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ORIGINAL ARTICLE

Education and internal migration: evidence from a child labor reform in Spain

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Abstract

We exploit a country-wide child labor regulation that eliminated the difference in school/work alternatives for children born at the beginning and the end of the year to identify the causal effect of education on migration at low levels of schooling. By not relying on changes to the school system, we are more confident that our results are not driven by unobserved changes in school quality evolving differentially across regions. The results of a difference-in-differences methodology combined with an exploration of maternal characteristics and a regression discontinuity design suggest that internal migration hardly changed after the reform. A consideration of the external validity of this finding is also provided.

Keywords Internal migration · Education · Child labor reform · Spain

JEL Classification J61 · R23 · I20

1 Introduction

In almost all countries, internal migrants appear as favorably selected in terms of education (e.g., Bernard and Bell 2018). A number of reasons (reviewed below) suggest that the positive correlation between education and migration might be the result of a causal relationship. In that case, migration would constitute an additional return to schooling, as education would afford individuals with a greater range of choices over

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jobs and locations and would contribute to the maximization of national income by improving the matches of workers to jobs. However, the positive correlation between education and migration might be the result of unobserved individual characteristics influencing both outcomes (e.g., McHenry 2013). As the returns to schooling play a crucial role in many discussions of public policy, it is important to find out whether schooling causes migration.

Most of the available evidence on the causal effect of education on migration is derived from supply-side sources of variation in educational attainment, attributable to reforms in compulsory schooling laws or the types of degrees granted (e.g., Machin et al. 2012; McHenry 2013; Weiss 2015; Haapanen and Böckerman 2017; Barone et al. 2019; Gevrek et al. 2021). However, those reforms may have coincided with changes that altered the (unobserved) quality of schooling or with other changes at the area level that could potentially confound the estimated effect of education (e.g., Stephens and Yang 2014; Sansani 2015). In this paper, we exploit a country-wide labor market reform that is free from those concerns.

In March 1980, a new law regulating labor relations (*Estatuto de los Trabajadores*, ET) was passed by the Spanish parliament that raised the legal working age (LWA) from 14 to 16 years without changing the years of compulsory education, set at 8 and starting in the calendar year children turned 6. Since children were allowed to leave school as soon as they reached the LWA, before this reform individuals born at the beginning of the year (between January and May) found themselves legally able to work before completing compulsory education, as they turned 14 before the end of the school year in June. Individuals born at the end of the year reached the LWA after completing compulsory education, as they turned 14 after the end of the school year in June. In 1980, when the LWA increased to 16, this difference in incentives for leaving school to work between those born at the beginning and the end of the year disappeared. Indeed, to be legally able to work individuals needed to be 16, and this occurred more than a year after completing compulsory education.

It might seem that the increase in LWA only delayed the decision to drop out of school by two years for both groups of individuals. However, when comparing outcomes before and after the reform, Del Rey et al. (2018) find a clear increase in the demand for education of individuals born at the beginning of the year (treatment group) relative to individuals born at the end of the year (control group). The authors show that the reason is that some children are impatient at the age of 14 and fail to realize that, if they finish compulsory education, their best choice will be to complete some form of post-compulsory education subsequently. Thus, by increasing the number of children attaining compulsory education, the reform made it possible for some of them to complete post-compulsory education.

Del Rey et al. (2018) and Bellés-Obrero et al. (2021a, b) exploit this reform to identify the causal effect of education on a number of outcomes. Here, we use it to provide further evidence on the causal effect of education on the propensity to migrate within a country. From the research design view, this reform is attractive for at least three reasons. First, by looking at differences between individuals born at the beginning and the end of the year, changes which occur across cohorts (e.g., minimum wage increases raising the incentive to work) can be accounted for in estimation. Second, the identification of the effects of this reform does not rely on changes which occur across regions over time, so we can control flexibly for different trends across regions in the factors affecting different birth cohorts. And third, the reform was applied in the middle of the more than 20-year period of validity of the Spanish educational system introduced in the 1970–1971 school year, so we can be confident that there were no other educational changes that could undermine our identification strategy.

Our approach is close in nature to the approaches of Malamud and Wozniak (2012) and Sakai and Masuda (2020), who identify the impact of education on migration from variations in the demand for education. But while Malamud and Wozniak (2012) estimate effects for the upper end of the education distribution and Sakai and Masuda (2020) study international migrations, we focus on the effects on within-country mobility of additional schooling at lower levels. Hence, our results seem more applicable to developing countries, where the average educational attainment is lower than in developed countries.

We find that the increase in LWA raised the average years of schooling of individuals born at the beginning of the year by 0.03 to 0.12 years, depending upon the population subgroup under study, but that these increases left internal migration patterns, both long- and short-distance, essentially unchanged. Thus, our results challenge a widespread believe in a positive effect of education on migration, of which we find no evidence when education is increased from low levels.

The rest of the paper is organized as follows: Section 2 provides background. Section 3 describes the data and the selection of the sample. Section 4 defines the internal migration measures. Section 5 discusses the empirical strategy. Section 6 presents the results. Finally, Sect. 7 summarizes the paper and considers the external validity of the findings. The online appendix discusses the sensitivity of our results to a variety of alternative specifications.

2 Background

2.1 The reform and its educational context

In the Spanish educational system, children from the same calendar year start school the same school year. This starts in September and runs through to June. Consequently, children born at the beginning of the year finish the school year at an older age (in months) than those born at the end of the year.

The *Ley General de Educación*, introduced in the 1970–1971 school year, specified the obligatory nature of primary education. This comprised 8 schooling years and started in the calendar year children turned 6. However, children were allowed to leave school as soon as they reached the LWA. As this was set at 14 years in 1944,[1] before the passage of the ET children born at the beginning of the year had the legal possibility of leaving school to work before completing compulsory education.

