Online Appendix: Education and Anti-Immigration Attitudes: Evidence from Compulsory Schooling Reforms across Western Europe

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A Online appendix

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A.1 Compulsory schooling reforms selected for this study

A.1.1 Selection of country-reforms

Compulsory schooling is the number of years, set by the law, during which every normal child must be receiving formal education. The six reforms presented in the main paper were selected following a two-step procedure.

First, of the 17 countries frequently covered by the ESS, we single out 14 Western democracies that have passed one or more major compulsory schooling reforms since WWII (Brunello, Fort and Weber 2009; Fort 2006; Gathmann, Jürges and Reinhold 2015; Murtin and Viarengo 2011). Due to their radically different political histories, we exclude Eastern European countries such as the Czech Republic, Hungary, and Poland. This exclusion criterion might also apply to Portugal and Spain, two countries where the major reform took place when each country was still a dictatorship (1964 for Portugal and 1970 for Spain). However, studies previously examining compulsory schooling laws in a multi-country framework often include the latter two countries among the set of Western European nations (e.g. Brunello, Fort and Weber 2009; d'Hombres and Nunziata 2016). In line with this previous work, we opted to include Portugal and Spain in our potential sample frame. This led us to consider post-World War II reforms in the following countries: Austria, Belgium, Denmark, Finland, France, Germany, Great Britain, Greece, Ireland, Italy, the Netherlands, Portugal, Spain, and Sweden.

Second, for major reforms in each of these countries, we then 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. Based on the RD specification detailed in the main paper, we drop the nine countries where we are unable to detect a statistically significant (at least at the 10% level) increase in the number of years of completed schooling associated with the its reform. The reforms dropped by this criterion are listed in Table A2, while Figure A1 shows graphically that the average years of completed schooling did not substantially change—at least in the ESS sample—in any of the countries that we excluded from our sample.[1] Table A2 briefly reviews the literature for factors that could account for the failure to reject the null hypothesis of no effect of the reform on years of completed schooling in each case. It should, of course, be cautioned that the RD design may be under-powered to detect effects in some cases, like Portugal (where $p = 0.11$).

In contrast, Figure 1 in the main paper shows that, in each of the countries that we include, the reforms succeeded in increasing the average number of years of completed schooling by compelling a significant fraction of students to meet the new legal requirement and consequently delay their exit from the secondary education system. The regression estimates in Table A6 provide information on exactly which levels of schooling were affected by each reform. Although there are some "spillovers" to years of school beyond those mandated by the reform, the effects are generally largest for the level of schooling required by each reform.

We limit our analysis to countries with a strong first stage in order to minimize the concern that results are driven by countries where the reform was relatively weak, and thus that the results may be spurious. As a result, our dataset differs significantly from the one used by d'Hombres and Nunziata (2016). Their study similarly pools ESS respondents across countries and the early survey waves in our sample to examine the effect of a change in the length of compulsory schooling on anti-immigrant preferences. Their findings align with some of those presented in Table 3 of the main paper, although we code outcomes in ways that we believe are more conceptually appealing and also examine closeness to far-right parties. However, as shown in Figure A1 (especially when compared to Figure 1 in the main paper), there is little evidence that ESS respondents in 7 of the countries additionally considered by d'Hombres and Nunziata (2016) were affected by compulsory schooling reforms. This lack of a first stage for these countries is demonstrated more formally in column (1) of Table A1, using the same optimal bandwidth RD specifications used in the main paper.[2] The estimates for the main outcomes also cast doubt on the findings of d'Hombres

[1] In the cases of Finland and Germany, the cohort relative to the reform is defined separately for each region.

[2] We do not consider Portugal's 1946 reform raising the number of years of compulsory schooling from

and Nunziata (2016), where each of these reforms is included in their pooled analysis (and represent around half their sample): columns (2)-(9) show that while attitudes became more favorable towards immigration in some countries, this generally occurred in the countries—like Italy and Spain—where we register the *weakest* effects of a reform on years of education obtained—for Italy and Spain the estimated first stage is in fact negative. This suggests that these reforms did not systematically affect anti-immigration attitudes in ways consistent with the claimed effect of education, likely due to the weak effects of the reforms themselves on completed years of schooling.

Beyond our substantive focus on far-right parties and our examination of other mechanisms, another key source of differentiation from d'Hombres and Nunziata (2016) is the use of what we regard as a more robust empirical design. In particular, we implement a regression discontinuity design that controls for cohort trends *either* side of the reform and upweights the observations closest to each discontinuity, as recommended by Calonico, Cattaneo and Titiunik (2014). In contrast, d'Hombres and Nunziata (2016) include a quadratic cohort trend specific to each reform, but which is not allowed to vary either side of the discontinuity. Various concerns have been raised about the use of such "global" trends, even when restricting the sample to within a narrower bandwidth of the discontinuity (Gelman and Imbens forthcoming). Moreover, in Appendix section A.8, we examine whether key aspects of our findings are reproducible using other datasets beyond the ESS. Ultimately, our two-step reform selection procedure and robust estimation strategy, combined with the successful reproduction of our findings using alternative data, enables us to confidently identify a causal relationship between education and anti-immigration attitudes, while also increasing our confidence that the cross-country heterogeneity (e.g. France vs. Great Britain) is not a fluke of the ESS data.

In the remainder of this section, we provide a brief description of each reform selected for

3 to 4 on account of its dissimilarity with the other reforms. We also exclude the Northern Ireland and Portugal 1986 reforms, respectively due to the small sample size (221 observations in total) and the small number of treated cohorts. The coding of reforms dates is based on that described in detail in this Appendix, and thus differs somewhat from d'Hombres and Nunziata (2016).

Table A1: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, in countries included by d'Hombres and Nunziata (2016) but removed from our sample due to their weak first stage

	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—Belgium									
Reform	0.084	-0.007	0.008	0.003	-0.038	0.010	-0.012	-0.028	-0.026
	(0.216)	(0.040)	(0.046)	(0.050)	(0.038)	(0.052)	(0.018)	(0.072)	(0.069)
Bandwidth	4	4	5	4	5	4	6	4	4
Observations	1,921	1,921	2,328	1,921	2,328	1,921	2,662	1,921	1,921
Outcome mean	12.06	0.17	0.50	0.40	0.21	0.35	0.03	-0.08	-0.07
Panel B: Reduced form RD estimates—Finland									
Reform	-0.442	0.076	0.067	0.132	0.036	0.037		0.114	0.132
	(0.462)	(0.078)	(0.083)	(0.095)	(0.042)	(0.106)		(0.107)	(0.121)
Bandwidth	6	6	8	6	7	5		6	6
Observations	463	463	630	463	559	391		463	463
Outcome mean	11.34	0.12	0.70	0.26	0.04	0.23		-0.22	-0.11
Panel C: Reduced form RD estimates—Germany									
Reform	-0.054	-0.010	-0.026	-0.024	-0.046	0.001	-0.000	-0.025	-0.024
	(0.155)	(0.031)	(0.044)	(0.036)	(0.040)	(0.040)	(0.000)	(0.052)	(0.054)
Bandwidth	9	9	11	13	8	11	5	10	10
Observations	2,303	2,303	2,839	3,374	2,040	2,839	952	2,547	2,547
Outcome mean	11.91	0.13	0.54	0.32	0.20	0.33	0.00	-0.16	-0.03
Panel D: Reduced form RD estimates—Greece									
Reform	0.259	0.030	0.027	-0.013	-0.056	0.027	-0.007	-0.011	-0.008
	(0.265)	(0.052)	(0.038)	(0.050)	(0.043)	(0.060)	(0.015)	(0.066)	(0.071)
Bandwidth	7	6	7	6	8	5	6	6	6
Observations	2,589	2,212	2,589	2,212	2,937	1,864	2,178	2,212	2,212
Outcome mean	10.94	0.35	0.85	0.60	0.59	0.64	0.01	0.40	-0.06
Panel E: Reduced form RD estimates—Italy									
Reform	-0.279	-0.108*	-0.091	-0.071	-0.062	-0.097	-0.002	-0.164^{+}	-0.169^{+}
	(0.402)	(0.050)	(0.059)	(0.057)	(0.066)	(0.063)	(0.013)	(0.090)	(0.092)
Bandwidth	11	9	11	10	8	10	12	8	9
Observations	1,427	1,179	1,427	1,305	1,059	1,305	868	1,059	1,179
Outcome mean	9.99	0.15	0.47	0.31	0.32	0.44	0.00	-0.08	-0.03
Panel F: Reduced form RD estimates—Portugal									
Reform	0.401	0.013	-0.012	-0.008	-0.015	0.028		0.002	0.001
	(0.251)	(0.031)	(0.029)	(0.031)	(0.031)	(0.033)		(0.047)	(0.046)
Bandwidth	10	10	11	11	10	11		11	11
Observations	4,443	4,443	4,896	4,896	4,443	4,896		4,896	4,896
Outcome mean	7.11	0.31	0.70	0.41	0.29	0.49		0.13	0.01
Panel G: Reduced form RD estimates—Spain									
Reform	-0.047	-0.019	-0.078^{+}	-0.038	-0.031	-0.062*		-0.091*	-0.092*
	(0.215)	(0.022)	(0.045)	(0.030)	(0.030)	(0.029)		(0.045)	(0.047)
Bandwidth	10	12	6	11	9	12		10	11
Observations	4,500	5,338	2,730	4,945	4,054	5,338		4,500	4,945
Outcome mean	10.31	0.15	0.57	0.30	0.22	0.33		-0.14	-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 $p < 0.01$, *** denotes $p < 0.001$.

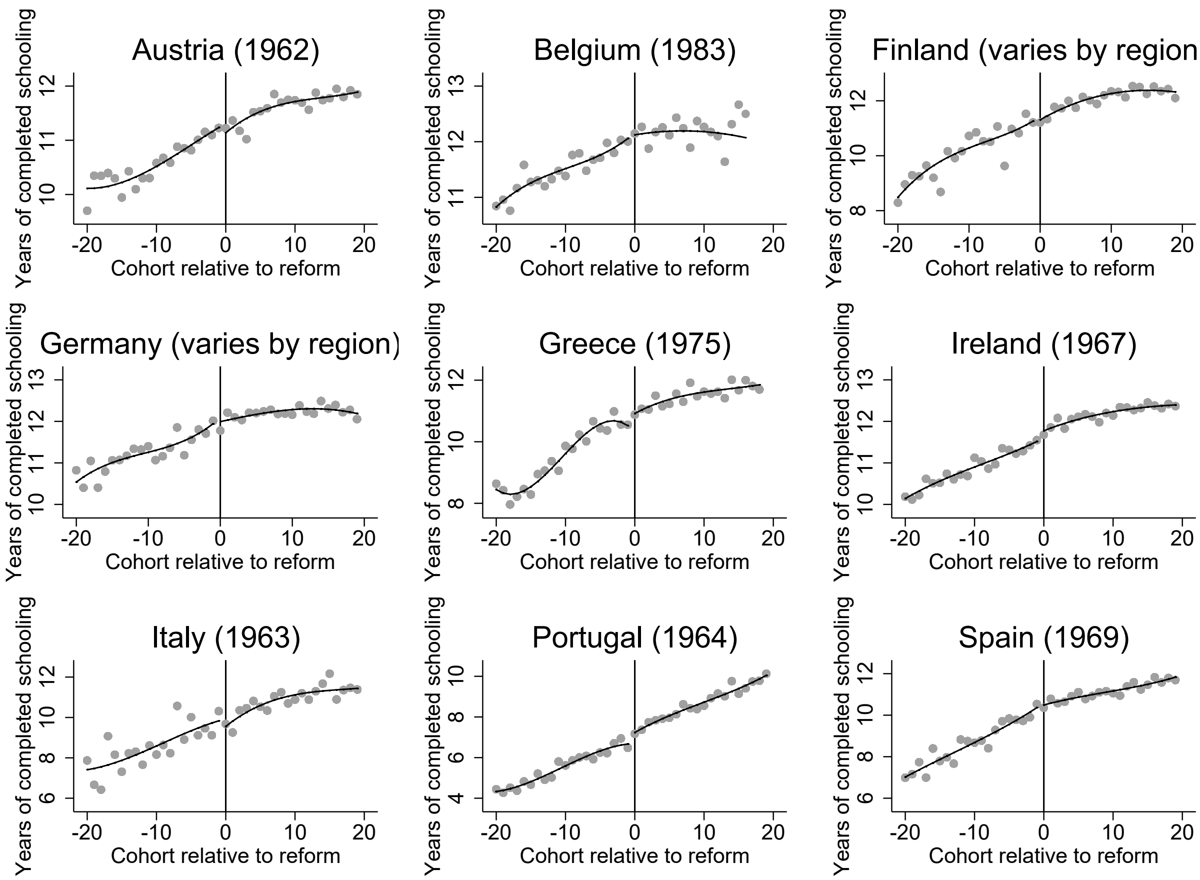

Figure A1: Years of completed schooling among students who completed at least the minimum years of schooling required by the reform in cases excluded from our analysis (third-order polynomials either side of the reform)

Table A2: Compulsory education reforms that were not included in the final analysis

