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 correlation: is it causal or is it an artifact of selection bias? We address this question by exploiting six major compulsory schooling reforms in five Western European countries—Denmark, France, Great Britain, the Netherlands, and Sweden—that have recently experienced politically-influential anti-immigration movements. On average, we find that compelling students to remain in secondary school for at least an additional year decreases anti-immigration attitudes later in life. Instrumental variable estimates demonstrate that, among such compliers, an additional year of secondary schooling substantially reduces the probability of opposing immigration, believing that immigration erodes a country's quality of life, and feeling close to far-right anti-immigration parties. These results suggest that rising post-war educational attainment has mitigated the rise of anti-immigration movements. We discuss the mechanisms and implications for future research examining anti-immigration sentiment.

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1 Introduction

Amidst the uncertainty ushered in by Brexit, refugee inflows in Germany and Sweden, 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). To explain this pattern, researchers and pundits have emphasized the mediating role of anti-immigration attitudes. Because less-educated voters 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" (Hainmueller and Hopkins 2014). Such assertions prompt the following question: is the relationship between education and anti-immigration attitudes causal? In other words, does education decrease an individual's likelihood of holding anti-immigration attitudes later in life?

Two non-competing sets of theories contend 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—both materially and symbolically—against those benefiting from these structural changes (e.g. Kitschelt 1997). Individuals without qualifications fall on the losing side of this cleavage. For such voters, the activation of physical and cultural boundaries—signified by economic protectionism and ethnocentric policies—may boost their disposable income (Scheve and Slaughter 2001;

Mayda 2006), in addition to their self-worth and social status (Lamont and Molnar 2002; 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, and different occupational choices—that make one less likely to hold anti-immigration attitudes.

Both theories rest on the assumption that the negative correlation between education levels and anti-immigration sentiment (e.g. Citrin et al. 1997; Hainmueller and Hiscox 2007; Mayda 2006) is, at least partly, causal. However, the validity of this underlying premise has not been definitively established (Hainmueller and Hopkins 2014). First, educated individuals may be less likely to exhibit anti-immigration attitudes due to unobserved differences in their upbringing, opportunities, or peers (Kam and Palmer 2008). Comprehensively controlling for such differences represents a major challenge, not least because political attitudes already differ across first-year high-school students that ultimately complete different levels of schooling (Jennings and Niemi 1981). Second, researchers risk introducing upward and downward biases by including variables as controls that are themselves consequences of education. For instance, extant studies often control for income, residential location, and partisanship (e.g. Citrin et al. 1997; Hainmueller and Hiscox 2007; Mayda 2006); each post-treatment variable is likely to be determined by education while also affecting immigration attitudes.

In this research letter, we leverage quasi-experimental variation to identify the effect of compulsory education reforms and an additional grade of high school on anti-immigration attitudes in five Western European countries currently facing significant anti-immigration movements. We use a regression discontinuity (RD) design to exploit six major compulsory schooling reforms that significantly impacted cohorts in Denmark, France, Great Britain, the Netherlands, and Sweden. The reforms, on average, increased a student's secondary schooling by 0.29 years, without affecting tertiary education. This set of reforms contrasts with d'Hombres and Nunziata (2016), who conduct a similar analysis including reforms that did not significantly increase schooling among the same survey respondents.[1]

Pooling across countries, we find that reforms inducing individuals to remain in secondary education significantly decrease their hostility to immigration. On average, an additional year of schooling reduces support for immigration restrictions and the belief that immigration makes the country a worse place to live by 8 and 18 percentage points respectively, and ultimately reduces closeness to far-right anti-immigration parties by more than a quarter of its mean. While lacking statistical power, our country-by-country analyses suggest that education induced greatest tolerance for immigrants in Denmark, France, and the Netherlands, and may have produced larger effects in Great Britain after the emergence of the United Kingdom Independence Party (UKIP). These findings thus indicate that the widely-cited attitudinal differences between those without high school education and the rest of the population cannot be solely attributed to selection bias. The paper concludes by considering possible mechanisms and implications for future research.

