Appendix for:
The Causal Effect of Education on Support for International Trade: Evidence from Compulsory Education Reforms

Omer Solodoch

A Reforms

In this section, I provide a brief description of the compulsory schooling reforms I use as an instrument for education attainment in eighteen countries. For each reform, the key coding decision is the pivotal birth cohort, i.e., the cohort first affected by the newly imposed reform. For most reforms, there is little disagreement in the literature about the pivotal cohort. In some cases, however, there is some degree of uncertainty and probable measurement error because respondents' birth year cohort is calculated based on the survey year minus their age. Also, some reforms first came into force for students born in the middle of a calendar year, meaning that even when the birth year is measured without error, respondents born in the pivotal cohort might still be untreated because they were born early in the same year. For example, in Taiwan's reform, those born on or after September 1, 1955 are the first to be affected, and in the UK, students born on or after April 1, 1933 were first affected by the 1947 reform. In such cases, some control units are coded as treated units and therefore the discontinuity at the cutoff is a conservative estimate for the effect of compulsory schooling reforms on educational attainment.

Table (A-1) Compulsory education reforms

Country	Reform year	Pivotal cohort	Change in years of compulsory schooling	Change in minimum school leaving age	Age at school entry
Austria	1966	1952	8–9	14–15	6
Comments: The 1962 School Amendment Act. Came into force on September 1, 1966. Sources: Mocan and Pogorelova (2017) and Hofmarcher (2021).					
Bulgaria	1959	1946	7–8	14-15	7
Comments: The 1959 Law on the Further Development of Public Education (Hofmarcher, 2021).					
Czech Republic	1948	1934	8–9	14-15	6
Czech Republic	1953	1939	9–8	15-14	6
Czech Republic	1960	1946	8–9	14-15	6
Comments: (1) Act No. 95/1948 (passed in April) on the Basic Regulation of Comprehensive Education; (2) Act No. 31/1953 on the Education System and Teacher Training. Treated cohorts (from 8 to 9 years of compulsory schooling) are 1934-38. (3) Act No. 186/1960 (passed in December) on the Education and Training System. Non-treated cohorts (8 years of compulsory schooling) are 1939-45. Source: Garrouste (2010).					
Denmark	1958	1944	7–8	14–15	7
Comments: The Executive Order on the Primary School Act, June 18, 1958. The reform required all municipalities to provide an 8th year of schooling, which disproportionately affected rural areas. Source: Cavaille and Marshall (2019)					
France	1959	1953	8–10	14–16	6
Comments: The *Berthoin* reform of January 6, 1959. The extension affected those who turned 6 after January 1, 1959. Source: Brunello, Fort, and Weber (2009) and Cavaille and Marshall (2019)					
Germany					
DE Schleswig-Holstein	1956	1942	8–9	14–15	6
DE Niedersachsen	1962	1948	8–9	14–15	6
DE Bremen	1958	1944	8–9	14–15	6
DE Nordrhein-Westphalia	1967	1954	8–9	14–15	6
DE Hessen	1967	1954	8–9	14–15	6
DE Rheinland-Pfalz	1967	1954	8–9	14–15	6
DE Baden-Wurtenberg	1967	1954	8–9	14–15	6
DE Bayern	1969	1956	8–9	14–15	6
DE Saarland	1964	1950	8–9	14–15	6
Comments: Ten states in West Germany extended compulsory education in different years from 1956-1969. Those reforms extended compulsory education from 8 to 9 years of schooling. In four states, which accounted for almost half of the German population, the reforms came into force in 1967. I use respondents' region of residence at the time of the survey as their region of residence as students. This approach certainly creates some degree of measurement error due to movers. In the ISSP data, however, the discontinuity at the cutoff is much clearer with the state-by-state coding compared to the approach of using 1954 as the cutoff year for all respondents. Source: Mocan and Pogorelova (2017) and Hofmarcher (2021).					
Hungary	1961	1947	8–10	14–16	6
Comments: Mocan and Pogorelova (2017) note that the extent to which the 1947 cohort was exposed to the compulsory education law is unclear. They therefore exclude the 1947 cohort from their analysis. In the ISSP data, however, the discontinuity at the cutoff is very clear for the 1947 cohort. Source: Mocan and Pogorelova (2017)					
Ireland	1972	1958	8–9	14–15	6
Comments: On April 12, 1972 a ministerial order modified the School Attendance Act of 1926 and extended compulsory schooling from 8 to 9 years. This order came into force on July 1, 1972. The first affected birth cohort is 1958. Source: Brunello, Fort, and Weber (2009).					
Japan	1947	1935	6–9	NA	6
Comments: Fundamental Law of Education, Law No. 25 of 1947. School Education Law, Law No. 26 of 1947. The first affected birth cohort, 1935, has turned 12 during the course of 1947. Source: Kemble (2004) and Kawaguchi (2011).					
Latvia	1970	1955	8-11	15-18	7
Comments: the Soviet Union extended compulsory education from 8 to 10 years starting in 1970, but in Latvia the extension was from 8 to 11 years of schooling, because of differences in the structure of upper secondary education. Source: Hofmarcher (2021).					
Netherlands	1975	1959	9-10	16-17	6
Comments: The reform extended compulsory education in the vocational track from three to four years of schooling. Source: Brunello, Fort, and Weber (2009) and Cavaille and Marshall (2019).					

Table (A-1): Compulsory education reforms, continued

Country	Reform year	Pivotal cohort	Change in years of compulsory schooling	Change in minimum school leaving age	Age at school entry
Portugal	1964	1957	4–6	12–14	8
Comments: The 1964 reform extended compulsory schooling from 4 to 6 years. The reform applied to those who entered school from 1964, i.e., mostly individuals born in 1957, although some of those born in 1956 were also included in the reform. I follow Mocan and Pogorelova (2017) in defining 1957 as the pivotal cohort. Source: Mocan and Pogorelova (2017).					
Slovakia	1948	1934	8-9	14–15	6
Comments: Act No. 95/1948 on the Basic Regulation of Comprehensive Education; Source: Hofmarcher (2021).					
Slovenia	1951-1952	1937	7–8	14–15	7
Comments: Hofmarcher (2021) notes that scholars disagree about the year (1951/1952) when compulsory schooling was extended. The first-stage results are consistent with the claim that the law passed in 1951 and the first affected cohort is 1937.					
Spain	1970	1957	6–8	12–14	6
Comments: The General Act on Education and Financing of the Educational Reform (Law 14/1970 of August 4, 1970). Source: Brunello, Fort, and Weber (2009).					
Sweden	1949–1962	1952	7–9	14–16	7
From 1949 to 1962, Swedish municipalities gradually extended compulsory education to 9 years. I follow Mocan and Pogorelova (2017) who define those born in 1952 or later as the treatment group.					
Taiwan	1968	1956	6–9	12–15	6
In September 1, 1968, Taiwan introduced a 9 year compulsory schooling law. The law required students born on or after 1 September 1955 to attend school longer than their older counterparts. I code the 1956 cohort as the pivotal cohort because it is the earliest *fully* affected birth cohort. Source: Zhang (2018).					
United Kingdom I	1947	1933	9–10	14–15	5
Source: Cavaille and Marshall (2019).					
United Kingdom II					
England, Wales and N. Ireland	1972	1958	10–11	15–16	5
Scotland	1976	1959	10–11	15–16	5
Comments: I code the 1934 cohort as the pivotal cohort because it is the earliest *fully* affected birth cohort. The 1933 cohort is only partially affected (students born on or after April 1, 1933). Source: Mocan and Pogorelova (2017) and Hofmarcher (2021).					

B Description of V-Indoc Measures

The quality (validity and reliability) of V-Indoc measures and the V-Dem methodology has been thoroughly assessed by recent literature. Below, I briefly describe (1) several important features in the V-Indoc's data generation process; and (2) the validity and reliability of V-Indoc measures.

Reliability. V-Indoc was created using the Varieties of Democracy (V-Dem) infrastructure for the data collection and processed the surveys of 760 country experts using the standard V-Dem measurement modeling and quality control processes. The recruitment of experts is based on the following selection criteria: validated expertise, local, in-depth knowledge, willingness to devote time to the project, impartiality, and diversity in professional background.

The V-Dem team has developed a Bayesian Item-Response Theory (IRT) estimation strategy that accounts for concerns regarding potential problems with expert-coded data. More specifically, since V-Indoc relies on multiple expert per country, they are able to account for the possibility that experts have different thresholds for their ratings ("Differential Item Functioning"—the concern that one expert's "some-what" may be another expert's "weakly") and their reliability ("discrimination parameter"—the degree to which they agree with other experts). This comprehensive approach generates high-quality measures, minimizes bias, and provides transparent information.

Validity. Neundorf et al. (2024) demonstrate that V-Indoc measures exhibit high levels of validity in the following terms:

(1) Face validity: V-Indoc measures conform to existing expectations about levels of indoctrination both across countries and over time within countries (for example, democracies generally possess higher (lower) levels of democratic (patriotic) indoctrination content than other types of regimes).