<table>
<tr><th>Country</th><th>Date of reform (being implemented)</th><th>Change in minimum school leaving age</th><th>Change in years of compulsory education</th><th>Year of birth of passing first affected cohort</th></tr>
<tr><td>Austria</td><td>1962 (1966)</td><td>14 to 15</td><td>8 to 9</td><td>1947 (1951)</td></tr>
<tr><td colspan="5">References and comments:
To code affected cohorts, we follow Fort (2006) and Moravec (1996). According to Brunello et al. (2016), implementation took place between 1962 and 1966: in their own analysis, they consequently code the 1951 cohort as the pivotal cohort. Arguing that most individuals in the 1951 cohort were not affected by the reform, Gathmann, Jürges and Reinhold (2015) code the first potentially affected cohort as those born in 1952. Whatever cohort we focus on, we find no evidence of a discontinuity in the ESS data in terms of completed years of schooling.</td></tr>
<tr><td>Belgium</td><td>1983</td><td>14 to 18</td><td>8 to 12</td><td>1969</td></tr>
<tr><td colspan="5">References and comments:
To code affected cohorts, we follow Brunello, Fort and Weber (2009) and Fort (2006). Despite the large increase in the required years of schooling, we find no evidence suggesting that the reform discontinuously increased educational attainment among those around the cutoff. One reason might be that affected individuals could complete their final years part-time, affecting how they might report the number of years spent in school. In addition, structural reforms implemented in 1971 had already provided strong incentives for schools to keep students in school longer (Fort 2006).</td></tr>
<tr><td>Finland</td><td>Varies by region (1972-77)</td><td>13 to 16</td><td>6 to 9</td><td>Varies by region (1961-66)</td></tr>
<tr><td colspan="5">References and comments:
To code affected cohorts, we follow Brunello, Fort and Weber (2009). One explanation for finding no effect of this compulsory schooling reform on education is provided by Pekkala Kerr, Pekkarinen and Uusitalo (2013) and Pekkarinen, Uusitalo and Pekkala Kerr (2009): based on Census data, they argue that the minimum age to leave school has de facto been 16 ever since 1957, more than a decade before the official legal change. Another possible explanation is that our analysis assumes that respondents living in a given region were also born there. While unlikely, cross-region mobility could could be disruptive enough to invalidate this assumption.</td></tr>
<tr><td>Germany</td><td>Varies by region (1949-69)</td><td>14 to 15</td><td>8 to 9</td><td>Varies by region (1934-55)</td></tr>
<tr><td colspan="5">References and comments:
To code affected cohorts, we follow Pischke and Von Wachter (2008) and Brunello, Fort and Weber (2009). Our analysis assumes that respondents living in a given region were also born there. In line with Mocan and Pogorelova (2014), but unlike Pischke and Von Wachter's (2008) difference-in-differences analysis, we find no evidence of a discontinuity in years of schooling completed. This may reflect a combination of the relatively small effects observed in Pischke and Von Wachter (2008) and the relatively small sample sizes available in the ESS.</td></tr>
</table>

Table A2: Continued (1)

Country	Date of reform (being implemented)	Change in minimum school leaving age	Change in years of compulsory education	Year of birth of passing first affected cohort
Greece	1975	12 to 15	6 to 9	1963

References and comments:
To code affected cohorts, we follow Brunello, Fort and Weber (2009) and Fort (2006). In contrast, Mocan and Pogorelova (2014) code 1965 as the first affected cohort. Whatever cohort we assume is the first potentially affected, we find no evidence of a discontinuity in years of schooling completed. This finding is in line with results in Mocan and Pogorelova (2014). The re-introduction of democracy in 1974 triggered a succession of reforms, including in public education. The increase in compulsory education thus happened alongside many other changes that could have also affected individuals decisions to drop out or stay on.

Country	Date of reform (being implemented)	Change in minimum school leaving age	Change in years of compulsory education	Year of birth of passing first affected cohort
Ireland	1972	14 to 15	8 to 9	1958

References and comments:
To code affected cohorts, we follow Brunello, Fort and Weber (2009) and Fort (2006) and find no evidence of a discontinuity around the 1959 cohort. Denny and Harmon (2000) provide one potential explanation for the absence of effect on completed years of schooling. Five years before the 1972 reform, a major education reformed made access to secondary schooling free. Until then, low income families had to pay a fee, which could represent a substantial amount of money if more than one child was over the age of 12 (age at which free primary education then ended). The reform also abolished entrance exams that compelled low-income students to follow the vocational track. According to evidence in Denny and Harmon (2000), enrollment in secondary education did increase considerably. By 1972, most cohorts were completing at least 9 years of schooling (95% for the pre-reform cohort born in 1957), and thus there was limited scope for the 1972 compulsory schooling reforms to further increase educational attainment. The multi-country framework we rely on requires that we pool across comparable reforms. Although the 1967 fees reform extend the length of education, it did so without altering compulsory school leaving ages. Furthermore, it profoundly changed low-income students' access to higher education: it is likely that the students most affected by this reform differ substantively from the marginal students affected by an increase in compulsory schooling in the other countries in our sample. Another reason to drop Ireland is the uncertainty around the first affected cohort. According to Denny and Harmon (2000) the analysis should focus on the cohort reaching the age of 12 in 1967. Instead, we find evidence that it is the cohort reaching the age of 12 in 1966 —the year before the reform went into effect— that is the first affected: this cohort stayed in school an additional 0.20 years, in comparison to older cohorts. We find some evidence, in line with our main results, that immigration attitudes for this cohort differ significantly from that of previous cohorts. Yet, given the nature of the reform and given the uncertainty around the reform date, we ultimately decided to drop Ireland from our main analysis.

Table A2: Continued (2)

<table>
<tr><th>Country</th><th>Date of reform (being implemented)</th><th>Change in minimum school leaving age</th><th>Change in years of compulsory education</th><th>Year of birth of passing first affected cohort</th></tr>
<tr><td>Italy</td><td>1963</td><td>11 to 13</td><td>6 to 9</td><td>1952</td></tr>
<tr><td colspan="5">References and comments:
According to Fort (2006), “compliance with the 1963 reform was not instantaneous: only in 1976, the proportion of children attending junior high school approached 100%.” The effect of the reform also varied by region: according to Brandolini and Cipollone (2002), effects of the reform were concentrated on the 1952 cohort. The reform mostly affected female students living in the central and southern regions. The sample size in the ESS is not large enough to subset our analysis by gender and region, potentially explaining the absence of a discontinuity around 1963.</td></tr>
<tr><td>Portugal</td><td>1964</td><td>12 to 14</td><td>4 to 6</td><td>1956</td></tr>
<tr><td colspan="5">References and comments:
To identify affected cohorts, we follow Brunello et al. (2016) and Fort (2006). They both build on the account provided by Vieira (1999), who identifies the first cohort affected by the reform as those students entering the school system in 1964. The age at school entry in Portugal was then 8 according to Vieira (1999). Using this coding we find some evidence that individuals entering the school system in 1964 are more likely to receive at least 6 years of education than those entering the school system in 1963 (45% vs. 59%). Yet, most of the pre/post-reform difference is driven by a drop in years of education for the cohort entering the school system in 1963, before the reform was implemented. Moreover, this reform affected a much lower level of schooling than in other countries, and the overall change in years of education is not statistically significant at the 10% level. As a result, we drop Portugal.</td></tr>
<tr><td>Spain</td><td>1970</td><td>12 to 14</td><td>6 to 8</td><td>1957</td></tr>
<tr><td colspan="5">References and comments:
Brunello, Fort and Weber (2009) follows Pons and Gonzalo (2002) and codes the first affected cohort as the one reaching the age of 12 the year before the reform is implemented. Fort (2006) instead codes the cohort reaching the age of 12 in 1970 as the first affected cohort. Irrespective of the way we code the different cohorts, we find no evidence of a discontinuity around the year 1970.</td></tr>
</table>

our final sample. Our main goal is to identify the nature of the "treatment"—namely a change in the *length* of compulsory education, versus a change in both the length and the *nature* of compulsory education. In other words we examine if the "treated" cohort got more of "something new" or "more of the same." Based on previous work (Brunello, Fort and Weber 2009; Fort 2006; Gathmann, Jürges and Reinhold 2015; Grenet 2013; Marshall 2016), we conclude that—with the potential exception of Sweden (see below)—the "treated" and "control" cohorts did not seem to experience significant differences other than the length of compulsory education that was required when they were students. We also examine whether changes in the legal voting age might have differentially affected the political socialization (and consequently political preferences) of pre- and post-treatment cohorts.

A.1.2 Denmark

Of the six reforms used in our final analysis, Denmark in particular requires careful discussion. According to Brunello, Fort and Weber (2009), compulsory schooling was increased by two years in 1972. Given that education started at seven in Denmark, one can assume that students reaching the age of 14 and 15 in 1972 were the first affected by the legal change. In contrast, Arendt (2005) argues that the reform was only implemented in 1975. Using the ESS data, we could not find any evidence of a discontinuity around 1975. The 1972 implementation date finds more support in the data. Yet, it is unclear why individuals reaching 14 in 1972 do not differ significantly from the cohort reaching 14 in 1971. Ultimately, we are unable to confidently tie the 1972 discontinuity in years of schooling in the ESS data to a change in the length of compulsory education.

However, a school reform in 1958 required that all municipalities provide an 8th year of schooling (Bingley and Martinello 2017). This disproportionately affected rural areas. By 1972, most students were staying on until 16, meaning that the 1970s reforms were effectively catching up to established practices. The Danish case is similar to the Irish case: an increase in compulsory schooling was preceded by an expansion in the "supply" of secondary education. We ultimately decided to keep the 1958 Danish reform in our sample on the basis of its large and statstically significant first stage. Table A11 below demonstrates that our overall finding that educational reforms reduce anti-immigration attitudes, on average across countries, is robust to removing the Danish reform from our analysis.

When the Danish Constitution was adopted in 1953, the voting age was 23 years. It was changed in 1961 to 21 years, in 1971 to 20 years, and in 1978 to eighteen years. The first cohorts of students affected by the 1961 change were born in 1939 and 1940, 4-5 years before the first cohorts of students affected by the change in schooling length.

A.1.3 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 other words, we can assume that the type of education received by the treated cohorts right after the increase in compulsory education did not change significantly from the education received by previous cohorts.

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 cohort affected by the reform. One other event is worth mentioning as a potential cause for the comparatively larger effect in France. The year following the reform, the Mai 68 events 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 the *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 unclear why Mai 68 would have *discontinuously* influenced those aged 14 as opposed to those aged 15.

A.1.4 Great Britain

In 1944, legislation was enacted under Prime Minister Winston Churchill's 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 requirement for intensive preparation, and thus affected children aged 14 (or younger) in 1947 (see Marshall 2016). Moreover, Marshall (2016) notes that "Given that the most significant post-war changes in the education system had already been implemented by 1947, the large rise in enrolment 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 system—which formally established three types of secondary school emphasizing academic, scientific, and practical skills—came into force in 1945." The results in Figure 1 in the main paper, as well as Marshall (2016) and Oreopoulos (2006), indicate that these earlier structural reforms did not affect enrolment rates. Furthermore, Marshall (2016) 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

From 1945 to the late 1970s, Great Britain had a mainly dual schooling system where tests assigned some students to a selective track ("the Grammar school"). Given the prestige of testing into the elite track, it is sensible to assume that students in these school were planning on graduating from high school with or without changes in compulsory schooling laws. It is most likely that the reforms affected students in non-selective schools.

In 1969, the voting age was lowered from 21 to 18, starting in 1970. Cohorts who reached 18, 19, or 20 in 1970 were the first affected, i.e. cohorts born between 6 and 8 years before the cohorts affected by the compulsory schooling reform of 1972.

A.1.5 The Netherlands

In the early 1970s, the Dutch system was characterized by early tracking. At the age of 12, students either took the general track or the vocational track. The vocational track only offered a maximum of four additional years of schooling, with most programs only offering three additional years. In the early to mid-1970s, the Netherlands reformed its educational system so that students in the vocational track would all receive four years of schooling. These reforms had differential effects across cohorts: according to Oosterbeek and Webbink (2007), "students who started a 3-years program of basic vocational education on August 1, 1971 could still graduate in 1974 after a 3-year program. All the following cohorts had to do a 4-years program. Hence students who started on August 1, 1972 could not obtain their diploma before 1976." The cohort born in 1960 and reaching the age of 12 (15) in 1972 (1975) should thus constitute the cut-off cohort. However, earlier cohorts were also affected. Indeed, by 1973 all basic vocational programs had been extended to four years, meaning that students born in 1958 and 1959 would face strong incentives to stay in school until 16. We consequently follow Brunello, Fort and Weber (2009) and focus on the cohort born in 1959 (and reaching 15 in 1974) as the main cut-off point.

By design, the students affected by these reforms were students in the vocational track. While the mix of skills taught in the final fourth year was more heavily weighted in favor of general skills, this does not appear to represent a dramatic change in the type of education received for those staying on an additional year. Indeed, these general skills were already present in the training received earlier in the program.

An additional concern might be the 1971 change in voting age from 21 to 18. However, the first cohort affected by this change was born in 1953, six years before the 1959 cohort affected by changes in compulsory schooling laws.

A.1.6 Sweden

Building on Meghir and Palme (2005), we focus on the education reform covering the years from 1949 to 1962. Before the reform, pupils received basic common compulsory education (*folkskolan*) until 6th grade (11-12 years old). In 7th grade, those with better marks would move on to junior secondary school (*realscolan*), followed by upper secondary and university education. Others received vocational schooling instead. Students would be distributed into different schooling depending on the track chosen. Compulsory schooling lasted between seven and eight years depending on the municipality (students usually started around 5-6 years old).

From 1949 to 1962, Sweden experimented with a more comprehensive educational system characterized by nine years of schooling (from age 6 until age 15). In this new system, grades were no longer the key factor determining the track students would take at the end of sixth grade. The extension of this new educational system was at first incremental: in 1961, only 25% of municipalities were among those "testing" the new system. Country-wide coverage was achieved in 1962. We consequently use 1962 as the 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 Brunello, Fort and Weber 2009 and define individuals reaching 14 in 1965 (i.e. born in 1951) as the first cohort affected by the reform. Figure 1 provides empirical support for choosing the 1951 cohort as the first to be "treated."