2 Research Design

After World War II, countries across Western Europe passed laws raising the legal age at which a child is permitted to leave compulsory education. Individuals belonging to the first cohorts subject to higher leaving ages are assigned a strong incentive to remain in school for at least an additional year. Compulsory schooling laws are widely used in labor economics to estimate the returns to schooling (e.g. Brunello, Fort and Weber 2009; Oreopoulos 2006). Using a RD design, we follow these approaches to identify the effects of education on anti-immigration attitudes.

[1] Appendix Table A1 demonstrates that we fail to detect a first stage for seven reforms included in their sample, which could significantly drive their estimates. Other extensions and methodological improvements include: examining secondary education's effects on closeness to far-right parties, as well as considering broader mechanisms underpinning the results; addressing the empirical concern that the effects reflect trends that differ between affected and unaffected cohorts; and reinforcing our findings with out-of-sample validation exercises.

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	1958	1958	14 to 15	7 to 8	1944
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: †The second reform in Great Britain was first passed in 1962, but not implemented until a 1972 statutory instrument. The British reforms did not affect Northern Ireland.

2.1 Data

We focus on six major compulsory schooling reforms across five countries; namely, one reform in Denmark, France, the Netherlands, and Sweden, and two reforms in Great Britain.[2] Table 1 lists these reforms, highlighting the date and nature of the reform, as well as the cohorts affected. Detailed information on each reform is provided in Appendix section A.1. Importantly for our question at hand, each country in our sample has since experienced politically-influential anti-immigration movements.

Our empirical strategy relies on 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 2014, restricting our sample to adults aged 30 and above and born after 1915.[3] A key feature of the ESS is its battery of

[2]We first identified 14 countries that passed reforms in the post-war period. Unlike Brunello, Fort and Weber (2009) and d'Hombres and Nunziata (2016), we dropped the nine countries—Austria, Belgium, Finland, Germany, Greece, Ireland, Italy, Portugal, and Spain—where we were unable to detect statistically significant increases in education attainment in our sample at the 10% level. Appendix Table A1 reports the correspondingly weak results for these countries.

[3]The ESS uses random probability methods to construct 2,000-person samples per country-round that are nationally representative of residents aged 15 and above. All surveys are conducted in-person.

Table 2: Immigration item wording

Item	Wording	Response categories
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)
7	Is there a particular political party you feel closer to than all the other parties? [If yes] Which party feel closer to? How close do you feel to this party?	Yes/No; list of political parties; Not at all close (1), Not close (2), Quite close (3), Very close (4).

immigration questions repeated in each survey wave. Since the reforms generally occurred in the 1960s and 1970s, these surveys capture downstream attitudes once anti-immigration movements developed and respondents reached middle-age.

2.2 Outcome of Interest: Anti-Immigration Attitudes

To measure anti-immigration attitudes, we derive our dependent variables from the seven survey items listed in Table 2. The first three items reflect preferences for the number of different types of immigrant allowed into the country. The second set of three items reflect beliefs about the consequences of immigration on the economy, national culture, and quality of life. The final item captures self-identified closeness to far-right anti-immigration parties; Appendix section A.2 details our coding of such parties.

Simply examining the raw responses encounters two issues. The first issue is that responses to identical questions may not be comparable because reference points and the interpretation of non-extreme responses are likely to vary by country and wave. Furthermore, ordinal scales impose a linear relationship that may not hold if the causal relationship is actually non-linear or because differences between levels are not perceived to be linear by respondents. Our solution is to re-code survey responses as binary variables anchored around reference points that are likely to be interpreted similarly across countries and time. Specifically, we transform items 1-3 into indicators for respondents that answer "none." Since social desirability bias might induce respondents to avoid offering such an uncompromising answer, we also define indicators for respondents answering either "none" or "few." Answers to items 4-6 bunch at 5, suggesting that respondents generally understood this as the point of indifference. We consequently re-code items 4-6 each as an anti-immigration indicator for respondents selecting an anti-immigration response category (i.e. 0-4). Finally, we code closeness to a far-right party as an indicator for any level of closeness among respondents that feel closest to such parties. Individuals who do not feel close to a party, or who feel close to a non far-right party, are coded as 0.