(2) Convergent validity: V-Indoc measures share empirical associations with other measures of the same concept (for example, V-Indoc's measure of Patriotic education in the curriculum is correlated (r=0.6) with Bromley, Meyer, and Ramirez (2011)'s measure of nationalism in textbooks).

(3) Construct validity: V-Indoc measures behave as theoretically expected when used in evaluations of hypotheses that involve the concept (for example, an analysis based on the V-Indoc measures corroborates a prominent argument in the literature, suggesting that military regimes should be less likely to engage in indoctrination relative to other types of autocratic regimes).

V-Indoc measures used in the paper:

Patriotic education in the curriculum. Country experts were asked: "How often does the language curriculum promote patriotism?" The survey clarified that patriotism means feelings of love, pride, loyalty and commitment to one's country, and provided examples (e.g., promoting patriotism can take the form of teaching narratives that celebrate the country's national origin stories). Possible responses were: 0. rarely or never; 1. sometimes; 2. often; and 3. extensively.

Pluralism in the curriculum. Experts were asked: "When historical events are taught, to what extent are students exposed to diverse views and/or interpretations of these events?" The survey clarified that the question refers to de-facto subject content and to how much space is given to alternative viewpoints in teaching historical events such as international conflicts. Response categories were located on the same 0-3 ordinal scale.

Nationalist ideology character in the curriculum. Experts were asked to select up to two out of ten options based on the following question: "How would you characterize the dominant societal model(s) or ideology(ies) promoted through the history curriculum?" The original, binary variable is equal to 1 for nationalist history curricula, and 0 otherwise (socialist/communist; restorative/conservative; democratic norms; democratic institutions; personality cult; religious; ethnicity/clan/tribe; other; no specific societal model or ideology).

Mathematics and science education. Experts were asked: "What proportion of instructional weekly hours is dedicated to mathematics and natural sciences in primary education? please approximate the proportion of instructional hours across grades of primary education." Possible responses were: 0. either a small proportion (less than 25%) or a large proportion (about 25% or more). The survey clarified that mathematics includes arithmetic, geometry, algebra, calculus, and that natural sciences include chemistry, biology, physics, as well as classes in computing and engineering.

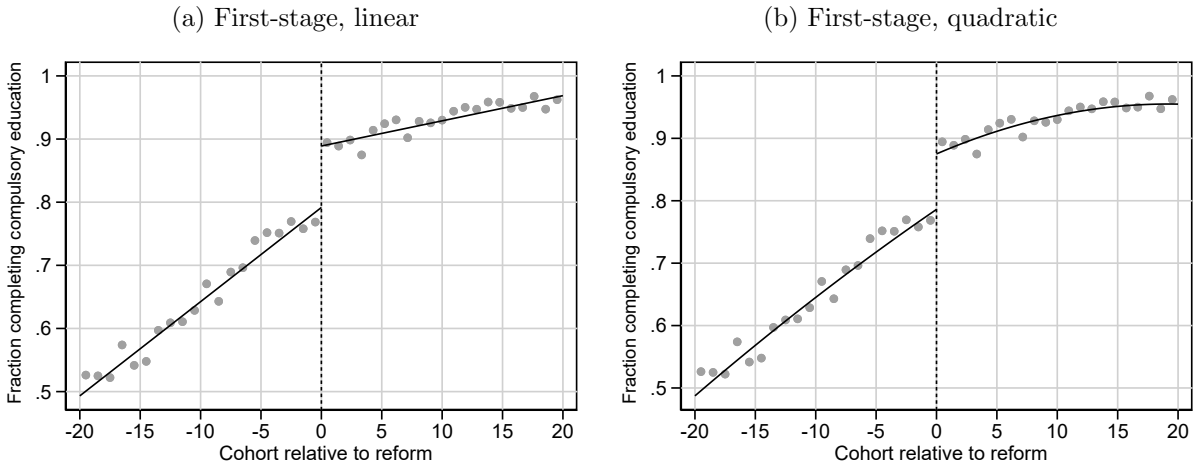
Average harmonized learning outcome score (ana_lo_score). Average learning outcomes from standardized, psychometrically-robust international and regional achievement tests taken from (Altinok, Angrist, and Patrinos, 2018). In order to maximize coverage by country, tests have been harmonized and pooled across subjects (math, reading, science) and levels (primary and secondary education). This variable has two important limitations. First the data are available from 1970-2015, while most compulsory schooling reforms were implemented before the 1970s. Therefore, I cannot include this variable in the fuzzy RD design I use throughout the paper. Instead, I include this variable only in a robustness test using linear probability models. Second, the data are in 5 year intervals. To deal with this limitation, I assign the missing subsequent years (for example, 1971-1974) with the most recent learning outcome score available in the data (for example, 1970).

Table (A-2) Summary statistics for cohort-level V-Indoc measures

Country	Measure	Mean	SD	Min	Max
Austria	Pluralism	0.534	0.202	-0.08	0.633
	Patriotism	-0.583	0.000	-0.583	-0.583
	Nationalism	-0.444	0.641	-0.874	0.8
	Math & Science	-0.601	0.000	-0.601	-0.601
Bulgaria	Pluralism	-0.002	0.258	-0.106	1.034
	Patriotism	0.543	0.106	0.277	0.751
	Nationalism	-0.367	0.128	-0.874	-0.315
	Math & Science	-0.616	0.132	-1.139	-0.563
Czechia	Pluralism	-1.416	0.868	-1.942	0.595
	Patriotism	0.774	0.432	-0.186	1.06
	Nationalism	-0.601	0.131	-0.874	-0.223
	Math & Science	-0.24	0.025	-0.387	-0.233
Germany	Pluralism	0.629	0.127	0.428	0.809
	Patriotism	-1.945	0.000	-1.945	-1.945
	Nationalism	-0.874	0.000	-0.874	-0.874
	Math & Science	-0.415	0.265	-0.635	0.007
Denmark	Pluralism	1.202	0.099	1.067	1.401
	Patriotism	-1.553	0.342	-1.882	-0.732
	Nationalism	-0.789	0.166	-0.874	-0.315
	Math & Science	-0.938	0.000	-0.938	-0.938
Spain	Pluralism	-0.125	0.689	-0.983	0.909
	Patriotism	0.097	0.97	-1.474	1.212
	Nationalism	0.891	0.966	-0.539	1.995
	Math & Science	-0.82	0.163	-0.911	-0.244
France	Pluralism	0.716	0.521	0.149	1.582
	Patriotism	0.61	0.393	-0.022	0.992
	Nationalism	0.423	0.376	-0.037	0.8
	Math & Science	0.749	0.716	-0.127	1.467
Hungary	Pluralism	-1.403	0.805	-1.811	0.747
	Patriotism	0.049	0.03	0.036	0.193
	Nationalism	-0.797	0.127	-0.874	-0.363
	Math & Science	-0.531	0.048	-0.542	-0.211
Japan	Pluralism	-0.278	0.000	-0.278	-0.278
	Patriotism	-0.579	0.059	-0.765	-0.554
	Nationalism	0.206	0.083	-0.065	0.241
	Math & Science	1.599	0.000	1.599	1.599
Latvia	Pluralism	0.915	0.082	0.752	0.976
	Patriotism	1.421	0.066	1.335	1.513
	Nationalism	-0.037	0.000	-0.037	-0.037
	Math & Science	0.14	0.256	-0.071	0.577
Netherlands	Pluralism	0.684	0.009	0.683	0.789
	Patriotism	-1.559	0.478	-2.083	-0.985
	Nationalism	-0.22	0.053	-0.483	-0.204
	Math & Science	-0.97	0.079	-1.368	-0.944
Portugal	Pluralism	0.083	0.651	-0.81	0.912
	Patriotism	0.403	1.055	-0.679	1.788
	Nationalism	0.44	1.354	-0.874	2.139
	Math & Science	1.086	0.524	0.505	1.7
Sweden	Pluralism	0.824	0.14	0.531	1.135
	Patriotism	-1.061	0.152	-1.171	-0.763
	Nationalism	-0.874	0.000	-0.874	-0.874
	Math & Science	-0.312	0.167	-0.372	0.323
Slovenia	Pluralism	0.214	0.14	-0.04	0.41
	Patriotism	-0.275	0.033	-0.295	-0.204
	Nationalism	-0.075	0.054	-0.176	-0.037
	Math & Science	1.527	0.000	1.527	1.527
Slovakia	Pluralism	-0.3	0.112	-0.464	-0.15
	Patriotism	0.61	0.131	0.43	0.82
	Nationalism	-0.575	0.269	-0.874	-0.156
	Math & Science	1.567	0.000	1.567	1.567
Taiwan	Pluralism	-0.813	1.025	-1.505	1.211
	Patriotism	0.942	0.000	0.942	0.942
	Nationalism	1.62	1.263	-0.874	2.473
	Math & Science	1.394	0.000	1.394	1.394
United Kingdom	Pluralism	-0.036	0.378	-0.459	0.951
	Patriotism	-0.372	0.135	-0.707	-0.161
	Nationalism	-0.049	0.029	-0.174	-0.037
	Math & Science	-1.301	0.35	-1.533	0.096
Full sample	Pluralism	-0.016	0.997	-1.942	1.582
	Patriotism	-0.238	1.043	-2.083	1.788
	Nationalism	-0.042	0.977	-0.874	2.473
	Math & Science	-0.05	0.972	-1.533	1.7

C Additional First-Stage Results

Figure (A-1) First stage results by order of the global polynomial regression function



Note: Proportion completing compulsory education by birth year cohort. Curves are linear (left panel) or quadratic polynomials (right panel) either side of the cutoff.