The experience of "treated individuals" was different from that of the control group in that all pupils were now under the same roof (and not in separate schools), and grades no longer affected which track one ended up in. Social interactions were thus most likely different, although the specific curriculum content was not. We consider Sweden to be a borderline case, but our overall findings do not change when Sweden is excluded (see section A.5).

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.1.7 Summary

Ultimately, of the 15 reforms from 14 countries that we considered, only six satisfy our requirements for observing a sufficient first stage. Among these reforms, two required an expansion of educational capabilities that started a few years before the reforms' official implementation dates (e.g. the Netherlands and Sweden). For these two cases, we draw on previous work by labor economists to identify the relevant cut-off cohort (Brunello, Fort and Weber 2009; Kootstra 2016; Meghir and Palme 2005). In line with this existing literature, we find significant cross-cohort differences in years of schooling before and after the selected cut-off cohort (see Figure 1), as well as in the ultimate regression estimates in column (1) of Table 3 that are used to define the inclusion of a reform. This careful selection of country cases and cut-off cohorts increases our confidence that our results capture a causal relationship between an increase in education and a decrease in anti-immigration attitudes.

A.2 Operationalization of outcome variables

A.2.1 Preferences over types of immigrant

The three items examining preferences over types of immigrant (items 1-3 in Table 2, main paper) can be tackled in several ways. One simple strategy is to examine the effect of a reform and an additional year of secondary schooling on each item taken individually. Yet, we find this analysis unsatisfying for two key reasons. First, as we explain in detail below, in the particular context of our empirical application, support for restricting access to immigrants can come in different flavors depending on what the respondent as in mind: it can mean support for restricting access to all migrants, or restricting access to migrants conditional on income, on race, or on both. Second, social desirability bias may discourage respondents from expressing the full extent of their sentiment: having opposed the entry of one type of migrant, they might offer more support to other types of immigrants to compensate. The main implication of such concept heterogeneity and social desirability is that the variance in responses to one item is only meaningful when interpreted alongside answers to the other two items.

A second—and our preferred—strategy is to examine the combination of answers respondents jointly provide to all three items as reported in Table A3. One striking feature is the virtual absence of certain response patterns: answers type (e) through (h) are each registered by less than 2% of respondents. The overwhelming majority of respondents (95%) offer one of the following four response patterns: (a) supportive of all three types of immigrants, (b) opposed to all three types of immigrants, (c) in favor of same-race migration but opposed to the other two types of immigrants, and (d) opposed to poor immigrants only. Individuals who offer response patterns (b), (c), and (d) differ in their definition of the "problematic" immigrant (any immigrant, any non-European immigrant, any poor immigrant, respectively). Yet, all express opposition to immigration. This suggests that these items should be considered jointly as types, rather than independently.

How much substantive meaning should we attach to each specific type of response patterns? Individuals who offer response pattern (b) can be assumed to be truly accepting of immigration. There is more ambiguity with regards to the other response patterns: do they capture different types of anti-immigrant sentiment? For instance, it is unclear whether types (c) and (d) are substantively different. Indeed, the stereotypical immigrant in Europe is both poor *and* an ethnic outsider: respondents with the same level of opposition to current waves of immigration into Europe might express this opposition differently, depending on whether they perceive immigrants through an economic lens (type (c)), an ethnic lens, or both (type (d)). Similarly, the difference between type (a) (limit all immigrants) and (d) (limit all except same race) is ambiguous. Type (a) might appear more anti-immigrant than type (d). However, type (d) respondents offer a response pattern that can be interpreted as more ethnocentric than type (a).

Ultimately, we decided to lump together all patterns of answers that express some form of opposition to immigration and consequently generate an indicator equal to 1 if respondents express support for limiting entry to *at least* one type of immigrant. Given that type (a) respondents, i.e. those who support all three types of immigrants, are the reference category coded 0, our estimates can also be understood as the likelihood that individuals who received more education express support for all three types of immigrants later in adulthood. We believe that this comparison is the most conceptually useful distinction to draw from this set of questions.

Table A3: Response patterns to items 1-3, where "no" denotes "none" or "few"

	Allow immigrants of ... same race	different race	poor	%
Type (a)	No	No	No	26
Type (b)	Yes	Yes	Yes	48
Type (c)	Yes	Yes	No	9
Type (d)	Yes	No	No	11
Type (e)	Yes	No	Yes	2
Type (f)	No	No	Yes	2
Type (g)	No	Yes	Yes	< 1
Type (h)	No	Yes	No	< 1

Notes: When using the "none" re-coding of each items, types break down as follow: 5% (A), 84% (B), 3%(C), 3% (D) and 1.5% (E). Other types represent less than 1%

Nevertheless, in the robustness check section below, we examine alternative coding approaches. Specifically, we examine an item-by-item approach, averaging across three items, and focusing only on type (a) respondents (i.e. those who oppose all types of immigrants). Each approach has its limits. The first does not account for the item dependency that we just described, and may thus introduce substantial noise into our definitions of anti-immigrant preferences—for standard measurement error reasons as well as the more systematic reasons we highlight. The second assumes that response patterns can be ordered from less to more anti-immigrant, something we questioned above (e.g. is type (a) more anti-immigrant than type (d)?). The third assumes that only people who reject all immigrants are truly anti-immigrant. However if ethnocentrism is what matters, then type (d) should be the most affected. Similarly, if social desirability bias matters, then education's effect on anti-immigration preferences should affect type (c) response patterns.

A.2.2 Coding of far-right anti-immigration parties

We identify the largest far-right anti-immigrant parties based on our knowledge of each country and on ESS documentation. We then use the Chapel Hill Expert Survey data (Bakker et al. 2015) to identify smaller, less well-known anti-immigration parties; parties that receive at least an 8 on

Table A4: Parties coded as anti-immigrant, by country

Country	**Party**
Denmark	Fremskridtspartiet
Denmark	Dansk Folkeparti (DF)
France	Front National (FR)
France	Mouvement National Republicain (MNR)
Great Britain	British National Party (BNP)
Great Britain	UK Independence Party (UKIP)
Netherlands	Lijst Pim Fortuyn (LPF)
Netherlands	Leefbaar Nederland (LV)
Netherlands	Partij voor de Vrijheid (PVV)
Sweden	Sverigedemokraterna (SD)

the 0-10 immigration policy scale are up for consideration. Table A4 lists the parties that we ultimately identified as far-right anti-immigration parties. Note that some parties did not exist for the full duration of our sample.

A.3 Checks on the RD 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 two common classes of statistical test to validate the "no sorting" assumption.

A.3.1 Density tests

Although selection into cohorts seems implausible since parents could not have anticipated the timing of compulsory education reforms more than a decade before their child was 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 prob-

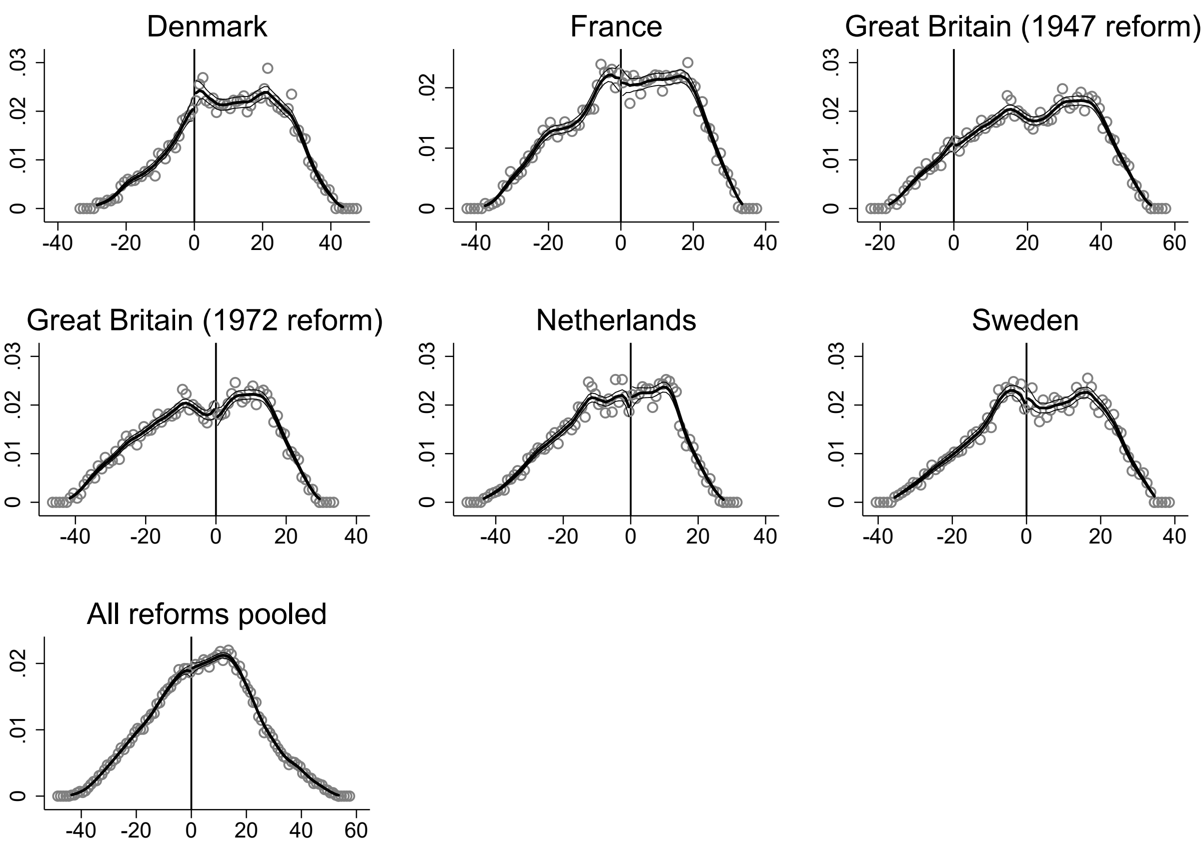

Figure A2: Density of data either side of the reform, pooled across countries lem with sampling. Fortunately, Figure A2 shows that there is no evidence of heaping, in any particular country or in the pooled sample. This graphical observation is supported by McCrary (2008) tests, which in each case fail to reject the null hypothesis that the density does not change at the reform. For example, for the pooled sample with a bandwidth of 5, the difference in density at the discontinuity is 0.022 (standard error of 0.039). Moreover, the test proposed by Frandsen (forthcoming) for the case of a discrete running variable similarly finds no difference in density at the discontinuity in the pooled sample (p=0.93 for k=0 and p=1.00 for k=0.1). The density test proposed by Calonico, Cattaneo and Titiunik (2014) also finds no significant difference in density (p=0.66).

A.3.2 Continuity tests

Even though the density of the data is similar across respondents 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 A5 by testing for continuity across the discontinuity for 13 pre-determined variables in the pooled sample. The estimates, which are based on the same estimation strategy used to estimate the results in the main paper, show that respondents either side of the discontinuity are generally statistically indistinguishable on characteristics determined before the reform occurred.

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

Table A6 shows the "first stage" RD estimates documenting the effect of the reforms on each additional year of schooling separately, both by each country separately and pooled across countries. As noted in the main paper, the results indicate that the largest increases in schooling are concentrated between the 8th and 13th years of formal schooling. The final column shows that the reforms did not significantly affect tertiary education.

Table A5: The “placebo effect” of compulsory education on predetermined variables, pooled across reforms

	Female	Ethnic minority	Father born in country	Father secondary education	Mother born in country	Mother secondary education	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	-0.014	-0.010*	0.012^{+}	0.027	0.008	-0.007	0.008	-0.004	0.003	0.000	-0.004	-0.012	0.008
	(0.012)	(0.004)	(0.007)	(0.017)	(0.007)	(0.018)	(0.009)	(0.010)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)
Bandwidth	13	15	15	7	15	6	11	10	12	14	15	11	9
Observations	33,948	38,238	38,197	17,041	38,315	15,327	29,176	26,789	31,549	36,211	38,381	29,176	24,278
Outcome mean	0.53	0.96	0.12	0.48	0.12	0.41	0.15	0.14	0.15	0.14	0.14	0.14	0.13

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 $p < 0.001$.