The second issue regards the likelihood that preferences for different types of immigrant (items 1-3) cannot be examined separately. Respondents that oppose immigration may express their opposition differently across items, depending on whether they demand restrictions based on income, race, or both. Appendix section A.2 explains in detail why we prefer a holistic approach distinguishing between unconditional support for any type of immigrants and wanting to restrict entry to one or more type(s) of immigrant(s). To capture anti-immigration preferences, we thus define an anti-immigration indicator for respondents who express support for limiting entry to *at least* one type of immigrant. We implement this approach using both the "none" and the "none or few" indicators mentioned above. Tables A14-A16 report broadly similar—but, as anticipated, weaker—results when using coding approaches that treat items 1-3 as if independent.

Finally, we aggregate across the resulting six binary variables to produce an additive scale of anti-immigration sentiment designed to capture an individual's latent disposition toward immigration. By averaging across these outcomes, standardized both across and also within countries, we reduce the measurement error arising from individual items. More information on these scales—which, for the across- and within-country standardizations, have high inter-item reliability coefficients of 0.78 and (a cross-country average of) 0.75, respectively—is available in Appendix section A.2.[4]

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 have been affected. We thus define our treatment—being affected by a compulsory education reform—for respondent i from cohort b in country c as:

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

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:

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

where y_{ibc} is an individual's anti-immigration attitudes, and f is a function of our running variable $x_{bc} := birth\ \ year_{bc} - birth\ \ year\ \ first\ \ affected_{bc}$. In our baseline specification, f is a local linear regression using a triangular kernel and the optimal bandwidth recommended by Calonico, Cattaneo and Titiunik (2014). Intuitively, this entails controlling for trends across (around ten) cohorts

[4]Table A13 reports similar results using factor analysis.

separately either side of the discontinuity, while assigning greater weight to cohorts immediately around each discontinuity. We emphasize our analysis of the pooled data, which maximizes the design's power. However, since the nature and beneficiaries of the reforms differ across countries, 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. We validate this assumption in Appendix section A.3. First, Figure A2 and the associated density tests indicate that individuals born around the discontinuity did not manipulate their birth year to sort into receiving the reform. Second, Table A5 shows that affected and unaffected cohorts around the reforms are similar across 13 predetermined covariates. Third, section A.1 describes the context of the reforms, highlighting that the compulsory education reforms are unlikely to be confounded by other changes differentially affecting those subject to the reform.

Many students would have stayed in school absent a reform. To identify the local average treatment effect of an additional year of secondary education 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:[5]

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

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

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

Identification of the local average treatment effect at the cutoff requires two additional assumptions: the reforms do not decrease education levels for any student (monotonicity), and only affect

[5]We use years of schooling—rather than an indicator for completing high school—because Marshall (2016*a*) demonstrates that such a coarsening can upwardly bias instrumental variables estimates.

