Healthier Starts: The Intergenerational Effects of Maternal General Knowledge Acquisition

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Abstract

We study the long-term causal impacts of increasing mothers' general education on infant health outcomes using quasi-experimental variation from a education reform in Spain that integrated more general education into secondary schooling. Educational programs that aim to deliver more general knowledge may improve women's earning potential and maternal prenatal investment by increasing the portability of skills across occupations and improving women's ability to make informed decisions about marriage, fertility options and health behavior. Drawing on detailed birth certificate records linked to granular enrollment data, we find that the reform led to a significant reduction in the incidence of very premature birth (less than 32 gestational weeks), and declines in very low birth weight (less than 1,500 grams) and premature births (less than 37 weeks). Additional high-quality survey data on adult information processing skills, substance abuse, and labor and marriage market choices allow us to unpack the mechanisms behind the causal effects of more general education on health at birth. The positive effects of the reform on infant health appear to be largely driven by improved information processing skills, which improve the ability to avoid risky behaviors during pregnancy, support better family planning decisions, and facilitate entry into the labor market.

JEL Codes: I12; I14; I28

Keywords: Health at Birth; Health Behaviors; Information Processing Skills; Intergenerational Transmission

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1 Introduction

Health at birth deficits, such as low birth weight and preterm birth, have persistent negative effects on individual well-being and impose substantial burdens on both health systems and families (Cunha et al., 2010; Currie, 2011; Almond et al., 2018). For individuals, being born preterm is the leading cause of infant mortality before the age of five, and the labor market penalties of being born low weight are large. A 10% decrease in birth weight is comparable to losing a quarter year of education, reducing full-time earnings by approximately 1% (Black et al., 2007; Currie and Moretti, 2007). For the economy as a whole, the financial burden is further reflected in high medical costs. The expense of saving a newborn life at a birth weight near 1,500 grams is estimated at approximately $550,000 (in 2006 dollars) (Almond et al., 2010). Despite significant advancements in medical care over the past decade, the challenge remains, as the global incidence of preterm birth has remained largely unchanged (WHO-UNICEF, 2023). This persistent trend has led to growing calls for interventions beyond medical care, emphasizing the need to address socioeconomic factors such as poverty and access to education to effectively tackle these deficits (Almond and Currie, 2011; Aizer and Currie, 2014). While prior research has shown that maternal education matters for child health, less is known about whether the type of education, particularly general versus vocational, also plays a role. This gap reflects the challenge of isolating the effects of different types of maternal schooling in real-world settings. Leveraging a nationwide reform in Spain, this paper provides the first causal evidence on the impact of mothers' general education on children's health at birth.

Standard human capital models posit two complementary channels by which increased maternal general education enhances information processing skills and, in turn, improves infant health (Cunha and Heckman, 2007, Heckman, 2007). First, stronger information-processing skills expand mothers' ability to make decisions and adopt health-promoting behaviors (Thomas et al., 1991), providing the cognitive flexibility needed to acquire, interpret, and act on new information (Grossman, 1972). Mothers with higher skills are more likely to learn about and adopt healthier habits, such as reducing smoking and increasing the use of prenatal care (Currie and Moretti, 2003), as well as to use contraception more effectively, reducing unplanned pregnancies and allowing greater control over birth timing (Rosenzweig and Schultz, 1989). Second, stronger information-processing skills can raise mothers' earning potential, either directly by equipping them with transferable skills valued in a changing labor market (Goldin, 2001, Hanushek et al., 2017), or indirectly through positive assortative mating with more educated partners (Behrman and Rosenzweig, 2004). Higher household income generated through these channels enables greater investment in health inputs, including prenatal care, nutrition, and housing, which contribute to improved infant health outcomes (Currie, 2009).

Spain's 1990 LOGSE reform provides a unique setting to study the effects of general education. The reform, enacted by the Spanish national government in 1990, aimed to improve workforce competitiveness and support EU integration by expanding general education in lower secondary education. Prior to the reform, students could choose between general and vocational education at age 14. LOGSE delayed this specialization by requiring all students to follow a common general education program until age 16. The reform was implemented gradually across provinces between 1989 and 2000, creating substantial variation in exposure across birth cohorts and regions. The pace of rollout was shaped by administrative and logistical constraints such as teacher shortages, infrastructure delays, and local planning constraints. These implementation frictions are plausibly unrelated to trends in infant health and provide a source of exogenous variation that we use for identification. Importantly, LOGSE increased exposure to general education without changing total years of schooling, as enrollment among 14- to 16-year-olds was already nearly universal, following

a 1980 labor law that set the minimum working age at 16. Since vocational and general education programs were typically offered within the same schools, the reform introduced minimal changes in peer composition, helping to isolate the effect of the role of education content in shaping longer-term outcomes.

Our analysis draws on a set of linked administrative and survey data that provides unusually detailed information on both educational exposure and infant health outcomes. To capture the phased implementation of LOGSE across provinces and birth cohorts, we use newly digitized enrollment records from the 1989–2000 school years and construct a continuous measure of intensity of exposure to the reform. This index reflects the share of 14-year-olds enrolled under the new system in each province and year, enabling a clean definition of first exposure that closely matches how the reform was implemented. We link this exposure index to population-wide birth records, which include rich maternal characteristics and precise indicators of infant health. To examine potential mechanisms, we complement the analysis with nationally representative surveys on education, cognitive skills, labor market outcomes, and maternal health behaviors.

We estimate the causal effect of maternal general education on infant health using quasi-experimental variation from LOGSE's staggered rollout across provinces and birth cohorts. Our identification strategy successfully allow us to deal with identification threats particular to the reform. One concern is that early- and late-adopting provinces may have differed in pre-existing trends in infant health. For instance, provinces on faster improvement trajectories in birth outcomes might have implemented the reform earlier, confounding the effects of LOGSE with underlying health dynamics. Families or schools may also have changed their behavior in anticipation of the reform, such as reallocating resources or planning around the expected new curriculum, potentially affecting exposure before formal implementation. Additionally, the impact of the reform may vary depending on how intensively and swiftly provinces implemented the new curriculum. If provinces that adopted LOGSE more comprehensively also made complementary investments in education, observed health improvements might partly reflect those additional changes. Our empirical strategy addresses these concerns by allowing treatment effects to vary flexibly with the timing and intensity of the reform, mitigating bias from assuming a uniform effect, and by estimating outcome dynamics relative to exposure, directly testing for differential pre-trends and anticipatory responses (see De Chaisemartin and D'Haultfœuille, 2024). We also assess robustness to province-level time-varying confounders and selective mobility by showing that reform rollout is uncorrelated with migration patterns and evolving provincial characteristics. Finally, we address concerns that changes in fertility behavior or peer composition around the time of the reform could confound our estimates, for instance, if the reform led to selective fertility or altered the characteristics of school cohorts. We investigate this directly by examining trends in fertility and maternal characteristics across cohorts and find no systematic differences by reform exposure.

Consistent with the policy design, the reform had the intended effect of increasing mothers' general knowledge and learning skills, while keeping years of schooling and educational attainment constant. Our estimates from labor force survey data point towards a significant shift in female enrollment from the pre- to post-reform high school systems. We fail to find any effect on either total years of schooling, the share of women obtaining a high school degree (regardless of the educational program), or the share of women obtaining a college degree. Hence, we can confidently isolate the effects of increased general education on health at birth outcomes without confounding them with changes in the quantity of education.

Our estimates suggest that greater exposure to general education during high school leads to better health outcomes at birth. We find the largest gains in the most severe and mortality-predictive

infant health at birth outcomes. Specifically, the reform led to a 37% reduction in the incidence of very preterm births (less than 32 gestational weeks). In settings with a similar incidence of very preterm birth, such as the U.S., where these births account for 52% of infant deaths before age five (Barfield, 2018), this reduction could potentially translate into a 19.2% decrease in infant mortality. We also find consistent reductions of 8.4% in premature births (less than 37 weeks) and 25.71% in very low birth weight (less than 1,500 grams), although these estimates are less precisely estimated. In absolute terms, these changes imply there were over 388 fewer very preterm births, 869 fewer premature births, and 270 fewer very low birth weight babies annually out of 150,000 births per maternal cohort. As an additional point of comparison, given that prior research finds a 54% reduction in the odds of very preterm birth from participation in a nutrition program (WIC) (Bitler and Currie, 2005), our findings suggest that two additional years of general education yield an effect nearly 70% as large.

To provide further support that the positive effects of the reform on infant health at birth come from more general education and not from other factors associated with the reform, we perform several additional analyses. First, we show that the estimated positive impacts of the reform on infant health at birth are indeed driven by students who shifted from vocational to general education as a result of the reform. This is consistent with gains from more general education driving our results, and not other factors that would have benefited similarly both students shifting from vocational studies and students continuing in general education. Second, we are also able to rule out that our results are driven by changes in peer composition by which low-ability students would have benefited from interactions with high-ability peers instead of from increased general education.[1] Finally, we find no evidence that the reform influenced women's decisions to delay motherhood or have fewer children. Both factors could lead to better infant health outcomes in the context of a quantity-quality theory of children, independent of education content.[2] In sum, increased general education and not other features of the reform, peer effects, or changes in fertility patterns appear to be behind the positive impacts of the policy on infant health at birth.

What explains the positive effects of mothers' increased general education on infant health at birth? To shed light on this question, we examine several potential channels identified in the literature. We first test whether an exogenous increase in general education during adolescence provided mothers with better information-processing skills in adulthood. Mothers exposed to more general education scored significantly higher in the 2012 PIACC literacy and numeracy assessments. These skill gains translated into lower rates of alcohol consumption during pregnancy and a higher likelihood of marriage before birth, pointing to healthier behaviors and greater ability to plan for motherhood and the timing of births. We also find that the reform led to a 3.2% increase in maternal labor force participation, with no accompanying changes in occupational status, partnership rates, or partner quality, indicating effects mainly on the extensive margin of labor supply. Overall, the observed reductions in the incidence of very preterm births and very low birth weight among mothers exposed to the policy reform are consistent with improvements in information-processing skills and related health behavioral responses, alongside gains in earning potential through higher maternal labor force participation.

This paper provides new causal evidence on the role of general maternal education in shaping early-life health. We contribute to a broader literature on the origins and intergenerational transmission

[1] The literature on peer effects in education has consistently shown that peers can influence a wide range of outcomes, from academic achievements to health-related behavior (for a review, see Sacerdote, 2011).

[2] According to Becker's theory, if education increases the value of a woman's time, women may decide to have fewer children of a better quality, possibly later in life (Becker, 1973; Goldin, 2006).

of inequality.[3] Several studies suggest that reforms expanding both the length and general content of education improve infant health. For example, the 1959 Norwegian school reform increased years of schooling while broadening general education and led to better birth outcomes (Grytten et al., 2014). Similar findings emerge from studies of U.S. college expansions, which raised both educational attainment and curricular breadth (Currie and Moretti, 2003). More recent evidence from piecemeal U.S. curriculum reforms targeting specific subjects like math suggests improvements in child outcomes (Dursun et al., 2024). In contrast, reforms that increased schooling duration without increasing general education content, such as the 1944 UK Education Act or U.S. school entry laws, have shown little to no impact on infant health (Lindeboom et al., 2009; McCrary and Royer, 2011). Together, these mixed findings suggest that schooling duration alone may not explain intergenerational gains. One plausible explanation is that the scope of general education content plays a central role. However, this hypothesis has been difficult to test directly as most nationwide reforms affect both general education content and duration, limiting causal inference in large-scale and policy-relevant settings. We leverage a unique nationwide policy reform that exogenously increased exposure to general education while holding years of schooling constant. Our results show that greater exposure to general maternal education improves early-life health, underscoring the importance of educational content as a policy lever for promoting intergenerational mobility, even in a context with universal prenatal health coverage, such as Spain.

We also provide new evidence on a theory-driven mechanism linking maternal education to infant health through improvements in information-processing skills. Most existing studies focus on income, prenatal care, or health behaviors as potential channels (Lindeboom et al., 2009; McCrary and Royer, 2011; Dursun et al., 2024). Much less is known about how these channels operate through cognitive abilities, in part because data rarely allow researchers to track individuals from secondary schooling through to early adulthood. We address this gap by linking exposure to a high school reform to maternal information-processing skills and to health behaviors, marriage outcomes, and labor supply. Our paper highlights that information-processing skills provide the foundation through which general education affects maternal decision-making and infant health. Our findings suggest that general education acquired during this formative period can influence multiple domains of women's lives with intergenerational consequences, reinforcing the view that adolescence is a crucial window for educational investments with far-reaching returns (Almond et al., 2018; Guryan et al., 2023; Currie and Aizer, 2024).

This paper is organized as follows. The second section reviews the institutional framework of the Spanish secondary education system in the pre- and post-reform periods. The third section describes the sample, data, and variables used, as well as the reform exposure index that underpins our identification strategy. The fourth section discusses the methodology employed to test the effect of more general education on health at birth. The fifth section presents our findings on the impacts of general education on maternal education and infant health outcomes, along with the results of several checks to validate our identification strategy. We discuss the underlying mechanisms through which increased general education may lead to better infant health at birth in section six. Finally, the final section draws some conclusions.

[3]This literature has examined a wide range of early-life determinants, including prenatal substance use, exposure to pollution, nutrition, poverty, cash and near-cash transfers, maternal stress, participation in social programs, and education (see Currie et al., 2010;Björklund and Salvanes, 2011; Almond and Currie, 2011;Aizer and Currie, 2014; Almond et al., 2018).

2 Institutional Background: the LOGSE Reform

The 1990 LOGSE reform was introduced by the national government with the aim of improving workforce competitiveness and supporting Spain's integration into the European Union. Before LOGSE, the Spanish education system was governed by the 1970 LGE reform, which allowed students to access either upper secondary general education or vocational studies at age 14. Following the LOGSE reform, all students were required to follow a common core of general education subjects until age 16. This change delayed early specialization and ensured that all students acquired core academic skills before entering the labor market.[4]

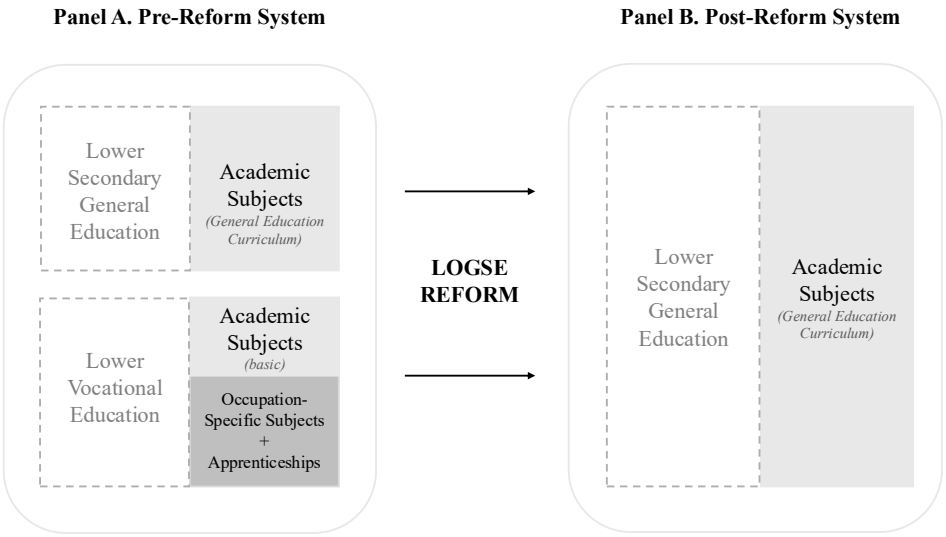
Figure 1 compares the pre- and post-reform lower secondary education curricula. Panel A shows that, before LOGSE, vocational students at age 14 received limited general instruction, focusing instead on occupation-specific training and apprenticeships. In contrast, students in the general education program followed a broader set of academic subjects, referred to as the general education curriculum. As illustrated in Panel B, the reform eliminated vocational instruction from lower secondary education, requiring all students to complete a common core in general education subjects through age 16. While the academic content of the general education program remained largely unchanged, the reform substantially altered the curricula for vocational students by extending to them the same general education curriculum previously followed only by their general education program peers.