Table A6: The effect of compulsory education on level of completed schooling

	Years of completed schooling	Completed at least ... of schooling								Any tertiary education
		6 years	7 years	8 years	9 years	10 years	11 years	12 years	13 years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Denmark										
Reform	0.448^{+}	0.021	0.002	0.085*	0.064	0.063	0.070^{+}	0.070^{+}	0.065	0.021
	(0.260)	(0.017)	(0.021)	(0.038)	(0.041)	(0.043)	(0.041)	(0.040)	(0.046)	(0.039)
Bandwidth	7	10	7	6	6	6	7	8	6	8
Observations	2,635	3,494	2,635	2,330	2,330	2,330	2,635	2,938	2,330	2,922
Outcome mean	10.86	0.95	0.94	0.83	0.77	0.73	0.67	0.60	0.49	0.39
Panel B: France										
Reform	0.285^{+}	-0.016	0.013	0.009	0.100***	0.070*	0.039	0.022	0.011	0.021
	(0.156)	(0.016)	(0.015)	(0.024)	(0.025)	(0.031)	(0.032)	(0.034)	(0.033)	(0.030)
Bandwidth	11	7	11	7	11	9	10	9	9	8
Observations	5,078	3,450	5,078	3,450	5,078	4,278	4,692	4,278	4,278	3,851
Outcome mean	10.84	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	0.552*	0.000	-0.001	0.018	0.019	0.220***	0.098*	0.086	0.128*	0.084
	(0.230)	(0.008)	(0.010)	(0.021)	(0.030)	(0.049)	(0.050)	(0.053)	(0.063)	(0.053)
Bandwidth	4	5	4	3	4	6	6	5	3	4
Observations	1,492	1,816	1,492	1,191	1,492	2,130	2,130	1,816	1,191	1,434
Outcome mean	10.70	0.99	0.99	0.98	0.95	0.75	0.47	0.34	0.29	0.20
Panel D: Great Britain (1972 reform)										
Reform	0.274*	0.001	-0.004	0.005	0.003	0.009	0.061*	0.078^{+}	0.067^{+}	0.028
	(0.127)	(0.006)	(0.007)	(0.008)	(0.010)	(0.015)	(0.029)	(0.040)	(0.040)	(0.034)
Bandwidth	7	13	10	12	11	8	8	6	6	8
Observations	3,754	7,001	5,451	6,479	5,956	4,270	4,270	3,233	3,233	4,177
Outcome mean	11.83	0.99	0.99	0.99	0.98	0.96	0.83	0.60	0.49	0.40
Panel E: Netherlands										
Reform	0.204^{+}	0.003	0.006	0.011	0.019	0.032^{+}	0.034	0.082**	0.040	0.022
	(0.107)	(0.007)	(0.010)	(0.013)	(0.017)	(0.017)	(0.024)	(0.028)	(0.034)	(0.032)
Bandwidth	11	10	10	9	8	12	11	10	8	8
Observations	5,867	5,323	5,323	4,800	4,282	6,412	5,867	5,323	4,282	4,273
Outcome mean	11.79	0.99	0.98	0.96	0.94	0.88	0.78	0.71	0.58	0.31
Panel F: Sweden										
Reform	0.280*	0.005	0.005	0.045*	0.034^{+}	0.055*	0.042	0.048	0.066^{+}	0.010
	(0.130)	(0.005)	(0.007)	(0.019)	(0.018)	(0.026)	(0.029)	(0.033)	(0.035)	(0.032)
Bandwidth	11	11	10	6	12	11	11	10	9	10
Observations	4,759	4,759	4,394	2,754	5,119	4,759	4,759	4,394	4,031	4,379
Outcome mean	11.41	0.99	0.99	0.96	0.89	0.79	0.72	0.60	0.49	0.31
Panel G: All reforms pooled										
Reform	0.290***	0.005	0.002	0.018**	0.032***	0.071***	0.056***	0.071***	0.059***	0.021
	(0.057)	(0.004)	(0.005)	(0.006)	(0.008)	(0.012)	(0.011)	(0.015)	(0.016)	(0.013)
Bandwidth	12	13	10	13	11	8	13	8	7	9
Observations	31,549	33,948	26,789	33,948	29,176	21,777	33,948	21,777	19,281	24,001
Outcome mean	11.33	0.98	0.97	0.94	0.90	0.82	0.71	0.57	0.46	0.31

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 $p < 0.001$.

A.5 RD robustness checks

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

A.5.1 RD estimates by bandwidth

Figure A3 examines how our estimates vary with the choice of bandwidth. We examine all bandwidths between 2 and 15.[3] 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 reflects weaker identification further away from the discontinuity. Moreover, the estimates close to the discontinuity suggest that the reforms may also have reduced the beliefs that immigration is bad for the economy and undermines local culture.

A.5.2 Rectangular kernel results

The estimates in Table A7 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, and hence is not our preferred specification.

A.5.3 Local quadratic and cubic regressions

Table A8 includes quadratic and cubic polynomial cohort trends either side of the discontinuity, rather than the linear trends used in the main paper. The results show similar point estimates when these more flexible trends around the discontinuity are permitted.

[3]Our estimator cannot use a 1-cohort bandwidth because the coefficients for the treatment effect and trends either side of the discontinuity are perfectly collinear.

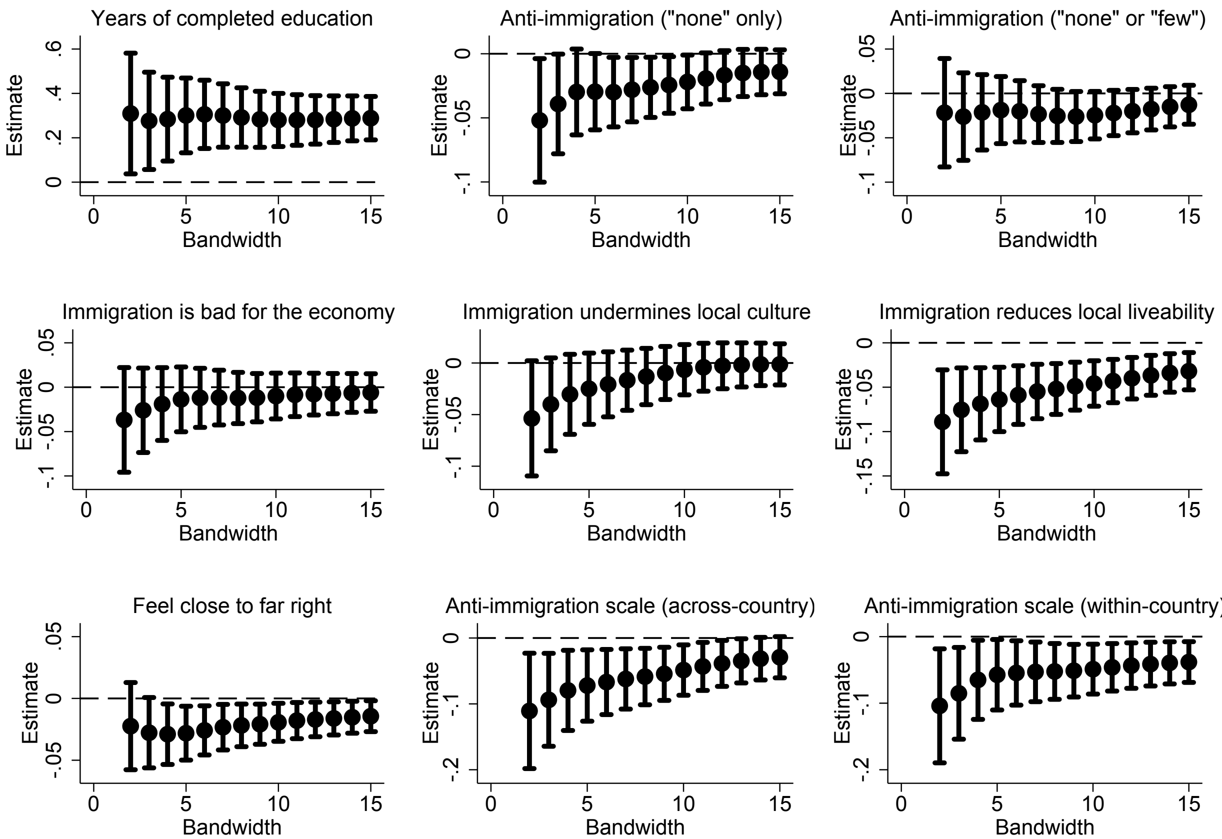

Figure A3: The effect of compulsory education on anti-immigration attitudes, by RD bandwidth

Table A7: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, pooled across reforms and using a rectangular kernel

	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)
Reform	0.275***	-0.019^{+}	-0.032*	-0.013	-0.006	-0.061***	-0.013^{+}	-0.067**	-0.051*
	(0.059)	(0.010)	(0.016)	(0.013)	(0.013)	(0.017)	(0.007)	(0.023)	(0.021)
Bandwidth	9	9	6	8	8	5	10	6	7
Observations	24,278	24,278	16,740	21,777	21,777	14,262	16,430	16,740	19,281
Outcome mean	11.32	0.18	0.55	0.36	0.28	0.34	0.05	0.00	0.02

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

Table A8: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, pooled across reforms using quadratic and cubic forms of the forcing variable

	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 estimates with quadratic trends in the forcing variable—all reforms pooled									
Reform	0.306***	-0.028*	-0.035*	-0.009	-0.007	-0.058***	-0.023*	-0.068**	-0.057**
	(0.072)	(0.013)	(0.016)	(0.015)	(0.014)	(0.015)	(0.010)	(0.024)	(0.022)
Bandwidth	17	16	16	19	17	17	15	15	18
Observations	42,510	40,436	40,436	46,219	42,510	42,510	23,358	38,381	44,414
Outcome mean	11.35	0.18	0.55	0.36	0.29	0.34	0.05	0.00	0.01
Panel B: Reduced form estimates with cubic trends in the forcing variable—all reforms pooled									
Reform	0.319***	-0.034*	-0.040$^+$	-0.015	-0.031	-0.068***	-0.023*	-0.075**	-0.069**
	(0.079)	(0.015)	(0.021)	(0.019)	(0.020)	(0.018)	(0.011)	(0.026)	(0.023)
Bandwidth	25	21	19	20	18	23	26	24	29
Observations	55,245	49,670	46,219	47,982	44,414	52,679	33,470	54,011	59,480
Outcome mean	11.34	0.17	0.55	0.36	0.29	0.34	0.05	-0.00	0.01

Notes: All specifications are estimated using local quadratic or cubic 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 $p < 0.001$.

A.5.4 Placebo reforms five and ten years earlier

Table A9 examines the effect of placebo reforms set to occur five and ten years before the actual reforms. The results clearly show that the placebo reforms neither increased schooling nor replicate our main findings. The statistically significant differences are broadly consistent with chance and, consistent with the results not being driven by cohort trends, do not replicate our main estimates.

A.5.5 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 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) 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 affected 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 A10 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 A9: The effect of placebo reforms (occurring five year earlier) on years of completed schooling and anti-immigration attitudes, pooled across reforms

	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: Placebo reform five years earlier									
Placebo reform	0.042	0.009	0.028*	-0.007	-0.015	0.005	-0.005	0.002	0.013
	(0.073)	(0.010)	(0.012)	(0.013)	(0.011)	(0.013)	(0.007)	(0.018)	(0.018)
Bandwidth	8	12	13	12	13	11	13	13	12
Observations	19,992	28,611	30,683	28,611	30,683	26,463	18,987	30,683	28,611
Outcome mean	-0.02	0.18	0.55	0.36	0.29	0.34	0.05	0.00	0.02
Panel B: Placebo reform ten years earlier									
Placebo reform	-0.067	-0.008	0.001	0.020	0.011	-0.021	0.015^{+}	0.006	-0.002
	(0.088)	(0.012)	(0.015)	(0.016)	(0.014)	(0.015)	(0.009)	(0.021)	(0.020)
Bandwidth	7	12	10	9	10	11	9	11	11
Observations	15,447	25,122	21,292	19,322	21,292	23,261	12,058	23,261	23,261
Outcome mean	-0.02	0.18	0.55	0.36	0.29	0.34	0.05	0.00	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 $p < 0.01$, *** denotes $p < 0.001$.

Table A10: Exclusion restriction tests, pooled across reforms

	Live with partner (1)	Never married (2)	Ever divorced (3)	Child at home (4)	Ever had a child at home (5)
Reform	-0.000	-0.001	0.015	-0.008	0.007
	(0.013)	(0.012)	(0.019)	(0.017)	(0.018)
Bandwidth	11	12	10	6	6
Observations	29,176	13,095	11,164	16,740	16,740
Outcome mean	0.67	0.13	0.29	0.31	0.50

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 $p < 0.001$.

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

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—the experience of schooling itself did not substantially change for affected cohorts. Table A11 below shows that excluding Sweden produces similar results. Ultimately, although no test can definitively support the exclusion restriction, these checks suggest that the most plausible exclusion restrictions are unlikely to be biasing our estimates.

A.5.6 Reform-by-reform exclusion

At the cost of some loss of precision, Table A11 reports the pooled results when each reform is separately removed from the sample. Most importantly, the results demonstrate that the removal of no single country causes the reduced form estimates to cease to be statistically significantly at the 10% level for the anti-immigration scales in columns (8) and (9). Consistent with the cross-country heterogeneity observed in Table 3 in the main paper, the point estimates associated with this "leave one out" approach increase when the reforms in Great Britain are removed and decrease when the reforms in Denmark, France, and the Netherlands are removed. The point estimates for Sweden lie around the cross-country average; the removal of Sweden thus does little to affect the estimates.[4]

A.6 Alternative measures of outcome variables

The following tests show that alternative operationalizations of our dependent variables produce substantively similar results.

A.6.1 Item-by-item exclusion from the anti-immigration scales

A potential concern is that our results are driven by a single idiosyncratic item within our scale, rather than a systematic shift in attitudes towards immigration. To assess the extent to which the results rely on a single survey item, Table A12 reports the pooled estimates for anti-immigration scales computed separately without each item. The results indicate that our findings are robust to removing any particular item.

A.6.2 Anti-immigration scale computed as the first factor

Rather than take the average across our six anti-immigration attitude items, which are of course correlated with one another, another approach is to use the first factor of a factor analysis containing our six items. The factor analysis suggests that our items align along a single dimension: for the across countries scale, the first factor has an eigenvalue of 2.19, while the second factor's eigenvalue is only 0.22 (and thus far below levels typically used to determine the existence of a second factor); for the within country factor analyses used for the within-country scales, we observe similar results when conducting the factor analysis in each country separately. Using

[4] 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.