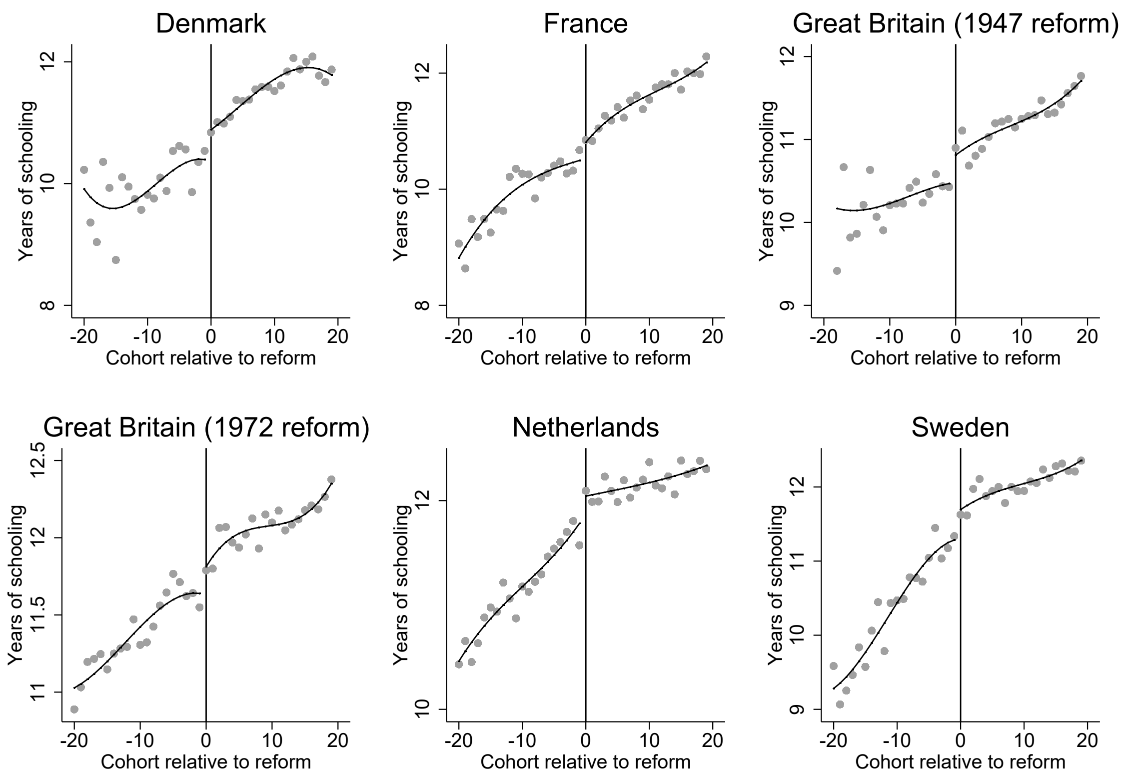

Figure 1: Years of completed schooling among cohorts around compulsory schooling reforms, by reform (third-order polynomials either side of the reform)

anti-immigration attitudes through their effect on completing additional years of schooling (exclusion restriction). These assumptions are 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 (Marshall 2016*b*). We nevertheless discuss possible exclusion restriction violations in Appendix section A.5.

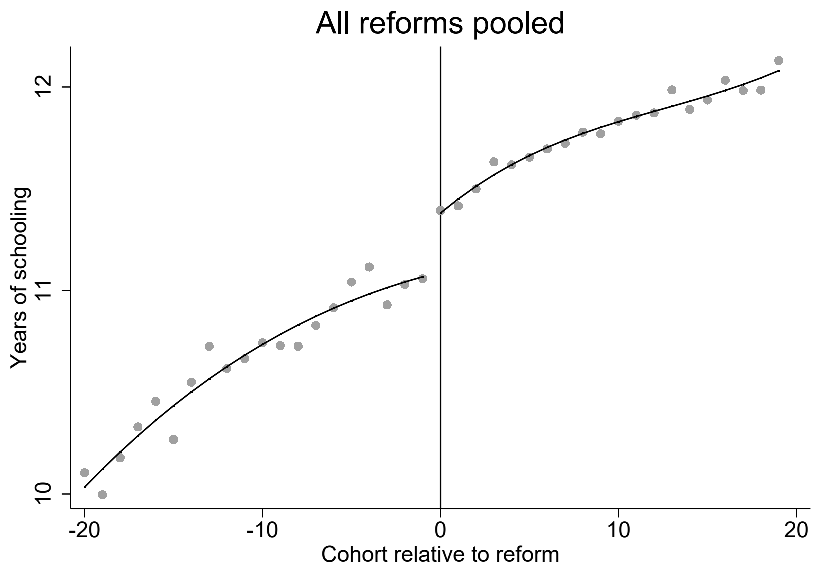

Figure 2: Years of completed schooling among cohorts around compulsory schooling reforms, pooled across reforms (third-order polynomials either side of the reform)

3 Results

3.1 Compulsory Schooling Increases Secondary Education

We 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 the kind of additional education varies across students.[6]

Figure 1 shows a notable discontinuity in the number of years of completed schooling around all six reforms. The reform-by-reform RD estimates in column (1) of panels A-F in Table 3 confirm that each reform significantly increased the average number of years of completed schooling among affected cohorts by at least 0.2 years. Figure 2 and panel G show that the pooled estimate

[6]This limit does not drive the results since no reform affected post-secondary education (see Table A6).

across countries—of 0.29 years—is also statistically significant. Appendix Table A6 shows that these increases in education are concentrated around students' 8th-13th years of schooling, but do not systematically affect tertiary education. In sum, the reforms achieved their goal of raising secondary education levels among many students that would not have otherwise remained in school.