After completing primary education, a student could choose between the Bachillerato (a three-year cycle followed by a one-year specialized track to attend university), or vocational training (a two-year cycle followed by another cycle of two or three

[1] Younger children were permitted to work in agriculture and family shops.

years). The first stage of vocational training was compulsory for students not taking the Bachillerato, but the enforcement of this regulation was not very effective until a new law (LOGSE, introduced starting in the 1991–1992 school year) raised the years of compulsory education to 10 for all students (Egido 1994).

The ET, passed in March 1980, prohibited child labor under the age of 16.[2] Thus, children could no longer work as an alternative to attending school until the age of 16. This change meant that for children born at the beginning of the year, they no longer had the legal possibility of leaving school to work before completing primary education. Although those children could join the labor market at the age of 16 before attaining the first stage of secondary education, by increasing the number of them attaining primary education, the law made it possible for some of them to complete secondary (or even higher) education. Cadena and Keys (2015) and Del Rey et al. (2018) rationalize this behavior based on impatience and time-inconsistent preferences. The reform also meant that for children born at the end of the year they no longer had the legal possibility of leaving school to work before completing the first stage of secondary education.

The available evidence confirms that the reform was fully effective in reducing formal child labor for the cohorts that turned 14 after the reform. For example, Del Rey et al. (2018) find that pre-reform cohorts had a probability of contributing to the Social Security before age 16 of 9.22% for boys and 7.57% for girls. For the post-reform cohorts, those figures dropped to 0.17% for both boys and girls.

2.2 Internal migration in Spain (1960–2011): stylized facts

Internal migration in Spain has varied widely over the last decades. In the 1960s and early 1970s, the rapid and polarized economic growth caused intense transfers of labor from rural regions in response to higher wages and more job opportunities in the more industrialized regions of the Basque Country, Catalonia, and Madrid (e.g., Ródenas 1994; Bover and Velilla 2005). For some years later, internal migration slowed down, notwithstanding persistent cross-regional differences in wages and unemployment rates. The dramatic increase in the national unemployment rate between 1975 and 1985 and the spatial redistribution of industrial activities brought about by the energy crisis have been blamed (among other factors) for this reduction in migration (e.g., Bentolila 1997; Paluzie et al. 2009).

In the mid-1980s, both interregional and intraregional migration started to grow. Nevertheless, the profile of the migrant had changed with respect to that of earlier decades, and net interregional migration was very low (Ródenas 1994; Antolin and Bover 1997; Maza and Villaverde 2004; Hierro 2009). In addition to the traditional income-maximizing migrant, there were workers migrating toward other regions in search of cheaper housing and preferred amenities, as well as toward larger towns of the same region where new jobs in the service sector were being created. Some correlational studies (e.g., Antolin and Bover 1997; Cabrer et al. 2009) have documented

[2] The only exception stated in the law concerned the participation of minors in specific public shows, that remained subject to authorization by the competent authorities provided that it did not endanger their physical health or their professional and human training (Art. 6.4). Although the ILO's Convention No. 138, ratified by Spain in May 1978, specifies 13 years as the age above which a person may participate in "light work," such exception has no place in the ET.

the importance for these migrations of the individual's educational attainment. Other studies (e.g., Bover and Arellano 2002; Paluzie et al. 2009) have pointed out the importance of the expansion of the service sector, which took place in all regions and mainly in large towns, to account for the increasing dispersion of migratory destinations and the intensification of short-distance moves.

The intense recession of 2008 reduced only slightly the internal migration of the Spanish-born population (Minondo et al. 2013).[3] It also modified the directionality of migration flows, reducing the appeal of territories specialized in the construction sector, which were more hit by the bursting of the housing bubble (Hierro et al. 2019). Perhaps as a consequence of workers in the construction sector having inferior resources to migrate, women and the more educated increased their share in internal migration during the recession (Minondo et al. 2013).

2.3 Education and internal migration

The theoretical literature has pointed out several channels through which education can influence migration, some of which are contradictory. On the one hand, as education progresses and specializes, the market necessary to secure a job widens, leaving some individuals to migrate (Schwartz 1973). Education provides information and skills that reduce the cost of obtaining information from alternative locations, making more educated individuals more responsive to the potential gains from moving (Levy and Wadycki 1974). Education may also reduce the importance of tradition and family ties and increase awareness of other cultures and localities (Greenwood 1975). Moreover, education leads to higher wages, which covers the out-of-pocket cost of migration more easily (Malamud and Wozniak 2012), and rises the opportunity cost of not working following a job loss (Mauro and Spilimbergo 1999). On the other hand, staying in school allows individuals to strengthen their local networks, which might help them find jobs locally (McHenry 2013). Education may also help people make better migration decisions, thereby reducing their total number of moves over the lifetime (Weiss 2015). In addition, living surrounded by better-educated peers can lead to improved civic societies and reduced crime rates, which could potentially reduce (out-)migration (Aparicio and Kuehn 2017).[4]

The available estimates of the effect on migration of additional schooling from low levels are mixed. Machin et al. (2012), who exploit a Norwegian reform that increased the years of compulsory education from 7 to 9, Weiss (2015), who exploits compulsory schooling reforms across 8 European countries, and Rauscher and Oh (2021), who use state-level variation in early US compulsory schooling laws, find a positive impact of education on interregional migration. In contrast, McHenry (2013) estimates a negative effect from variations in US compulsory schooling laws in the twentieth century, and Barone et al. (2019) find a negative though statistically insignificant effect using an Italian reform that increased the years of compulsory education from 5 to 8.

[3] The foreign-born population is not included in our sample.

[4] Location-specific factors do also play a role in the migration decision (e.g., Borjas et al. 1992). However, we estimate with region fixed effects to account for different migration propensities across locations.

Education has been considered a counteracting factor to the detrimental effect of distance on migration by making individuals less sensitive to some costs of migrating (e.g., Schwartz 1973; Bauernschuster et al. 2014). However, education can also foster short-distance moves if, as noted by Denslow and Eaton (1984), it raises the ability to process information from nearby sources more than that from far away sources. Available estimates of the effect on distance moved of additional schooling from low levels suggest that education reduces the distance moved, especially among male migrants (Rauscher and Oh 2020).