Table A11: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, pooled across reforms and excluding each reform separately

	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 estimates without Denmark									
Reform	0.286***	-0.034**	-0.029+	-0.013	-0.013	-0.058**	-0.009	-0.072**	-0.050*
	(0.060)	(0.013)	(0.017)	(0.016)	(0.016)	(0.019)	(0.009)	(0.028)	(0.023)
Bandwidth	10	8	7	8	7	6	9	6	8
Observations	23,295	18,839	16,646	18,839	16,646	14,410	11,779	14,410	18,839
Outcome mean	11.39	0.18	0.52	0.36	0.29	0.35	0.04	-0.01	0.00
Panel B: Reduced form estimates without France									
Reform	0.267***	-0.015	-0.025	0.000	0.008	-0.051**	-0.022*	-0.056*	-0.040+
	(0.060)	(0.012)	(0.018)	(0.014)	(0.014)	(0.018)	(0.009)	(0.028)	(0.022)
Bandwidth	12	10	7	10	10	6	11	6	9
Observations	26,084	22,097	15,831	22,097	22,097	13,740	12,902	13,740	20,000
Outcome mean	11.42	0.17	0.53	0.36	0.27	0.33	0.06	-0.02	0.02
Panel C: Reduced form estimates without Great Britain (1947 reform)									
Reform	0.280***	-0.020*	-0.032+	-0.007	-0.006	-0.065***	-0.020**	-0.075**	-0.067**
	(0.061)	(0.010)	(0.017)	(0.013)	(0.011)	(0.017)	(0.008)	(0.024)	(0.022)
Bandwidth	12	11	7	11	15	7	11	7	8
Observations	27,518	25,450	16,816	25,450	33,391	16,816	17,262	16,816	18,983
Outcome mean	11.40	0.16	0.52	0.35	0.26	0.32	0.05	-0.05	-0.00
Panel D: Reduced form estimates without Great Britain (1972 reform)									
Reform	0.301***	-0.028*	-0.025	-0.025	-0.036*	-0.078***	-0.015+	-0.088***	-0.075**
	(0.071)	(0.013)	(0.017)	(0.016)	(0.016)	(0.018)	(0.008)	(0.027)	(0.025)
Bandwidth	10	8	8	8	7	6	11	6	7
Observations	21,338	17,507	17,507	17,507	15,527	13,507	17,041	13,507	15,527
Outcome mean	11.20	0.17	0.53	0.34	0.26	0.32	0.04	-0.04	0.03
Panel E: Reduced form estimates without the Netherlands									
Reform	0.303***	-0.035*	-0.019	-0.001	-0.011	-0.045*	-0.023*	-0.061*	-0.042+
	(0.069)	(0.015)	(0.018)	(0.016)	(0.017)	(0.018)	(0.011)	(0.029)	(0.024)
Bandwidth	11	7	7	9	7	7	8	6	8
Observations	23,309	15,492	15,492	19,478	15,492	15,492	9,186	13,447	17,495
Outcome mean	11.21	0.19	0.56	0.38	0.31	0.35	0.06	0.03	0.03
Panel F: Reduced form estimates without Sweden									
Reform	0.299***	-0.023*	-0.035*	-0.013	-0.007	-0.062***	-0.028**	-0.071**	-0.064**
	(0.065)	(0.011)	(0.016)	(0.015)	(0.013)	(0.018)	(0.010)	(0.024)	(0.024)
Bandwidth	11	12	9	10	11	7	8	8	8
Observations	24,417	26,430	20,247	22,395	24,417	16,093	12,041	18,145	18,145
Outcome mean	11.31	0.20	0.61	0.38	0.32	0.38	0.06	0.08	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 $p < 0.01$, *** denotes $p < 0.001$.

Table A12: The effect of compulsory education on years of completed schooling and anti-immigration scales, pooled across reforms and excluding each item separately

	Reduced form		Instrumental variables	
	Anti-immigration scale (across)	Anti-immigration scale (within)	Anti-immigration scale (across)	Anti-immigration scale (within)
	(1)	(2)	(3)	(4)
Panel A: Removing anti-immigration (“none” only)				
Reform	-0.061**	-0.051*		
	(0.025)	(0.022)		
Years of completed schooling			-0.197*	-0.167*
			(0.077)	(0.071)
Bandwidth	7	8	7	8
Observations	19,281	21,777	19,281	21,777
Outcome mean	-0.00	0.01	-0.00	0.01
Schooling mean			11.32	11.32
Panel B: Removing anti-immigration (“none” or “few”)				
Reform	-0.070**	-0.054*		
	(0.024)	(0.022)		
Reform			-0.212**	-0.179*
			(0.078)	(0.072)
Bandwidth	7	9	7	9
Observations	19,281	24,278	19,281	24,278
Outcome mean	0.00	0.02	0.00	0.02
Schooling mean			11.32	11.32
Panel C: Removing immigration is bad for the economy				
Reform	-0.083**	-0.065**		
	(0.026)	(0.022)		
Years of completed schooling			-0.246**	-0.208**
			(0.083)	(0.073)
Bandwidth	6	8	6	8
Observations	16,740	21,777	16,740	21,777
Outcome mean	0.00	0.02	0.00	0.02
Schooling mean	0.00	0.00	11.32	11.32
Panel D: Removing immigration undermines local culture				
Reform	-0.079**	-0.064**		
	(0.025)	(0.021)		
Years of completed schooling			-0.234**	-0.208**
			(0.080)	(0.073)
Bandwidth	6	8	6	8
Observations	16,740	21,777	16,740	21,777
Outcome mean	0.00	0.02	0.00	0.02
Schooling mean			11.32	11.32
Panel E: Removing immigration reduces local livability				
Reform	-0.057*	-0.043*		
	(0.024)	(0.020)		
Years of completed schooling			-0.173*	-0.144*
			(0.072)	(0.066)
Bandwidth	7	9	7	9
Observations	19,281	24,278	19,281	24,278
Outcome mean	0.00	0.02	0.00	0.02
Schooling mean			11.32	11.32
Panel F: Removing feel close to far-right				
Reform	-0.066**	-0.055*		
	(0.025)	(0.022)		
Years of completed schooling			-0.199**	-0.181*
			(0.077)	(0.072)
Bandwidth	7	9	7	9
Observations	19,281	24,278	19,281	24,278
Outcome mean	-0.01	0.01	-0.01	0.01
Schooling mean			11.32	11.32

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 $p < 0.001$.

the across and within country factor analyses to compute our anti-immigration scales, Table A13 reports similar results to those in the main paper.

A.6.3 Alternative operationalizations of anti-immigration preferences

As discussed in the main paper, we code an anti-immigration preference as answering “none” (or “none” or “few”) in response to at least one of the questions asking about preferences for immigration from the same ethnic group, a different ethnic group, or from poorer countries outside Europe. In this section, we examine whether our results are robust to alternative coding decisions. In particular, we consider three alternative approaches:

- Approach (1): we focus on respondents who express opposition to all three types of immigrants (type (a) in Table A3). We generate a dummy variable equal to 1 if the respond says “none” (or “none” or “few”) to *all* three question.
- Approach (2): we average responses across all three questions.
- Approach (3): we examine each of the three items on limits to immigration separately.

Section A.2 discusses the limits of all these alternative coding approaches.

Table A14 reports the pooled results, where columns (1) and (2) replicate the results from Table 3 in the main paper, columns (3) and (4) report the results using approach (1), and columns (5) and (6) report the results using approach (2). Consistent with our argument that respondents are reluctant to report anti-immigration attitudes across the board (approach (1)), column (3) shows that only 5% do so, and we find no significant effects of education or these hardened anti-immigrant respondents. For the “none” or “few” version of approach (1) and both estimates for approach (2), we observe negative coefficients. Although none reaches statistical significance, the results are similar to our preferred coding decision, and thus broadly support the main findings. In the case of column (6), the p value is 0.13.

Table A13: The effect of compulsory education on years of completed schooling and anti-immigration attitudes, using the first factor to compute the anti-immigration attitudes scales

	Anti-immigration scale (across)	Anti-immigration scale (within)
	(1)	(2)
Panel A: Reduced form RD estimates—Denmark		
Reform	-0.132	-0.155^{+}
	(0.084)	(0.087)
Bandwidth	10	10
Observations	3,434	3,434
Outcome mean	0.09	0.15
Panel B: Reduced form RD estimates—France		
Reform	-0.239**	-0.255**
	(0.091)	(0.095)
Bandwidth	8	8
Observations	3,806	3,806
Outcome mean	0.24	0.33
Panel C: Reduced form RD estimates—Great Britain (1947 reform)		
Reform	-0.200	-0.248
	(0.328)	(0.353)
Bandwidth	5	4
Observations	312	254
Outcome mean	0.72	0.79
Panel D: Reduced form RD estimates—Great Britain (1972 reform)		
Reform	-0.071	-0.095
	(0.181)	(0.192)
Bandwidth	12	12
Observations	952	952
Outcome mean	0.31	0.41
Panel E: Reduced form RD estimates—Netherlands		
Reform	-0.187*	-0.207*
	(0.072)	(0.082)
Bandwidth	7	7
Observations	3,753	3,753
Outcome mean	-0.17	-0.09
Panel F: Reduced form RD estimates—Sweden		
Reform	0.073	0.083
	(0.082)	(0.086)
Bandwidth	11	11
Observations	1,846	1,846
Outcome mean	-0.58	-0.53
Panel G: Reduced form RD estimates—all reforms pooled		
Reform	-0.147**	-0.162***
	(0.045)	(0.048)
Bandwidth	8	7
Observations	13,429	11,939
Outcome mean	0.01	0.09
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled		
Years of completed schooling	-0.428**	-0.464**
	(0.153)	(0.164)
Bandwidth	8	7
Observations	13,429	11,939
Outcome mean	0.01	0.09
Schooling mean	11.32	11.32

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 $p < 0.001$.

Table A14: The effect of compulsory education on alternatively-coded anti-immigration preference, pooled across all reforms

	Main paper approach: oppose at least one		Approach (1): oppose all three		Approach (2): Average	
	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reduced form RD estimates—all reforms pooled						
Reform	-0.026*	-0.027^{+}	-0.001	-0.008	-0.009	-0.020
	(0.011)	(0.016)	(0.005)	(0.012)	(0.007)	(0.013)
Bandwidth	9	7	14	10	11	8
Observations	24,278	19,281	36,211	26,789	29,176	21,777
Outcome mean	0.18	0.55	0.05	0.28	0.11	0.41
Panel B: Fuzzy RD (instrumental variables) estimates—all reforms pooled						
Years of completed schooling	-0.087*	-0.083^{+}	-0.005	-0.029	-0.030	-0.062
	(0.038)	(0.049)	(0.018)	(0.041)	(0.024)	(0.041)
Bandwidth	9	7	14	10	11	8
Observations	24,278	19,281	36,211	26,789	29,176	21,777
Outcome mean	0.18	0.55	0.05	0.28	0.11	0.41
Schooling mean	11.32	11.32	11.34	11.32	11.32	11.32

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 $p < 0.001$.

Next, we examine each of the three items separately, i.e. approach (3). Table A15 shows that, for each type of immigrant, we observe some evidence suggesting that educated respondents become more tolerant of such immigrants. Yet, the reduction in anti-immigration preferences among those affected by a reform are primarily driven by reduced strong opposition to immigrants from poorer countries. As argue previously, this cannot be interpreted as evidence that respondents only care about poor immigrants (response pattern (c), see Table A3). Indeed, restrictive answers to this question can also be part of response patterns (a)—"limit all three types"—and (d)—"accept only same race."

A.6.4 Ordinal measurement of anti-immigration outcomes

As noted 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 and comparable across countries. Nevertheless, Table A16 presents the results when using the raw (standardized) ordinal measures of our seven continuous items, as well as combining those seven items as an index (across and within countries).[5] As can be seen, we find broadly similar results: the pooled estimates in panels G and H show that all items report negative coefficients, while some individual items and both overall scales are statistically significant. While the individual items are less precisely estimated, which is consistent with our arguments for our preferred operationalizations, the results nevertheless suggest that the ordinal variables produce similar results to our preferred coding.

A.7 Additional ESS results

This section presents several additional results cited in the main paper from our ESS dataset.