The Spanish central government allowed education centers up to ten school years, from 1989-1990 to 1999-2000, to fully implement the LOGSE reform. This resulted in a staggered roll-out across provinces with students' exposure to the reform varying substantially both across space and time. Panel A in Figure 2 uses enrollment data from national education registries to show how, over the transition period, the share of 14-year-olds enrolled in the post-reform system steadily increased, while enrollment in the pre-reform system declined at a similar rate. Importantly, dropout rates remained close to zero throughout this period. At the start of the rollout in 1989–1990 school year, 5% of 14-year-olds were in the post-reform secondary education system. Enrollment shifted gradually, reaching a 50–50 split by 1996–1997 school year, and 99% in the post-reform system by 1998–1999 school year. Panel B in Figure 2 highlights that the pace of implementation differed markedly across provinces, contributing to significant spatial variation in students' exposure to the reform. This variation is visually represented through shading, with darker provinces adopting earlier. Some provinces reached over 50% enrollment of 14-year-olds in the post-reform system as early as the 1994–1995 school year, while others did not cross this threshold until 1998–1999 school year.

The timing of implementation varied due to administrative decisions made across multiple levels of government including local, regional, and national. These differences arose from the complex and often lengthy processes involved in navigating the regulatory framework. Regional and local education authorities were responsible for adapting school infrastructure and the supply of subject-specific teachers to meet the new curricular requirements, while the national government set the legislative framework and administered the funding. As noted by Marchesi (2020), the implementation process faced a range of administrative challenges that varied considerably across provinces. First, local systems faced acute teacher shortages driven by the expanded academic requirements for vocational students. Second, infrastructure-related delays arose from the need to reconfigure

[4]For the legal foundations of the reforms, see Organic Law 1/1990, which implemented the LOGSE reform (*Ley Orgánica de Ordenación General del Sistema Educativo*), and Law 14/1970, which introduced the earlier LGE reform (*Ley General de Educación*).

Figure 1: Educational Curricula Before and After the Reform At Age 14



Notes: The general education curriculum under the pre-reform system (Panel A) included core academic subjects such as mathematics, sciences, Latin, social sciences, languages (native and foreign), and physical education. Vocational students under the pre-reform system (Panel A) received basic academic instruction limited to social sciences, languages, and physical education, alongside five occupation-specific subjects and apprenticeships. In the post-reform system (Panel B), the common general education curriculum included the same core academic subjects in the pre-reform system (mathematics, academic stream-specific subjects such as sciences and Latin, social sciences, languages (native and foreign), and physical education) plus ethics, and technology and digitalization.

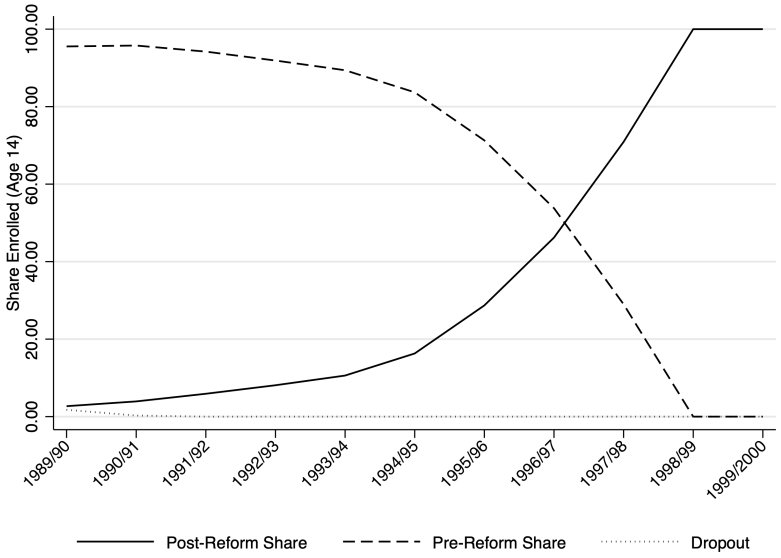
facilities in order to integrate these students into the enhanced general education framework. Third, unexpected changes in cohort sizes also disrupted planning and implementation. These shocks are arguably orthogonal to potential changes in the health of the next generation, and provide a source of exogenous variation that we exploit for identification.

Due to key institutional features, the LOGSE reform increased general education content while keeping other critical factors largely unchanged. First, the reform did not affect total years of schooling. According to official figures secondary school enrollment among 14- to 16-year-olds was already nearly universal, around 95%, before the reform.[5] As documented by Bellés-Obrero and Duchini (2021), this was largely due to a 1980 national labor law setting the minimum working age at 16, which effectively limited the outside options available to teenagers and kept them in school.[6] Our first-stage results confirm this in section 5.1. Second, the LOGSE reform led to limited changes in peer composition, as students were already learning in integrated environments prior to its implementation given that vocational and general education programs were frequently housed within the same school buildings, a pattern supported by the evidence in section 5.3.3.

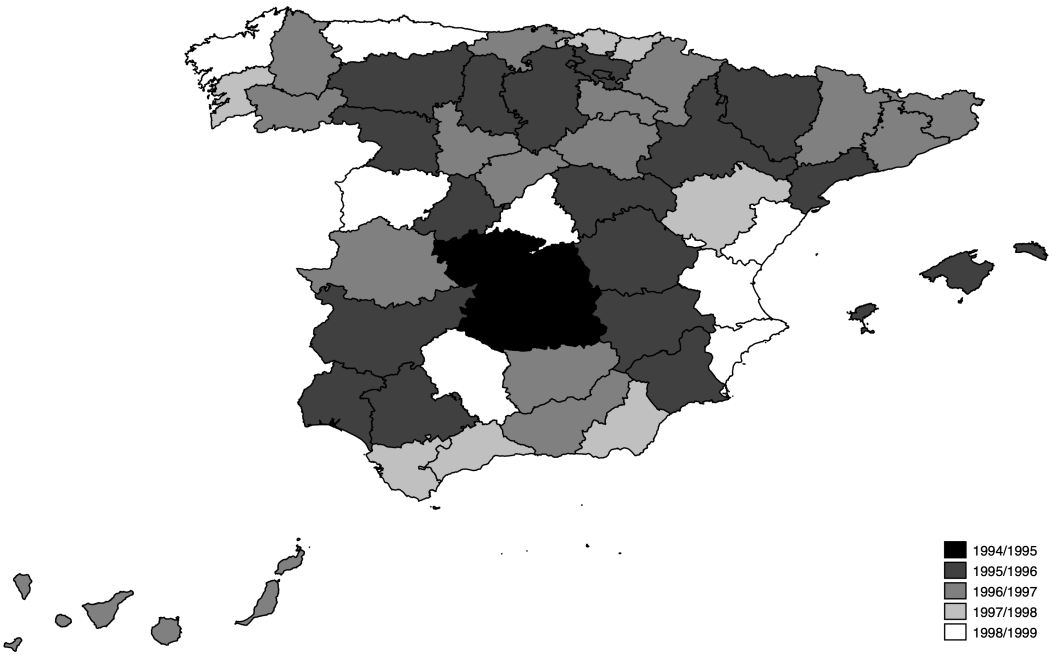
[5] See Servicio de Estudios Estadísticos (1994) for enrollment rates.
[6] Law 8/1980 of the Workers' Statute.

Figure 2: Geographic and Temporal Variation in LOGSE Reform Implementation

Panel A. Enrollment Shares of 14-Year-Olds in Pre- and Post-Reform Systems, by School Year



Panel B. School Year in Which Each Province Reached 50% Post-Reform Enrollment



Notes. Panel A shows the levels of enrollment of 14-year-olds across the pre- and post-reform high school system during the transition period. Panel B shows the first school year in which a province reached more than 50% of 14-year-old students enrolled under the post-reform education system. Provinces are shaded from white to black, with darker shades indicating earlier adoption and lighter shades indicating later adoption. Source: Annual Education Statistics reports, Statistical Office of the Education Ministry, multiple years: 1989–2001.

3 Data

To measure the causal impact of mothers' increased general education on children's health at birth, we use a rich set of high-quality administrative registers that offer both fine-grained educational exposure measures and complete population coverage of birth outcomes. Table A1 in the Appendix provides an overview of the data sources, including the register name, unit of observation, a brief description, main outcome variables, and other available information.

To measure exposure to the LOGSE reform, we construct a province-level intensity index using administrative enrollment records from 1989–1990 to 1999–2000, which we manually digitized. The index captures the granular variation in implementation intensity at age 14 across birth cohorts and provinces (see Panel A in Table A1 in the Appendix). Specifically, the LOGSE exposure index reflects the number of 14-year-old students enrolled in the old and new systems during the transition period, based on high-resolution province-by-cohort data. Focusing on 14-year-olds allows for a sharp definition of treatment that aligns with the institutional design of the reform. This approach minimizes contamination from partially treated or untreated individuals, ensures that we capture true variation in general education exposure (rather than downstream educational choices), and reduces measurement error in treatment assignment. The index is calculated as shown in equation (1):

$$I_{tk} = \frac{LOGSE_{tk}}{LOGSE_{tk} + LGE_{tk}} \tag{1}$$

Where I_{tk} is a continuous treatment variable that captures the intensity and timing of LOGSE implementation, measured as the proportion of 14-year-old students in cohort t and province k enrolled under the new system. The numerator reflects the number of students enrolled in the post-reform (LOGSE) general education program, while the denominator includes all 14-year-olds enrolled in either the post-reform (LOGSE) system or the pre-reform (LGE) general or vocational programs. To precisely identify first-time exposure, we include in the numerator repeaters from earlier cohorts who, due to grade repetition, were 14 years old and enrolled when the new system was introduced. We exclude repeaters from the current cohort who had not progressed to the appropriate grade and thus were not yet exposed. The index ranges from 0 (no student enrolled under LOGSE) to 1 (full coverage within a province-cohort). Figure A1 in the Appendix presents three province-level maps illustrating the evolution of the LOGSE exposure index (I_{tk}) across representative school years during the transition period. By combining fine-grained administrative data with a cohort- and grade-specific construction strategy, the index captures detailed variation in exposure intensity across provinces and birth cohorts.

To measure birth outcomes for the children of mothers exposed to the reform, we use population-wide microdata from birth certificates provided by the Spanish National Statistical Institute (INE) (see Panel A in Table A1 in the Appendix). The dataset covers the universe of births in Spain from 2000 to 2018, drawn from the official civil registry, where registration is mandatory and completed shortly after delivery. In addition to detailed child-level outcomes, such as gestational age, birth weight, and birth order, the data include rich information on maternal characteristics, including, province of residence, and cohort of birth. This level of detail is crucial for accurately linking mothers to their exposure to the LOGSE reform, which we define based on the mother's cohort

and province of residence at the time of childbirth.[7] The combination of comprehensive population coverage and unusually rich maternal detail makes these data well suited for linking to education records and for studying how changes in maternal education affect children's health at birth.

In our main analysis, we focus on first births to mothers born between 1975 and 1985, aged 25 to 33 at the time of delivery. These women turned 14 during the 1990s and were differentially exposed to the LOGSE reform depending on their cohort and province of residence. Restricting to first births helps avoid concerns that education might affect completed fertility, and focusing on mothers aged 25 and older ensures that most had completed their education by the time they gave birth.[8] The upper bound of age 33 corresponds to the oldest observable age for the youngest cohort in our data. This selection yields a large and representative sample of 1,576,204 first births between 2000 and 2018.

Following standard practice in the literature, we proxy health at birth using birth weight and gestational age. In particular, we focus on the incidence of low birth weight (less than 2,500 grams), very low birth weight (less than 1,500 grams), preterm birth (less than 37 weeks), and very preterm birth (less than 32 weeks). These clinically meaningful and widely accepted measures are widely used to document the strong association between maternal education and infant health, and to capture the impact of early health on later-life outcomes.[9]

The incidence of adverse birth outcomes in our sample is consistent with rates observed in other high-income countries, supporting the credibility of our data. This pattern is in line with expectations given Spain's universal public healthcare system, which provides extensive prenatal, delivery, and postpartum care. Summary statistics and detailed definitions for these health outcomes at birth are available in Table A3 in the Appendix.[10]

To further support the validity of our identification strategy and examine potential mechanisms linking maternal education to infant health, we draw on several complementary administrative and survey data sources. These additional datasets, summarized in Panel B of Table A1 in the Appendix, allow us to assess changes in maternal educational trajectories, labor market outcomes, cognitive skills, and health behaviors, thereby enabling a richer understanding of how the reform may have improved early-life health.

To study the educational choices of potential mothers, we use the 1991–2018 Spanish Labor Force Survey (LFS), a nationally representative, continuous quarterly survey conducted by the national statistical office. The LFS collects demographic, labor market, and educational attainment information for all household members aged 16 and older across roughly 65,000 households (about 160,000 individuals) per quarter. In line with prior studies and to avoid seasonality concerns, we use data from the second quarter of each year.[11] Our analytic sample includes 194,687 Spanish women born between 1975 and 1985, the cohorts directly affected by the reform, aged 17 to 33 at the time of interview, closely mirroring the women in our main birth register sample.

[7] We use the mother's province of residence at the time of delivery as a proxy for the province where she attended high school. This is a reasonable assumption in the Spanish context, where internal migration is relatively low. Using microdata from the 2021 Spanish Census, we estimate that over 90% of women give birth in the same province where they lived at age 14. In section 5.4, we also show that the staggered rollout of the reform was not associated with changes in cross-province migration patterns among women in our sample.

[8] We examine potential impacts of the reform on fertility timing and selection into motherhood in section 5.3.2.

[9] See, for example, Currie and Moretti, 2003; Behrman and Rosenzweig, 2004; Almond et al., 2005; Oreopoulos et al., 2006; Chou et al., 2010; McCrary and Royer, 2011, among others.

[10] The 1–2% incidence of very preterm births aligns with prevalence rates in other high-income countries (Larroque, 2004; Barfield, 2018; Ohuma et al., 2023).

[11] See for instance Amuedo-Dorantes et al. (2023).