Some previous studies underline differences between men and women in the effect of education on migration (e.g., Melzer 2013; Barone et al. 2019; Lovén et al. 2020; Sakai and Masuda 2020; Gevrek et al. 2021). Several reasons may account for the differences. For one thing, a reform may have non-identical effects on boys' and girls' education. Second, education may trigger "second round" effects. For example, if in 1980 Spain the economic benefits from marriage were mainly a result of specialization and were particularly large if the woman had low wage potential, education and marriage would be positively linked among men and negatively linked among women (Becker 1991). The increase in the proportion of single women would increase migration associated with employment (Mincer 1978), but would reduce migration associated with marriage and child bearing (Mulder and Wagner 1993). Another possible reason is the social context in which a reform is implemented. As discussed by Bellés-Obrero et al. (2021b), the ET was passed just a few years after the end of Franco's dictatorship, which had lasted almost 40 years and during which women's rights were generally ignored or suppressed. The end of the dictatorship progressively raised the level of gender equality and improved women's access to economic opportunities. Thus, women might have started to see migration as less necessary to weaken the social and cultural constraints that prevented their social development.

3 Data and sample selection

We pool individual-level microdata from the 2001 and 2011 Spanish population censuses, conducted by the National Statistics Institute (INE, www.ine.es). We discard the 1991 census because many of those who turned 14 after the reform may still be studying in 1991. The Spanish Labor Force Survey (LFS) underestimates the number of migrants (Martí and Ródenas 2004, 2007), and the Continuous Sample of Work Histories, which tracks work establishments' locations, does not enable the identification of migrations during childhood and raises the issue that the place of work might not be the place of residence.

We use the 5% representative sample of the 2001 census drawn by the INE, and the survey sample on which the 2011 census was based. In contrast to previous censuses, the 2011 census is not exhaustive but is based on a sampling survey aimed at 12.3% of the population. This survey oversampled municipalities with population not greater than 10,000 inhabitants, so we use the weights provided by INE to correct the overrepresentation of the rural population. As persons who moved permanently abroad are not included in the Census, the analysis is limited to internal migration.

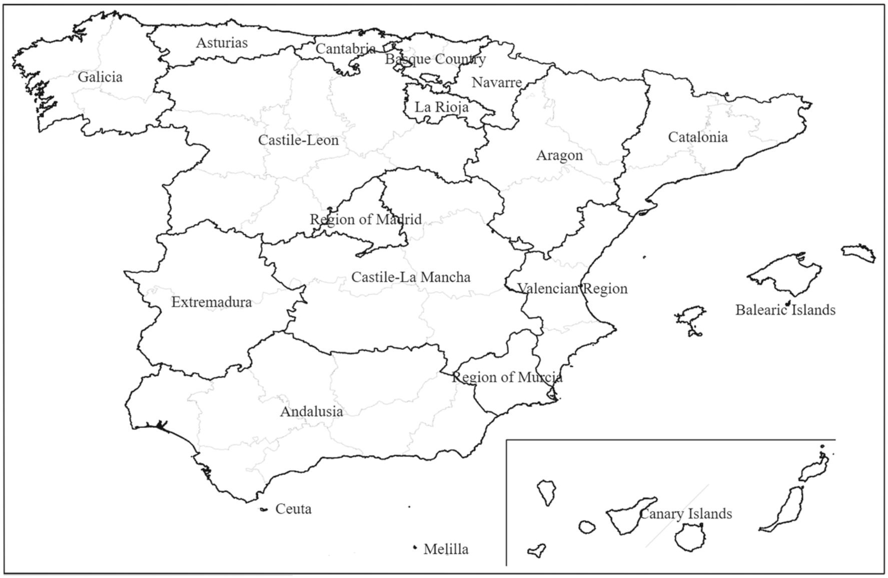

Fig. 1 Map of Spanish regions with provincial division

The 2001–2011 censuses provide comparable information on characteristics of interest to this research, such as year and month of birth, educational attainment (multiple-year increments of education or highest degree completed), and place of residence at three points in time (at the census date, 10 years prior, and at birth). For individuals who once changed their municipality, both censuses provide the year of arrival in the municipality and in the region of residence at the census date, plus the prior municipality and province. Although retrospective information on the circumstances of the migration decision is not collected, individual-level variables are unlikely to be a source of omitted variable bias as individuals are classified into treatment and control groups essentially by chance.

To identify the place of residence, the census provides the province and, for municipalities with population greater than 20,000 inhabitants, a municipality identification code.[5] Smaller municipalities are not disclosed to preserve confidentiality, although their population category is recorded in the data.

Immigrants and natives who lived abroad between birth and the census date are excluded from the sample to guarantee that this includes individuals who studied in Spain and were thus affected by the reform. We further limit the sample to individuals born in March, April, May, August, September, and October of 1957–1965 and 1967–1975. Individuals born in January and February (November and December) are excluded from the treatment (control) group because they tend to be more dissimilar

[5] Since the early 1980s Spain has been organized into 17 regions (known as autonomous communities and corresponding to EU NUTS 2 territories) and two autonomous towns (the enclaves of Ceuta and Melilla in North Africa). As shown in Fig. 1, these 17 regions are divided into a total of 50 provinces (EU NUTS 3 territories), with boundaries which were set in 1927. Each province is subdivided into a varied number of municipalities.

