshall (2016*a*) demonstrates that such a coarsening substantially upwardly biases instrumental variables estimates.

below.

3 Findings

3.1 Compulsory schooling increases late secondary education

Figure 1 and table 3 first verify that the compulsory education reforms indeed increased schooling among affected cohorts. We measure formal education as the number of completed years of education up to a limit of 13 years. This limit captures the end of secondary education, after which additional education varies across students.[8]

Figure 1 shows a notable discontinuity in the number of years of completed schooling around all six reforms. Column (1) of panels A-F in table 3 reports RD estimates for each country separately, indicating that most reforms significantly increased the number of years of completed schooling among affected cohorts by around 0.3 years. The 1947 reform in Great Britain produced a particularly dramatic increase of 0.6 years. Panel G confirms that the average effect pooled across countries is large and statistically significant. Table A4 in the Appendix shows that these increases in education are concentrated around students' 9th-13th years of schooling, but do not systematically affect tertiary education. In sum, these results demonstrate that the reforms achieved their goal of substantially raising secondary education levels among students that would not have otherwise remained in school.

3.2 Compulsory schooling decreases anti-immigration attitudes

Figure 2 and columns (2)-(7) of table 3 compare anti-immigration attitudes between pre- and post-reform cohorts. We start with columns (2) and (3), which examine preferences for the number of immigrants, considering both types of coding rule ("none" in column (2), and "few" or "none" in

[8]The limit does not drive our results since no reform affected post-secondary education (see table A4).

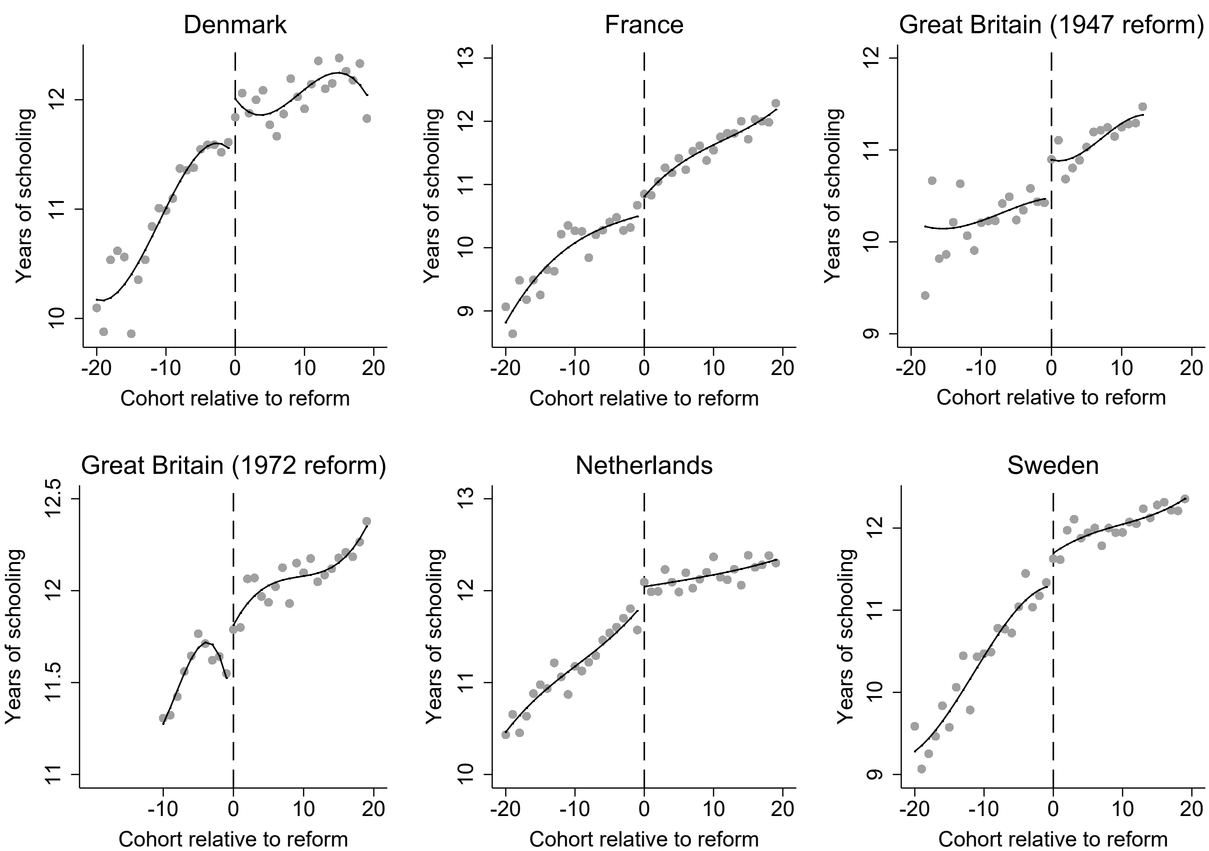

Figure 1: Years of completed schooling among cohorts around compulsory schooling reforms, by reform (third-order polynomials either side of the reform)

Table 3: The effect of compulsory education on years of completed schooling and anti-immigration attitudes

