Healthier Starts: The Intergenerational Effects of Maternal General Knowledge Acquisition

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January 31, 2025

(Draft)

Abstract

We study the long-term causal impacts of increasing mothers' general education on infant health outcomes using a comprehensive 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 enhancing women's ability to make informed decisions about marriage and fertility options and health behavior. Using quasi-experimental variation from a schooling reform in Spain that exogenously integrated more general education into the high school system, 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). Linked survey data on adult information processing skills, substance abuse, and labor and marriage market choices to administrative education records 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 are driven by better information processing skills, resulting in a better ability to avoid risky behaviors during pregnancy and improved family planning decision making.

JEL Codes: I26 I28 J13 J16

Acknowledgments: We would like to thank Gabriella Conti, Lindsay Macmillan, Isabel Ruiz, Cristina Bellés-Obrero, Bahadir Dursun and Judit Vall, and the seminar participants at 2023 EEA-ESEM Congress at Barcelona School of Economics, 2023 Risky Behaviour Conference at LSE, 2024 Annual Conference of the European Society for Population Economics and internal seminars at the Universidad Autonoma de Barcelona, UCL and Fundanción Ramón Areces for their insightful comments. Pilar Cuevas-Ruiz thanks the Fundación Ramón Areces for funding her research through a grant for postgraduate studies in social sciences at foreign universities and research centers. Cristina Borra acknowledges funding from the Spanish National Research Plan 2017-2020 (RTI2018-098217-B-I00) and 2021-2023 (PID2022-141375OB-I00). Almudena Sevilla thanks the European Research Council Consolidator Grant (CoG), SH3, ERC-2017-COG for funding her time through the PARENTIME project.

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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 are 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 the high medical costs. The expense of saving a newborn life at a birth weight near 1,500 grams estimated at approximately $550,000 (in 2006 dollars) (Almond et al., 2010). Despite significant advancements in medical care over the past decade, health at birth deficits, such as low birth weight and preterm birth, have remained stable worldwide (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 in order to effectively tackle these deficits (Almond and Currie, 2011; Aizer and Currie, 2014). While prior research has provided valuable insights into the effect of maternal years of schooling on infant health, the returns to different types of maternal knowledge remain underexplored. This gap arises in part from the difficulty of randomly assigning educational programs that equip future mothers with varying sets of skills. This paper provides the first causal evidence on the impact of mothers' general knowledge on children's health at birth, using variation from a schooling reform in Spain that expanded general education in high schools.

Standard theories of human capital formation suggest two main reasons why increasing mothers' general knowledge could improve infant health (Cunha and Heckman, 2007; Heckman, 2007). First, an educational program that emphasizes general knowledge over practical skills boosts earning potential by providing transferable skills, especially amid shifting demand and technological changes (Goldin, 2001;Hanushek et al., 2017). Positive assortative mating may amplify this effect, as a woman's education is causally linked to her partner's education, increasing household income through a multiplier effect (Behrman and Rosenzweig, 2004). Women with greater purchasing power will tend to acquire more and higher-quality material health inputs, such as better medical care, food, and housing, which can improve their children's health outcomes (Currie, 2009). Second, general knowledge improves mothers' information processing skills, enabling broader, more effective decision-making, while skill-specific knowledge is more limited in fostering the cognitive flexibility needed for such decisions (Thomas et al., 1991). Better processing skills improve an individual's capacity to acquire and apply knowledge about healthy behaviors and effective family planning (Grossman, 1972). For instance, an increase in mothers' ability to learn about healthy habits can lead to a reduction in smoking rates and an increase in the use of prenatal care (Currie and Moretti, 2003). Improvements in processing skills can also explain a mother's ability to use contraceptive methods effectively, leading to a reduced likelihood of unplanned pregnancies and greater control over the timing of births (Rosenzweig and Schultz, 1989).

To examine the impact of general maternal education on infant health at birth, we exploit a 1990 educational reform in Spain that exogenously increased general knowledge. The reform aimed to align Spain's educational standards after joining the European Union. Its main objective was to improve workforce competitiveness and resilience by delivering more general knowledge in the high school system. In particular, the reform required students to complete two additional years of general education, preventing them from entering vocational training at age 14 and instead mandating a common general education program through age 16.

The reform affected secondary school cohorts born between 1975 and 1985 who became 14 years old over the reform period. As a result of the staggered roll-out of the reform across provinces, students varied in their exposure to general knowledge depending on their province and birth cohort. We construct a measure of intensity of exposure to more general knowledge by manually digitizing rare enrollment figures from 1989–2000 educational registers, which provide detailed information on the distribution of students across educational programs.

We link administrative education records to population birth registers, labor force surveys, adult skills assessment surveys, and drug usage surveys to estimate difference-in-difference (DiD) models that rely on the exogenous variation in the exposure to the new general education system induced by the reform across provinces and cohorts. To limit concerns about the endogeneity of the reform's implementation, we estimate dynamic event-study analyses that lend support to the credibility of the parallel-trends assumption, and show that our estimates hold using alternative estimators robust to the presence of heterogeneous treatment effects in staggered settings (Callaway et al., 2021; Borusyak et al., 2024).[1] We also show that the reform roll-out is uncorrelated with pre-reform outcomes of interest, cross-province migration patterns, and province time-varying factors ruling out any potential anticipatory effect. Our empirical strategy also minimizes concerns about multiple hypothesis testing and the influence of other potential confounding factors that may bias our results, such as changes in fertility decisions and peer composition due to the policy change. Crucially, since the policy reform did not alter educational attainment or average years of schooling, 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.

We find that children born to mothers with increased exposure to general education as a result of the reform have better health outcomes at birth. The reform led to an overall 37% reduction in the incidence of very premature births (less than 32 gestational weeks). We also find coincident 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 premature births, 869 fewer premature births, and 270 fewer very low birth weight babies annually out of 150,000 births per maternal cohort. We conduct an additional exercise to assess whether the gains stem from students who, absent the reform, would have followed the vocational pathway or remained in general education while benefiting from the new system. By collecting new data on the share of 14-year-olds enrolled in the old vocational and general education programs and replicating our main empirical approach with these alternative treatment definitions, we find that the entire effect is driven by students who would have otherwise pursued vocational training.[2] Thus, the reform's impact can be interpreted as the effect of two additional years of general education. 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 education yield an effect nearly 70% as large. Since complications from very preterm births account for 52% of infant deaths before the first year of age in the U.S. (Barfield, 2018), the 37% reduction in very preterm births translates into a 19.24% decrease in infant mortality.

What explains the positive effects of mothers' increased general education on infant health at birth? To answer this question, we empirically test whether women's increased earning potential and improved infor-

[1] See Roth et al. (2023) and de Chaisemartin and D'Haultfœuille (2023) for a review.

[2] This result is robust to the identification strategy in Bertrand et al. (2018) and Bellés-Obrero and Duchini (2021), which implements an IV approach exploiting variation in the share of students already in general education in the last year before the reform was approved.

mation processing skills may be behind the effects of the education changes brought about by the reform on infant health. We find a 3.22% increase in maternal labor force participation that is not accompanied by any change in mothers' occupational choices as a result of the policy change. We fail to find any significant impacts of the reform on the likelihood of having a partner or the quality of their partners' occupation. This evidence suggests that the education affected the extensive margin of maternal labor supply rather than the quality of maternal employment or women's value in the marriage market. Turning to the effects of more general education on women's knowledge and health behavior, we find that mothers exposed to more general education scored significantly higher in the 2012 PIACC literacy and numeracy assessments. These improved information processing skills during adulthood are concurrent with a significant reduction in the likelihood of consuming alcohol while pregnant and a significant increase in the likelihood of being married before birth. This evidence suggests that the reform improved women's information processing skills, reducing the likelihood of risky health behavior during pregnancy and increasing women's control over the timing of births and family planning practices. Overall, the observed reductions in the incidence of very preterm births and very low birth weight among mothers exposed to the policy reform seem to be driven by increased maternal labor market opportunities and improved information processing skills.

Our paper contributes to a broad body of literature that aims to understand the origins and intergenerational transmission of inequality. Much of this literature has looked at a wide range of determinants of health at birth, such as prenatal substance abuse, maternal exposure to air pollution during pregnancy, nutrition, poverty, cash and near-cash transfers, health, stress, participation in social programs, and education, as reviewed by Currie et al. (2010), Aizer and Currie (2014), Björklund and Salvanes (2011), Almond and Currie (2011) and Almond et al. (2018). Prior research looking at the effects of maternal education on infant health has mainly focused on maternal years of schooling, producing mixed results. Some studies find positive effects, such as Currie and Moretti (2003), who use U.S. college openings as an instrument to show improvements in birth weight and gestational age, and Grytten et al. (2014), who find similar benefits from Norway's 1960 compulsory schooling reform. In contrast, McCrary and Royer (2011) exploit U.S. school entry age policies and find little impact on fertility or infant health. Studies in the U.K. also report limited effects using variation from the 1947 compulsory schooling reform and schooling costs during adolescence (Lindeboom et al., 2009; Carneiro et al., 2013). One explanation for these conflicting findings is that women who are more likely to attend college may gain more from education than those who are required to stay in school but are at risk of dropping out (Conti et al., 2010). Another possibility is that differences in educational content contribute to these varying results. Recent evidence finds that increased math coursework can improve child outcomes (Dursun et al., 2024). Understanding how broader educational content impacts infant health has been challenging because of the absence of reforms that provide more general education without increasing years of schooling and the challenge of tracking intergenerational effects with current data. We address these gaps by first, leveraging an unique policy shock that increased general education in secondary schooling while keeping total years constant and second, by linking population-wide birth records to maternal education histories. Our findings suggest that more general education shapes early-life health, providing new insights into the role of maternal education in improving intergenerational mobility.

Our paper also contributes by empirically exploring the theoretical mechanisms through which increased general education optimizes the health at birth production function. Our linked administrative and survey data allow us to identify the causal effects of more general education on a wide set of maternal outcomes that were explicitly targeted by the policy —such as cognitive skills, educational attainment, labor force participation, and occupational choices— and additional adult outcomes including information processing

skills, health behaviors, and marital choices. In doing so, our work paints a richer picture compared with previous papers using similar policy reforms to study adult outcomes, which tend to focus on a single outcome of interest.[3] Our findings suggest that improved information processing skills help mothers make better decisions regarding risky health behaviors, as well as marriage and fertility timing. This sheds light on an important channel through which policies that increase maternal general knowledge can lead to long-term positive effects on child outcomes.

This paper is organized as follows. The second section reviews the institutional framework of the Spanish secondary education systems over the period from 1970-2002 period. 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.

2 Institutional Background: the LOGSE Reform

Our analysis exploits a major comprehensive education reform in Spain, known as LOGSE,[4] implemented in the 1990s, as an exogenous variation in educational pathways to test the effects of more general education on infant health at birth.

2.1 Pre-Reform System

Before the introduction of LOGSE, the Spanish education system was governed by the 1970 LGE law.[5] Under this framework, primary and lower secondary general education was provided through a unified curriculum, compulsory for all students aged 6 to 13. Upon completion of lower secondary general education, students were given the choice of following either upper secondary general education or lower vocational education.[6] Upper secondary general education was a three-year program that emphasized subjects such as mathematics, languages, natural and social sciences. Lower vocational education was a two-year curriculum that focused primarily on practical training with limited exposure to general education subjects. Students who completed either the lower vocational or upper secondary general education could have access to upper vocational education. However, only students who had graduated from upper secondary general education were eligible to enroll in the pre-college program, a mandatory requirement for university entry. A visual representation of the main pathways of the Spanish high school system prior to the LOGSE is provided in Appendix Figure A1 for further clarification.

[3] See for instance Malamud and Pop-Eleches (2011) and Pekkala Kerr et al. (2013) for educational attainment, and Bertrand et al. (2020) and Bellés-Obrero and Duchini (2021) for labor market outcomes.

[4] Organic Law 1/1990 (*Ley Orgánica de Ordenación General del Sistema Educativo*).

