Estudios sobre la Economía Española

Education and Internal Migration: Evidence from a Child Labor Reform in Spain

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Estudios sobre la Economía Española 2021/34
Diciembre de 2021

fedea

EDUCACIÓN Y MIGRACIÓN INTERNA: EVIDENCIA DE UNA REFORMA DEL TRABAJO INFANTIL EN ESPAÑA

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En marzo de 1980, el parlamento español aprobó una nueva ley que reguló las relaciones laborales (Estatuto de los Trabajadores, ET). Elevó la edad legal para trabajar (ELT) de 14 a 16 años sin cambiar la edad de finalización de la educación obligatoria, fijada en 14. Antes de esta reforma, las personas nacidas a principios de año (entre enero y mayo) se encontraban legalmente capacitadas para trabajar sin haber completado su educación obligatoria, ya que cumplían 14 años antes del final del año escolar, en junio. Esto proporcionó un incentivo (o al menos la posibilidad legal) de dejar la escuela para trabajar antes de completar la educación obligatoria. Alternativamente, las personas nacidas al final del año alcanzaban la ELT después de completar su educación obligatoria, ya que cumplían 14 años después del final del año escolar. En 1980, cuando la ELT aumentó a 16, se eliminó esta diferencia de incentivos a empezar a trabajar legalmente entre los nacidos al principio y al final del año. De hecho, para poder trabajar legalmente, las personas debían tener 16 años, y esto ocurría más de un año después de completar la educación obligatoria.

Aprovechamos el aumento de la ELT provocado por el ET para investigar el impacto del incremento de la educación causado por la reforma (documentado en Del Rey et al, 2018) sobre la probabilidad de migración, la distancia recorrida por los migrantes y la intensidad de algunos flujos migratorios dirigidos (por ejemplo, la migración a una región más rica o una ciudad mayor de la misma región).

En España, el rápido y polarizado crecimiento de los años sesenta y la primera mitad de los setenta provocó intensas transferencias de población desde regiones principalmente rurales hacia los grandes polos industriales, ubicados en las regiones del País Vasco, Cataluña y Madrid. Durante algunos años después de mediados de la década de 1970, los flujos migratorios internos se desaceleraron, a pesar de las persistentes diferencias regionales y el alto nivel de desempleo sostenido. En 1982, la migración tanto interregional como intrarregional volvió a aumentar casi de manera constante hasta 2007. Sin embargo, el perfil del migrante en las décadas de 1980 y 1990 cambió con respecto al de décadas anteriores, y la migración interregional neta fue muy baja. Además del migrante tradicional que maximiza los ingresos, había personas educadas que migraban a través de las regiones en busca de viviendas más baratas y servicios preferidos, así como trabajadores cualificados que migraban hacia ciudades más grandes de la misma región donde se estaban creando nuevos empleos en el sector de servicios.

En nuestro estudio encontramos poca evidencia de que el aumento de la ELT afectase al comportamiento migratorio de los hombres. No ocurre así con las mujeres para las que la evidencia que hemos reunido sugiere que (i) redujo en un 4% la probabilidad de migración interregional, (ii) redujo en casi un 4% la distancia recorrida por las migrantes interregionales, (iii) aumentó en aproximadamente 3% la probabilidad de trasladarse de municipio dentro de la región de nacimiento a lo largo de la vida, y (iv) estimuló en cerca del 13% la reubicación de ciudades grandes a medianas situadas dentro de la misma región.

La causa de las distintas respuestas migratorias según género podría tener que ver con el proceso de igualación de género que vivía España en el momento de la reforma (Belles et al, 2021a y 2021b). Efectivamente, el ET se promulgó pocos años después del fin de la dictadura de Franco, que había durado casi 40 años. Durante la dictadura, España era una sociedad dominada por los hombres, y los derechos de las mujeres generalmente se ignoraban o se suprimían. El fin de la dictadura elevó el nivel de igualdad de género y mejoró el acceso de las mujeres a las oportunidades económicas. Por lo tanto, las mujeres, y especialmente las mujeres más educadas, podrían haber comenzado a ver la migración a regiones más desarrolladas socialmente como menos necesaria para debilitar las limitaciones sociales y culturales que impedían su desarrollo personal y social. Además, el aumento de la educación y la participación de las mujeres en el mercado laboral podría haber reducido la diferencia en la capacidad de generar ingresos en la pareja, por lo que, según la teoría del poder conyugal, la propensión a la migración interregional de las parejas sería menor.

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EDUCATION AND INTERNAL MIGRATION: EVIDENCE FROM A CHILD LABOR REFORM IN SPAIN

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Abstract: We exploit a child labor regulation that raised the minimum working age from 14 to 16 while leaving the age for compulsory education at 14 to provide new evidence on the causal effect of education on migration. Individuals born at the beginning of the year are more likely to complete compulsory and post-compulsory education if they turn 14 after the reform. Men's internal migration flows were unaffected by the reform. For women, long-distance migration and the distance moved by migrants declined after the reform, whereas certain types of short-distance moves increased. Some implications of these findings and a consideration of their external validity are also provided.

Keywords: Internal migration, education, child labor reform, Spain.

JEL codes: J61, R23, I20.

Funding: González Chapela received financial support from the Government of Aragón, grant S32-20R.

Acknowledgements: We are grateful to the Instituto Nacional de Estadística for providing us some of the data and for assistance with the data. We benefited from electronic correspondence with Andres Santos and David Roodman.

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1. INTRODUCTION

In March 1980, a new law regulating labor relations (*Estatuto de los Trabajadores*, ET) was passed by the Spanish parliament. It raised the legal working age (LWA) from 14 to 16 years without changing the school leaving age, set at 14. Before this reform, individuals born at the beginning of the year (between January and May) found themselves legally able to work before completing their compulsory education, as they turned 14 before the end of the school year in June. This provided an incentive (or at least the legal possibility) for leaving school to work before completing compulsory education. Individuals born at the end of the year reached the LWA after completing their 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 between those born at the beginning and the end of the year was removed. Indeed, to be legally able to work individuals needed to be 16, and this occurred more than a year after completing compulsory education.

When comparing outcomes before and after the reform, Del Rey et al. (2018) find a clear increase in the educational attainment of individuals born at the beginning of the year relative to individuals born at the end of the year (treatment and control groups, respectively). Del Rey et al. (2018) and Bellés-Obrero et al. (2021a, 2021b) exploit this increase in the relative demand for education of individuals born at the beginning of the year to identify the causal effect of education on a number of outcomes.[4] In this paper,

[4] They find, for example, that the reform decreased working accidents and mortality at ages 14–29 for men and women, that, for men, it increased wages and the overall probability of working, and that, for prime-age women, it deteriorated health habits and the health of their offspring at the moment of delivery.

we use it to provide new evidence on the causal effect of education on the propensity to migrate within a country.