	Years of completed schooling (1)	Anti-immigration ("none" only) (2)	Anti-immigration ("none" or "few") (3)	Immigration is bad for the economy (4)	Immigration undermines local culture (5)	Immigration reduces local livability (6)	Anti-immigration scale (7)
Panel A: Reduced form RD estimates—Denmark							
Reform	0.319	-0.011	-0.019	-0.024	-0.026	-0.023	-0.020
	(0.212)	(0.021)	(0.033)	(0.030)	(0.030)	(0.029)	(0.021)
Bandwidth	7	11	11	12	9	10	10
Observations	2,878	4,587	4,587	4,965	3,735	4,166	4,166
Outcome mean	11.72	0.11	0.60	0.32	0.21	0.21	0.29
Panel B: Reduced form RD estimates—France							
Reform	0.285*	-0.053**	-0.031	-0.052	-0.062*	-0.092**	-0.062**
	(0.156)	(0.025)	(0.030)	(0.036)	(0.033)	(0.036)	(0.025)
Bandwidth	11	12	11	8	9	8	8
Observations	5,078	5,465	5,078	3,861	4,278	3,861	3,861
Outcome mean	10.84	0.20	0.61	0.38	0.36	0.40	0.39
Panel C: Reduced form RD estimates—Great Britain (1947 reform)							
Reform	0.600**	-0.048	0.052	-0.063	-0.067	0.001	-0.037
	(0.272)	(0.062)	(0.056)	(0.063)	(0.073)	(0.049)	(0.050)
Bandwidth	3	4	4	4	3	6	3
Observations	1,191	1,492	1,492	1,492	1,191	2,130	1,191
Outcome mean	10.70	0.33	0.76	0.45	0.46	0.51	0.5
Panel D: Reduced form RD estimates—Great Britain (1972 reform)							
Reform	0.259	-0.026	-0.033	0.056	0.081	0.006	0.012
	(0.207)	(0.042)	(0.056)	(0.051)	(0.057)	(0.052)	(0.035)
Bandwidth	3	4	3	4	3	4	4
Observations	1,647	2,152	1,647	2,152	1,647	2,152	2,152
Outcome mean	11.79	0.23	0.61	0.42	0.40	0.43	0.42
Panel E: Reduced form RD estimates—Netherlands							
Reform	0.204*	-0.018	-0.056*	-0.050	-0.040	-0.111***	-0.054**
	(0.107)	(0.024)	(0.034)	(0.033)	(0.026)	(0.037)	(0.023)
Bandwidth	11	8	8	8	8	6	7
Observations	5,867	4,282	4,282	4,282	4,282	3,293	3,789
Outcome mean	11.79	0.14	0.49	0.29	0.15	0.29	0.27
Panel F: Reduced form RD estimates—Sweden							
Reform	0.280**	-0.014	-0.010	0.002	-0.002	-0.021	-0.008
	(0.130)	(0.013)	(0.026)	(0.029)	(0.023)	(0.023)	(0.016)
Bandwidth	11	10	11	10	7	10	11
Observations	4,759	4,394	4,759	4,394	3,188	4,394	4,759
Outcome mean	11.40	0.04	0.21	0.26	0.10	0.15	0.15
Panel G: Reduced form RD estimates—all reforms pooled							
Reform	0.317***	-0.028**	-0.026*	-0.010	-0.004	-0.043***	-0.025***
	(0.069)	(0.011)	(0.015)	(0.012)	(0.012)	(0.015)	(0.010)
Bandwidth	7	9	8	11	10	8	8
Observations	19,524	24,798	22,125	29,725	27,461	22,125	22,125
Outcome mean	11.43	0.17	0.53	0.36	0.28	0.33	0.33
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled							
Years of completed schooling		-0.098**	-0.087*	-0.035	-0.014	-0.138***	-0.082**
		(0.039)	(0.050)	(0.044)	(0.043)	(0.052)	(0.034)
Bandwidth		9	8	11	10	8	8
Observations		24,798	22,125	29,725	27,461	22,125	22,125
First stage F statistic		22.6	21.7	24.3	23.5	21.4	21.7
Years of completed schooling mean		11.43	11.44	11.43	11.43	11.44	11.44

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

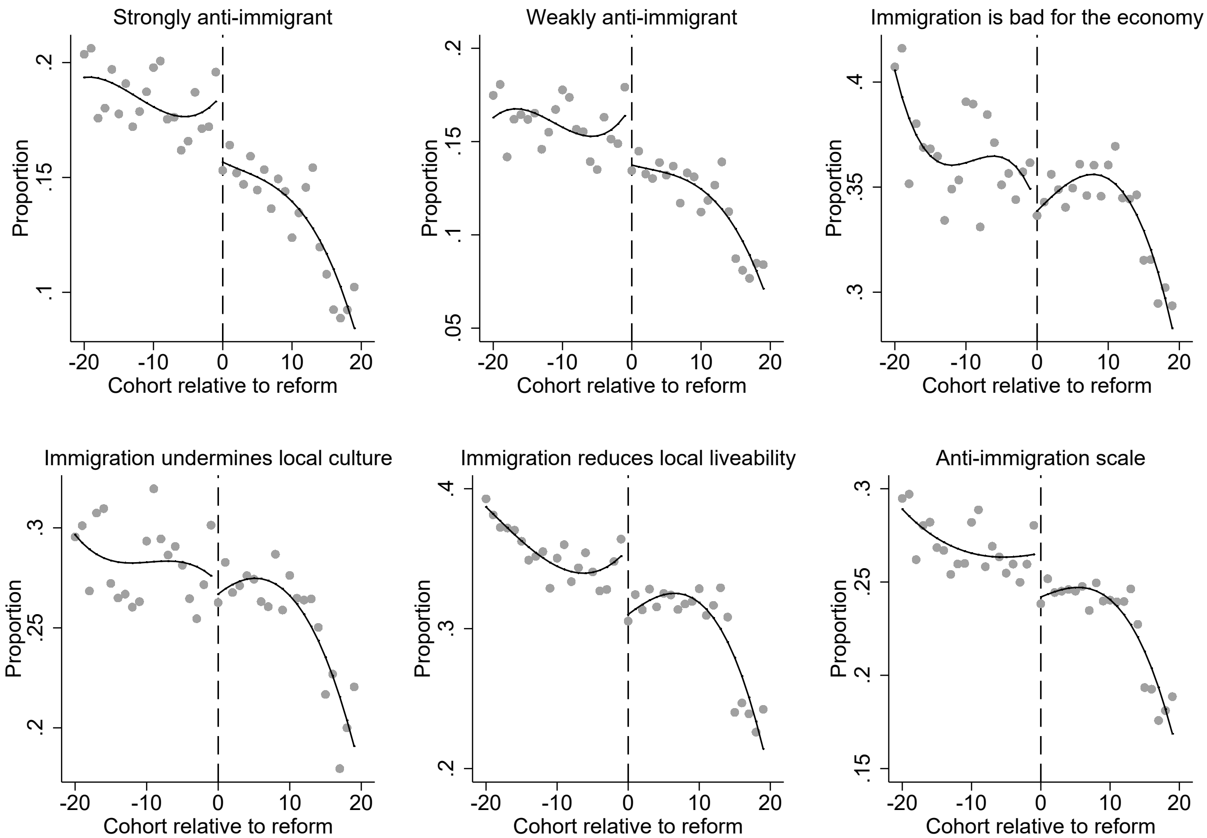

Figure 2: Anti-immigrant attitudes among cohorts around compulsory schooling reforms, pooled across reforms (third-order polynomials either side of the reform)

column (3)). In all countries, our point estimates are negative: affected cohorts, on average, are less likely to oppose immigrants later in life. The effects are greatest in France and the Netherlands. Pooling across countries to increase the precision of our estimates, the difference in attitudes is statistically significant and indicates that affected students are around 3 percentage points less likely to oppose immigration later in life. Given that only a minority of students were affected by the reform, these differences across cohorts are substantial.

Columns (4)-(6) examine assessments of the cost and benefits of immigration. The most consistent finding is that the reforms reduce the belief that immigration make one's own country a worse place to live by 4.3 percentage points later in life. This difference is large in all countries except Great Britain, even where not statistically significant in the small country-by-country samples. In contrast, the two items considering the economy and culture are more ambiguous: while students exposed to Danish, French, and first British reforms register notable reductions in the belief that immigration harms the economy or undermine local culture, the other reforms do not.