[5] Law 14/1970 (*Ley General de Educación).*

[6] Selection for further education was not based on ability.

2.2 Post-Reform System

The LOGSE reform extended compulsory lower secondary general education from age 14 to 16.[7] This represented a major change for students who would previously have opted for lower vocational education at 14, as they were now required to complete two additional years of general education before moving on to occupation-specific instruction. Table A1 in the Appendix compares the pre-reform and post-reform high school curricula. Under the old system, while lower vocational education devoted most of the instruction to specific occupational skills, lower secondary general education already focused on academic subjects, closely mirroring the curriculum introduced by LOGSE. Consequently, as a result of the reform, those who would have previously pursued vocational education at 14 were instead compelled to follow a common general education program until 16, whereas those who would have continued to general education experienced no major changes.[8] After completing lower secondary education, students could choose to continue in upper secondary general education or move on to lower vocational studies, thus ensuring a broader general knowledge base for all. Figure A1 in the Appendix also shows the main pathways of the Spanish high school system before and after LOGSE.

2.3 LOGSE Reform Implementation Process

The new high school system and, thus the extension of general education had to be fully implemented by the 1999-2000 school year. The old and new education systems coexisted during the 1990-2000 transition period. The time series evidence presented in Figure 1 shows the changes in enrollment patterns between the pre- and post-reform systems during the 1990s, as indicated by the share of 14-year-old students enrolled in the two systems. The cohort born in 1975, turning 14 in 1989, was the last to graduate fully under the pre-reform high school system, while the 1985 cohort, turning 14 in 1999, was the first to study exclusively under the post-reform high school system. The Spanish central government granted education centers ten school years to fully implement the new system, and provinces differed in the pace at which they introduced the LOGSE system at different levels of education. As we will see in section 5.4, the staggered implementation of the reform across provinces and over time is effectively random for all practical purposes.

[7] Starting from the 1991-1992 school year, the reform also increased the school leaving age from 14 to 16, requiring students to remain enrolled under either the old LGE system or the new LOGSE system (Felgueroso et al., 2014; Bellés-Obrero and Duchini, 2021). However, this was merely an institutional change, as by the 1991-1992 school year, the enrollment rate for students aged 14-16 was already 95.05% (Servicio de Estudios Estadísticos, 1994).

[8] Similarly to the reforms studied in Malamud and Pop-Eleches (2011) and Pekkala Kerr et al. (2013), the LOGSE comprehensive schooling reform broadened the high school curriculum by forcing all students to complete a full package of common core academic subjects, rather than enrolling students in stand-alone technical high schools as in Brunner et al. (2023) and Dougherty (2018) or expanding coursework in specific subjects as in Goodman (2019) and Dursun et al. (2024).

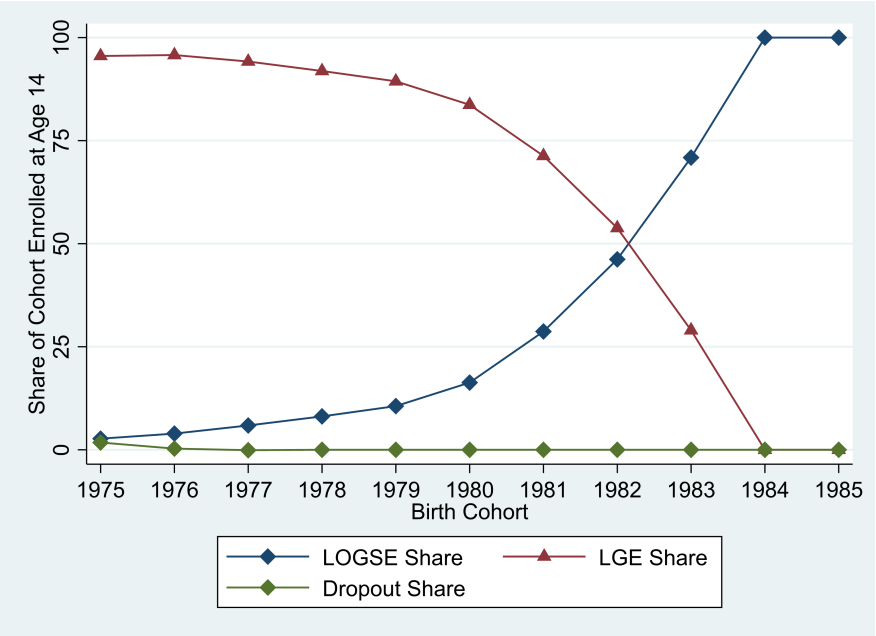


Figure 1: The LOGSE and The LGE Enrollment Shares by Birth Cohort

Notes. This figure shows the levels of enrollment of 14-year-olds across the two educational systems by birth cohort, as well as the share of those not in the education system. The 1975 cohort was the first be eligible for the reformed high school system. Source: Annual Education Statistics reports, Statistical Office of the Education Ministry, multiple years: 1989-2001.

3 Data

Our research employs five primary data sources, linked by the year of birth and the province of residence of women whose offspring's health at birth is under examination.

3.1 Childbirth Register Data

Information on health at birth comes from the 2000-2018 birth registers available from the Spanish Statistical Office. Birth registers collect data from the birth bulletins, which are filled in when a child's birth is recorded. The data include certain characteristics of the birth, such as birth order, gestational age, birth weight, parents' demographic and employment backgrounds, and province of residence.[9] In our analysis, we focus on outcomes related to the health at birth of children born to mothers belonging to the 1975-1985 cohorts, turning 14 during the 1900's. We restrict our attention to first births among mothers aged 25 to 33. We focus on first births because education can potentially impact completed fertility.[10] The age range was chosen because most mothers would have completed their education by age 25, and 33 is the oldest age for the youngest cohort (born in 1985) in our data. By focusing on mothers who completed their education before having children, we are able to examine the full returns to education on infant health at birth. This approach

[9]Throughout the analysis, we assume that mothers attended high school in their province of residence at the time of birth. According to the 2021 Spanish Census, over 90% of women give birth in the same province they lived at 14-years old. See also in section 5.4 that the reform did not induce changes in cross-province migration patterns among women in our sample.

[10]We rule out any impacts of the reform on fertility choices in section 5.4.2.

results in an analysis of 1,521,770 first births between 2000 and 2018 to mothers born between 1975 to 1985, aged 25 to 33.

Health at birth is proxied by birth weight and gestational age. Specifically, we look at 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).[11] We also look at infant mortality outcomes, such as the incidence of fetal death and the rate of survival 24 hours after birth. Summary statistics for these health outcomes at birth are available in Table A2 in the Appendix. In our full sample, newborns with low birth weight represent 7% of our sample, while 6.6% of newborns in our sample are classified as preterm births. Very low birth weight and very preterm births are both around 1%, and the mortality rates at birth are just over 0.1%. These results are expected, due to the fact that pregnant women in Spain have extensive medical contact, facilitated by the universal coverage the public healthcare system provides, which fully covers prenatal, delivery, and postpartum care. Note also that the incidence of very preterm births constituting 1–2% of all live births is consistent with the prevalence of these rates in high-income countries (Larroque, 2004; Barfield, 2018; Ohuma et al., 2023).

To further investigate the mechanisms through which the education reform may have affected infant health at birth, we also study variables related to mothers' background. Specifically, we consider mothers' marital status, labor market participation, occupation, partnership choice, and motherhood entry age. Summary statistics for these labor and marriage outcomes, along with their definitions, can be found in Table A3 in the Appendix.[12] In our full sample, about 65% of mothers are married, slightly over 1% fail to report a known father for the child, and 86% engage in paid work outside the home. Conditional on employment, 55.8% work in qualified occupations and 33.6% in non-qualified occupations. Conditional on having a registered father for the child, 47% of the fathers work in qualified occupations.

3.2 Spanish Labor Force Survey

Information on educational outcomes is taken from the Spanish Labor Force Survey (LFS), a quarterly continuous survey administered to approximately 65,000 households and 160,000 individuals per quarter. It collects data on employment, education, and socio-demographic characteristics for all individuals aged 16 and over regardless of their labor force status. These include educational attainment, age at highest qualification, occupation, nationality, and province of residence. In line with prior studies, we draw on information from the second quarter of each year, from 1991 to 2018.[13] Our sample comprises all female respondents aged 17-33 who are Spanish nationals, for a total of 201,701 women born between 1975 and 1985.[14]

Since we are interested in women's entire educational pathways and not just their highest qualification, we

[11] These outcomes have been used in the literature to document the strong association between maternal educational attainment and infant health (Currie and Moretti, 2003; Chou et al. 2010; McCrary and Royer 2011, among others). They have also been used as proxies to measure the impact of health at birth on future child outcomes (Behrman and Rosenzweig, 2004; Almond et al., 2005; Oreopoulos et al., 2006, among others).

[12] Note that, given that labor market participation and occupation data changed from 2017 onwards, we use 2000-2016 data to study mothers' and fathers' employment and occupation outcomes.

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

[14] The Spanish LFS selects individuals based on current age and not based on year of birth. Given that the Spanish education system sorts individuals according to year of birth, we leave out of the analysis the few individuals who would have already turned 16 before March when the second quarter interviews would have started.

use the LFS data to examine education levels at ages 17 to 24, prior to the completion of their education. Panel A of Table A4 in the Appendix presents the summary statistics of the enrollment patterns: 33% completed at lower secondary general education, 45% completed upper secondary general education, and 16% completed vocational studies by the age of 25.[15] In Panel B of Table A4, we study four categories of educational attainment describing degree completion patterns of women aged 25-33, after respondents had the opportunity to fully complete their education. On average, respondents obtained their highest qualification at 20.2 years of age, with 34% finishing high school without continuing their studies, 25% obtaining a vocational qualification, and 36% obtaining a college degree.

3.3 The PIAAC data

We use survey data on adult skills from The Program for the International Assessment of Adult Competencies (PIAAC) to assess the impact of returns on more general education on women's information processing skills as a channel through which mothers' educational pathways may impact offspring's health at birth. Developed by the OECD and gathered from August 2011 to March 2012, this survey offers data on the proficiency of adults in essential information-processing skills that can be compared across countries. These skills include literacy, which is the capacity to comprehend and appropriately react to written materials, and numeracy, which is the ability to employ mathematical and numerical concepts effectively. In every participating country, a demographically representative sample of adults aged 16 to 65 was surveyed at home using the language spoken in their country. Prior to assessing their skills, participants completed a preliminary questionnaire collecting demographic information, such as gender, birth year, and region of residence. Our analysis uses the literacy and numeracy assessments from the 2011 wave, the only available PIAAC data in Spain. The PIAAC assessments measured these skill areas using a scale that extends to 500 points. We normalize scores to have an average of zero and a standard deviation of one. To ensure consistency with our main analysis of health at birth, we limit our sample to women aged 25 to 33 born between 1979 and 1985 (turning 14 between 1993 and 1999), aligning with the 2011 survey data constraints. This adjustment accounts for the fact that only those born in 1979 or later could have reached the age of 33 by 2011, narrowing our focus to a specific and comparable age cohort.

3.4 The Alcohol, Tobacco, and Illegal Drugs Survey

To explore the returns of more general education on mothers' risky health behaviors, we use data from the 2007 and 2018 Alcohol, Tobacco, and Illegal Drugs Survey collected by the Spanish Observatory on Drugs and Addictions and published by the Spanish Health Ministry. The Alcohol, Tobacco, and Illegal Drugs surveys collect information on the consumption of psychoactive substances (including alcohol, tobacco, hypnosedatives, illegal drugs, and new substances) as well as some demographics characteristics such as age, sex, province of residence, and household composition. Conducted on a nationwide basis, the survey targets Spain's resident population aged 15 to 64 years, ensuring nationally representative results. It employs personal interviews at respondents' addresses, where an interviewer remains present throughout the process and collects completed questionnaires. To explore maternal risky health behaviors, we use survey data from 2017, which contains information regarding whether the mother consumed any alcoholic drinks during pregnancy. We narrow the sample to include only mothers exposed to the curriculum change, namely those

[15] See also definition of high school enrollment and degree completion categories in Table A4 in the Appendix.

born between 1975 and 1985, yielding a sample of 2,314 mothers.[16] We also use 2007 survey data to examine maternal lifetime consumption of alcohol and tobacco, as well as to explore drug use, specifically assessing whether there was any use of cannabis, cocaine, ecstasy, LSD, amphetamines, heroin, and tranquilizers. For this set of variables, we select women aged 25 to 33, born between 1975 and 1983 —excluding those born after 1983, as women from 1983 onwards did not reach the age of 25 by 2007— and who report having at least one offspring cohabiting in the same household, yielding a sample of 1,003 mothers. Table A5 in the Appendix presents the summary statistics and definitions of these variables.

