

Compulsory Schooling Laws and Migration Across European Countries

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Abstract Educational attainment is a key factor for understanding why some individuals migrate and others do not. Compulsory schooling laws, which determine an individual's minimum level of education, can potentially affect migration. We test whether and how increasing the length of compulsory schooling influences migration of affected cohorts across European countries, a context where labor mobility is essentially free. We construct a novel database that includes information for 31 European countries on compulsory education reforms passed between 1950 and 1990. Combining this data with information on recent migration flows by cohorts, we find that an additional year of compulsory education reduces the number of individuals from affected cohorts who migrate in a given year by 9 %. Our results rely on the exogeneity of compulsory schooling laws. A variety of empirical tests indicate that European legislators did not pass compulsory education reforms as a reaction to changes in emigration rates or educational attainment.

Keywords Migration · Compulsory schooling · Education

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Introduction

Educational attainment is a key factor for understanding why some individuals migrate and others do not (see, e.g., Borjas 1987). For instance, in most European countries, individuals with secondary educational attainment display much lower emigration rates compared with both the primary- or tertiary-educated. Compulsory schooling laws determine an individual's minimum level of education, and they thus have the potential to affect migration. Our article addresses how changes in compulsory schooling laws affect migration across European countries. To the best of our knowledge, we are the first to analyze how compulsory schooling laws affect international migration in a multicountry setting. Results from our analysis are of particular interest to policymakers who might be concerned whether and how much of a country's investments in education could be lost to "brain drain."

Our analysis tests how across-country and across-time differences in compulsory schooling laws influence European migration of affected cohorts, exploiting that education laws change over time and that hence different cohorts face different lengths of compulsory schooling in each country. We construct a novel database that contains information on changes to the length of compulsory education passed between 1950 and 1990 in 31 European countries: all 28 European Union (EU) countries plus Norway, Liechtenstein, and Macedonia. We merge this database with recent cohort data on migration flows for 2008–2012. The European setting with basically unrestricted labor mobility is ideal for our analysis because it allows us to isolate the role of education policies.¹ Many other countries tend to place stricter limits on the entry of low-educated compared with highly educated individuals, which makes it difficult to disentangle the effects of education policies from those of migration restrictions.²

To isolate the effect of compulsory schooling laws in place when a certain cohort was in school on that cohorts' migration decisions later in life, we have to control for other determinants of migration choices that could be correlated with such laws. Including country, age group, and year fixed effects as well as the interactions of country and age group with year dummy variables, we are able to account for any macroeconomic variables that do not vary by age group (e.g., GDP per capita) as well as cohort-specific factors that influence migration propensities (e.g., different labor market prospects of young and old individuals). Additionally, the European Union has 24 official working languages that are quite distinct.³ Aparicio Fenoll and Kuehn (2016) showed that exposure to foreign language classes during compulsory education increases migration to countries where these languages are spoken. Hence, reforms to compulsory education that also alter the particular content of school curricula related to foreign languages might have an additional and potentially different effect on

¹ EU law guarantees free labor mobility, but countries can impose temporary restrictions for nationals of new member states. Prior to 2014, some EU member states required that Bulgarian and Romanian nationals obtained residence and work permits. Norway and Liechtenstein belong to the Schengen area, which has guaranteed free mobility since 2001 and 2011, respectively. Croatia joined the EU only in 2014. Macedonia is an EU candidate country, and since 2009, its residents can travel visa-free to the Schengen area.

² For instance, Canada and Australia use a point system that grants visas to more-skilled and higher-educated individuals; see Aydemir (2011) for an analysis of the selection effects of such systems.

³ Linguists identify at least seven European language families: Celtic, Italic, Germanic, Baltic-Slavic, Greek, Uralic, and Semitic (see Gray and Atkinson 2003; Harding and Sokal 1988).

migration. Thus in our estimations, we also control for the fact that certain cohorts in some countries learned foreign languages during compulsory education but others did not. Our findings, which thus result from refined comparisons of cohorts, reveal that one additional year of compulsory schooling reduces migration of affected cohorts in a given year by 9 %.

One potential explanation for this finding is that increases in the length of compulsory schooling shift a significant fraction of the population from low to medium educational attainment and, as mentioned earlier, emigration rates of medium-educated individuals are lower compared with those of low-educated individuals in the majority of European countries (see Fig. S1, Online Resource 1).⁴ In line with this hypothesis, we find that additional years of compulsory education effectively translated into higher average educational attainment. Hence, although this article focuses on the impact of compulsory schooling laws on migration, our findings also shed light on the more general question of how educational attainment affects migration. On the other hand, our analysis has the advantage of allowing for general equilibrium effects of education policies on migration that go beyond their effect on educational attainment. For instance, increasing the length of compulsory schooling is likely to improve labor market opportunities for teachers and might reduce crime rates, given that individuals are obliged to stay in school for additional years. Such aspects might additionally affect individuals' future migration choices.

The validity of our results relies on the exogeneity of education reforms with respect to migration and educational attainment. Following Lleras-Muney (2002) and Landes and Solmon (1972), we perform two empirical tests regarding the potential relationship of compulsory schooling reforms with different socioeconomic variables, migration out-flows, and educational attainment measured around the time when reforms were passed. Similar to Aaronson and Mazumder (2011), we also add country-specific birth cohort trends to our main specification. Our results indicate that European lawmakers did not pass compulsory education reforms as a reaction to changes in migration rates or increased educational attainment.

Our study is closely related to literature using education laws and related policies to estimate the causal effect of education on within-country migration.⁵ For instance, Machin et al. (2012) used a change in compulsory schooling laws in Norway and found education to increase internal mobility. Results for the effect of education on state-to-state migration in the United States are mixed. Malamud and Wozniak (2010b) used the risk of being drafted for the Vietnam War as an instrument for college-level education and estimated a positive causal effect of education on migration. They also found that when years of schooling were instrumented by quarter of birth, the estimates turned negative but not significant (Malamud and Wozniak 2010a). As the authors suggested, if the effect of education on migration depends on individuals' baseline educational attainments, such contrasting results might arise. McHenry (2013) used changes to compulsory schooling laws across U.S. states and showed that for lower levels of

⁴ For instance, Brunello et al. (2009) provided evidence for a number of European countries for which compulsory schooling laws significantly affect educational attainment for all but the upper part of the education distribution.