3.2 Compulsory Schooling Decreases Anti-Immigration Attitudes

Figure 3 and columns (2)-(9) of Table 3 compare anti-immigration attitudes between pre- and post-reform cohorts. We start by examining preferences for restricting one or more type of immigrant, considering both our coding rules ("none" in column (2), and "none" or "few" in column (3)). The point estimates are generally negative: affected cohorts are less likely to oppose expanding immigration later in life. These estimates are greatest in France and the Netherlands, but predominantly negative across reforms. Pooling across countries to increase the precision of our estimates, panel G shows that the difference in attitudes is statistically significant and indicates that, on average, affected students are almost 3 percentage points—or 15%, relative to the stronger anti-immigration sample mean—less likely to oppose immigration later in life.

Columns (4)-(6) next consider the possible roots of such decreased opposition to immigration by examining its perceived socioeconomic impacts. The most consistent finding is that the reforms reduce the probability that an affected respondent later in life expresses that immigration make one's own country a worse place to live by 6.1 percentage points—an 18% decrease on the sample mean. This pooled estimate is driven primarily by Denmark, France, and the Netherlands. Although the pooled estimates in columns (4) and (5) are also negative, the point estimates for economic and cultural considerations are smaller in magnitude. These results could suggest that local factors—potentially reflecting reduced day-to-day interaction or weaker community impacts, like house price shocks—may be more important than reduced economic or cultural threat in explaining education's effects, or instead that elite messaging engenders vague anti-immigration sentiments