Finally, column (7) presents the results for our anti-immigration attitudes scale—the mean of the preceding five items. The overall pattern is the same: the reforms consistently produced large reductions in anti-immigration sentiments among affected cohorts. This finding is strongest in France and the Netherlands, and weakest in Great Britain and Sweden.

3.3 Late secondary education decreases anti-immigration attitudes

The preceding results demonstrate that compulsory schooling reforms decreased anti-immigration sentiments among affected cohorts. To examine the effects of schooling itself, we turn to the instrumental variables (IV) results reported in panel H of table 3. Our estimates show that secondary schooling substantially decreases anti-immigration attitudes among students that complied with the reform.[9] The estimates imply that, for each such student, an additional year of schooling reduces the probability of expressing anti-immigration attitudes by around 10 percentage points. Moreover, the probability of stating that immigration negatively affects quality of life in the country declines by 14 percentage points.

The magnitude of these estimates indicate that secondary education plays a key role in inducing tolerant attitudes towards immigration later in life among students that would otherwise have dropped out. Our estimates—which are around three times larger than the cross-sectional correlations reported in table A11—also suggest that relatively uneducated reform-compliers could be particularly susceptible to education's tolerance-inducing effects.

[9] The F statistic for each regression indicates a strong first stage (see table A10 for full first stage estimates).

3.4 Robustness checks

The reduced form results are robust to standard specification tests. First, figure A2 in the Appendix shows that the pooled estimates robustly reduce anti-immigration attitudes for any bandwidth between 2 and 15, with the strongest effects estimated closest to the discontinuity. Second, table A5 shows that our results are unaffected by increasing the weight attached to estimates further from the discontinuity by using a rectangular—instead of a triangular—kernel. Third, a placebo test in table A6 using a placebo reform occurring five years earlier indicates that our findings do not simply reflect pre-trends or other proximate social or institutional changes.

As noted above, our IV estimates rely on the exclusion restriction that the compulsory schooling reforms only affected downstream anti-immigration attitudes by increasing schooling. Although many downstream experiences are themselves a function of schooling, a violation could occur if, for example, an individual that does not remain in school when subject to the reform married earlier and had children earlier because their peer group was altered by others continuing in school. However, table A7 in the Appendix shows that the reform did not significantly affect such life experiences. Violations could also arise if the reforms simultaneously changed the nature of schooling. With the exception of Sweden—where the reform integrated the student population by changing the tracking system—Appendix section A.1 indicates that the experience of schooling itself did not substantially change for affected cohorts. Table A8 shows that excluding Sweden produces similar results. Although no test can definitively support the exclusion restriction, these checks suggest that the most plausible exclusion restrictions are not biasing our estimates.

4 Conclusion

The transition out of high school has attracted significant attention from researchers and journalists who routinely highlight the substantial gap in immigration attitudes between high school drop outs and the rest of the population. It is believed that such gaps have important political consequences as right-wing parties running anti-immigration campaigns seek to exploit these divisions. This research letter demonstrates that an additional year of late high school education substantially decreases the probability that an individual opposes immigration and believes that immigration makes it less attractive to live in the country. By leveraging exogenous variation in exposure to a reform increasing the number of years of compulsory schooling, we are able to overcome the significant challenges encountered by previous studies investigating the causal relationship between education and anti-immigration attitudes.

Although a strength of our design is that we can isolate the effects of late *secondary* education, given that the reforms did not affect tertiary education, future work is required to examine whether university education—the salient difference in educational attainment among young people today—induces comparable or perhaps greater tolerance for immigration. Recent evidence from the U.S. suggests that such effects might differ by campus (Mendelberg, McCabe and Thal forthcoming).

Another important challenge for future research is to disentangle the mechanisms driving schooling's effects. This research letter establishes a significant causal effect, but cannot identify whether it is educational content or the changes in life trajectory it induces that drive tolerance toward immigration. Understanding these mechanisms is essential for policy-makers and activists seeking to influence tolerance for immigration. The education-as-character-shaping perspective suggests that ever-increasing absolute education levels will eventually bring about tolerance, although reformers could expedite this process by altering curricula and raising school leaving ages. In contrast, the education-as-cleavage perspective suggests that relative education levels are central and that attitudes may be contingent upon the particular pressures and inequalities defining contemporary societies.

Future studies might build on our findings by utilizing surveys conducted closer to the reforms themselves to separate whether tolerance towards immigration emerges quickly after leaving school, as socialization theories suggest, or only later in life, as life trajectory theories suggest. Future studies might also exploit survey-specific contextual variation, in order to examine how the effects of education are amplified by changes in the return to education or in anti-immigrant elite messaging. Regardless, we hope that this letter inspires rigorous analysis seeking to understand a defining issue of early twenty-first century politics in advanced democracies that demands academic attention.

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A Online appendix

Contents

A.1 Compulsory schooling reforms selected for this study

The six reforms presented in the paper were selected following a two-step procedure. First, of the 17 countries frequently covered by the ESS, we single out 12 countries that have passed compulsory schooling reforms since WWII. Drawing from previous studies in labor economics (**???**), we identify the date of birth of the first affected cohort and examine whether the effects of the reforms on formal education are captured in the ESS data. Second, we drop the seven countries where compulsory education reforms did not significantly increase education levels in our sample: Austria, Finland, Germany, Greece, Ireland, Italy, and Spain.

In this section, we provide a brief description of each reform considered in the study. Based on previous work (**???**Grenet 2013; Marshall 2016*b*), we highlight that—with the potential exception of Sweden (see below)—the "treated" and "control" cohorts did not experience other significant differences other than the length of compulsory education that was required when they were students. We focus on two main types of potential confounds. One is educational changes that could have simultaneously affected the *type* of education, rather than the *length* of education, received by cohorts affected by the reforms. The other is changes in the legal voting age that could have differentially affected the pre- and post-treatment cohorts.

A.1.1 Denmark

In 1971, compulsory schooling requirements were increased by two years, from 7 to 9 years. Pupils who were 14 (or younger) in 1971 were potentially affected by the reform. While the reform was implemented starting in 1971, the official legal amendment was not passed until 1975 (**?**). In other words, individuals born before 1957 (14 in 1970) completed compulsory schooling in 7 years, whereas individuals born from 1957 onward (14 in 1971) had to spend 9 years in full-time education following this reform. In our data, we find a clear discontinuity when coding the first affected cohort as being 14 in 1970 (i.e. those born in 1956), not 1971. Such anticipatory effects were not documented by the other study that examines the Danish 1971 reform (**?**), although there remains some debate in the literature about the exact nature of the reform (see **?**).