Table 1 Descriptive statistics. Spanish population censuses 2001–2011

	Men born in:		Women born in:	
	March–May	August–October	March–May	August–October
Year of birth	1965.8 (5.6)	1965.9 (5.6)	1965.8 (5.6)	1965.8 (5.6)
Age	41.5 (7.5)	41.5 (7.5)	41.7 (7.4)	41.6 (7.4)
Years of schooling	9.8 (3.7)	9.8 (3.6)	10.1 (3.8)	10.2 (3.8)
Interregional lifetime migrant[a]	11.9	11.6	13.9	13.5
Distance migrated[b]	413.9 (366.7)	412.4 (362.3)	397.6 (347.4)	399.2 (354.6)
Interregional 10-year migrant[a]	4.3	4.3	4.3	4.3
Intraregional lifetime migrant[a,c]	28.3	27.8	32.1	31.8
Intraregional 10-year migrant[a,d]	13.1	13.1	12.1	12.2
Observations	175,426	168,988	175,118	169,812

Sample means, with standard deviations in parentheses. The sample includes natives born in 1957–1965 and 1967–1975. See text for further details of sample selection

[a]Percent

[b]Taken over interregional lifetime migrants

[c]Taken over interregional lifetime non-migrants

[d]Taken over interregional 10-yr non-migrants

in family background characteristics (see, e.g., Buckles and Hungerman 2013 and the evidence presented in the online appendix). Individuals born in June and July are excluded because they can be misclassified into treatment and control groups as the end of the school year is not fixed and their exact date of birth is unknown.

Individuals born in 1957–1965 turned 14 before the implementation of the reform, while those born in 1967–1975 turned 14 after the implementation of the reform. The cohort born in 1966 turned 14 in 1980, the year the reform was introduced. We drop this cohort because the law was passed in March and it is not clear the degree of enforcement of the law in the months immediately after its passage.[6] We also apply the restrictions of not being a student at the census date or a soldier. Table 1 provides descriptive statistics for the analysis sample, separately for men and women. Although the treatment group is slightly larger, both groups match well in the selected characteristics.

[6] Including the 1966 cohort among those who turned 14 after the reform leaves the main conclusions intact.

4 Internal migration measures

The census microdata provide great flexibility in defining internal migrants. As for the geographic units, we group the different measures into interregional and intraregional measures, the former being more likely to capture the link between education and the ability to respond to spatial disequilibria across labor markets (Malamud and Wozniak 2012). As for time periods, we analyze migration over an individual's lifetime and over the last ten years, the latter focusing on long-term effects of the reform. For interregional lifetime migrants, we investigate whether the reform affected the distance moved.

In defining migrants, we use the indicator for once changing municipalities provided by the INE combined with the year of arrival in the municipality/region of residence at the census date. To deal with migration during childhood, which is probably non-autonomous (i.e., not decided by the individual), we rely on whether individuals were legally able to work in their year of arrival in the municipality/region of residence. Migrations by individuals legally able to work are considered as autonomous, and migrations by individuals legally unable to work as non-autonomous (Chapela 2022).[7] Thus, individuals who arrived in 1979 or earlier being 14 years old or older are classified as migrants. The same occurs if individuals arrived in 1980 or later being 16 years old or older. Non-migrants are individuals who have resided since birth in the same place and individuals who arrived in 1979 or earlier (1980 or later) being younger than 14 (16).

Interregional migration is measured in three ways. First, we create an indicator of lifetime migration, denoted *INTER1*. Second, we create an indicator for moving regions over the last ten years, denoted *INTER2*. And third, for individuals with $INTER1 = 1$, we measure the linear distance (in kilometers) between the municipality of origin and that of destination.[8] The resulting measure is denoted *KM_INTER1*.

Intraregional migration is measured in three ways. In all cases, we exclude interregional migrants to enable a cleaner comparison between intraregional migrants and non-migrants. First, for individuals with $INTER1 = 0$, we construct an indicator of intraregional lifetime migration predicated on cross-municipality movement, denoted *INTRA1*. Second, for individuals with $INTER2 = 0$, we create an indicator for moving municipalities over the last 10 years, denoted *INTRA2*. And third, following Bover and Arellano (2002), for individuals with $INTER2 = 0$, we analyze the propensity to move to small (less than 10,000 inhabitants), medium (10 to 100 thousand inhabitants), and large towns (more than 100,000 inhabitants) of the same region.

[7] Our main conclusions do not change if individuals who were legally unable to work on arrival are excluded from the sample, or if the age limit for classifying migrations as autonomous is set at 14 or 16 years regardless of the year of arrival.

[8] The municipality of destination is not recorded in the data. We assume it is the municipality of residence at the census date if the migrant has not returned to their birth region; if the migrant has returned to their birth region, it is assumed to be the municipality prior to that of residence (unless the previous municipality is in the birth region, in whose case the observation is discarded). When the municipality of origin (destination) is not greater than 20,000 inhabitants, the distance is calculated from (to) the capital city of the province of origin (destination).

5 Empirical strategy

The effects of the reform on the probability of migration and the distance moved by migrants are developed from the following model:

$$\begin{aligned}\text{Outcome}_{ict} =& \alpha + \beta_1 \text{Treatment}_{ic} + \beta_2 \text{Treatment}_{ic} * \text{Post}_c + \delta_t \\ &+ \beta_3 \delta_t * \text{Treatment}_{ic} + \theta_j + \mu_c + \upsilon_{jc} + \varepsilon_{ict}, \end{aligned} \quad (1)$$

where Treatment_{ic} and Post_c are indicator variables taking value one for individuals born in the months of March–May and for the cohorts of 1967–1975, respectively. The term δ_t is an indicator variable taking value one for observations from the 2011 census, and the interaction of δ_t with Treatment_{ic} allows for differential time effects between treatment and control groups. To reduce the risk that geographic and temporal factors could interfere with our estimates, we include θ_j, μ_c, and υ_{jc}. These terms represent region of origin, cohort (year of birth), and region-by-cohort indicator variables that account for unobserved characteristics constant across regions and cohorts and different trends across regions in the factors affecting different cohorts. For example, by excluding individuals aged 14–15 from the labor market, the reform might have created employment opportunities for older teenagers. If the magnitude of this change differed across locations, internal migration could be altered, confounding the causal effect of interest. The region of origin is the region of birth when looking at lifetime outcomes, whereas it is the region of residence 10 years prior to the census date when analyzing 10-year mobility. ε_{ict} is an error term.