In Figure 1, I plot the first-stage results using a third-order polynomial regression function of the forcing variable. In Figure A-1, I show that the discontinuity at the cutoff remains very clear using either linear (left panel) or second-order polynomials (right panel).

Figure A-2 shows the probability of completing compulsory education (i.e., the number of years of schooling students are required to complete by law following the reform) among cohorts around each of the reforms.[1] As the Figure shows, there is a discontinuity at the cutoff in all cases, although some cases exhibit a larger jump in the probability of completing compulsory years of schooling following the reforms.

[1]Since the 1948 reform in the Czech Republic (from 8 to 9 years of compulsory schooling) was quickly cancelled in 1953 and then re-introduced in 1960, I treat both reforms as one, where birth cohorts from 1939-1945 are coded as pre-treatment reform together with the pre-1935 cohorts.

Figure (A-2) First-Stage by Reform

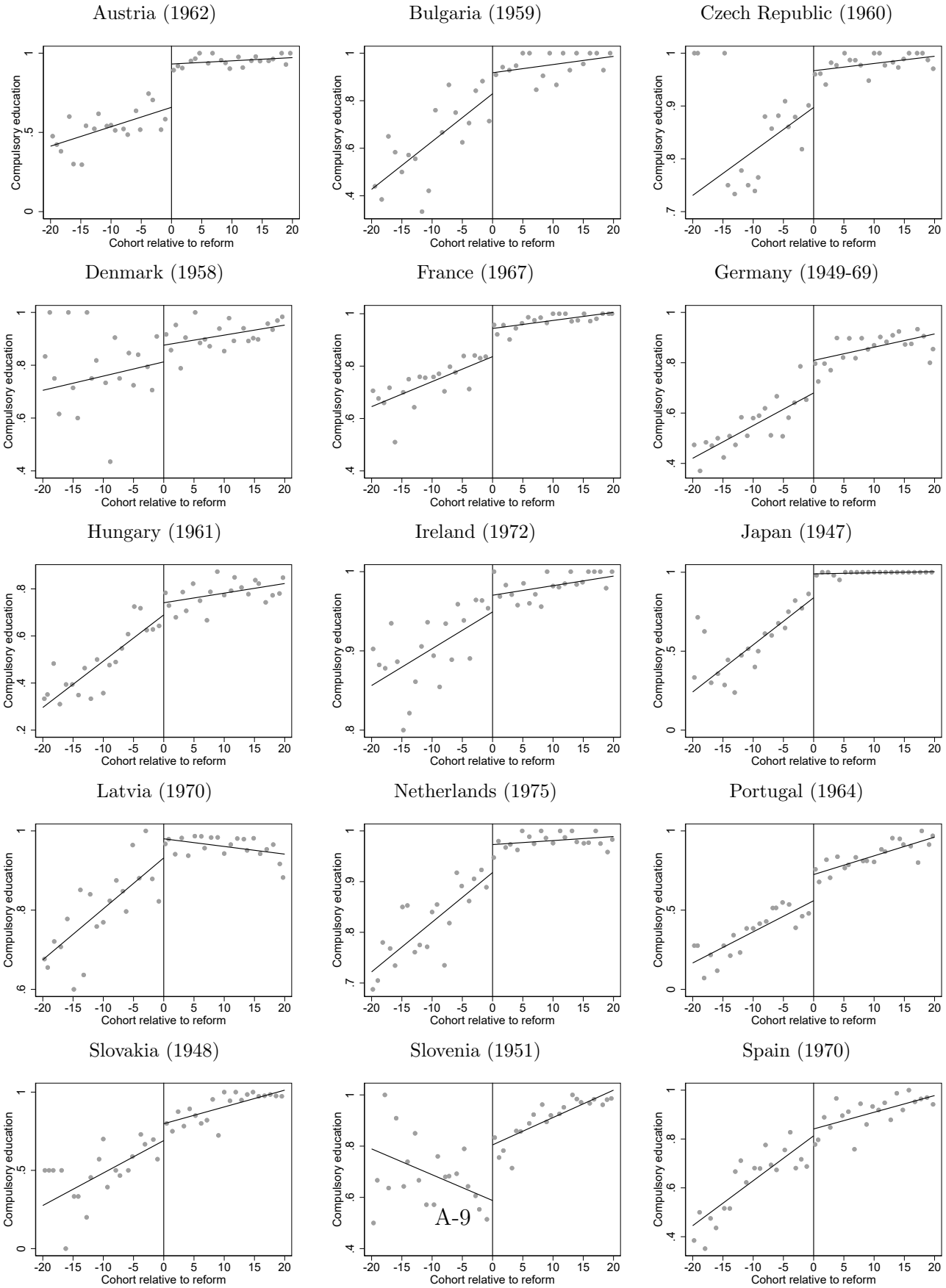
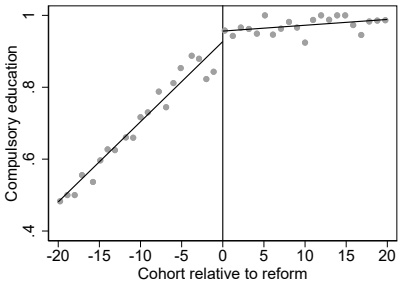
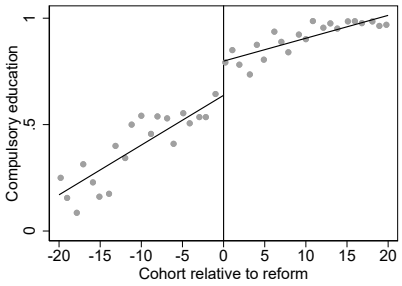


Figure (A-3) Figure A-2, continued

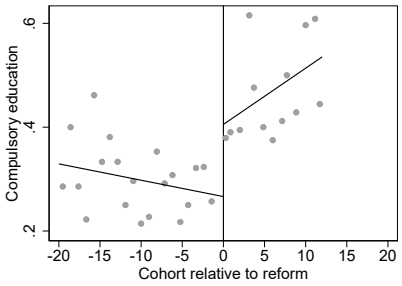
Sweden (1962)



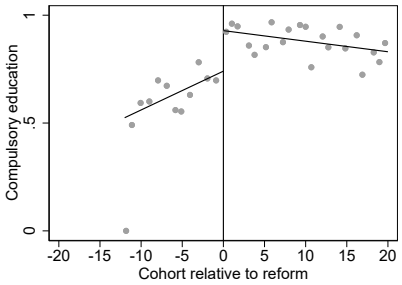
Taiwan (1968)



United Kingdom (1947)



United Kingdom (1972)



Note: Fraction of respondents completing compulsory schooling required by the reform, first-order polynomials either side of the reform.

As shown in Table A-1, nine reforms (45%) raised compulsory schooling to 9 years; four reforms (20%) raised compulsory schooling to 8 years; another 20% raised it to 10; and two reforms (10%) raised it to 11. Pooling across reforms, Table A-3 shows that the reforms significantly increased secondary education. The largest increases are concentrated between the 8th and 11th years of formal schooling, i.e., at those levels of schooling required by the reforms. Conversely, the reforms did not increase tertiary education.

Table (A-3) The effect of compulsory education reforms on level of completed schooling

	(1) 7 years	(2) 8 years	(3) 9 years	(4) 10 years	(5) 11 years	(6) 12 years	(7) Tertiary
Reform	0.041**	0.050**	0.092**	0.083**	0.043*	0.022	0.004
	(0.013)	(0.014)	(0.017)	(0.020)	(0.017)	(0.018)	(0.012)
Covariates	✓	✓	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓	✓	✓
Bandwidth	9	7	8	6	11	14	11
Observations	16569	13287	14947	11553	19708	24308	19593

Note: Entries are local linear regression estimates with a triangular kernel using the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. Pre-treatment covariates include respondents' sex, ethnicity, and parents' immigrant status. Robust standard errors in parentheses, clustered by subnational region. * $p < 0.05$, ** $p < 0.01$.

Table A-4 shows that the first-stage effect remains significant across four different measures of formal education. First, the reforms increased the probability of completing the country-specific years of schooling—as eventually required by the compulsory education law—by 10 percentage points. Column 2 shows that the reforms increased years of schooling by 0.36 years when measuring schooling as the number of completed years of education up to a limit of 12 years. A very similar result is produced by using the schooling-13 measure, which has a limit of 13 years. Finally, a slightly smaller but substantively similar estimate is presented in column 4, where the maximum number of years of schooling is 22 (only 1 percent of the sample completed at least 22 years of schooling).