3.5 LOGSE Exposure Index

Our fourth data source consists of administrative enrollment data from the Statistical Office of the Education Ministry. We digitized the province-year data on the number of 14-year-old students enrolled in each academic level during the LOGSE implementation period. Our data spans from the 1989-1990 school year to the 1999-2000 school year, including the first iteration of the LOGSE pilot study, the so-called *Bachillerato Experimental*, which began in 1989.[17] We create an aggregate index to indicate the level of exposure to the LOGSE reform at age 14, which is broken down by province of residence and year of birth. The 1975 cohort was the first to be exposed to the LOGSE pilot (1989-1990 school year) and the 1985 cohort was the last to be exposed to the previous education system (1999-2000 school year). The index is calculated as follows:

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

Where I$_t k$ is a continuous treatment function that reflects the implementation of LOGSE, which is the proportion of students under the LOGSE for cohort t in province k. The numerator represents the number of 14-year-old students enrolled in the new general education system, and the denominator represents the total number of 14-year-old students enrolled in both systems – the new general education system and the old vocational or general education tracks. Student numbers in each academic level include repeaters. We exclude students who repeated a grade and would not belong to the birth year cohort considered. The LOGSE index measures the varying intensity of exposure to the reform across provinces and birth cohort years during the transition period between the old and new high school systems.

To give a sense of the implementation of the reform, Figure 2 shows how the LOGSE exposure index fluctuated during the transition period between 0 (i.e. no student was under LOGSE) and 1 (i.e. all students under LOGSE in a given province and school year). Lighter colors correspond to lower levels of

[16] Note that this sample is not restricted based on household composition or age, given that pregnancy can occur across a woman's reproductive years. Had we narrowed the age range to 25 to 33 years, as in our primary health-at-birth analysis, we would have excluded mothers born before 1984 who were exposed to the policy change—since the alcohol consumption during pregnancy variable is unique to the 2017 survey.

[17] The *Bachillerato Experimental* was the pilot of the LOGSE reform, which was approved in the Experimental Reform of Secondary Education in 1983. It was implemented between the 1989-1990 and 1997-1998 school years in a limited number of high schools, mainly in the Basque Country and Navarre regions. *Bachillerato Experimental* was divided into two cycles, beginning with the first cycle of lower secondary education (from age 14 to 16), comprising the first and second years of high school. These two years were compulsory and resulted in a two-year delay in access to vocational studies. The second cycle took place in upper secondary education, comprising the third and fourth years of high school (from age 16 to 18). Students could only continue into the third year if they passed the first cycle.

LOGSE implementation (captured by $I_t k$) for cohort t in province k. The LOGSE was gradually expanded at varying rates across provinces during the transition period. At the beginning of the period (1992-1993 school year), less than 10% of the 14-year-old student population was under the new system in almost all provinces, but by the end of the transition period (1997-1998 school year), over 60% of 14-year-old students were under the new system.

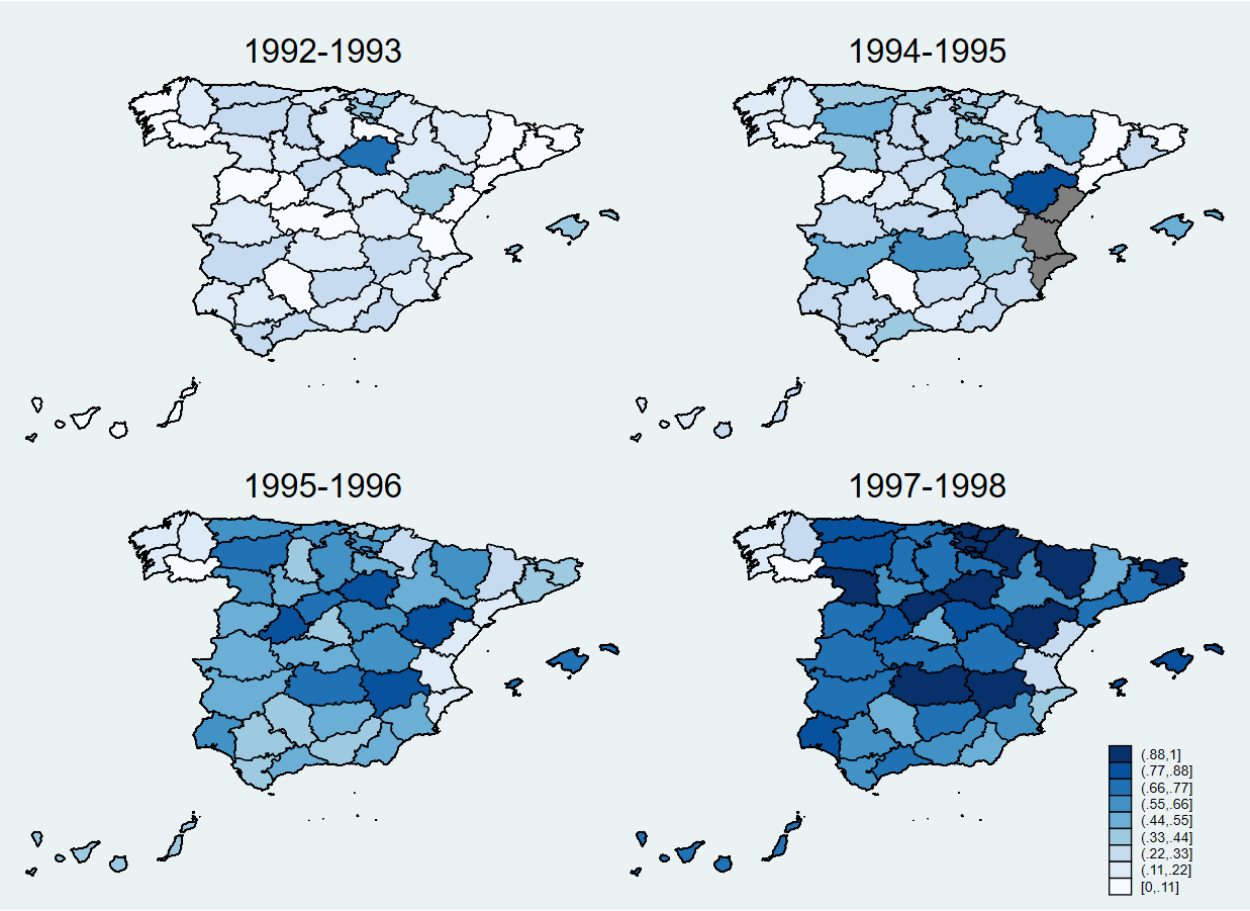


Figure 2: Geographic Variation in LOGSE Implementation

Notes: This figure shows the proportion of students under the 1990 LOGSE reform at age 14 (scale of 0 to 1) by province in the 1992-1993, 1994-1995, 1995-1996, and 1997-1998 school years. Increasing levels of LOGSE exposure are indicated by darker blue shading; provinces in grey represent missing data. Source: Education Statistics 1990-1991 and 1999-2000 (Education Ministry).

4 Empirical model

In order to identify the effects of more general education on infant health at birth, our research design leverages the staggered implementation of a comprehensive education reform that triggered an exogenous increase in general secondary education. We compare the health outcomes of children of mothers with different exposure to the policy change during the implementation process. We estimate the following dose-response DiD model separately for each outcome variable:

$$Y_{itk} = \alpha + \beta I_{tk} + \eta_t + \theta_k + \epsilon_{itk} \quad (1)$$

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), very premature birth (less than 32 weeks) and infant mortality at birth.

I_{tk} is our key regressor, which captures the intensity of LOGSE exposure for mothers born in year t in province k. 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 better health outcomes at birth.

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. Standard errors are clustered at the province level. ϵ_{itk} denotes the model error term.[18]

In section 5.4, we discuss the additional assumptions needed for two-way fixed effects (TWFE) estimators to be valid in our continuous treatment setting in light of the recent literature regarding DiD applications beyond the canonical binary setting (Callaway et al., 2024).

5 Results and Discussion

We present our findings in four sections. In the first section, we evaluate the effect of the reform on women's educational attainment, enrollment patterns, and degree completion. Our analysis reveals that while the reform did not significantly impact degree completion rates or years of schooling, it did increase the provision of general education by shifting students from the old vocational and general education program into the new general education high school system. We present our main results on the impact of the reform on infant health in the second section. Our findings indicate that the reform significantly reduced the incidence of very preterm births (less than 32 weeks) and led to less precisely estimated reductions in very low birth weight (less than 1,500 grams) and premature birth (less than 37 weeks). Finally, we conduct a series of identification and robustness checks to probe the validity of our identifying assumptions and alleviate concerns regarding potential sample selection bias arising from fertility decision changes and the confounding impact of peer group changes.

5.1 Effects on Education

Effects on Enrollment

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.[19]. We find that the LOGSE exposure leads to an increase of 10.38 percentage points in the share of female students

[18]We adjust the standard error for multiple hypothesis testing.

[19]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.

enrolled in lower secondary general education, a rise of 33.16% (column 2). Meanwhile, the reform reduces 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). Overall, the data presented in Table 1 suggest that the reform led to a mechanical shift of women away from the old system into the new system, which extended the lower secondary general education stage.

Table 1: Reform Effects on High School Enrollment

	(1) No Degree	(2) Lower Secondary General Educ	(3) Upper Secondary General Educ	(4) Vocational Educ
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
Observations	109,339	109,339	109,339	109,339
R-squared	0.003	0.019	0.022	0.012

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 1 on a sample of female Spanish nationals aged 17-24, born between 1975 and 1985. All specifications include a constant and birth year and province fixed effects. Standard errors are clustered at the province level for each specification. Data are from the 1991-2018 Spanish LFS. ***Significant at the 1% level, **Significant at the 5% level, * Significant at the 10% level.

Effects on Degree Completion

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.[20] 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 of Table 2 show that the LOGSE did not lead to a larger proportion of women finishing their schooling with a particular qualification. Hence, the LOGSE reform neither succeeded in increasing the share of women with a high school diploma or vocational credentials, nor did it affect the share of women with college degrees. In sum, our degree completion estimates suggest that the reform did not increase educational attainment or years of schooling.

All in all, the results from Tables 1 and 2 suggest that the reform did not affect women's educational attainment, but rather induced a change in educational pathways towards more general education.

5.2 Effects on Health at Birth

Table 3 reports the reduced-form effects of the reform on our main health outcomes at birth: birth weight, gestational age, and infant mortality at birth. In order to capture the effects of completed education, we restrict our analysis to mothers aged 25 to 33.[21]

[20] 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.

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

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
Observations	85,004	85,348	85,348	85,348	85,348
R-squared	0.033	0.004	0.026	0.011	0.022

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 1 on a sample of female Spanish nationals aged 25-33, born between 1975 and 1985. All specifications include a constant and birth year and province fixed effects. Standard errors are clustered at the province level for each specification. Data are from the 1991-2018 Spanish LFS. ***Significant at the 1% level, **Significant at the 5% level, * Significant at the 10% level.

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 (less than 2,500 grams and less than 1,500 grams, respectively). Column 1 of Panel A 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%.[22] Column 2 of Panel A 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, Panel A). 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 P-value=0.3526).[23] 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.

Next, 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 (less than 37 weeks and less than 32 weeks, respectively). Column 1 of Panel B 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 of Panel B 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 of Panel B 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%. However, only the estimates for very

[22]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.