⁵ Using data on educational attainment entails problems of reverse causality if individuals' education choices are influenced by their desire to migrate. For instance, McKenzie and Rapoport (2011) found that Mexican boys from a household with international migration experience are more likely to drop out of school.

education, additional attainment has a negative effect on state-to-state migration, similar to our findings.⁶ In the context of international migration, most studies have used observed educational attainment to predict wages in the destination country. Taking the number of immigrants as given, their focus has been on the sign of the selection effect. Results from these studies, all based on Mexican-U.S. migration, have ranged from negative self-selection of immigrants with individuals from the bottom of the skill distribution being more likely to migrate (Fernández-Huertas Moraga 2011), to positive self-selection (Chiquiar and Hanson 2005), to a U-shaped relationship (Caponi 2010).⁷

Our analysis of how compulsory schooling laws affect international migration is important because findings regarding the relationship between education policies and internal mobility cannot simply be extrapolated to the context of international migration. Within a country, educational attainment is easily transferable, but education obtained in one country might not be fully recognized in another country (see Chiswick 2008; Greenwood and McDowell 1991). Furthermore, the degree to which human capital is transferable across countries tends to depend on foreign language proficiency. Hence, the relationship between compulsory schooling laws and international migration is likely to be governed by a different set of rules. We account for some of these differences in our estimations by controlling for cohorts' exposure to foreign languages during compulsory education and the number of years that cohorts in different countries have lived in the EU, a relatively integrated labor market that has recently tried to make at least university degrees comparable across countries.

Estimation Strategy

Our estimation strategy makes use of all three dimensions of variation in the data, comparing individuals across time, age groups, and countries. We could have estimated the effects of compulsory schooling laws on migration by comparing individuals only across age groups, considering different cohorts from the same country who were affected by different laws. However, such a specification would be affected by differences in the propensities to migrate by age. Another alternative would have been to compare individuals of the same age from different countries. However, nationals of different countries have different propensities to migrate, independently of education policies. A third approach would be to observe individuals of a certain age and country at different points in time, using the fact that education laws change over time. However, because we have data on migration flows for only five years, variability over time is very limited. But even if we disposed of additional data, migration patterns

⁶ Regarding a related outcome variable—immigrant assimilation—and using an analysis similar to the current article, Lleras-Muney and Shertzer (2015) considered changes in education policies, including compulsory schooling laws and the imposition of English as language of instruction. The authors found no effect of these policies on migrant assimilation in the United States between 1910 and 1930.

⁷ Quinn and Rubb (2005) proposed to reconcile these different findings by looking at education-occupation matches. They found that individuals who work in occupations that require less education are more likely to migrate, while the opposite is true for individuals whose level of education is below that required for their occupation. Alternatively, McKenzie and Rapoport (2010) found that the selection effect along educational lines regarding Mexican-U.S. migration depends on the size of networks, with larger (smaller) networks attracting disproportionately more-uneducated (educated) individuals.

change over time. We improve on these approaches by combining them all and using fixed effects to account for the potential problems mentioned earlier. Our empirical strategy compares migration decisions of (1) different cohorts from the same country who were exposed to different educational policies because of policy changes, and (2) identical cohorts from different countries who were exposed to different educational policies because of differences in legislation across countries. Using fixed effects, this strategy allows us to control for confounding factors that vary across age, time, and countries, and across pairwise combinations of age and time and countries and time. As a result, our estimated coefficients arise from refined comparisons of cohorts, and they are robust to the potential influence of a long list of unobserved factors.

To assess the effect of changes to the length of compulsory schooling on migration, we estimate the following model:

$$m_{a,i,t} = \alpha_0 + \alpha_1 CS_{a,i,t} + \alpha_2 \mathbf{L}_{a,i,t} + \alpha_3 \mathbf{X}_{a,i,t-1} + \alpha_4 EU_{a,i,t} + \alpha_5 D_a + \alpha_6 D_t + \alpha_7 D_t + \alpha_8 D_{a,t} + \alpha_9 D_{i,t} + \varepsilon_{a,i,t}, \quad (1)$$

where $m_{a,i,t}$ is the natural logarithm of the number of individuals of age a who migrate from country i in year t . $CS_{a,i,t}$ denotes the number of years of compulsory schooling that individuals who are of age a in year t faced in country i when they were younger, and $\mathbf{L}_{a,i,t}$ are indicator variables that take on a value of 1 if these individuals were taught foreign languages during their compulsory education. We include these last controls because exposure to foreign language classes during compulsory education has been linked to increased migration flows of affected cohorts to countries where these languages are spoken (see Aparicio Fenoll and Kuehn 2016).⁸ Because only certain European languages are studied during compulsory education, our set is restricted to English, German, French, Spanish, and Italian.⁹ $\mathbf{X}_{a,i,t-1}$ are control variables, such as total population, the stock of migrants from country i living in other European countries, and the difference in unemployment rates between country i and other European countries. All three variables are measured in $t - 1$, and we include them disaggregated by age group. Although gravity models of migration typically include differences in GDP per capita across countries as explanatory variables, such differences in our specification are captured by interaction terms of year and country dummy variables. Including differences in unemployment rates by age group allows us to capture the fact that countries' relative economic attractiveness might vary by cohort. Following Bertrand et al. (2004), we cluster standard errors at the country level to allow for serial correlation in migration outflows over time.

⁸ For countries where students can choose among foreign languages, we consider all options for two reasons. First, for most of these countries, we do not have information on languages chosen by each cohort. Second, we wish to avoid measuring individual choices, which would turn foreign languages into a bad control. Limited reliability of data on years of exposure to foreign language classes is the main reason why we do not consider such a refinement.