The coefficient of interest, β_2, is a difference-in-differences estimate that compares the outcome of individuals born in March–May of 1967–1975 with the outcome of individuals born in the same months of 1957–1965, using individuals born in August–October as controls. Identification of the reform's effect requires that migration trends would be the same for individuals born in March–May and August–October in absence of the reform. If, according to Myers (1999), the migration of the mother fosters children's subsequent decision to migrate, one potential concern is that the mobility of the mothers in the treatment and control groups changed differentially over time. However, the evidence presented in the online appendix does not appear to challenge seriously the assumption that mothers' mobility remains stable.

Equation (1) is estimated by weighted least squares (WLS). Standard errors are clustered at cohort level. Although μ_c controls for within-cohort correlation of errors created by factors affecting the entire cohort, one might still be concerned of correlation within cohort subsets (e.g., shocks hitting individuals of the same cohort living in certain regions or born in certain months). Since the number of cohorts is not large (eighteen), we provide the wild cluster bootstrap p-value for testing the null hypothesis that $\beta_2 = 0$. We also apply the reweighting scheme suggested by Hainmueller (2012) that improves covariate balance between treatment and control groups and helps making the common trend assumption more plausible. The variables δ_t, θ_j, μ_c, and υ_{jc} have been used as covariates to apply the balance procedure.

To estimate the effects of the reform on the probability of migration to small, medium size, or large towns, we consider a multinomial choice among four different alternatives: (0) no migration, (1) migration to a small town, (2) migration to a medium size town, and (3) migration to a large town. Let Outcome_{ict} denote here a random variable taking the values $\{0,\ 1,\ 2,\ 3\}$. We model $\Pr[\text{Outcome}_{ict} = k|\mathbf{x}_{ict}]$, for $k = 0,\ 1,\ 2,\ 3$, using multinomial logit (MNL):

$$\Pr[\text{Outcome}_{ict} = k|\mathbf{x}_{ict}] = \exp(\mathbf{x}_{ict}\boldsymbol{\pi}_k) \Bigg/ \left[\sum_{h=0}^{3} \exp(\mathbf{x}_{ict}\boldsymbol{\pi}_h)\right], \tag{2}$$

where

$$\begin{aligned}\mathbf{x}_{ict}\boldsymbol{\pi}_k =& \alpha_k + \beta_{k1}\text{Treatment}_{ic} + \beta_{k2}\text{Treatment}_{ic} * \text{Post}_c + \delta_{k\,t} \\ &+ \beta_{k3}\delta_{k\,t} * \text{Treatment}_{ic} + \theta_{k\,j} + \mu_{k\,c} + \upsilon_{kjc}\end{aligned} \tag{3}$$

and $\boldsymbol{\pi}_0 = \mathbf{0}$. Estimation of (2) is carried out by maximum likelihood. The term υ_{kjc} represents region-specific linear trends in the year of birth, as more flexible functions produce convergence failures. We develop separate estimates for each of the three town of origin sizes, thus allowing for different effects of the reform depending on the town of origin size.

The partial effect of $\text{Treatment}_{ic} * \text{Post}_c$ on $p_k^i \equiv \Pr[\text{Outcome}_{ict} = k|\mathbf{x}_{ict}]$, denoted γ_k^i, is calculated as (Ai and Norton 2003, Appendix A of Dinc and Erel 2013):

$$\begin{aligned}\gamma_k^i \equiv \frac{\Delta^2 p_k^i}{\Delta\text{Treatment}_{ic}\Delta\text{Post}_c} =& \left.p_k^i\right|_{\text{Treatment}_{ic}=1,\ \text{Post}_c=1} - \left.p_k^i\right|_{\text{Treatment}_{ic}=0,\ \text{Post}_c=1} \\ &- \left.p_k^i\right|_{\text{Treatment}_{ic}=1,\ \text{Post}_c=0} + \left.p_k^i\right|_{\text{Treatment}_{ic}=0,\ \text{Post}_c=0}.\end{aligned} \tag{4}$$

Simplification occurs because $\left.p_k^i\right|_{\text{Treatment}_{ic}=0,\ \text{Post}_c=1} = \left.p_k^i\right|_{\text{Treatment}_{ic}=0,\ \text{Post}_c=0}$, so letting ξ_{ict}^k denote expression (3) with the term $\beta_{k2}\text{Treatment}_{ic} * \text{Post}_c$ excluded and Treatment_{ic} set equal to 1, we get

$$\gamma_k^i = \exp\left(\beta_{k2} + \xi_{ict}^k\right) \Bigg/ \left[\sum_{h=0}^{3} \exp\left(\beta_{h2} + \xi_{ict}^h\right)\right] - \exp\left(\xi_{ict}^k\right) \Bigg/ \left[\sum_{h=0}^{3} \exp\left(\xi_{ict}^h\right)\right] \tag{5}$$

We estimate $E\left[\gamma_k^i\right]$, and to avoid imposing nonlinear constraints, we evaluate whether $E\left[\gamma_k^i\right] = 0$ by testing the joint hypothesis

$$\beta_{12} = \beta_{22} = \beta_{32} = 0, \tag{6}$$

as $\gamma_k^i = 0\ \ \forall i$ when (6) holds.

6 Results

6.1 Years of schooling

We start showing the results of estimating Eq. (1) with Outcome$_{ict}$ representing years of schooling, these coded into 0, 2.5, 6, 8, 10, 11.5, 12, 15, 17, 18, and 20 years to roughly correspond to multiple-year increments of education or degrees of the educational system. Table 2 presents the results separately by sex and "social development" of the birth region in 1980. Using data from the LFS, we classify as "more socially developed" those regions that score high both in total activity rate and secondary educational attainment for women (Madrid, Basque Country, Cantabria, Canary Islands, Catalonia, Balearic Islands, La Rioja, Asturias, and Navarre). The remaining regions (Andalusia, Ceuta, Melilla, Aragon, Castile-Leon, Castile-La Mancha, Extremadura, Galicia, Murcia, and Valencia) are classified as "less socially developed."

As expected, the increase in LWA raised the years of schooling of individuals born at the beginning of the year in the post-reform cohorts. On average, the increase was of 0.10 years for men and 0.09 years for women. These figures are close to (the absolute value of) the effect of the abolition of compulsory conscription in France on men aged 17–23: 0.11 years (Maurin and Xenogiani 2007). The working paper (González-Chapela et al. 2021) shows that the bulk of the increase in education occurred at low levels of schooling.