[23]Figure A2 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.

preterm births remain significant after a multiple hypothesis testing correction. As shown in Figure 3, 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 3 suggest that the positive effects observed for premature births are primarily driven by reductions at the very preterm threshold.

Panel C displays the effects of the reform on our infant mortality measures: the likelihood of fetal death and the likelihood of survival 24 hours after birth. Consistent with expectations for a universal public health care system with a 99.9% chance of survival 24 hours after birth and a 0.2% incidence of fetal death, we find that the reform had no impact on mortality outcomes.

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	-0.4446	0.9091	0.3526
1975's Cohort Mean	3199.580	0.070	0.007
Std.Dev	506.73	0.25	0.09
Observations	1,446,005	1,446,005	1,446,005
R-squared	0.002	0.000	0.000
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	0.8611	0.2507	0.0949
1975's Cohort Mean	39.145	0.069	0.007
Std.Dev	1.90	0.25	0.08
Observations	1,296,160	1,296,160	1,296,160
R-squared	0.001	0.000	0.000
Panel C: Mortality at Birth			
	Fetal Death	Survive 24h after Birth	
Index	-0.0002	0.0000	
	(0.000)	(0.000)	
Romano-Wolf p-value	0.8611	0.9940	
1975's Cohort Mean	0.002	0.997	
Std.Dev	0.05	0.05	
Observations	1,521,770	1,521,770	
R-squared	0.001	0.001	

Notes. Standard errors are in parentheses. The estimates are obtained from estimating 1 on a sample of mothers aged 25-33 and born between 1975 and 1985. All specifications include a constant and birth year and province fixed effects. Standard errors are clustered at the province level for each specification. 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.

5.3 Interpreting the Results

In this section, we examine whether the estimated positive impacts of the reform on infant health at birth are driven by students who would have continued to vocational training instead of general education prior

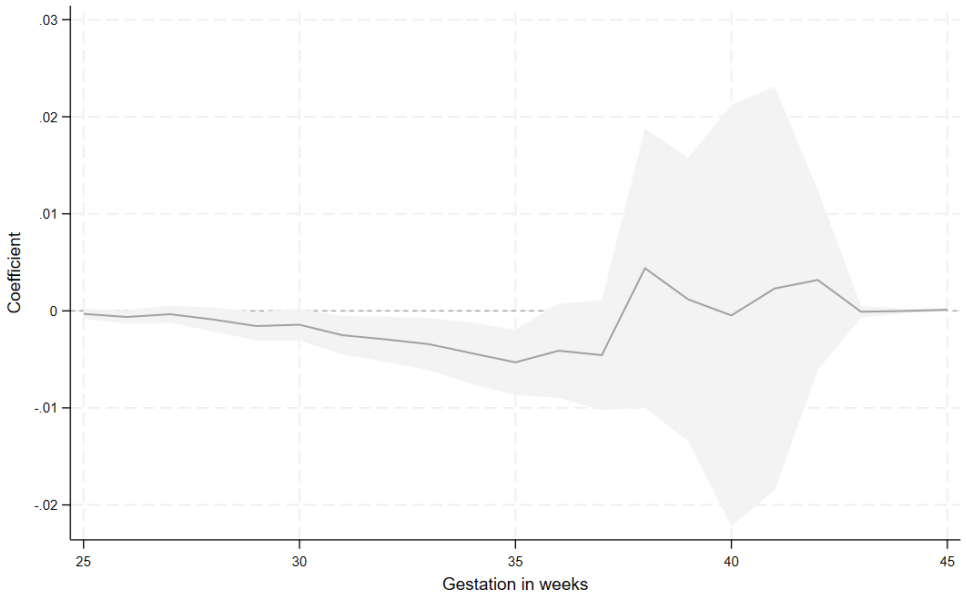


Figure 3: Reform Effects along the Distribution of Gestational Weeks

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 likelihood of having a birth with gestation weeks of at most the amount indicated on the x-axis. All coefficients were computed estimating 1. Standard errors are clustered at the province level. Source: Data were sourced from the Childbirth Microdata of Vital Statistics (INE) for the years 2000-2018.

to the reform, or by those who would have followed the general track regardless but still experienced gains from the new system. Identifying the group of students who would have followed the old 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 new indices computing the proportion of 14-year-olds enrolled either in the old vocational vocational or the old general education programs. Second, following Bertrand et al. (2018) and Bellés-Obrero and Duchini (2021), we measure the proportion of students in either the old or new 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 1 using supplementary enrollment data on the share of students at 14-years old under the old vocational program and the share of students under the old 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 *Old Vocational Index* as the share of 14 years old enrolled in the old vocational program:

$$OldVocationalIndex_{tk} = \frac{LGEVocationalEducation_{tk}}{LOGSE_{tk} + LGETotal_{tk}}$$

Similarly, the *Old General Index* measures the share of 14 years old enrolled in the old general education

program: [24]

$$OldGeneralIndex_{tk} = \frac{LGEGeneralEducation_{tk}}{LOGSE_{tk} + LGETotal_{tk}}$$

Table A6 in the Appendix reports results from this exercise, regressing the very preterm birth outcome - the most robustly estimated health at birth outcome - on the indices. Estimates reported in columns 1 and 2 indicate that the reform's main impacts are primarily driven by the substitution of general education for vocational training. Specifically, column 2 of Table A6 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), a result that closely mirrors our main estimate (column 1), suggesting that the reform's effects are primarily driven by vocational students.

To perform the second exercise, we add the share of students under the old general program to the share of students in the new general education program under LOGSE, that is adding up our $OldGeneralIndex_{tk}$ 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: [25]

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

Following Bellés-Obrero and Duchini (2021), we instrument the staggered roll-out of the share of students enrolled in either the old or new general education programs, captured by $GeneralIndex_{tk}$, with the 1975-cohort pre-reform cross-province variation in this share, interacted with cohort fixed effects. [26]

[24] Figure A3 in the Appendix illustrates the Old Vocational and Old 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 Old Vocational and Old General 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. (1). Specifically, the Vocational 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 old vocational training, while the Old General Index_{tk} represents a corresponding decline of 66 percentage points (from around 66% to 0%).

[25] 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. (1).

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

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

Results from this exercise are presented in Table A8 in the Appendix. [27] Consistent with findings in Table A6 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.

5.4 Threats to Identification

We address three threats to identification: (i) the validity of our identifying assumptions, (ii) potential sample selection bias because of changes in fertility decisions, and (iii) the confounding impact of peer group changes.

5.4.1 Identifying Assumptions

Our identification strategy exploits the within-region variation in the timing of LOGSE implementation across Spanish provinces. Treatment exogeneity requires that the rollout of the reform be exogenous with respect to pre-reform outcomes of interest (Goodman-Bacon, 2021). Moreover, it requires that provinces' past enrollment and health at birth patterns do not influence the implementation of the policy, either because provinces that anticipate larger benefits are more eager to implement the policy (the so-called anticipation effects) or because provinces subject to negative shocks are more likely to be treated earlier (the so-called Ashenfelter (1978)'s dip).

We provide three exercises that support the assumption of exogeneity in the implementation process. First, Tables A9 and A10 in the Appendix report placebo estimates using labor force and birth register data from five cohorts prior to the first cohort exposed to the reform (1970-1975) and find no evidence of differential pre-reform trends. In particular, we create a lead index that captures differences in the implementation of the reform across cohorts and provinces. We test whether differences in prior school enrollment patterns and health at birth outcomes are spuriously related to the subsequent rollout of the reform, and find that the coefficients on the lead index are not statistically different from zero supporting the exogeneity assumption.

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where i is an individual in cohort t and province k, whose children's health outcome is observed. The dependent variable Y_{itk} represents the outcome of interest, such as very low weight or very preterm birth.

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:

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

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

[27] First-stage results presented in Table A7 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 old and new 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.

Second, the assumption of exogeneity also requires that the timing of the reform rollout across provinces be uncorrelated with other province time-varying determinants of our outcomes of interest. Table A11 in the Appendix presents estimates on the correlation between exposure to the reform and province time-varying characteristics using data collapsed to the year-province level for the 1990-2000 period. We find that the timing of the reform rollout does not predict any province's time-varying characteristics, including GDP per capita, female labor force participation, female employment rate, population, infant mortality, as well as neonatal and post-neonatal mortality. We also show in Table A12 in the Appendix that our results are robust to the inclusion of these time-varying province-level controls.

Third, we demonstrate in Table A13 of the Appendix that migration patterns across Spanish provinces do not exhibit spurious correlations with the staggered implementation of the reform across provinces, alleviating endogeneity concerns due to the confounding impacts of inter-provincial migration patterns.

Additionally, to deliver consistent estimates for the average treatment effect on the treated (ATT), our identification strategy requires the relatively strong assumption of homogeneity of treatment effects over time and across intensity levels. To analyze the dynamics of treatment effects and to test for treatment homogeneity over time, we conduct an event-study analysis using the dynamic TWFE model, incorporating indicators that measure the proximity to the implementation of the LOGSE reform. Our approach involves estimating the following specification:

$$Y_{itk} = \alpha + \beta_g \times \sum_{g=-6}^{g=+2} D_{g(kt)} + \gamma_t + \theta_k + \varepsilon_{itk} \tag{2}$$

where Y_{itk} is binary indicator equals to 1 for very premature birth (less than 32 weeks) and $D_{g(kt)}$ is a set of indicator variables that take value 1 if, for a child born to a mother from cohort t in province k, the implementation of LOGSE was g cohorts away. [28] [29] To define $D_{g(kt)}$, we transform our LOGSE exposure index into a binary indicator, which takes value 1 when in k province and for t cohort at least 75% of the student population under LOGSE. Therefore, we compare children born to mothers enrolled in the high school system when more than 75% of their cohort was under LOGSE in their province with those born to mothers from the last set of provinces and cohorts that adopted the reform, considering the latter as a 'never treated' group.[30]

We replicate our results using the alternative estimators introduced by Borusyak et al. (2024) and Callaway et al. (2021) that deliver consistent estimates even in the presence of heterogeneous treatment effects across time. Figure 4 displays the event-study figures and demonstrates that the estimates align with the parallel trends assumption: regardless of the estimator used, the coefficients for cohorts prior to the implementation of LOGSE are consistently near zero, showing no significant pretrends. Figure 4 also clarifies the dynamics of treatment effects: all the recently developed robust estimators show treatment effects that increase over time in the post-periods. The increasing treatment effects over time could be attributed to the prolonged

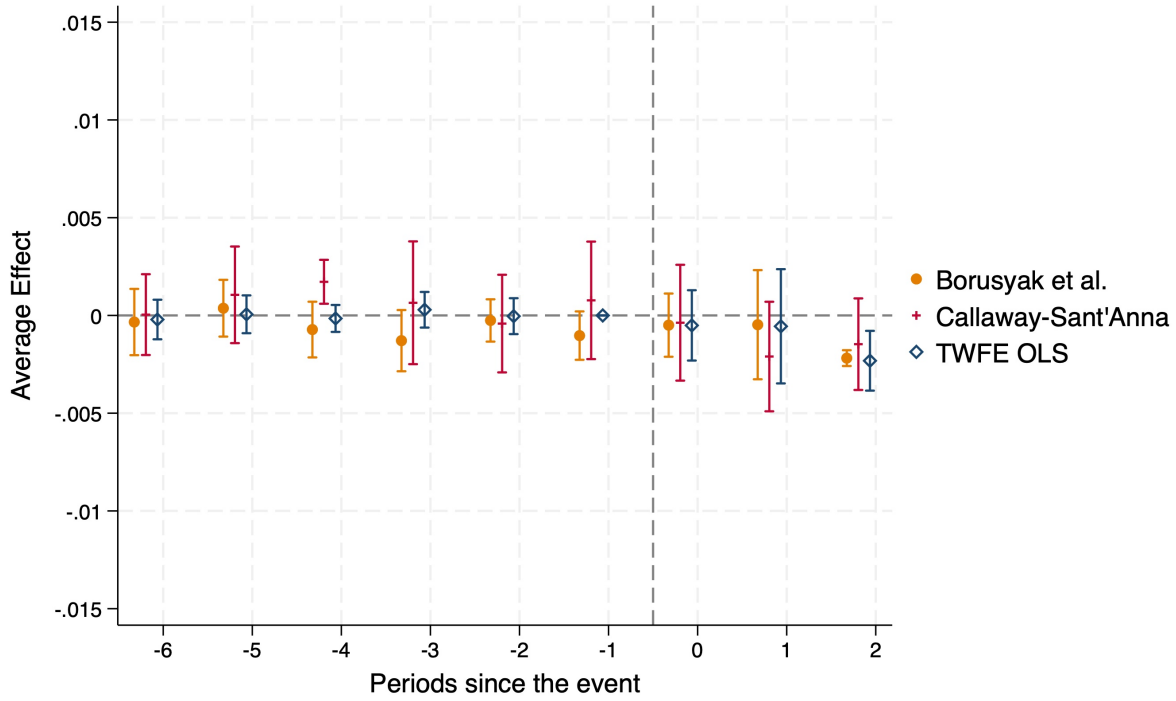
[28] All other variables are defined as in equation (1).