The education system underwent structural changes several years after the extension of leaving age. In 1975, tracking was abolished in secondary school. These changes happened post-reform and do not confound our identification strategy. Similarly, the reform does not coincide with the reduction in the voting age from 20 to 18 that occurred in 1978 (i.e. it first affected those born in 1960, rather than 1956).

A.1.2 France

In 1959, the compulsory schooling age was increased from 14 to 16—the *Berthoin* reform. This reform first affected individuals who were starting compulsory education in 1959, namely students aged 6 or above in 1959. The reform was consequently fully implemented once this cohort reached the age of 14 in 1967.

Up to the 1959 reform, the educational system was mostly characterized by a two-track system. A short track combined five years of primary school and three years of secondary education, leading to a final test taken at the age of 14 (the *Certificat d'Etudes Primaires*). The longer track combined five years of primary school with seven years of secondary school leading to the selective *Baccalaureat*. The 1958 reform launched a gradual process of unification of secondary education into a four-year curriculum that would align with the new compulsory leaving age. The unification process ended in 1977 with the creation of the *College Unique*. According to Grenet (2013), the reform mainly affected pupils from underprivileged background (e.g. the drop out rate among sons and daughters of farm workers decreased by 20%). This expansion to accommodate new students did not result in a dramatic change in the type of education received for those staying on.

In 1978, France changed the legal voting age from 21 to 18. The cohorts affected were born between 1958 and 1960, several years after the 1953 discontinuity this study focuses on.

One event is worth mentioning as a potential cause for the comparatively larger effect in France.

The year following the reform, Mai 68 broke out. The first "treated" individuals would have been 15 or 16 at that time. However, many of the Mai 68 events took place in universities and in high schools (less so in *college*, where these students would have been). Nevertheless, Mai 68 has often been interpreted as a youth revolt against the morally and culturally conservative mainstream, and thus could have interacted with the additional year of secondary education to produce long-term differences in anti-immigration attitudes. While this could account for the larger effects observed in France, it is unlikely to violate the RD identifying assumption because it is hard to believe that Mai 68 *discontinuously* influenced those aged 14 as opposed to 15.

A.1.3 Great Britain

In 1944, legislation was enacted under Prime Minister Winston Churchill war government to increase the school leaving age from 14 to 15 for all students. The Education Act 1944 raised the leaving age in England and Wales, while the Education (Scotland) Act 1945 subsequently enacted the same reform in Scotland. The new leaving age came into force on April 1st 1947, following a required for intensive preparation, and thus affected children aged 14 (or younger) in 1947 (see (Marshall 2016*b*). Moreover, Marshall (2016*b*) notes that "Given that the most significant post-war changes in the education system had already been implemented by 1947, the large rise in enrollment reflected the higher leaving age rather than other changes in the education system. Fees for secondary schooling were removed in 1944, while the new Tripartite systemwhich formally established three types of secondary school emphasizing academic, scientific and practical skillscame into force in 1945." The results in figure 1, as well as Marshall (2016*b*) and Oreopoulos (2006) indicate that these earlier structural reforms did not affect enrollment rates. Furthermore, Marshall (2016*b*) notes that other proximate reforms did not differentially affect cohort either side of the reform: "Spending increased in the 1950s as the National Health Service expanded following its roll-out on July 5th 1948, and the Beveridge Report's social welfare provisions were implemented. Such universal programs did not differentially impact cohorts either side of the school leaving age reform."

Britain's second major educational reform, which raised the school leaving age from 15 to 16, was implemented in 1972. Conservative Prime Minister Harold Macmillan presided over plans to raise the school leaving age to 16 in the Education Act 1962. However, it was not until Conservative Prime Minister Edward Heath that schooling leaving age increase was finalized in Statutory Instrument 444 (1972). Statutory Instrument 59 (1972) similarly raised the leaving age in Scotland, although it was not fully implemented until the Education Act 1976 due to teacher shortages. As with the first reform, the reform discontinuity does not coincide with unaffected students becoming eligible to vote at the 1974 elections.

A.1.4 The Netherlands

In the 1970s, when the 1975 reform took place, the Dutch system implemented early tracking. At the age of 12, students either took the general track (with an additional 4 or 6 years) or the vocational track (with an additional 3 or 4 years). In 1975, all three-year vocational programs were extended to four years. The focus of the extra year had to be on general skills rather than vocational skills: 20 weekly lessons in general training (**?**). At the same time, the compulsory school leaving age was increased by one year (from 15 to 16).

The implementation of the reform was incremental such that in 1973, all three years vocational programs had become four years programs. We follow **?** and consider those who were 15 in 1974 (born in 1959) to be those who were the most consequentially affected.

The reform mostly affected individuals receiving a vocational education. This expansion from three to four years was aimed at accommodating students with a low social background to make sure they met the new legal leaving age. A second goal was to make sure students in vocational programs were exposed to more "general" education. This general content was already present in their training and the reform does not represent a dramatic change in the type of education received for those staying on an additional year.

In addition, the reform discontinuity (i.e. those born in 1959) does not coincide with the lowering of the voting age from 21 to 18 in 1971 (which mainly affected those born in 1953).

A.1.5 Sweden

Building on **?**, we focus on the education reform covering the 1949 and 1962 period. Before the reform, pupils attended a basic compulsory school (*folkskolan*) until 6th grade (11-12). In 7th grade, those with better marks moved on to junior secondary school (*realscolan*), followed by upper secondary and university. Others remained in basic compulsory schooling followed by vocational schooling. Compulsory schooling lasted between seven and eight years depending on the municipality (students usually started around 5-6).

From 1949 to 1962, Sweden experimented with a new education structure creating a nine-year compulsory comprehensive school and consequently increasing the compulsory number of schooling years from 8 until 9 (i.e. from age 6 until age 15). Within this school, students could pick after 6th grade between different tracks: academic, general and vocational. Grades were no longer the determining factor. Over the 1949-1962 period, new municipalities were added to the "test" group every year. The coverage of the reform did not increase sharply until the early 1960s (in 1961, 25% of municipalities were in the "test" group). The national implementation took place in 1962 (75% remaining joined the new system) and we use this year as our main reform year. In some municipalities, the reform applied to individuals who were in 1st grade in 1962 (born in 1955 or after). In others, it applied to individuals in all cohorts up to 5th grade (born in 1951 or after). We follow **?** and define individuals reaching 14 in 1965 (born in 1951) as the first cohort affected by the reform. Figure 1 supports this cut-off point empirically.