Internal migrants appear as favorably self-selected in terms of education, see e.g. Bernard and Bell (2018). Although education might discourage migration at some schooling levels (e.g., McHenry 2013, Aparicio and Kuehn 2017), a number of reasons suggest that the positive correlation between education and migration might be causal (e.g., Malamud and Wozniak 2012, Haapanen and Böckerman 2017, and Rauscher and Oh 2021). On the other hand, the positive correlation between education and migration might be the result of unobserved characteristics influencing both outcomes. For example, McHenry (2013) shows that under certain conditions people with higher discount rates choose less schooling and less migration effort. Also, as argued by DaVanzo (1981), some people might be intrinsically more migration-prone, and since obtaining education may involve moving away from one's place of residence (e.g., Faggian et al. 2007, Böckerman and Haapanen 2013, Lovén et al. 2020), the most migration-prone could end up more educated.

To solve the selection issue, the previous research has focused mostly on 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, Gevrek et al. 2021). However, at least some of these reforms coincided with changes that altered the unobserved quality of schooling (e.g., Sansani 2015), or with changes at the area level that could potentially confound the estimated effect of educational attainment (Stephens and Yang 2014). The increase in the LWA brought about by the ET is different because it raised the demand for education through a decrease in the incentives for leaving school for a specific fraction of the population (individuals born at the beginning of the year).

From the research design view, this reform is attractive for at least four reasons. First, individuals cannot opt into or out of the scope of the reform, so they can be considered as randomly assigned to treatment and control groups, accounting for self-selection into education. Second, by looking at differences between individuals born at the beginning and the end of the year, changes which occur across birth cohorts can be accounted for in estimation. Third, the identification of the effects of the reform does not rely on changes which occur across regions over time, so we can allow cohort effects to vary flexibly across regions. And fourth, the reform was applied in the middle of the more than 20-year period of validity of the Spanish educational system introduced in the 1970–71 school year, so we can be confident that there were no other reforms that could undermine our identification strategy. One potential concern with our approach is that individuals born at the beginning and the end of the year might differ in terms of family background characteristics (e.g., Buckles and Hungerman 2013). However, the evidence presented below do not appear to challenge seriously the assumption that mothers' mobility remains stable when making the before-after comparison.

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 induced by draft-avoidance behavior among men affected by the Vietnam conflict, and the abolition of tuition fees for secondary school entrants in the Philippines, respectively. 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 age mainly around 14 years. Because of earlier reported gender differences in migration effects (Melzer 2013, Lovén et al. 2020, Sakai and Masuda 2020, Gevrek et al. 2021), our analysis will be conducted by men and women separately.

Internal migration is an important way for individuals to enhance their quality of life. Education can affect the probability of migrating by changing the returns to migrating, by reducing the nonmonetary costs of migrating, or both; see e.g. Haapanen and Böckerman (2017). Also, at least since Schwartz (1973) education is considered a counteracting factor to the detrimental effect of distance on migration by making individuals less sensitive to the nonmonetary costs of migrating. Residents of rural areas worry about "brain drain," the idea that the educated will migrate to cities. While in a general equilibrium setting internal migration is usually desirable because it boosts the speed at which workers and firms adjust to localized shocks (e.g., Blanchard and Katz 1992), if the educated are more likely to migrate from rural areas, these areas will benefit less from investing in education, hindering educational expansion and economic development at the national level (Rauscher and Oh 2020). We exploit the increase in the LWA brought about by the ET to investigate the impact of education on the probability of migration, the distance moved by migrants, and the intensity of some directed migration flows (e.g., migration to a richer region or to a larger town of the same region).

In Spain, the rapid and polarized growth of the 1960s and first half of the 1970s caused intense transfers of employment from mainly rural regions towards the major industrial hubs, located in the regions of the Basque Country, Catalonia, and Madrid (e.g., Bover and Velilla 2005). For some years after the mid-1970s, internal migration flows slowed down, notwithstanding persistent regional differentials and sustained high unemployment. In 1982, both interregional and intraregional migration started to increase almost steadily until 2007 (Bover and Velilla 2005, Minondo et al. 2013). Nevertheless, the profile of the migrant in the 1980s and 1990s changed with respect to that of earlier decades, and net interregional migration was very low (Antolin and Bover 1997, Maza and Villaverde 2004). In addition to the traditional income-maximizing migrant, there were educated people migrating across regions in search of cheaper housing and preferred amenities, as well as skilled workers migrating towards larger towns of the same region where new jobs in the service sector were being created (Bover and Arellano 2002, Bover and Velilla 2005). Some studies (e.g., Antolin and Bover 1997, Bover and Arellano 2002) have documented the importance for these migration decisions of the individual's educational attainment.

The rest of the paper is organized as follows. Section 2 provides background on the reform. Section 3 describes the data and the selection of the sample. Section 4 explains the measures of internal migration used in the primary analysis. Section 5 discusses the empirical strategy. Section 6 presents the results of the primary analysis. Section 7 gathers the results of several supplementary analyses. Section 8 summarizes the paper and provides some implications of the findings and a consideration of their external validity.

2. THE 1980 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 start school at an older age (in months) than those born at the end of the year. Individuals can drop out of school as soon as they reach the LWA.

The statutory minimum working age in Spain was set at 14 years in 1944 (although younger children were permitted to work in agriculture and family shops). Compulsory schooling was extended from 12 to 14 years in 1964, but the enforcement of this regulation was not very effective until a new educational law (*Ley General de Educación*, LGE) was introduced in the 1970–71 school year. Under the LGE, compulsory education was between 6 and 14 years and covered primary 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 years). The first stage of vocational training was compulsory for students not taking the Bachillerato, but, again, the enforcement of this regulation was not very effective until a new educational law (LOGSE, introduced starting in the 1991–92 school year) increased the compulsory schooling age until 16 years for all students (Egido 1994).

The ET, passed in March 1980, prohibited child labor under the age of 16 (the only exception was the authorized participation of minors in specific public shows). As a result, 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 an incentive to stop attending school before completing compulsory education. Furthermore, although children born at the beginning of the year could join the labor market at the age of 16 before attaining the first level of post-compulsory education, by increasing the number of them attaining compulsory education, the law made it possible for some of them to complete post-compulsory education. See Cadena and Keys (2015) and Del Rey et al. (2018) for an explanation of this behavior based on impatience and time-inconsistent preferences.

The reform was fully effective in reducing child labor for the cohorts that turned 14 after the reform (individuals born in 1967 or later). For the cohorts that were 14 and 15 at the time the reform was introduced, which could already be legally working before the reform (cohorts of 1965 and 1966), Del Rey et al. (2018) show that work almost disappeared for the 14-year-old cohort, but not so for the 15-year-old cohort (although work was partly reduced in comparison with individuals born before 1965). Hence, in our baseline specification we will consider the 15-year-old cohort as the last pre-reform cohort, but we will exclude that potentially treated cohort in assessing the robustness of the results.