[29] We conduct this exercise for the likelihood of very premature birth (less than 32 weeks) as this outcome variable has the most precisely estimated coefficient for the reform's effects, as indicated in Panel B of Table 3. See exercises for the rest of the health at birth outcomes in the Figure A5 of the Appendix.

[30] The choice of the 75% threshold maximizes the variability in treatment status across time units, allowing us to observe three post-reform periods while maintaining a 'never treated' group. See the event study exercise replicated for the different thresholds in Figure A4 of the Appendix.

exposure to more general education.

Figure 4: Effects of the Reform on the Likelihood of Very Preterm Birth Based on Distance to/from LOGSE Implementation



Notes. This figure combines the event-study plots constructed using three different estimators: a dynamic version of the TWFE model (in blue with diamond markers); Callaway and Sant'Anna (2021) (in red with cross markers); and Borusyak et al. (2024) (in orange with circular markers). The outcome is a binary variable that takes value 1 for very preterm birth (less than 32 weeks). The time variable is the birth cohort of the mother and the treatment group is defined by the province where at least 75% of the student population was under LOGSE during the corresponding cohort period of the mother. We restrict our sample to mothers born between 1975 and 1983, rather than including those born up to 1985 as in estimates from Eq. (1). This decision is based on the fact that from 1984 onwards, more than 75% of the student population in all provinces was under LOGSE. Model 2 requires the use of control units as a "never treated" group, which limits the maximum number of post-periods that can be estimated to two. We estimate six pre-periods because the youngest cohort studying in a province with more than 75% of its population under LOGSE was the 1978 cohort. The bars represent 95 percent confidence intervals. Standard errors are clustered at the province level.

Using a continuous treatment additionally requires being able to rule out selection into treatment intensity levels (De Chaisemartin and D'Haultfoeuille, 2022; Callaway et al., 2024). In essence, this assumption arises because of potential heterogeneity in gains. If provinces that expect higher gains self-select into higher intensity treatment levels, this selection bias would contaminate the continuous treatment DiD. There is little scope for self-selection in our setting since individuals do not endogenously choose the amount of treatment, especially given the absence of spurious correlations between the reform rollout and inter-provincial migration patterns (as shown in Table A13 of the Appendix). In addition, we find limited treatment effect heterogeneity when comparing earlier versus later-adopting provinces across intensity levels. Specifically, following Bailey

et al. (2024), we include an interaction term in (1) between the main reform exposure index and an indicator for provinces being "early adopters" (i.e., having over 75% of the 14-year-old population under the new system by 1997, when about one-third of provinces had reached the 75% cutoff). As shown in Table A14 of the Appendix, the interaction term fails to be significant for both very low birth weight and very preterm birth, which mitigates concerns regarding heterogeneity in gains across intensity levels.[31]

5.4.2 Sample Selection Bias: Fertility Decisions

In addition to concerns regarding treatment exogeneity or strong parallel trends, there is also the issue of sample selection bias due to changes in fertility decisions, as we only sample women who became mothers. If the reform had also modified women's decisions to have children, this would have led to a selected sample of those observed giving birth. Nonetheless, the reform's lack of impact on women's years of schooling suggests a limited causal role for the reform in a woman's fertility decisions.

Prior studies on the effects of education on infant health at birth have also looked at the effect of years of schooling on fertility patterns (Currie and Moretti, 2003; McCrary and Royer, 2011). 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). Moreover, Becker (1965)'s quality/quantity trade-off suggests that more years of education may also induce women to have fewer children of higher quality. However, little is known about the effects of changes in maternal educational pathways on fertility.

To test the impact of LOGSE on fertility patterns, we create a panel of annual birth rates by mother's age at the province level using Childbirth Register data and estimate an aggregate version of equation (1). The findings, presented in Column 1 of Table A9 in the Appendix, reveal no impact of LOGSE on birth rates. Further analysis, presented in Column 2 of Table A9 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 A6 in the Appendix, which illustrates the reform's influence—or lack thereof—on birth rates across mothers' age groups. Figure A6 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, we can assert that the reform did not lead to a selected sample of observed mothers and women who were more and less exposed to the reform and who were equally likely to belong to our sample of mothers.

5.4.3 Potential Confounding Factor: The Role of Peers

Thus far, our analysis has assumed that the integration of more general education into secondary schooling drives the reform's positive effects on infant health directly. However, a remaining concern is that the policy reform may have affected maternal outcomes, and thus infant health at birth, through changes in peer

[31] The econometric literature has yet to offer an estimator suitable for staggered treatments with continuously varying intensity levels within groups (de Chaisemartin and D'Haultfœuille, 2023). In our setting, provinces begin treatment at different dates and intensities. Unlike typical continuous and staggered treatments, the intensity of treatment systematically increases over time across all provinces until reaching the maximum intensity level, where 100% of the 14-year-old population is under the new system. Indeed, this feature restricts the set of comparisons that we can make and the set of estimators we can use.

composition. Comprehensive schooling reforms, such as LOGSE, which transition from an earlier to a later tracking system, inherently trigger changes in the composition of peers by moving mixed-ability students together for a longer period. 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).[32] The characteristics of the Spanish high school system make it less likely to trigger significant effects through peer composition changes for the following reasons: (i) both pre- and post-reform systems did not track students by ability, minimizing self-selection; (ii) both pre- and post-reform systems generally provided vocational studies at standard high schools where all students were mixed; and (iii) high enrollment rates between the ages of 14 and 16 meant there was limited scope for introducing a substantial number of low-motivated, out-of-school students into education centers.

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 (1) between the reform exposure index and indicators for municipality size or type of maternal occupation.[33]

[32]For instance, research on similar schooling reforms affecting tracking or students' peer groups has found negative effects of increased variability in peer quality on mental health, increased risky health behaviors like smoking or drug consumption, and higher mortality rates, mostly due to a poorer school environment, exposure to more adverse peer groups, and increased 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 (1). 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.

Panel A in Table A16 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 no significant differential impacts due to the municipality size of the mother's place of residence. While there appear to be some significant differences in the incidence of low birth weight and the number of gestational weeks at the 10% level (Columns 2 and 4, Panel B), these differences are arguably not relevant due to their varying implications for health at birth and low statistical power. All in all, the results from Table A16 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.

6 Mechanisms

We have shown that the reform's main effect was to induce a switch from the old vocational and general education high school programs to the new general education high school system, while keeping both educational attainment and years of schooling constan (Tables 1 and 2). However, the boost to women's general education did significantly reduce the incidence very preterm births (Table 3). In this section, we empirically test how extending mothers' general education maps onto the documented changes in infant health by increasing women's earning potential and improving their information processing skills.

Effects on Women's Earning Potential

Panel A of Table 4 presents the effects of LOGSE on labor market outcomes. Increased general education can improve women's permanent income and maternal prenatal investments by increasing skill portability across occupations in the labor market (Goldin, 2001; Hanushek et al., 2017). Our data show that the number of mothers already in the labor force at the time of the birth of their first child increased significantly after the implementation of the reform. (Column 1, Panel A). The reform significantly increases the likelihood of a mother being engaged in paid work outside the home by 2.75 percentage points, which represents a 3.22% rise. We observe no economically or statistically significant effects on the type of female occupation (Columns 2 and 3, Panel A). These findings suggest that the reform had positive effects on maternal labor force participation at the time of their first birth due to a greater share of mothers being engaged in paid work outside the home, potentially leading to an increase in mothers' earning potential and prenatal investment.

Panel B of Table 4 reports the effects of the reform on assortative mating. Increased earning potential resulting from more general education may also contribute to positive assortative mating, leading to higher household permanent income and prenatal investment through a multiplier effect (Behrman and Rosenzweig, 2004. Our data show no significant effects of the reform on mothers' partner quality (Columns 1 and 2, Panel B), as measured by their partner's occupation (McCrary and Royer, 2011). The results suggest that it seems unlikely that mate selection is behind the estimated positive impacts of maternal general education on infant health.

Table 4: Reform Effects on Women's Earning Potential

	(1)	(2)	(3)
Panel A. Labor Market Outcomes			
	Working Mother	**Qualified Job**	**Non-Qualified Job**
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
R-squared	0.028	0.016	0.015
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	
R-squared	0.012	0.014	

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 1 on a sample of first deliveries of mothers aged 25-33 and born between 1975 and 1985. All specifications include a constant and main controls for birth year and province of residence. Romano-Wolf p-values based on 1,000 studentized bootstrap replications.Data are from the 2000-2016 Vital Statistics (INE) childbirth microdata.

Effects on Information Processing Skills, Risky Health Behaviors, and Family Planning

The following analysis in this subsection examines women's knowledge, as well as their health and reproductive behavior, further exploring the theoretical channels through which LOGSE improved infants' gestational age. In contrast to vocational education, which provides specific skills for particular occupations, the broad and more transferable nature of comprehensive curricula could potentially enhance women's information processing skills, thereby increasing their ability to learn about health behavior during pregnancy and effectively use contraceptives, and thus leading to greater control over the timing of births. In Table 5, we empirically investigate these channels by first exploring the effects of the reform on women's information processing skills during adulthood, and second, examining whether these improved information skills translated into a higher ability to avoid risky health behaviors and a reduced likelihood of unplanned pregnancies.

Panel A of Table 5 shows that the reform did improve women's information processing skills during adulthood. Specifically, our analysis shows that mothers exposed to more general education scored 0.911 and 1.822 standard deviations higher in the PIACC literacy and numeracy assessments. These estimates remain robust after multiple hypothesis testing, confirming the reform's positive impact on information processing skills. [34]

Panel B of Table 5 reports whether these improved information skills translate into healthier maternal behavior. Column 1 of Panel B indicates a significant decrease of 8.7 percentage points in the likelihood of drinking alcohol during pregnancy as a result of the reform. Estimated impacts of the reform on women's consumption of alcohol, tobacco, and other illegal drugs, regardless of pregnancy status, are generally statistically insignificant but indicative of a negative pattern. Overall, these findings seem to suggest that healthier maternal behavior as a result of better information processing skills may play a role in explaining the lower incidence of preterm births as a consequence of LOGSE.

Panel C of Table 5 reports the reform's effects on the likelihood of marriage before birth and duration of marriage as well as reported father's presence. Our data show that there is a significant increase of 2.35 percentage points in the likelihood of marriage before birth, representing a 3.05% increase (Column 1). This higher share of married mothers due to the policy change could plausibly indicate a greater ability to plan ahead for motherhood, which may also explain the improved health outcomes at birth. In addition, this evidence of more frequent pre-birth marriages aligns with a trend, though not statistically significant, towards longer marriages before the first birth (Column 2) and fewer instances of births where the father is not reported (Column 3). These trends may collectively hint at a shift towards more intentional and possibly better family planning practices.