Table (A-4) The effect of compulsory education reforms on probability of completing compulsory schooling and years of schooling

	(1) Compulsory	(2) Schooling (12)	(3) Scooling (13)	(4) Schooling (22)
Reform	0.109^{**}	0.366^{**}	0.382^{**}	0.305^{+}
	(0.016)	(0.101)	(0.117)	(0.167)
Covariates	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Bandwidth	8	8	8	9
Observations	14947	14947	14947	16569

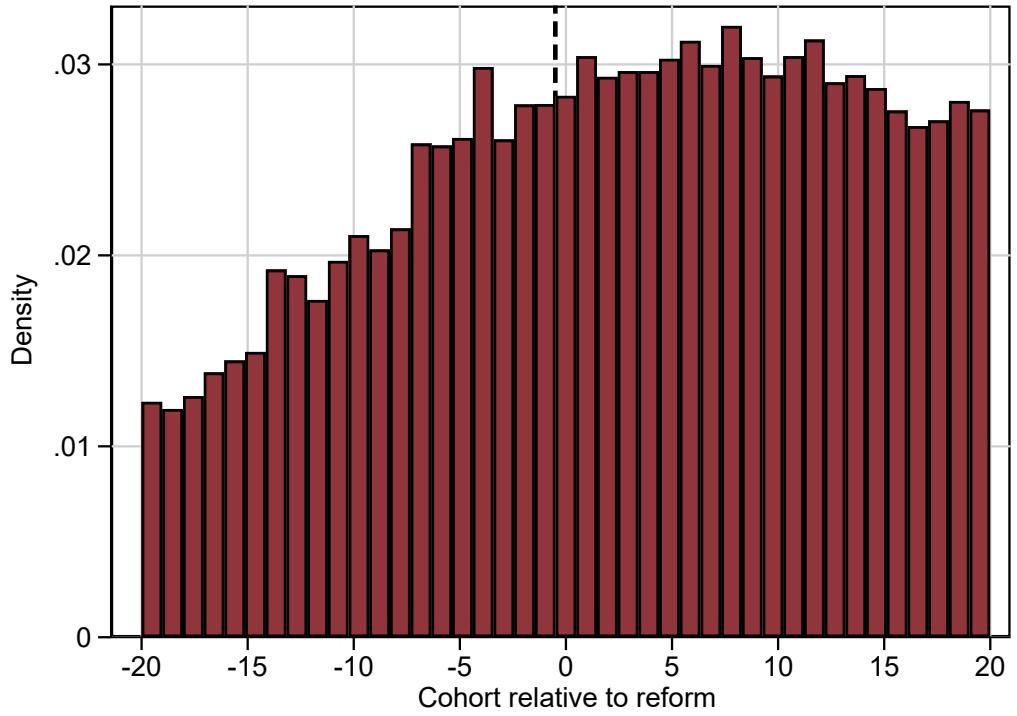
Note: Entries are local linear regression estimates with a triangular kernel using the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. Pre-treatment covariates include respondents' sex, ethnicity, and parents' immigrant status. Robust standard errors in parentheses, clustered by subnational region. $\dagger p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

D Falsification and Robustness Tests

D.1 Sorting Test

Since respondents have no control over their birthyear and their parents could not have anticipated the timing of the compulsory schooling reforms, selection into cohorts is highly implausible. Nevertheless, in Figure A-4, I conduct a sorting test that examines whether there is an unusually large proportion of respondents being born immediately following the year when the reforms came into force by computing the density of the birth cohort running variable. The smooth density across the threshold suggests that there is no evidence for sorting of respondents around the threshold as expected given that respondents' parents could not have precise control to manipulate the birth year of their children relative to reform.

Figure (A-4) Sorting test

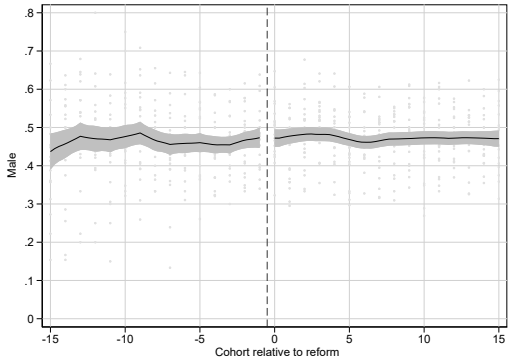


D.2 Balance Test

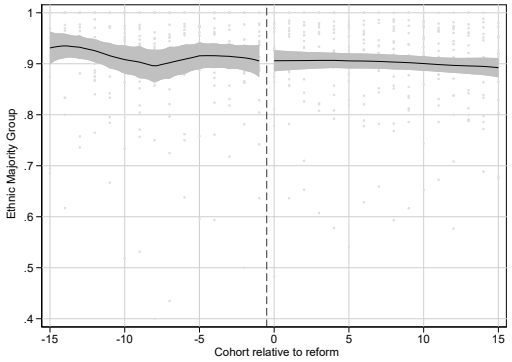
Using local polynomial smooth plots, panels a-c in Figure A-5 show that respondents' pre-treatment demographic characteristics are essentially continuous through the discontinuity. Notably, locality type (panel d) is a post-treatment variable, because higher levels of education due to the reforms can potentially affect people's choices about where to live later in life. Nevertheless, control and treated cohorts also exhibit a similar share of respondents living in big cities. Overall, the balance test confirms that there is no significant change in the gender, ethnic or urban composition of the sample following the compulsory education reforms, which is consistent with the assumption that the reforms constitute an exogenous source of variation in education.

Figure (A-5) Balance Test

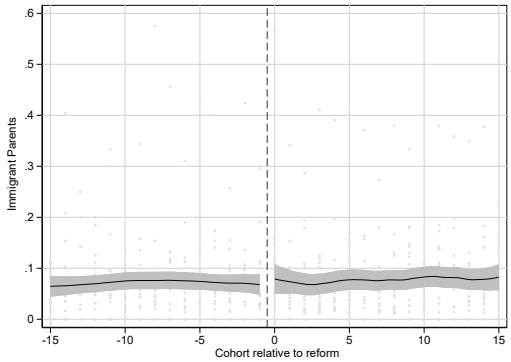
(a) Male



(b) Ethnic majority group



(c) Immigrant parents



(d) Big city

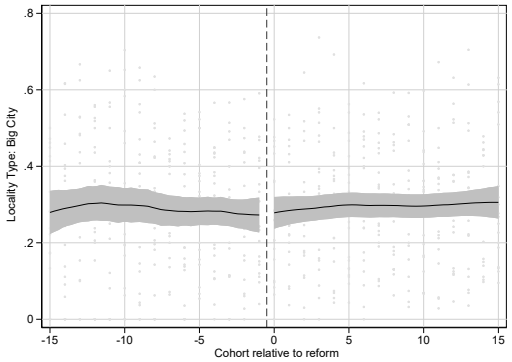
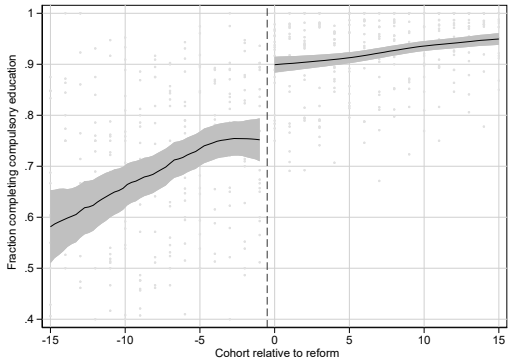


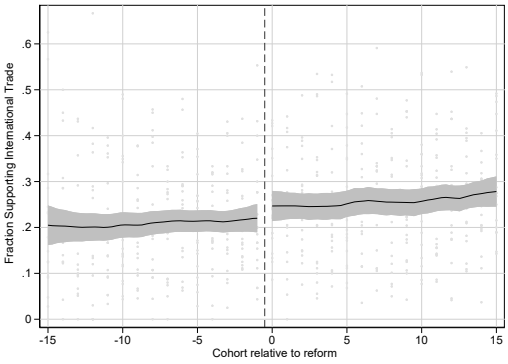
Figure A-6 presents the first-stage and reduced form RD results using the same local polynomial smooth plots. Overall, the analysis demonstrates that even though control and treated respondents are similar across pre-treatment characteristics, the level of education is significantly larger in post-reform cohorts, and support for trade liberalization is discontinuous at the threshold even before accounting for non-compliance using the reforms as an IV for schooling.

Figure (A-6) First Stage and Reduced Form

(a) First-stage: compulsory education



(b) Reduced-form: pro-trade



D.3 Placebo Cutoffs

Throughout the analysis, the cutoff point is determined by the first birth cohort included in the compulsory education reform. In Table A-5, I conduct a placebo cutoff test where I replace the pivotal cohort with either younger or older cohorts by either 5 or 10 years. Using a 5-year bandwith either side of the cutoff means that only the real cutoff point in column 3 divides the sample by exposure to the compulsory education treatment. All other columns use artificial cutoff values at which the probability of treatment assignment does not change. They compare slightly older pre-reform cohorts to slightly younger pre-reform cohorts, as in columns 1-2, or slightly older post-reform cohorts to slightly younger post-reform cohorts, as in columns 4-5. Consistent with the claim that the the reform-induced education affected public support for international trade, the results show that the effect can be found only when using the real cutoff point.