⁹ Russian is the most widely taught second foreign language in Latvia, Estonia, and Lithuania, but we ignore this option given that we do not have data on migration flows to Russia. Although individuals in Finland and Belgium can additionally study Swedish and Dutch, respectively, we do not include these options explicitly given that they apply to all cohorts and are hence indistinguishable from country fixed effects. The same holds true for any controls accounting for the fact that some countries—such as the UK and Ireland, or Germany and Austria—share the same official language.

We also include as a control variable the number of years that individuals in country i who are of age a in year t have lived in the EU ($EU_{a,i,t}$). Joining the EU implies that migration costs for citizens of new member countries are significantly reduced. How this affects their migration decisions depends on their age and on how many years of their working lives they could still spend in a different EU country. Hence, different migration patterns for different cohorts could arise. Including the variable $EU_{a,i,t}$ ensures that we are not erroneously attributing changes in migration patterns caused by a country's EU membership to changes in the length of compulsory education. Furthermore, the European context and our period of analysis (2008–2012) also call for the inclusion of interaction terms of country and year dummy variables, particularly because in 2009 and 2011, work restrictions in some countries for nationals of Central European countries that joined the EU in 2004 and 2007, respectively, were finally lifted.¹⁰

Our identification strategy makes use of the fact that education reforms that changed the length of compulsory schooling during the twentieth century for different cohorts generate within- and across-country variation. Figure 1 displays an example. Consider the situation of four individuals: A , B , C , and D . Individuals A and B are Portuguese citizens and were born in 1968 and 1985, respectively, while individuals C and D , also born in 1968 and 1985, are from Denmark. In Portugal in 1986, the length of compulsory schooling increased from six to nine years. Given a school entry age of 6, all individuals age 6 or younger in 1986—those born in 1980 or later—were affected by this reform. Individual B is hence assigned nine years of compulsory education, while individual A is assigned six years. In Denmark, on the other hand, the length of compulsory schooling increased from seven to nine years in 1971. Given a school entry age of 7, all individuals age 7 or younger in 1971—that is, those born in 1964 or later—were affected. Hence, both individuals from Denmark are assigned the same length of compulsory schooling of nine years. Controlling for a large variety of other potential determinants of migration, our main estimation then tests whether differences in the length of compulsory schooling have an effect on cohorts' subsequent migration decisions.

Although one expects additional years of compulsory schooling to lead to higher educational attainment, this need not be the case. Reforms might not be enforced, or even prior to reforms, individuals could already be staying in school beyond the minimum years required by law. To test whether changes to the length of compulsory schooling have an effect on actual educational attainment, we run the following regression:

$$YS_{a,i} = \alpha_0 + \alpha_1 CS_{a,i} + \alpha_2 L_{a,i} + \alpha_3 X_{a,i} + \alpha_4 EU_{a,i} + \alpha_5 D_a + \alpha_6 D_i + \varepsilon_{a,i}, \quad (2)$$

where $YS_{a,i}$ denotes the average years of schooling of individuals of age a who live in country i . Variables on the right side of the equation are as defined before, but they lack the time dimension because we have information on average years of schooling for only one year (2010). Otherwise, by including the same controls as in our main specification, we make this regression as comparable to a first-stage estimation as possible.

¹⁰ When in place, these restrictions applied to individuals of all ages.

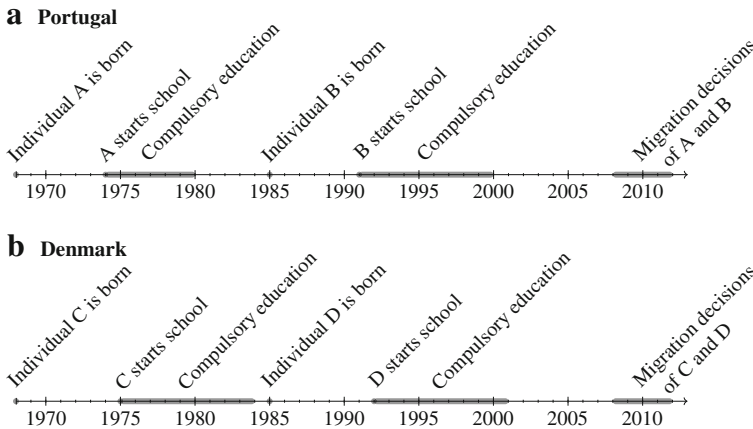


Fig. 1 Identification strategy: Example

Finally, to test for the potential endogeneity of education reforms, we run two regressions. First, Landes and Solmon (1972) proposed addressing endogeneity issues of education reforms by testing whether compulsory schooling laws can “predict” past educational attainment. If so, the authors argued, then exogeneity would not hold, and causality would be likely to run from educational attainment to compulsory schooling. Because our research question considers the effect of compulsory schooling laws on migration, our main concern is the exogeneity of education laws with respect to migration. To test for it, we estimate the following regression:

$$E_{i,\tau-10} = \alpha_0 + \alpha_1 CS_{i,\tau} + \alpha_2 Y_{i,\tau} + \alpha_3 \Delta Y_{i,\tau-10} + \alpha_4 D_i + \alpha_5 D_\tau + \varepsilon_{i,\tau-10}, \quad (3)$$

where $E_{i,\tau-10}$ denotes country i 's emigration rate measured 10 years prior to the reform passed in τ . $Y_{i,\tau}$ represents socioeconomic variables (GDP per capita, population growth, log of migration, share of the urban population, and average years of schooling) measured at the time of the reform, and $\Delta Y_{i,\tau-10}$ are their 10-year variation rates. Dummy variables for reform years are denoted by D_τ . Although exogeneity of education reforms with respect to educational attainment in our context is of secondary importance, we also rerun the estimation with country i 's average educational attainment 10 years prior to the reform as the dependent variable.