3. DATA AND SAMPLE SELECTION

For the analysis we pool data from the 2001 and 2011 Spanish population censuses, conducted by the National Statistics Institute (INE, www.ine.es).[5] 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 total population. The survey oversampled in municipalities with population not greater than 10,000 inhabitants as well as main dwellings which were pre-registered to the fieldwork.

Both censuses contain information at the individual level 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), place of residence at three points in time (at the Census date, 10 years prior, and at birth), and year of arrival in both the region and the municipality of residence. For individuals who arrived in the current

[5] We discarded the 1991 Census because many of the individuals who turn 14 after the reform were still studying in 1991, and because many of them would move as a child. The Spanish Labor Force Survey (LFS) severely underestimates the number of migrants (Martí and Ródenas 2004, 2007) and lacks information to construct a precise measure of lifetime migration. An alternative dataset, the Continuous Sample of Work Histories, tracks the work establishments' locations of individuals included in the sample during their careers. This information precludes knowing whether individuals left their birthplaces during childhood, and raises the issue that workers who find a job in a nearby place may actually not change their residence.

region/municipality, the previous province of residence is also recorded in the data. We limit the analysis to internal migration because natives who moved permanently abroad are not included in the Census.

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.[6] Smaller municipalities are not disclosed to preserve confidentiality, but their population category is recorded in the data (until 2,000 inhabitants, between 2,001 and 5,000, between 5,001 and 10,000, and between 10,001 and 20,000).

Both 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. The reason why we omit those individuals born in the first and last two months of the year is because the literature about the differences in season of birth (e.g., Buckles and Hungerman 2013) suggests that individuals born in the second and third quarters of the year are significantly more similar than individuals born in the first and fourth quarters, an assumption which we shall assess in Section 7.1. 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

[6] 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). 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.

1980, the year the reform was introduced. We drop this cohort because the law was passed in March (towards the end of the school year in June) and it is not clear the degree of enforcement of the law during this first year. We also apply the restriction of not being a student at the census date. As the selected cohorts turn 26–44 (36–54) in the year in which the 2001 (2011) Census was conducted, there are few individuals still studying at these ages. Finally, military personnel are also removed as their migration decisions are probably non-autonomous.

4. INTERNAL MIGRATION MEASURES

The census data provide great flexibility in defining internal migrants. For expositional purposes, we focus the analysis on a few migration measures, and use alternative ones to assess the robustness of the findings. In addition, we also investigate whether the reform fostered the distance moved by migrants, as at least since Schwartz (1973) education has been considered a counteracting factor to the detrimental effect of distance on migration (for further evidence, see e.g. Bauernschuster et al. 2014).

We group the different measures into interregional and intraregional measures. In principle, interregional migration is more likely to capture the link between education and the ability to make changes when the same worker faces different opportunities in different labor markets (Malamud and Wozniak 2012). However, the expansion of the service sector that took place in Spain since the late 1970s, and which occurred within all regions but mainly in larger towns (Bover and Arellano 2002, Bover and Velilla 2005), suggests that a fraction of intraregional moves may have been conducted towards larger towns where the new jobs were being created.

We measure interregional migration in two ways. First, we create an indicator of lifetime migration, denoted *INTER1*, using the reported year of arrival in the region of residence. Following González (2020), we rely on whether individuals were legally able to work in their year of arrival to distinguish between autonomous (i.e. decided by the individual) and non-autonomous migration. Migrations by individuals legally able to work are considered as autonomous, and migrations by individuals legally unable to work as non-autonomous. Individuals who were legally able to work in their year of arrival in their region of residence are therefore classified as migrants. Nonmigrants are individuals who have resided since birth in the same region and individuals who were legally unable to work in their year of arrival.

And second, we create a rough measure of the distance moved by migrants. We calculate the linear distance (in kilometers) between the capital cities of the migrant's province of birth and province of destination, the latter being the province of current residence if the migrant did not return to her/his birth region, or else the province of residence prior to returning to her/his birth region. The resulting measure is denoted KM_INTER1.

We measure intraregional migration in two ways. (In both cases, we exclude interregional migrants to enable a cleaner comparison between intraregional migrants and nonmigrants.) First, for individuals with $INTER1 = 0$, we construct a measure of intraregional lifetime migration predicated on cross-municipality movement (*INTRA1*), using the reported year of arrival in the municipality of residence. Individuals who were legally able to work in their year of arrival are classified as migrants. Nonmigrants are individuals who have resided since birth in the same municipality and individuals who were legally unable to work in their year of arrival.[7]

[7] The measure of distance associated to *INTRA1* cannot be calculated because for individuals who moved regions as a child, the province of arrival in their current region (which would be their province of origin) is not recorded in the data.

And second, following Bover and Arellano (2002), for individuals who did not move regions over the 10 years prior to the Census we analyze their propensity to move to small (less than 10,000 inhabitants), medium (10 to 100 thousand inhabitants), and large towns (more than 100,000 inhabitants) over the previous 10 years.

5. EMPIRICAL STRATEGY

The increase in the LWA brought about by the ET lowered the incentives of leaving school for individuals born in the first months of the year relative to individuals born in the last months of the year. To investigate the effects of this reform on internal migration, we follow Del Rey et al. (2018) and compare migration outcomes for individuals born in March, April, and May with migration outcomes for individuals born in August, September, and October before and after the introduction of the reform. The pre-reform birth cohorts are made up of individuals who turned 14 before the implementation of the reform (cohorts born between 1957 and 1965), while the post-reform cohorts include individuals who turned 14 after the introduction of the reform (cohorts born between 1967 and 1975).

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

$$\begin{aligned} Outcome_{ict} = \alpha + \beta_1 Treatment_{ic} + \beta_2 Treatment_{ic} * Post_c + \delta_t \\ + \beta_3 \delta_t * Treatment_{ic} + \theta_j + \mu_c + \varepsilon_{ict}, \end{aligned} \tag{1}$$

where $Treatment_{ic}$ and $Post_c$ are binary indicators 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 a binary indicator taking value one for observations from the 2011 Census, θ_j and μ_c denote individuals' region of birth and cohort (year of birth) fixed effects, respectively, and ε_{ic} is an error term. The interaction of δ_t with $Treatment_{ic}$ allows for differential time effects between treatment and control groups.

The coefficient of interest, β_2, is thus a difference-in-differences estimate that compares the outcomes of individuals born in March–May between 1967 and 1975 with the outcomes of individuals born in the same months of 1957–1965, using individuals born in August–October as controls. Equation (1) is estimated by weighted least squares (WLS), weighting by the inverse probability of selection.[8] Standard errors are clustered at cohort level. Since the number of cohorts included is not large (eighteen), we also provide the wild cluster bootstrap *p*-value for testing the null hypothesis that $\beta_2 = 0$.[9]

To estimate the effects of the reform on the probability of migration to small, medium, 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

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[8] Weighting is needed to correct for endogenous sampling when studying migration to small, medium, or large towns (e.g., Solon et al. 2015). For other migration outcomes, ordinary least squares (OLS) and WLS yield substantially similar results. So, we use weights throughout.