The experience of "treated individuals" was different from that of the control group in that all pupils were under the same roof (and not in separate schools) and grade no longer affected which track one ended up in. Social interactions were thus most likely different though the specific curriculum content was not. We consider Sweden as a borderline and have ran the analysis with and without this country and found similar results.

As with other countries, the reform discontinuity—those born in 1951 versus those born before—does not coincide with the lowering of the voting age from 20 to 18 in 1972 (which affected individuals born in 1953 onward).

A.2 Outcome Variables

A.2.1 "Letting more immigrants in": some descriptive statistics

In section 2.2 of the main paper, we discuss the issue of item substituability: the same level of anti-immigration preferences can be expressed through different combinations of answers. For instance, one might strongly oppose immigrants from a different ethnicity ("allow none") but conditional on same ethnicity, be willing to let in poor migrants ("allow some"). The same logic applies for strongly opposing the entry of poor immigrants, but conditional on only letting in rich migrants, be fine with ethnically different migrants. In this paper, we consider either set of attitudes as constituting opposition to immigration, hence our decision to generate an indicator variable equal to one if respondents oppose at least one type of migrants.

The examples given above focus on poor immigrants and on immigrants from a different ethnic background than that of the majority. The ESS also asks about opposing immigrants from the same ethnicity as the majority. In practice, differences in how respondents answer this latter item does not drive our results. Indeed, individuals who oppose letting in more immigrants belonging to the majority groups almost always oppose letting in at least one of the other two types of migrants. The converse is not true.

Tables A1 and A2, illustrate this point. We recompute our anti-immigration ("allow none") indicator variable using only the items asking about "poor" and "different ethnicity" (as opposed to also including "same ethnicity"). We then cross-tabulate with our main outcome variable computed using all three items. Only 193 respondents out of $54,000$ are coded as 0 on the former and 1 on

Table A1: Consequences of including and excluding the "same ethnicity" item

		Without "same Ethnicity" 0	1	
All three items	0	45,771	0	45,7771
	1	**193**	8,376	8,568
		45,964	8,376	54340

Table A2: Answers conditional on opposing either poor immigrants or immigrants from a different ethnicity

Same race/ethnic group as majority	Percent
Allow many	**4**
Allow some	**25**
Allow few	35
Allow none	35
Total	100

the latter. In other words, less than 1% of respondents oppose immigrants from the same ethnic background while supporting immigrants from a different background or from poor countries. In contrast, as shown in table A2, conditional on opposing either "poor" immigrants or immigrants from a "different ethnicity", a third of respondents (29%) welcome immigrants from the same ethnicity.

A.3 Checks on the identification assumptions

The key concern in RD designs is that a variable other than the treatment simultaneously changes at the discontinuity. In addition to the discussion above of the lack of other major reforms affecting students affected by the compulsory schooling reforms, we now provide three common classes of statistical test to validate the "no sorting" assumption.

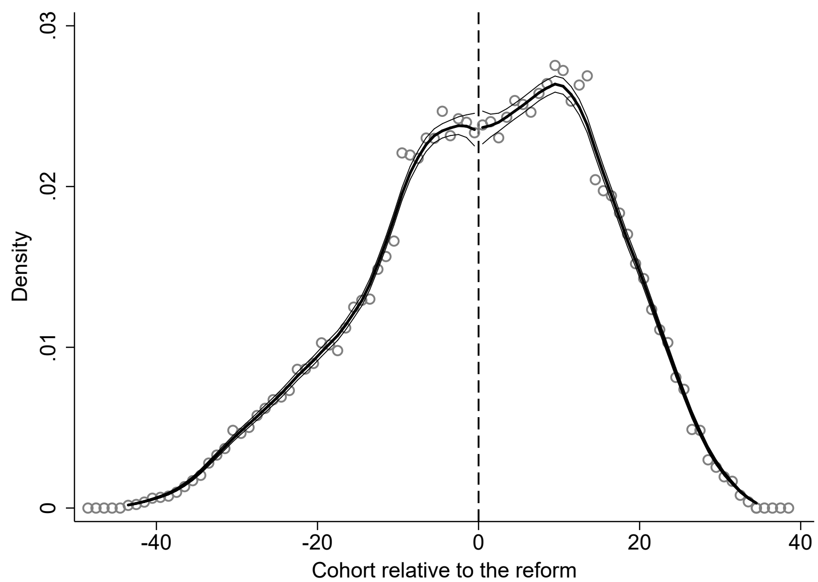

Figure A1: Density of data either side of the reform, pooled across countries

A.3.1 Density tests

Although selection into cohorts is implausible since parents could not have anticipated the timing of compulsory education reforms before their children were born, we nevertheless first examine whether there is heaping around the reform. If our sample contains more respondents affected by the reform than not, this could indicate either strategic sorting or a problem with sampling. Fortunately, figure A1 shows that there is no evidence of heaping. This graphical observation is supported by a **?** test, which fails to reject the null hypothesis that the density does not change through the reform. Based on a bandwidth of 5, the difference in density at the discontinuity is 0.009 (standard error of 0.039). Moreover, the test proposed by **?** for the case of a discrete running variable similarly finds no difference in density at the discontinuity (p=0.85 for k=0 and p=1.00 for k=0.1), while the density test proposed by Calonico, Cattaneo and Titiunik (2014) also finds no significant difference in density (p=0.40). Unreported country-by-country results similarly show that there are not significant differences in density.

A.3.2 Continuity tests

Even though the density of the data is similar across those either side of the reforms, it remains possible that students that were just eligible for a reform are different from those that were ineligible. We examine this possibility in table A3 by testing for continuity across the discontinuity for 13 pre-determined variables. The estimates, which are based on based on the same estimation strategy used to estimate the results in the main paper, show that respondents either side of the continuity are statistically indistinguishable on characteristics determined before the reform occurred. The one statistically significant difference is broadly consistent with chance.

A.4 Which levels of schooling were affected by the reforms?

Table A4 shows the "first stage" RD estimates documenting the effect of each additional year of schooling separately, both across countries and pooled across countries. As noted in the main paper, the results indicate that the largest increases in schooling are concentrated between the 9th and 13th years of formal schooling. The final column shows that the reforms did not significantly affect tertiary education.

A.5 Robustness checks

This section documents the results of the robustness checks cited in the main paper.