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)	Feel close to far-right (7)	Anti-immigration scale (across) (8)	Anti-immigration scale (within) (9)
Panel A: Reduced form RD estimates—Denmark									
Reform	0.448^{+}	0.004	-0.036	0.008	-0.032	-0.089*	-0.057*	-0.095^{+}	-0.094^{+}
	(0.260)	(0.028)	(0.032)	(0.039)	(0.035)	(0.035)	(0.022)	(0.049)	(0.051)
Bandwidth	7	10	11	8	9	9	9	10	10
Observations	2,635	3,494	3,790	2,938	3,215	3,215	3,161	3,494	3,494
Outcome mean	10.86	0.17	0.69	0.35	0.26	0.27	0.08	0.03	0.09
Panel B: Reduced form RD estimates—France									
Reform	0.285^{+}	-0.053*	-0.031	-0.052	-0.062^{+}	-0.092*	-0.011	-0.127*	-0.125*
	(0.156)	(0.025)	(0.030)	(0.036)	(0.033)	(0.036)	(0.014)	(0.053)	(0.052)
Bandwidth	11	12	11	8	9	8	9	8	8
Observations	5,078	5,465	5,078	3,861	4,278	3,861	4,219	3,861	3,861
Outcome mean	10.84	0.20	0.61	0.38	0.36	0.40	0.04	0.08	0.01
Panel C: Reduced form RD estimates—Great Britain (1947 reform)									
Reform	0.552*	-0.024	0.053	-0.060	-0.068	-0.006	0.090**	-0.008	-0.006
	(0.230)	(0.049)	(0.050)	(0.062)	(0.073)	(0.054)	(0.035)	(0.086)	(0.080)
Bandwidth	4	6	5	4	3	5	6	5	5
Observations	1,492	2,130	1,816	1,492	1,191	1,816	362	1,816	1,816
Outcome mean	10.70	0.32	0.76	0.45	0.46	0.51	0.06	0.33	0.13
Panel D: Reduced form RD estimates—Great Britain (1972 reform)									
Reform	0.274*	-0.031	-0.040	0.049	0.073*	0.021	-0.121*	0.016	0.017
	(0.127)	(0.030)	(0.033)	(0.034)	(0.035)	(0.035)	(0.058)	(0.054)	(0.051)
Bandwidth	7	8	8	8	8	8	8	8	8
Observations	3,754	4,270	4,270	4,270	4,270	4,270	618	4,270	4,270
Outcome mean	11.83	0.22	0.61	0.43	0.40	0.43	0.11	0.16	-0.03
Panel E: Reduced form RD estimates—Netherlands									
Reform	0.204^{+}	-0.018	-0.056	-0.050	-0.040	-0.111**	-0.016	-0.109*	-0.115*
	(0.107)	(0.024)	(0.034)	(0.033)	(0.026)	(0.037)	(0.013)	(0.046)	(0.048)
Bandwidth	11	8	8	8	8	6	10	7	7
Observations	5,867	4,282	4,282	4,282	4,282	3,293	5,275	3,789	3,789
Outcome mean	11.79	0.14	0.49	0.29	0.15	0.29	0.04	-0.13	-0.05
Panel F: Reduced form RD estimates—Sweden									
Reform	0.280*	-0.014	-0.010	0.002	-0.002	-0.021	0.029	-0.015	-0.022
	(0.130)	(0.013)	(0.026)	(0.029)	(0.023)	(0.023)	(0.020)	(0.033)	(0.045)
Bandwidth	11	10	11	10	7	10	9	11	10
Observations	4,759	4,394	4,759	4,394	3,188	4,394	1,561	4,759	4,759
Outcome mean	11.41	0.04	0.21	0.26	0.10	0.15	0.03	-0.40	-0.02
Panel G: Reduced form RD estimates—all reforms pooled									
Reform	0.290***	-0.026*	-0.027^{+}	-0.009	-0.007	-0.061***	-0.021*	-0.072**	-0.056**
	(0.057)	(0.011)	(0.016)	(0.013)	(0.013)	(0.017)	(0.009)	(0.025)	(0.021)
Bandwidth	12	9	7	10	9	6	9	7	8
Observations	31,549	24,278	19,281	26,789	24,278	16,740	14,940	19,281	21,777
Outcome mean	11.33	0.18	0.55	0.36	0.29	0.34	0.05	0.00	0.01
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled									
Years of completed schooling		-0.087*	-0.083^{+}	-0.031	-0.024	-0.180**	-0.065*	-0.214**	-0.183**
		(0.038)	(0.049)	(0.044)	(0.042)	(0.058)	(0.031)	(0.077)	(0.079)
Bandwidth		9	7	10	9	6	9	7	8
Observations		24,278	19,281	26,789	24,278	16,740	14,910	19,281	21,777
Outcome mean		0.18	0.55	0.36	0.29	0.34	0.05	0.00	0.01
Years of completed schooling mean		11.32	11.32	11.32	11.32	11.32	11.32	11.32	11.32
First stage F statistic		22.0	20.6	22.6	22.1	19.0	12.9	19.3	21.1

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 $p < 0.01$, *** denotes $p < 0.001$.