The richness of the LFS allows us to examine both enrollment patterns during schooling years and completed educational attainment. To capture enrollment patterns before education is complete, we analyze the share of women enrolled in different programs between ages 17 and 24. To measure completed education, we use data on the highest qualification earned and the age of completion between ages 25 and 33, when most women have exited the education system. These analyses provide a detailed picture of how the reform influenced exposure to general education and offer strong support for the first stage of our identification strategy. Summary statistics and definitions are reported in Table A4 in the Appendix.

We use data from the 2011 Spanish wave of the Program for the International Assessment of Adult Competencies (PIAAC) to examine whether improved information-processing skills represent a key mechanism linking increased general education to infant health. Developed by the OECD, PIAAC provides internationally comparable data on adult literacy and numeracy competencies, skills closely associated with general education and relevant for decision-making, planning, and interpreting health-related information. We focus on these two domains and standardize the original scores (which range from 0 to 500) to have a mean of zero and a standard deviation of one, allowing us to interpret results in terms of standard deviation units and to compare effect sizes across outcomes. The survey also collects basic demographic information, including gender, year of birth, and region of residence, which we use to link respondents to our enrollment index.

We focus on a sample of 412 interviewed women aged 25 to 33 born between 1979 and 1985, who turned 14 during the years when the LOGSE reform was being implemented. This sample overlaps closely with the cohorts analyzed in our main specification and allows us to assess skill acquisition for mothers exposed to the reform. The timing of the survey provides a unique opportunity to observe cognitive outcomes in a nationally representative sample of adults affected by the reform, offering rare insight into a mechanism that is typically unobserved in similar studies. Summary statistics and variable definitions are provided in Table A5, Panel A, in the Appendix.

We complement this analysis by examining risky health behaviors using data from the Spanish Alcohol, Tobacco, and Illegal Drugs Surveys. Conducted by the Spanish Observatory on Drugs and Addictions, these surveys provide nationally representative data on psychoactive substance use and basic demographics, including year of birth and province of residence, for residents aged 15 to 64. Interviews are conducted in person at respondents' homes, with an interviewer present throughout the completion of the questionnaire to ensure data quality.

To capture health behaviors during pregnancy, we use the 2017 wave, the only one reporting alcohol consumption during pregnancy, and restrict the sample to mothers born between 1975 and 1985, yielding 2,365 observations.[12] To examine lifetime substance use, we draw on the 2007 wave, which includes questions on alcohol, tobacco, and drugs such as cannabis, cocaine, ecstasy, amphetamines, and tranquilizers. For this analysis, we restrict the sample to women aged 25 to 33 born between 1975 and 1983 who report at least one co-residing child, resulting in 1,048 observations.[13] Table A5, Panel B, in the Appendix presents the definitions and summary statistics of the relevant variables. Together, these data offer rare insight into behaviors that are typically unobserved in administrative sources and allow us to assess whether exposure to general education reduces risky maternal behavior, a key mechanism emphasized in the literature. Summary statistics and variable definitions are provided in Table A5, Panel B, in the Appendix.

[12] We do not restrict this sample by current household composition or age, as pregnancy can occur throughout the reproductive years, and the survey includes retrospective questions on behaviors during past pregnancies.

[13] We exclude women born after 1983, as they had not yet reached age 25 by 2007.

Finally, we examine increased maternal earning potential as a second channel through which general education may affect infant health, drawing on detailed information from the birth register data. Specifically, we analyze mothers' marital status, labor force participation, occupation, partner characteristics, and age at first birth. These outcomes capture broader socioeconomic returns to general education and provide insight into whether improved economic conditions for mothers contribute to better birth outcomes for their children. Table A6 in the Appendix presents variable definitions and summary statistics.[14]

4 Empirical model

To identify the causal impact of increased maternal general education on infant health at birth, we exploit the staggered implementation of a major education reform in Spain. Simple correlations between the type of maternal education and child health outcomes may suffer from endogeneity concerns. For instance, reverse causality can arise if healthier women are more likely to pursue general education instead of vocational studies, while omitted variables, such as maternal ability, could simultaneously affect both educational choices and infant health. We draw on quasi-experimental variation in exposure to general education generated by the differential rollout of the LOGSE reform across provinces and over time. We estimate a generalized DiD model using maternal exposure to the reform as a continuous treatment variable in a two-way fixed effects (TWFE) regression. Our baseline model is specified as

$$\mathrm{Y}_{itk} = \alpha + \beta \mathrm{I}_{\mathrm{tk}} + \eta_{\mathrm{t}} + \theta_{\mathrm{k}} + \epsilon_{itk} \tag{2}$$

where Y_{itk} is a child's outcome of interest for mother i from cohort t and province k. Y_{itk} represents weight at birth, number of gestational weeks, and indicators for low birth weight (under 2,500 grams), very low birth weight (under 1,500 grams), premature birth (less than 37 weeks), and very premature birth (less than 32 weeks). I_{tk} is our key regressor, which captures the intensity of LOGSE exposure for mothers born in year t in province k. Cohort year fixed effects η_{t} are included to monitor the socio-economic situation of each cohort, while province fixed effects θ_{k} account for any province-level factors correlated with education systems.[15]

Our coefficient of interest is β, which shows the relationship between our LOGSE exposure index and health outcomes at birth. A positive coefficient indicates that an increase in mothers' general education is associated with higher birth weight, longer gestation, and higher incidence of adverse outcomes such as preterm birth or low birth weight.

Our identification strategy relies on three assumptions for our coefficient of interest, β, to identify the average treatment effect on the treated (ATT) of increased maternal general education on infant health outcomes. First, in the absence of the reform, infant health trends would have evolved similarly across provinces. This parallel trends assumption could be violated if provinces that implemented the reform earlier were already on diverging health trajectories. Second, treatment timing must be unrelated to prior changes in infant health or anticipatory behaviors. In our setting, this rules out the possibility that provinces with worsening birth outcomes implemented the reform earlier, or that families or schools adjusted behavior in anticipation of LOGSE. Third, identification

[14] Because the coding of labor market participation and occupation changed after 2016, we limit the analysis of mothers' and fathers' employment and occupational outcomes to births occurring between 2000 and 2016.

[15] We estimate equation (2) using ordinary least squares (OLS) and cluster standard errors (ϵ_{itk}) at the province level. We then adjust the standard error for multiple hypothesis testing.

under a two-way fixed effects model assumes homogeneous treatment effects across units and over time. This assumption may be problematic if, for example, the reform had stronger effects in provinces where a larger share of students were previously enrolled in vocational tracks, or if the benefits of general education accumulated differently depending on when or how intensely the reform was implemented.

We present several pieces of evidence on the plausibility of the identifying assumptions. First, we implement the DiD event study estimator developed by De Chaisemartin and D'Haultfœuille (2024) (hereafter CH estimator), which is well suited to our setting where treatment intensity, measured as exposure to the LOGSE reform, varies continuously across provinces and increases gradually over time until full implementation. The CH estimator accommodates staggered continuous treatments and is robust to heterogeneous treatment effects, addressing a key concern in our context where the effects of LOGSE may evolve with the timing and intensity of exposure. By relaxing the assumption of homogeneous treatment effects across cohorts and provinces, this approach helps assess whether the baseline TWFE estimates remain valid. Moreover, it allows for formal tests of parallel trends and no anticipation by flexibly examining pre-reform dynamics. Taken together, these features make the CH estimator particularly appropriate for evaluating the causal impact of a reform like LOGSE, where both the rollout and the potential effects may vary meaningfully across provinces and over time.

Second, we further explore pretrends and anticipatory effects by estimating placebo regression using a lead version of the LOGSE exposure index to assess whether differences in prior school enrollment patterns or health at birth outcomes predict the subsequent rollout of the reform. Third, we test whether the timing of LOGSE implementation is correlated with province-level time-varying factors that could confound our estimates, including GDP per capita, female labor force participation and employment, population size, and neonatal and post-neonatal mortality. Lastly, we examine cross-province migration patterns to address concerns about selective mobility that could bias our estimates if women relocated in response to the reform or if movers differed systematically from stayers in characteristics linked to both educational attainment and infant health outcomes. These analyses, explored in detail in section 5, help assess the plausibility of the identifying assumptions and address potential limitations of the TWFE model in our setting.

5 Results and Discussion

We present our findings in four sections. The first section evaluates the effect of the reform on women's educational attainment, enrollment patterns, and degree completion. While the reform did not significantly affect total years of schooling or attainment levels, it increased the provision of general education by shifting students from the old vocational program into the new general education system. The second section presents our main results on the impact of the reform on infant health. We find that the reform significantly reduced the incidence of very preterm births (less than 32 weeks) and led to smaller, less precisely estimated reductions in very low birth weight (less than 1,500 grams) and preterm birth (less than 37 weeks). In the third section we assess whether these effects are indeed driven by increased general education or could instead reflect alternative explanations such as changes in peer composition or fertility behavior. The results support the interpretation that the main channel operates through general education content. Finally, we conduct a series of identification and robustness checks to probe the validity of our identifying assumptions and alleviate concerns regarding potential confounding factors.

5.1 Effects on Education

Effects on Enrollment

The reform led to a significant increase in the provision of general education for women. In Table 1, we study the impact of the reform on female school enrollment by looking at the educational choices of women aged 17 to 24, i.e., while they may still be enrolled in the education system.[16] We find that the reform increased the share of female students enrolled in lower secondary general education by 10.38 percentage points, a rise of 33.16% (column 2). Meanwhile, the reform reduced the share of women enrolled in upper secondary general education and vocational education by 13.90% and 25.51%, respectively (columns 3 and 4). We find no evidence of a significant impact of the reform on the share of women without any high school credentials (column 1). These findings suggest that the reform triggered a mechanical shift in enrollment away from the pre-reform system, where both vocational and upper secondary programs were accessible at age 14, toward the extended lower secondary general education program mandated under the post-reform system.

Table 1: Reform Effects on High School Enrollment

	(1) No Degree	(2) Lower Secondary General Education	(3) Upper Secondary General Education	(4) Vocational Education
Index	0.0014	0.1038***	-0.0609**	-0.0467***
	(0.004)	(0.020)	(0.026)	(0.015)
1975's Cohort Mean	0.011	0.313	0.438	0.183
Std. Dev.	0.10	0.46	0.50	0.39
Obs	109,339	109,339	109,339	109,339

Notes: Estimates are based on eq. (2) using a sample of female Spanish nationals aged 17–24, born between 1975 and 1985. Standard errors are shown in parentheses. Data come from the 1991–2018 waves of the Spanish Labor Force Survey (LFS). *** Significant at the 1% level, ** at the 5% level, * at the 10% level.

Effects on Degree Completion

The reform did not lead to an increase in women's overall educational attainment or total years of schooling. Table 2 shows the impact of the reform on female educational attainment by looking at LFS respondents' age at qualification and highest qualification completed by age 33.[17] Column 1 of Table 2 shows that the reform did not change the age at which the highest qualification was obtained, and thus had no effect on total years of schooling. Columns 2 to 5 show that the reform did not lead to a higher proportion of women completing their education with a particular qualification. Specifically, the reform neither increased the share of women earning a high school diploma or vocational credentials, nor did it affect the share completing a college degree.

Taken together, the results from Tables 1 and 2 suggest that the reform did not affect women's overall educational attainment or total years of schooling, but instead led to an increase in the share

[16] The Spanish LFS does not provide information on current enrollment. We infer enrollment choices by looking at the highest educational attainment before education is completed. See panel A of Table A4 for the exact definition of educational enrollment choices.

[17] Our sample here comprises women aged 25 to 33, allowing us to limit our analysis to women who completed their education. See Panel B of Table A4 in the Appendix for the exact definition of degree completion outcomes.

of women enrolled in educational programs with a mandatory common core of general academic subjects.

Table 2: Reform Effects on Degree Completion

	(1) Age at Highest Qualification	(2) No Degree	(3) High School Degree	(4) Vocational Degree	(5) College Degree
Index	0.2198	-0.0007	0.0045	0.0334	-0.0330
	(0.184)	(0.005)	(0.018)	(0.028)	(0.028)
1975's Cohort Mean	19.641	0.013	0.360	0.246	0.343
Std. Dev.	4.32	0.11	0.48	0.43	0.47
Obs	85,004	85,348	85,348	85,348	85,348

Notes: Estimates are based on eq. (2), using a sample of female Spanish nationals aged 25–33, born between 1975 and 1985. Standard errors are shown in parentheses. Data come from the 1991–2018 waves of the Spanish Labor Force Survey (LFS). *** Significant at the 1% level, ** at the 5% level, * at the 10% level.

5.2 Effects on Health at Birth

Baseline Estimates

Having established that the reform induced a shift in women's enrollment toward the extended post-reform general education system, while keeping overall educational attainment and schooling length constant, we can proceed to estimate the effects of more general education on infant health at birth. Table 3 presents our baseline DiD estimates from equation (2) for the effects of the LOGSE reform on health at birth outcomes: weight at birth, number of gestational weeks, and indicators for low birth weight (under 2,500 grams), very low birth weight (under 1,500 grams), premature birth (less than 37 weeks), and very premature birth (less than 32 weeks). To capture the effects of completed education, we restrict our analysis to mothers aged 25 to 33.[18]

Panel A of Table 3 focuses on the effects of the reform on birth weight and the incidence of low and very low birth weight. Column 1 shows no effects of the reform on birth weight. The corresponding 95% confidence intervals allow us to reject positive impacts of LOGSE on birth weight larger than 0.75%.[19] Column 2 shows negative, non-significant effects on low birth weight, and we can rule out reductions in the incidence of low birth weight larger than 10%. There is a marginally significant drop of 0.18 percentage points in the incidence of very low birth weight at the 10% level (column 3). Given the overall incidence of very low birth weight, this represents a decrease of 25.71%. However, this result does not hold after a multiple hypothesis testing correction (Romano-Wolf

[18] See McCrary and Royer (2011) on the different implications of completed versus ongoing education at the time of entering motherhood.

[19] The upper bound of the coefficient interval was calculated as the sum of the point estimate of the coefficient (10.5373) and 1.96 times the standard error (6.948). This result was then divided by the population average (3199.580). The calculation can be summarized as follows: (10.5373 + 1.96 * 6.948)/3199.580.

P-value=0.3526).[20] The estimates from Panel A thus suggest that the reform had limited effects on birth weight, with any potential improvements appearing to be concentrated at the lower tail of the birth weight distribution.

In panel B, we evaluate the impact of the reform on gestational length, defined as the number of gestational weeks, as well as the incidence of preterm and very preterm births. Column 1 shows no significant impacts of the reform on the number of gestational weeks. We can reject increases in the number of weeks larger than 0.21%. Column 2 reports a significant decrease in the incidence of premature births by approximately 0.58 percentage points, which corresponds to an 8.4% reduction relative to the baseline incidence. Column 3 also indicates a significant decrease of 0.26 percentage points in the incidence of very preterm births. Given the overall incidence of very preterm births, this represents a decrease of 37%. Of all outcomes examined, significance after multiple testing correction is preserved in the estimates for very preterm births. As shown in Figure A2 of the Appendix, this aligns with a significant decrease in the likelihood of births occurring between 30 and 36 gestational weeks and non-significant effects on the likelihood of full-term births. The more precise estimates on very preterm births and the results from Figure A2 in the Appendix suggest that the positive effects observed for premature births are primarily driven by reductions at the very preterm threshold.