A.5.1 RD estimates by bandwidth

Figure A2 examines how our estimates vary with the choice of bandwidth. We examine all bandwidths between 2 and 15; our estimator cannot use a 1-cohort bandwidth. For all bandwidths, our main findings are robust in terms of consistently producing statistically significant negative coefficients. As the bandwidth increases, and thus more data is included either side of the discontinuity, the effect decreases in magnitude. This likely reflect weaker identification further away from the

Table A3: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, pooled across countries

	Female	Ethnic minority	Father born in country	Father education level	Mother born in country	Mother education level	Survey round 1	Survey round 2	Survey round 3	Survey round 4	Survey round 5	Survey round 6	Survey round 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Reform	-0.034**	-0.008	0.009	0.035	0.001	-0.018	0.011	-0.012	-0.005	0.012	0.002	-0.014	0.002
	(0.016)	(0.005)	(0.009)	(0.052)	(0.010)	(0.042)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.010)
Bandwidth	8	10	10	6	8	6	7	7	8	7	7	7	8
Observations	22,125	27,374	27,335	14,929	22,088	15,479	19,524	19,524	22,125	19,524	19,524	19,524	22,125
Outcome mean	0.53	0.96	0.12	1.98	0.12	1.69	0.15	0.14	0.15	0.14	0.14	0.15	0.14

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A4: The effect of compulsory education on level of completed schooling

	Completed at least ... of schooling								Any
	6 years	7 years	8 years	9 years	10 years	11 years	12 years	13 years	tertiary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Denmark									
Reform	0.028	0.030	0.031	0.045*	0.048*	0.036	0.028	0.008	0.004
	(0.017)	(0.019)	(0.023)	(0.026)	(0.027)	(0.031)	(0.035)	(0.035)	(0.039)
Bandwidth	7	7	6	6	8	8	8	9	8
Observations	2,878	2,878	2,478	2,478	3,286	3,286	3,286	3,735	3,272
Outcome mean	0.96	0.95	0.93	0.91	0.86	0.80	0.75	0.67	0.46
Panel B: France									
Reform	-0.016	0.013	0.009	0.100	0.070**	0.039	0.022	0.011	0.021
	(0.015)	(0.015)	(0.024)	(0.025)	(0.031)	(0.032)	(0.034)	(0.033)	(0.030)
Bandwidth	7	11	7	11	9	10	9	9	8
Observations	3,450	5,078	3,450	5,078	4,278	4,692	4,278	4,278	3,851
Outcome mean	0.96	0.94	0.90	0.82	0.73	0.62	0.51	0.38	0.25
Panel C: Great Britain (1947 reform)									
Reform	0.004	-0.001	0.020	0.040	0.226***	0.094	0.099*	0.129*	0.104*
	(0.011)	(0.014)	(0.028)	(0.041)	(0.054)	(0.062)	(0.059)	(0.068)	(0.060)
Bandwidth	3	3	2	2	5	4	4	3	3
Observations	1,191	1,191	839	839	1,816	1,492	1,492	1,191	1,142
Outcome mean	0.99	0.99	0.97	0.96	0.74	0.46	0.34	0.29	0.20
Panel D: Great Britain (1972 reform)									
Reform	0.006	-0.004	0.011	0.003	0.020	0.050	0.078*	0.063	-0.003
	(0.015)	(0.017)	(0.020)	(0.023)	(0.022)	(0.041)	(0.046)	(0.049)	(0.052)
Bandwidth	3	3	3	3	4	4	5	4	4
Observations	1,647	1,647	1,647	1,647	2,152	2,152	2,706	2,152	2,107
Outcome mean	0.99	0.99	0.98	0.98	0.96	0.84	0.60	0.48	0.39
Panel E: Netherlands									
Reform	0.003	0.006	0.011	0.019	0.032*	0.034	0.082***	0.040	0.022
	(0.007)	(0.010)	(0.013)	(0.017)	(0.017)	(0.024)	(0.028)	(0.034)	(0.032)
Bandwidth	10	10	9	8	12	11	10	8	8
Observations	5,323	5,323	4,800	4,282	6,412	5,867	5,323	4,282	4,273
Outcome mean	0.99	0.98	0.96	0.94	0.88	0.78	0.71	0.58	0.31
Panel F: Sweden									
Reform	0.005	0.005	0.045**	0.034*	0.055**	0.042	0.048	0.066*	0.010
	(0.005)	(0.007)	(0.019)	(0.018)	(0.026)	(0.029)	(0.033)	(0.035)	(0.032)
Bandwidth	11	10	6	9	11	11	10	9	10
Observations	4,759	4,394	2,754	5,119	4,759	4,759	4,394	4,031	4,379
Outcome mean	0.99	0.99	0.96	0.89	0.79	0.72	0.60	0.49	0.31
Panel G: all reforms pooled									
Reform	0.004	0.005	0.015***	0.031***	0.072***	0.054***	0.071***	0.051***	0.019
	(0.004)	(0.005)	(0.007)	(0.009)	(0.011)	(0.013)	(0.017)	(0.016)	(0.014)
Bandwidth	9	8	7	8	9	9	6	7	8
Observations	24,798	22,125	19,524	22,125	24,798	24,798	16,888	19,524	21,893
Outcome mean	0.98	0.97	0.95	0.92	0.84	0.72	0.60	0.49	0.33

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

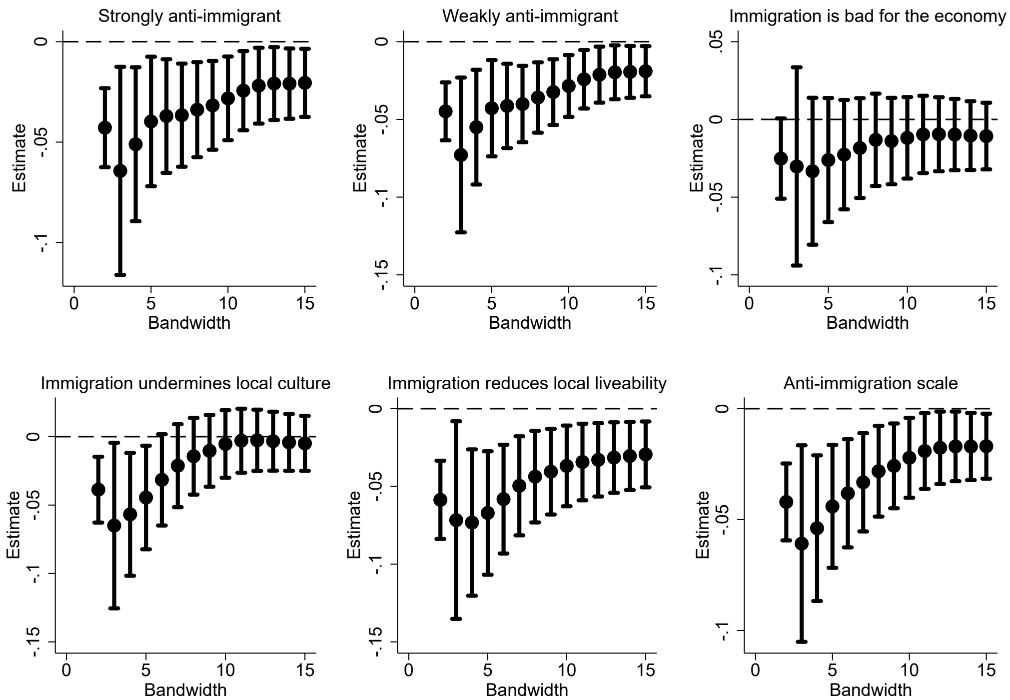

Figure A2: The effect of compulsory education on anti-immigration attitudes, by RD bandwidth

discontinuity. Fortunately, our findings are strongest closest to the discontinuity. Moreover, the estimates close to the discontinuity in fact suggest that the reforms may also reduces the beliefs that immigration is bad for the economy and undermines local culture.