[9] Canay et al. (2021) provide homogeneity restrictions on the distribution of covariates for the validity of the wild bootstrap-based test in settings where the number of clusters is small but the number of observations per cluster is large. Our setting probably fulfills only partially those restrictions. According to Example 3 of Canay et al. (2021), the homogeneity restrictions are difficult to satisfy if $Treatment_{ic} * Post_c$ is constant within clusters, as occurs in the pre-reform cohorts. However, the alternative inference procedures of Ibragimov and Müller (2016) and Canay et al. (2017) do not seem applicable in our setting as they rely on cluster-level estimators of the coefficients, whereas β_2 is identified across clusters.

variable taking the values $\{0, 1, 2, 3\}$. We model $\Pr\left[Outcome_{ict} = k \middle| \mathbf{x}_{ict}\right]$, for $k = 0, 1, 2, 3$, using multinomial logit (MNL):

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

where

$$\begin{aligned} \mathbf{x}_{ict}\boldsymbol{\pi}_k &= \alpha_k + \beta_{k1} Treatment_{ic} + \beta_{k2} Treatment_{ic} * Post_c + \delta_{kt} \\ &+ \beta_{k3}\delta_{kt} * Treatment_{ic} + \theta_{kj} + \mu_{kc} \end{aligned} \tag{3}$$

and $\boldsymbol{\pi}_0 = \mathbf{0}$. Estimation of (2) is carried out by maximum likelihood. Separate estimates are developed 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 $Treatment_{ic} * Post_c$ on $p_k^i \equiv \Pr\left[Outcome_{ict} = k \middle| \mathbf{x}_{ict}\right]$, denoted γ_k^i, is calculated as (Ai and Norton 2003, Appendix A of Dinc and Erel 2013):

$$\begin{aligned} \gamma_k^i \equiv \frac{\Delta^2 p_k^i}{\Delta Treatment_{ic} \Delta Post_c} &= p_k^i\Big|_{Treatment_{ic}=1,\, Post_c=1} - p_k^i\Big|_{Treatment_{ic}=0,\, Post_c=1} \\ &- p_k^i\Big|_{Treatment_{ic}=1,\, Post_c=0} + p_k^i\Big|_{Treatment_{ic}=0,\, Post_c=0}. \end{aligned} \tag{4}$$

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

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

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 Educational attainment

We start showing the effect of the reform on educational attainment in our sample drawn from the Census. (The main analysis of Del Rey et al. 2018 is conducted using data taken from the Spanish LFS 2000–2013.) The choice of categories given by the educational attainment question of the Census includes the degrees of the educational system in force in Spain in the 1970s and 1980s, but some categories are rather aggregate (for example, one category combines the Bachillerato and the ensuing pre-university year). Thus, we estimate Equation (1) with $Outcome_{ict}$ representing a binary indicator taking value one for individuals who have not completed primary education (early school leavers, henceforth ESL), a binary indicator taking value one for individuals who, at most, have completed primary education (dropouts), a binary indicator taking value one for individuals who have completed a university degree lasting at least three years, or a continuous variable measuring the years of education (coded into 0, 2.5, 6.5, 8, 10, 11.5, 12, 15, 16, 17, and 19 years to roughly correspond to multiple-year increments of education or degrees).

Panel 1 of Table 1 presents the results separately for men and women. As expected, the increase in the LWA raised the educational attainment of individuals born in the months of March–May in the post-reform years. At weighted means of the dependent variables (listed in the last row of the panel), the probability of being an ESL decreases by 3.4% for men and 6.5% for women. The probability of being a dropout also falls, by 2.5% for men and by 1.5% for women, indicating that the reform induced some students to complete some post-compulsory education. Indeed, the effects of the reform extends to higher education, as the probability of having studied at the university level (completing a degree of at least 3 years) increases by 3.5% for men and 2.4% for women (the latter is not statistically significant). All these variations translate into an average increase of 0.092 years of education for men and 0.085 years for women, which 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). Allowing year of birth effects to vary across birth regions leaves these results almost unchanged. Panel 2 of Table 1 shows that excluding the cohort born in 1965 changes little the results for men, but raises somewhat the estimated effects for women. Overall, these results support the positive effect of the reform on educational attainment, and suggest the existence of different effect sizes by level of education and sex.

6.2 Interregional migration

The results of estimating Equation (1) with $Outcome_{ict}$ representing *INTER1* or *KM_INTER1* are presented in Table 2, whose last row also lists the weighted means of the dependent variables. For men, the results suggest that the increase in the LWA had a negative though small effect on both the probability of migrating and the distance moved by migrants. For women, the reform decreased both the probability of migrating and the distance moved by migrants. Women born in the months of March–May are 0.54 percentage points less likely of being a lifetime migrant in the post-reform cohorts, a 4% drop. Among women who migrated, those born in March–May move on average near 15 kms (or 4%) closer to their birth place in the post-reform cohorts. Both of these effects attain significance at or around 5%. Allowing year of birth effects to vary across birth regions or excluding the cohort born in 1965 change little these results.

6.3 Intraregional migration

Table 3 shows the results of estimating Equation (1) with *INTRA1* as the dependent variable. The last row of the table also lists the weighted means of *INTRA1*. For men, the estimated β_2 is negative and small. For women, it is positive, larger, and statistically different from zero at 5%. It indicates that women born in the months of March–May are 0.83 percentage points (or 3%) more likely of having moved municipalities within their birth region in the post-reform cohorts. Allowing year of birth effects to vary across birth regions or excluding the cohort born in 1965 hardly change these results.

Average partial effects on the probabilities of no migration, migration to a small town, migration to a medium town, or migration to a large town, are presented in Table 4 separately by town of origin size and sex. At weighted means of these probabilities (listed in the table in italics), the largest effects are observed on the probability of migrating to a large town by men. However, the null hypothesis (6) is only rejected for women living in large towns 10 years prior to the Census date (column 6). Among these women, those born in the months of March–May in the post-reform cohorts are 1.34 percentage points less likely of no migrating (a near 2% fall), and 0.88 percentage points more likely of migrating to a medium size town (a near 13% increase). These results change little when year of birth effects are allowed to vary across birth regions or when the cohort born in 1965 is excluded.

7. SUPPLEMENTARY ANALYSES

7.1 Identification

Individuals born in the months of March–May and August–October might differ in terms of family background characteristics. As we are estimating the effects by looking at the difference between treatment and control groups before and after the reform, the differences between individuals born in March–May and August–October should cancel out when making the before-after comparison if they are constant over time. However, it could be the case that the differences could evolve over time and could, somehow, change between the pre-reform and post-reform cohorts, biasing our results.