Health at birth gains from increased general education align with evidence from similar education exit interventions during high school in developed countries that expanded general education provision alongside years of schooling (e.g., the 1960 Norwegian Reform; see Grytten et al. (2014)). Reforms that increase years of schooling without increasing general education often yield null effects, as shown by Lindeboom et al. (2009) for the 1959 UK Education Act and by McCrary and Royer (2011) for U.S. school entry policies. For prematurity, our estimate of –0.0029 (s.e. = 0.0015), re-scaled to reflect a one-year change in education, allows us to reject effect sizes larger than 0.0020, the point estimate obtained by McCrary and Royer (2011) for the impact of education on prematurity.[21] Thus, in light of existing evidence, our findings suggest that health-at-birth gains may be more closely linked to expansions in general education than to changes in schooling length alone.

[20] Figure A3 of the Appendix visually represents the impact of the reform across the birth weight distribution. The confidence intervals largely overlap the zero line, indicating the lack of significant reform effects on most of the birth weight spectrum. The intervals slightly dip below the zero line for weights under 2,500 grams, suggesting a small decrease at the lower end of the birth weight distribution. This graphical evidence aligns with the statistical findings in Panel A of Table 3, indicating that any positive influence of the reform on birth weight is limited, with a slightly more noticeable effect at the very low birth weight threshold.

[21] In Figure A6, we contrast our estimates with the findings from studies that have conducted high-quality evaluations of similar schooling exit interventions with varying programmatic elements and institutional factors. Column 1 reports the papers selected for our external validity exercise in chronological order. Differences in contexts are shown in Columns 2 and 3, which identify both the policy shock and the respective country and time frame. Differences in the programmatic elements of each reform are shown in Columns 4 and 5, which identify whether the reforms led to additional effective years of schooling, increased the provision of general versus vocational education, or both. Column 6 presents effect sizes for a one-year policy change, with most estimates being bound around zero.

Table 3: Reform Effects on Health at Birth

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
Index	10.5373	-0.0011	-0.0018*
	(6.948)	(0.003)	(0.001)
Romano-Wolf p-value	0.3377	0.7413	0.2857
1975's Cohort Mean	3199.580	0.070	0.007
Std.Dev	506.73	0.25	0.09
Obs	1,446,005	1,446,005	1,446,005
Panel B: Gestational Age			
	Weeks	Preterm	Very Preterm
Index	0.0217	-0.0058**	-0.0026***
	(0.032)	(0.003)	(0.001)
Romano-Wolf p-value	0.7413	0.2048	0.0899
1975's Cohort Mean	39.145	0.069	0.007
Std.Dev	1.90	0.25	0.08
Obs	1,296,160	1,296,160	1,296,160

Notes. The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1975 and 1985. Romano-Wolf p-values are based on 1,000 studentized bootstrap replications. Standard errors are in parentheses. Data are from the 2000–2018 Childbirth microdata of Vital Statistics (INE). *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Event Study Figures

To test the parallel trends and no anticipation assumptions and to examine the dynamics of treatment effects, we estimate the dynamic event study version of De Chaisemartin and D'Haultfœuille (2024). This approach involves estimating the following specification: [22]

$$Y_{itk} = \alpha + \beta_g \times \sum_{g=-4}^{5} D_{g(kt)} + \gamma_t + \theta_k + \varepsilon_{itk} \tag{3}$$

where Y_{itk} is the health at birth outcome and $D_{g(kt)}$ denotes the treatment status of i child born to a mother from cohort t and province k, g year away from the implementation of the LOGSE reform.[23] To obtain CH estimator, we discretize treatment into an ordered variable, $D_{g(kt)}$, which defines treatment groups based on the share of mothers in cohort t and province k. $D_{g(kt)}$ takes value of 0 if less than 25% of the cohort was exposed, 1 if exposure was between 25% and 50%, 2 if it was between 50% and 75%, and 3 if more than 75% of the cohort was fully educated under the LOGSE reform. We restrict the sample to mothers born between 1972 and 1983, ensuring four pre-intervention periods where all groups are untreated and limiting the observation period to the

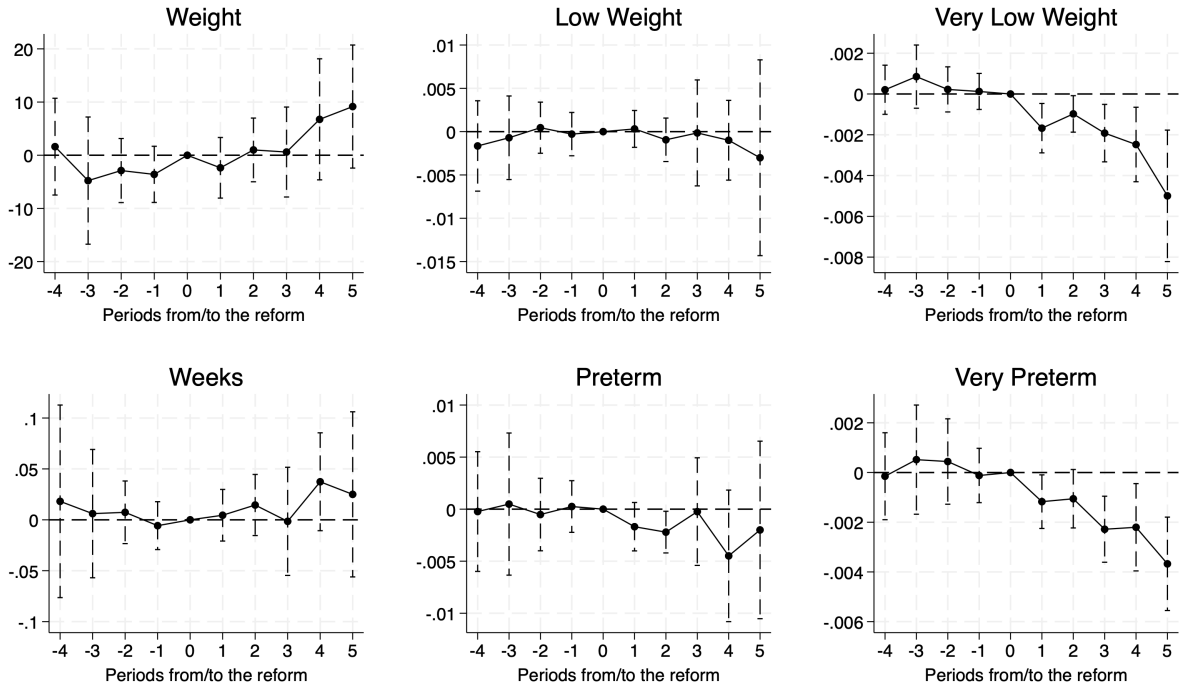
[22] We use the `did_multiplegt_dyn` Stata package, available at: https://github.com/chaisemartinPackages/did_multiplegt_dyn.

[23] All other variables are defined as in equation (2).

1983 cohort, where at least one group remains untreated.[24] This is satisfied by ensuring that some provinces had less than 25% of their students under the post-reform system before the 1984 cohort.

Figure 3 presents the dynamic estimates. The dynamic event-study figures show that the estimates align with the parallel trends assumption: the coefficients for cohorts prior to the implementation of the LOGSE reform are consistently near zero for all outcomes and do not exhibit discernible pre-trends. Figure 3 also clarifies the dynamics of the treatment effects: CH estimators show treatment effects that significantly decrease the likelihood of low birth weight and very preterm birth, consistent with our baseline estimates reported in Table 3. The increasing effects over time reflect increasing treatment intensity, as later cohorts contain a higher proportion of mothers fully exposed to LOGSE.

Figure 3: Dynamic Reform Effects on Health at Birth



Notes. This figure presents dynamic event-time dummy coefficients from estimating eq. (3) on a sample of mothers aged 25–33, born between 1972 and 1983. We exclude observations from the Basque Country, as cohorts born before 1975 were already exposed to the LOGSE reform pilot, which would contaminate the placebo periods. As detailed in Appendix Table A7, the main health at birth findings (Table 3) remain robust when estimating eq. (2), excluding Basque Country observations. The bars represent 95% confidence intervals. Standard errors are clustered at the province level. Data are from the 2000-2018 Childbirth microdata of Vital Statistics (INE).

5.3 Assessing Increased General Education Vis-a-Vis Competing Explanations

In this section, we examine whether the positive effects of the reform on infant health at birth are explained by increased general education, rather than by other aspects of the policy, such as

[24] The CH estimator requires that least one group remains untreated throughout. They referred to this assumption as the "non-pathological design" assumption.

peer effects or changes in fertility decisions. The analyses presented strengthen the case for general education as the key driver and help rule out these alternative explanations.

5.3.1 Increased General Education

First, we show that the estimated positive impacts of the reform on infant health at birth are driven by students who, prior to the reform, would have followed vocational training, that is, those whose educational content changed the most. If similar effects were found among students who would have followed vocational training and students who would have remained in general education regardless of the reform, it would suggest that other features of the policy, rather than increased general education, could explain the results.

Identifying the group of students who would have followed the pre-reform vocational and general education programs in the absence of the policy reform is not feasible, as it would require observing the path not taken. Instead, we perform two additional exercises to tease out whether the positive effects come from crowding out vocational studies. First, we regress our outcomes on two indices capturing the proportion of 14-year-olds enrolled in either the pre-reform vocational or the pre-reform general education programs. Second, following Bertrand et al. (2018) and Bellés-Obrero and Duchini (2021), we measure the proportion of students in either the pre- or post-reform general education system and implement an IV strategy, exploiting the fact that provinces with more such students require fewer adjustments to implement the reform compared to those with more vocational training students.

To perform the first exercise, we estimate our main equation (2) using enrollment data on the share of students at 14-years old under the pre-reform (LGE) vocational program and the share of students under the pre-reform (LGE) general education program. We would expect stronger results for the index measuring changes in vocational enrollment if the positive impacts of the reform were mainly due to students who would have followed the vocational training program and stronger results for the index measuring changes in general education enrollment, otherwise. We construct the Pre-reform Voc Index$_{tk}$ as the share of 14-year-olds enrolled in the pre-reform vocational program:

$$\text{Pre-reform Voc Index}_{tk} = \frac{LGEVocationalEducation_{tk}}{LOGSE_{tk} + LGETotal_{tk}}$$

Similarly, the Pre-reform GenEd Index$_{tk}$ measures the share of 14-year-olds enrolled in the pre-reform general education program: [25]

$$\text{Pre-reform GenEd Index}_{tk} = \frac{LGEGeneralEducation_{tk}}{LOGSE_{tk} + LGETotal_{tk}}$$

[25] Figure A4 in the Appendix illustrates the pre-reform Vocational and pre-reform General indices alongside the main LOGSE exposure index, plotted by birth cohort during the 1990s transition period. Unlike the primary LOGSE exposure index, which increases and ranges between 0 and 1, the Pre-reform Voc Index$_{tk}$ and Pre-reform GenEd Index$_{tk}$ indices decrease over time and do not vary between 0 and 1. At the beginning of the transition period, these indices capture the relative share of students in the old vocational or general education programs at the provincial level. By the end of the transition, both indices are zero by construction. Consequently, we expect the coefficients for these indices to exhibit opposite signs compared to the LOGSE exposure index in eq. (2). Specifically, the Pre-reform Voc Index$_{tk}$ reflects an average cross-province decline of 34 percentage points (from approximately 34% to 0%) in the share of students enrolled in the pre-reform vocational training, while the Pre-reform GenEd Index$_{tk}$ represents a corresponding decline of 66 percentage points (from around 66% to 0%).

Table A8 in the Appendix reports results from this exercise. Estimates for gestational age (Panel B) show that the reform's main impacts are primarily driven by the substitution of general education for vocational training.[26] Appendix Table A9 further shows that the effects on birth outcomes are similar across maternal socio-economic status groups. This indicates that, even though average effects are driven by students who would have otherwise followed the vocational program, children benefit equally from the reform regardless of their mother's socio-economic background.

To perform the second exercise, we add the share of students under the pre-reform general program to the share of students in the post-reform general education program under LOGSE, that is adding up our Pre-reform GenEd Index$_{tk}$ to our main LOGSE implementation index, to observe the impact of crowding out students from the vocational training. We would expect estimates of the same sign as our main estimates (Table 3) if our results are driven by vocational students as long as including general education students from the old system together with LOGSE students should not alter the results. We construct this alternative implementation index as follows: [27]

$$\text{General Index}_{tk} = \frac{LOGSE_{tk} + LGE \text{ General Education Program}_{tk}}{LOGSE_{tk} + LGETotal_{tk}}$$

Following Bellés-Obrero and Duchini (2021), we instrument the staggered rollout of the share of students enrolled in either the pre-reform or post-reform general education programs, captured by General Index$_{tk}$, with the 1975 cohort pre-reform cross-province variation in this share, interacted with cohort fixed effects. [28]

[26] Specifically, the coefficient in column 8 of Panel B shows that switching from vocational training to general education reduces the likelihood of very premature births by 0.78 percentage points, or approximately 37.88% (0.34 * 0.0078). This result closely mirrors our baseline very preterm birth estimate (column 7, Panel B), suggesting that the reform's effects are primarily driven by vocational students.

[27] Notice that, similarly to our measure of intensity of exposure to LOGSE, this index is increasing but it does not vary from 0 to 1. At the beginning of the transition period, this is about 0.5 percentage units as 50% of the students were in the old general education program. At the end of the transition period, the index is 1 as 100% of the students are under the new comprehensive system receiving general education. As a consequence, we expect the coefficients for this index to have the same sign as our LOGSE exposure index in eq. (2).

[28] This approach corresponds to estimating the following 2SLS model:

$$Y_{itk} = \alpha + +\beta GeneralIndex_{tk} + X'_{tk}\lambda + \eta_t + \theta_k + \epsilon_{itk},$$

where i is a mother belonging to cohort t and province k, whose children's health outcome is observed. The dependent variable Y_{itk} represents the health at birth outcome.

The vector X_{tk} includes time-varying controls specific to cohort t and province k such as GDP per capita, female employment rate, female labor force participation rate, log population, and infant, neonatal, and post-neonatal mortality rates. Cohort fixed effects (η_t) account for socio-economic conditions specific to each cohort, while province fixed effects (θ_k) capture unobserved province-level factors. Standard errors are clustered at province level (ϵ_{itk}). The corresponding first-stage regression is:

$$\text{General Index}_{tk} = \alpha + \sum_{t=1977}^{1985} \beta_t \left(GeneralIndex_{tk}^{1975} \cdot \eta_t \right) + X'_{tk}\lambda + \eta_t + \theta_k + \epsilon_{itk},$$

with $t = 1977, \ldots, 1985$.

Results from this exercise are presented in Table A11 in the Appendix.[29] Consistent with findings in Table A8 of the Appendix, our IV estimates confirm that the positive impact of the policy reform is driven by students who would have taken the vocational track and that our results are robust to the inclusion of students who would not have taken that path.