Second, in the spirit of Lleras-Muney (2002), we explore the potential determinants of compulsory schooling laws by running the following regression:

$$CS_{i,\tau} = \alpha_0 + \alpha_1 Y_{i,\tau} + \alpha_2 \Delta Y_{i,\tau-5} + \alpha_3 \Delta Y_{i,\tau-10} + \alpha_4 D_i + \alpha_5 D_\tau + \varepsilon_{i,\tau}, \quad (4)$$

where $CS_{i,\tau}$ are years of compulsory schooling that were passed in year τ , and we test for relationships with socioeconomic variables and their 5- and 10-year variation rates.

Data

For our analysis, we use Eurostat data on immigration by five-year age groups and citizenship for 2008–2012, available for all 28 EU countries plus Norway,

Liechtenstein, and Macedonia. In particular, we construct migrant outflows for each year by summing the number of citizens of each country who leave for any of the following destination countries: Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, Germany, Greece, Hungary, Ireland, Italy, Latvia, Liechtenstein, Lithuania, Luxembourg, Macedonia, Malta, Netherlands, Norway, Poland, Romania, Slovakia, Slovenia, Spain, and Sweden. For arrivals in Germany and Austria, missing data for 2009–2012 and 2010, respectively, are complemented with data from the Statistische Bundesamt and Statistik Austria. Data on migrants who arrive in the UK come from the International Passenger Survey of the Office for National Statistics (ONS). We also rely on Eurostat for data on national unemployment rates, total population, and the stock of migrants by country of origin. These three variables are considered disaggregated by five-year age groups and are measured one year prior to migration—that is, in 2007–2011.

We restrict our sample to young individuals aged 25–44, who are most likely to migrate for work-related reasons. We exclude individuals younger than age 25 because it is difficult to disentangle migration from education decisions for this group, particularly in the presence of a large-scale EU program (Erasmus) that provides subsidies for studying abroad.¹¹ Regarding older workers, the number of years that individuals have to work to become eligible for pension payments varies widely across countries: for example, France and Spain require 15 years, while Germany requires 5 years. Hence, for older individuals, such policy aspects that are unrelated to individuals' educational attainment might influence their migration decisions.

Our database on compulsory schooling reforms that includes information on the length of compulsory education for each cohort in each country is mainly based on the following four sources: Brunello et al. (2009), Garrouste (2010), Hörner et al. (2007), and Murtin and Viarengo (2011). The assignment of years of compulsory schooling to age groups is not always straightforward. Thus, for age groups in which only some individuals were affected by changes to compulsory education, we construct a weighted average for years of compulsory schooling.¹² As weights, we use the number of individuals of each exact age within the age group, which we obtain from Eurostat. Table S1 in Online Resource 1 displays all effective changes to the length of compulsory education for cohorts in our sample. A detailed description of these education reforms for all countries (including countries where no effective changes occurred) can be found in section A.1 of Online Resource 1. As mentioned earlier, our database also includes information on reforms regarding foreign languages in compulsory school curricula. For each cohort and country, we have information, mainly from the European Commission's Education, Audiovisual and Culture Executive Agency (Eurydice), regarding the starting age for studying foreign languages during compulsory education as well as the specific languages studied. We summarize this information in Table S2 in Online Resource 1; in section A.1 of the [online supplement](#), we describe these reforms in more detail.

¹¹ For 2007–2013, the EU allocated €3.1 billion to the Erasmus program.

¹² One potential concern could be that individuals did not obtain their schooling in their country of origin. However, using data by citizenship, instead of country of previous residence, mitigates this concern substantially.

For our estimation that tests whether education reforms were effective at increasing educational attainment, we consider data on average years of education for each age group from Barro and Lee (2013) for 2010.¹³ For our exogeneity checks, we have data on migration outflows from various editions of the United Nations Statistical Yearbooks, available for 1950–1995 for 24 of the 31 countries in our sample.¹⁴ We use data from the OECD for GDP per capita, which are available beginning with 1960. However, for former communist countries (such as the Czech Republic, Croatia, Hungary, Poland, Slovakia, and Slovenia), this series starts only in 1990. To avoid losing observations, we hence set missing values for GDP per capita for 1960–1989 for these countries to 0, and we define an indicator variable for missing data. Finally, we again rely on data from Barro and Lee (2013) for past educational attainment and on World Bank data for population growth and the percentage of the population living in urban areas. These last three variables are available from 1960 onward for all countries except Macedonia. Thus, our final sample for the exogeneity check includes 23 countries.¹⁵

Table 1 provides summary statistics for all our variables. For the variables included in our main specification, we have observations for 620 cells defined by the combination of country, age group, and year. On average, 4,163 individuals in each age group from each country migrate each year. However, we observe significant variation in these migration flows. For instance, in 2009, only 1 individual aged 40–44 migrated from Liechtenstein, while 54,766 individuals aged 25–29 migrated from Poland. Average years of compulsory schooling are 9 years, ranging from 6 years (for older cohorts in most countries) to 13 years for younger cohorts in Germany. We observe a maximum difference of 32 percentage points in 2008 between Macedonia's unemployment rate for individuals aged 25–29 and unemployment for the same age group in other European countries. Other control variables that we include into our main specification, disaggregated by age group and measured one year before migration, are the stock of migrants in other European countries and total population. Regarding the latter, each country has, on average, a little more than 1 million inhabitants per age group, ranging from only 2,224 individuals aged 25–29 in Liechtenstein to more than 7 million individuals aged 35–39 in Germany in 2008. Regarding the stock of migrants,

¹³ Unfortunately, we cannot exploit changes over time because these data are available only every five years. Barro and Lee (2013) provided data for all countries in our sample except for Liechtenstein.

¹⁴ We use the following editions: 1952, 1954, 1957, 1959, 1962, 1966, 1968, 1970, 1977, 1985, 1989, and 1996. For Croatia, Macedonia, and Slovenia, we use data for Yugoslavia that are available for 1956–1977. For the Czech Republic and Slovakia, data for Czechoslovakia are used until 1992. Because no data on emigration from the USSR are available, we cannot assign data to Estonia, Latvia, or Lithuania. Data for Bulgaria, Romania, and Liechtenstein are only sporadically available, and numbers for emigration flows from Ireland exceed those of the Irish population; hence, we do not include any of these countries. For Spain, Norway, Finland, and the UK, data before 1962, before 1960, for 1960–1966, and for 1961–1963, respectively, could not be used because they included only intercontinental migration in the case of Spain; excluded migration to other Scandinavian countries; or in the case of the UK, referred only to migration to other Commonwealth countries.