Cancho-Candela et al. (2007) show that the distribution of births in Spain between March–May and August–October became less dissimilar in the 1960s than in the 1950s. This change is part of a general trend of declining birth seasonality in Spain which started in the 1960s, when environmentally regulated fertility was largely taken over by patterns dominated by sociocultural factors (Cancho-Candela et al. 2007). We have investigated whether differences in maternal characteristics for births in March–May and August–October might have changed between the pre-reform and post-reform cohorts using data from the 1991 Socio-Demographic Survey (ES1991), conducted during the last quarter of 1991 by the INE.[10] In addition to the individual's date of birth, the ES1991 contains information on a number of maternal characteristics, such as educational attainment (highest degree completed) and place of birth (whether she was born in the same municipality, province, region, and country than the interviewee). We focus on a sample of individuals selected along the lines of the census sample, on whom the following model is estimated:

$$Outcome_{ic} = \alpha + \beta_1 Treatment_{ic} + \beta_2 Treatment_{ic} * Post_c + \theta_j + \mu_c + \varepsilon_{ic}\,. \qquad (7)$$

The dependent variable $Outcome_{ic}$ is here a binary indicator taking value one for individuals born to mothers with more than primary education, or a binary indicator taking value one for individuals born to mothers living out of their birth region/province at childbirth. The other terms are as in Equation (1).

The WLS estimates of Equation (7) are shown in Table 5. In comparison with the weighted mean of the dependent variable (listed in the last row of the table), the largest estimate of β_2 is found in column (4). However, it does not attain significance at standard levels. The β_2 reported in column (1) is statistically different from zero at 10%. It

[10] Birth certificate data in Spain are available starting in 1975.

suggests that among individuals born in March–May, the probability of being born to a mother with more than primary education decreases 1.97 percentage points in the post-reform cohorts, a 12% fall. Allowing year of birth effects to vary across birth regions changes little these results. Estimating by OLS or excluding the cohort born in 1965 produces generally smaller and statistically insignificant estimates of β_2 for the four outcomes shown in the table.

Therefore, the evidence presented here do not appear to challenge seriously the assumption that mothers' mobility remains stable when making the before-after comparison. However, the mothers of individuals born in March–May might have attained less schooling on average after the reform. In Spain, the mother's educational attainment exercises a powerful influence on the educational demand of youngsters; see for example Albert (2000) and Petrongolo and San Segundo (2002). As a result, the incentives to stay on in education for individuals born in March–May induced by the reform might have been partially counteracted by the lower educational attainment of their mothers, thus reducing the impact of the reform on educational attainment.[11]

7.2 Robustness

In this section, we study the effects of the reform on a number of measures of migration beyond those considered in the primary analyses. Interregional migration is measured in four alternative ways. First, we create an indicator, denoted *INTER2*, for whether the region of residence differs from the region of birth. Second, we create an indicator for moving regions over the 10 years prior to the Census date, denoted *INTER3*. Third, we

[11] The reduction in the impact of the reform might have been similar for both sexes, as analyses conducted by Petrongolo and San Segundo (2002) suggest that the effect of family controls on the demand for education are identical for males and females.

calculate the linear distance between the capital cities of the province of birth or of residence 10 years prior to the Census, and the province of current residence, denoting the resulting measures *KM_INTER2* and *KM_INTER3*. And fourth, following Barone et al. (2019), we split the sample into poorer and richer regions according to their GDP per capita in 1980.[12] For individuals born in poorer regions, we redefine *INTER1* to indicate migration over the lifetime to a richer region, denoted *INTER1b*. Similarly, for individuals born in poorer regions, we redefine *INTER2* to indicate residence in a richer region at the Census date, denoted *INTER2b*.

The results are presented separately for men and women in Panels 1 and 2 of Table 6. Consistent with our previous findings, there is little evidence that the increase in the LWA affected men's migration behavior. In contrast, for women the reform tended to decrease both the probability of migrating and the distance moved by migrants. For example, women born in the months of March–May appear 0.94 percentage points (or 5%) less likely of residing out of their birth region in the post-reform cohorts. Also, according to *INTER2b* the reform discouraged moves of women from poorer to richer regions, although this conclusion is not supported by *INTER1b*. As to distance, out-of-birth region women born in March–May live some 12 kms (or near 3%) closer to their birth place in the post-reform cohorts. The tendency to live closer is also observed among women who moved regions over the previous 10 years, but the smaller size of the sample prevents the estimated effect from being accurately measured.

Intraregional migration is measured in three alternative ways. First, for individuals with $INTER2 = 0$, we create an indicator for whether the province of residence differs from the province of birth, denoted *INTRA2*. Second, for individuals with $INTER3 = 0$,

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

we create indicators for moving provinces or municipalities over the previous 10 years, denoted *INTRA3* and *INTRA4*, respectively. And third, for individuals with $INTER3 = 0$, we create indicators for moving to a municipality in a higher or lower population group over the previous 10 years, denoted *BIGGER* and *SMALLER*, respectively.

Panels 1 and 2 of Table 7 show the results for men and women, respectively. In both cases, the evidence is quite consistent with our previous findings. For men, the only visible effect is on the probability of moving provinces over the previous 10 years (column 2 of Panel 1), which increases in the post-reform cohorts. The effect, however, is only marginally significant. For women, the probability of moving to smaller towns rises in the post-reform cohorts (column 5 of Panel 2), and there is also weak evidence that the reform increased the probability of moving municipalities over the 10 years prior to the Census (column 3 of Panel 2). The fact that the reform left almost unchanged the probability of moving provinces among women (columns 1 and 2 of Panel 2), but raised the probability of moving municipalities, indicates that the reform fostered within province moves.

7.3 Placebos

Finally, we subject our results to a placebo analysis using the pre-reform cohorts 1957–1965. Specifically, we examine the effect of seven fake reforms affecting the cohorts 1958–65, 1959–65, 1960–65, 1961–65, 1962–65, 1963–65, or 1964–65, using the same econometric specifications and definition of treatment status as before. Under the assumption of a common trend between treatment and control groups, $Treatment_{ic} * Post_c$ should be insignificant for any of these fake reforms.

The results are presented graphically in Figure 1. Each graph of this figure plots the effects of the fake reforms on the outcome shown in the graph header, denoting each reform by the earliest cohort affected. For each reform, we plot the estimated coefficient of $Treatment_{ic} * Post_c$ (or average marginal effect $E\left[\gamma_k^i\right]$) and its conventional cluster-robust 95% confidence interval. Note that since standard errors are not adjusted for few clusters, the Type I error rate can be in excess of 5%.

None of the fake reforms produces significant effects on *INTRA1*, so the common trend that is necessary for a causal interpretation of the increase in the LWA is fulfilled here. As to the change in the probability of migrating from a large to a medium size town, the estimated effects are substantially smaller (in absolute value) than the 0.88 percentage points increase reported in Table 4. Also, there are almost no significant differences between treatment and control groups in that probability, thus giving credence to the causal interpretation of the impact of the reform. As to *INTER1* and *KM_INTER1*, the graphs suggest that the increase in the LWA reversed a trend towards preserving the difference in lifetime migration rates and distance moved between treatment and control groups.