[34] Note that because the PIAAC dataset only contains information on women's region of residence, the impact of the new comprehensive curricula on information processing skills is measured by women's exposure to these curricula at the regional level. This approach yields results that are highly consistent with our main infant health analysis, despite being less precisely estimated due to the lower variability at the regional level. As detailed in Appendix Table A17, this analysis also identifies a significant reduction in the incidence of premature and very premature births when estimating equation 1 at the regional level.

Table 5: 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		
R-squared	0.088	0.121		
Panel B. Risky Health behavior				
	Mother Drinks During Pregnancy	**Mother Drinks**	**Mother Smokes**	**Mother Consumes Drugs**
Index	-0.087**	-0.403*	-0.340*	-0.060
	(0.039)	(0.214)	(0.177)	(0.237)
Romano-Wolf P-value	0.036	0.109	0.228	
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
R-squared	0.047	0.119	0.126	0.063
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	0.043			
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	
R-squared	0.053	0.016	0.004	

Notes. Standard errors are in parentheses. The estimates were obtained by applying Eq. (1) 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 son or daughter 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). Specifications in Columns (1) and (2) of Panel A include a constant and main controls for birth year and region of residence, as the main regressor is at the regional level. The remaining specifications include a constant and main controls for birth year and province of residence. Romano-Wolf p-values based on 1,000 studentized bootstrap replications. Data are from the 2011 PIAAC Survey for Spain (Panel A); the 2017 Alcohol, Tobacco, and Illegal Drug Survey (Column 1, Panel B); the 2007 Alcohol, Tobacco, and Illegal Drug Survey (Columns 2-4, Panel B); and the 2000-2018 Vital Statistics (INE) childbirth microdata (Panel C).

7 Conclusions

This paper contributes to the ongoing discussion on how education can best reduce the intergenerational transmission of human capital. We exploit a unique policy shock that integrated more general education into secondary schooling while keeping both educational attainment and years of education constant. We provide causal evidence on the intergenerational returns to general education. In particular, by leveraging the staggered introduction of a comprehensive educational policy reform across Spanish provinces, we implement a DiD research design and compare the health outcomes of children born to mothers with different intensities of exposure to the reform. Using detailed administrative data from birth certificates, we find that two additional years of general education led to significant reduction of 37.14% in the incidence of very preterm birth (less than 32 gestational weeks).

Our findings are in line with mechanisms proposed in economic theory and previous literature on the effects of general knowledge acquisition on improved earning potential and information processing skills (Grossman, 1972; Thomas et al., 1991; Goldin, 2001). Information on mothers' occupations, marriage market allocation, and fertility choices from birth registers, together with survey data on adult literacy and numeracy assessments, as well as on alcohol, tobacco, and illegal drug consumption, allows us to identify possible underlying channels through which the reform may have affected children's health at birth. Our results suggest that the observed positive effects on infant health may stem from increased maternal labor supply and improved adult information processing skills, which reduced risky health behaviors and potentially improved family planning.

Further research might investigate the impact of educational curricula on maternal labor, health, and social outcomes. While we only focus on health at birth, further work could examine the effects of changes in mothers' educational pathways on children's long-term health outcomes. It would also be worth examining other variables that can serve as proxies for understanding the mechanisms through which infant health at birth is affected by changes in maternal education, such as mothers' prenatal care visits, mental disorders (e.g. anxiety or depression), or earnings.

Overall, our study shows the potential benefits of integrating more general education into the high school system. We highlight the importance of considering the impact of educational policies, particularly those concerning school curricula, on health outcomes, as opposed to focusing solely on years of education. The global challenge of reducing preterm births and their negative short- and long-term consequences on mortality and morbidity further amplifies the importance of our findings. Perin et al. (2022) provide updated estimates of cause-specific mortality for children under 5 years across all 194 WHO Member States from 2000 to 2019, revealing that complications arising from preterm births account for 17.7% of deaths in this age group. Regarding very preterm births, Barfield (2018) finds that they account for 52% of infant deaths, along with substantial medical complications, neurodevelopmental disabilities, and associated healthcare costs, in the US where the incidence is 1.5%. Thus, this paper contributes to the broader debate on the role of educational pathways in promoting social mobility and reducing inequality.

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Appendix

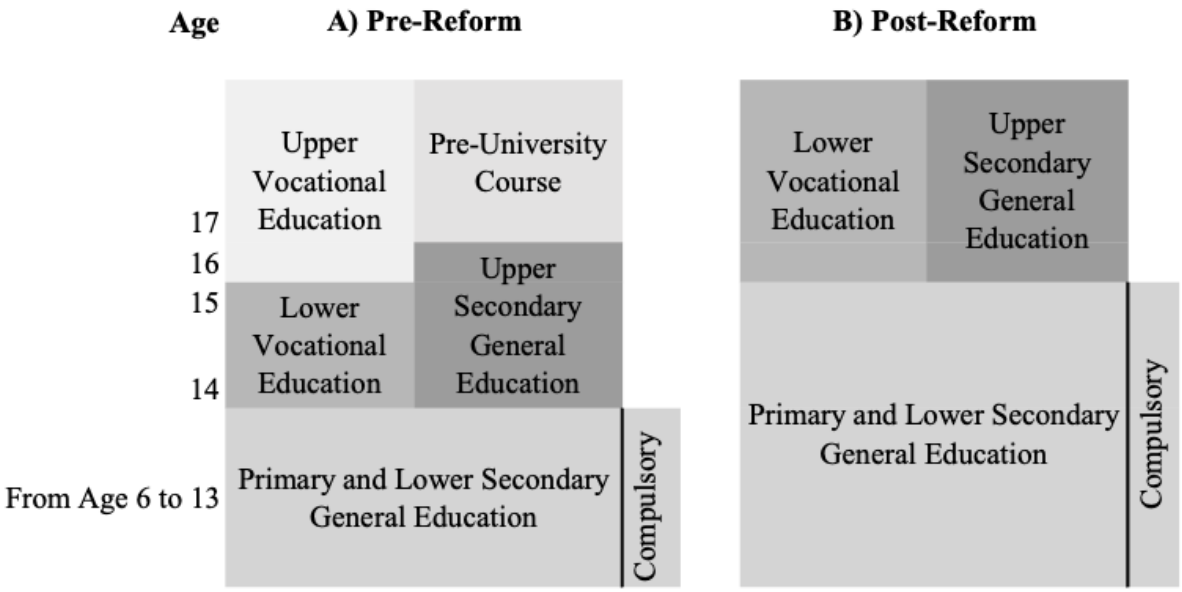


Figure A1: The Spanish High School System Pre- and Post- Reform

Notes. The artistic education program (Music and Dance and Dramatic Arts) is not included. Source: Spanish Education Ministry.

Table A1: Educational Curricula Before and After the Reform At Ages 14 and 15

Age	Pre-Reform System		Post-Reform System
	Lower Secondary General Education	**Vocational Education**	**Lower Secondary General Education**
14			
	Math	5 Occupation-Specific Subjects	Math
	Social Sciences	Apprenticeships	Social Sciences
	Spanish Language	Spanish Language	Spanish Language and Literature
	Sciences	Humanistic Education	Sciences
	Foreign Language	Foreign Language	Foreign Language
	Physical Education	Physical Education	Physical Education
	Drawing	Civic, Social and Political Education	Education in Civic and Ethical Values
	Religious Education	Religious Education	Religious Education (voluntary)
	Music and Arts		Music and Arts
			Technology and Digitalisation
			1 Optional Subject (Second Foreign Language or Humanistic Studies)
15			
	Math	5 Occupation-Specific Subjects	Math
	Social Sciences	Apprenticeships	Social Sciences
	Spanish Language and Literature	Spanish Language	Spanish Language and Literature
	Foreign Language	Foreign Language	Foreign Language
	Physical Education	Physical Education	Physical Education
	Latin	Civic, Social and Political Education	Education in Civic and Ethical Values
	Religious Education	Religious Education	Religious Education or Study Time (voluntary)
	1 Occupation-Specific Subject	Humanistic Studies	3 Academic Field-Specific Subjects (Biology and Geology, Physics and Chemistry, Music and Arts, Technology, Second Foreign Language, Classical Culture)
	Sciences		

Notes. Educational curricula corresponding to lower vocational studies (*FP I*, ISCED 2011 level 3) and the first two years of upper secondary education (*BUP* ISCED level 3) of the previous educational system (*LGE*) and the last two years of lower secondary education (*ESO* ISCED 2011 level 3) of the new educational system. Source: Laws (Real Decreto 160/1975, Real Decreto 707/1976, Real Decreto 1007/1991).

Table A2: 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.
Fetal Death	0.001	0.03	0	1	1,576,204	Dummy variable equals to 1 if the first-born is born dead; 0 otherwise.
Survive 24h after Birth	0.999	0.04	0	1	1,576,204	Dummy variable equals to 1 if the first-born survives the first 24 hours after the birth; 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 A3: 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 variable equals to 1 if the mother is actively engaged in paid work outside the home even if she is not currently working, and 0 otherwise.
Qualified Job	0.558	0.496	1,234,724	Dummy variable equals to 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 variable equals to 1 if the mother is employed in a non-skilled 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 variable equals to 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 variable equals to 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 variable equals 1 if the mother was married before the first birth conditional on having a registered father/partner, 0 otherwise.
Duration of Marriage Before Birth	34.342	24.860	1,020,212	Number of months between the date of marriage and the date of the first birth conditional on being married.
Absent Father	0.014	0.116	1,576,204	Dummy variable equals to 1 if if the father's name is not listed on the birth register at the time of childbirth, and 0 otherwise.

Notes. Sample: mothers aged 25-33 at their first birth and 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 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 female Spanish nationals aged 17-25 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: Maternal Health behavior Summary Statistics

	Mean	Std.Dev.	N	Definition
Mother Drinks During Pregnancy	0.067	0.249	2,365	Dummy variable that takes the value 1 if the woman consumed any alcoholic drink during pregnancy; 0 otherwise.
Mother Drinks	0.838	0.367	1,048	Dummy variable that takes the value 1 if the woman has ever consumed any alcoholic drink, regardless of pregnancy status; 0 otherwise.
Mother Smokes	0.658	0.474	1,048	Dummy variable that takes the value 1 if the woman has ever smoked, regardless of pregnancy status; 0 otherwise.
Mother Consumes Drugs	0.296	0.457	1,048	Dummy variable that takes the value 1 if the woman has ever consumed any substance, including alcohol, tobacco, hypnosedatives, illegal drugs, and new substances, regardless of pregnancy status; 0 otherwise.

Notes: The summary statistics for "Mother Drinks During Pregnancy" are derived from the 2017 Spanish Alcohol, Tobacco, and Illegal Drugs Survey, focusing on a cohort of women born between 1975 and 1985. The data for "Mother Drinks", "Mother Smokes" and "Mother Consumes Drugs" are based on the 2007 Spanish Alcohol, Tobacco, and Illegal Drugs Survey, sampling women born between 1975 and 1983 who were aged 25 to 33 years old.