Table (A-5) Placebo Cutoff Tests

Cutoff:	(1) Reform_{t-10}	(2) Reform_{t-5}	(3) Reform_{t0}	(4) Reform_{t+5}	(5) Reform_{t+10}
(a) ITT					
Reform	0.014	0.002	0.034**	0.001	0.007
p-value	[0.134]	[0.846]	[0.000]	[0.930]	[0.530]
(b) LATE					
Reform	0.538	0.081	0.241**	0.011	0.075
p-value	[0.129]	[0.806]	[0.000]	[0.912]	[0.497]
Observations	10732	10680	10155	9020	7366

Note: Entries are ITT and LATE estimates using the local-randomization fuzzy RD approach with ±5 birth cohort years around each cutoff. The dependent variable is the pro-trade dummy variable. The treatment is a dummy variable for completing the number of schooling years that are considered as compulsory education by the new reforms. * $p < 0.05$, ** $p < 0.01$.

D.4 Local Quadratic Regressions

In Table 2, I present the main results using local linear regression estimates and the binary dependent variable of support for international trade. In Table A-6 I show that the results remain similar using local quadratic polynomials instead, with either the pro-trade dummy variable or the original 5-point scale of support for trade.

Table (A-6) Effect of Secondary Education on Support for International Trade using Local Quadratic Polynomials

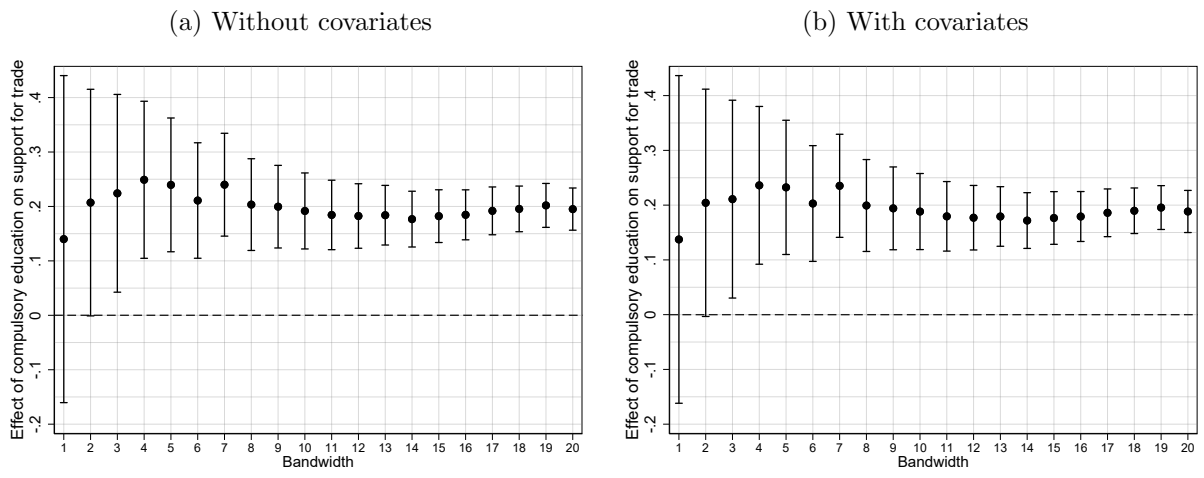
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimate:	First-stage	First-stage	ITT	LATE	LATE	ITT	LATE	LATE
DV:	Compulsory education	Schooling	Pro-trade (1-5)			Pro-trade (binary)		
Reform	0.105**	0.407**	0.085+			0.035*		
	(0.017)	(0.122)	(0.044)			(0.017)		
Compulsory education				0.803+			0.332*	
				(0.410)			(0.158)	
Schooling					0.208*			0.086*
					(0.103)			(0.040)
Covariates	yes	yes	yes	yes	yes			
Country-year FE	yes	yes	yes	yes	yes			
Observations	27176	27176	27176	27176	27176	27176	27176	27176
First stage F-statistic	280	179		280	179		280	179

Note: Entries are local quadratic regression estimates with a triangular kernel and the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth (±17 birth cohort years) and standard errors in parentheses clustered by subnational region. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

D.5 Bandwidth Selection and Model Specification

Throughout the paper, I use optimal bandwidths as recommended by Cattaneo, Idrobo, and Titiunik (2023a). In Figure A-7, I examine the extent to which the treatment effect is sensitive to alternative bandwidths. The results show that the point estimates are very stable across bandwidths, and, despite some loss of precision, their precision remains high even at the smaller bandwidths with fewer observations than the optimal bandwidth of ±11 birth cohorts. The only bandwidth in which the effect is imprecisely estimated is the smallest one, ±1. Yet despite the much smaller sample size (N=2,671) and the more severe measurement error due to imprecise information about respondents' exact date of birth, the effect estimate remains substantively large (15 percentage points), and statistically indistinguishable from the next estimate using the subsequent bandwidth of ±2 birth cohorts.

Figure (A-7) Compulsory Schooling Effect by Bandwidth



Note: IV estimates for the effect of completing compulsory education on support for international trade. Bandwidth is number of birth year cohorts either side of the cutoff.

D.6 Alternative Measures of Schooling

Table A-7 shows that the effect of education on support for international trade is robust to using a different coding of the schooling treatment. Schooling significantly increases pro-trade preferences whether schooling is measured as a binary indicator of completing the years of schooling required by each of the reforms, or a count of completed years of schooling with an upper bound of 12, 13, or 22 years.

Table (A-7) Effect of compulsory schooling on support for trade, alternative measures of schooling, continuity-based RD framework

	(1)	(2)	(3)	(4)
Schooling	0.320^{*}	0.083^{*}	0.087^{*}	0.125^{+}
	(0.135)	(0.041)	(0.042)	(0.076)
Covariates	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Treatment	Compulsory	Schooling (12)	Scooling (13)	Schooling (22)
Bandwidth	12	9	9	10
Observations	20409	15936	15936	17454

Note: Entries are local linear regression estimates with a triangular kernel using the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. Pre-treatment covariates include respondents' sex, ethnicity, and parents' immigrant status. Robust standard errors in parentheses, clustered by subnational region. $\dagger p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

Table A-8 presents IV estimation results using a local randomization RD approach (focusing on the effect of schooling not at the cutoff but rather in a neighborhood around the cutoff). Again, the effect of schooling is robust to using alternative measures of schooling.

Table (A-8) Effect of compulsory schooling on support for trade, alternative measures of schooling, local randomization RD framework

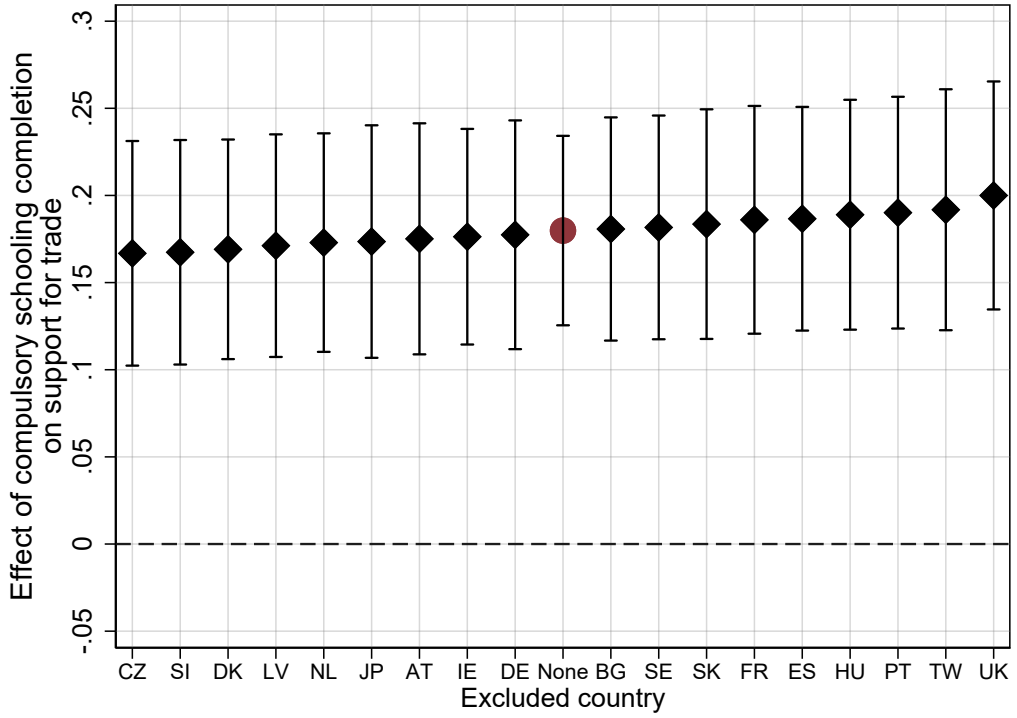
	(1)	(2)	(3)	(4)
Schooling	0.176^{**}	0.038^{**}	0.035^{**}	0.028^{**}
	(0.035)	(0.008)	(0.007)	(0.005)
Covariates	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Treatment	Compulsory	Schooling (12)	Scooling (13)	Schooling (22)
Bandwidth	11	11	11	11
Observations	18947	18947	18947	18947
First stage F-stat	283	185	211	199

Note: Entries are local linear regression estimates with a triangular kernel using the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. Pre-treatment covariates include respondents' sex, ethnicity, and parents' immigrant status. Robust standard errors in parentheses, clustered by subnational region. $\dagger p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

D.7 Country-by-Country Exclusion

Figure A-8 reports the local average treatment effect of completing compulsory schooling on support for trade using the pooled sample (when none of the countries is removed) and when each country is separately removed from the sample. The effect remains stable, substantial and statistically significant across all samples, demonstrating that the results are robust to removing any particular country from the analysis.