¹⁵ Aggregate migration outflow data are available for 24 countries from 1950–1995, but data for all other control variables are not available for Macedonia, and for all other countries only from 1960 onward. The final sample for the exogeneity check includes the following countries: Austria, Belgium, Croatia, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Italy, Luxembourg, Malta, Netherlands, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, and the UK.

Table 1 Summary statistics

Variable	Mean	SD	Min.	Max.
Main Variables				
Emigration outflow by origin, age, and year	4,163.1	6,596.9	1	54,766
Years of compulsory education	9.07	1.17	6	13
Age group				
25–29	0.25	0.433	0	1
30–34	0.25	0.433	0	1
40–44	0.25	0.433	0	1
Year				
2008	0.2	0.4	0	1
2009	0.2	0.4	0	1
2011	0.2	0.4	0	1
2012	0.2	0.4	0	1
Difference in unemployment, all EU destinations by age	0.00	5.86	–10.23	31.75
Stock immigrants in all EU destinations by age	34,608	57,552	52	407,107
Stock population in origin by age	1,166,457	1,554,559	2,224	7,176,550
Compulsory foreign language classes				
English	0.77	0.41	0	1
German	0.67	0.46	0	1
French	0.47	0.49	0	1
Spanish	0.42	0.48	0	1
Italian	0.10	0.28	0	1
Years lived in the EU	0.09	0.27	0	1
<i>N</i> = 620	15.01	14.04	0	46.50
Pseudo-First Stage				
Average years of schooling by cohort, 2010	12.094	0.998	8.556	14.187
<i>N</i> = 116				
Exogeneity Checks: 23 Countries (1960–1995)				
Reforms: Length of compulsory schooling	9.08	1.34	6.00	13.00
Emigrants (logs)	9.46	2.24	0.69	14.03
Population growth (annual %)	0.49	0.49	–0.89	3.80
Urban population (% of total population)	70.32	13.83	37.00	96.78
Average years of schooling by cohort	7.98	1.60	2.93	11.24
GDP per capita ^a	11,573	6,696	1,980	41,014
5-year variation rates				
Emigrants (logs)	–0.30	0.82	–4.26	1.58
Population growth	–0.01	0.54	–2.72	4.69
Urban population	0.04	0.04	–0.01	0.27
Average years of schooling by cohort	0.08	0.06	–0.05	0.30
GDP per capita ^b	0.46	0.16	0.06	0.75
10-year variation rates				
Emigrants (logs)	–0.48	1.12	–6.79	2.30
Population growth	–0.10	0.72	–4.55	4.21

Table 1 (continued)

Variable	Mean	SD	Min.	Max.
Urban population	0.08	0.09	-0.02	0.51
Average years of schooling by cohort	0.17	0.12	-0.12	0.59
GDP per capita ^c	1.14	0.34	0.45	1.94

N = 492

Notes: Unemployment rates, stock of immigrants, and population by age group refer to years $t - 1$ (i.e., 2007–2011). Five- and 10-year variation rates are defined as % changes with the exception of log-migration and population growth, which are defined as simple differences

^a *N* = 358

^b *N* = 286

^c *N* = 223

each country has on average approximately 34,607 individuals of each age group living in other European countries—that is, more than eight times the average annual outflow.

However, although only 52 individuals aged 40–44 from Liechtenstein were residing abroad in 2011, the stock of migrants from Romania aged 30–34 in other European countries was 407,107 in 2012. Finally, we also include indicator variables for cohorts who were exposed to different foreign languages during their compulsory education. More than two-thirds of individuals in our sample potentially learned English, followed by 47 % and 43 % who could have learned German or French, respectively. Only recently, individuals in most countries can also choose Spanish or Italian as compulsory foreign languages, affecting less than 10 % of individuals in our sample.

Results

We first test whether changes to the length of compulsory schooling have an effect on actual educational attainment (see Eq. (2)). As mentioned earlier, we do not conduct a two-stage least squares (2SLS) estimation, but we make the regression as comparable to a first-stage estimation as possible.¹⁶ In particular, we regress average educational attainment for different age groups on our measure of years of compulsory schooling, and we control for the same variables as in our main specification. Table S3 in Online Resource 1 shows the results. The estimated coefficients indicate that policies that increased the length of compulsory schooling were effective at increasing average years of education of affected cohorts. Given that already prior to reforms, individuals could have been staying in school longer, the increase is less than proportional. In particular, we find that one additional year of compulsory schooling increases average educational attainment of affected cohorts by 0.26 years. Our estimate thus is close to the additional 0.2 and 0.23 years that Oreopoulos (2007) estimated for an increase in the school-

¹⁶ Running a 2SLS IV estimation with data for 2010 only results in a negative and significant estimate of the impact of education on migration. However, because of the reduced sample size, the instrument is weak according to the Stock and Yogo (2005) criterion. Furthermore, as mentioned in the Introduction, it is not clear that compulsory schooling reforms affect migration only via educational attainment, something that would invalidate the exclusion restriction.

leaving age from 15 to 16 years in the United States and Canada, respectively. Thus, part of the effect of compulsory schooling reforms on indirectly related outcomes, such as migration, operates through changes in educational attainment.

We then turn to our main model, as specified in Eq. (1), which estimates the effect of changes to the length of compulsory education on migration. Results are displayed in Table 2. The first column corresponds to the basic regression that includes dummy variables for year, age group, and countries; lagged control variables for unemployment differences, stock of migrants, and population by age group; dummy variables for compulsory foreign language classes; and the number of years individuals have lived in the EU. In column 2, we add the interaction term for age and year. Column 3 presents results for the most complete specification that also includes the interaction term between country and year. Our coefficient of interest is negative, significant, and very stable across all specifications. One additional year of compulsory schooling reduces the number of individuals from the affected cohorts who migrate in a given year by 9 %.