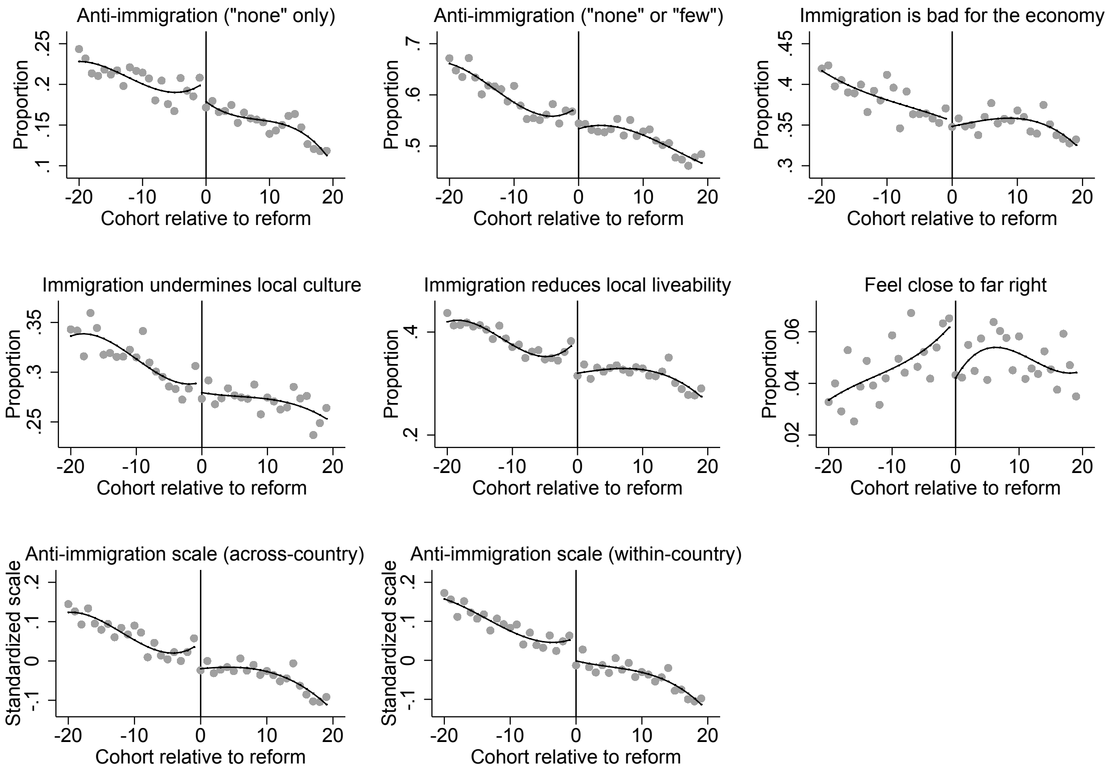

Figure 3: Anti-immigration attitudes among cohorts around compulsory schooling reforms, pooled across reforms (third-order polynomials either side of the reform)

Note: The *y*-axis differs across outcomes.

that educated respondents are more resistant to.

Pooling across countries, column (7) shows that reduced opposition to immigration translates into lower support for far-right anti-immigration parties. This primarily reflects the pronounced differences in Denmark, Great Britain (after the 1972 reform), and the Netherlands, where the reform roughly halved the probability of feeling close to far-right parties. The low frequency of such responses—5% on average in our pooled sample —reduces the precision of these estimates, but our validation of the results in a larger France-specific survey (Appendix Table A20) supports this finding.

Finally, columns (8) and (9) examine two anti-immigration attitude scales—the mean of the preceding six items, standardized first across and then within countries. Although we observe similar heterogeneity across countries, the pooled results ultimately demonstrate that anti-immigration sentiments significantly declined among affected cohorts by around 0.06 standard deviations on average.

3.3 Secondary Education Decreases Anti-Immigration Attitudes

Since the educational attainment of many students in affected cohorts was unaltered by the reforms, we now estimate the effects of an additional year of schooling itself using our instrumental variables (IV) strategy. The results in panel H of Table 3 show that secondary education substantially decreases anti-immigration attitudes among students that complied with the reform.[7] For such students, an additional year of schooling reduces the probability of expressing anti-immigration attitudes by 8 percentage points, the probability of stating that immigration negatively affects quality of life in the country declines by 18 percentage points, the probability of feeling close to the far right by 6 percentage points, and the overall anti-immigration scales by 0.2 standard deviations.

These large average effects indicate that secondary education has played an important role in inducing tolerant attitudes towards immigration later in life among students that would otherwise have left secondary education. Our estimates—which are around three times larger than the cross-sectional correlations reported in Appendix Table A18—also suggest that relatively uneducated reform-compliers could be particularly susceptible to education's tolerance-inducing effects.