Figure (A-8) Country-by-country exclusion

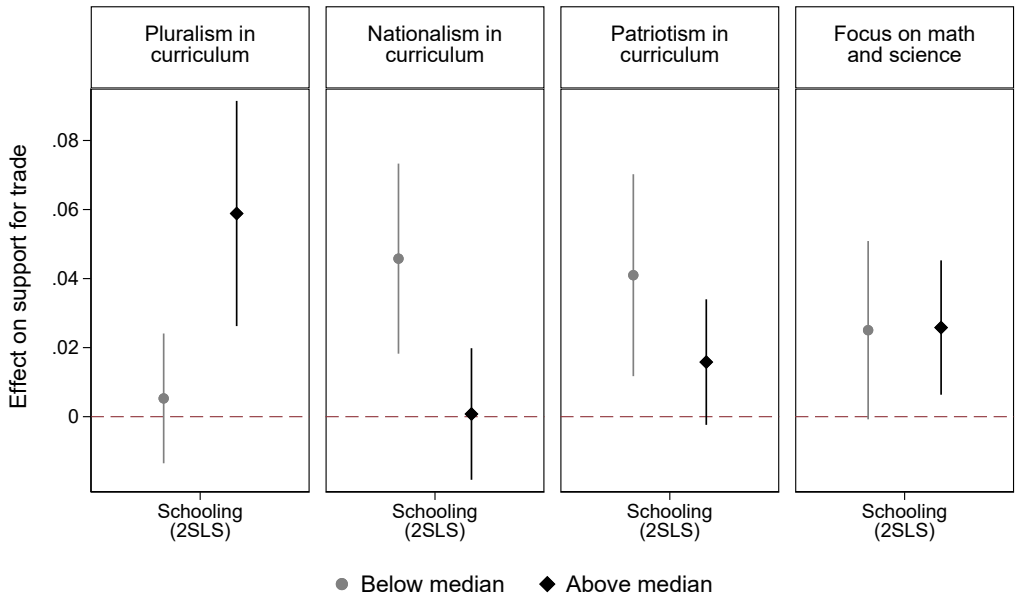


Note: Entries are LATE estimates using the Cattaneo, Idrobo, and Titiunik (2023b) optimal bandwidth (±11 birth cohort years) with robust standard errors and 95% confidence intervals.

D.8 Effect of Education by the Content of School Curricula

Figure 4 in the main text divides the sample by the median value of each of the four school curricula measures and shows that the effect of education is concentrated in cohorts exposed to higher levels of pluralism and to lower levels of nationalism in the curriculum. Figure A-9 adds an additional split-sample analysis, using "focus on math and science education". In contrast to the significant interaction between pluralism or nationalism and schooling, the effect of schooling is statistically and substantively similar across different degrees of emphasis given to math and science education.

Figure (A-9) Effect of years of schooling by the content of education, including focus on math and science

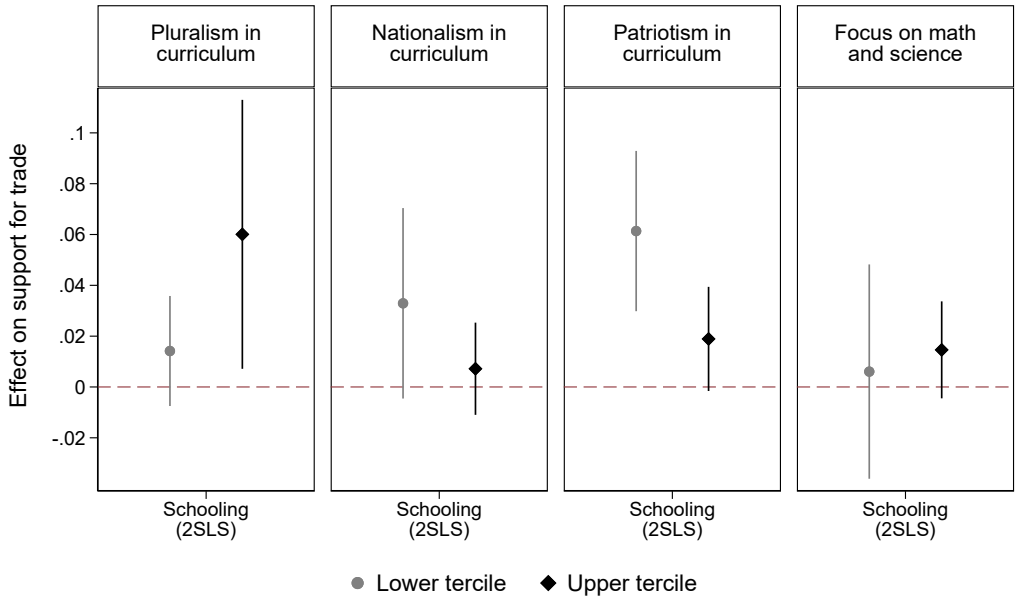


Note: IV estimates with 95% confidence intervals for the effect of completing compulsory education on support for international trade using the optimal bandwidth based on a local randomization approach (±11 birth cohort years).

At the cost of some loss of precision, Figure A-10 shows that the results are robust to using the lower and upper terciles instead below/above median values of each of the four school curricula measures.

Figure A-11 shows that the results are also robust to using binary indicator for completing compulsory schooling instead of the years of schooling measure. Specifically, completing compulsory education increases support for trade by 26 percentage points ($p < 0.001$) for respondents from cohorts exposed to higher levels of pluralism in education, while compulsory education has no effect among those respondents exposed to lower levels of pluralism. Completing compulsory education for respondents from cohorts exposed to lower levels of nationalism as a dominant societal model in the school curriculum increases support for

Figure (A-10) Effect of schooling years by the content of education, lower vs. upper tercile



Note: OLS estimates for the effect of completing compulsory education on support for international trade using the optimal bandwidth (±11 birth cohort years).

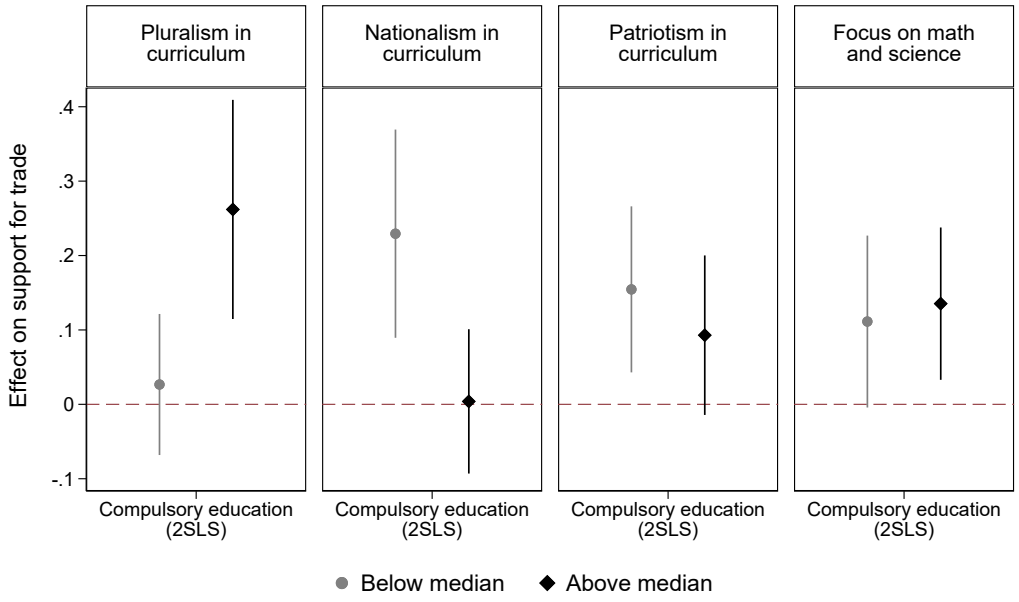
trade by 22 percentage points ($p < 0.001$), whereas compulsory education completion has no effect on individuals from cohorts exposed to higher levels of nationalism. The difference in the effect of education under exposure to low vs. high levels of patriotism is very small and becomes statistically indistinguishable from zero. But this is overall consistent with the main results in Figure 4 showing that this difference is quite small using the years of schooling measure.

Instead of splitting the sample by the median value of school curricula measures, Table A-9 presents linear probability model (LPM) and IV estimates for the interaction effect of schooling and school curricula on public support for trade openness. Here, I standardize both the schooling treatment and the school curricula moderators to have a mean of 0 and a standard deviation (SD) of 1.[2] The linear term for the schooling treatment remains positive, fairly stable, and statistically significant across all model specifications.