Having been exposed to English, French, German, Italian, or Spanish during compulsory education affects the odds of migrating to particular European countries (see Aparicio Fenoll and Kuehn 2016) but does not significantly affect the general odds of migrating. Table S4 in Online Resource 1 displays the full set of estimated coefficients. After we control for macroeconomic and cohort-specific factors by including age-by-year and country-by-year dummy variables, age-specific controls do not play a significant role; see column 3. Coefficients in columns 1 and 2, when significant, show the expected signs. Higher unemployment rates and larger cohorts are related to more out-migration. On the other hand, older cohorts in countries that have been in the EU longer migrate less; and as more individuals of a cohort leave, the number of emigrants falls.¹⁷

Discussion

Why would additional years of compulsory schooling reduce migration? One potential explanation is linked to the fact that an increase in the length of compulsory schooling shifts a significant fraction of the population from low to medium educational attainment, and medium-educated individuals display lower emigration rates compared with low-educated individuals in the majority of European countries. However, this opens up a second question: Why do medium educated individuals migrate less? Or, put differently, Why would returns to migration be higher for primary- and tertiary-educated individuals (i.e., at both tails of the education distribution)?

Several aspects could explain such a pattern in returns to migration. First, the median voter in most European countries has an intermediate level of education (see Fig. S2). As a result, labor market regulations in each country are likely to be tailored to medium-educated individuals. McHenry (2013), who found compulsory schooling to

¹⁷ The presence of network effects could imply that as more individuals leave, more follow. However bear in mind that unlike the literature's typical example regarding Mexican-U.S. migration, the stock of immigrants in our case is spread over 30 countries. The negative coefficient is in line with the notion that after a large fraction of an age group has migrated, those who remain have a much lower propensity to leave.

Table 2 Migration and years of compulsory schooling

	(1)	(2)	(3)
Years of Compulsory Schooling	-0.098* (0.039)	-0.100* (0.041)	-0.090* (0.041)
Age by Year Dummy Variables	No	Yes	Yes
Country by Year Dummy Variables	No	No	Yes
Number of Observations	620	620	620
R^2	.923	.924	.98

Notes: The dependent variable is the natural logarithm of the number of individuals from a certain age group and country migrating in one year. The variable years of compulsory schooling refers to the weighted average of compulsory schooling faced by the corresponding age group, with weights defined by the number of individuals of each exact age. All regressions contain year fixed effects, age indicators, country dummy variables, a variable for lagged age-specific differences in unemployment rates, the lagged population size of the age group, the lagged stock of migrants from each country by age group, and dummy variables for exposure to compulsory foreign language classes, as well as the number of years individuals have lived in the EU. Standard errors, shown in parentheses, are clustered by country.

* $p \leq .05$

reduce mobility across U.S. states, suggested that staying in school allows individuals to strengthen their community networks, which might help them find jobs. Low- and high-educated individuals, on the other hand, could find foreign labor markets relatively more appealing. For instance, low-educated individuals could migrate to countries that offer higher minimum wages, better public services, and higher social benefits, as argued by the literature on welfare magnets initiated by Borjas (1999). Highly educated individuals could be responding more strongly to demand for their skills in foreign labor markets and/or so-called brain gain policies that some countries implement to attract highly skilled and talented individuals (e.g., researchers); see Giannoccolo (2012) or Mahroum (2005) for a survey of such policies in Europe and OECD countries, respectively.

Second, the model in Stark (1991: chapter 11) proposed a different mechanism that can give rise to a U-shaped pattern of returns to migration. The model is based on the facts that educational degrees are not automatically valid across countries and that degree recognition is costly in terms of time and money. In such an environment, only highly educated individuals find it worthwhile to pay the cost of degree recognition. For medium-educated individuals, on the other hand, the costs are too high relative to the additional wage they could obtain abroad; and without a recognized degree, they are able to earn only a minimum wage in the destination country. Hence, when a minimum wage and a higher wage level exist in some destination countries but wages are increasing in education in both destination and origin countries, a U-shaped pattern for migration can arise: individuals with higher education have their degree recognized and migrate, medium-educated individuals stay, and low-educated individuals migrate to earn the minimum wage of the destination country. The model's outcome is hence in line with European data on lower emigration rates of secondary-educated compared with both primary- or tertiary-educated individuals. The model also suggests a potential mechanism for our findings.

However, reasons unrelated to individuals' educational attainment could also explain why laws that increase the length of compulsory education might lead to less

migration by affected cohorts. As Eisenberg (1988) described, typical arguments in favor of compulsory schooling laws in the United States during the nineteenth century suggested that such laws would lead to improved civic societies and would thus entail reductions in crime rates. These laws made schooling compulsory, and they are therefore different from reforms considered here that change the length of compulsory education. Nevertheless, cohorts affected by additional years of compulsory education will also live surrounded by better-educated peers, which for similar reasons could potentially reduce out-migration. Finally, increases in the length of compulsory schooling require hiring additional teachers. This implies improved local labor market opportunities and incomes, mostly for the parental generation of affected cohorts, and growing up under better economic circumstances could make these cohorts less likely to migrate.

Endogeneity Concern

Since the seminal study by Angrist and Krueger (1991), compulsory schooling laws have been used in studies of the causal relationship between education and many outcomes, such as earnings (Harmon and Walker 1995), health (Brunello et al. 2013), and citizenship (Milligan et al. 2004). More relevant to the current study and as discussed earlier, changes in the length of compulsory education have also been used in analyses of the effect of education on internal migration in Norway (Machin et al. 2012) and the United States (Malamud and Wozniak 2010a; McHenry 2013). Exogeneity with respect to educational attainment is a necessary condition for these approaches to be valid. Previous studies have found mixed results regarding the exogeneity of compulsory schooling laws. Lleras-Muney (2002) for U.S. laws passed between 1915 and 1939, and Nasif Edwards (1978) for U.S. laws passed in 1960, established that those were exogenous to educational attainment. However, studies of other periods in U.S. history found that compulsory law changes might have been endogenous to educational attainment (e.g., for 1940–1955, see Eisenberg 1988; Landes and Solmon 1972; Nasif Edwards 1978; Stigler 1950: appendix B). Given these mixed results and that, to the best of our knowledge, there are no studies regarding the exogeneity of compulsory schooling laws in Europe, we formally address this issue.