A.5.2 Rectangular kernel results

The estimates in table A5 demonstrate that the results are similar when we use a rectangular instead of a triangular kernel for the RD estimation. This places greater weight on observations further from the discontinuity.

Table A5: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, using a rectangular kernel

	Years of completed schooling (1)	Anti-immigration ("non" only) (2)	Anti-immigration ("none" or "few") (3)	Immigration is bad for the economy (4)	Immigration undermines local culture (5)	Immigration reduces local livability (6)	Anti-immigration scale (7)
Reform	0.355***	-0.020**	-0.019**	-0.016	-0.007	-0.049***	-0.026**
	(0.077)	(0.010)	(0.009)	(0.013)	(0.015)	(0.017)	(0.011)
Bandwidth	5	9	9	8	6	5	6
Observations	14,316	24,798	24,798	22,125	16,888	14,316	16,888
Outcome mean	11.45	0.16	0.14	0.35	0.27	0.33	0.25

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a rectangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.5.3 Placebo reform five years earlier

Table A6 examines the effect of placebo reform set to occur five years before the actual reforms. The results clearly show that the placebo reform neither increased schooling nor replicates our main findings. If anything, the coefficients are positive, although our immigration scale measure shows that overall we do not detect any consistent effect on anti-immigration attitudes.

A.5.4 Exclusion restriction tests

The fuzzy RD/instrumental variables estimates in the main paper rely on two additional assumptions beyond those required for the reduced form RD estimates. In particular, the fuzzy RD estimates require the standard instrumental variables assumptions that: (1) compulsory schooling reforms do not decrease years of completed schooling for any student (monotonicity); and (2) the compulsory schooling reforms only affect anti-immigration attitudes through their effect on additional years of completed schooling (exclusion restriction). The former assumption is intuitively likely to hold (see also Marshall 2016*b* for a formal check in Great Britain), but the exclusion restriction may not. Although the proximity of the reform to the decision to remain in school means that many downstream life events are also a function of schooling, Marshall (2016*b*) notes that raising the school leaving age could, for example, affect life choices—such as marriage or having children—by changing the marital pool or social networks available to those that did not continue in school, but without operating through schooling itself.

We assess exclusion restriction violations of this type by examining whether the reform altered experiences that could have been directly affect by the reform, as opposed to by the increased schooling it induced, *and* subsequently influenced immigration attitudes later in life. To test whether the reform affected marital or fertility choices, table A7 estimates the effect of the compulsory reforms on indicators of such life choices. The results show that none of these indicators were significantly affected by the reform, and thus suggest that the most plausible violations

Table A6: The effect of placebo reforms (occurring five year earlier) on years of completed schooling and anti-immigration attitudes

	Years of completed schooling (1)	Anti-immigration ("non" only) (2)	Anti-immigration ("none" or "few") (3)	Immigration is bad for the economy (4)	Immigration undermines local culture (5)	Immigration reduces local livability (6)	Anti-immigration scale (7)
Placebo reform	0.091	0.015	0.035**	-0.022*	-0.029**	-0.009	-0.006
	(0.087)	(0.012)	(0.017)	(0.013)	(0.013)	(0.014)	(0.009)
Bandwidth	5	8	7	11	10	10	11
Observations	13,775	20,174	18,059	26,304	24,299	24,299	26,304
Outcome mean	0.21	0.17	0.53	0.36	0.28	0.33	0.33

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A7: Exclusion restriction tests

	Never married (1)	Ever divorced (2)	Child at home (3)	Ever had a child (4)
Reform	-0.004	-0.002	0.012	-0.025
	(0.010)	(0.010)	(0.018)	(0.016)
Bandwidth	9	11	5	8
Observations	23,748	28,439	14,316	22,125
Outcome mean	0.14	0.15	0.36	0.47

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

of the exclusion restriction are unlikely to significantly affect our findings.

A.5.5 Resulting removing Sweden

As noted above, Sweden's reform differed from other reforms because it included a significant change in the tracking system as well as a rise in the compulsory school leaving age. This creates the possibility that the change in the experience of schooling due to the reduction in tracking violates the exclusion restriction, and could thus invalidate the IV estimates. To address this, table A8 reports the reduced form and IV results excluding Sweden. The results are somewhat stronger.

A.6 Additional results

A.6.1 Ordinal measurement of anti-immigration outcomes

As above and in the main paper, there are good reasons to believe that a binary coding of our outcome variables is more informative than a continuous coding, which imposes a linear functional form and implies that the differences between ordered categories are equal. Nevertheless, table A9 presents the results when using the original (standardized) ordinal measures of our main

Table A8: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, excluding Sweden

	Years of completed schooling (1)	Anti-immigration ("none" only) (2)	Anti-immigration ("none" or "few") (3)	Immigration is bad for the economy (4)	Immigration undermines local culture (5)	Immigration reduces local livability (6)	Anti-immigration scale (7)
Panel A: Reduced form RD estimates—all reforms pooled							
Reform	0.325***	-0.032**	-0.032**	-0.013	-0.013	-0.042***	-0.026**
	(0.077)	(0.013)	(0.015)	(0.014)	(0.015)	(0.015)	(0.011)
Bandwidth	7	9	9	11	8	9	9
Observations	16,336	20,767	20,767	24,966	18,493	20,767	20,767
Outcome mean	0.19	0.19	0.6	0.38	0.31	0.37	0.37
Panel B: Fuzzy RD (instrumental variables) estimates—all reforms pooled							
Years of completed schooling		-0.109**	-0.109**	-0.047	-0.042	-0.143**	-0.089**
		(0.045)	(0.054)	(0.049)	(0.049)	(0.056)	(0.037)
Bandwidth		9	9	11	8	9	9
Observations		20,767	20,767	24,966	18,493	20,767	20,767
Outcome mean		0.19	0.6	0.38	0.31	0.37	0.37
First stage F statistic		18.8	18.8	19.8	18.2	18.6	18.8
Years of completed schooling mean		11.43	11.43	11.44	11.43	11.43	11.43

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

outcomes.[10] As can be seen, we find broadly similar results: although the point estimates rarely reach statistical significance, the point estimates consistently point in the same direction. Moreover, the IV estimates in panel H suggest that the magnitudes are non-trivial: each additional year of schooling reduces the outcome by around 0.1 standard deviations.