Across both the LPM and IV models, and consistent with the split-sample analysis, I find that the effect of schooling is larger when school curricula are more pluralistic and less patriotic, but does not change as a function of the focus given to skill-building subjects such as mathematics and science in school curricula. The IV estimates are somewhat larger but overall consistent with the LPM estimates. More specifically, as shown in column 6, a one SD increase in schooling heightens support for trade by 20 (7) percentage points when

[2]The standard deviation of *Schooling* is 2.6 years.

Figure (A-11) Effect of compulsory schooling by the content of education



Note: IV estimates for the effect of completing compulsory education on support for international trade using the optimal bandwidth (±11 birth cohort years).

school curricula are one SD more (less) pluralistic than the sample mean; in column 7, a one SD increase in schooling raises support for trade by 4 (14) percentage points when school curricula are one SD more (less) patriotic than the sample mean.

In contrast, as shown in column 8, a 1-SD increase in schooling heightens support for trade by 9 percentage points regardless of the extent to which school curricula focus on teaching mathematics and science. In other words, a more intense focus on subjects related to the development of labor-market skills does little to change the pro-trade effect of schooling.

Table (A-9) Effect of schooling years and cohort-specific content of education on support for trade

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) IV	(6) IV	(7) IV	(8) IV
Schooling	0.086**	0.097**	0.091**	0.089**	0.089**	0.137**	0.088**	0.093*
	(0.005)	(0.005)	(0.004)	(0.004)	(0.033)	(0.047)	(0.033)	(0.040)
Pluralism in the curriculum	-0.046	-0.041	-0.004	-0.052	-0.047	-0.021	0.038	-0.047
	(0.032)	(0.031)	(0.031)	(0.031)	(0.034)	(0.034)	(0.047)	(0.034)
Patriotism in the curriculum	-0.007	-0.021	-0.016	-0.024	-0.006	0.015	0.017	-0.002
	(0.023)	(0.024)	(0.024)	(0.024)	(0.025)	(0.028)	(0.028)	(0.030)
Math and science education	-0.004	0.007	0.028	-0.020	-0.003	0.071	0.083	-0.001
	(0.032)	(0.033)	(0.033)	(0.032)	(0.033)	(0.049)	(0.051)	(0.034)
Schooling X Pluralism in the curriculum		0.026**				0.064*		
		(0.004)				(0.029)		
Schooling X Patriotism in the curriculum			-0.031**				-0.048*	
			(0.004)				(0.022)	
Schooling X Math and science education				-0.009*				-0.008
				(0.004)				(0.031)
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	14509	14509	14509	14509	14509	14509	14509	14509
R-squared	0.100	0.103	0.105	0.101	0.100	0.095	0.104	0.101
First-stage F-statistic					215	45	107	47

Note: Entries are LPM and IV estimates with robust standard errors in parentheses. All independent variables presented in the table are standardized to have a mean of zero and a standard deviation of one. The sample is restricted to include a bandwidth of 11 birth cohorts on either side of the compulsory education reform. In the IV specifications (cols. 6–8), both schooling and the interaction of schooling with the school curricula measure are instrumented by respondents' birth cohort relative to the reform and the interaction of relative birth cohort with school curricula, respectively. * $p < 0.05$, ** $p < 0.01$.

One concern regarding this measure of relative time devoted to learning math and science is that it might not correspond with actual skill acquisition and human capital building. The curriculum might attempt to build such skills by devoting more time to these subjects but fail to do so effectively. Some education systems might be more effective in teaching such subjects, succeeding in making students more knowledgeable and skilled in fewer teaching hours per week.

To deal with this potential problem, in Table A-10, I use a measure of de facto education quality. Specifically, I use an average harmonized learning outcome score from standardized, international achievement tests in math, reading, and science (Altinok, Angrist, and Patrinos, 2018). Since data on learning outcome score are available from 1970-2015, while most compulsory schooling reforms were implemented before the 1970s, I use linear probability models. As Table A-9 showed, the IV estimates were somewhat larger than the LPM estimates, but overall quite similar in terms of the sign and size of the effects. Since the data are in 5 year intervals, I assign the missing subsequent years (for example, 1991-1994) with the most recent learning outcome score available in the data (for example, 1990).

Table (A-10) Effect of schooling years and cohort-specific content of education on support for trade, learning outcomes score

	(1)	(2)	(3)	(4)
Schooling	0.153**	0.143**	0.114**	0.132**
	(0.013)	(0.012)	(0.016)	(0.017)
Pluralism in the curriculum	-0.068	-0.034	0.024	-0.009
	(0.082)	(0.086)	(0.087)	(0.086)
Patriotism in the curriculum	-0.016	-0.018	-0.001	-0.031
	(0.039)	(0.051)	(0.052)	(0.051)
Learning outcome score	-0.039	-0.099	-0.093	-0.088
	(0.078)	(0.075)	(0.075)	(0.075)
Schooling X Pluralism in the curriculum		0.048**		
		(0.011)		
Schooling X Patriotism in the curriculum			-0.044**	
			(0.012)	
Schooling X Learning outcome score				-0.034*
				(0.016)
Covariates	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Observations	3683	3683	3683	3683
R-squared	0.117	0.120	0.120	0.118

Note: Entries are linear probability model estimates with robust standard errors in parentheses. All independent variables presented in the table are standardized to have a mean of zero and a standard deviation of one. * $p < 0.05$, ** $p < 0.01$.

Table A-10 shows that using the measure of learning outcome score does not change the results. The effect of education on support for trade liberalization is not stronger in cohorts exposed to (de-facto) higher-quality education. In fact, in cohorts where the learning outcome score is higher by 1 standard deviation, the effect of education (an increase of 1 SD or about 2.6 years of schooling) is 3 percentage points *smaller*.

E Economic Knowledge

Table E-1 summarizes three competing theories of the effect of education on trade preferences. All three theories suggest that education has a positive effect on public support for international trade, but for different reasons. In this section, I address the possibility that the effect of *secondary* education stems from exposure to economic knowledge and ideas about the benefits of trade in school.

Table E-1: Competing Theories

Theory/model	What does education do?
(1) Material self-interest	Increases material benefits from international trade
(a) Factoral	Upgrades workers' professional skills
(b) Sectoral	Reduces employment in import-competing industries
(c) Occupational	Reduces employment in offshorable, routine-task-intensive occupations
(2) Cultural openness	Socializes students to have more cosmopolitan worldviews; Reduces perceived cultural threat from globalization
(3) Economic knowledge	Informs students about the virtues of trade openness

The assumption in previous work has been that most students are not exposed to economic knowledge about the virtues of international trade before college. For example, Hainmueller and Hiscox (2006)'s have argued that: "College-educated individuals, in particular, are likely to be far more informed than others about the aggregate efficiency gains associated with expanded trade, especially if they have had any contact at all with economics courses and with the theory of comparative advantage."

If indeed most students are not exposed to economic ideas and knowledge about the virtues of trade openness in secondary schools, then this potential mechanism should not play a significant role in producing the effect of education. Below, I provide more details about the extent to which secondary schools include economic education in the curriculum. I also test this hypothesis using data from UNESCO's (1961) World Survey of Education.

Information on economic education in secondary schools is hard to find. There are very few studies on the extent to which social sciences—and even fewer on the extent to which economics courses—are taught at the secondary level (UNESCO, 2010, chp. 8). In some notable cases, this is because only very recently some countries or subnational states decided to introduce mandatory economic education.

In Germany, for example, only the federal state of Baden-Württemberg has passed a curriculum reform in 2016 introducing mandatory economic education as a separate school subject (Eberle and Oberrauch, 2023). In Hungary, the National Core Curriculum included economic education as an educational goal and a formal subject area only in 2007 (Dancs and Fülöp, 2020). In England, the new National Curriculum subject of citizenship (which included elements of economics) was introduced in 2000, and the program of personal, social, health and economics (PSHE) education was introduced in 2009. However, out of nearly four million children in secondary schools in England, only about 50,000 (or 1.25%) took an economics class in 2010 (Brant and Cullimore, 2012). Furthermore, since the compulsory

Table E-2: Denmark's list of subjects taught at secondary schools

TIME-TABLE FOR 'GYMNASIE' SECTIONS
(in periods per week)

Subject	Classical languages line			Modern languages line			Mathematics-science line		
	1	2	3	1	2	3	1	2	3
Religious knowledge	1	1	1	1	1	1	1	1	1
Danish	4	4	4	4	4	4	4	4	4
French	5	5	4	5	5	4	5	5	4
History	3	3	4	3	3	4	3	3	4
Greek	6	6	6	—	—	—	—	—	—
Latin	6	6	6	4	4	3	—	—	—
Ancient literature and art	—	—	1	1	1	1	1	1	1
English	—	—	—	5	6	5	—	—	—
German	—	—	—	5	4	4	—	—	—
English or German	—	—	—	—	—	—	2	2	—
English and/or German	3	3	—	—	—	—	—	—	—
Mathematics	—	—	—	—	—	—	6	6	6
Physics	—	—	—	—	—	—	6	6	6
Geography with physics	2	2	—	2	2	—	—	—	—
Geography	—	—	—	—	—	—	2	2	—
Natural history	—	—	4	—	—	4	—	—	4
	30	30	30	30	30	30	30	30	30
Physical exercises, singing, etc.	6	6	6	6	6	6	6	6	6

education reforms I study were implemented between the 1940s and the 1970s and given the RD design I employ (which focuses on respondents who were in secondary school around the time of those reforms), this much more recent trend of economic education should have no bearing on the results.