5.3.2 Fertility Decisions

In this section, we show that the estimated positive impacts of the reform on infant health at birth are not driven by affected mothers having fewer, higher-quality children later in life. According to Becker (1965)'s quantity–quality tradeoff, as the value of a woman's time increases with education, and since children are a time-intensive good, women may choose to have fewer children and invest more in each. Furthermore, increased education can delay motherhood, as women pursue further studies or career opportunities before having children (Goldin, 2023).[30] Since we observe only the sample of women who become mothers, the positive impacts could merely reflect sample selection, where better birth outcomes result from changes in who becomes a mother and when, rather than from improved infant health due to increased general education.[31]

To test the impact of LOGSE on fertility patterns including the probability of motherhood and the timing of motherhood, we create a panel of annual birth rates by mother's age at the province level using administrative birth records and estimate an aggregate version of equation (2). The findings, presented in column 1 of Table A12 in the Appendix, reveal no impact of LOGSE on birth rates. Further analysis, presented in column 2 of Table A12 in the Appendix, shows no effects of the reform on motherhood entry age. This suggests that mothers, regardless of their level of exposure to the reform, tend to have their first born at similar ages. These results are visually supported by Figure A12 in the Appendix, which illustrates the reform's influence, or lack thereof, on birth rates across mothers' age groups. Figure A5 shows that both the coefficients and their confidence intervals consistently hover around zero for all ages, indicating no significant influence of the reform on fertility patterns across the spectrum of mother's ages. Thus, women with different levels of exposure to LOGSE were equally likely to become mothers, and we can conclude that the reform did not lead to a selected sample of women having fewer, higher-quality children later in life.

5.3.3 Peer Effects

A remaining concern is that the policy reform may have affected maternal outcomes, and thus infant health at birth, through changes in peer composition. Schooling exit interventions like LOGSE, which delay the timing of choosing the educational pathway, inherently trigger changes in peer composition by keeping mixed-ability students together for a longer period. The reform could have

[29] First-stage results presented in Table A10 in the Appendix, report coefficients of negative sign and increasing magnitude for the interaction terms between cohort fixed effects and the pre-reform share of students enrolled in the pre- and post-reform general education programs. The negative sign reflects that provinces with a higher initial share of students in these programs required smaller adjustments to comply with the reform. The increasing magnitude indicates that provinces progressively diverged from their pre-reform shares as they moved closer to the full implementation of the reform, where all students where under the new system.

[30] Additional years of schooling may reduce teen pregnancies through the so-called "incarceration effect" defined as a delay in fertility equal to the amount of additional time spent in school, since this may reduce the time available for engaging in risky behavior, and hence improve health at birth by preventing pregnancies at a young age (see Black et al., 2008; Cygan-Rehm and Maeder, 2013; Geruso and Royer, 2018). However, the lack of impact of the reform on women's years of schooling suggests a limited causal role for the reform through this "incarceration effect".

[31] We do not study infant mortality patterns given that just 0.07% of infants die during delivery. Birth weight and gestational age outcomes include all newborns, including those dying during delivery. Results for infant mortality outcomes are available upon request.

triggered positive impacts to low-ability students who would benefit from interactions from high ability peers.[32] Several characteristics of the Spanish high school system make substantial peer composition effects less likely. First, both the pre- and post-reform systems did not track students by ability, minimizing self-selection. Second, vocational studies were generally offered within the premises of regular high schools in both systems, ensuring that students remained in mixed settings. Third, high enrollment rates between the ages of 14 and 16 meant that there was limited scope for introducing a substantial number of low-motivated out-of-school students back into the system. In this section, we provide additional tests showing that peer effects are not driving our results.

To examine whether potential peer composition changes induced by the reform could partially explain the reported improvements in health at birth outcomes, we conduct two additional exercises. First, we use the municipality size of the mother's place of residence as a proxy for exposure to peer composition changes and explore how these effects vary between mothers from rural (less than 50,000 inhabitants) and urban (more than 50,000 inhabitants) areas. We hypothesize that students in less populated areas will be less affected by changes in peer composition than those in more populated areas. Exposure to changes in the school environment is expected to be limited due to tight-knit social structures in less populated areas, such as small towns and rural areas. In particular, peers in the school setting are less likely to influence students with extended social connections outside of school, such as family or community ties, due to pre-existing social networks that are less dependent on the school environment (Carrell et al., 2009). On the contrary, students from urban areas might be more affected by the introduction of new students into their classrooms. If no differences are observed between these two groups, this would suggest that the role of peer composition changes is limited in our analysis. Second, we use mothers' occupation as a proxy for their educational achievement and explore whether children born to higher and lower-achieving mothers benefit differently from the reform, as the reform has no effects on educational attainment, degree completion rates, or type of job (see section 6 below). We define high-achieving mothers as those with jobs in managerial or professional occupations. If we observe no differential effects of the policy change on children born to mothers with different professional achievements, this would suggest that changes in peer composition did not affect the impacts on birth weight and gestational age. To perform these two exercises, we include an interaction term in equation (2) between the reform exposure index and indicators for municipality size or type of maternal occupation.[33]

Panel A in Table A13 in the appendix shows that the estimated coefficients corresponding to the interaction between the LOGSE exposure index and a mother's occupation are statistically equal to zero, which suggests that the reform had no differential effect on the health of infants born to mothers of different abilities. Panel B provides further support for the lack of peer effects by showing

[32] The literature on peer effects shows that peers influence outcomes from academic performance to health behaviors (see Sacerdote, 2011). Evidence from similar reforms increasing peer group heterogeneity points to negative - not positive - effects, including worse mental health, higher risk behaviors, and increased mortality, often due to poorer school environments, adverse peer exposure, and greater competition (Basu et al., 2018; Böckerman et al., 2021; Fischer et al., 2021; Galama et al., 2018).

[33] We estimate the following model:

$$y_{itk} = \alpha + \beta_1 D_i * I_{tk} + \beta_2 D_i + \beta_3 I_{tk} + \gamma_t + \theta_k + \epsilon_{itk}$$

Where D_i is a dummy variable that takes on a value of 1 if the mother i either (a) has a job in a managerial or professional occupation or (b) lives in an area with less than 50,000 inhabitants. All other variables are defined as in equation (2). The interaction $D_i * I_{tk}$ identifies the impact of the reform on the gap in health at birth outcomes between infants born to mothers (a) with high- and low-skilled jobs or (b) living in rural and urban areas. The coefficient of interest, β_1, measures how differences in the health at birth of infants born to mothers exposed to different changes in peer composition are affected by the reform.

no significant differential impacts due to the municipality size of the mother's place of residence. All in all, the results from Table A13 suggest that changes in peer composition due to modifications to educational pathways are unlikely to be responsible for the estimated impacts of the reform on infant health at birth.

5.4 Additional Identification Checks

We provide a battery of exercises that probe the robustness of our estimates. First, we perform placebo tests to assess whether prior school enrollment patterns and health at birth outcomes are spuriously correlated with the later rollout of the LOGSE reform. Using labor force and birth register data for five cohorts born before the reform (1970–1975), we test whether pre-reform education and health outcomes were systematically associated with future exposure. Tables A14 and A15 in the Appendix report the placebo estimates, showing that the coefficients on the lead index are not statistically different from zero, providing support for both the parallel trends and no anticipation assumptions.

Second, we address remaining concerns about the potential endogeneity of the LOGSE reform rollout across provinces by testing whether the timing of implementation correlates with other province-level, time-varying determinants of infant health. Table A16 in the Appendix reports estimates from regressions using data collapsed to the province-year level for the 1990–2000 period. We find no evidence that reform exposure is associated with changes in GDP per capita, female labor force participation, female employment, population size, or infant, neonatal, and post-neonatal mortality. Additionally, Table A17 in the Appendix shows that our main results remain robust to the inclusion of these province-level controls.

Third, one may worry about migration patterns, as mothers may have moved across provinces in response to differences in the timing of the reform's implementation. If migration were systematically related to the rollout of the LOGSE reform, our estimates could reflect changes in population composition rather than the true effect of the reform. Table A18 in the Appendix shows that migration patterns across Spanish provinces do not exhibit spurious correlations with the staggered implementation of the reform, alleviating concerns that inter-provincial migration confounds our results.

6 Mechanisms

We have shown that the reform's main effect was to induce a switch from the pre-reform vocational education program to the post-reform general education program, while keeping both educational attainment and years of schooling constant (Tables 1 and 2). The boost to women's general education did significantly improve infant health at birth (Table 3). In this section, we examine how extending mothers' general education maps onto improvements in infant health through better information-processing skills, which may influence both health behaviors and earning potential.

We begin by examining whether the reform improved women's information-processing skills in adulthood. Panel A of Table 4 shows that treated women scored 0.911 and 1.822 standard deviations higher in the PIAAC literacy and numeracy assessments, respectively. These effects remain robust

to multiple hypothesis testing, confirming that the reform substantially improved women's ability to process information.[34]

Effects on Risky Health Behaviors and Family Planning

By improving information-processing skills, general education may help mothers adopt healthier behaviors during pregnancy and make informed reproductive decisions, thereby improving infant health (Currie and Moretti, 2003; Rosenzweig and Schultz, 1989). To test this channel, we analyze alcohol consumption during pregnancy, substance use, and family planning.

Panel B of Table 4 explores whether these improved information skills translated into healthier maternal behaviors. We find that the reform reduced the likelihood of drinking alcohol during pregnancy by 8.7 percentage points (column 1), a 29% decline relative to the pre-reform mean. While the estimated effects on lifetime alcohol, tobacco, and drug use are less precisely estimated, they consistently point to a broader pattern of reduced risky behavior. These findings are in line with evidence from reforms that expand college education, which have been shown to reduce smoking through changes in behavior or preferences (Currie and Moretti, 2003; Carneiro et al., 2013). In contrast, school-leaving age reforms that only extend time in school often show mixed or null effects on health behaviors (McCrary and Royer, 2011). The effects on alcohol use during pregnancy and smoking suggest a behavioral response to general education during high school, providing a plausible channel through which general education acquired in high school may improve infant health at birth.

Panel C of Table 4 examines whether the reform influenced family formation and its timing. Exposure to LOGSE increased the likelihood of being married before the birth of the first child by 2.35 percentage points, a 3.05% increase (column 1). While the reform had no statistically significant effect on the duration of marriage before birth or the likelihood of the father being absent from the birth certificate, the direction of the estimates suggests a shift toward more stable and intentional family planning. Similar to evidence from college-access expansions showing increased marriage rates (Currie and Moretti, 2003; Carneiro et al., 2013), our findings suggest that general education may potentially influence the timing and structure of family formation by improving information processing skills that enable to changes in maternal behavior.

Effects on Women's Earning Potential

General education may also raise women's earning potential by enhancing skill portability and adaptability in dynamic labor markets, either directly or through positive assortative mating (Goldin, 2001; Behrman and Rosenzweig, 2004; Hanushek et al., 2017). To test this channel, we analyze maternal labor market outcomes and partner characteristics at the time of first birth, focusing on both the extensive and intensive margins of labor supply as well as assortative mating patterns.

Panel A of Table 5 presents the estimated effects of the LOGSE reform on maternal labor market outcomes. We find that exposure to the reform significantly increased the likelihood of mothers being engaged in paid work outside the home by 3.08 percentage points, corresponding to a 3.6% increase relative to the pre-reform mean (column 1). In contrast, we detect no statistically or economically significant changes in the type of occupation held by mothers (columns 2 and 3), suggesting that the reform primarily affected the extensive margin of labor supply rather than occupational upgrading.

[34] Because the PIAAC dataset only includes region of residence, we measure exposure to the reform at the regional level. Despite this coarser variation, the results are consistent with our main findings on infant health. Appendix Table A19 further shows that regional exposure to LOGSE is associated with significant reductions in premature and very premature births.

Table 4: Reform Effects on Information Processing Skills, Health Behavior, and Family Planning

	(1)	(2)	(3)	(4)
Panel A. Information Processing Skills				
	Literacy Proficiency	Numeracy Proficiency		
Index	0.911***	1.822***		
	(0.284)	(0.387)		
Romano-Wolf p-value	0.015	0.003		
1979's Cohort Mean	0.142	0.078		
Std.Dev.	0.785	0.777		
Obs	410	410		
Panel B. Risky Health Behavior				
	Mother Drinks During Pregnancy	Mother Drinks	Mother Smokes	Mother Consumes Drugs
Index	-0.087**	-0.425*	-0.387**	-0.061
	(0.039)	(0.216)	(0.180)	(0.236)
Romano-Wolf p-value	0.036	0.090	0.160	0.801
1975's Cohort Mean	0.030	0.847	0.706	0.280
Std.Dev.	0.170	0.360	0.457	0.450
Obs	2,314	1,003	1,003	1,003
Panel C. Family Planning				
	Marriage Before Birth	Duration of Marriage Before Birth	Father Absent	
Index	0.0235**	0.149	-0.0015	
	(0.011)	(0.778)	(0.002)	
Romano-Wolf p-value	0.1029	0.7802	0.6723	
1975's Cohort Mean	0.848	36.17	0.011	
Std.Dev.	0.360	24.55	0.103	
Obs	1,521,770	980,853	1,521,770	

Notes. The estimates were obtained by applying eq. (2) to different samples, based on data availability: women aged 25–33 born between 1979 and 1985 (Panel A); mothers born between 1975 and 1985 (column 1, Panel B); women aged 25–33 cohabiting with a child and born between 1975 and 1983 (columns 2–4, Panel B); and first deliveries by mothers aged 25–33 and born between 1975 and 1985 (Panel C). Columns (1) and (2) of Panel A include a constant and controls for birth year and region of residence. The remaining specifications control for birth year and province of residence. Romano-Wolf p-values are based on 1,000 studentized bootstrap replications. Standard errors are shown in parentheses. Data sources: 2011 PIAAC Survey (Panel A), 2017 and 2007 Spanish Alcohol, Tobacco, and Illegal Drugs Surveys (Panel B), and 2000–2018 Vital Statistics (INE) (Panel C). *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Panel B of Table 5 examines whether the reform influenced assortative mating patterns. If general education increases women's labor market prospects, it may also raise the likelihood of partnering with higher-earning spouses, thereby amplifying household income through a multiplier effect (Behrman and Rosenzweig, 2004). We find no evidence that the reform changed partner quality, measured by the occupational status of mothers' partners (columns 1 and 2). These null effects are consistent with prior findings showing that educational reforms influence mate selection only when peer composition is substantially altered (McCrary and Royer, 2011), which was not the case in our setting, as shown in section 5.3.3. Overall, we fail to find strong evidence that assortative mating served as a key channel through which general education influenced infant health.

These findings support the hypothesis that general education improves women's ability to process information and make informed choices about health and family planning, while also facilitating entry into the labor market. The evidence points to improvements in information-processing skills as the primary mechanism linking general education to better infant health.