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

Table A15: The effect of compulsory education on anti-immigration preferences, distinguishing type of immigrant

	Same ethnicity		Different ethnicity		Poor country	
	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")	Anti-immigration ("none" only)	Anti-immigration ("none" or "few")
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reduced form RD estimates—Denmark						
Reform	-0.011	-0.017	0.017	-0.025	-0.006	-0.040
	(0.022)	(0.038)	(0.012)	(0.036)	(0.028)	(0.036)
Bandwidth	10	9	7	8	9	9
Observations	3,494	3,215	2,635	2,938	3,215	3,215
Outcome mean	0.09	0.54	0.02	0.22	0.15	0.65
Panel B: Reduced form RD estimates—France						
Reform	-0.034	-0.017	-0.017	0.001	-0.038	-0.038
	(0.023)	(0.031)	(0.017)	(0.031)	(0.023)	(0.030)
Bandwidth	9	11	9	11	12	11
Observations	4,278	5,078	4,278	5,078	5,465	5,078
Outcome mean	0.11	0.49	0.06	0.36	0.17	0.54
Panel C: Reduced form RD estimates—Great Britain (1947 reform)						
Reform	-0.009	-0.010	0.015	0.024	0.000	0.078
	(0.042)	(0.059)	(0.032)	(0.048)	(0.050	(0.052)
Bandwidth	6	4	7	7	5	5
Observations	2130	1492	2465	2465	1816	1816
Outcome mean	0.21	0.62	0.12	0.45	0.27	0.69
Panel D: Reduced form RD estimates—Great Britain (1972 reform)						
Reform	-0.024	-0.009	-0.014	-0.034	-0.047	-0.039
	(0.024)	(0.034)	(0.021)	(0.036)	(0.030)	(0.035)
Bandwidth	9	9	9	8	8	8
Observations	4,848	4,848	4,848	4,270	4,270	4,270
Outcome mean	0.14	0.49	0.10	0.41	0.19	0.55
Panel E: Reduced form RD estimates—Netherlands						
Reform	0.010	-0.041	0.009	-0.052^{+}	-0.028	-0.057
	(0.019)	(0.032)	(0.016)	(0.030)	(0.023)	(0.035)
Bandwidth	8	9	9	9	8	8
Observations	4,282	4,800	4,800	4,800	4,282	4,282
Outcome mean	0.08	0.38	0.07	0.33	0.12	0.44
Panel F: Reduced form RD estimates—Sweden						
Reform	-0.009	0.010	-0.000	-0.013	-0.001	0.005
	(0.011)	(0.023)	(0.008)	(0.021)	(0.013)	(0.023)
Bandwidth	9	10	11	10	8	12
Observations	4,031	4,394	4,759	4,394	3,632	5,119
Outcome mean	0.02	0.15	0.01	0.11	0.03	0.18
Panel G: Reduced form RD estimates—all reforms pooled						
Reform	-0.010	-0.014	-0.001	-0.019	-0.019*	-0.022
	(0.009)	(0.014)	(0.005)	(0.013)	(0.010)	(0.015)
Bandwidth	10	9	16	9	11	8
Observations	26,789	24,278	40,436	24,278	29,176	21,777
Outcome mean	0.11	0.43	0.07	0.32	0.15	0.50
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled						
Years of completed schooling	-0.034	-0.046	-0.002	-0.062	-0.066^{+}	-0.071
	(0.029)	(0.047)	(0.018)	(0.045)	(0.034)	(0.048)
Bandwidth	10	9	16	9	11	8
Observations	26,789	24,278	40,436	24,278	29,176	21,777
Outcome mean	0.11	0.43	0.07	0.32	0.15	0.50
Schooling mean	11.32	11.32	11.35	11.32	11.32	11.32

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 $p < 0.001$.

Table A16: The effect of compulsory education on ordinally-coded anti-immigration attitudes

	Anti-immigration from own ethnicity (ordinal)	Anti-immigration from different ethnicity (ordinal)	Anti-immigration from poor countries (ordinal)	Immigration is bad for the economy (ordinal)	Immigration undermines local culture (ordinal)	Immigration reduces local livability (ordinal)	Feel close to far-right (ordinal)	Anti-immigration scale (across) (ordinal)	Anti-immigration scale (within) (ordinal)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Reduced form RD estimates—Denmark									
Reform	-0.059	-0.027	-0.120^{+}	-0.044	-0.061	-0.150*	-0.265**	-0.087^{+}	-0.085
	(0.071)	(0.070)	(0.069)	(0.079)	(0.080)	(0.073)	(0.094)	(0.051)	(0.053)
Bandwidth	8	8	8	8	7	8	9	9	9
Observations	2,846	2,862	2,855	2,836	2,586	2,879	3,161	3,208	3,208
Outcome mean	0.13	-0.30	0.22	-0.05	-0.06	-0.18	0.15	-0.02	0.09
Panel B: Reduced form RD estimates—France									
Reform	-0.086	-0.048	-0.116*	-0.209**	-0.106	-0.090	0.021	-0.073^{+}	-0.073^{+}
	(0.057)	(0.058)	(0.058)	(0.079)	(0.066)	(0.057)	(0.058)	(0.043)	(0.044)
Bandwidth	10	10	10	6	9	10	8	10	10
Observations	4,583	4,573	4,592	2,964	4,249	4,639	3,806	4,691	4,691
Outcome mean	0.12	0.09	0.11	0.06	0.19	0.24	-0.04	0.11	0.01
Panel C: Reduced form RD estimates—Great Britain (1947 reform)									
Reform	0.057	0.120	0.158	-0.017	-0.113	-0.023	0.443*	0.009	0.007
	(0.112)	(0.093)	(0.111)	(0.116)	(0.131)	(0.098)	(0.187)	(0.076)	(0.075)
Bandwidth	4	6	4	4	3	5	5	5	5
Observations	1,460	2,083	1,457	1,453	1,151	1,763	312	1,812	1,812
Outcome mean	0.44	0.36	0.46	0.21	0.46	0.40	0.03	0.38	0.15
Panel D: Reduced form RD estimates—Great Britain (1972 reform)									
Reform	-0.027	-0.002	-0.033	0.004	0.060	0.040	-0.546*	-0.013	-0.011
	(0.060)	(0.057)	(0.057)	(0.067)	(0.067)	(0.069)	(0.271)	(0.054)	(0.053)
Bandwidth	9	10	10	8	8	8	8	8	8
Observations	4,763	5,333	5,343	4,193	4,200	4,215	617	4,263	4,263
Outcome mean	0.17	0.24	0.17	0.14	0.25	0.17	0.29	0.19	-0.04
Panel E: Reduced form RD estimates—Netherlands									
Reform	-0.037	-0.072	-0.122^{+}	-0.119^{+}	-0.106^{+}	-0.173*	-0.090	-0.112*	-0.129*
	(0.066)	(0.062)	(0.073)	(0.064)	(0.059)	(0.070)	(0.061)	(0.049)	(0.055)
Bandwidth	7	8	6	6	6	5	8	6	6
Observations	3,730	4,203	3,241	3,245	3,265	2,817	4,241	3,293	3,293
Outcome mean	-0.11	0.05	-0.11	-0.10	-0.28	-0.07	-0.03	-0.10	-0.06
Panel F: Reduced form RD estimates—Sweden									
Reform	0.002	-0.021	0.014	0.049	0.071	0.026	0.156^{+}	0.021	0.024
	(0.051)	(0.051)	(0.052)	(0.062)	(0.056)	(0.057)	(0.092)	(0.042)	(0.048)
Bandwidth	10	10	10	9	8	10	8	10	10
Observations	4,284	4,275	4,262	3,904	3,593	4,306	1,388	4,389	4,389
Outcome mean	-0.70	-0.58	-0.74	-0.30	-0.59	-0.55	-0.08	-0.54	0.00
Panel G: Reduced form RD estimates—all reforms pooled									
Reform	-0.018	-0.026	-0.048	-0.034	-0.030	-0.061*	-0.048	-0.034	-0.042^{+}
	(0.031)	(0.028)	(0.031)	(0.030)	(0.028)	(0.030)	(0.031)	(0.024)	(0.021)
Bandwidth	8	9	8	8	9	8	11	7	8
Observations	18,860	21,281	18,847	18,844	21,450	18,957	17,906	19,261	21,755
Outcome mean	-0.01	-0.02	-0.00	-0.01	-0.02	-0.00	0.02	-0.00	0.01
Panel H: Fuzzy RD (instrumental variables) estimates—all reforms pooled									
Years of completed	-0.089	-0.084	-0.153	-0.137	-0.140	-0.221*	-0.240^{+}	-0.155*	-0.120^{+}
schooling	(0.093)	(0.090)	(0.093)	(0.095)	(0.095)	(0.098)	(0.127)	(0.077)	(0.069)
Bandwidth	8	9	8	8	9	8	10	7	10
Observations	21,309	23,725	21,291	21,282	23,898	21,413	16,425	19,261	26,761
Outcome mean	-0.01	-0.02	-0.00	-0.01	-0.02	-0.00	0.02	-0.00	0.01

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 $p < 0.001$.

Table A17: First stage estimates corresponding to panel H of Table 3 in the main paper

	Years of completed schooling							
	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform	0.297***	0.325***	0.295***	0.297***	0.338***	0.323***	0.334***	0.308***
	(0.063)	(0.072)	(0.062)	(0.063)	(0.078)	(0.090)	(0.077)	(0.067)
Bandwidth	9	7	10	9	6	10	7	8
Observations	24,278	19,281	26,789	24,278	16,740	15,917	19,281	21,777
Outcome 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 $p < 0.001$.

A.7.1 First stage estimates

Table A17 reports the first stage estimates corresponding to panel G 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.

A.7.2 Cross-sectional correlation between education and anti-immigration attitudes

Table A18 reports the cross-sectional correlation between years of completed schooling and anti-immigration attitudes, controlling for country-survey fixed effects.

A.7.3 Restricting the Great British sample to the post-UKIP period

Unlike the other countries in our sample, Great Britain did not have a popular anti-immigration party until the rise of the United Kingdom Independence Party (UKIP) starting in 2005. Although UKIP was mainly a factor in European Union (EU) elections, its anti-EU stance won popular support, and a significant component of its rhetoric—especially in the run-up to the Brexit referendum—centered on opposition to immigration. In the period covered by the ESS prior to

Table A18: Correlation between years of completed schooling and anti-immigration attitudes, pooled across countries

	Anti-immigration (“none” only) (1)	Anti-immigration (“none” or “few”) (2)	Immigration is bad for the economy (3)	Immigration undermines local culture (4)	Immigration reduces local livability (5)	Feel close to far-right (6)	Anti-immigration scale (across) (7)	Anti-immigration scale (within) (8)
Years of completed schooling	-0.026***	-0.041***	-0.036***	-0.032***	-0.036***	-0.004***	-0.067***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	54,007	54,007	54,007	54,007	54,007	36,701	54,007	54,007
Outcome mean	0.16	0.53	0.35	0.26	0.32	0.05	-0.04	0.00

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

UKIP (i.e. 2002 onward), the British National Party (BNP) did not have a major national platform, while the main parties all downplayed immigration as a salient electoral issue. To the extent that elite messaging drives anti-immigration attitudes, we might then expect education's ability to resist anti-immigration rhetoric only to emerge after UKIP was established.

We examine this possibility empirically by restricting our ESS sample to surveys conducted between 2006 and 2014, once UKIP had been established. The results, in Table A19, provide some evidence to suggest that education's effect were more pronounced during this period. Consistent with elite messaging driving anti-immigration sentiment, and in line with our findings from our other countries, we find that both reforms in Great Britain made voters less likely to express anti-immigration attitudes across several indicators after the emergence of UKIP. The positive effect in column (7) of panel A does not conform with this finding, although it rests on a small number of observations. The negative point estimates found in the 2014 BES data examined in Table A22 broadly support these findings, although the point estimates are less precisely estimated. Although this evidence is less than definitive, there is reason to believe that British voters responded similarly to the mainland European counterparts once a major far-right party emerged in Great Britain.

A.8 Replicating the results using country-specific surveys from France and Great Britain

The estimates derived using the pooled data (panels G and H in Table 3) are driven primarily by Denmark, France, and the Netherlands. The estimates for Sweden, though smaller in magnitude, are substantively in line with these mainland European countries. In contrast, in Great Britain, there is less systematic evidence that anti-immigration sentiments were affected by the 1947 and 1972 reforms. However, in each case, the country-specific point estimates are often relatively imprecise due to the relatively small ESS samples in each country and the necessity of focusing on observations around each discontinuity. Only once pooled do we observe strong and systematic ef-

Table A19: The effect of compulsory education on years of completed schooling and anti-immigration attitudes in Great Britain after 2005

	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—Great Britain (1947 reform)									
Reform	0.749*	-0.147^{+}	-0.007	-0.188*	-0.192^{+}	-0.102	0.090**	-0.293*	-0.277*
	(0.348)	(0.083)	(0.047)	(0.089)	(0.112)	(0.075)	(0.035)	(0.139)	(0.131)
Bandwidth	3	4	7	3	3	4	6	3	3
Observations	861	1,071	1,754	861	861	1,071	362	861	861
Outcome mean	10.71	0.33	0.75	0.45	0.46	0.52	0.06	0.33	0.13
Panel B: Reduced form RD estimates—Great Britain (1972 reform)									
Reform	0.188	-0.028	-0.072^{+}	0.043	0.069^{+}	0.005	-0.121*	0.008	0.009
	(0.123)	(0.031)	(0.044)	(0.034)	(0.035)	(0.038)	(0.058)	(0.053)	(0.050)
Bandwidth	9	10	6	11	10	9	8	11	11
Observations	3,631	4,091	2,437	4,470	4,091	3,631	618	4,470	4,470
Outcome mean	11.80	0.22	0.61	0.43	0.40	0.43	0.11	0.17	-0.03

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 $p < 0.001$.

fects. In this section, we extend our analysis by examining whether these cross-country differences can be replicated using different larger datasets that permit more precise estimates within specific countries. In section A.9, we consider in detail the factors behind this heterogeneity.

As noted in the main paper, the estimation strategy used in this paper requires a dataset large enough to include a significant number of respondents from the pre- and post-reform cohorts. We identified three such datasets, two for France and one for Great Britain. Unfortunately, we could not identify comparable large surveys for Denmark, the Netherlands, and Sweden. Our replication consequently focuses on the difference between France (substantive and statistically significant effects of education) and Great Britain (no systematic effect). This section documents empirical patterns in line with the findings presented in the main paper. These results further strength our conclusion that more time spent in school can affect anti-immigration attitudes. However, as the British case illustrates, it is not always sufficient.

A.8.1 France: education affects proximity to the far right

Data. From 1985 to 2004, a sample of French voters were asked about their attitudes toward decentralizing policies and local government ("Enquête interrégionale des phénomênes politiques"—EIPP—available through the Centre de Données Socio-Politiques).[6] In order to measure preferences and beliefs accurately at the regional level, sample sizes were substantially higher than most surveys (e.g. the ESS): 12,000 to 14,000 respondents were interviewed each year. Pooling our data across all waves, we construct a dataset with 240,000 unique observations.