To further examine the prevalence of economics education in secondary schools, I use data from UNESCO's World Survey of Education (UNESCO, 1961). I review the curriculum subjects in each of the countries in my sample. To illustrate, Table E-2 presents Denmark's list of subjects taught at upper secondary schools. As the Table shows, neither economics nor social science education is included in the list. In contrast, Table E-3 shows that the Netherlands included both "Political economy" and "Commercial subjects" in HB schools.

As Table E-4 shows, in 5 out of the 18 countries, general secondary schools have offered either economics or social science courses at least to some extent: France, Ireland, Netherlands, Slovenia (former Yugoslavia), and Sweden. However, even in these five countries, potential exposure to economic education has been very limited, as those courses or streams

Table E-3: Netherlands's list of subjects taught at HB secondary schools

TIME-TABLE FOR HB SCHOOLS A AND B
(in periods per week)

Subject	Year of study									
	1	2	3	4		5		Total		
				A	B	A	B	A	B	
Dutch	4	4	4	4	3	4	3	20	18	
French	5	3	3	4	3	4	2	19	16	
English	3	3	3	4	2	4	2	17	13	
German	–	3	3	4	2	4	2	14	10	
Commercial subjects	–	–	1	7	–	6	(2)	14	3	(1)
Political economy	–	–	–	2	–	2	1	4	1	
Geography	3	3	2	2	2	2	1	12	11	
History	4	2	2	2	2	3	2	13	12	
Constitution	–	–	1	1	–	1	–	3	1	
Mathematics	5	5	5	–	5	–	5	15	25	
Mechanics	–	–	–	–	2	–	2	–	4	
Physics	–	2	3	–	3	–	3	5	11	
Chemistry	–	–	2	2	4	2	4	6	10	
Botany and zoology	2	2	–	–	2	–	2	4	8	
Cosmography	–	–	–	–	–	–	1	–	1	
Freehand drawing	3	2	1	–	1	–	(1)	6	8	(7)
Line drawing	–	–	–	–	1	–	(1)	–	2	(1)
Physical training	3	3	3	2	2	2	2	13	13	
Total	32	32	33	34	34	34	34	165	167	(163)

were *elective*. In France, for example, Chatel (2010) notes that the economics teaching stream had very few pupils in the 1950s-1960s. Over the next 40 years, economic and social sciences streams attracted increasing numbers of pupils: by 2007, there were 98,470 pupils in première and 98,035 in terminale in these streams. However, these figures represent only 4.4% of all students in general secondary schools in France even after the considerable rise in the number of students learning economics.

Table E-4 further shows that in 8 other countries, some specialized (commercial/ technical/ vocational) secondary schools have also offered economics education. Again, the prevalence of exposure to economics education should be fairly limited in these cases, as economics courses have been included in the curriculum only in some specialized schools and usually only in particular programs in these schools.

Nonetheless, to examine the extent to which the inclusion of respondents potentially exposed to economics education affects the results, I re-run the analysis excluding (1) the five countries that have offered economics in general schools; and (2) respondents with vocational

Table E-4: The prevalence of economics education in secondary schools, 1961

	General secondary schools	Specialized secondary schools
Austria	No	Yes, in commercial schools
Bulgaria	No	No
Czech Republic	No	Yes, in vocational and technical schools
Denmark	No	Yes, in commercial schools
France	Yes, elective	Yes, in vocational and technical schools
Germany	No	Yes, in commercial and vocational schools
Hungary	No	Yes, in technical schools
Ireland	Yes, elective	No
Japan	No	No
Latvia	No	Yes, in specialized secondary schools
Netherlands	Yes, in HB schools	Yes, in commercial schools
Portugal	No	No
Slovakia	No	Yes, in vocational and technical schools
Slovenia	Yes, in Gimnazije	Yes, in vocational schools for commerce
Spain	No	Yes, in vocational and technical schools
Sweden	No (potentially in social sciences, elective)	Yes, in commercial and vocational schools
Taiwan	No	No
United Kingdom	No	No

Source: Based on UNESCO's (1961) World survey of education, v.3: Secondary education.

or technical education from countries that have offered economics in specialized schools.

Table E-5 presents the results. Column 1 replicates the analysis in Table 4 in the main text. Column 2 excludes the five countries with economics in general schools around the time of the reforms. Column 3 excludes respondents with vocational secondary education in countries with economics courses in specialized schools. Finally, column 4 uses both exclusion criteria and restricts the sample to respondents from countries and schools without potential exposure to economics courses. As the table shows, excluding those cases does not change the results—the effect of education remains statistically and substantively similar across all models.

Table E-5: Effect of schooling on support for trade, excluding cases with potential exposure to economics education

	(1) Pro-trade	(2) Pro-trade	(3) Pro-trade	(4) Pro-trade
Schooling	0.039^{**}	0.044^{**}	0.036^{**}	0.041^{**}
	(0.009)	(0.010)	(0.008)	(0.009)
Covariates	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Economics in general schools	included	excluded	included	excluded
Economics in specialized schools	included	included	excluded	excluded
Observations	11589	8111	10083	7270
First-stage F-statistic	180	123	177	121

Note: Entries are IV estimates with robust standard errors in parentheses, using the ±11 optimal bandwidth. †$p < 0.1$ * $p < 0.05$, ** $p < 0.01$.

References

Altinok, Nadir, Noam Angrist, and Harry A Patrinos (2018). "Global data set on education quality (1965-2015)". *World Bank Policy Research Working Paper* 8314.

Brant, Jacek and Duncan Cullimore (2012). "Participation, progression and value added: Business and economics for 14–19-year-olds in England". *Curriculum Journal* 23.1, pp. 79–98.

Bromley, Patricia, John W Meyer, and Francisco O Ramirez (2011). "The worldwide spread of environmental discourse in social studies, history, and civics textbooks, 1970–2008". *Comparative Education Review* 55.4, pp. 517–545.

Brunello, Giorgio, Margherita Fort, and Guglielmo Weber (2009). "Changes in compulsory schooling, education and the distribution of wages in Europe". *The Economic Journal* 119.536, pp. 516–539.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik (2014). "Robust nonparametric confidence intervals for regression-discontinuity designs". *Econometrica* 82.6, pp. 2295–2326.

Cattaneo, Matias D, Nicolás Idrobo, and Rocío Titiunik (2023a). "A practical introduction to regression discontinuity designs: Extensions". *arXiv preprint arXiv:2301.08958*.

Cattaneo, Matias D, Nicolás Idrobo, and Rocio Titiunik (2023b). "A practical introduction to regression discontinuity designs: Extensions". *arXiv preprint arXiv:2301.08958*.

Cavaille, Charlotte and John Marshall (2019). "Education and anti-immigration attitudes: Evidence from compulsory schooling reforms across Western Europe". *American Political Science Review* 113.1, pp. 254–263.

Chatel, Elisabeth (2010). "Economics as a social science in French lycées: A programme shaped by the evolution of a school discipline". *JSSE-Journal of Social Science Education*.

Dancs, Katinka and Márta Fülöp (2020). "Hungary:: Past and present of social science education in Hungary". *JSSE-Journal of Social Science Education* 19.1.

Eberle, Mira and Luis Oberrauch (2023). "What a difference three years of economics education make: Evidence from lower stream schools in Germany". *International Review of Economics Education* 42, p. 100259.

Garrouste, Christelle (2010). "100 Years of Educational Reforms in Europe: a contextual database." PhD thesis. European Commission's Joint Research Centre (JRC).

Hofmarcher, Thomas (2021). "The effect of education on poverty: A European perspective". *Economics of Education Review* 83, p. 102124.

Kawaguchi, Daiji (2011). "Actual age at school entry, educational outcomes, and earnings". *Journal of the Japanese and International Economies* 25.2, pp. 64–80.

Kemble, Benjamin G (2004). "My parents, my sensei: Compulsory education and a home-schooling alternative in Japan". *Tex. Int'l LJ* 40, p. 335.

Mocan, Naci and Luiza Pogorelova (2017). "Compulsory schooling laws and formation of beliefs: Education, religion and superstition". *Journal of Economic Behavior & Organization* 142, pp. 509–539.

Neundorf, Anja, Eugenia Nazrullaeva, Ksenia Northmore-Ball, Katerina Tertytchnaya, and Wooseok Kim (2024). "Varieties of Indoctrination: The Politicization of Education and the Media around the World". *Perspectives on Politics*, 1–28. DOI: 10.1017/S1537592723002967.
UNESCO (1961). "World survey of education, v.3: Secondary education". *ED.60/IX.3/A*.
— (2010). "World Social Science Report: Knowledge Divides".
Zhang, Junchao (2018). "The impact of 9-year compulsory education: quasi-experimental evidence from Taiwan". *Applied economics* 50.45, pp. 4866–4878.