For men, the increase in years of schooling was similar across regions, whereas for women it was geographically concentrated: Nearly zero for women born in more socially developed regions and 0.12 years for women born in less socially developed ones. A tentative explanation for these findings is that young women in more socially developed regions had a smaller margin of improvement (they were the most educated group, having 11 years of schooling on average). Furthermore, we show in the online appendix that mothers in the treatment group attained less schooling on average after the reform, particularly if the individual was born in more socially developed regions, which may have counteracted the reform's positive effect on education.

6.2 Interregional migration

The results of estimating Eq. (1) with Outcome$_{ict}$ representing *INTER1*, *INTER2*, and *KM_INTER1* are shown in Panel 1 of Table 3. For men, the increase in LWA appears to have exerted small, statistically insignificant effects on both the probability of long-distance migration and the distance moved by long-distance migrants. The results for women are similar except those in column (4), where women born at the beginning of the year appear 0.51 percentage points (ppt) less likely of being a lifetime migrant in the post-reform cohorts, a 4% drop attaining significance at 10%.

If the reform had truly reduced the probability of migration among women, we should observe a larger reduction among women born in less socially developed regions (where the increase in years of schooling was the strongest), and a much smaller one among women born in more socially developed regions (where the increase in years of schooling was nearly zero). Panels 2 and 3 of Table 3 show that this is not the case.

Table 2 Changes in years of schooling

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Born in more SDR	Born in less SDR	All	Born in more SDR	Born in less SDR
Treatment	− 0.089***	− 0.042	− 0.117***	− 0.116***	− 0.021	− 0.172***
	(0.019)	(0.031)	(0.026)	(0.036)	(0.036)	(0.040)
	[0.001]	[0.214]	[0.001]	[0.012]	[0.572]	[0.008]
Treatment*Post	0.099***	0.093**	0.095***	0.090**	0.033	0.118**
	(0.016)	(0.035)	(0.023)	(0.039)	(0.048)	(0.045)
	[0.000]	[0.017]	[0.001]	[0.041]	[0.517]	[0.027]
Census, birth region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	344,414	135,863	208,551	344,930	136,271	208,659
Mean of schooling years	9.911	10.567	9.453	10.244	10.974	9.747

WLS estimates. Each column is a separate regression. "Treatment" are individuals born in March–May and the control group are those born in August–October. Cohorts 1957–1965 and 1967–1975, the latter indicated by "Post". Standard errors clustered at cohort level are in parentheses and wild bootstrap p-values in brackets. More socially developed regions (SDR) are Madrid, Basque Country, Cantabria, Canary Islands, Catalonia, Balearic Islands, La Rioja, Asturias, and Navarre. *, **, and ***: Conventionally significant at 10, 5, and 1%

Table 3 Changes in interregional migration

	Men			Women		
	(1) *INTER1*	(2) *INTER2*	(3) *KM_INTER1*	(4) *INTER1*	(5) *INTER2*	(6) *KM_INTER1*
Panel 1: All						
Treatment	0.459	0.031	7.974	0.491**	− 0.095	6.619
	(0.286)	(0.156)	(6.042)	(0.213)	(0.182)	(7.057)
	[0.156]	[0.865]	[0.213]	[0.061]	[0.636]	[0.408]
Treatment*Post	− 0.202	0.129	− 8.869	− 0.508*	− 0.089	− 11.842
	(0.247)	(0.177)	(7.409)	(0.260)	(0.176)	(7.243)
	[0.452]	[0.486]	[0.263]	[0.061]	[0.651]	[0.126]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	344,414	344,414	35,734	344,930	344,930	42,062
Mean of dep. var	11.503	4.412	453.231	13.178	4.413	428.501
Panel 2: Born in more SDR						
Treatment	0.216	− 0.103	9.801	0.353	0.272	− 14.750
	(0.267)	(0.241)	(18.917)	(0.468)	(0.312)	(14.088)
	[0.413]	[0.658]	[0.630]	[0.473]	[0.411]	[0.335]
Treatment*Post	− 0.158	0.193	− 33.813	− 0.511	− 0.498	− 0.865
	(0.325)	(0.237)	(21.687)	(0.424)	(0.337)	(9.807)
	[0.628]	[0.439]	[0.157]	[0.270]	[0.171]	[0.927]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,863	135,863	12,787	136,271	136,271	15,057
Mean of dep. var	9.771	4.541	462.243	11.144	4.592	436.936
Panel 3: Born in less SDR						
Treatment	0.593	0.061	8.105	0.553*	− 0.355	14.982*
	(0.422)	(0.110)	(7.259)	(0.301)	(0.217)	(8.413)
	[0.220]	[0.629]	[0.290]	[0.106]	[0.145]	[0.059]

For women born in more socially developed regions, the estimated effect of the reform (-0.51) is even a bit larger (in absolute value) than the effect for women born in less socially developed regions (-0.44).

We could alternatively interpret these findings as suggesting that the increase in years of schooling counteracted a tendency to migrate less between regions by individuals born at the beginning of the year in the post-reform cohorts. However, the similarity of the estimated effects for women born in more and less socially developed

Table 3 (continued)

	Men			Women		
	(1) *INTER1*	(2) *INTER2*	(3) *KM_INTER1*	(4) *INTER1*	(5) *INTER2*	(6) *KM_INTER1*
Treatment*Post	− 0.165	0.138	4.702	− 0.440	0.228	− 15.174
	(0.360)	(0.265)	(8.225)	(0.350)	(0.201)	(10.691)
	[0.666]	[0.612]	[0.576]	[0.238]	[0.293]	[0.181]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	208,551	208,551	22,947	208,659	208,659	27,005
Mean of dep. var	12.713	4.323	448.676	14.562	4.291	424.320

WLS estimates. Each column of each panel is a separate regression. The dependent variable in columns (1), (2), (4), and (5) is the binary indicator shown in the column header scaled as a percentage. Columns (3) and (6) include lifetime migrants only. "Treatment" are individuals born in March–May and the control group are those born in August–October. Cohorts 1957–1965 and 1967–1975, the latter indicated by "Post". Standard errors clustered at cohort level are in parentheses and wild bootstrap *p*-values in brackets. More socially developed regions (SDR) are Madrid, Basque Country, Cantabria, Canary Islands, Catalonia, Balearic Islands, La Rioja, Asturias, and Navarre. *, **, and ***: Conventionally significant at 10, 5, and 1%

regions suggests that if education fostered long-distance migration, the effect was very small. For all these reasons, the safest conclusion appears to be that the increase in LWA left essentially unchanged the patterns of long-distance migration.