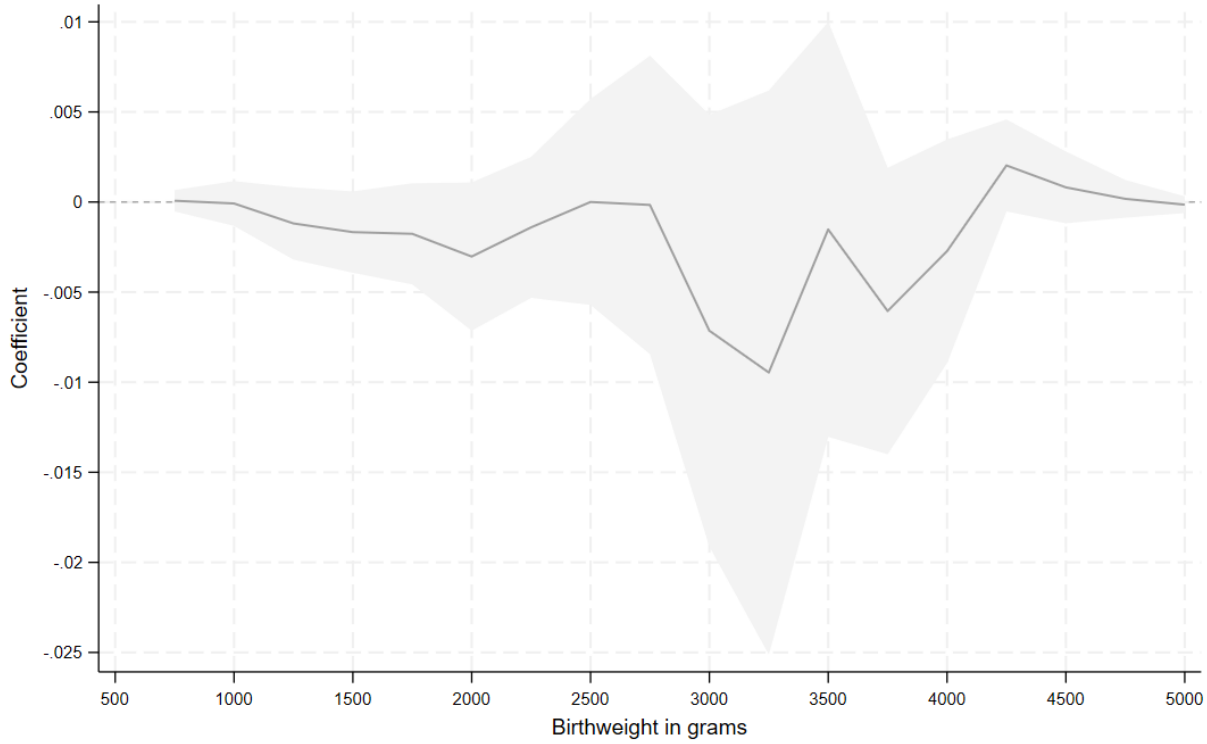


Figure A2: Reform Effects along the Distribution of Weight at Birth

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 likelihood to have birth weight of at most the amount on the x axis. All coefficients were computed estimating 1. Standard errors are clustered at the province level. Source: Data were sourced from the Childbirth Microdata of Vital Statistics (INE) for the years 2000-2018.

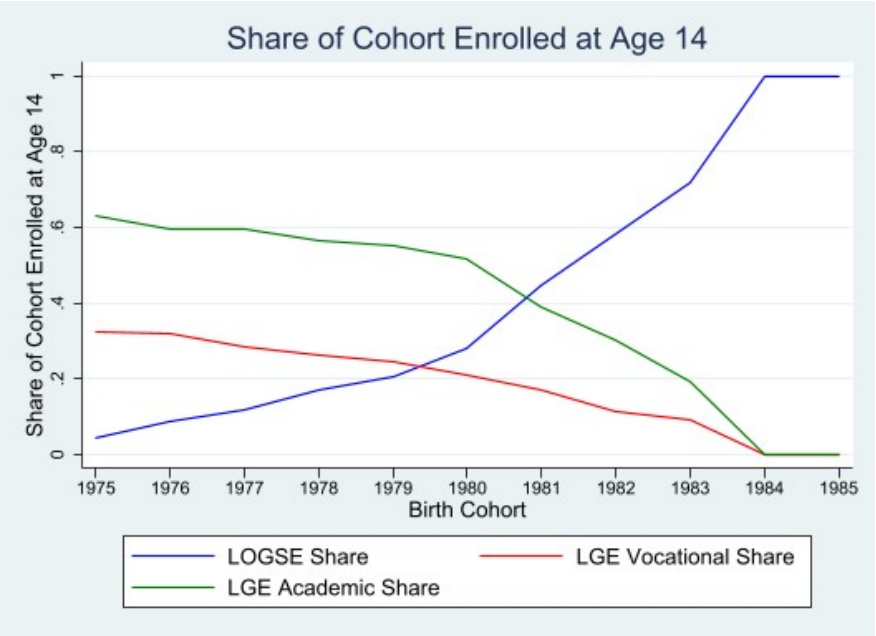


Figure A3: LOGSE, LGE Vocational and LGE General Education Enrollment Shares by Birth Cohort

Notes. This figure shows the share of 14-year-olds by birth cohort under general education (LOGSE share, blue line), old vocational programs (LGE Vocational share, red line), and old general programs (LGE Academic share). The 1975 cohort was the first to be eligible for the reformed high school system. Source: Annual Education Statistics reports, Statistical Office of the Education Ministry, multiple years: 1989-2001.

Table A6: Effects of Educational Pathways on Very Low Weight, Premature and Very Premature Birth

	(1)	(2)	(3)
Outcome: Very Preterm Birth			
Index	-0.0026***		
	(0.001)		
Vocational Index		0.0078***	
		(0.002)	
Old General Index			0.0006
			(0.002)
Obs	1,296,160	1,295,433	1,295,839
R-square	0.000	0.000	0.000

Notes: The outcome variable is a dummy for very premature births (under 32 weeks). Standard errors are in parentheses. The estimates are obtained from estimating Equation 1 on a sample of mothers aged 25-33 and born between 1975 and 1985. LOGSE Index in Column (1) refers to the main LOGSE exposure index. The estimates for this index are also reported in Column (3), Panels A and B of Table 3. Vocational Index in Column (2) refers to the share of 14-year-olds under the old vocational program at the province level. Old General Index in Column (3) refers to the share of 14-year-olds under the old general education program. The 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 A7: First Stage Regressions

Dependent Variable: $GeneralIndex_{tk}$		
	(1)	(2)
General Index1975 * 1977 FE	0.3124*	0.2534
	(0.167)	(0.165)
General Index1975 * 1978 FE	0.1909	0.2117
	(0.183)	(0.166)
General Index1975 * 1979 FE	0.2776	0.3063*
	(0.179)	(0.170)
General Index1975 * 1980 FE	-0.1484	-0.1668
	(0.097)	(0.110)
General Index1975 * 1981 FE	-0.1873	-0.2472
	(0.221)	(0.183)
General Index1975 * 1982 FE	-0.4764*	-0.5066**
	(0.243)	(0.218)
General Index1975 * 1983 FE	-0.5036**	-0.5966**
	(0.249)	(0.225)
General Index1975 * 1984 FE	-0.7857***	-0.7793***
	(0.206)	(0.156)
General Index1975 * 1985 FE	-0.7854***	-0.8779***
	(0.206)	(0.180)
	(0.024)	(0.227)
Controls	NO	YES
Observations	1,408,246	1,403,065
R-squared	0.808	0.816

Notes: The table reports the first-stage regression results for the instrumented share of students enrolled in the new and old general education programs ($GeneralIndex_{tk}$). Instruments are derived from the 1975 pre-reform cross-province variation, interacted with cohort fixed effects. The sample includes Spanish mothers aged 25–33, born between 1975 and 1985. Cohort and province fixed effects are included in both regressions (Columns 1 and 2), along with additional province time-varying controls for GDP per capita, female employment rate, female labor force participation rate, log population, and mortality rates (Column 2). Robust 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 A8: 2SLS Regressions

	Very Preterm Birth	
	(1)	(2)
General Index	-0.0062	-0.0055**
	(0.004)	(0.003)
Controls	NO	YES
Obs	1,198,136	1,194,339
R-squared	0.000	0.000

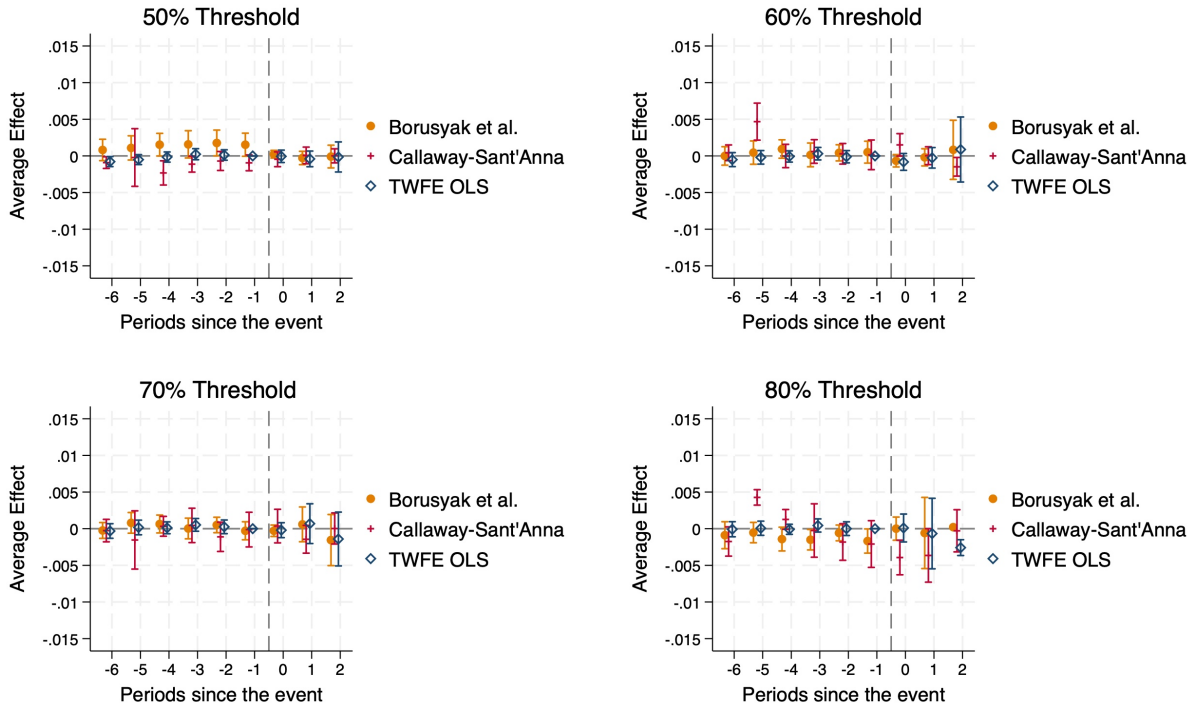
Notes: This table presents the results of 2SLS regressions examining the effect of exposure to the old and new general education programs (as measured by General Index) on Very Preterm Birth. Cohort and province fixed effects are included in all regressions, along with additional province time-varying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

Table A9: Placebo Check for Spurious Correlations Between School Enrollment and Degree Completion Differences Prior (1984-1989) 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)	
Observations	148,370	148,370	148,370	148,370	
R-squared	0.006	0.012	0.024	0.014	
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)
Romano Wolf P-Value	0.999				
Observations	106,171	126,764	126,764	126,764	126,764
R-squared	0.029	0.009	0.017	0.016	0.020

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 1 on a sample of mothers aged 25-33 and born between 1970 and 1975. All specifications include a constant and main controls for birth year and province of residence. Standard errors are clustered at province level for each specification. Romano-Wolf p-values based on 1,000 studentized bootstrap replications. Data are from the 1987-2018 Spanish LFS. ***Significant at the 1% level, **Significant at the 5% level, * Significant at the 10% level.

Figure A4: Effects of the Reform on the Likelihood of Very Preterm Birth Based on Distance to/from LOGSE Implementation Across Thresholds



Notes. This figure combines the event-study plots constructed using three different estimators: a dynamic version of the TWFE model (in blue with diamond markers); Callaway and Sant'Anna (2021) (in red with cross markers); and Borusyak et al. (2024) (in orange with circular markers). The outcome is a binary variable that takes value 1 for very preterm birth (less than 32 weeks). The time variable is the birth cohort of the mother and the treatment group is defined by the province where at least 50%, 60%, 70% and 80% of the student population was under LOGSE during the corresponding cohort period of the mother. The bars represent 95 percent confidence intervals. Standard errors are clustered at the province level. Data are from the 2000-2018 Childbirth microdata of Vital Statistics (INE) for a sample of mothers born between 1975 and 1983.