8. DISCUSSION AND CONCLUSION

Most previous research has used extensions in compulsory schooling laws or changes in degrees granted to assess the causal effect of education on migration, finding mixed results. In this paper, we have explored the migration effects of a child labor regulation reform introduced in Spain in 1980 that increased the LWA from 14 to 16, while the school-leaving age remained at the age of 14. Thus, the reform eliminated the difference in school/work alternatives available to individuals born at the beginning and the end of the year, as they would all have attained compulsory education by the time they reached the legal age to work.

In a sample of prime-aged natives drawn from the Spanish population censuses 2001 and 2011, we find that the increase in the LWA raised the average years of schooling of individuals born at the beginning of the year by 0.08–0.10. Although these individuals are more likely to complete both compulsory and post-compulsory education after the reform, the bulk of the reform's effects is found generally around 14 years of age.

We find little evidence that the increase in the LWA affected men's migration behavior. In contrast, the evidence that we have assembled for women suggests that it (i) lowered by 4% the probability of interregional lifetime migration, (ii) reduced by near 4% the distance moved by interregional lifetime migrants, (iii) increased by about 3% the probability of moving municipalities within the birth region over the lifetime, and (iv) stimulated by near 13% the relocation from large to medium size towns situated within the same region.

Del Rey et al. (2018) find that, for men, the increase in the LWA increased wages and the overall probability of working, while it decreased the probability of working in a low-skilled sector. As we find no evidence that the reform affected men's migration propensities, those effects do not seem the consequence of possible benefits to migration. On the other hand, Del Rey et al. (2018) find insignificant labor market effects for women, which hints that women's higher probabilities of migrating within their birth region might be due to non-labor market incentives. For example, by improving the ability to acquire better information and analyze it more productively, education might improve the ability to respond appropriately to spatial disequilibria across housing markets, particularly in the face of family and life-cycle needs.

The reason for the different migration responses by gender might have to do with the gender equalization process that Spain was experiencing at the time the reform took place. As discussed by Bellés-Obrero et al. (2021b), the ET was enacted just a few years after the end of Franco's dictatorship, which had lasted almost 40 years. During the dictatorship, Spain was a male-dominated society, with women's rights generally ignored or suppressed. The end of the dictatorship raised the level of gender equality and improved women's access to economic opportunities. Thus, women, and especially more educated women, might have started to see migration to more socially developed regions as less necessary to weaken the social and cultural constraints that prevented their social development.[13] In addition, women's increasing education and participation in the labor market might have reduced the difference in earning capacity between the partners, so according to marital power theory, couples' interregional migration propensity would be lower (e.g., Smits et al. 2003).

The reduction in the distance moved by female migrants that we estimate is consistent with evidence for the U.S. showing that individuals required to attend school as a child moved closer to their state of origin (Rauscher and Oh 2021). Thus, it appears that migrants capitalize on the skills conferred by education to find opportunities closer to their birth place and hence to reduce the costs of migrating.

As to the external validity of our findings, some considerations are worth noting. First, the compliant population associated with the reform is made up of individuals who are about to stop studying and start working. The increase in the LWA provided incentives to them to continue studying. Compulsory schooling laws, which determine an individual's minimum level of education, can change the educational attainment of all individuals of a given cohort if applied effectively. Therefore, the compliers of these two types of reform are different and, as a matter of fact, the impact of the reform studied here

[13] Del Rey et al. (2018) divide the 17 regions in Spain into two groups according to the social development of the region in 1980. For women born in less socially developed regions, we find that the reform reduced the probability of living in a socially developed region at the Census date, but it did not change the probability of migrating (in the sense used to construct *INTER1*) to a socially developed region.

on educational attainment differs from that of compulsory schooling laws: Across European countries, one additional year of compulsory schooling increases average educational attainment by 0.26 years (Aparicio and Kuehn 2017). And second, the effect of the reform is analyzed in a period in which the profile of the Spanish internal migrant had changed with respect to the profile in the 1960–1973 period. The 1980s and 1990s are characterized by migration of educated people in search of cheaper housing and better quality of life, as well as of skilled workers towards larger towns where new jobs in the service sector were being created.

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TABLES AND FIGURES

Table 1. Changes in educational attainment

Panel 1: Full sample	Men				Women			
	(1) ESL	(2) Dropout	(3) University degree	(4) Years of education	(5) ESL	(6) Dropout	(7) University degree	(8) Years of education
Treatment	0.737**	1.527***	-0.195	-0.082***	1.658***	1.292**	-0.359	-0.109***
	(0.310)	(0.291)	(0.203)	(0.018)	(0.357)	(0.510)	(0.280)	(0.036)
	[0.036]	[0.000]	[0.368]	[0.001]	[0.000]	[0.029]	[0.230]	[0.014]
Treatment*Post	-0.619**	-1.365***	0.614***	0.092***	-1.079**	-0.759	0.531	0.085**
	(0.266)	(0.349)	(0.179)	(0.016)	(0.395)	(0.526)	(0.326)	(0.038)
	[0.024]	[0.003]	[0.001]	[0.000]	[0.014]	[0.175]	[0.155]	[0.047]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	344,414	344,414	344,414	344,414	344,930	344,930	344,930	344,930
Mean of dep. var.	18.172	53.784	17.494	9.982	16.505	50.301	22.223	10.307
Panel 2: Excluding the cohort born in 1965	Men				Women			
	(1) ESL	(2) Dropout	(3) University degree	(4) Years of education	(5) ESL	(6) Dropout	(7) University degree	(8) Years of education
Treatment	0.862**	1.545***	-0.230	-0.083***	1.875***	1.594***	-0.528*	-0.132***
	(0.315)	(0.317)	(0.219)	(0.020)	(0.314)	(0.486)	(0.258)	(0.032)
	[0.017]	[0.000]	[0.301]	[0.002]	[0.000]	[0.022]	[0.085]	[0.013]
Treatment*Post	-0.691**	-1.420***	0.631***	0.095***	-1.293***	-1.027*	0.633*	0.103**
	(0.278)	(0.362)	(0.192)	(0.016)	(0.363)	(0.507)	(0.329)	(0.036)
	[0.023]	[0.002]	[0.006]	[0.000]	[0.002]	[0.072]	[0.069]	[0.014]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	324,540	324,540	324,540	324,540	324,696	324,696	324,696	324,696

Notes: WLS estimates. Each column of each panel is a separate regression. The dependent variable in columns (1)–(3) and (5)–(7) is a binary indicator for the outcome indicated in the column header scaled as a percentage. "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". Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.
Source: Spanish population censuses 2001 and 2011.