Education reforms could be endogenous to migration or educational attainment in two ways: (1) *reverse causality*, if education reforms were enacted because of past migration outflows or past educational attainment, and (2) *omitted variables*, if determinants of cohort-specific migration patterns (e.g., differences in cohort-specific labor market conditions between origin and destination countries) persisted over time and had influenced reforms that were implemented when our cohorts were in school. Regarding reverse causality, education reforms are predetermined with respect to migration patterns in 2008–2012, but migration patterns could be highly persistent over time. However, education reforms could at most be driven by aggregate migration, whereas our approach considers cohort-specific migration flows. Still, it is possible that compulsory schooling reforms took place in countries that were exhibiting certain long-term trends in educational attainment for reasons unrelated to education reforms. Therefore, we follow Aaronson and Mazumder (2011) by including country-specific birth-cohort trends into our main specification. Results in Table S5 in Online Resource 1 show that the estimated coefficient of interest is not significantly different from the one in our main regression.

From a political economy point of view, migration flows are unlikely to influence education policies. As discussed earlier, governments design their education policies focusing on the median voter who stays, instead of targeting those who migrate. Moreover, the time between the implementation of education reforms and students' completion of compulsory education and entry into the labor market—be it at home or abroad—is likely to exceed governments' mandates. Hence, because governments might not be able to reap the potential fruits in terms of more or less migration, migration flows or brain drain concerns are very unlikely determinants of policies affecting compulsory education.¹⁸ However, as Eisenberg (1988) noted, laws introducing compulsory schooling in the United States in the 1880s were passed after opposition to such laws was limited.¹⁹ To formally test for the potential endogeneity of the passage of compulsory schooling laws, we follow Landes and Solmon (1972). Using a broader set of reforms, including those that did not imply changes to compulsory schoolings for cohorts in our sample (see Table S6, Online Resource 1), we estimate Eq. (3) and check whether reforms are able to predict past migration rates or past educational attainment. Results displayed in Table 3 show that none of the estimated coefficients are significant at the 10 % level. Hence, we find no evidence that compulsory education reforms in Europe were passed as a reaction to changes in migration or educational attainment.

Regarding omitted variables, to proxy labor market conditions in our specification, we control for differences in cohort-specific unemployment rates in the year before migration. Our estimated coefficients remain unchanged, suggesting that differences in labor market conditions between countries are not driving education reforms implemented in the past. One might think that unemployment rates at the time of the reform could be a relevant omitted variable; however, those are unlikely to affect migration patterns in 2008–2012, particularly after contemporaneous unemployment is controlled for. In general, to address these concerns, one would like to know more about the determinants of education reforms.²⁰

To this end, our exogeneity check, following Lleras-Muney (2002) and as specified in Eq. (4), tests which variables are correlated with changes in the length of compulsory schooling. Again, we use the broader set of reforms (see Table S6, Online Resource 1) and regress the length of compulsory schooling on a variety of potentially related variables measured in the reform year regarding demographics (log migration out-flow, population growth), urban development (percentage urban population), education (average years of schooling), and economic development (GDP per capita). We also include 5- and 10-year variation rates for all variables, as well as year and country fixed effects. Table 4 shows the results from this estimation. We estimate a significant relationship between the timing of compulsory schooling reforms and the growth rate

¹⁸ To our knowledge, the only example of a government explicitly providing training such that its citizens become better migrant workers is the training of nurses in the Philippines (see Lorenzo et al. 2007). However, the effect of such specialized training of adult workers on migration is much more immediate than the one resulting from education reforms regarding compulsory schooling.

¹⁹ Opposition tended to arise because of limitations to child labor and parental freedom of decision, as well as taxation for financing schools; see Eisenberg (1988) and Butts and Cremin (1953).

²⁰ To the best of our knowledge, no established theory in political economy addresses education reforms. However, increasing the length of compulsory education requires additional resources, such as teachers and facilities. Thus, the actual implementation of those reforms depends on the availability of resources.

Table 3 Exogeneity check: Predictive power of compulsory schooling laws on past outcomes: Migration and educational attainment

	(1)	(2)	(3)
A. Migration			
Years of compulsory schooling	0.156 (0.124)	0.046 (0.15)	0.111 (0.124)
Contemporaneous controls	Yes	No	Yes
Controls: 10-year variation rate	No	Yes	Yes
Number of observations	710	546	546
R^2	.837	.862	.880
B. Educational Attainment			
Years of compulsory schooling	-0.028 (0.069)	-0.004 (0.065)	-0.024 (0.06)
Contemporaneous controls	Yes	No	Yes
Controls: 10-year variation rate	No	Yes	Yes
Number of observations	513	492	492
R^2	.956	.952	.958

Notes: The dependent variable is the logarithm of outmigration in panel A and average years of education measured 10 years before the reform in panel B. All regressions include dummy variables for year and country. Contemporaneous controls are logarithm of outmigration, population growth, urban over total population, GDP per capita, and average years of schooling, as well as their variation rates over the past 10 years. In panel A, we exclude the variation rate of the logarithm of outmigration; in panel B, we exclude average years of education. Standard errors, shown in parentheses, are clustered by country.

in urban population 10 years prior to reforms. To ensure that our main result is not altered by this relationship, we conduct two robustness checks. First, we check which countries are driving this result and rerun our main estimation excluding them (Malta, Norway, Portugal, and Sweden). Our results remain robust (see Table S7, Online Resource 1). Second, we include as an additional control variable into our main regression the 10-year variation rate in urban population measured at the time each cohort entered compulsory schooling. Results displayed in Table S8 of Online Resource 1 show that our coefficient of interest is not very different from the one in our main specification.