Table 5: Reform Effects on Women's Earning Potential

	(1) Working Mother	(2) Qualified Job	(3) Non-Qualified Job
Panel A. Labor Market Outcomes			
Index	0.0308**	-0.0220	-0.0026
	(0.013)	(0.105)	(0.845)
Romano-Wolf p-value	0.019	0.750	0.750
1975's Cohort Mean	0.852	0.490	0.317
Std.Dev.	0.355	0.493	0.465
Obs	1,467,386	1,187,645	1,187,645
Panel B. Assortative Mating			
	Mate Qualified Job	Mate Non-Qualified Job	
Index	-0.0083	0.0036	
	(0.008)	(0.005)	
Romano-Wolf p-value	0.750	0.750	
1975's Cohort Mean	0.609	0.289	
Std.Dev.	0.237	0.289	
Obs	1,009,937	1,009,937	

Notes: The estimates are obtained from estimating eq. (2) on a sample of first deliveries of mothers aged 25–33 and born between 1975 and 1985. Standard errors are shown in parentheses. Romano-Wolf p-values are based on 1,000 studentized bootstrap replications. Data are from the 2000–2016 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

7 Conclusions

This paper provides novel causal evidence on the intergenerational returns to general education. We take advantage of the staggered rollout across Spanish provinces of a major educational reform that extended the provision of general education in lower secondary school, while holding overall educational attainment constant. To identify the causal impact of increased maternal general education on infant health, we link newly digitized province-level enrollment records with individual-level birth certificate data, and focus on first births to mothers who were differentially exposed to the reform across cohorts and provinces. Our DiD estimates point to sizable improvements in the most severe health at birth outcomes: exposure to the reform reduced the incidence of very preterm births by 37%. We also find reductions in premature births and very low birth weight births, though these estimates are less precisely estimated, consistent with the largest effects being concentrated at the extreme left tail of the gestational age distribution.

Our findings are consistent with mechanisms theoretically predicted by prior economic literature, linking increased general education to higher earning potential and improved information processing skills. We use linked administrative and survey data, including information on mothers' occupations, marital status, fertility timing, adult literacy and numeracy assessments, and self-reported substance use, to explore potential channels through which the reform affected infant health. The evidence suggests that the observed improvements in infant health are primarily driven by improved maternal information processing skills, which reduced risky behaviors during pregnancy, potentially led to better family planning and higher maternal labor force participation.

The health at birth gains induced by the reform translate into substantial economic returns, with our results suggesting that the medical cost savings associated with the LOGSE reform considerably exceed its implementation costs, making it highly cost-effective. Estimates from neonatal intensive care units (UCIN) indicate that the average medical cost per very preterm infant is approximately €90,000, due primarily to prolonged stays in intensive care and related interventions (Ministry of Health, 2022). We estimate that the reform reduced the incidence of very preterm births by approximately 0.26 percentage points (Panel B, Column 3, Table 3). This implies an estimated medical cost saving of €234 per child (0.0026 × €90,000). The total implementation cost of the reform is estimated at €7.8 million (Marchesi, 2020). Given its staggered rollout, the reform affected approximately 1.06 million girls born between 1975 and 1985 (out of a total of 3.14 million female births). Of those exposed, about 60% became mothers and are included in our sample, implying an average cost of €12.21 per child (€7.8 million ÷ [0.6 × 1,064,507]). This means that the health-related savings alone are almost 20 times greater than the per-child implementation cost. This simple cost-benefit calculation likely understates the full impact of the reform, as early-life health improvements are known to yield long-term gains in human capital and economic well-being.[35] Our findings highlight that education policy can shape the early determinants of health, positioning general schooling as a driver of skill formation as well as a powerful tool for reducing inequality and promoting upward mobility across generations.

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Appendix

Figures

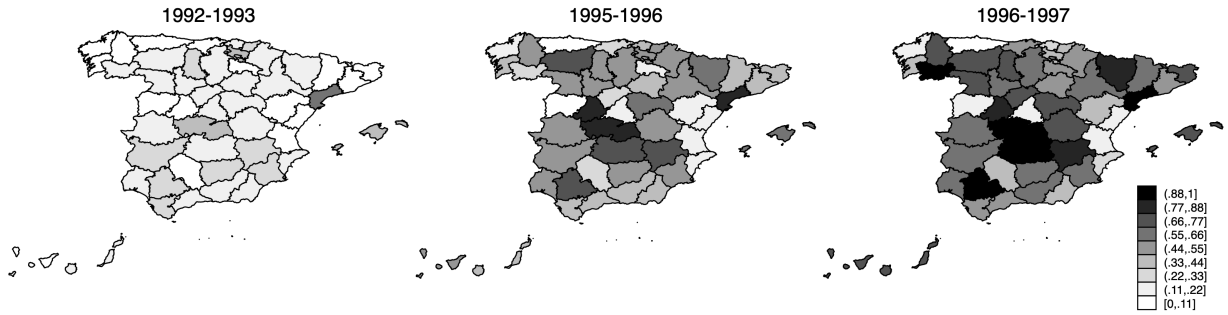


Figure A1: Share of Students Age 14 Enrolled in the Post-Reform System During the Transition Period

Notes: This figure depicts the LOGSE exposure index at different points during the transition period. Lighter colors correspond to lower levels of LOGSE implementation (captured by I_{tk}) for cohort t in province k. The three panels reflect an increasing share of students exposed to LOGSE, illustrating the progressive rollout of the reform over time. At the beginning of the transition period for the 1979 birth cohort (1993–1994 school year), less than 10% of the 14-year-old student population was under the new system in almost all provinces. By the end of the transition for the 1983 birth cohort (1997–1998 school year), over 60% of 14-year-old students were enrolled under LOGSE. Source: 1989/1990– 1999/2000 Enrollment Records (Education Ministry).

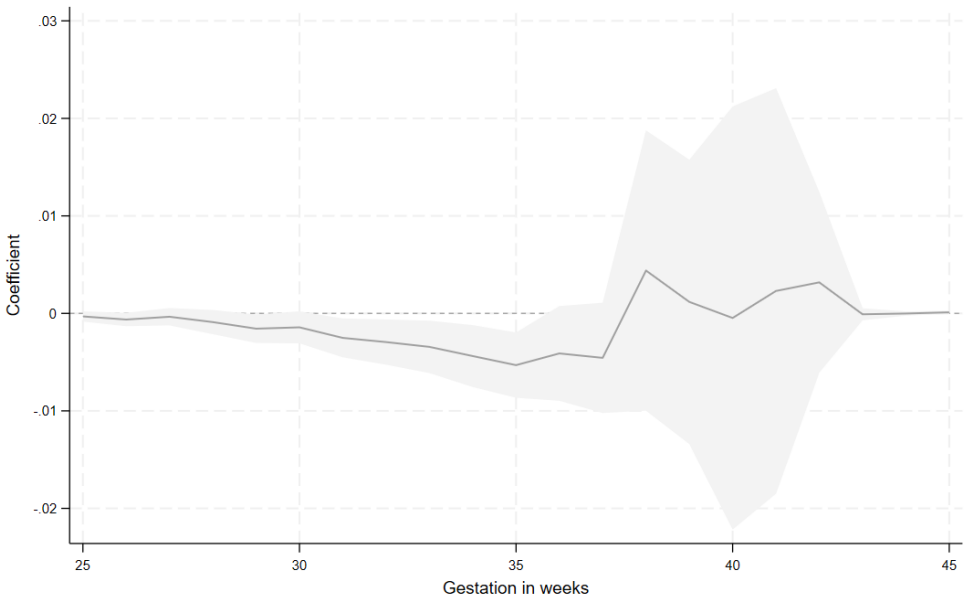


Figure A2: Reform Effects along the Distribution of Gestational Weeks

Notes. The figure presents the DiD estimates for a sample of mothers aged 25-33, born between 1975 and 1985. These estimates are derived from different regressions that calculate the likelihood of having a birth with gestation weeks of at most the amount indicated on the x-axis. All coefficients were computed estimating eq. (2). Source: Data were sourced from the Childbirth Microdata of Vital Statistics (INE) for the years 2000-2018.

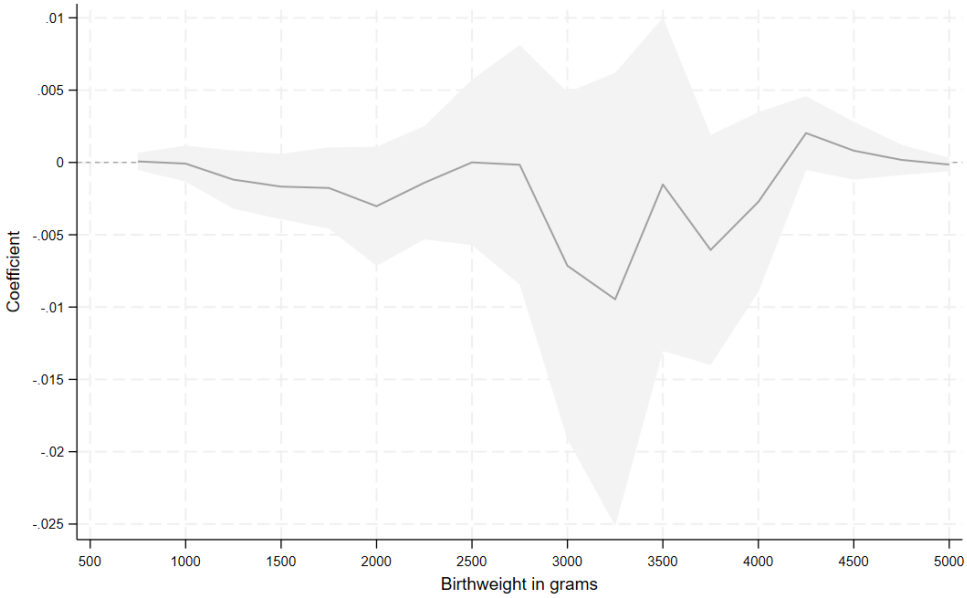


Figure A3: Reform Effects along the Distribution of Weight at Birth

Notes. The figure presents the DiD estimates for a sample of mothers aged 25-33, born between 1975 and 1985. These estimates are derived from different regressions that calculate the likelihood to have birth weight of at most the amount on the x axis. All coefficients were computed estimating eq. (2). Source: Data were sourced from the Childbirth Microdata of Vital Statistics (INE) for the years 2000-2018.

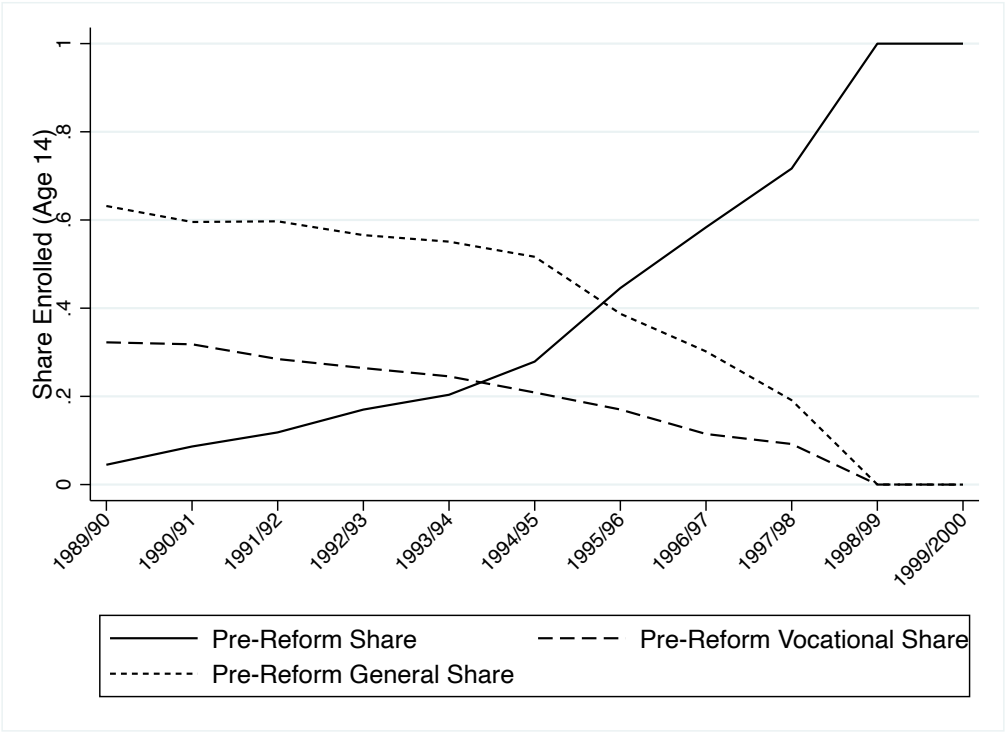


Figure A4: Post-reform (LOGSE), Pre-reform (LGE) Vocational and Pre-reform (LGE) General Education Enrollment Shares by School Year

Notes. This figure shows the share of 14-year-olds by birth cohort under the post-reform general education program (LOGSE share, solid line), pre-reform vocational programs (Pre-reform Vocational share, dashed line), and pre-reform general programs (pre-reform General share, short dashed line). Source: 1989/1990– 1999/2000 Enrollment Records. (Education Ministry).

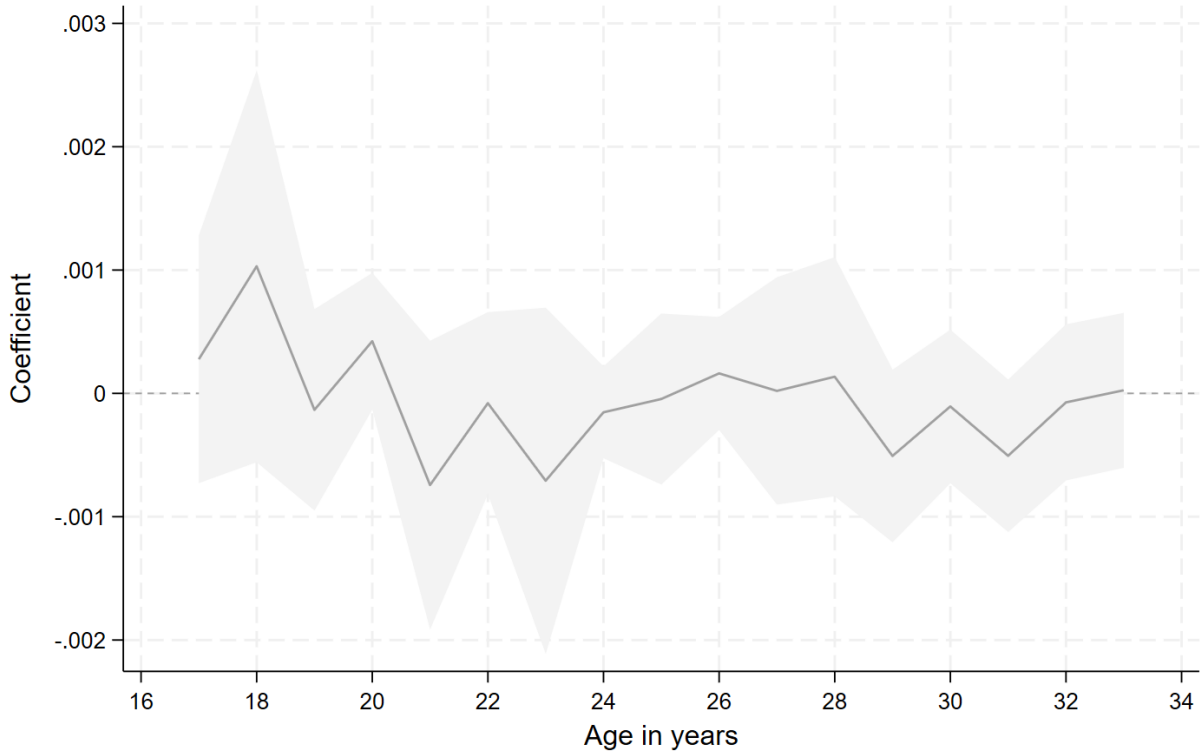


Figure A5: Reform Effects on Birth Rate by Mothers' Age

Notes: The figure presents the differences-in-differences estimates for a sample of mothers aged 25-33, born between 1975 and 1985. These estimates are derived from different regressions that calculate the impact of the reform on birth rates by mothers' age as indicated on the x-axis. Birth rate is calculated as the number of births divided by the number of women born in Spain per mother age and province. All coefficients were computed using a uniform kernel function, a first-order polynomial, MSE-optimal bandwidths, and fixed effects for both province and cohort. Standard errors are clustered at the province level. Source: Data were sourced from the Childbirth Microdata of Vital Statistics (INE) for the years 1975-1985 and 2000-2018.