Table 2. Changes in interregional migration

	Men		Women	
	(1)	(2)	(3)	(4)
	INTER1	*KM_INTER1*	*INTER1*	*KM_INTER1*
Treatment	0.455	2.929	0.495**	8.663
	(0.279)	(4.848)	(0.211)	(6.921)
	[0.152]	[0.546]	[0.061]	[0.280]
Treatment*Post	-0.226	-11.383	-0.538*	-14.545**
	(0.246)	(7.109)	(0.260)	(6.437)
	[0.409]	[0.154]	[0.052]	[0.030]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes
Observations	344,414	40,337	344,930	47,341
Mean of dep. var.	11.503	409.888	13.178	388.724

Notes: WLS estimates. Each column is a separate regression. The dependent variable in columns (1) and (3) is the binary indicator shown in the column header scaled as a percentage. Columns (2) and (4) include 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”. Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.
Source: Spanish population censuses 2001 and 2011.

Table 3. Changes in intraregional migration

	Men	Women
	(1)	(2)
	INTRA1	*INTRA1*
Treatment	0.640*	-0.267
	(0.357)	(0.401)
	[0.093]	[0.597]
Treatment*Post	-0.327	0.825**
	(0.433)	(0.365)
	[0.489]	[0.026]
Census, birth region, and cohort FE	Yes	Yes
Census FE*Treatment	Yes	Yes
Observations	304,077	297,589
Mean of dep. var.	26.027	28.984

Notes: WLS estimates. Each column 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". Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.

Source: Spanish population censuses 2001 and 2011.

Table 4. Changes in the probabilities of intraregional migration (%)

	Men			Women		
	Town of origin size					
Town of destination size	(1) Small	(2) Medium	(3) Large	(4) Small	(5) Medium	(6) Large
No migration	*87.917*	*86.974*	*84.720*	*87.003*	*88.072*	*86.107*
Treatment	0.148	0.000	-0.212	0.549***	0.117	0.173
	(0.167)	(0.259)	(0.214)	(0.196)	(0.191)	(0.216)
Treatment*Post	-0.559*	-0.004	0.481	-0.009	-0.158	-1.337***
	(0.330)	(0.498)	(0.399)	(0.376)	(0.449)	(0.423)
Small	*5.217*	*3.755*	*3.783*	*5.515*	*3.300*	*3.321*
Treatment	0.026	0.222***	-0.031	-0.004	-0.073	-0.021
	(0.189)	(0.082)	(0.131)	(0.164)	(0.111)	(0.106)
Treatment*Post	0.083	-0.030	0.105	0.120	0.017	0.188
	(0.377)	(0.161)	(0.252)	(0.349)	(0.212)	(0.204)
Medium	*4.545*	*6.483*	*7.779*	*4.701*	*5.981*	*6.988*
Treatment	-0.170	-0.072	0.263*	-0.271	-0.014	-0.189
	(0.153)	(0.153)	(0.157)	(0.188)	(0.155)	(0.129)
Treatment*Post	-0.216	0.106	-0.002	0.248	-0.052	0.877***
	(0.297)	(0.316)	(0.284)	(0.397)	(0.330)	(0.283)
Large	*2.321*	*2.788*	*3.719*	*2.782*	*2.647*	*3.584*
Treatment	-0.005	-0.151	-0.019	-0.274**	-0.030	0.038
	(0.155)	(0.102)	(0.114)	(0.108)	(0.070)	(0.105)
Treatment*Post	0.693**	-0.073	-0.584**	-0.359	0.193	0.272
	(0.289)	(0.214)	(0.238)	(0.229)	(0.150)	(0.219)
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Test of Equation (6)	[0.131]	[0.861]	[0.166]	[0.439]	[0.687]	[0.047]
Observations	112,033	104,914	113,684	102,302	109,297	119,536
Log-likelihood	-511,710.3	-878,774.7	-1,059,799	-473,887.8	-819,187.7	-998,902.7
R-squared	0.028	0.033	0.030	0.044	0.040	0.038

Notes: Weighted estimates. Each column is a separate regression, where the dependent variable indicates no migration, migration to a small town, migration to a medium town, or migration to a large town over the previous 10 years. The percentages for each outcome are in italics. For example, among men living in a small town 10 years prior to the Census, 87.9% did not migrate, 5.2% 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". Conventional cluster-robust standard errors 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%.
Source: Spanish population censuses 2001 and 2011.

Table 5. Changes in maternal characteristics

	(1) More than primary education	(2) Out-of-birth region residence	(3) Out-of-birth province residence	(4) Out-of-birth province residence (within same region)
Treatment	0.837	0.815	1.244	0.610
	(0.786)	(0.976)	(0.993)	(0.538)
	[0.356]	[0.406]	[0.254]	[0.305]
Treatment*Post	-1.974*	-0.093	-0.854	-0.966
	(1.071)	(1.274)	(1.402)	(0.892)
	[0.085]	[0.936]	[0.582]	[0.328]
Birth region and cohort FE	Yes	Yes	Yes	Yes
Observations	22,606	22,578	22,578	17,540
Mean of dep. var.	16.734	23.797	28.402	6.043

Notes: WLS estimates. Each column is a separate regression, where the dependent variable is a binary indicator for the outcome indicated in the column header scaled as a percentage. Column (4) excludes out-of-birth region residents. “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”. Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.
Source: 1991 Socio-Demographic Survey.

Table 6. Changes in interregional migration

Panel 1: Men	(1) *INTER2*	(2) *INTER3*	(3) *INTER1b*	(4) *INTER2b*	(5) *KM_INTER2*	(6) *KM_INTER3*
Treatment	0.516	-0.145	0.339	0.648	4.661	-12.852
	(0.372)	(0.159)	(0.372)	(0.463)	(4.819)	(20.884)
	[0.211]	[0.399]	[0.380]	[0.188]	[0.362]	[0.527]
Treatment*Post	-0.382	0.221	-0.281	-0.709*	-3.590	6.618
	(0.314)	(0.186)	(0.295)	(0.400)	(6.439)	(15.894)
	[0.271]	[0.282]	[0.368]	[0.116]	[0.579]	[0.676]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	344,414	344,414	186,348	186,348	55,242	13,783
Mean of dep. var.	16.651	3.903	9.428	16.135	447.581	469.471
Panel 2: Women	(1) *INTER2*	(2) *INTER3*	(3) *INTER1b*	(4) *INTER2b*	(5) *KM_INTER2*	(6) *KM_INTER3*
Treatment	0.981***	-0.054	0.599*	1.214***	9.178***	-1.119
	(0.264)	(0.148)	(0.295)	(0.319)	(2.981)	(9.410)
	[0.003]	[0.726]	[0.103]	[0.008]	[0.002]	[0.912]
Treatment*Post	-0.936***	-0.096	-0.378	-1.110**	-11.550**	-9.582
	(0.295)	(0.154)	(0.341)	(0.388)	(4.294)	(11.700)
	[0.003]	[0.568]	[0.302]	[0.009]	[0.022]	[0.442]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes	Yes
Observations	344,930	344,930	187,167	187,167	61,084	13,795
Mean of dep. var.	17.980	3.918	10.741	17.319	430.027	454.304