3.4 Robustness Checks

The pooled reduced form estimates are robust to various potential concerns, which Appendix sections A.5, A.6 and A.8 address in detail. First, specification tests show that all findings are robust across bandwidths ranging from 2 to 15, the choice of kernel weighting, the inclusion of local

[7] Table A17 reports the first stage for each regression.

quadratic and cubic cohort trends, and placebo reforms occurring five and ten years earlier. Second, the results are robust to "leave one out" checks removing any particular reform or any item from the anti-immigration scales. Third, we find similar results for several alternative operationalizations of our outcomes. Fourth, we validate our findings by replicating our analysis using larger country-specific surveys in France and Great Britain. Finally, our examination of alternative potential channels suggests that plausible exclusion restrictions are not driving our IV estimates.

4 Discussion

The transition out of high school has attracted significant attention from researchers and journalists highlighting the substantial gap in immigration attitudes between those that did not complete secondary education and the broader population. Leveraging exogenous variation in exposure to compulsory schooling reforms in five Western European countries that subsequently experienced significant anti-immigration movements, we demonstrate that an additional year of secondary education substantially decreases anti-immigration attitudes.

By establishing that this relationship is indeed causal, this research letter lays the groundwork for future studies trying to disentangle the mechanisms driving secondary education's potential role in explaining and addressing the rise of far-right anti-immigration parties.[8] To help frame this agenda, we conclude by discussing our preliminary exploration of these mechanisms using this paper's identification strategy; Appendix section A.9 comprehensively details the analysis we now summarize.

According to the "education-as-character-shaping" literature, secondary education directly triggers attitudinal change. This could occur via the promotion of motivational values in teenage-hood that translate into lasting lower levels of prejudice. However, our examination of a battery of

[8]Since 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 tolerance for immigration.

ESS items capturing basic human values fails to detect any evidence that affected cohorts embrace values associated with lower levels of prejudice more generally.

Similarly, based on the widely-cited economic returns to education (e.g. Brunello, Fort and Weber 2009; Oreopoulos 2006), the "education-as-cleavage" literature might predict that reduced anti-immigration sentiment reflects lower individual exposure to competition from immigrant labor. Yet, research shows that anti-immigration preferences are poorly predicted by objective and subjective measures of immigration's consequences on individuals' pocketbooks (Hainmueller and Hopkins 2014), which is reinforced by our finding that education does not significantly alter perceptions of immigration's economic effects. Rather, what appears to matter is whether individuals think immigration policy serves immigrants' interests at the expense of their *country's* interests. Over their lifetime, educated voters may become more exposed to worldviews discouraging of such sociotropic and ethnocentric zero-sum reasoning (Hainmueller and Hopkins 2014). However, we also find little evidence to suggest that the relatively pro-immigration attitudes of affected cohorts extend to broader liberal-cosmopolitan positions (Kitschelt 1997) on women and gay rights or support for EU integration.

The cross-country heterogeneity in the effects of compulsory schooling reforms suggests that differences in receptivity to elite-messaging across education levels could also be a key mechanism. The starkest contrast in our findings is between Great Britain's negligible effects and systematic changes in Denmark, France, the Netherlands, and—to a lesser extent—Sweden. This difference mirrors the comparatively weaker institutionalization of far-right parties in Britain, where its majoritarian system has (until recently) enabled the major parties to depoliticize immigration debate. Consistent with this potential mechanism, Appendix Table A19 tentatively suggests that cohorts affected by Britain's 1947 reform developed less anti-immigration attitudes after UKIP emerged. This messaging explanation might also explain why the relatively non-specific sentiment that immigration has reduced local livability is most affected by secondary education. There are, of course, alternative explanations for cross-country heterogeneity based on the reforms themselves and other contextual features. Ultimately, these sources of spatial and temporal heterogeneity merit further research.

This analysis of causal mechanisms is only tentative. Future studies might benefit from recovering survey data collected closer to the reforms themselves to separate whether tolerance towards immigration emerges quickly after leaving school, as "education-as-character-shaping" suggests, or only later in life, as "education-as-cleavage" and elite messaging suggest. Future research might also exploit survey-specific contextual variation to examine how education's effects may be amplified by changes in the return to education or anti-immigrant elite messaging. Regardless, we hope that this letter inspires rigorous analysis seeking to understand a defining issue of contemporary Western politics.

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