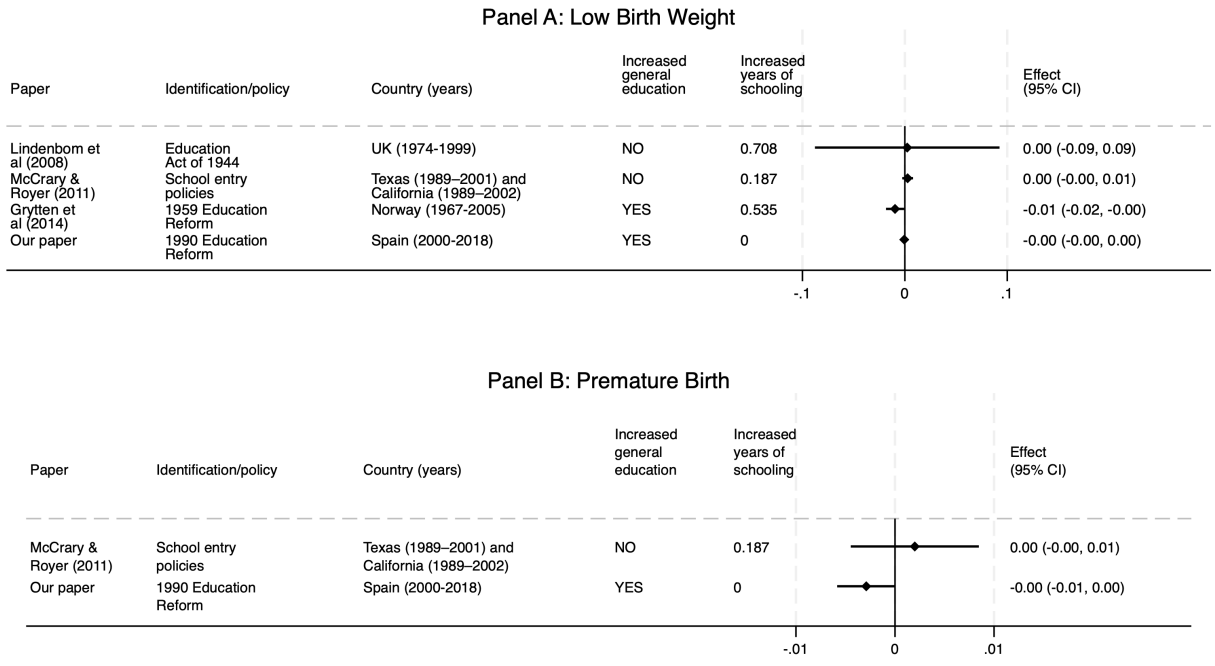


Figure A6: Comparison to Other Secondary Schooling Reforms' Impact on Health at Birth Outcomes

Notes:

Lindeboom et al. (2009): They use the 1944 Education Act in the UK that raised the minimum school leaving age by one year from 14 to 15 years of age in a fuzzy regression discontinuity strategy. They report both first and second stage estimates. To obtain comparable figures we need to compute the reduced form estimates that correspond to a 1-year reform. We obtain those by multiplying first stage estimates in Column 4 of Table 3 and second stage estimates in Column 2 of Table 5b. We use the all-years sample. (beta: 0.0023=0.006*0.708; se: 0.046=sqrt$(0.006^2 * 0.008^2 + 0.708^2 * 0.066^2)$).

McCrary and Royer (2011): They use US school entry policies that manipulate schooling by one year (p.179). They use data from California and Texas. We agregate estimates from both samples by computing averages weighted by the sample sizes. Estimates for educational attainment from Columns 3 and 6 in Table 3. Estimates for low birth weight form Columns 1 and 4 in Table 3. Estimates for prematurity from Columns 2 and 5 in Table 3.

Grytten et al. (2014): They use a 1959 education reform in Norway that increased the minimum years of schooling from 7 to 9 and introduced changes in curriculum with the aim of improving schooling quality (Black et al 2005). They use an instrumental variables strategy and report both first and second stage estimates. To obtain comparable figures we compute the reduced form estimates that correspond to a 1-year reform. We obtain those by multiplying first and second stage estimates in Column 3 of Table 3 (p. 88) (beta: 0.0096=0.018*0.535; standard error: 0.0022=sqrt$(0.018^2 * 0.0099^2 + 0.535^2 * 0.0085^2)$.

Our paper (2025): We use the 1990 LOGSE reform in Spain that increased the amount of general education for two years. Here we use estimates for low birth weight from Panel A in Column 2 in Table 3 and for pre-term birth from Panel B in Column 2 of Table 3. To obtain comparable figures, estimates estimates are re-scaled for one year.

Tables

Table A1: Data Overview

Register Name	Unit	Data Description	Outcomes
Panel A: Main Datasets			
Enrollment Records (1989/1990–1999/2000)	Province by school year	Administrative records on all 14-year-old students. Sample: students born 1975–1985. Link: province and school year (14-year-old cohort).	Total share of enrolled 14-year-old students; share under LOGSE (post-reform); share under LGE (pre-reform); share in general education; share in vocational education.
Vital Statistics (2000–2018)	Child	Administrative records on all births in Spain. Sample: first-borns; mothers born 1975–1985, aged 25–33. Link: birth cohort and province of residence.	Gestational age; birth weight; parents' demographics and occupation.
Panel B: Other Datasets			
Spanish Labor Force Survey (1991–2018)	Mother	Representative survey of the Spanish population. Sample: women born 1975–1985, aged 17–24 and 25–33. Link: birth cohort and province of residence.	Enrollment outcomes; degree completion outcomes.
Program for the International Assessment of Adult Competencies (2011)	Mother	Representative survey of the Spanish population. Sample: women born 1979–1985, aged 25–33. Link: birth cohort and region of residence.	Numeracy and literacy assessments.
Alcohol, Tobacco, and Illegal Drugs Survey (2007, 2017)	Mother	Representative survey of the Spanish population. Sample: 2007 (mothers born 1975–1983); 2017 (mothers born 1975–1985), aged 25–33. Link: birth cohort and province of residence.	2007: Ever drinks, smokes, or uses drugs; 2017: Drinks during pregnancy.

Notes: This table summarizes the administrative and survey data sources used to construct the key variables in our analysis.

Table A3: Health at Birth Summary Statistics

	Mean	Std.Dev.	Min	Max	N	Definition
Weight	3202.492	513.35	42	6580	1,498,404	Weight at birth in grams of the first-born.
Low Weight	0.072	0.26	0	1	1,498,404	Dummy variable equals to 1 if the weight at birth of the first-born is under 2500 grams; 0 otherwise.
Very Low Weight	0.008	0.09	0	1	1,498,404	Dummy variable equals to 1 if the weight at birth of the first-born is lower than 1500 grams; 0 otherwise.
Weeks of Gestation	39.158	1.90	19	46	1,343,460	Number of weeks of gestation of the first-born.
Preterm	0.066	0.24	0	1	1,343,460	Dummy variable equals to 1 if the first-born is born under 37 weeks of gestation; 0 otherwise.
Very Preterm	0.008	0.09	0	1	1,343,460	Dummy variable equals to 1 if the first-born is born under 32 weeks of gestation; 0 otherwise.

Notes. Sample: mothers aged 25–33 at their first birth and born between 1975 and 1985. Data are from the 2000–2018 Vital Statistics (INE) childbirth microdata.

Table A4: Education Summary Statistics

	Mean	Std.Dev.	Definition
Panel A: High School Enrollment			
No Degree	0.010	0.10	Dummy variable equals to 1 if the highest educational degree is lower than primary school; 0 otherwise
Lower Secondary General Education	0.331	0.47	Dummy variable equals to 1 if the highest educational degree is compulsory general education; 0 otherwise.
Upper Secondary General Education	0.453	0.50	Dummy variable equals to 1 if the highest educational degree is general secondary (post-compulsory) education or college; 0 otherwise.
Vocational Education	0.157	0.36	Dummy variable equals to 1 if the highest educational degree is vocational secondary education; 0 otherwise.
N	109,339		
Panel B: Degree Completion			
No Degree	0.011	0.10	Dummy variable equals to 1 if the highest educational degree is lower than primary school; 0 otherwise
High School Degree	0.337	0.22	Dummy variable equals to 1 if the highest educational degree is lower or upper secondary education; 0 otherwise.
College Degree	0.361	0.23	Dummy variable equals to 1 if the highest educational degree is college; 0 otherwise.
Vocational Degree	0.254	0.19	Dummy variable equals to 1 if the highest educational degree is lower or upper vocational education; 0 otherwise.
Age at Highest Qualification	20.209	18.13	Age in years at completion of highest qualification.
N	85,348		

Notes. Panel A presents summary statistics on high school enrollment among women aged 17-24 and born between 1975 and 1985. Panel B presents summary statistics on degree completion for sample of women within the age range of 25 to 33. Source: 1991-2018 Spanish LFS.

Table A5: Information Processing Skills and Health Behavior Summary Statistics

	Mean	Std.Dev.	N	Definition
Panel A: Information Processing Skills				
Literacy Assessment	258.660	44.489	412	Score on the 2011 PIAAC literacy assessment (scale: 0–500); standardized (mean = 1, SD = 1) for main analysis.
Numeracy Assessment	247.484	45.888	412	Score on the 2011 PIAAC numeracy assessment (scale: 0–500); standardized (mean = 1, SD = 1) for main analysis.
Panel B: Health Behavior				
Mother Drinks During Pregnancy	0.067	0.249	2,365	Dummy = 1 if the woman consumed alcohol during pregnancy; 0 otherwise.
Mother Drinks	0.838	0.367	1,048	Dummy = 1 if the woman has ever consumed alcohol; 0 otherwise.
Mother Smokes	0.658	0.474	1,048	Dummy = 1 if the woman has ever smoked; 0 otherwise.
Mother Consumes Drugs	0.296	0.457	1,048	Dummy = 1 if the woman has ever used substances (alcohol, tobacco, hypnosedatives, illegal or new drugs); 0 otherwise.

Notes. Panel A is based on the 2011 PIAAC and includes women born 1975–1985 aged 25–33. Panel B reports data from the 2007 and 2017 Spanish Alcohol, Tobacco, and Illegal Drugs Surveys. "Mother Drinks During Pregnancy" is from 2017 (cohort 1975–1985), while the remaining variables are from 2007 (cohort 1975–1983).

Table A6: Maternal Background Characteristics Summary Statistics

	Mean	Std.Dev.	N	Definition
Panel A: Labor Market Outcomes				
Working Mother	0.860	0.345	1,521,820	Dummy = 1 if the mother is actively engaged in paid work outside the home, even if not currently working; 0 otherwise.
Qualified Job	0.558	0.496	1,234,724	Dummy = 1 if the mother works a highly trained job (managerial position, scientific or academic profession, administrative or office worker, qualified personnel in primary sector, qualified personnel in secondary sector and construction) conditional on being employed, 0 otherwise.
Non-Qualified Job	0.336	0.472	1,234,724	Dummy = 1 if the mother is employed in a nonskilled job (catering, protection and sales workers, plant and machinery operators and assemblers, or elementary occupations) conditional on being employed; 0 otherwise.
Panel B: Assortative Mating				
Mate Qualified Job	0.556	0.246	1,096,786	Dummy = 1 if the mother's partner works in a highly trained job (managerial position, scientific or academic profession, administrative or office worker, qualified personnel in primary sector, qualified personnel in secondary sector and construction) conditional on being employed, 0 otherwise.
Mate Non-Qualified Job	0.370	0.233	1,096,786	Dummy = 1 if the mother's partner works in a non-skilled job (catering, personal, protection and sales workers, plant and machinery operators and assemblers, or elementary occupations) conditional on being employed, 0 otherwise
Panel C: Family Planning				
Marriage Before Birth	0.647	0.478	1,576,204	Dummy = 1 if the mother was married before first birth (conditional on registered partner); 0 otherwise.
Duration of Marriage Before Birth	34.342	24.860	1,020,212	Months between marriage and first birth (conditional on being married).
Absent Father	0.014	0.116	1,576,204	Dummy = 1 if father's name is missing on birth register at childbirth; 0 otherwise.

Notes. Sample includes mothers aged 25–33 at their first birth, born between 1975 and 1985. Data in Panel A and B are derived from the Vital Statistics childbirth microdata (INE) for the years 2000-2016, while data in Panels C cover the years 2000-2018.