Following Barone et al. (2019), we also investigated whether migration from poorer to richer regions (or vice versa) changed as a consequence of the reform. To do this, we classified regions according to their GDP per capita in 1980,[9] and for individuals born in poorer (richer) regions, we redefined *INTER1* to indicate migration over the lifetime to a richer (poorer) region. We found no evidence that the reform had an impact on poorer-to-richer migration or vice versa (results not shown).

6.3 Intraregional migration

Panel 1 of Table 4 presents the results for *INTRA1* and *INTRA2*. For men, the estimated effects are small and statistically insignificant. For women, they are larger and statistically different from zero at or around 5%. Column (3) suggests that women born at the beginning of the year are 0.84 ppt (or 3%) more likely of having moved municipalities within their birth region in the post-reform cohorts, while column (4) suggests that those women are 0.47 ppt (or 4%) more likely of having moved municipalities within their birth region in the last 10 years.

Panel 2 (3) of Table 4 shows the results for individuals born in more (less) socially developed regions. Again, the estimated effects for men are small and statistically insignificant, as they are for women born in less socially developed regions. We see

[9] The data on GDP per capita are from Carreras and Tafunell (2005, Table 17.27).

Table 4 Changes in intraregional migration

	Men		Women	
	(1) *INTRA1*	(2) *INTRA2*	(3) *INTRA1*	(4) *INTRA2*
Panel 1: All				
Treatment	0.647*	0.130	− 0.298	− 0.448*
	(0.343)	(0.223)	(0.408)	(0.239)
	[0.067]	[0.549]	[0.547]	[0.092]
Treatment*Post	− 0.299	0.076	0.843**	0.507**
	(0.431)	(0.278)	(0.366)	(0.235)
	[0.527]	[0.783]	[0.022]	[0.049]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes
Observations	304,077	329,515	297,589	330,088
Mean of dep. var	26.027	13.150	28.984	12.372
Panel 2: Born in more SDR				
Treatment	0.364	0.299	− 0.585	− 0.959**
	(0.532)	(0.447)	(0.662)	(0.392)
	[0.511]	[0.530]	[0.417]	[0.031]
Treatment*Post	− 0.181	− 0.051	1.391**	1.301**
	(0.650)	(0.376)	(0.591)	(0.471)
	[0.786]	[0.892]	[0.035]	[0.009]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes
Observations	121,088	129,200	118,973	129,584
Mean of dep. var	30.326	16.573	32.918	15.585
Panel 3: Born in less SDR				
Treatment	0.826*	0.029	− 0.106	− 0.117
	(0.418)	(0.289)	(0.466)	(0.203)
	[0.069]	[0.929]	[0.833]	[0.552]
Treatment*Post	− 0.344	0.140	0.417	− 0.076
	(0.436)	(0.368)	(0.491)	(0.265)
	[0.439]	[0.717]	[0.428]	[0.788]
Census, origin region, cohort, and region-by-cohort FE	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes
Observations	182,989	200,315	178,616	200,504
Mean of dep. var	22.922	10.765	26.199	10.192

WLS estimates. Each column of each panel is a separate regression, where the dependent variable is the binary indicator shown in the column header scaled as a percentage. Interregional migrants are excluded. “Treatment” are individuals born in March–May and the control group are those born in August–October. Cohorts 1957–1965 and 1967–1975, the latter indicated by “Post”. Standard errors clustered at cohort level are in parentheses and wild bootstrap *p*-values in brackets. More socially developed regions (SDR) are Madrid, Basque Country, Cantabria, Canary Islands, Catalonia, Balearic Islands, La Rioja, Asturias, and Navarre. *, **, and ***: Conventionally significant at 10, 5, and 1%

that the increase in women's intraregional migration rates is driven by women born in more socially developed regions, whose demand for education hardly changed as a consequence of the reform.

Average partial effects on the probabilities of no migration, migration to a small town, migration to a medium size town, or migration to a large town are presented in Table 5 separately by town of origin size and sex. Although a few β_{k2} appear statistically significant, once we account for few clusters the null hypothesis (6) is only weakly rejected for women living in large towns (column 6). Among those women, those born at the beginning of the year are 0.85 ppt more likely of migrating to a medium size town (a 12% increase). This increase is driven by women born in more socially developed regions (results not shown), who are 1.18 ppt more likely of doing so (vs. 0.55 ppt in the case of women born in less socially developed regions).

All these findings suggest that the reform left unaffected the patterns of short-distance migration of both men and women. However, an alternative interpretation is that the increase in schooling counteracted a tendency to migrate more within regions by individuals born at the beginning of the year in the post-reform cohorts. While we cannot yet rule out this interpretation, it begs the question of what caused the general increase in intraregional migration of individuals born at the beginning of the year.

6.4 Supplementary analyses

We have investigated whether differences in maternal characteristics for births in March–May and August–October changed between the pre-reform and post-reform cohorts using data from the 1991 Socio-Demographic Survey.[10] Section A.1 of the online appendix shows that mothers' mobility remains stable when making the before-after comparison, although mothers in the treatment group attained less schooling on average after the reform. Table A4 in the online appendix shows that the effects yielded by a regression discontinuity design do not differ much from those of the difference-in-differences methodology, giving further credence to our conclusions. Finally, we have found no evidence that the laws of linguistic normalization passed by some Spanish regions in 1983 interfere with our estimates; see section A.3 of the online appendix.