Educational effects of the 1967 reform. In 12 of the 20 available waves, respondents were asked the age at which they left school. Using this variable, we can identify whether individuals left school before or after having reached the minimum years of schooling required by the 1967 reform. This roughly corresponds with leaving school before reaching 16 versus leaving school

[6]cdsp.sciences-po.fr/fr.

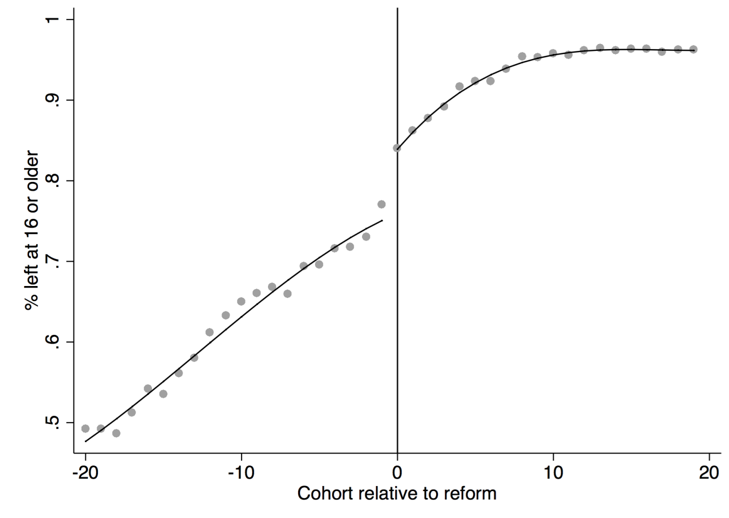

Figure A4: Proportion who left school at 16 or older, thus completing the minimum years of schooling required by the 1967 reform (third-order polynomials either side of the reform), EIPP dataset

at 16 or older. Figure A4 shows that the percentage of respondents leaving school at 16 increased markedly for the first cohort affected by the 1967 reform.

Measuring anti-immigrant sentiment: proximity to anti-immigrant far-right parties. In 20 waves of the survey, respondents were provided with a list of political parties active at that time and asked to "point to the party (they) felt the closest to, or to put it somewhat differently, to the party (they) felt the least distant from." We generate an indicator equal to 1 if respondents signal proximity to the Front National, the main anti-immigrant party in France. Because of a party split in the late 1990s, between 1999 and 2004, respondents wishing to signal support for an anti-immigrant far-right party could also pick Bruno Megret's National Republican Movement. Individuals who picked any other party were coded as 0. Only 4% of respondents express support for anti-immigrant parties, well below the 18% of votes received, on average, by these parties in presidential elections.

In one version of the analysis, we exclude individuals who express no support for any party by choosing the response category "do not know." These individuals represent around 20% of respondents. In a second version of the analysis, we code these individuals as 0. The results are substantively similar using either versions of the outcome. However, the inclusion of "do not know" cases affects statistical significance.

Results. Columns (1) and (2) of Table A20 report RD estimates analogous to those in the main paper. Column (1) excludes respondents who did not identity with any party, while column (2) includes these respondents. The overall pattern is the same as the one described in the main paper: among affected cohorts, the reform consistently produced reductions in anti-immigration sentiments—as measured through a sense of proximity to a anti-immigrant far-right party. Figure A5 documents these pre and post-reform differences graphically.

Although the estimates are more precise than in the ESS, the estimated effect size appears to be somewhat smaller that the one documented in the main paper. This might be due to differences in the time period covered by data collection: the latter centers on the 2010s in the ESS data and the 1990s in this additional dataset. Indeed, the political salience of the far-right is much higher in the former than the latter period.

A.8.2 France: education affects the expression of anti-immigrant sentiment

Data. Since 2000, the French statistics bureau has regularly measured French residents' opinion on a set of social policy issues ("Barometre de la DREES" available through the Quetelet network).[7] Every year, between 3,000 and 4,000 individuals are interviewed. By pooling across 13 waves (every year from 2000 to 2013 with the exception of 2003), we construct a dataset containing c.32,000 unique responses.

Educational effects of the 1967 reform. The survey does not include any questions on the number of years spent in school or on the age at which the respondent left school. As a result, we

[7] www.reseau-quetelet.cnrs.fr/spip.

Table A20: The effect of compulsory education in France, reduced form RD estimates (EIPP dataset)

	Proximity to anti-immigration party	
	(DNK = missing)	(DNK = 0)
	(1)	(2)
Reform	-0.007^{+}	-0.004
	(0.004)	(0.003)
Bandwidth	7	7
Observations	56,240	72,323
Outcome mean	0.04	0.03

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 $p < 0.001$.

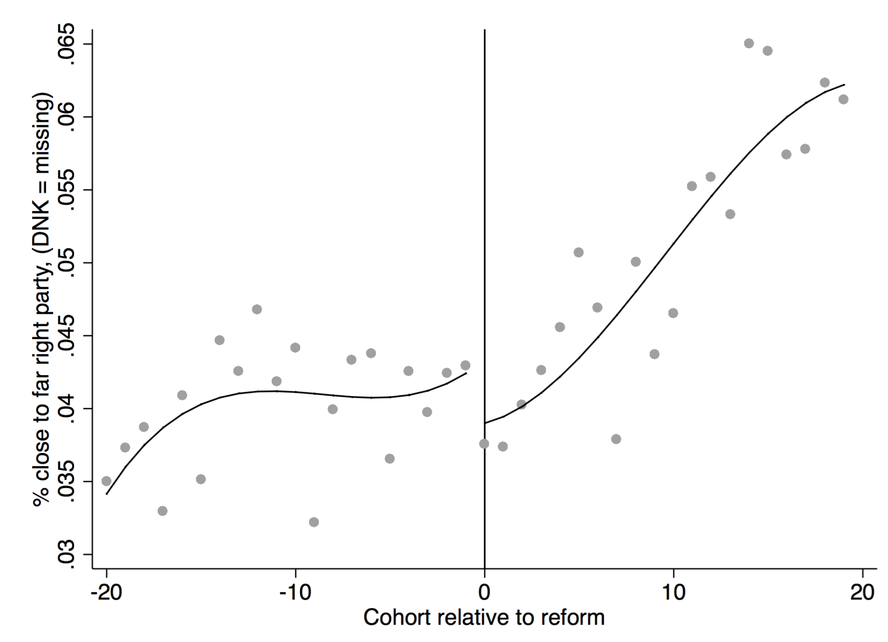

Figure A5: Proportion who feel close to the anti-immigrant far-right - All years (third-order polynomials either side of the reform) cannot confidently link the existence of a significance attitudinal difference across cohorts to a significant difference in years of schooling. The reduced form estimates presented below should thus be treated with some caution, although our previous results demonstrate that the reform did increase the educational attainment of those responding to the ESS surveys over a similar period.

Measuring anti-immigrant sentiment Each year, respondents are asked to answer two questions on immigration. First, respondents are asked to express agreement or disagreement with the following statements: "there are too many immigrant workers in France." Respondents are provided with only two possible answers: "mostly agree" (45%) or "mostly disagree" (54%). A second item asks respondents to pick one of the following two opposite statements: "It is in the interest of society that immigrants keep their own customs and traditions" or "It is in the interest of society that immigrants abandon their own customs and adopt that of the host country instead." Because of widely shared cultural hostility to multiculturalism and because of a strong integrationist republican norm, answers to the second item are, in a French context, hard to interpret. Many individuals who would offer pro-immigrant answers to the ESS items might still consider it best for society that immigrants adopt French customs and traditions (see Tiberj 2008 for survey evidence). Indeed, 76% answer that immigrants should adopt French customs and traditions.

We consequently focus on a combination of these two outcomes. First, we examine whether an individual expresses agreement with the claim that there are too many immigrant workers in France (coded as 1, 0 otherwise). Second, we consider answers to the customs and tradition item alongside the item on immigrant workers. Individuals who believe immigrants should adopt French traditions are coded as "expressing anti-immigrant sentiment" if and only if they also agree that there are too many immigrant workers in France. All other respondents are coded as 0.

Finally, within the subset of individuals who think there are too many immigrant workers and would want them to adopt French customs, we identify those who are "personally (...) very worried" about "migration flows from poor to rich countries." Other response categories provided are

Table A21: The effect of compulsory education in France, reduced form RD estimates (DREES dataset)

	Too many immigrant workers (1)	Too many and Should adopt French customs (2)	Too many and Should adopt French customs and Worry about migration from poor countries (3)
Reform	-0.024	-0.055*	-0.043*
	(0.020)	(0.023)	(0.018)
Bandwidth	9	10	10
Observations	11,637	9,829	11,533
Outcome mean	0.57	0.51	0.23

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 $p < 0.001$.

"somewhat worried," "not too worried," and "not at all worried." Our final measure thus identifies individuals who think they are too many immigrant workers, would want them to adopt French customs and are worried about immigration from poor countries.

Results Table A21 reports our RD estimates using analogous specifications to the main paper. Among affected cohorts, the 1967 reform decreases the likelihood of believing that there are too many immigrant workers who do not integrate (column (2)). The evidence using the "too many immigrant worker" item alone is less strong (column (1)). Column (3) presents results when including the item on concern about migration from poor countries. Individuals who received an additional year of high school are less likely to express such response pattern. Figure A6 documents the latter results graphically.

A.8.3 Great Britain: education does not affect the expression of anti-immigrant sentiment

Data. We pool data from the 13 waves of the 2014-2018 British Election Study (BES) panel. Survey responses was collected over 4 years. An average of 30,000 individuals answered the survey at

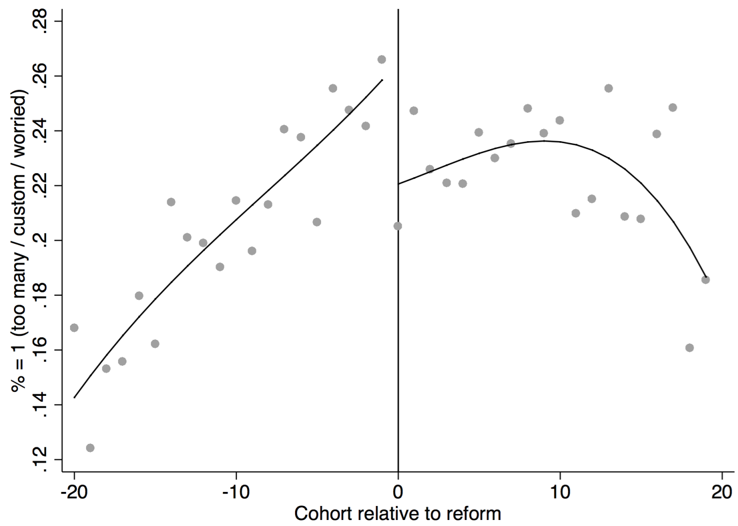

Figure A6: Proportion holding anti-immigrant preferences (third-order polynomials either side of the reform), DREES dataset

each wave. Respondents are contacted through YouGov's online panel, meaning that a significant portion of individuals are re-interviewed at different waves. Depending on the survey item under consideration, the final dataset aggregates between 25,000 and 30,000 unique responses.

Educational effects of the 1972 reform. In 2014, the cohorts first affected by the 1947 reform would be in their 80s. However, only 5% of the sample is aged 75 or over. This dataset thus only allows us to examine the consequences of the 1972 reform. Respondents were asked the "age at which they finished full-time education." Using this variable, we distinguish individuals who left before the age of 16 (coded as 0) and individuals who left at 16 or older (coded as 1). Figure A7 shows that the 1972 reform significantly affected drop-out rates: around 85% of individuals reaching the age of 15 in the 1971 go on to leave school at 16 or older. By 1972 and 1973, this share jumped to 95%.

Measuring anti-immigrant sentiment: feeling toward UKIP, and economic and cultural consequences of immigration. To measure anti-immigrant attitudes we rely on three measures. One

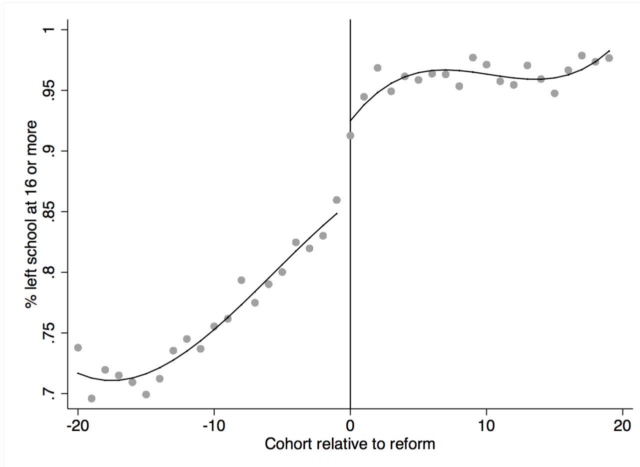

Figure A7: Proportion leaving school at 16 or older (third-order polynomials either side of the reform), BES dataset

measure asks respondents how much they like or dislike a given party. Respondents answered using a 0 (strongly dislike) to 10 (strongly like) scale. We examine responses for the two anti-immigrant party, the BNP and UKIP. We also use two items similar to the ones asked in the ESS. One item asks respondents whether they think that "immigration is good or bad for Britain's economy." The second items asks whether they think that "immigration undermines or enriches Britain's cultural life." Responses are collected using a 1 (bad for the economy/undermines cultural life) to 7 (good for the economy/enriches cultural life) scale.