A.6.2 First stage estimates

Table A10 reports the first stages estimates corresponding to panel H of table 3 in the main paper. The results demonstrate a consistently strong first stage, indicating that the reform increased the number of years of completed schooling by around 0.3 years for each of our instrumental variable specifications. Column (1) is omitted to match the column numbering in the main paper.

[10]The standardization occurred before observations outside the RD bandwidth were dropped. Hence, the outcome means are not quite zero.

Table A9: The effect of compulsory education on ordinally-coded anti-immigration attitudes

	Allow fewer immigrants of different race/ ethnicity (1)	Allow fewer immigrants of same race/ ethnicity (2)	Allow fewer immigrants from poorer countries (3)	Immigration is bad for the economy (4)	Immigration undermines local culture (5)	Immigration reduces local liveability (6)	Anti-immigration scale (7)
Panel A: Reduced form RD estimates—Denmark							
Reform	-0.066	0.027	-0.061	0.041	-0.001	0.019	-0.008
	(0.067)	(0.065)	(0.062)	(0.075)	(0.075)	(0.074)	(0.056)
Bandwidth	8	7	9	7	7	7	7
Observations	3,213	2,819	3,646	2,811	2,840	2,829	2,868
Outcome mean	-0.1	-0.35	0.02	0.11	0.17	0.39	-0.18
Panel B: Reduced form RD estimates—France							
Reform	-0.084	-0.046	-0.116**	-0.205***	-0.107*	-0.092*	-0.074
	(0.056)	(0.056)	(0.058)	(0.078)	(0.067)	(0.058)	(0.050)
Bandwidth	10	10	10	6	9	10	9
Observations	4,583	4,573	4,592	2,964	4,249	4,639	4,274
Outcome mean	0.08	0.11	0.13	-0.1	-0.26	-0.23	0.15
Panel C: Reduced form RD estimates—Great Britain (1947 reform)							
Reform	-0.105	-0.011	-0.023	-0.274	-0.089	0.002	-0.072
	(0.072)	(0.072)	(0.072)	(0.186)	(0.073)	(0.134)	(0.057)
Bandwidth	1	1	1	2	1	3	1
Observations	482	478	480	818	471	1,161	490
Outcome mean	0.41	0.41	0.47	-0.26	-0.57	-0.39	0.42
Panel D: Reduced form RD estimates—Great Britain (1972 reform)							
Reform	-0.012	-0.049	-0.041	0.024	0.054	0.244	0.075
	(0.108)	(0.065)	(0.063)	(0.071)	(0.071)	(0.166)	(0.125)
Bandwidth	3	1	1	1	1	2	2
Observations	1,619	700	703	696	702	1,166	1,177
Outcome mean	0.14	0.3	0.21	-0.25	-0.37	-0.19	0.24
Panel E: Reduced form RD estimates—Netherlands							
Reform	-0.036	-0.069	-0.122*	-0.117*	-0.107*	-0.177**	-0.109**
	(0.064)	(0.059)	(0.073)	(0.063)	(0.060)	(0.071)	(0.052)
Bandwidth	7	8	6	6	6	5	6
Observations	3,730	4,203	3,241	3,245	3,265	2,817	3,292
Outcome mean	-0.14	0.06	-0.09	0.06	0.23	0.08	-0.1
Panel F: Reduced form RD estimates—Sweden							
Reform	0.002	-0.02	0.014	0.048	0.072	0.026	0.023
	(0.050)	(0.049)	(0.052)	(0.061)	(0.057)	(0.058)	(0.043)
Bandwidth	10	10	10	9	8	10	10
Observations	4,284	4,275	4,262	3,904	3,593	4,306	4,387
Outcome mean	-0.71	-0.54	-0.72	0.25	0.54	0.58	-0.55
Panel G: Reduced form RD estimates—all reforms pooled							
Reform	-0.024	-0.043	-0.044	-0.021	-0.021	-0.051	-0.049*
	(0.030)	(0.035)	(0.030)	(0.029)	(0.029)	(0.037)	(0.029)
Bandwidth	7	5	7	7	8	5	5
Observations	19,117	13,989	19,082	19,111	21,812	14,079	14,293
Outcome mean	-0.07	-0.02	-0.02	-0.02	-0.01	0.05	-0.02
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled							
Reform	-0.094	-0.048	-0.153*	-0.112	-0.092	-0.114	-0.102
	(0.091)	(0.088)	(0.091)	(0.095)	(0.097)	(0.099)	(0.078)
Bandwidth	9	10	9	10	10	10	9
Observations	24,298	26,858	24,262	26,873	27,052	27,013	24,764
Outcome mean	-0.06	-0.01	-0.01	-0.02	-0.02	0.05	-0.02

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A10: First stage estimates corresponding to panel H of table 3 in the main paper

	Years of completed schooling					
	(2)	(3)	(4)	(5)	(6)	(7)
Reform	0.287***	0.303***	0.277***	0.283***	0.310***	0.301***
	(0.060)	(0.065)	(0.056)	(0.058)	(0.067)	(0.065)
Bandwidth	9	8	11	10	8	8
Observations	24,798	22,125	29,725	27,461	22,125	22,125
First stage F statistic	22.6	21.7	24.3	23.5	21.4	21.7
Outcome mean	11.43	11.44	11.43	11.43	11.44	11.44

Notes: All specifications are estimated using local linear regression using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth and a triangular kernel. The reported optimal bandwidth is rounded down to the nearest integer. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.6.3 Cross-sectional correlation between education and anti-immigration attitudes

Table A11 reports the cross-sectional correlation between years of completed schooling and anti-immigration attitudes, controlling for country-survey fixed effects.

Table A11: Correlation between years of completed schooling and anti-immigration attitudes

	Anti-immigration (“non” only)	Anti-immigration (“none” or “few”)	Immigration is bad for the economy	Immigration undermines local culture	Immigration reduces local livability	Anti-immigration scale
	(1)	(2)	(3)	(4)	(5)	(6)
Years of completed schooling	-0.026***	-0.040***	-0.037***	-0.033***	-0.035***	-0.034***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	54,007	54,007	54,007	54,007	54,007	54,007
Outcome mean	0.16	0.53	0.35	0.26	0.32	0.32

Notes: All specifications are estimated using OLS, and include country-survey fixed effects. Robust standard errors are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.