Our results suggest that compulsory schooling laws that were passed during the second half of the twentieth century in Europe are exogenous to changes in emigration rates or educational attainment. However, we cannot rule out that changes in migration rates or educational attainment led to governments adopting other policies that affected returns to migration (e.g., certain labor market policies). Nevertheless, for our main result to be due to alternative policies instead of education reforms, such policies would have had to affect returns to migration of different cohorts distinctively, and they also would have had to occur simultaneously with education reforms.

Other Robustness Checks

After the end of communism, some countries in Central and Eastern Europe split. However, they share a common past, including education reforms that occurred before 1990. To account for this, we cluster standard errors by country using the following

Table 4 Exogeneity check: Timing and content of education reforms

	(1)	(2)	(3)
GDP per Capita	-1.00e-05 (0.00002)	-0.00003 (0.00002)	-0.00003 (0.00003)
Population Growth (%)	0.205 (0.254)	0.173 (0.322)	0.067 (0.321)
Urban Population (%)	0.048 (0.038)	0.034 (0.037)	0.028 (0.037)
Out-migration (log)		0.072 (0.197)	0.101 (0.195)
Average Years of Schooling			0.253 (0.314)
5-Year Variation Rates			
GDP per capita	0.707 (0.586)	0.812 (0.940)	0.630 (1.052)
Population growth (%)	-0.225 (0.228)	-0.195 (0.238)	-0.136 (0.229)
Urban population (%)	2.784 (3.169)	1.641 (3.864)	2.534 (3.947)
Out-migration (log)		0.080 (0.114)	0.086 (0.103)
10-Year Variation Rates			
GDP per capita	-0.115 (0.384)	-0.202 (0.538)	-0.150 (0.580)
Population growth (%)	0.025 (0.109)	0.043 (0.113)	0.078 (0.106)
Urban population (%)	4.034 (2.462)	5.468 [†] (2.867)	4.997 [†] (2.863)
Out-migration (log)		-0.050 (0.176)	-0.089 (0.176)
Average years of schooling			0.246 (1.388)
Number of Observations	587	492	492
R ²	.746	.755	.757

Notes: The dependent variable is the number of years of compulsory schooling in force in a particular year. All regressions include dummy variables for year and country. Standard errors, shown in parentheses, are clustered by country.

[†] $p \leq .10$

country definitions pertinent to 1990: (1) the Soviet Union (Estonia, Latvia, Lithuania), (2) Yugoslavia (Croatia, Macedonia, and Slovenia), and (3) Czechoslovakia (Czech Republic and Slovakia). Table S9 shows that results from this estimation are almost indistinguishable from those in our main specification. In the wake of this same event, some of these countries reduced the length of compulsory schooling. Given that the end of the Cold War also led to important migration outflows, one might wonder whether such reductions in compulsory schooling could be driving our results. However, this would be possible only if differences in migration outflows by age group persisted to the present. We rerun our main estimation excluding all countries that reduced the length of compulsory schooling at some point in time (see Table S10, Online Resource 1). Our coefficient of interest is somewhat smaller, but the difference is not statistically significant.

As mentioned earlier, four countries in our sample (Croatia, Liechtenstein, Macedonia, and Norway) did not belong to the EU during 2008–2012 and hence might have faced different migration restrictions than other countries in our sample. We repeat our estimation excluding these countries. Table S11 shows that results remain robust. Finally, evidence suggests that some countries implemented reforms gradually over many years. In this case, our assignment of years of compulsory education to cohorts is somewhat inaccurate. Maybe not surprisingly,

when we run our main estimation excluding those countries (Norway and Finland), our coefficients are somewhat larger, although the difference is not statistically significant (see Table S12).

Conclusion

Previous research has used education reforms to test whether more education is associated with more or less within-country mobility, finding mixed results. We consider an international context with basically unrestricted mobility—namely, Europe—and test for the direct effect of education policies on migration. We show that increases in the length of compulsory schooling reduce the propensity to migrate across European countries. One additional year of compulsory education reduces migration of affected cohorts in a given year by 9 %. After we perform a variety of exogeneity checks, our results show that European lawmakers did not pass education reforms as a reaction to changes in migration rates or educational attainment.

We also find that additional years of compulsory education effectively translated into higher average educational attainment, and hence part of the effect of compulsory schooling reforms on migration operates through changes in educational attainment. In this context, identification comes from individuals at the low to medium part of the education distribution, whose educational attainment changes as a result of the reforms (the so-called compliers). Hence, although our results show that governments can be fairly confident that more years of compulsory schooling are unlikely to be lost to brain drain, this might not apply to reforms of higher education. How education policies that alter individuals' propensity to go into higher education affect migration choices seems an interesting question for future research.

In 2014, unemployment was 24 % to 26 % in Spain and Greece; Germany had one of the lowest unemployment rates, at 5 %. European authorities are trying to foster mobility to reduce such large differences. Machin et al. (2012) argued that lower average educational attainment in Europe compared with the United States might be a reason for the relatively low labor mobility in Europe. Our results, however, suggest that different distributions of education matter—in particular, the higher share of medium-educated individuals in Europe compared with the United States.²¹ In this context, if high degree recognition costs prevent medium-educated individuals from migrating (see Stark 1991), then reducing those costs could be key for increasing returns to migration and fostering mobility. Finally, one of the top priorities of the European Union's 2020 agenda is to improve educational outcomes. Education policies that lead to a more-educated and better-prepared workforce are essential for future growth and job creation. Our results show that increasing educational attainment while increasing labor mobility across countries requires coordinated education and labor market policies.²²

²¹ Alternative explanations for the low European mobility focus on relatively high unemployment benefits (Antolin and Bover 1997) and stronger employment protection (Belot 2007) in European countries compared with the United States.

²² Boldrin and Canova (2001) argued that EU policies aimed at achieving convergence in economic conditions across Europe seem to discourage migration.

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