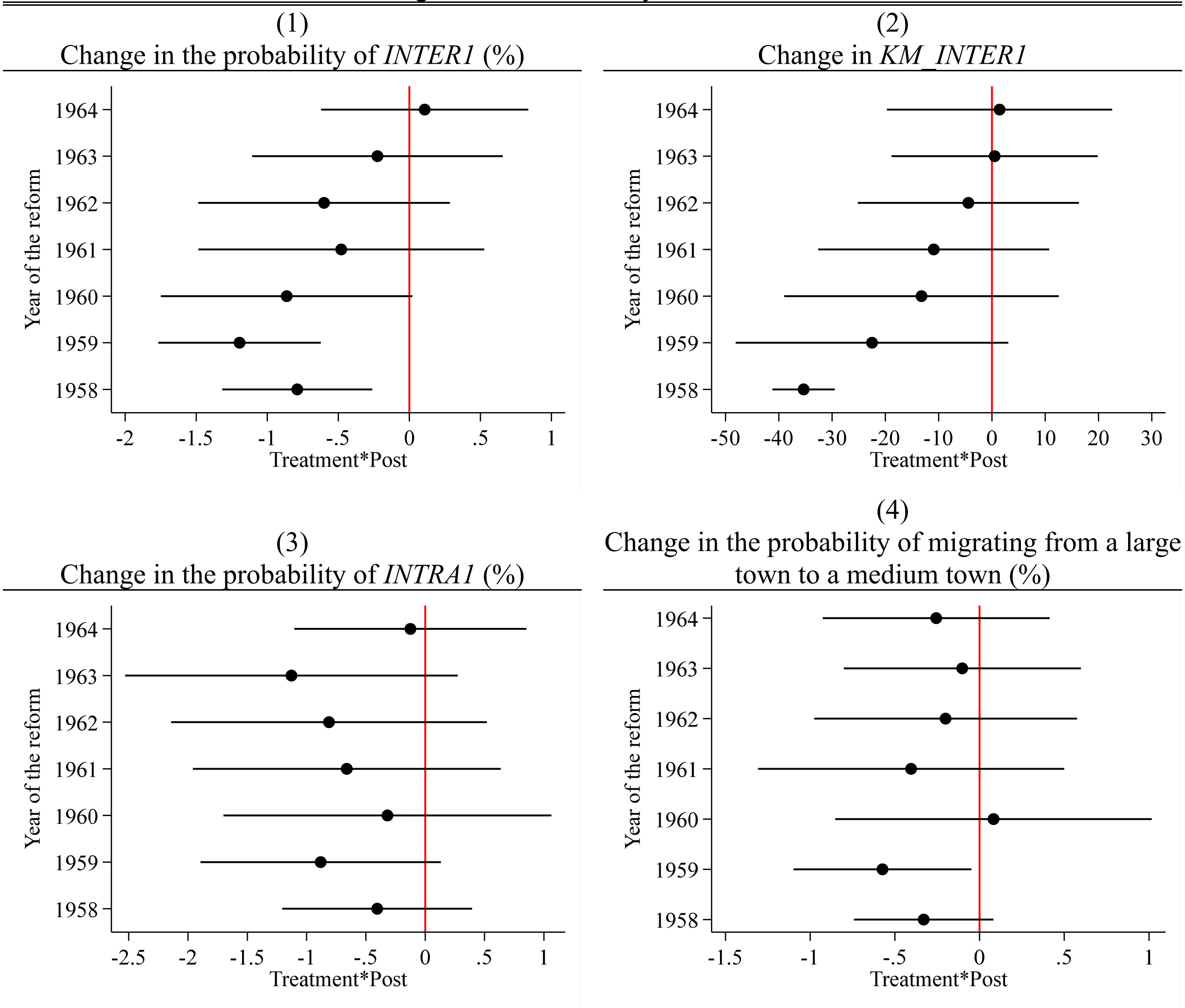
Notes: WLS estimates. Each column of each panel is a separate regression. The dependent variable in columns (1)–(4) is the binary indicator shown in the column header scaled as a percentage. Columns (5) and (6) include migrants only. "Treatment" are individuals born in March–May and the control group are those born in August–October. Cohorts 1957–1965 and 1967–1975, the latter indicated by "Post". Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.
Source: Spanish population censuses 2001 and 2011.

Table 7. Changes in intraregional migration

Panel 1: Men	(1) *INTRA2*	(2) *INTRA3*	(3) *INTRA4*	(4) *BIGGER*	(5) *SMALLER*
Treatment	0.313	0.002	0.340	-0.038	0.256**
	(0.191)	(0.072)	(0.210)	(0.134)	(0.111)
	[0.139]	[0.975]	[0.142]	[0.784]	[0.034]
Treatment*Post	-0.101	0.126*	-0.120	0.100	0.038
	(0.316)	(0.064)	(0.283)	(0.141)	(0.152)
	[0.443]	[0.077]	[0.675]	[0.484]	[0.798]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes
Observations	289,172	330,631	330,631	330,631	330,631
Mean of dep. var.	4.800	1.353	15.193	3.262	6.792
Panel 2: Women	(1) *INTRA2*	(2) *INTRA3*	(3) *INTRA4*	(4) *BIGGER*	(5) *SMALLER*
Treatment	0.275	0.071	-0.375	-0.007	-0.508**
	(0.231)	(0.068)	(0.224)	(0.089)	(0.186)
	[0.271]	[0.329]	[0.110]	[0.939]	[0.027]
Treatment*Post	0.014	-0.021	0.485*	0.051	0.449**
	(0.235)	(0.080)	(0.269)	(0.084)	(0.175)
	[0.956]	[0.787]	[0.095]	[0.582]	[0.027]
Census, birth region, and cohort FE	Yes	Yes	Yes	Yes	Yes
Census FE*Treatment	Yes	Yes	Yes	Yes	Yes
Observations	283,846	331,135	331,135	331,135	331,135
Mean of dep. var.	5.634	1.405	14.570	3.162	6.201

Notes: 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". Conventional cluster-robust standard errors are in parentheses and wild bootstrap *p*-values in brackets. *, **, and ***: Conventionally significant at 10, 5, and 1%.
Source: Spanish population censuses 2001 and 2011.

Figure 1. Placebo analyses. Women



Notes: Weighted estimates. Graphs (1)–(3) plot the point estimate and the conventional cluster-robust 95% confidence interval of $Treatment_{ic} * Post_c$ from Equation (1). Graph (4) plots the average partial effect and the conventional cluster-robust 95% confidence interval of $Treatment_{ic} * Post_c$ from Equation (2). Both equations are estimated on the cohorts 1957–1965 assuming that the reform affected the cohorts 1958–65, 1959–65, 1960–65, 1961–65, 1962–65, 1963–65, or 1964–65. The dependent variable is shown in the graph header. Each fake reform is denoted by the earliest cohort affected.
Source: Spanish population censuses 2001 and 2011.