Table A7: Reform Effects on Health at Birth — Excluding Basque Country

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
Index	11.2050	-0.0010	-0.0023*
	(7.791)	(0.003)	(0.001)
Obs	1,378,639	1,378,639	1,378,639
Panel B: Gestational Age			
	Weeks	Late Preterm	Very Preterm
Index	0.0428	-0.0073**	-0.0033***
	(0.037)	(0.003)	(0.001)
Obs	1,228,825	1,228,825	1,228,825

Notes: The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1975 and 1985. Standard errors are shown in parentheses. Data are from the 2000–2018 Childbirth microdata of Vital Statistics (INE), excluding observations from the Basque Country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Effects of Educational Pathways on Health at Birth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Weight at Birth Measures									
	Weight			Low Weight			Very Low Weight		
Index	10.5373			-0.0011			-0.0018*		
	(6.948)			(0.003)			(0.001)		
Pre-reform Voc Index		-4.9956			-0.0005			0.0022	
		(14.681)			(0.006)			(0.002)	
Pre-reform GenEd Index			-16.7962**			0.0024			0.0020
			(7.951)			(0.003)			(0.001)
Romano-Wolf P-Value	0.3377	0.9111	0.2208	0.7413	0.9281	0.8432	0.2857	0.6014	0.5225
Obs	1,446,005	1,444,983	1,445,546	1,446,005	1,444,983	1,445,546	1,446,005	1,444,983	1,445,546
Panel B: Gestational Age									
	Weeks			Preterm			Very Preterm		
Index	0.0217			-0.0058**			-0.0026***		
	(0.032)			(0.003)			(0.001)		
Pre-reform Voc Index		-0.0783			0.0097*			0.0078***	
		(0.060)			(0.005)			(0.002)	
Pre-reform GenEd Index			0.0014			0.0060*			0.0006
			(0.045)			(0.004)			(0.002)
Romano-Wolf P-Value	0.7413	0.5305	0.9760	0.2048	0.3207	0.3876	0.0899	0.0280	0.9291
Obs	1,296,160	1,295,433	1,295,839	1,296,160	1,295,433	1,295,839	1,296,160	1,295,433	1,295,839

Notes. Estimates are obtained from estimating eq. (2) on a sample of mothers aged 25-33, born between 1975 and 1985. The coefficients reported in columns (1), (4), and (7) estimate the effect of the main LOGSE exposure index, as employed in our OLS TWFE main estimates in Table 3. The coefficients presented in Columns (2), (5), and (8) estimate the effects of the Pre-reform Voc Index, which represents the share of 14-year-olds enrolled in the pre-reform vocational education program under LGE. The coefficients displayed in Columns (3), (6), and (9) estimate the effects of the Pre-reform GenEd Index, which corresponds to the share of 14-year-olds enrolled in the pre-reform general education program under LGE. Standard errors are in parentheses. Romano-Wolf p-values based on 1,000 studentized bootstrap replications The data come from the 2000-2018 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A9: Reform Effects on Health at Birth Outcomes by Socio-economic Status

	(1) Weight	(2) Low Weight	(3) Very Low Weight	(4) Weeks	(5) Late Preterm	(6) Very Preterm
Panel A: Index (I_{tk})						
Index*SES_Index	2.5272	-0.0012	0.0002	-0.0054	0.0009	0.0003
	(1.803)	(0.001)	(0.000)	(0.009)	(0.001)	(0.000)
Index	9.7984	-0.0008	-0.0018*	0.0226	-0.0059**	-0.0026***
	(6.961)	(0.003)	(0.001)	(0.032)	(0.003)	(0.001)
SES_Index	6.9225***	-0.0025***	-0.0007***	0.0070	-0.0028***	-0.0009***
	(0.972)	(0.000)	(0.000)	(0.007)	(0.001)	(0.000)
Obs	1,446,005	1,446,005	1,446,005	1,296,160	1,296,160	1,296,160
Panel B: Pre-Reform Vocational Index (Pre-reform Voc Index$_{tk}$)						
Pre-Reform Voc Index*SES_Index	2.8029	0.0016	-0.0009	0.0068	-0.0002	-0.0014*
	(4.083)	(0.003)	(0.001)	(0.026)	(0.003)	(0.001)
Pre-Reform Voc Index	-10.5374	0.0010	0.0017	-0.0923	0.0089	0.0076***
	(15.018)	(0.007)	(0.002)	(0.066)	(0.006)	(0.002)
SES_Index	6.6597***	-0.0031***	-0.0004*	0.0029	-0.0024**	-0.0004
	(1.176)	(0.001)	(0.000)	(0.006)	(0.001)	(0.000)
Obs	1,349,457	1,349,457	1,349,457	1,207,848	1,207,848	1,207,848
Panel B: Pre-Reform GenEd Index (Pre-reform GenEd Index$_{tk}$)						
Pre-Reform GenEd Index*SES_Index	-0.7594	0.0008	-0.0005	0.0235*	-0.0029	-0.0011**
	(3.193)	(0.002)	(0.000)	(0.013)	(0.002)	(0.000)
Pre-Reform GenEd Index	-21.2554***	0.0030	0.0025	-0.0250	0.0070*	0.0011
	(7.873)	(0.004)	(0.002)	(0.048)	(0.004)	(0.002)
SES_Index	7.6765***	-0.0031***	-0.0003	-0.0066	-0.0011	-0.0002
	(1.565)	(0.001)	(0.000)	(0.004)	(0.001)	(0.000)
Obs	1,350,020	1,350,020	1,350,020	1,208,254	1,208,254	1,208,254

Notes: Estimates are obtained from estimating heterogeneity specifications of wq. (2) by interacting the reform exposure indices with maternal socio-economic status (SES), on a sample of mothers aged 25–33, born between 1975 and 1985. *SES_Index* is a standardized socio-economic status measure constructed using the Stata command `swindex`, which derives the first principal component from six baseline characteristics: maternal marital status, maternal and paternal occupational training, maternal and paternal age, and urban residence. Panel A presents results using the main reform exposure index (I_{tk}). Panels B and C report effects using two pre-reform exposure measures: the *Pre-reform Vocational Index*, defined as the share of 14-year-olds in a province-cohort cell enrolled in pre-reform vocational education, and the *Pre-reform General Education Index*, defined analogously for pre-reform general education. Robust standard errors in parentheses. The data come from the 2000–2018 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A10: First Stage Regressions

Dependent Variable: General Index$_{tk}$	(1)
General Index$_{1975}$ * 1977 FE	0.2534
	(0.165)
General Index$_{1975}$ * 1978 FE	0.2117
	(0.166)
General Index$_{1975}$ * 1979 FE	0.3063*
	(0.170)
General Index$_{1975}$ * 1980 FE	-0.1668
	(0.110)
General Index$_{1975}$ * 1981 FE	-0.2472
	(0.183)
General Index$_{1975}$ * 1982 FE	-0.5066**
	(0.218)
General Index$_{1975}$ * 1983 FE	-0.5966**
	(0.225)
General Index$_{1975}$ * 1984 FE	-0.7793***
	(0.156)
General Index$_{1975}$ * 1985 FE	-0.8779***
	(0.180)
Controls	YES
Obs	1,403,065
R-squared	0.816

Notes: The table reports the first-stage regression results for the instrumented share of students enrolled in the post- and pre-reform general education programs General Index$_{tk}$. Instruments are derived from the 1975 pre-reform cross-province variation, interacted with cohort fixed effects. The sample includes mothers aged 25–33, born between 1975 and 1985. Cohort and province fixed effects are included, along with additional province time-varying controls for GDP per capita, female employment rate, female labor force participation rate, log population, and mortality rates. Standard errors in parentheses. Data are from 1975–1985 and 2000–2018 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A11: 2SLS Regressions

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
General Index$_{tk}$	29.1627	-0.0043	-0.0023
	(23.974)	(0.009)	(0.004)
Obs	1,333,446	1,333,446	1,333,446
Panel B: Gestational Age			
	Weeks	Preterm	Very Preterm
General Index$_{tk}$	0.1098	-0.0010	-0.0055**
	(0.088)	(0.009)	(0.003)
Obs	1,194,339	1,194,339	1,194,339

Notes. This table presents the results of 2SLS regressions examining the effect of exposure to the post- and pre- general education programs (as measured by General Index$_{tk}$) on health at birth outcomes. Cohort and province fixed effects are included in all regressions, along with additional province time-varying controls for GDP per capita, female employment rate, female labor force participation rate, log population, and mortality rates. The sample includes mothers aged 25–33, born between 1975 and 1985. Standard errors are reported in parentheses. Data are from 2000–2018 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A12: Reform Effect on Fertility Patterns

	(1) Birth Rate	(2) Motherhood Entry Age
Index	-0.0012	-0.0630
	(0.004)	(0.043)
1975's Cohort Mean	0.069	29.518
Std. Dev.	0.03	2.36
Obs	4,995	1,521,770

Notes. The estimates are obtained from estimating eq. 2 on a sample of mothers aged 25–33 and born between 1975 and 1985. *Birth Rate* (column 1) is calculated as the number of first births divided by the number of women born in Spain per mother age and province. *Motherhood Entry Age* (column 2) is the mother's age at first birth. Standard errors are shown in parentheses. Data are from the 1975–1985 and 2000–2018 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A13: Peer Composition Changes

	(1) Weight	(2) Low Weight	(3) Very Low Weight	(4) Weeks	(5) Preterm	(6) Very Preterm
Panel A: High-Achieving vs Low-Achieving						
Index*High Skills	-2.9079	-0.0014	-0.0008	0.0003	0.0026*	-0.0006
	(5.048)	(0.002)	(0.001)	(0.018)	(0.002)	(0.001)
High Skills	15.1676***	-0.0096***	-0.0014***	0.0484***	-0.0099***	-0.0018***
	(3.535)	(0.001)	(0.000)	(0.012)	(0.001)	(0.000)
Index	10.5011	-0.0012	-0.0017	0.0370	-0.0071**	-0.0024**
	(6.924)	(0.003)	(0.001)	(0.035)	(0.003)	(0.001)
Obs	1,343,986	1,343,986	1,343,986	1,199,747	1,199,747	1,199,747
Panel B: Rural vs Urban						
Index*Rural	-1.2944	0.0027*	0.0004	0.0299*	0.0017	0.0003
	(2.392)	(0.001)	(0.000)	(0.015)	(0.001)	(0.001)
Rural	-4.4536	-0.0009	-0.0002	0.0027	-0.0011	-0.0002
	(3.122)	(0.001)	(0.000)	(0.010)	(0.001)	(0.000)
Index	11.3312	-0.0028	-0.0021**	0.0035	-0.0069**	-0.0028***
	(7.263)	(0.003)	(0.001)	(0.033)	(0.003)	(0.001)
Obs	1,446,005	1,446,005	1,446,005	1,296,160	1,296,160	1,296,160

Notes: The estimates are obtained from estimating equation in footnote (33) on a sample of mothers aged 25–33, born between 1975 and 1985. Standard errors in parentheses. Data are from the 2000–2018 Vital Statistics (INE) childbirth microdata. *** p<0.01, ** p<0.05, * p<0.10.

Table A14: Placebo Check for Spurious Correlations Between School Enrollment and Degree Completion Differences Prior to the LOGSE

	(1)	(2)	(3)	(4)	(5)
Panel A: School Enrollment					
	No Degree	Lower Secondary General Educ	Upper Secondary General Educ	Vocational Education	
Lead Index	0.001	0.026	0.009	0.008	
	(0.008)	(0.027)	(0.031)	(0.020)	
Obs	148,370	148,370	148,370	148,370	
Panel B: Degree Completion					
	Age at Highest Qualification	No Degree	Secondary Degree	Vocational Degree	College Degree
Lead Index	0.582*	0.002	-0.036	0.026	0.037
	(0.320)	(0.010)	(0.031)	(0.024)	(0.027)
Obs	106,171	126,764	126,764	126,764	126,764

Notes. The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1970 and 1975. Romano-Wolf p-values are based on 1,000 studentized bootstrap replications. Standard errors in parentheses. Data are from the 1987–2018 Spanish LFS. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A15: Placebo Check for Spurious Correlations Between Health at Birth Outcomes Differences Prior to the LOGSE

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
Lead Index	0.3735	0.0035	0.0016
	(6.007)	(0.003)	(0.001)
Obs	903,569	903,569	903,569
Panel B: Gestational Age			
	Weeks	Preterm	Very Preterm
Lead Index	-0.0142	0.0046	0.0020
	(0.024)	(0.003)	(0.001)
Obs	808,176	808,176	808,176

Notes. The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1970 and 1975. Standard errors in parentheses. Data are from the 1995–2013 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A16: Balance Test: Correlation Between LOGSE Exposure Index and Province Characteristics

	(1)	(2) Mean	(3) Std. Dev.
GDP per capita	-0.4738	10.543	3.33
	(0.291)		
Female Employment Rate	0.0089	0.263	0.06
	(0.008)		
Female Labor Force Participation Rate	0.0067	0.364	0.06
	(0.010)		
Log Population	0.0005	13.192	0.82
	(0.008)		
Infant Mortality Rate	-0.4077	6.179	2.03
	(0.657)		
Neonatal Mortality Rate	0.0157	3.910	1.56
	(0.526)		
Post-neonatal Mortality Rate	-0.4220	2.269	0.95
	(0.349)		
Obs	535		

Notes: Column (1) reports OLS coefficients; standard errors are in parentheses. Each specification includes controls for cohort and province of residence. The unit of analysis is province-by-year, and the coefficient is on the LOGSE Exposure Index. Columns (2) and (3) report the mean and standard deviation of the dependent variables. *GDP per capita* is the gross domestic product at market prices (in thousands of euros, based on 1985 prices) per capita as of July 1. *Female Employment Rate* is the proportion of employed women among the total female population aged 16–64. *Female Labor Force Participation Rate* is the proportion of women aged 16–64 who are either employed or actively seeking employment. *Log Population* is the natural logarithm of the population as of July 1. *Infant mortality* (deaths in the first year), *neonatal mortality* (deaths in the first 28 days), and *post-neonatal mortality* (deaths in months 2–12) rates are measured per 1,000 live births. Data from the Autonomous Cities of Ceuta and Melilla are excluded. Data source: 1990–2000 Spanish Statistical Office.

Table A17: Reform Effects on Health at Birth - Adding Province Time-Varying Controls

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
Index	10.3399	-0.0005	-0.0020*
	(7.533)	(0.003)	(0.001)
Obs	1,441,287	1,441,287	1,441,287
Panel B: Gestational Age			
	Weeks	Preterm	Very Preterm
Index	0.0311	-0.0058**	-0.0029***
	(0.029)	(0.003)	(0.001)
Obs	1,292,251	1,292,251	1,292,251

Notes. The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1975 and 1985. Province-level time-varying controls are included for GDP per capita, female employment rate, female labor force participation rate, log population, and mortality rates, as detailed in Table A16. Standard errors are shown in parentheses. Data are from the 2000–2018 Childbirth microdata of Vital Statistics (INE). *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A18: Spurious Correlation Between Migration Patterns Across Provinces and LOGSE Exposure Index By Age Group

	(1) 17–24	(2) 25–33	(3) Full Sample
Index	-0.0080	-0.0072	-0.0098
	(0.012)	(0.018)	(0.010)
1975's Cohort Mean	0.095	0.154	0.120
Std.Dev	0.293	0.360	0.326
Obs	109,339	85,348	194,687

Notes. Outcome: a dummy variable that takes the value 1 if a woman with Spanish nationality is currently residing in a province different from her birth province. Standard errors are in parentheses. The estimates are obtained from estimating eq. (2) on a sample of women aged 17–24 (column 1), 25–33 (column 2), and 17–33 (column 3) born between 1975 and 1985. Data are from the 1991–2018 Spanish LFS. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table A19: Reform Effects on Health at Birth - Reform Exposure at Region Level

	(1)	(2)	(3)
Panel A: Weight at Birth Measures			
	Weight	Low Weight	Very Low Weight
Aggregate Index	9.318	-0.002	-0.001
	(7.296)	(0.003)	(0.001)
Obs	1,446,005	1,446,005	1,446,005
Panel B: Gestational Age			
	Weeks	Preterm	Very Preterm
Aggregate Index	0.019	-0.005*	-0.002*
	(0.036)	(0.003)	(0.001)
Obs	1,296,160	1,296,160	1,296,160

Notes: The estimates are obtained from estimating eq. (2) on a sample of mothers aged 25–33 and born between 1970 and 1975. The index captures exposure to the reform at the regional level, rather than at the provincial level as in our main analysis (Table 3). Standard errors are in parentheses. Data are from the 1995–2013 Vital Statistics (INE) childbirth microdata. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.