Table A10: 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
R-squared	0.002	0.000	0.000
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
R-squared	0.002	0.000	0.000
Panel C: Mortality at Birth			
	Fetal Death	Survive 24h after Birth	
Lead Index	0.0002	0.0000	
	(0.000)	(0.000)	
Obs	949,274	949,274	
R-squared	0.000	0.000	

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 1 on a sample of mothers aged 25-33 and born between 1970 and 1975. All specifications include a constant and main controls for birth year and province of residence. Standard errors are clustered at the province level for each specification. 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 A11: 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 and standard errors are in parentheses. Each specification includes controls for cohort and province of residence. The unit of analysis is provincexyear and the coefficient is on the LOGSE exposure Index. Column (2) and (3) reports the mean and standard deviation of the dependent variables. *GDP per capita* is the gross domestic product at market prices (current prices) measured in thousands of euros ased on the 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 is not included. Data are from 1990 – 2000 Spanish Statistical Office. ***Significant at 1% level, **Significant at 5% level, * Significant at 10% level.

Table A12: 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
R-squared	0.002	0.000	0.000
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
R-squared	0.001	0.000	0.000
Panel C: Mortality at Birth			
	Fetal Death	Survive 24 hours	
Index	-0.0001	-0.0000	
	(0.000)	(0.000)	
Obs	1,516,612	1,516,612	
R-squared	0.001	0.001	

Notes. The estimates are obtained from estimating Eq. 1 on a sample of mothers aged 25-33 and born between 1975 and 1985. All specifications include a constant, birth cohort fixed effects, and province fixed effects. 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 A11. Standard errors are clustered at the province level for each specification. Data are from the 2000-2018 Childbirth microdata of Vital Statistics (INE). Standard errors are in parentheses. ***Significant at the 1% level, **Significant at the 5% level, * Significant at the 10% level.

Table A13: 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
R-squared	0.022	0.036	0.026

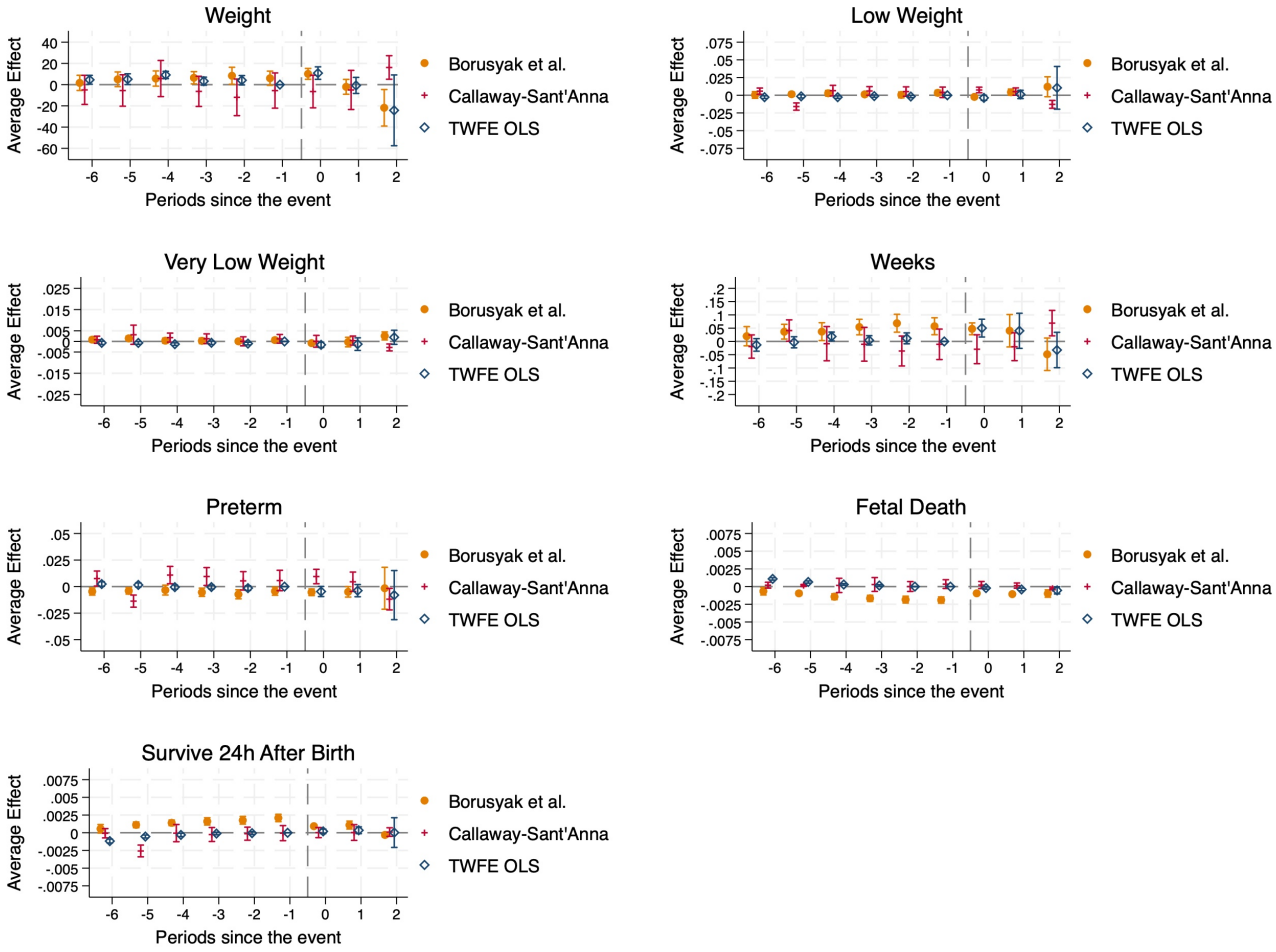
Notes. Outcome: A dummy variable that takes the value 1 if a woman with Spanish nationality is currently residing in a province different from her birth province. Standard errors are in parentheses. Each specification includes controls for cohort and province of residence. The estimates are obtained from estimating eq. (1) on a sample of women aged 17-24 (Column 1), 25-33 (Column 2) and aged 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 A14: Reform Effects on Health at Birth - Early vs Late Adopters

	(1) Very Low Weight	(2) Preterm	(3) Very Preterm
Index	-0.0014	-0.0057*	-0.0026**
	(0.001)	(0.003)	(0.001)
Index*EarlyAdopter	-0.0010	-0.0004	0.0002
	(0.001)	(0.002)	(0.001)
Obs	1,446,005	1,296,160	1,296,160
R-squared	0.000	0.000	0.000

Notes: Standard errors (in parentheses) are clustered at the province level for each specification. Estimates are obtained by estimating Eq. 1 plus an interaction term between the LOGSE exposure index and a dummy variable that equals one if a province had 75% of its 14-year-old population under the new high school system by 1997 (index_early*EarlyAdopter). The sample includes mothers aged 25-33, born between 1970 and 1975. All specifications include a constant and control for birth year and province of residence. Data are from the 1995-2013 Vital Statistics (INE) childbirth microdata. Significance levels: ***1%, **5%, *10%.

Figure A5: Effects of the Reform on Birth Outcomes Based on Distance to/from LOGSE Implementation Across at 75% Threshold



Notes. This figure combines the event-study plots constructed using three different estimators: a dynamic version of the TWFE model (in blue with diamond markers); Callaway and Sant'Anna (2021) (in red with cross markers); and Borusyak et al. (2024) (in orange with circular markers). The outcomes are weight at birth (in grams), low birth weight (less than 2,500 grams), very low birth weight (less than 1500 grams), number of gestational weeks, premature birth (less than 37 weeks), fetal death (born dead) and survival 24h hours after birth. The time variable is the birth cohort of the mother and the treatment group is defined by the province where at least 75% of the student population was under LOGSE during the corresponding cohort period of the mother. The bars represent 95 percent confidence intervals. Standard errors are clustered at the province level. Data are from the 2000-2018 Childbirth microdata of Vital Statistics (INE) for a sample of mothers born between 1975 and 1983.

Table A15: 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
R-squared	0.543	0.010

Notes. Standard errors are in parentheses. The estimate is obtained from estimating eq. 1 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. All specifications include a constant and main controls for birth year and province of residence. Motherhood Entry Age (Column 2) is mother's age at first birth. Standard errors are clustered at province level for each specification. 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.

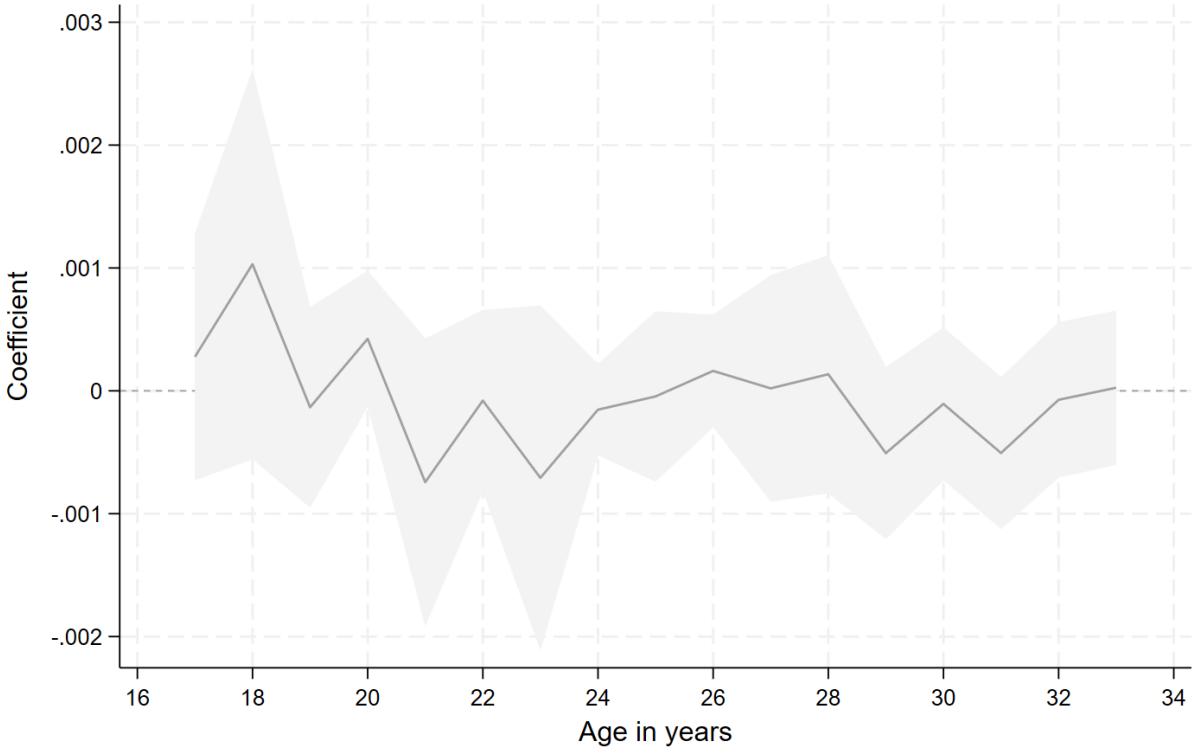


Figure A6: Reform Effects on Birth Rate by Mothers' Age

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.

Table A16: Peer Composition Changes

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

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. 33 on a sample of mothers aged 25-33, born between 1975 and 1985. All specifications include a constant and main controls for birth year and province of residence. Data are 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 A17: 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
R-squared	0.001	0.000	0.000
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
R-squared	0.001	0.000	0.000
Panel C: Mortality at Birth			
	Fetal Death	Survive 24h after Birth	
Aggregate Index	-0.000	0.000	
	(0.000)	(0.000)	
Obs	1,521,770	1,521,770	
R-squared	0.001	0.001	

Notes. Standard errors are in parentheses. The estimates are obtained from estimating eq. (1) on a sample of mothers aged 25-33 and born between 1970 and 1975. All specifications include a constant and main controls for birth year and region of residence. Standard errors are clustered at the region level for each specification. The index captures exposure to the reform at the regional level, rather than at the provincial level as in our main analysis (Table 3). 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.