Results. Figure A8 plots average UKIP likeness score by cohort. Younger cohorts give a much lower score to UKIP than older cohorts. Still there is no evidence that cohorts who reached 15 in 1972, and were consequently affected by the reform, give a significantly lower scores than cohorts who reached the age of 15 right before the reform. The same applies for other items (not shown). These results are consistent with our main findings. They are also consistent with the discussion in section A.7.3, where we document that restricting our sample to years following the emergence of UKIP does not change the null results for the 1972 reform. The larger BES panel data yields the same conclusion. Results for the anti-immigration items are presented in Table A22.

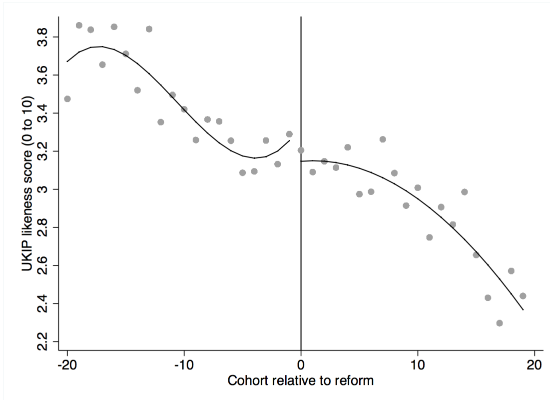

Figure A8: Likeness score for UKIP (third-order polynomials either side of the reform), BES dataset

Table A22: The effect of compulsory education on anti-immigration attitudes in GB (BES dataset)

	Undermines Brit econ (1)	Undermines Brit culture (2)	Like UKIP (3)	Like BNP (4)
Reform	-0.018	-0.070	-0.075	-0.042
	(0.076)	(0.086)	(0.136)	(0.179)
Bandwidth	9	8	7	8
Observations	11,559	10,395	11,138	2,545
Outcome mean	3.77	3.46	3.17	0.90

Notes: Outcome variables are coded such that higher values indicate more (less) anti-immigrant (pro-immigrant) answers. 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 $p < 0.001$.

A.9 Investigating mechanisms

In this section, we examine the mechanisms linking cohorts affected by compulsory schooling reforms to anti-immigration attitudes. The subsection below reviews the literature in search of plausible mechanisms. We briefly describe some of the tests we ran to probe the existence of these causal channels.

As mentioned in the main paper, one can distinguish two broad families of theories and, consequently, two broad families of causal mechanisms. One hypothesizes a direct effect of education through a decrease in ethnocentrism and prejudice. The other hypothesizes an indirect effect of education through changes in one's labor market outcomes or social networks. The mechanisms linking the latter to anti-immigrant sentiment are manifold. One type of mechanism emphasizes the role of material self-interested (Hainmueller and Hopkins 2014; Malhotra, Margalit and Mo 2013). Another focuses on cultural values and social norms (Hainmueller and Hiscox 2007; Kitschelt 1997).

A.9.1 Education's effect on cognitive skills and tolerance

According to the "education-as-character-shaping" literature, one source of attitudinal change is the *experience* of schooling itself. First, schooling improves students' cognitive skills, namely the capacity to understand complex ideas and engage in various forms of reasoning. Using compulsory schooling laws as an instrument for an additional year of education, Meghir, Palme and Simeonova (2013) find that secondary schooling in Sweden improves cognitive skills, as measured using scores from standardize tests given to men of military enlistment age. It is commonly hypothesized, especially in social psychology, that higher cognitive abilities in turn promote racial tolerance. For instance, a recent study by Hodson and Busseri (2012) finds that lower cognitive ability in childhood translates into greater prejudice in adulthood, as measured using right-wing ideology and the extent of inter-group contact.

Table A23: The effect of compulsory education on tolerance in France, reduced form RD estimates (EIPP dataset)

	Doctor (1)	Baker (2)	Teacher (3)	In-law (4)	Priest (5)	Neighbor (6)	Boss (7)	Index (8)
Reform	-0.035	0.044	-0.023	0.003	-0.017	-0.019	-0.001	-0.016
	(0.072)	(0.065)	(0.069)	(0.056)	(0.045)	(0.049)	(0.064)	(0.043)
Bandwidth	5	7	6	8	7	7	7	7
Observations	2,936	3,886	3,351	4,229	3,767	3,876	3,835	3,913
Outcome mean	3.53	3.51	3.45	3.61	3.77	3.75	3.59	3.63

Notes: Outcome variables are coded such that higher values indicate more tolerant answers. 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 $p < 0.001$.

To examine the plausibility of this causal mechanism, we turn to the French dataset entitled "Enquête interrégionale des phénomênes politiques" (EIPP) described in section A.8. In one wave of the survey (1988), respondents were asked whether they would feel uncomfortable having an EU migrant as one's doctor, priest, neighbor, boss, baker, or son/daughter in-law. Respondents were also asked if they would be uncomfortable if their children's school teacher was a EU migrant. Answers were collected using a 4-point scale from "very" (1) to "not at all" (4) uncomfortable. Even with only one wave, the sample is large enough to contain at least 300 observations from each cohort around the reform year. Yet, as Table A23 reports, we find no effect of the reform on any one of the items described above (column (1)-(7)). Column (8) presents results using an index averaging answers to all 7 items (Cronbach's alpha of 0.87).

This evidence is only tentative. Ideally, the items used to measure tolerance would ask about non-EU migrants or about poor EU migrants. Moreover, the difficulties of mediation analysis are well-established, and observing no average effect on a mechanism may not necessarily prove that the mechanism does not drive the results (Bullock, Green and Ha 2010). Nevertheless, we see this test as indicative that pre- and post-reform cohorts do not systematically differ when it comes to tolerance of EU foreign-born individuals. Future research will have to examine whether this null result extends to other types of immigrants.

A.9.2 Education's effect on human values

Schooling affects students beyond analytical reasoning. Among all the non-cognitive constructs and measures used by education specialists, we focus on education's potential effect on value orientation as captured by Schwartz's (1992) human value typology, which identifies fundamental values that individuals from all cultures seek to pursue (i.e. motivational values). More specifically, we focus on what Schwartz calls the value of universalism defined as "understanding, appreciation, tolerance and protection for the welfare of all people and for nature." This value has been repeatedly associated with lower levels of prejudice (Davidov and Meuleman 2012; Sagiv and Schwartz 1995).

Thanks to a battery of items on basic human values asked in each wave of the ESS, we examine whether individuals from cohorts affected by a reform are more likely to report that the following statements describe them well: "It is important to her/him to listen to people who are different from her/him. Even when she/he disagrees with them, she/he still wants to understand them," and "She/he thinks it is important that every person in the world should be treated equally. She/he believes everyone should have equal opportunities in life." Answers were collected using a scale ranging from "very much like me" (1) to "Not like me at all" (6). Results are listed in Table A24 over columns (1) and (2). We find no evidence suggesting that answers to these two questions differ systematically across pre- and post-reform cohorts.

These null results are not particularly surprising given the nature of the treatment, namely being required to remain in school for several years. A change in fundamental values is unlikely to occur in such a short period of time. The "education-as-character-shaping" channel might require a longer exposure to a very specific type of training that emphasizes these values. Such conditions may be more likely to be met in tertiary education, especially in humanities or social studies degree.

Table A24: The effect of compulsory education on cosmopolitan values, reduced form RD estimates (ESS dataset)

	HV: All treated equally (1)	HV: Understand people who are different (2)	Gay and Lesbian people live as want (3)	Euro integration go further (4)
Reform	0.001	-0.023	-0.014	-0.087*
	(0.026)	(0.026)	(0.025)	(0.037)
Bandwidth	11	11	12	8
Observations	28,209	28,243	31,337	14,880
Outcome mean	4.60	4.60	4.60	4.61

Notes: Outcomes variables are coded such that higher scores indicate less identification with the human values listed. In the case of the gay rights and EU integration, higher values indicate a less (more) liberal-cosmopolitan (illiberal-authoritarian) response. 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 $p < 0.001$.

However, unlike secondary education, tertiary education is not compulsory. In other words, even if tertiary education has an effect in itself, it may be more likely to be a "reinforcing" effect: values influence individuals' educational choices (e.g. concentration and career plans); the type of education they receive, in turn, further strengths their commitment to these values.

A.9.3 Education's effect on socialization networks

Because more educated individuals face better labor market and economic conditions, they are less likely to perceive a zero-sum relationship between their material well-being and that of immigrants. There is some evidence that self-interested zero-sum reasoning affects anti-immigrant sentiment (see Dancygier 2010; Gerber et al. 2017; Malhotra, Margalit and Mo 2013; Quillian 1995). However, an emerging consensus in the literature is that most of the heavy lifting is done by non-material and non self-regarding behavioral motives such as socially sanctioned cultural values, or identity concerns (Citrin et al. 1997; Hainmueller and Hopkins 2014; Sides and Citrin 2007). Empirically, anti-immigration sentiment translate into a form of sociotropic reasoning where immigration is assessed through its economic and cultural consequences for the country as a whole. The determinants of this latter form of reasoning are not yet well understood.

We examine whether education, through its effect on personal and professional social networks, can affect the likelihood of being socialized into worldviews resistant to such ethnocentric reasoning. Building on Kitschelt (1997), we identify the "liberal-cosmopolitan" pole of the cultural cleavage as being more favorable to pro-immigrant sentiment. Indeed, it provides a more flexible definition of national boundaries and of "who belongs." It also encourages a form of "positive" individualism that is less welcoming to nationalist frames in which the interest of the group—here, the nation—should overrule the interests of the individual (Graham, Haidt and Nosek 2009; Haidt 2012).

We might consequently expect that the effect of education on anti-immigration sentiment is part of a larger set of causal relationships, where an additional year of high school is found to affect other policy attitudes associated with liberal-cosmopolitanism such as support for gay rights, support for women's rights and support for EU integration. We examine this issue in detail using both the ESS and the two French datasets described in section A.8. Below, we list the items available in the different surveys:

- ESS: Please say to what extent you agree or disagree with each of the following statements: Gay men and lesbians should be free to live their own life as they wish [1/Agree strongly - 5/Disagree strongly] - Reversed -
- ESS: Now thinking about the European Union, some say European unification should go further. Others say it has already gone too far. Using this card, what number on the scale best describes your position? [0/too far - 10/go further] - Reversed -
- EIPP: Some support European integration, others oppose it. What is your position? [1/Totally supportive -4/Totally opposed]

- EIPP: Do you find:
  - ... homosexuality morally wrong? [1/Not wrong at all - 5/Very wrong]
  - ... cohabitation without being married morally wrong? [1/Not wrong at all - 5/Very wrong]
  - ... abortion morally wrong? [1/Not wrong at all - 5/Very wrong]
- EIPP: Do you agree/disagree that the death penalty should be reinstated? [1/Disagree strongly - 5/Agree strongly]
- EIPP: Dummy variable equal to 1 if an individual offers a "non-cosmopolitan" answer to items on homosexuality (= 1), abortion (= 1) and the death penalty (= 1).
- DREES: Do you mostly agree [1]/ mostly disagree [0] that:
  - ... gay and lesbians are no different from other people [reversed coded in Table A26]
  - ... it is normal for a woman to sacrifice a lot in order to succeed in her job
  - ... ideally women should stay home to raise the children
- DRESS: index variable equal to 1 if an individual offers a "non-cosmopolitan" answer to both the item on gay and lesbians (= 0) and the item on women needing to stay at home (= 1)

Using the same RD design as in the main paper, we find no evidence of a systematic effect of secondary education on any of these items. Results for the two items available in the ESS are available in column (3) and (4) of Table A24. The results for the EIPP items are in Table A25, while results for the DREES items are presented in Table A26. One exception is the European integration item. The estimate computed using the French EIPP data indicates no effect of the schooling reform on the attitudes of French respondents toward the EU. In contrast, the estimate

Table A25: The effect of compulsory education on cosmopolitan values in France, reduced form RD estimates (EIPP dataset)

	EU integration	Homosexuality	Cohabitation	Abortion	Death penalty	Non-cosmo index
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.020	0.006	0.003	0.004	-0.016	0.002
	(0.035)	(0.045)	(0.021)	(0.035)	(0.039)	(0.006)
Bandwidth	6	6	8	8	9	8
Observations	12,817	13,714	18,124	17,840	19,432	18,507
Outcome mean	2.02	2.98	3.73	3.13	2.26	0.03

Notes: Outcome variables are coded such that higher values indicate more (less) illiberal/authoritarian (liberal-cosmopolitan) answers. 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 $p < 0.001$.

computed using the pooled ESS data documents a positive effect on support for the EU. The French data was mostly collected during the "permissive consensus" era. Starting in the early 2000s, EU integration became politicized and following the Great Recession, it has now become a highly polarizing issue. In contrast to the French data, the ESS was collected during this period of politicization, potentially explaining the different results. Absent evidence supporting this interpretation (e.g. French data collected in the 2000s), we refrain from drawing strong conclusions and merely highlight the discrepancy between the two datasets.

Table A26: The effect of compulsory education on cosmopolitan values in France, reduced form RD estimates (DREES dataset)

	Gay no different (1)	Women sacrifice for work (2)	Women stay home (3)	Non-cosmo Index (4)
Reform	0.015	0.008	-0.009	-0.015
	(0.016)	(0.017)	(0.019)	(0.012)
Bandwidth	9	12	9	9
Observations	11,906	15,411	11,864	12,038
Outcome mean	0.82	0.36	0.31	0.09

Notes: Outcome variables are coded such that higher values indicate more (less) illiberal/authoritarian (liberal-cosmopolitan) answers. 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 $p < 0.001$.

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