7 Conclusion

We have investigated the internal migration effects of a child labor regulation introduced in Spain in 1980 that raised the LWA from 14 to 16 years, while the school-leaving age remained at 14. This reform eliminated the difference in school/work alternatives available to individuals born at the beginning and the end of the year on their 14th birthday, as all of them would have attained compulsory education by the time they reached the new LWA. In addition, not letting individuals born at the beginning of the year decide whether to complete compulsory education raised their levels of post-compulsory education.

[10] In the census, maternal characteristics are only available for individuals living at home with their mothers, which occurs in 17% of our sample. Birth certificate data are available starting in 1975.

Table 5 Changes in probabilities of intraregional migration (%)

Destination town	Origin town					
	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	Small	Medium	Large	Small	Medium	Large
No migration	*88.409*	*87.474*	*85.376*	*87.584*	*88.601*	*86.754*
Treatment	0.208	− 0.049	− 0.071	0.572***	0.177	0.096
	(0.151)	(0.254)	(0.218)	(0.197)	(0.192)	(0.217)
Treatment*Post	− 0.442	− 0.189	0.332	− 0.496	− 0.252	− 1.062**
	(0.297)	(0.504)	(0.408)	(0.381)	(0.441)	(0.416)
Small	*4.917*	*3.706*	*3.712*	*5.172*	*3.250*	*3.266*
Treatment	− 0.014	0.241***	− 0.034	0.026	− 0.057	− 0.041
	(0.184)	(0.083)	(0.132)	(0.167)	(0.115)	(0.099)
Treatment*Post	0.013	0.069	0.094	0.409	0.009	0.136
	(0.360)	(0.169)	(0.252)	(0.325)	(0.221)	(0.191)
Medium	*4.426*	*6.122*	*7.613*	*4.586*	*5.603*	*6.821*
Treatment	− 0.139	− 0.048	0.217	− 0.289	− 0.068	− 0.125
	(0.165)	(0.138)	(0.153)	(0.196)	(0.147)	(0.137)
Treatment*Post	− 0.204	0.206	0.064	0.407	0.060	0.848***
	(0.333)	(0.292)	(0.283)	(0.397)	(0.307)	(0.301)

Table 5 (continued)

Destination town	Origin town					
	Men			Women		
	(1) Small	(2) Medium	(3) Large	(4) Small	(5) Medium	(6) Large
Large	*2.249*	*2.699*	*3.299*	*2.658*	*2.547*	*3.158*
Treatment	− 0.055	− 0.144	− 0.112	− 0.309***	− 0.052	0.070
	(0.160)	(0.101)	(0.087)	(0.101)	(0.067)	(0.116)
Treatment*Post	0.633**	− 0.086	− 0.490***	− 0.320	0.182	0.077
	(0.302)	(0.214)	(0.181)	(0.212)	(0.143)	(0.239)
Census, origin region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Test of Eq. (6)	[0.222]	[0.535]	[0.124]	[0.299]	[0.642]	[0.094]
Observations	111,818	103,833	113,169	102,103	108,128	119,079
Log likelihood	− 510,744.8	− 870,488.0	− 1,043,575	− 468,235.5	− 803,073.2	− 974,888.6
R-squared	0.033	0.044	0.041	0.046	0.049	0.049

Weighted estimates. Each column is a separate regression, where the dependent variable indicates no migration, migration to a small town, migration to a medium size town, or migration to a large town over the last 10 years. The percentages for each outcome are in italics. For example, among men living in a small town, 88.4% did not migrate, 4.9% migrated to a small town, etc. Interregional migrants are excluded. "Treatment" are individuals born in March–May and the control group are those born in August–October. Cohorts 1957–1965 and 1967–1975, the latter indicated by "Post". Standard errors clustered at cohort level are in parentheses and Kline and Santos' (2012) score bootstrap p-values in brackets. R-squared equals one minus the ratio of the log likelihood of the fitted function to the log likelihood of a function with only an intercept. *, **, and ***: Conventionally significant at 10, 5, and 1%

In a sample of Spanish-born adults drawn from the 2001–2011 population censuses, we find that the increase in LWA raised the average years of schooling of individuals born at the beginning of the year by 0.10 in the case of men and 0.09 in the case of women. Additional investigations show that the reform had the largest impact on the years of schooling of women born in less socially developed regions, whereas it had no significant impact on the years of schooling of women born in more socially developed regions. We build on these findings to provide a causal analysis of the effect of education on migration. While the main analysis relies on the difference-in-differences methodology, an exploration of maternal characteristics and a regression discontinuity design are also conducted. Overall, our findings suggest that the increase in the demand for education brought about by the increase in LWA hardly changed the internal migration patterns of both men and women.

As to the external validity of this conclusion, some considerations are worth noting. First, the compliant population associated with the increase in LWA is individuals who are about to stop studying and start working, to whom the reform provided incentives to continue studying. In contrast, compulsory schooling laws can change the educational attainment of all individuals of a given cohort if applied effectively. Second, across European countries one additional year of compulsory schooling raises education by 0.26 years on average (Aparicio and Kuehn 2017). Although the smaller impact on schooling of the reform investigated here might explain our null results, the same reform has been shown to be associated with changes in a number of labor and health outcomes. Third, our conclusion mainly applies to individuals at the bottom of the education distribution. Higher levels of education may have unique features that make them difficult to compare with lower levels of education. And fourth, the effects of the reform occurred in a period in which the profile of the internal migrant in Spain had changed: migration became more amenity-driven and included a larger share of mainly skilled workers attracted to service sector jobs.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s13209-023-00272-4.

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Data availability The dataset analyzed in this study is constructed from publicly available data published by Spain's National Statistics Institute (INE). We are not allowed to make these data available to third parties. Instructions for how other researchers can obtain these data are available at INE website (www.ine.es).

Declarations

Conflict of interest The authors declare none.

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